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An archaeology of Galileo's science of motion

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Dans l'énigme du discours scientifique, ce que [l'archéologie] met en jeu, ce n'est pas son droit à être une science, c'est le fait qu'il existe.

Michel Foucault. L'archéologie du savoir, p. 251.

Dank aan:

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Charlotte. Zonder woorden, wat daar tussen ligt is wat telt.

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0 What this thesis is not

One of these delightful examples of analytical epistemology is the so-called preface paradox. Supposedly, you cannot sincerely write a piece of work, and then add in a preface that you are sure that there will be some mistakes in it. This would come down to asserting all the following statements simultaneously (with p_i *all the claims made in the work):* $p_1, p_2, \ldots p_n, \sim(p_1 \& p_2 \& \ldots p_n)$. *Seeing this as paradoxical involves the assumption of a strong form of closure in the author's writing and beliefs, though. The closure that characterizes at least* this *thesis is not of that kind. It is imposed plainly and simply by the fact that I had to stop writing at a prefixed date. I have been writing new parts, reworking old bits, moving around fragments up till the very last day. All this has been aimed at attaining some kind of overall coherence for the thoughts expressed here. But tomorrow's closure would have been slightly different, I am sure. So rather than using this preface to express the trivial belief in my own fallibility, I will introduce some of the constraints that made possible the partial closure attained.*

0.1 I started out three and a half year ago with a project that was entitled "Towards an integrated model for the relation theory-experiment in physics." There is no integrated model in this thesis. During the first year of working on that project I read Shapin & Schaffer's *Leviathan and the airpump* and Peter Dear's *Discipline and experience* and I was lost. The seventeenth century it would be.

0.2 Nevertheless, the same kinds of problems that exercised me from the beginning are dealt with here. Chapter 1, which doesn't pretend to be a proper introduction, provides some kind of rational reconstruction of my own parcours. It starts from problems having to do with underdetermination, holism and theory-testing, and it ends with Galileo.

0.3 Some of the other chapters make an inverse movement. They start with narrating aspects of Galileo's science of motion, and they end with analyses that maybe could provide elements for an integrated model. But there is no integrated model. There is also no clear-cut separation between the narrative and the analytic. My thinking about Galileo, and my thinking about philosophy of science developed together. It is not yet the time to severe them; if it ever is.

0.4 Chapter 1, which doesn't pretend to be a proper introduction, does sketch a historiographical perspective. It's the kind of perspective that I think is most sensible and fruitful. I stand by it. But I don't defend it against other kinds of approaches. I even don't really speak about other kind of approaches. In my head, I have written chapters comparing my analyses with those of Alexandre Koyré and Edmund Husserl. I would comment on their links with Ernst Cassirer's work. I would try to assess the backgrounds for the great interest in Galileo in between the two world wars, in the works of people like Burtt and Heidegger. These chapters have not been written.

0.5 I start this thesis with an extended discussion of Newton. But I don't want to trace the influence of Galileo on Newton. When the first chapter is done, there is only one other place where his name recurs. I start with Newton to introduce the neo-Kantian perspective that I find so attractive. This perspective determines how I understand the Foucauldian idea of "archaeology," and in particular how this might apply to Galileo. However, the resulting picture of Galileo's science of motion *should* be relevant for understanding Newton's mathematical principles of natural philosophy. But that is more a promissory note than a substantiated claim.

0.6 The studies on Galileo that make up the rest of the thesis could have been presented without this historiographical framework. It would have taken some time to rephrase a few of the issues, but it would have been perfectly possible. It would have made them less rewarding to write, though.

0.7 I do believe in the fruitfulness of the category of the scientific revolution. But every generation of philosophers and historians have the task to rethink this category. Whether a scientific revolution happened depends on us, not on "history." However, this thesis is not a narrative about the scientific revolution; not even about the scientific revolution as I would conceive of it. It stays too close to Galileo, and it stays too close to his theory of motion.

0.8 This is not a thesis about Galileo. It is a thesis about Galileo's science of motion. I am fascinated *by science*. I want to understand what it takes to develop a mathematical representation of nature. It is the imaginative leap from lived experience to disciplined formula that haunts my writing.

0.9 This is not a thesis about Galileo's science. I am completely silent on his astronomical work. This is a serious lacuna, which I can only acknowledge. But I believe that there are also good reasons to focus on the independent development of his science of motion. It might be driven much more by its own research questions and problems than some scholars have wanted to make us believe. There is still a more serious blind spot. The specific mathematical problems that confronted Galileo in developing his science are almost completely neglected. They were serious and determined the kind of science that he finally presented. I take some consolation from the fact that other authors have accorded these issues the attention they deserve and continue to do so.

1.0 Finally, this thesis is not under 150 pages. The road to hell is definitely paved with good intentions.

1 Mechanics and archaeology

Any casual perusal of twentieth century writings in philosophy of science reveals the recurring presence of Newton's science of mechanics in introducing diverging views on the nature of (physical) science. Whether Henri Poincaré, Ernst Cassirer, or Karl Popper want to illustrate their views on the nature of physical hypotheses; whether Patrick Suppes, Joseph Sneed, or Bas van Fraassen want to show how a model-theoretic view applies to physical theories; whether Pierre Duhem, Norwood Russell Hanson, or Clark Glymour want to show the niceties of theory testing; all of them have recourse to Newton's theory as one of their prime examples. (The list could be extended ad libitum*.)*

This situation implies the potentially crucial role of detailed studies in the actual contents and functioning of Newton's paradigmatic theory. Some extremely interesting work has indeed been done over the last decades in the seemingly rather narrow field of Newton studies – which for reasons of professional specialization probably escaped the attention of many philosophers of science. But anyone seriously interested in questions involving conventionalism, underdetermination, and related epistemological issues can learn some highly relevant lessons from studying the recent Cambridge Companion to Newton, which brings together much of the outcome of decennia of high-quality work by both philosophers and historians of science.

I want to take some of the central lessons that can be learned from this work as my starting point in the present thesis. In this chapter I will start by giving a quick sketch of what I take to be these lessons, and by pointing out the resulting possibility of opening up a rather new way of questioning the history of seventeenth century mechanics. This will take us on a quick ride from Newton's Principia*, over Kant's* Kritik der reinen Vernunft *to Michel Foucault's* L'archéologie du savoir*, that will set the issues that lie behind the studies undertaken in all subsequent chapters.*

One warning before entering on this ride: I call this a rather new way of questioning, but I do perfectly realize that many of these questions have already been posed under different guises. Even the overall perspective, which might lay some claim to originality, is in all probability a small variation on many old themes. But as philosophy is all about asking the right questions, I do believe there is value in reformulating old questions in slightly different ways: this might lead us to see connections we didn't see before, to perceive new angles on well-worn subjects that actually can make a difference.

1.1 Newtonian physics and Kantian principles

1.1.1 The Newtonian style

Newton's *Principia* consists of four different parts: an introductory section containing eight definitions, the famous scholium on absolute space and time, and the axioms or laws of motion; and three books, respectively entitled twice "The motion of bodies" (for books 1 and 2), and "The system of the world" (book 3). Together they embody a powerful and coherent research program for linking mathematical representations with real world structures, which was dubbed "the Newtonian style" by Bernard Cohen.[1] A crucial passage where Newton himself expresses clearly what he is up to in his Principia occurs in a scholium to section 11 of the first book:

> Mathematics requires an investigation of those quantities of forces and their proportions that follow from any conditions that may be supposed. Then, coming down to physics, these proportions must be compared with the phenomena, so that it may be found out which conditions [or laws] of forces apply to each kind of attracting bodies. And then, finally, it will be possible to argue more securely concerning the physical species, physical causes, and physical properties of these forces.[2]

The first two books investigate forces treated abstractly. They contain purely mathematical exercises in determining the implications of the laws of motion under different conditions (such as systems of 1, 2, or more bodies, with centripetal forces that vary inversely with distance, with the square of distance, etc. – the second book makes similar exercises for different kinds of proposed forces of resistance). The goal of these exercises is to investigate all sorts of systematic relations that hold in these different kinds of situations, and that enable us to characterize forces by the characteristics of these configurations. This is of course made possible by the nature of Newton's laws of motion, which function as the means to read off the direction and strength of forces from characteristics of bodies' motions. Remember the basic logical situation expressed by the first two laws taken conjointly: any non-inertial motion implies the presence of force along a well-defined direction and with a strength measured by the deviation. As stressed especially by George Smith and William Harper, these mathematical models function as theoretical measurement instruments which allow the determination of parameters that characterize forces from parameters that characterize motion.[3]

In a second stage, "coming down to physics", these models are put to use in the third book to measure the characteristics of the forces that can be found in our solar system. To this end Newton

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[1] Cohen 1980.

[2] *Principia*, pp. 588-589.

[3] E.g. Smith 2002a; Harper 2002a.

starts by enumerating a number of "phenomena", such as Kepler's laws. These empirically established regularities are then probed by the mathematical models from the first book. The ensuing deduction of the universal law of gravitation is much more subtle than it is often made out to be; I will here focus on the two most significant aspects of the argument that are highlighted in the magnificent reconstructions by Howard Stein, Michael Friedman, George Smith and William Harper: the essential use of successive approximations, and the crucial role played by Newton's philosophical rules.[4] Taken together these reasoning strategies – because that is what they come down to – are supposed to lead to what Newton calls "a more secure" way of arguing about the forces of nature. But before commenting on these, we must first see how the mathematical models are put to use.[5]

In a first step Newton starts from some astronomical regularities (his phenomena) and assumes that the reference frame in which they are described (e.g. for the characteristics of the motion of the Jovian moons a frame with Jupiter at rest) is approximately an absolute frame of reference. This is of course a crucial assumption, because otherwise he would not be able to compare the dependencies expressed by these astronomical regularities with the mathematical dependencies described in his mathematical models from the first book. Remember that these express the consequences for different force configurations that flow from the laws of motion; i.e. it is assumed that all unperturbed motion is necessarily inertial and that we can use all deviations from this motion as a criterion to infer the presence of a force with properties measured by the deviation. Given this assumption, Newton can use these astronomical regularities to ascertain first (on the basis of Kepler's area law) that they are caused by centripetal forces, the strength of which can also be measured (on the basis of Kepler's harmonic laws). It turns out that each observed astronomical orbit[6] is governed by an inverse square force [Props. 1 to 3]. Newton then introduces his famous "moon test": the acceleration thus found for the moon in its orbit around the earth[7] is compared with the acceleration due to gravity as measured by pendula (as was done by Huygens). It is found out that both values agree within rather severe limits. On this basis Newton then concludes that the force deflecting the moon in its orbit is the same as what gives earthly bodies their weight: it is a force of gravity [Prop. 4]. The same kind of conclusion is then extended first to all other satellite systems for which an inverse square was deduced, and then further to the planets that are not orbited [Prop. 5]. If we now also assume that the same regularities would continue to hold if there would have been other satellites at other distances from the orbited astronomical object

[4] Stein 1970, 1991; Friedman 1992 (chapter 3); Smith 2001, 2002a,b; Harper 2002a,b.

[5] See Stein 1991 for a keen analysis of the structure of the argument leading up to the law of universal gravitation; Harper 2002a and Ducheyne 2006 are useful overviews of the different steps in the argument.

[6] This only holds for the planets orbiting the sun, not if we describe them as orbiting the earth. This is not prejudicing the question whether the solar system is heliocentric or Ptolemaic; it is merely a mathematical consequence of the fact Kepler's laws hold true from the former perspective, but not from the latter.

[7] To be precise, this mathematical argument for the acceleration of the moon is not based on a harmonic law (which does not hold for the moon), but on the motion of the moon's apogee.

[Scholium to Prop. 4], then we can conclude, in Howard Stein's terminology, that all these astronomical objects give rise to an *acceleration field* – i.e. *any* body that would be placed at some determinate distance from this central object would undergo the *same* (gravitational) acceleration (a generalization of Galileo's observation on free fall) [Prop. 6].

In a second step, Newton combines all the different acceleration fields that have been established in the first step. This implies that we are no longer dealing only with e.g. the acceleration of the earth towards the sun, but also and simultaneously with the acceleration of the sun towards the earth. Yet if we now take account of the third law of motion (equality of action and reaction) it immediately follows that the respective forces exerted on each other by two astronomical bodies are in the same proportion as their masses [Prop. 7]. As a result, the universal law of gravitation is deduced from the phenomena. At this point we are also in a position to ascertain the masses of the different objects in our solar system. This allows us to determine the centre of gravity of the system, which is found to be close to the sun's position [Prop. 8].

Now it is time to bring in some of the subtleties of the foregoing derivation.

First, it must be noticed that Kepler's laws of planetary motion are only known to be approximately true to start with. So how secure an inductive basis is this? Answer: Newton has been careful enough to prove that the mathematical relations that he has established on their basis also hold approximately if the astronomical regularities are only approximately true! That is, the theoretical measurements executed through his mathematical models are still reliable. But there is more. Newton turns this apparent inexactness into an evidentiary use that strengthens the derivation of an *exact* inverse square law from *approximately* holding regularities even further.[8] Consider: once we have found out the law that would hold exactly if there were no other perturbing forces, we can interpret all deviations from the motion that would follow from this law alone as due to such perturbing forces. But having established the force laws that hold in the whole solar system, Newton is in a position to check whether the deviations are indeed systematic, i.e. due to the gravitational interaction with (in the first approximation) a third body. In George Smith's terminology: by assuming the validity of the exact law, we can turn deviations from the primary phenomena into second order phenomena, which in their turn can be embedded in the theoretical framework. This possibly introduces new deviations of the theoretically deduced second order phenomena from the "empirically established" second order phenomena. (Empirically goes within quotation marks because they are of course established on the basis of the deviations from the *theoretical* expectations.) And so on. If this is possible, that is, if the deviations indeed turn out to be systematic, then we have strong grounds to assume that the exact law from which we started this process of successive approximations holds true. (If these deviations would have been mere artefacts stemming from our using the wrong force law to start with, it would have

[8] See especially Smith 2001, 2002a, 2002b.

been very improbable that they could have actually been turned into second order phenomena – they would not have been true disturbances.)

Secondly, both in the first and second step leading up to the law of universal gravitation, Newton makes some far-reaching extrapolations. What has not yet been mentioned about these is that Newton backs them up by his rules for natural philosophy.[9] These express the following methodological maxims: that we must try to posit as few different causes as possible [Rules 1 and 2], that "those qualities of bodies that cannot be intended and remitted [i.e., qualities that cannot be increased and diminished] and that belong to all bodies on which experiments can be made should be taken as qualities of all bodies universally" [Rule 3], and that we must not overrule "propositions gathered from evidence by induction" by mere contrary hypotheses, but only by new measurements [Rule 4].[10] William Harper has been arguing forcefully that we should not read these rules as mere appeals to an ideal of simplicity.[11] They also and primarily serve to impose rather severe *constraints* on the constructed causal models for the phenomena that help to *generate further evidence*. By demanding that the cause of the acceleration of the moon should be ascribed to the same cause as the fall of bodies near the earth, we can compare different measurements of this unified cause, which now *must* yield resilient data. The most striking instance of this is the second and most controversial step of Newton's argument for universal gravitation (most controversial at least for his contemporaries such as Huygens)[12], where he applies his third law of motion to combine the different acceleration fields to derive one universal force law. It is only through this move that we compare the masses of the different astronomical objects, which must now also be found to agree with the measurements of the relative inertial masses as measured by orbital phenomena. Newton's appeals to making "general by induction" propositions that are gathered from evidence reveal what Harper calls an "ideal of empirical success". This ideal requires that one tries to impose as much constraints as possible to strengthen the evidential basis for the system of the world.

A more secure way of reasoning, indeed![13]

Along the way, and surely not as an accidental by-product, Newton has also solved the controversial question about the true constitution of our solar system:[14] Copernican or Ptolemaic? The centre of gravity of the system almost coincides with the position of the sun, hence it is heliocentric.

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[9] These were only explicitly introduced under this name in the second edition of the *Principia*, although the first two rules were already stated in the first edition.

[10] *Principia*, pp. 794-796.

[11] Harper 2002a,b.

[12] The presence of a material ether that would be responsible for the transmittance of force would make it highly unlikely that momentum would be conserved as demanded by the third law.

[13] It remains to be mentioned that Newton does not stop at the point he has derived his law of gravitation, but immediately pushes it to explain a host of other previously unexplainable phenomena, such as e.g. the tides and the precession of the equinoxes

[14] Cf. especially Stein 1970, 1991; see also Friedman 1992 (chapter 3); and DiSalle 2002c.

Even more important from a strictly conceptual point of view is that this also gives us a good and empirically justified approximation to a true and absolute frame of reference. Remember that the first step of the argument for universal gravitation required that we had to assume that every local frame of reference used to describe the astronomical phenomena sufficiently approximates an absolute frame to use the mathematical models of the first book – mathematical models that strictly speaking *presuppose* absolute space and time. In this way, Newton is achieving something extraordinary: rather than starting out from absolute space and time, and then describing true motions and absolute accelerations, he starts from the observed (and thus relative) motions and accelerations and then infers which reference frame comes closest to defining truly absolute space.[15] Because he succeeds, he can then conclude that the phenomena that he started from were indeed close enough to the true motions and accelerations. But in doing this, he is ultimately reinterpreting their status: we measured the times with any reliable clocks that were at our disposal, but as a result of the incorporation of the phenomena in the theoretical framework, we must now conclude that these clocks indeed do not deviate too much from absolute inertial clocks and can be taken to measure true time (i.e. they are not merely reliable but also *valid*).

What can it mean to say that this procedure gives us an "*empirically* justified approximation to a true and absolute frame of reference"? How could we ever be able to justify this if we have no direct empirical access to absolute space and time? Well, simply: the laws of motion implicitly *define* what it means for a frame to be absolute, and the deductions from the third book establish that these conditions apparently hold for the frame defined by the centre of gravity of our solar system.

To state it this bluntly is of course to invite doubts. What about all the criticisms that have already been levelled against conventionalist philosophy of science? I would like to argue, on the basis of the foregoing description of the Newtonian style, that they are partly well-taken, and partly misdirected – at least when it comes to understanding Newtonian mechanics.[16] But before doing that, let us first take a small detour and introduce some elements of Kantian philosophy.

1.1.2 Constitutive and regulative principles

One philosopher who had an unusual sharp insight in the Newtonian style was Immanuel Kant, or so Michael Friedman has argued very convincingly.[17] Kant had of course already ventured into

[15] Newton apparently neglects the fact that his theory allows for an infinite class of what we would call "inertial" frames of reference, all moving with uniform speed with respect to each other. This possibility does not invalidate his procedure, but it importantly relativizes the claim that the sun is a stationary point in our universe.

[16] How far these lessons would extend to other sciences is a debatable point. I will point out some more general lessons that I think could be learned, but in general one should be cautious with transferring methodological insights from one specific science, which are unavoidably partly determined by its particular domain, to science in general (whatever that might be).

[17] Friedman 1992; see also Friedman 2003.

cosmology before entering upon the project of his critical philosophy, but this insight shows especially in his critical philosophy which after all was an attempt to reconcile metaphysics with the lessons to be learned from Newton's exact science. Friedman has shown in particular that we cannot adequately understand Kant's views on time as respectively a form of intuition and a formal intuition (i.e. as an object of intuition and not merely as its empty form) without taking into account the function of Newton's first law of motion as *first* picking out an unequivocal determination of time (an inertial motion allows us to derive the temporal metric from the spatial metric – which can be represented intuitively).[18] Now, this of course implies that we cannot think of the law of inertia as stating an empirical fact about already well-defined true motions. Such motions would have to be motions *in* absolute space and time, and the latter are sharply rejected as possible objects of experience by Kant – it simply makes no sense to speak of such an "infinite, self-subsistent nonentity"[19] as absolute space. It is the other way round: the law of inertia *defines* what it takes to be true motion. Yet if it is only such true motion that allows us to represent *pure* time, we must be able to *apply* it to empirical intuition to actually achieve *objective* time determinations. And as Friedman has argued in a revealing analysis, based on sometimes rather subtle indications, this is where Kant's deep insight in the Newtonian style shows.[20]

To be applicable the law of inertia requires that we are able to single out a privileged frame of reference. And this is what Newton has shown to be possible. Starting from purely relative motions, he constructs, through his deduction of the empirical law of gravitation, a reference frame that is (approximately) "inertial". Kant can even claim that this construction exemplifies his Postulates of Empirical Thought:[21] we start with phenomena that are merely *possible* motions (because only determined relatively) from which we then derive inverse square laws, hence claiming that they are *actual* motions (because otherwise the derivation of the inverse square law would make no sense), and in a last stage we finally determine the centre of mass of the solar system thus proving that the *necessary* condition for these motions to be truly actual is satisfied. In such a frame of reference the laws of motion cannot be false, exactly *because* they make possible the construction of the frame of reference in the first place. That is why they are called constitutive principles. (We cannot "test" their truth without supposing this very truth that is in question – the laws are explicitly stated to hold true only for motion with respect to absolute space and time; therefore they are what would make empirical tests possible in the first place.) Remember the function that was played by the models from the first book of the *Principia*. They give examples of how to recognize natural motion and, as a result,

[18] I won't delve into the magnificent intricacies of Kant's transcendental philosophy, but let me point out that this possibility of a formal intuition of time is really crucial.

[19] *Critique,* A39/B56.

[20] Cf. especially chapters 3 and 4 of Friedman 1992. In this subsection, I will merely be condensing Friedman's magisterial treatment to the points that are of primary interest for my further discussions.

[21] *Critique,* A218-219/B265-266.

absolute space and time.[22] Hence, if we succeed in finding instances of them in the empirical world, then we have empirically determined circumstances in which they necessarily hold true. In Kantian terminology: we have transformed appearances (*Erscheinungen*) in objective experience (*Erfahrung*).[23] The Newtonian style can thus be summed up in the following well-known Kantian slogan:

> The understanding does not draw its (a priori) laws from nature, but prescribes them to it.[24]

As we saw in our description of the Newtonian style, we don't simply recognize experience in the appearances without further ado. Prescribing laws to nature takes quite some work, both mathematically and empirically. Most importantly, it depends completely on the contingent nature of the appearances we started with. It could very well have turned out that it was not possible to construct an "inertial" frame on the basis of astronomical phenomena. That is, given the fact that we succeeded in constructing a suitable frame of reference, the laws of motion are necessarily true – but it is not necessary that we succeed. In that case they would simply have no objective reality.

This complication is reflected in Kant's philosophy in the crucial distinction between mathematical and dynamical principles of pure understanding (a fact apparently often neglected). The former are constitutive with respect to intuition (all possible figures that we can generate in our intuition satisfy Euclidean geometry), but the latter are only constitutive *with respect to experience* and regulative *with respect to intuition*. If we forget this fact, we are easily misled in transferring Kant's pronouncements on the status of geometrical space to the absolute space of Newtonian physics. Yet the former is only one element in the construction of the latter. That Newton's laws of motion are regulative with respect to intuition expresses the fact that *they state rules to seek out those elements presented to us in empirical intuition that enable us to constitute objective experience.*

The centre of gravity frame of the solar system is of course only an approximation to a truly inertial frame of reference. Kant, being a true cosmologist, immediately goes on to state that not even the centre of mass of the Milky Way galaxy would define the truly privileged frame of reference: only the common centre of gravity of all matter would suffice to that end.[25] Now as the determination of this frame of reference must stay beyond our reach, we can only construct ever better approximations – absolute space itself must remain an *idea of reason*. This idea of reason expresses the ideal situation in which all the laws of nature would hold exactly true, and consequently points towards nature (in its

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[22] Cf. also Newton's thought experiments on rotating bodies, often interpreted as attempts to prove the existence of absolute space and time; it has been argued convincingly by Howard Stein that the most sensible way to read what Newton is doing with them is "making something visible" (Stein 1970, p. 279), i.e. some of the *consequences* of his *definition* of true motion which allow one to recognize true rotations.

[23] "Experience is possible only through the representation of a necessary connection of perceptions." *Critique*, B 218.

[24] *Prolegomena*, p. 72.

[25] Cf. Friedman 1992, p. 143.

formal meaning) “as the totality of rules, under which all appearances must stand, if they are to be thought in an experience as connected”.[26] This idea of reason is what drives the program of successive approximations that we have seen to constitute such a prominent aspect of the Newtonian style.

1.1.3 Holism, conventionalism, and the a priori

Kant’s idea that the laws of motion are constitutive a priori principles makes good sense of the characteristics of Newton’s way of proceeding in the *Principia*. Yet it need not be recalled that Kantian philosophy generally fell in some kind of disrepute as the consequence of the development first of non-Euclidean geometries and then of Einstein’s theories of relativity. It seems undeniable that as the constitutive principles no longer can be held to be unique, their origin cannot simply lie in the constitution of the human understanding. But this implies that the question of their justification needs to be reopened, as it seems that their presence in any system of knowledge must go back to some kind of choice.

There have of course been numerous attempts to reinterpret the Kantian principles in the light of the development of mathematics and natural science since his days, and with the use of formal instruments that became available as a result of the development of modern logical systems. Yet I think that none of these have brought us a better understanding when it comes to making sense of the Newtonian style and the role played therein by the laws of motion. It is of course completely beyond the reach of this thesis to argue for this claim in detail. Still, in this and the next subsection I want to give a short sketch of some of the reasons for holding this view and of some of its consequences, as this positively guides the historiographical perspective that I will try to develop in sections 1.2 and 1.3.[27]

The question concerning the justification for Newton’s laws of motion has often been seen as a touchstone for philosophical interpretations of science exactly because of their elusive character. As already indicated in the previous subsection, it seems impossible to test their validity in any direct way. To test the law of inertia one first needs to be able to pick out an appropriate frame of reference. Clocks are calibrated by how well they measure inertial time, not the other way round. (We know the earth is not good enough a time-keeper *because* we know that momentum is lost, and that its angular

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[26] *Prolegomena* § 36. This is contrasted with “nature in its *material* meaning, namely according to intuition, as the totality of appearances”.

[27] Let me just point towards the increased interest in Neo-Kantian philosophy of science that seems to have arisen in the last decade of the twentieth century as a circumstantial indicator that such a position at least has been gaining credibility for some time. There is accordingly a growing literature which contains more detailed arguments. Especially interesting are the assessments of the logical positivist’s heritage in the twentieth century, interpreted from the angle of their particular ways of interpreting the Kantian idea of constitutive principles. See especially Richardson 1998; Friedman 1999, 2000; DiSalle 2002a,b. See also Friedman 2001, Richardson 2002 for more general programmatic statements. Cassirer 1953 [1910] remains an inspiring historical predecessor of this recent movement, and still contains many valuable discussions.

speed is thus not exactly constant; as Newton himself reminds us: "It is possible that there is no uniform motion by which time may have an exact measure."[28] Yet this does not render time inaccessible to us, exactly because we can *start from* the laws of motion – which even allow us to estimate the momentum that is lost due to tidal friction etc.) A similar conundrum holds for the concepts entering in the second and third laws. Consider the simple question: how would one measure the forces if not by the accelerations that they cause? (The apparent obvious idea of using a balance to measure forces is of no avail. Coming to see the fruitfulness of distinguishing between weight and mass is exactly one of the main insights that is encapsulated *in* Newton's laws.) Any possible test of the laws presupposes a model in which they are already supposed to hold true.

The most obvious answer to this problem is to claim that these laws are empirically tested through the role they play in the total system of physical laws of which they are part. The laws of motion may not be testable in isolation, but they are tested *through* e.g. the law of universal gravitation. Although the initial plausibility of this view is hard to deny, it does not really do justice to the *precise role* that these laws actually play. This can be nicely brought out by considering Pierre Duhem's views on the matter. While Duhem forcefully advocated the view that the laws of motion are being tested through the complex system of which they are a part, a consideration of his argument based on the foregoing description of the Newtonian style will show why we might want to resist such an interpretation.

Duhem famously argued that logic is not adequate to the task of scientific methodology. When an empirical test falsifies a theoretical prediction, it actually falsifies an elaborate conjunction of theoretical claims, but logic itself gives no indications at all about which claim to blame. Duhem concluded that we never test a single theoretical claim and, as a result, that the situation of the Newtonian laws of motion is not so special. Now, it is of course one thing to notice that logic gives no indications on how to distribute blame, but it is another thing to conclude that, as a result, there are no more fine-grained distinctions to be made that come into play during theory testing. The main upshot of Duhem's discussion has generally been taken that we can always save a theoretical claim in the face of contradictory evidence by blaming auxiliary hypotheses – but what does it mean to "save" a theory? At face value, that it can be squared with the evidence; but this only raises the further question: where does this evidence come from?

As we have seen in our discussion of the Newtonian style, evidence for the truth of the universal law of gravitation is generated by comparing the models of the first book with the empirical phenomena. Now, as Duhem reminded us, any empirical deviation from the theoretically predicted orbits can always be ascribed to a number of factors: the falsity of this empirical law, the presence of as yet unidentified systematic disturbances, observational errors. Let us focus on the first two

[28] *Principia,* p. 410.

options.[29] As explained, Newton's evidentiary strategy (as analyzed by George Smith) consists in first holding on to the truth of the law in ideal circumstances, and then attempting to turn deviations in second order phenomena. This provides a first direction in which to overcome Duhem's challenge: *from the perspective of ongoing research* there is a clear epistemic advantage in first trying to find out if the deviations cannot be ascribed to systematic disturbances. This is not so much a good strategy because it allows one to "save" the theory, but rather because it allows one to generate *further* evidence. Duhem famously claimed that a theory only serves as a classification of empirical phenomena, but he apparently forgot that many of these phenomena exist only by the grace of theories. So even if saving the phenomena is the only goal of theories, it does not necessarily follow that any way of saving the phenomena is as good as another.[30] This doesn't imply that the law of gravitation is not being put to test, as there is no guarantee that this program of turning deviations into disturbances, and thus in higher order phenomena, will succeed – many constraints have to be satisfied for this to work out.

Duhem was of course perfectly aware of the fact that physical investigations are often structured around such programs of looking for successive approximations, but he ascribed this fact to nothing but the desire not to overthrow an already established framework.[31] Now, this is of course quaint to claim in the case of Newton, who was for the first time building such a framework. Most importantly, it also misses the important *epistemic* role played by this kind of research program.

This is not all, however, as we have up to now been talking mainly about the law of gravitation. Let us now take a step back and inquire further into this possibility of generating evidence. Both the first and higher order phenomena can only become evidence for any kind of claim (whether this is the law of universal gravitation or any other force law) because of the prior existence of the mathematical models of the first book of the *Principia*. But as we have seen, these models are explicitly based on the presumed validity of the laws of motion – without them no kinematic phenomena could ever serve as evidence for claims about the forces generating them. There would simply be no way that empirically determined parameters could be interpreted as measuring theoretical parameters.

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[29] The third option is of course always a possible source of error, to which Duhem rightly drew attention. Duhem's discussions bring many important factors to the fore, which indeed show that there always is a leeway in the interpretation of empirical tests, and I would not like to underrate the importance of this simple point. All I want to question is the *general* methodological lesson that Duhem tried to draw from this point.

[30] A side remark: this is also the reason why Bas van Fraassen's (1980) brand of empiricism, constructive empiricism, has proven so invincible over the past two decennia. His careful statement of his position exactly allows for this forward-looking dimension of scientific methodology, often thought to be only comprehensible from a realist perspective, without surrendering the claim that saving the phenomena is the only true epistemic goal of theories. The crux of course lies in the fact that this forward looking dimension in the end is also directed towards saving "the" phenomena, albeit towards a much more fine-grained plethora of phenomena.

[31] Duhem p. 211.

Let us return for a moment to the deviations that were turned into disturbances by Newton's evidential reasoning in the third book of the *Principia*. It is important to realize that when he (or for that matter, any other cosmologist coming after him) was confronted with deviations from the predicted phenomena, he did not simply inquire into *whatever* causes might lie behind this deviation, but that he asked what further *forces* were responsible. That is, the criterion for deciding whether the deviations can be turned into physically meaningful disturbances is to ask whether they can be fitted into the framework defined by the three laws of motion (which need not always imply that they can be explicitly described within this framework – although the latter fact would seriously diminish their evidentiary value).

We are getting to the heart of the matter. It would be wrong-headed to claim that Newton's laws of motion are being put to the test through the gathering of empirical evidence for or against the law of universal gravitation because such a picture neglects the fact that this gathering is only made possible by these laws. Consider what would happen if no evidence had been generated for the law of universal gravitation, nor for any other force law capable of explaining the astronomical phenomena: would that imply that the laws of motion stand falsified?

No – it would imply that these phenomena cannot be given a "mechanical" explanation (in the sense that the term takes with Newton); either because they are the result of too complex an interplay of diverse forces that could not be isolated by any (mathematical and observational) means at our disposal, or because they are simply no mechanical phenomena. To decide between these two options would be no easy matter, especially as we cannot try to experimentally isolate some of these complex forces. (Newton was lucky enough to have found nature isolating the effects of the forces sufficiently in the case of our solar system).

To put it in Kantian terms: the laws of motion would have no objective reality, but this is something else than claiming that they stand falsified. They are impotent rather than false.

Something like the foregoing scenario is exactly the fate that befell the second book of the *Principia* where Newton tried his hand at developing a mechanical theory of resistance forces in a medium.[32] The phenomena proved to be intractable, but the laws of motion were not at all put into doubt because of that. It only required much more experimental and theoretical work (that is still going on) before this complex phenomenon could be more adequately modelled mechanically. (This modelling is obviously much messier than the neat case of the planetary motions. The latter case actually presents some kind of ideal type for mechanical explanation and evidence generation. The case of resistance forces would rather be grist to the mill of the Cartwright school of philosophers of science that stresses the insufficiency of theoretical principles in modelling almost any real-life empirical phenomenon.[33] I agree, but this does not invalidate any of the points made here. All of the

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[32] See especially Smith 2001.

[33] Cartwright 1999; Morgan & Morisson 1999.

physical cases studied by the Cartwright-ites still presuppose the validity of an abstract and theoretical frame such as the one constituted by Newton's laws of motion, exactly as happened with all subsequent attempts to model resistance in a medium. This is for good reason, for in all of these cases it would make no sense *to measure forces* in their absence.)

Taking these insights together we can see a multi-layered picture of the logic behind theory-testing and of the peculiar role played therein by laws such as Newton's laws of motion. These constitutive principles first make it possible to interpret the data with an eye to finding evidence for any possible empirical force law. In the course of this process it is often a good research strategy to hold any such serious candidate for a force law temporarily fixed, to try to isolate further empirical factors. In this way these empirical laws can temporarily play a role similar to the constitutive principles, i.e. they allow for the generation of evidence. Yet, I think it is important to distinguish between both cases. In the one case we define the domain of research, in the other case we are only trying to find means to get on with the research. To put it slightly more metaphorically: the constitutive principles first open up a space of possibilities, whereas working on the presupposition that an empirical law holds true comes down to (temporarily) carving out a subset of these possibilities. The notion of presupposition doesn't seem to be strong enough to capture the function of constitutive principles (although the latter of course act as a *kind* of presuppositions.)[34]

To dispel any lingering suspicions: the essential difference between these constitutive principles and empirical laws lies in the function they play within scientific research, not in their un/revisability. That is, they are necessary presuppositions, but it is not necessary that *they* are the presuppositions. Both constitutive principles and empirical laws such as the law of gravitation can be subject to revisions, but the effect will be profoundly different. (Remember all the talk about scientific revolutions.) But this is a distinction that a holist is in no position of making. When Quine, e.g., is commenting on the difference between core and periphery in his famous image of the network of belief, we can find him stating that the difference is solely constituted by "the relative likelihood, in practice, of our choosing one statement rather than another for revision in the event of recalcitrant

[34] Further confirmation for this kind of picture can be found in the works of the structuralist school in philosophy of science. While they explicitly present their work as descriptive in nature, i.e. they use the set-theoretic apparatus to give a description of the structure of scientific theories, the resulting multi-layered picture comes very close to the picture sketched here in many respects. I won't spell this out in detail, but invite the reader to take a look at some of the reconstructions presented in Balzer et al. 1987. Particularly worth mentioning is their stress on the fact that a theory doesn't have one big application, but that application is piece-meal extended through the construction of the appropriate models instantiating special force laws. Yet these models of different local applications are "tied up" through imposing constraints that single out admissible combinations of what they call potential models. One example would be that the mass of the moon should have the same value within different models. (Remember that it was seen that the imposition of constraints was one of the main features of the Newtonian style.)

experience."[35] Yet he can provide no *grounds* for this general practice, let alone that he can make room for finer distinctions than those in terms of revisability.

But what about decisions to revise the constitutive principles of a theory? Can we do any more than merely state that they are convention*s*; that we define natural motion to have such-and-such characteristics for no other reason than that otherwise we could not get our research off the ground; and that in the presence of enduringly recalcitrant phenomena we just have to make another, more convenient choice?

In trying to answer this question, it is useful to go back for a moment to the father of modern conventionalism, Henri Poincaré. As has become clear from the careful reconstructions by Michael Friedman and Robert DiSalle, his position differed significantly from the conventionalism that was later propagated by logical empiricists such as Schlick (but which they still ascribed to him).[36] Essential to Poincaré's position was his picture of the hierarchy of sciences.[37] Arithmetic was on top of this hierarchy, and its basic principles were synthetic a priori in the exact Kantian sense (because based on iteration, which cannot be proved empirically but only grounded in our inner intuition of time). Next came first the theory of mathematical magnitude (the system of real numbers), and then geometry, which both were partly determinable a priori, partly based on convention. Depending on the conventional choice for one of the possible geometries of space, one could then pick out the most convenient physical principles. These in turn finally allow us to formulate empirical physical laws (e.g. particular force laws) that are directly confronted with nature. Now what is most interesting about this picture is that the conventional choices are embedded within a larger framework of constitutive principles.

Consider the crucial case of geometry. Poincaré developed a subtle group-theoretic argument, based on the prior work of Helmholtz, to show that *given* an established set of empirical judgements on what are properly spatial structures, this set can be shown to have a specific group-theoretical structure that delineates the possible geometries of space, which according to Poincaré's argument must have a constant curvature. But as has been stressed by DiSalle, this implies that the conventional choice merely picks out one of the options which already have a well-determined *physical content*. That is, the convention is not responsible for the specific interpretation of what it means to be spatial; it merely fixes the leeway that exists within this interpretation. (That they have physical content does not imply that they are empirical claims; that would be to forget their constitutive character. They are not empirical claims because they first give meaning to the idea that space would have a geometrical character – "we would not recognize as spatial displacements any changes that did not conform to that structure".[38])

[35] Quine 1951, p. 40. Similar statements can be easily found in Duhem's writings..

[36] Friedman 1999 (chapter 4); DiSalle 2002a.

[37] Cf. especially Poincaré 1968 [1902].

[38] DiSalle 2002a, p. 180.

It is of course well-known that the development of the general theory of relativity invalidated Poincaré's argument.[39] But I think we can follow DiSalle in drawing a general lesson from his attempt. The major difference between Poincaré's conventionalism and that of the logical positivists is that the former is much more restricted, and still infuses any theory with a specific physical content, whereas for the logical positivists it is precisely the physical content that becomes the conventional part.[40] From the former perspective it does make a difference which are the constitutive principles of a theory. And this seems exactly right for the case of the Newtonian theory. The three laws of motion do seem to express *something* about motion and force, even if they cannot be subject to any kind of straightforward tests. It is not a merely conventional decision to claim that natural motion has certain characteristics. The constitutive principles seem to be intricately interwoven with their domain of application in a way that pure conventions are not.

It is no accident that we can find this view expressed in the writings of Poincaré himself, when he states:

> La loi de l'accélération, la règle de la composition des forces ne sont-elles donc que des conventions arbitraires? Conventions, oui; arbitraires, non; elles le seraient si on perdait de vue les expériences qui ont conduit les fondateurs de la science à les adopter, et qui, si imparfaites qu'elles soient, suffisent pour les justifier. Il est bon que, de temps en temps, on ramène notre attention sur l'origine expérimentale de ces conventions.[41]

This is exactly what I will try to do in the next subsection. I will try to unravel some of the empirical grounds that led Newton to adopt his tree laws of motion as the axiomatic basis for his theory (as constitutive principles, to put it in a language unknown to him). This will then set the stage for introducing the central set of questions that lies at the basis of the present thesis.

1.1.4 The empirical grounding of Newton's principles

My presentation of Newton's laws of motion as constitutive principles incapable of direct empirical verification might be met with justified doubts, as Newton himself stated in a scholium immediately following the introduction of his laws: "The principles I have set forth are accepted by mathematicians and confirmed by experiments of many kinds."[42] If we keep in mind the concluding remarks from the previous subsection, however, it is clear in what direction I think we should interpret this statement. When we take a closer look at these "experiments of many kinds", it becomes clear that

[39] But see the attempts of Herman Weyl to resuscitate the argument for the case of variable curvature by restricting it to local displacements. For an interesting assessment of Poincaré's argument in face of general relativity, see DiSalle 2002a.

[40] DiSalle 2002a, p. 175.

[41] Poincaré 1968 [1902], p. 128.

[42] *Principia*, p. 424.

they do not so much give a direct empirical proof of the laws of motion but that they rather show that these laws indeed represent something basic about "our" understanding of the adduced phenomena. (The importance of the quotation marks will become clear as we go along.)

The "experimental confirmation" of the first two laws is extremely laconic, especially in the first two editions, where it is simply stated that "by means of the first two laws and the first two corollaries Galileo found that the descent of heavy bodies is in the squared ratio of the time and that the motion of projectiles occurs in a parabola, as experiment confirms, except insofar as these motions are somewhat retarded by the resistance of the air."[43] The third edition expands a little bit on this by explaining how the uniform force of gravitation causes these phenomena by the second law and the composition of inertial and accelerated motion. But of course, notwithstanding Newton's claim, we must not forget that a direct confirmation would only be possible on the supposition that we have independent means to *measure* force other than by acceleration. Newton's explicit definition ("impressed force is the action exerted on a body to change its state either of resting or of moving uniformly straight forward"[44]) is obviously of no help. As already explained in the previous section, it seems that there is simply no way to check whether the conditions stated in the laws of motion hold independently from these laws themselves.

I would suggest that a better way to look at the matter is to say that Newton accepts the empirical phenomena so judiciously selected by Galileo (accelerations are as times squared, and projectiles follow parabolic paths) because *they allow him to introduce a coherent measure of force*. He reinterprets these empirical facts by uncovering the possibility of introducing constitutive principles which allow him to connect the phenomena to a mathematical dynamical framework.[45] Now, that something is a coherent measure is of course not a matter of simple intuition, nor a purely mathematical fact. It also requires that this measure squares with the ways in which mathematicians already understood some of the characteristics of motion and the forces of nature.

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43 *Prinicipia*, p. 424.

44 *Principia*, p. 405.

45 A fascinating story is to be told, and no doubt many aspects of it have already been told, about how Newton gradually came to see the possibility of this reinterpretation during the years 1684 (the *De motu* manuscript) up till 1687 (the publication of the *Principia*) (cf. e.g. chapter 1 of De Gandt 1995; and chapters 7 and 8 of Westfall 1971). He started out *without* the second law, but he did decide to measure the first instance of the effect of a centripetal force by a generalized version of Galileo's empirical law (this is hypothesis 4 in *De motu*). (Why do I call it an empirical law, if the times squared relation – which is all that is used by Newton – is actually a purely kinematical consequence of uniform acceleration? Because the fact that the force of weight, assumed to be constant, gives rise to a uniform acceleration, is an empirical claim for Galileo.) By detaching it from its limited context and seeing under what circumstances it can be applied as a general measure, Newton is actually transforming it into a constitutive principle. (Compare this with the way in which the empirically established result that the speed of light is invariant is reinterpreted by Einstein to become a constitutive principle; cf. DiSalle 2002a,b, which provides a main inspiration for this way of putting the problem.) If I were able to reconstruct this process in more detail (which I am not for the moment) then the status of Galileo's law as a confirmation of the second law of motion would become still much clearer. I submit that it would turn out to strengthen the interpretation I am offering in this section.

This shows itself clearly in the fact that the parabolic path of projectiles can only be taken as "confirming", or suggesting, the laws of motion if we *presuppose* the possibility of a dissection into simultaneously acting components, and it is well-known that exactly this was denied in sixteenth century Aristotelian philosophy. Newton, however, sees the possibility of this decomposition as a direct *consequence* of the validity of his laws of motion. We can thus see how his laws are meant to express some of the essential presuppositions of the new anti-Aristotelian ways of explaining natural phenomena. He can then adduce these phenomena as a confirmation of *this fact*. Notice that Newton is especially keen on stressing that his principles are "accepted by mathematicians".[46]

Now, this decomposition will only be physically meaningful if it has an intelligible structure; that is, if we can assign an independent physical interpretation to the simultaneously acting components. So the coherence of Newton's proposed measure of force also rests on the fact that it can do justice to *a prior grasp of the proposed domain* of his theory. This is obviously true for the first law (which after all was stated by Descartes and others, and could be ascribed to Galileo[47]). What Galileo's phenomena then show is that we can indeed assign a force that is responsible for the change in motion of bodies. We *can* "assign a force" because the force of gravitation (the weight of a body) is an incontestable instance of a force of nature, and because we know how to shield off (or at least account for) other forces, such as friction. This becomes clear in Newton's discussions where he pays careful attention to ensure that one can practically establish the conditions in which the laws of motion do show themselves most clearly (in these cases actually the third law, to which I shall pay more attention in a moment – all that matters for the moment is that these techniques are always directed towards isolating the inertial component of motion): by correcting the results of experiments for the resistance of air through an extra set of control experiments, by placing the bodies on flat water which is as polished a surface as we can find, …[48] Both this identification of the force of gravitation and the techniques for isolating the inertial component of motion were clearly part of the common practice of most of the seventeenth century natural philosophers. They did not yet measure forces by the proportion of acceleration to mass, but they were explicitly engaged (or it was at least plausible for Newton to assume that they were) in attempts to trace any change in motion to *independently ascertainable or accountable* forces. (Ascertainable: like the force of gravitaton; accountable: like the force of friction which we know how to account for in adding up – or subtracting – all that enters into the complete empirical phenomenon.) It was moreover already implicit in many of these attempts that the deviation from an inertial path then provides some kind of measure for the force exerted (as in

[46] Cf. also Gabbey 1980, p. 285: "[in the second law,] the *vis impressa* is given a directional specification which is tailor-made for direct application in the composition and resolution of forces, and therefore in the task of explaining directional changes in the motion of bodies in terms of the forces acting on them."

[47] Cf. chapter 6, section 6.1.5, and chapter 8, section 8.3.3.

[48] This care on Newton's part is stressed by Ludwig 1992.

Galileo's *Dialogo* discussion of the extruding power of the rotation of the earth, as well as in some of Huygens' work).[49]

The empirical examples introduced as confirmation of the third law at first sight look more convincing as straightforward tests of this law than did the references to the Galilean phenomena. Newton quotes the work of Wren, Wallis, and Huygens on the impact of two bodies, and offers further empirical proof by considering the impact of two bodies both suspended from a thread. But, again, we are of course not empirically proving that action equals reaction, because action and reaction are defined as … that which is conserved in impact. Alan Gabbey has offered a convincing reconstruction of Newton's path to his *Principia* definitions of inherent and impressed force as the outcome of a critique of Descartes' notions of force.[50] It is in the course of this development that Newton in all probability realized that something like what eventually became his third law would allow him to introduce a coherent measure for the different forces involved in impact phenomena. This insight is reflected in an extremely revealing passage from Newton's own comment on his definition of "inherent force of matter", or "force of inertia":

> A body exerts this force only during a change of its state, caused by another force impressed on it, and *this exercise of force is, depending on the viewpoint, both resistance and impetus*: resistance insofar as the body, in order to maintain its state, strives against the impressed force, and impetus insofar as the same body, yielding only with difficulty to the force of a resisting obstacle, endeavors to change the state of that obstacle.[51]

What is a change in inertial motion for one body in a collision is an impressed force for the other. That is, the second and third laws are both intrinsically related with what Newton calls the force of inertia; in the Opticks he even speaks about the force of inertia as "a passive Principle by which Bodies persist in their Motion or Rest, receive Motion in proportion to the Force impressing it, and resist as much as they are resisted."[52] The three laws *taken together* express this one passive principle, which is an essential quality of all bodies. Newton introduced his third law to ensure that the measure of force remains invariant under the different perspectives from which collision phenomena can be considered. By imposing this law as an axiom he can consider all interactions as involving equilibrium between impressed and inherent (resisting) forces – by definition. In other words: Newton "discovered" the action-reaction law in analyzing collision phenomena *because* he was actively trying to construct a set

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[49] Cf. Herivel 1965, p. 40, for an early use of this principle by Newton; see also De Gandt 1995, pp. 11, 17, who recounts how Newton's earliest attempts to treat planetary motion as the result of centripetal forces in the manuscript *De motu* also used this measure implicitly, without introducing an explicit axiom justifying this use (cf. ibid., p. 19ff).

[50] Gabbey 1980a.

[51] *Principia*, p. 404 (my emphases).

[52] *Opticks,* p. 397.

of concepts which would be conserved. It is of course a contingent matter of fact that this was possible – this is the restricted empirical import of the law.

It is only this search for the "passive Principle" that brings into focus the intimate relationship between a body's inertia and the phenomenon of impact. What is important for our discussion here is that such was already the direction in which earlier writers were trying to analyze the phenomenon. Descartes did so explicitly, and as mentioned probably provided the starting point for Newton's analysis of the problem. Huygens tackled the problem from another perspective: he did not explicitly analyze the forces involved, but instead cleverly exploited the Galilean relativity of motion to establish the impact rules.[53] But again, this implies that the clue to understanding the phenomenon was sought in its relation to inertial motion, as Huygens' device actually consists in imposing the constraint that the common centre of gravity of the two colliding bodies remains invariant in its inertial state. What Newton's analysis then adds is a dynamical understanding of this property, which again becomes a *consequence* of his laws of motion.

We have seen in the previous sections how the laws of motion, as constitutive principles, first made it possible to generate evidence for theoretical claims concerning the forces of nature. It was also claimed that, although not directly testable, they are not fruitfully thought of as nothing but mere conventions. Another way to think of this situation is by recalling the often contested Kantian idea that there can (and must) be synthetic a priori principles.[54] The present discussion is aimed at making plausible that we can lay bare part of the grounds for this synthetic nature of the Newtonian laws of nature. Rather than searching these in the constitution of the human mind, they are to be sought in human practices.

Embedded in the practice of seventeenth century natural philosophers and mathematicians was a particular way of selecting and analyzing appearances. Their apparently successful mathematical description of some natural phenomena (essentially limited to fall, projection, and percussion) was certainly not based on any kind of direct observation, but rather achieved by an *actively directed search process*. (Cf. the care that needs to be taken to correct for the effects of air friction *before* one can see the pure phenomenon.) Now, it is clear that this search process is regulated by the idea that "inertial motion" (or what could be interpreted as such with hindsight) is somehow the natural motion of a free particle, and that the only facts that stand in need of explanation are deviations from this motion.[55] This is the core of the prior cognitive grasp of the domain of mechanics that I referred to earlier in this section. Of course, the laws of motion do more than reflect this grasp; they add a further level of intelligibility to these phenomena by providing precise relationships between force, acceleration, and the new concept of mass. (Consider the phenomenon of free fall: why would *all*

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[53] For Huygens, see chapter 4 of Westfall 1971; Gabbey 1980b; and, for a slightly different interpretation, Vilain 1993.

[54] This is of course what the logical positivists denied, and why they were so taken by the idea of conventions: all a priori elements of a theory are supposed to be purely analytic.

[55] As we will see in the rest of this thesis, this involves a crucial redefinition of what it takes for a motion to be "natural."

bodies fall with the same acceleration when their weights can differ greatly?) But this does imply that they are still *grounded in these carefully selected phenomena.* (Compare with what was said a moment ago about the restricted empirical import of the third law of motion.)[56]

To put it in Kantian terms: the examples adduced by Newton serve to make plausible that the laws of motion, as constitutive principles, have objective reality (find application in the empirical world; can be used to transform appearances in experience).

The mathematical models from the first book in the *Principia* allowed Newton to discern relevant facts about the structure of the astronomical phenomena because they exploit a prior understanding, which has already been shaped by close interaction with forces of nature and their effects on the motion of bodies. It makes sense to try to isolate a "pure" phenomenon solely due to one force acting on a freely moving particle, exactly because it has already been proven that this is possible *in principle* provided one is dealing with the right kind of phenomena. And the laws of motion are a good instrument to effect this isolation, because they provide these earlier efforts with a completely intelligible structure. They show with hindsight why these earlier attempts at a mathematical description of natural phenomena could be partially successful. The *Principia* is then a persistent and successful attempt to transpose these conditions of (partial) success to the new domain of astronomical phenomena, following the methodological maxim expressed in Newton's third rule of philosophizing: "those qualities of bodies that cannot be intended and remitted [i.e., qualities that cannot be increased and diminished] and that belong to all bodies on which experiments can be made should be taken as qualities on all bodies universally."[57]

(To put it this way of course involves a gross oversimplification from a historical perspective. Newton first explicitly formulated his laws in the attempt to treat astronomical phenomena; i.e. in trying to deal with centripetal force laws, and in extending this treatment to a more complex system such as our solar system. But he formulated these laws because he was aware that such treatment *was only possible under a specific set of conditions*. And in trying to assess as precisely as possible which could be these conditions, he could not but exploit the prior understanding of earthly phenomena of motion as was present in the work of his predecessors and in his own youthful work.)

One important complication arises, however, in trying to extend mechanical explanations to the sphere of astronomical phenomena. It was claimed that the empirical examples could be seen as making plausible that the laws of motion have objective reality. Yet we can ensure this objectivity only if we are in a position to ascertain whether any acceleration is indeed due to a force; that is, if we can distinguish true motion from merely relative motion so as to exclude that we would be dealing with (in modern parlance) pseudo-forces. This becomes especially relevant when trying to deal with

[56] I.e.: "Newton "discovered" the action-reaction law in analyzing collision phenomena *because* he was actively trying to construct a set of concepts which would be conserved. It is of course a contingent matter of fact that this was possible – this is the restricted empirical import of the law."

[57] *Principia*, p. 795.

astronomical phenomena where it is notoriously difficult to decide which objects are undergoing a truly rotational motion, a fact which of course exercised all seventeenth century natural philosophers up to and including Newton (not to mention the church men).

At this point we are confronted with Newton's true genius. By introducing the third law among the constitutive principles of his theory, he actually offers a criterion to distinguish free particles. If every action has a necessary reaction-counterpart, then we know that a particle is acted upon by checking whether another particle is suffering an equal but opposite reaction. (How do we know that a body's weight is a force? Because we measure it through a reaction force.) And taken together, Newton's three laws give us a conceptual basis for recognizing dynamically closed systems in nature. (A closed system being a system of particles not acted upon by forces originating from outside the system.) This comes out most clearly in the fourth corollary to these laws: "The common centre of gravity of two or more bodies does not change its state whether of motion or of rest as a result of the actions of the bodies upon one another; and therefore the common centre of gravity of all bodies acting upon one another (*excluding external actions and impediments*) either is at rest or moves uniformly straight forward."[58] That is, there is always an appropriate reference system with respect to which to describe the motions of the bodies: the one associated with their common centre of gravity. As we saw in section 1.1.1, this insight contains the crux of Newton's ingenious unravelling of the true constitution of our solar system.

The third law was primarily grounded in collision phenomena, whereas the application to the solar system of course involves attractive centripetal forces.[59] In citing evidence for this broader validity of his law, Newton introduces a thought experiment which implies that a violation of the third law for attracting bodies would result in a violation of the first law, a thought experiment which he can moreover back up by an experiment involving a lodestone and iron attracting each other while placed on water: the fact that they remain in equilibrium once they touch is taken to show that they "sustain their mutual endeavors toward each other"[60]; if one of their endeavors had prevailed, they would have formed one body that would have gone off "indefinitely with a motion that is always accelerated, which is absurd and contrary to the first law of motion".[61] We again see how the three laws are intimately related and taken together express the essential passivity of all matter. The third law is what always allows one to see an inertial system in the midst of dynamical interactions, and with respect to

[58] *Principia*, p. 421 (my emphases).

[59] As already noticed in section 1.1.1, this application involves the potentially contentious claim that the third law holds for the gravitational force between any pair of satellite and central body (hence excluding mediating mechanisms such as provided by an aether). See Stein 1991; Harper 2002b for much more detailed discussions.

[60] *Principia*, p. 428.

[61] Ibid.

this system there is essentially equilibrium (not static equilibrium, but equilibrium between impressed and inertial forces)[62].

But, as was already stressed by Kant, this equilibrium must remain an *idea of reason*. (It expresses the ideal situation in which all the laws of nature would hold exactly true, and consequently points towards nature (in its formal meaning) "as the totality of rules, under which all appearances must stand, if they are to be thought in an experience as connected".[63])

The three laws as principles that are constitutive with respect to experience give one the guidelines along which to try to achieve this idea of reason, whatever the forces of nature turn out to be. And at the end of his discussion of the empirical confirmation of his laws of motion, Newton gives away the background to this idea of reason. At that place he gives a crash course in mechanics in its traditional meaning, the theory of machines, claiming that this shows the "wide range and the certainty of the third law of motion".[64] Now, this involves a bit of a stretch on Newton's part, as the action-reaction principle as it was illustrated through collision involving two bodies, whereas the working of machines is always dependent on three bodies, with the third body, the machine itself, acting as the reacting force for the "force" and the "resistance", which strictly speaking don't make up an action-reaction pair. But of course, Newton is not so much interested in the fine-grained details of the example, as that he is eager to draw a central lesson: "if the action of an agent is reckoned by its force and velocity jointly, and if, similarly, the reaction of a resistant is reckoned jointly by the velocities of its individual parts and the forces of resistance arising from their friction, cohesion, weight, and acceleration, the action and reaction will always be equal to each other in all examples of using devices or machines."[65]

All earlier treatments of the mechanical machines were grounded in the conservation of a theoretical quantity of moment in situations of equilibrium. (An agent can *sustain* a resistance if their respective moments are equal.)[66] Newton's important advance is that he can extend this to *all* situations: notice the presence of "acceleration" among the factors entering into the force of the resistant. Driving the whole Newtonian research program is the insight in the passivity of all matter, which allows him to search for equilibrium even in dynamical situations. But this search is grounded in an idea of nature as an essentially closed system. Exactly because he can let go off the idea of picturable mechanisms, Newton is capable of achieving a picture of nature as a giant machine, which is nothing more than a device for redistributing a theoretical quantity amongst its parts. Notwithstanding his introduction of forces of attraction not mediated by particular mechanisms,

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[62] See especially Westfall 1971, p. 470.

[63] *Prolegomena* § 36.

[64] *Principia*, p. 430.

[65] Ibid.

[66] Much more on this follows in chapter 5, where I will discuss Guidobaldo's and Galileo's conceptualization of the mechanical machines.

Newton could have been able to claim that he was the true heir of the attempts at mechanical explanations of the phenomena of nature. His intervention consists in having seen (with hindsight) what was truly essential to these attempts.

Now, does this long analysis of Newton's introduction to his *Principia* answer the query about the justification for the laws of motion? It is clear that it doesn't. It only pushes back the question one level back.

It is plausible to conclude on basis of the foregoing that Newton could make a strong point in claiming that we would not recognize as caused by forces any phenomena of motion that did not conform to his three laws. That is, we could interpret his achievement as being a transcendental deduction, more or less along the lines of Poincaré's establishment of the possible geometries of space. But for whom would this transcendental deduction have binding force? Only for someone already engaged in the kind of practice that Newton takes as his starting point. That is, we must put the "we" between quotation marks.

1.2 The relative a priori and archaeology

1.2.1 The relative a priori

The main tenet of a broadly understood neo-Kantian philosophy is that we can perfectly make sense of the constitutive role of certain principles within a system of knowledge without having to claim that they are fixed and absolutely universal. This is expressed in the catchphrase of the "relative a priori." Yet while this may open up prospects for retaining attractive features of the Kantian project without having to founder on its all too absolutist longings, such an undertaking is not without its own problems. As should have already become clear from the foregoing discussions, the fact that the constitutive principles are revisable does not (and cannot) imply that the dynamics behind their revisability answers to the same logic as that of general empirical claims; but the alternative is not directly clear – to put it with the title of Michael Friedman's Kant lectures: what is the "dynamics of reason"?[67] It is moreover clear that this relative character threatens to compromise one of the main tenets of the Kantian philosophy: the fact that the constitutive principles were taken to be defining for human understanding provided a strong foothold for grounding the rationality of objective knowledge.

Section 1.1.4 contained a kind of an applied example of this general philosophical problem. How can we understand the Newtonian constitutive principles in their historicity without abandoning their transcendental role? We must be careful not to misconstrue the import of this "historicity". It is clear that for Kant the Newtonian laws also had a history; i.e. they were first discovered at a certain point in time, by Isaac Newton, Lucasian professor of mathematics at Cambridge. But the important

[67] Friedman 2001.

thing is that for him they had no other possible history, aside from the fact that someone else could have discovered them at some other place and some other time. That is, this history is the history of recovering something that could not have been otherwise – its discovery is in a sense self-explanatory given the constitution of the human understanding. But if we really want to take serious the idea of the relative a priori, it can never be a simple question of equating historical origins with transcendental ones: a relativized Kantian perspective directs us towards the latter. How can (mathematical) principles at a certain point of time assume transcendental force; i.e. how can something *become* constitutive?

The provisional answer that I suggested was to anchor these principles in a practice of searching for (and tentatively giving of) mathematical and mechanical explanations. The main advantage is that this allows us to introduce a truly historical perspective. Whereas constitutive principles somehow seem to withdraw themselves from history, human practices are through and through historical. What happens through the introduction of constitutive principles is that a certain perspective on this practice becomes codified and thus opens up the possibility of "starting" a more or less a-historical tradition. (Once one is working within the framework defined by the Newtonian principles, historical time drops out. It is immaterial whether results were achieved in the seventeenth, eighteenth or twenty-first century; what counts is their place within this ideal tradition. This implies that the start immediately looses its character as an origin.)[68] I am of course exaggerating. That's why I spoke of "more or less". There are continually historical punctures within this ideal tradition. Consider the important problem of continuum mechanics: Newton's laws as formulated in the *Principia* seem primarily suited for point particles; but arguably, the majority of interesting empirical problems seem to demand a continuum perspective.[69] The historical consideration of this kind of problems forced scientists to try to develop tools to bridge this gap – tools which were missing in the a-historical ideal world where everything was already well-defined, although not yet formulated. This is after all one of these points were the constitutive principles are subject to revision; in this case, admittedly, of an apparently primarily technical nature. (Although one would be well-advised not to underestimate the conceptual consequences of this revision.) But what's important is that after an initial period of uncertainty and tentative proposals, the tradition reasserts itself as ideal and a-historical; a process which typically implies the recuperation of the prior tradition within the new perspective.

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[68] This specific a-temporality of ideal physical theories has been stressed at numerous places by Gaston Bachelard. Cf. "le rationalisme est une philosophie qui *continue*; il n'est jamais vraiment une philosophie qui *commence*." Bachelard 2004 [1949], p. 54 (cf. also pp. 122-123). Earlier in the same book he had already introduced the concept of a *rational memory*: "Cette mémoire de la raison, mémoire des idées coordonnées, obéit à de tout autres lois psychologiques que la *mémoire empirique*. Les idées mis en ordre, les idées réordonnées et coordonnées dans le temps logique, déterminent une véritable émergence de la mémoire." Ibid., p. 2.

[69] Cf. especially Truesdell 1968; see also Wilson 2000 for an intuitive introduction to some of the problems that arise within this context.

Yet it is clear, as already indicated, that in itself this does not solve the problem of the transcendental origin, but rather pushes it one level back: wherein was this practice grounded? And this is no trivial question. The practice of explaining phenomena and the empirical principles involved therein was already guided by a specific (if still controversial and often hesitant) way of proceeding. Newton was in a position to claim that this was due to an implicit use of his laws of motion, but this is of course an instance of how a historical practice is turned into an a-historical tradition. The question that *we* must ask ourselves, as critical investigators of the historical relative character of constitutive principles, is the following: to solve *which kind of problems* were these principles already "implicitly" used? How did one distinguish potential solutions from misguided ones – why could one think that something like inertial motion was part of the solution? How was the prior grasp of physical problems grounded before experience (in the Kantian sense) became organized by a set of tightly knit constitutive principles? In short: *what made it possible to formulate these Newtonian conditions of possibility?*

If we want to understand the normative force that accrues to Newton's laws in their function as constitutive principles, we must uncover the logic behind this prior grasp. We must try to lay bare part of the process through which a set of empirical problems became more definite and took on a character which would allow for their solutions to become exemplars for Newton's own undertaking. We must excavate the intelligence that is deposited in these ways of proceeding. We must attempt an archaeology of seventeenth century writings *on what could become* classical mechanics.

1.2.2 Archaeology of knowledge

Late in his career, Edmund Husserl started thinking about his philosophical project as one of archaeology:[70] his phenomenological investigations were directed at revealing the genuine grounds of the norms that govern our knowledge. Michel Foucault, who made the expression of "archaeology of knowledge" famous, used it to refer to a project that he conceived to be in explicit opposition with Husserl's phenomenology.[71] However, the goal of the project remained exactly the same (which of course also explains the need that Foucault felt to antagonize against the phenomenologists). The grounds that Foucault thought to uncover were explicitly historical in nature, and could in no way be ascribed to the activity of a constituent (transcendental) subject. In what follows, I will give a quick sketch of what I take to be the broad outlines of the project of archaeology of knowledge. This is

[70] See footnote 6 on page 111 of Hyder 2003.

[71] Passages that must be read as implicit attacks on phenomenology and especially the late philosophy of Husserl abound in Foucault's *Archéologie du savoir.* See also Hyder 2003 on this opposition.

explicitly inspired by Foucault's writings, but I am not interested in faithful exegesis here; I am interested in its potential fruitfulness as a historiographical tool to deal with philosophical problems.[72]

Foucault has been read in many different ways, and has been claimed for many different causes. Whatever the ambiguities in his writings and the development in his thinking, I think that a strong case can be made to interpret his program as a subject-less neo-Kantianism. (This is true both with respect to the epistemological side and, maybe somewhat more surprising, the political side. I will only focus on the first aspect.) This is a reading that Foucault himself began to stress in the early eighties. To illustrate this, I have chosen the following clear quote, from a piece on Foucault that Foucault himself wrote for an encyclopaedia, under the pseudonym of Maurice Florence:

> Si Foucault s'inscrit bien dans la tradition philosophique, c'est dans la tradition *critique* qui est celle de Kant et l'on pourrait nommer son entreprise *Histoire critique de la pensée.* … Si par pensée on entend l'acte qui pose, dans leur diverses relations possibles, un sujet et un objet, *une histoire critique de la pensée serait une analyse des conditions dans lesquelles sont formées ou modifiées certaines relations de sujet à objet, dans la mesure où celles-ci sont constitutives d'un savoir possible*. Il ne s'agit pas de définir les conditions formelles d'un rapport à l'objet : il ne s'agit pas non plus de dégager les conditions empiriques qui ont pu à un moment donné permettre au sujet en général de prendre connaissance d'un objet déjà donné dans le réel. La question est de déterminer ce que doit être le sujet, à quelle condition il est soumis, quel statut il doit avoir, quelle position il doit occuper dans le réel ou dans l'imaginaire, pour devenir sujet légitime de tel ou tel type de connaissance ; bref, il s'agit de déterminer son mode de « subjectivation » ; car celui-ci n'est évidemment pas le même selon que la connaissance dont il s'agit a la forme de l'exégèse d'un texte sacré, d'une observation d'histoire naturelle ou de l'analyse du comportement d'un malade mental. Mais la question est aussi et en même temps de déterminer à quelles conditions quelque chose peut devenir un objet pour une connaissance possible, comment elle a pu être problématisée comme objet à connaître, à quelle procédure de découpage elle a pu être soumise, la part d'elle-même qui est considérée comme pertinente. Il s'agit donc de déterminer son mode d'objectivation, qui lui non plus n'est pas le même selon le type de savoir dont il s'agit.[73]

So let us start with a particularly interesting and convincing way of understanding Kant's critical philosophy, as e.g. expounded masterfully by Henry Allison.[74] On this reading, the main goal of this philosophy is a reconfiguration of our epistemic norms. Its true originality doesn't lie in any

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[72] There exists of course an extensive secondary literature on Foucault; let me just mention two general introductory works which consider Foucault's work from perspectives that explicitly connect it with more analytically oriented philosophy of science: Dreyfus and Rabinow 1983, and Gutting 1989. (Hacking 1982 is an appealing brief discussion of some broadly conceived Foucauldian insights, which places these squarely within the traditional concerns of more traditional analytic philosophy.)

[73] Florence 1984, p. 942 (my emphases). Also Foucault 2001b, pp. 345-346.

[74] Allison 2004.

substantive thesis, but (in Allison's terminology) in the metaphilosophical standpoint that it propagates. Critical philosophy doesn't primarily ask what to believe, but what to take as norm for judging our knowledge. Kant's Copernican Revolution is a metaphor for leaving a "theocentric" for an "anthropocentric" model of knowledge (again in Allison's terminology). A God's eye view of things, where someone has an immediate grasp of objects, isn't a sensible norm to use in analyzing *our* knowledge. We should rather start from the necessary presence of "epistemic conditions", i.e. those conditions without which our representations could not possibly relate to objects. The task of critical philosophy is to analyze these conditions; i.e. to ask: *how can something become an object for our knowledge?* It is not that things transcending the conditions of human cognition cannot exist, but that they cannot possibly count as objects for us. To put it metaphorically: whereas a theocentric model conceives of objects as *given* to our knowledge, on an anthropocentric model objects are *taken as given*. What is given to us is cognized only on taking it in. (It is important that it is in no way denied that all empirical knowledge requires that something is given; it is only that the givenness refers to the objects not yet taken under any empirical description – this is the infamous thing in itself; i.e. the things considered apart from all epistemic conditions and, as a result, non-representable.)

For Kant these epistemic conditions were intimately and necessarily related with the human mind. His critical analysis starts from a fixed subjective point that carries the possibility of objectivation in itself. Husserl blamed Kant for taking a much too abstract view on what this subjective point consisted in, and wanted to start his own critical investigations from "lived experience". Foucault blames both Husserl and Kant that they assume that there is something like a fixed point.[75] His project is still critical in that it investigates the possibility of a relation between subject and object, but he wants to start his analyses *from* this relation, without the assumption of a fixed point – the question becomes how *both* objectivity and subjectivity are co-constituted.[76] It is the relation itself that is constitutive for a possible knowledge. The indefinite particle is no accident: the grounds of this relation are sought in particular historical configurations, and with changing configurations we can find the possibility of different knowledges. (Admittedly, this sounds awkward in English – "savoir" stands for something wider than the limited use of knowledge in English, which comes closer to Foucault's use of "connaisance." Maybe it helps to keep in mind the verbal use in expressions like "savoir lire" or "savoir conduire une voiture": knowing as being able to enact a

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[75] It is a matter of debate whether this is an accurate characterization of the position of the late Husserl, who explicitly engages with the historical character of constitution, but that need not bother us here.

[76] Ernst Cassirer's *Philosophie der symbolischen Formen* similarly tries to investigate how objectivity and subjectivity become co-constituted through a mutual and historical process (Cassirer 1922-1927). The important difference is that he does assume one regulative principle of reason that drives human history; his history is Hegelian through and through (see especially the introduction to the first book of the *PSF*) and in this respect sharply different from Foucault's. (Heidegger, in his *Frage nach dem Ding* (1967 [1935-1936]), also suggestively argues that the Kantian project can only be properly understood if one starts from this relation itself, rather than from the categories as presented in the *Kritik*.)

particular way of interacting with things and situations. I would suggest that we understand "savoir" as somehow midway between propositional knowledge and know-how, and I will try to avoid too awkward formulations by using circumlocutions as realm or body of knowledge).

These historical configurations have received many names, both in Foucault's and other authors' hands, partly depending on the chosen level of analysis: discursive formation, episteme, regime of truth, style of reasoning (Hacking)… The common insight behind these different denominations is that a statement only becomes a candidate to function in a body of knowledge *in relation to* other statements, methodological principles, associated subject functions ("who is speaking here?"), and non-discursive technologies and practices. Depending on the particular historical configuration in which a statement emerges, it will make a completely different, possibly nonsensical, claim. (Consider the statement "this table consists of innumerable atoms and vast empty space" when uttered by a Greek philosopher around the 3rd century BC, a condensed matter physicist in 2006, an inhabitant of the Amazon forest who has never met a Westerner, a surrealist poet, a computer that is programmed to produce random sound-bits, or a British philosopher around 1920.) It is only this set of relations that first makes it possible for meaning and reference of these statements to emerge, for subjectivity and objectivity to constitute itself.

The archaeologist tries to get a grip on these configurations by paying attention to what statements *do*, to the role they play within the context in which they are put forward. (To use an example from Foucault: the statement that species evolve functions completely different before and after Darwin; this difference, however, is not simply due to the fact that the meaning of the words used has changed, but rather that the whole configuration of relations in which this statement can play a role has been reorganized.[77]) They can play several roles because they establish a relation with the possible objects to which they can refer, because they invoke particular subject-functions, because they simultaneously refer to a number of other statements, because they have a specific materiality (written, drawn in a diagram, spoken in front of a classroom, …). As said, this complex of functions is made possible because a statement always appears as part of a larger "field of utilization," which is simultaneously a "field of stabilization". (The identity of a statement as *this* statement depends on its place in such a wider field.) The task that confronts an archaeologist is to try to excavate the way this stabilizing function is exerted: what kind of objects are deemed possible, how are these differentiated in simultaneously discursive and non-discursive practices – what are their conditions of appearance *as* objects; who is speaking from what kind of position using which means – how are the possibilities of subjectivation inscribed in discursive and non-discursive practices; in what kinds of relations must concepts stand among each other; … ?

Naming this set of relations that tie statements to their context of functioning a "configuration" is a circumspect way of calling attention to the systematic aspect of this set while attempting not to

[77] Foucault 1969, p. 136.

push this systematicity beyond the limits of plausibility (which Foucault at some points gives the impression when writing his methodological reflections in *L'archéologie du savoir*). Both aspects are vital. Without any degree of systematicity the use of statements would lack the stability that makes it possible for knowledge to constitute itself. But this systematicity must always remain a historical stability; i.e. it can not be frozen in absolute structural or discursive "laws". The particular configurations that can be found at certain points of time are the outcome of local and contingent (but analyzable) power plays, and accordingly need extra-discursive relations to stay in place. (The analysis of these power plays is the subject of what Foucault called genealogy, which must be understood as a level of analysis that is complementary to archaeology. I want to focus here on the archaeological level.)

These configurations not only make possible the appearance of some statements but simultaneously and necessarily exclude many others. Any such configuration restricts what can be claimed. It is clear why we can understand Foucault's project as *critical* philosophy: it still is an attempt to trace the conditions under which something can become an object for knowledge. These complex configurations are a form of epistemic conditions that make it possible for representations to relate to objects, but the locus of these conditions has been shifted from a constituting subject to a historical environment – with subjectivity now as one of its elements rather than as its organizing principle. (Remember all the talk about the death of man. But keep in mind that we need not take this to extremes: it implies that particular forms of subjectivity – in particular the subject as constituting authority – have outrun their course; they are no longer able to function in stable configurations. There are particular ways in which *we* simply can no longer think about what it is to be a subject. This is not claiming that subjectivity itself has gone out of the door. Subjectivity remains grounded as something that is given, and as such constrains all possible relations in which it can enter, exactly as objectivity already was for Kant. Maybe we should speak about the human in itself – as the human not yet engaged in any discursive or any other relations; empirically unreal but transcendentally necessary.) It is not only in an ironic mode that Foucault frequently uses the phrase of the historical a priori to describe the object of his analyses.

A prime application of this kind of archaeological analysis is directed towards the *possibility of the formation* of scientific disciplines (in Foucault's case: the human sciences). A discipline is (a part of) a configuration that is characterized by a set of statements that are aimed at saying the truth – statements that are "within the truth" ("dans le vrai"[78]). It is only *within* this realm that one can then differentiate between true and false statements (the false statements are "*disciplined* errors"[79] – compare e.g. with what happens in the seventeenth century with the famous number of angels that fit on the point of a needle: they don't even function in false statements, they are relegated the realm of

[78] Foucault 1971, p. 36.

[79] Ibid., p. 37.

senseless statements, to the "teratology" of knowledge). It is the task of epistemological analysis to determine what sets apart the true statements from the false ones. But it is the task of archaeological analysis to determine how such a thing as epistemology can exist at all. Another way for stating this is as follows: epistemology questions science, archaeology *savoir*.[80] It is only on the groundwork of a partly constituted knowledge and know-how that science can emerge. We first have to find out how objects are carved out, how concepts are discursively related, before we can see how they are used to state truths. There first has to be a basis on which to recognize *what* and *how* scientists want to know – what it takes to be a scientific object and a scientist.

An archaeology of knowledge tries to uncover what is taken to be significant, interesting, self-evident at particular times in history. It tries to locate the basic structures of intelligibility that reside within historical configurations. Needless to stress that this intelligibility (a term not used by Foucault) is not rooted in a purely subjective ground, that it rather also includes what it can mean to be to be a particular kind of subject (e.g. an empirical scientist). At one point Foucault refers to these structures as the brute being of order.[81] The term most often used by him, however, is that of a positivity; an obvious and partly ironical bow to the basic positivistic insight that all knowledge must be referred back to something more basic, but in all probability primarily a way of consciously turning Bachelard's psychoanalysis of the scientific spirit on its head. (Bachelard tried to purge scientific thinking from its impeding unconscious structures; Foucault stresses that there are always unconscious structures which play an enabling, *positive* role.) In these structures of intelligibility, in these positivities, is the specific normativity grounded which will be proper to the scientific disciplines that are first grafted on particular historical configurations.

(If I were to summarize the philosophy behind archaeology, I would say it is positivism with a functionally organized basic layer – an organization which relates the different relata that first make factuality possible – a fundamental level which exactly because of its *organized* form is subject to historical transformation. Facts are facts: a strict positivism seems to be exempt from historical changes. But, the archaeologist wants to suggest, maybe some facts can only become facts if they are situated in the right kind of configuration.)

Transformations in these basic structures open up the possibility of forming new scientific disciplines or bring along drastic changes in existing scientific disciplines. These are obviously the moments where archaeology finds its own natural anchorage. These are the moments on which much becomes visible to the analyst's eye. New stabilizations can take place quickly but always involve a violent moment of restructuration. There is not one general story to be told about how such transformations take place. Sometimes it is the introduction of a new kind of object that starts such

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[80] This juxtaposition of course depends on how one chooses to use the term "epistemology." In Foucault's case the opposition must be understood as primarily directed towards the work of people like Bachelard. I have no objection to understanding archaeology as a kind of historical epistemology – although it still would have to be a special kind.

[81] Foucault 1966, p. 12.

transformation. Sometimes it is rather new concepts that in their particular way of combination open up a new realm of statements and knowledge. Sometimes it is the definition of a new legitimate perspective for the knowing subject that is determining. And sometimes it is all these factors (and others) together. Only a local and detailed investigation will tell.

1.2.3. A note on Thomas Kuhn

Again, I am not going to enter in detailed exegetical exercises. But I think it is worthwhile to draw some parallels with the work of Thomas Kuhn, especially as this work is explicitly directed towards the physical, and often mathematical, sciences, which also constitutes my domain of questioning. It is moreover notable that, from a certain point in his career, Kuhn himself began referring to his position as Kantianism with moveable categories.[82]

It is notoriously difficult to extract one coherent philosophical position from Kuhn's *Structure of scientific revolutions*, but it is of course up to our own judgement to see which is the most attractive position that we can find in that fascinating little book. I think the most promising candidate has been presented e.g. by Joseph Rouse. This is a reading in which Kuhn is primarily interested in science as a practice.[83] Rather than interpreting the Kuhnian talk about paradigms as primarily directed towards theoretical commitments, comprehensive worldviews, core beliefs etc., and the ensuing problems surrounding incommensurability as involving untranslatability of theoretical and empirical claims, one can also stress that

> …accepting a paradigm is more like acquiring and using a set of skills than it is like understanding and believing a statement.
>
> Among the skills that might constitute the grasp of a paradigm are the appropriate application of concepts to specific situations; the deployment of mathematical tools (not just solving equations, but choosing the right ones, applying them correctly to the situation at hand, knowing their limitations and the ways those limitations can be circumvented, etc.); the use of instrumentation and experimental techniques and procedures; and the recognition of significant opportunities to extend these skills in illuminating ways to new situations.[84]

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[82] Kuhn 2000, pp. 104, 264. It is an ironic twist of history that Kuhn also began to recognize that Carnap's attempts had been directed towards a similar project (although not from a historical perspective, of course), whereas he had took these to be one of his main targets at the time of the *Structure*. For more on this relation, see e.g. Reisch 1991; Earman 1993; Irzik and Grunber 1995. Hoyningen-Huene 1995 is a useful and laudable attempt to give a full-scale overview of Kuhn's writings, placing them in an overall Neo-Kantian framework, which, however, is not very deeply engaged with.

[83] Rouse 2003; see also Rouse 1987.

[84] Rouse 2003, pp. 107-108.

This way of reading Kuhn will be considerably less focused on questions concerning rationality and semantics than has been common. "Living in a new world", one of these contentious Kuhnian metaphors, also takes on quite another sense: this is rather a matter of doing and behaving than a matter of believing and thinking. Again Rouse: "If proponents of different paradigms do not fully communicate, it is not so much that they cannot correctly construe one another's sentences or follow one another's arguments. The problem is more that they cannot grasp the *point* of what the others are doing or recognize the *force* of their arguments."[85] That is, the real roots of incommensurability often lie at the level of *savoir* rather than at that of science.

Here I just want to stress the one Kuhnian insight which I really take to be central, and probably also the most original of Kuhn's contributions to philosophy of science. In the 1969 postscript to his *Structure* Kuhn tried to clear up some of the ambiguities that had surrounded the term "paradigm" in that book. (Ambiguities which I don't think annul any of the ideas that were expressed through the use of the concept – which is another thing than claiming that they all stand up to critical scrutiny.) To that end, he introduced two new terms to cover the terrain that he had earlier expressed through this one term. On the one hand one needs to take account of the central function of *exemplars* within the normal functioning of science. These are "accepted examples of actual scientific practice" that serve "implicitly to define the legitimate problems and methods of a research field".[86] On the other hand there is a broader aspect to this shared practice, what Kuhn calls the *disciplinary matrix.* This is the organized complex of elements that make up such a practice, centrally including the exemplars, but also other elements such as values, shared concepts, apparatuses, … .

Now, this central role for exemplars as constitutive for a field's legitimate problems and methods is a brilliant insight. It is through scientists' enculturation via exemplars that they learn to recognize significant similarities and differences. This is something that would be impossible to grasp on any account that stresses explicit knowledge and rules, because such a view cannot even make a beginning with understanding the open-endedness of all scientific research. These exemplars determine what can become an object for a particular scientific knowledge; they determine how concepts are to be put to use. That is, exemplars embody crucial features of a science's underlying positivity. As such they provide an essential clue to understanding how the stabilizing function of a discipline is exerted in at least the physical sciences. It is primarily on the basis of these structures of intelligibility as exemplified in exemplars that scientists can grasp the point of what others are doing. This anchors the common ground on which a science can constitute itself in its glorious abstractness and ideality.[87]

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[85] *Ibid*., p. 113.

[86] Kuhn 1996 [1970/1962], p. 10.

[87] Dreyfus and Rabinow 1983, p. 60, similarly suggest that Kuhnian exemplars might provide for a missing element in Foucault's analyses.

1.3 An archaeology of Galileo's science of motion

1.3.1 A new field of stabilization

In this thesis I want to investigate the new field of stabilization that emerged in the beginning of the seventeenth century and that gave meaning to the idea that phenomena of motion could have a mathematical character. This inevitably requires that I make some preliminary choices in the aspects and authors to study. I will focus on Galileo's attempts to construct a mathematical science of motion. This seriously restricts my investigations, and it might even be thought that such focus on one author goes counter to the archaeological focus on anonymous configurations that *manifest* themselves in individual authors, rather than seeing individual authors as being at the *origin* of changes in our ways of conceiving things. Yet there are also some things to be gained from such a focus – and this need not imply that I turn Galileo in a "founder" of a tradition. (However, there are good reasons why he could be considered to be so from a certain perspective. Given our present day practices of knowledge, and especially the ways we investigate the world in physical disciplines, there is a good claim to be made that Galileo is the first single author in which we can recognize elements that are sufficiently familiar to us. This is an interesting claim in its own right, but it is not what I am primarily interested in, nor do I think that it necessarily makes him a "founder" in any interesting sense.)

I am interested in archaeology of early seventeenth century knowledge as a crucial element to understand how it became possible that a scientific theory such as Newton's could be formulated by the end of that century. I want to start giving an answer to what Foucault left as an open question: "selon quel ordre et quels processus s'accomplit l'émergence d'une region de scientificité dans une formation discursive donnée?"[88] Once we have put ourselves in this mode of questioning, I think it makes good sense to focus on a single author as Galileo. This thesis will accordingly try to excavate the historical a priori underlying his mathematical science of motion.

To understand the emergence of a new scientific field it is necessary to uncover the way in which "nature" functions discursively as a normative instance that regulates the kind of claims that can be scientifically made about objects under study – it is a crucial element in what Foucault called "the internal epistemic controls that any scientific discipline exerts on itself."[89] Since it is a central element *within* any structure of intelligibility, we cannot assume that "nature" is at work as a fixed point in the transformations that we want to investigate. We are on the contrary trying to see how a new configuration of relations between different elements opens up a new kind of access to what can then become objective reality. It is only the presence of such a basic structure of intelligibility that makes it possible to discern things *within* nature. It is only on this basis that some things can start to function as

[88] *Ibid*., p. 240.

[89] Foucault 2001a, p. 896.

evidence for claims *about* nature. The important challenge is then to see how this transformation can take place if it is not through noticing things within nature or by making new claims about nature: in which Archimedean point can such transformations find their point of leverage? This challenge of course derives its pertinence from the fact that we should be able to answer this question without ending up in an untenable idealism. Chapter 5, which deals with this issue, is the pivotal point of the present thesis.

To understand the changes wrought through the work of someone like Galileo, it is necessary to understand the kind of position from which he could start. As will become clear, the link with the category of the mixed sciences is crucial. These were the mathematical sciences as applied to physical phenomena. On the one hand, the category refers to a discursive practice that was inscribed in the sixteenth-century Aristotelian field of knowledge. Philosophers discussed about the possible worth of these sciences; its practitioners tried to position their endeavours with respect to these philosophical discussions. On the other hand, these practitioners were forging themselves an interesting place in society which could be gained independently from these philosophical discourses. They stressed the characteristics of their endeavours which could make these particularly well suited to be accorded a central and legitimate place. Chapter 2 will introduce a few elements that are directly relevant to these issues.

There is also another angle from which to approach these mixed sciences. We can try to see how its practitioners discursively organize the content of their knowledge within their treatises. In chapter 3, I will investigate Guidobaldo del Monte's mechanical writings to see the kind of coherence he was imposing on the science of mechanics. To this end, I will primarily pay attention to the *use* to which he puts some of his central concepts. How does this allow him to mathematically represent facts about physical instruments such as a balance?

In chapter 4 we will see how this kind of discursive organization provides Galileo with a model for his own first attempts at developing a mathematical natural philosophy. However, this implies that he breaches the rules of the philosophical discourse concerning the mixed sciences. We will see how he does this by exploiting exactly these properties that are singled out in chapter 2 as providing the mixed sciences with a legitimate position of their own. This is then a first important element in the stabilization of a new field of knowledge. The values embedded in late sixteenth-century society did allow for a different structuring of this field.

At this point, we are confronted with the problem about "nature" mentioned above. Galileo claims to be discoursing on natural phenomena, but his way of engaging the objects of his study is to a large extent determined by what he had learned from works in the mixed science tradition such as Guidobaldo's. He notices crucial facts about physical bodies by seeing how they behave on an instrument such as the balance. If this enterprise is to *make sense*, it must be because it is stabilizing around a new mode of functioning for "nature."

Peter Machamer introduced the notion of "model of intelligibility" to capture the multiple functions the balance plays within Galileo's science: "Its physical concreteness, mathematical describability, and physical manipulability leading to experimental possibilities gave intelligibility and structure to the abstract concepts of the mechanical world picture."[90] This notion fits very nicely with the kind of archaeological framework that I have described in section 1.2. The balance is an exemplar embodying the structure of intelligibility that grounds the discipline in question. The question about the stabilization of the new field of knowledge thus can be sharpened to the question: why would a mechanical instrument like the balance become a model of intelligibility for a science *of* nature? If we can understand how this could have happened, we can make a start with answering Foucault's query how a "region of scientificity" could arise within a given discursive field.

The answer that will be proposed in chapter 5 shows how a number of different elements present in the traditional discourses on machines could be put together in a new kind of configuration. It is only the simultaneous presence of these elements that could lock the stability of the new field of knowledge. Once this stability sediments in something like a model of intelligibility, it starts constraining further investigations directly. It is these models that from now on determine how phenomena present themselves, what kinds of questions can be asked, which are proper evidential considerations; i.e. they are constituting a region of scienctificity.

Up to this point, we will have assumed that Galileo retains the internal discursive organization of the mixed sciences but places it within a different field of knowledge. However, the latter fact has repercussions for this internal organization. As the regulative functioning of nature has been restructured, the representational relation linking mathematical structures to concrete physical events also takes on a different character. In chapter 6 we will see how this throws light on the important question of idealization within Galileo's science. The principle of inertia, e.g., describes behaviour that can impossibly be empirically exemplified, and as a result one could wonder *what is the sense* in accepting its truth. (Guidobaldo would certainly have felt this bewilderment.)

I will also introduce a further important element of a model of intelligibility's mode of functioning in chapter 6. The normativity embodied in a balance or a pendulum is not only situated on the theoretical level. The way the phenomena under study present themselves also depends crucially on the way we bodily engage with e.g. a pendulum. This performative reason, as I will call it, is an important epistemic condition that makes possible the mathematical representation of phenomena of motion. As a result, at the end of chapter 6, I will be in a position to sketch what I take to be the different levels at which Galileo's models of intelligibility function simultaneously to play their particular roles.[91]

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[90] Machamer 1998b, p. 71.

[91] Cf. chapter 6, section 6.3.2. This section, taken together with chapter 5, section 5.4, expresses the true analytic core of my study of Galileo's science of motion.

Chapters 7 and 8 form a kind of case study where we can see how this stabilizing function of models of intelligibility is being exerted. To this end, I will analyze the development of Galileo's thinking on a specific problem with which he was confronted in his theory of motion. I don't want to burden Galileo with the impossible task of founding a whole new structure of intelligibility (how could such structure be shared if it was founded by one individual – it would not be a structure of intelligibility but a structure of idiosyncracy); but neither do I want to diminish the important role he played in the stabilization of this field. To investigate this role we must follow the attempts that we can trace throughout the whole of his writings to formulate a set of problems in a new way.[92] It is here that we can witness the transformation and stabilization of a discursive formation *at work*.

Chapter 9, finally, will be devoted to a study of Galileo's way of discursively stabilizing the field in which his sciences are to function. Let me stress that this happens very tentatively. A completely stabilized discursive formation is an ideal type: useful for analytical ends, but very improbable to be found realized in any historical situation. This situation is similar with that of the Kuhnian distinction between normal and revolutionary science.

1.3.2 A new field of stabilization – a new rationality (?)

To counter some of the suspicions that may have arisen (after all, invoking Foucault may already have been enough to have this effect): I am not interested at all in questioning the status of knowledge *as* knowledge. What I do want to investigate is under what conditions something can become an object for our knowledge. It may very well be (and "of course" I believe this to be the case) that we have found out about physical capacities which are as real as one can like, and which for that matter have always been real. But this does not automatically imply that the structures of intelligibility which first make these into possible objects for our knowledge have been there all along. Whatever matters of the fact we may notice about the physical world, these are simply unintelligible from within some other historical configurations. Being real does not imply being present.

This implies an element of relativity, but I believe this is healthy relativism, far removed from sweeping claims, but on the contrary open to local resolution. As repeatedly stated, the kind of exercise that I propose in this thesis is to uncover the grounds for the normativity of something like Newton's laws of motion (knowing well that I will only be able to take a first small step). But it may very well be that on investigating these grounds, we decide that they indeed involve the kind of basic relations between us and the world that we want to uphold as defining good practice. It is not because our science is grounded in a particular and partly contingent way of engaging with the world that these ways can no longer be taken to be defining for what is to count as objective knowledge. It is up to us

[92] This way of phrasing the issue provides a clear link with Jardine's (2000 [1991]) question-oriented historiographical framework, which indeed shows many similarities with what I take to be an archaeology of knowledge.

whether we want to continue our epistemic practices or not. Critical philosophy investigates the grounds on which something can become problematical; it does not decide whether we can still recognize and want to uphold these grounds. (How to make such decisions? That is an interesting and difficult philosophical problem, which has not yet received the attention it deserves. See however van Fraassen 2002 for an interesting attempt from an analytical philosopher who has gradually come to see the importance of this kind of question in his attempts to develop a "new," *voluntarist* epistemology.)

One of the main reasons why it is tempting to elevate the truths expressed in physical theories to a more absolute status is that these theories seem to have achieved a relative high degree of autonomy from the particular historical configurations in which they first emerged. (Certainly when we compare them with Foucault's main object of study: the sciences of man.) That is, these sciences are, in Foucault's words, organized by a set of *internal* epistemological controls.[93] The example of Newton provides an illuminating example. By codifying some central features of what he took to be exemplary achievements, he is able to partially detach these results from their locally situated practices. From now on, his laws of motion *define* what it takes to be within the domain of mechanics. The theory itself helps to pick out which problems to treat. It starts actively constraining the practical context in which it functions, rather than the other way around.

We should not forget Kuhn's insight, however: even in the presence of a set of constitutive principles, there is still an important role for skill in applying the theory. Exemplars embody the presuppositions on which the theory operates, but seeing how this fleshes out in any particular context remains dependent on the scientists' abilities to *perceive* the relevant similarities and to *exploit* them to treat new situations. (This actually depends on a plethora of skills: recognizing that a phenomenon is "sufficiently" similar to known phenomena; knowing how to effect necessary calculations, which includes seeing which shortcuts in calculation are harmless; knowing how to set up a good and relevant experiment, which includes skills in manipulating instruments in the right way; etc.)[94] This can again nicely be illustrated by Newton himself. It took considerable skill to see that and how astronomical phenomena could be understood as mechanical. It is interesting to note e.g. that the investigations that led to the *Principia* really took off at the moment that Newton "saw" that Kepler's area law was intimately bound up with the law of inertia.[95]

The fact that these constitutive principles necessarily remain embedded within a practical context which only gives meaning to their application helps to underscore the central point I am trying to make here, which is that the ultimate justification of these principles (and of all the further claims that they make possible) lies in the practices of which they express some of the basic presuppositions. Rather than expressing truths about the ontological structure of reality they formalize something basic

[93] Foucault 2001a, p. 896.

[94] See Rouse 1987, especially chapter 4, for an interesting treatment of the different roles of skills in scientific practice.

[95] Cf. e.g. Cohen 1980, p. 250. ("And then, perhaps suddenly, the significance of the area law would have burst upon his consciousness.")

about our way of engaging with the world within a certain practice. To repeat: *within* this way of engaging we encounter many facts which constrain our possibilities of engagement and simultaneously allow us to formulate true (or false, for that matter) claims about nature. The fact that they allow this is already an important part of their justification. An epistemic practice which would not enable the generation of constraints on its own internal development would not appear very *valuable*.

Of course, everybody is free to hypostatize epistemic conditions into ontological categories. But this actually means that one pretends to be able to step outside *any* way of engaging with the world to see its noumenal structure. Falling back on a theocentric model of knowledge may be a recurrent feature of Western philosophical activity, but why not try to resist such a soothing (?) move which actually teaches us nothing new?

I will not enter any further into these debates in the rest of this thesis. (Debates which are of course a bit more sophisticated than I have presented them here.) I prefer a detailed investigation in a particular way of engaging with the world above abstract philosophical musings concerning its status. In the end it is only this kind of detailed, local *and* historical work that can really teach us something about the specific rationality of our practices and the theories that are grounded therein.

2 Mathematical sciences in the sixteenth century

Many of the twentieth-century discussions concerning Galileo have been structured around the question whether a Platonist metaphysics and/or epistemology lay at the origin of his mathematical forays into the traditional field of philosophy of nature. I will not enter into a sustained discussion of the different positions that have already been defended on this issue, but I will offer some elements of what I take to be the beginning of a defensible answer to this question in the very last chapter of this thesis. Of course, this assessment will be based on what will be learned in the next chapters. In the present chapter I will sketch part of the historical background against which these studies in the next chapters must be read.

By introducing some of the late sixteenth century philosophical views on the relation between mathematics and the empirical world, I will try to place Galileo's endeavours to develop a mathematical science of nature in their own context. However, rather than attempting to trace influences, I hope to uncover the broad outlines of the conceptual space within which philosophers reflected on the possibilities of a mathematical science, as well as the picture of Platonism that was at work in these reflections. This will later allow us to ascertain part of the dynamics between this background and Galileo's ways of implicitly restructuring this conceptual space through his practice.

In a second main section, I will connect these philosophical reflections with the changing institutional and social status of mathematical practitioners and practice in the sixteenth century. (But let me immediately add the caveat that only in the next chapter I will have a look at these sciences as they were practiced, rather than as how they were presented and perceived – which is the limited focus of the present chapter.) This is important to properly understand the position from which Galileo was working and writing. We will accordingly see in some of the ensuing chapters how the availability of this kind of position made possible some of the peculiar features of Galileo's contributions to the stabilization of a new field of knowledge.

2.1 Philosophical debates on the status of mathematics

2.1.1 Aristotle and the mixed sciences

Aristotle's views on the relations between the different fields of knowledge are multifaceted and present too many subtleties (or internal problems) to be adequately dealt with here.[96] Yet one of the organizing principles of Aristotle's views on science required that a science should be homogenous, i.e. that its principles deal with the same genus as its objects.[97] Both natural philosophy and pure mathematics seem to fit this bill easily, but applied mathematics at first sight present a problem, as it uses purely mathematical principles but applies them to natural things such as visual rays, sounds, or celestial motions. As shown by Richard McKirahan, Aristotle's own pronouncements on these *subalternate* or *mixed sciences* are not entirely coherent and sometimes seem to be directed towards different distinctions at once.[98] It is nevertheless useful to start by introducing some of Aristotle's own ideas. This will be important for understanding the status that the mixed science of mechanics can hold in the sixteenth century landscape of knowledge. It will also prepare the ground for the summary of some sixteenth-century views on mathematical sciences in the next subsections. And finally, it will also be crucial to understand some features of Galileo's practice, which in crucial respects must be situated in the tradition of these mixed sciences, as will become clear in some of the next chapters.

The requirement of homogeneity is not necessarily violated when mathematical principles are applied to natural objects such as visual rays because they can be applied to these objects *qua* geometrical objects. Visual rays are not identical to geometrical lines, since they have different qualitative properties, but in a subordinate science they are treated as if they just had geometrical properties. To quote from McKirahan's neat summary:

> Another way of expressing this connexion is to say that the subordinate science takes its subject over from the superior science, but adds a further element to it. The optician studies lines *in sight*, the musician, numbers *in sound*. From the point of view of the manner of treating the subject

[96] This is of course reflected in the extensive commentary literature that arose around this topic in the hey-days of scholastic philosophy; see e.g. Weisheipl 1959 and Grant 1996 for good introductory treatments (Weisheipl 1965 sketches some of the early medieval discussions). The structure of the mixed, middle, subordinate, or subalternate sciences has received a considerable amount of attention in the literature. Cf. Machamer 1978; McKirahan 1978; Wallace 1984 (chapter 3); Lennox 1986; Laird 1987, 1997; Mandosio 1994; Dear 1995 (chapter 2); Gingras 2001; Biener 2004; for some of the aspects most relevant to my purposes. (On the terminological side, I will use the terminology of "mixed sciences", which seems to have become in vogue precisely around Galileo's time, instead of the more typically medieval "middle sciences", or the more literally Aristotelian "subalternate sciences".)

[97] Most of the following views are to be found in the *Posterior Analytics*.

[98] McKirahan 1978.

> matter, this difference is irrelevant – what is added is accidental. It is the *modus considerandi* that determines the structure, organization, and approach used in the proofs of the science.[99]

This leaves us with some further questions of course. To assess the import of this distinction it is particularly important to unpack the *"qua"*-quantifier. That it is possible to study physical objects *qua* mathematical seems to imply that there is a true description of these objects involving only mathematical properties. If this is indeed possible, then it becomes understandable what it would mean to give mathematical demonstrations concerning them. More specifically, given that an object has a particular mathematical property, it would become possible to give a mathematical explanation of why it has this property; an explanation which would involve more general mathematical principles. One thus sees arising a double task for any investigation in these mixed sciences: a first has to do with establishing that a class of physical objects has a particular property which can be described mathematically; a second task then consists in showing that it has this property in virtue of further mathematical principles. Hence, the physical part of a mixed science gives one knowledge of the fact to be explained (an explanation *quia*), whereas the mathematical part possibly gives one knowledge of the reasoned fact (an explanation *propter quid*).

Is it possible to have true descriptions of physical objects involving only mathematical properties? According to at least one modern commentator, Aristotle was of this opinion.[100] On this interpretation he must have had a genuinely positive attitude towards the possibility and possible worth of the mixed sciences. A more traditional interpretation of Aristotle would have it that mathematical objects only exist in pure extension (intelligible matter) underlying physical objects, which can only be reached through radical abstraction from anything sensible.[101] On this view a physical sphere is never truly, i.e. mathematically, spherical, since the exactitude of mathematical objects is due to the fact that they inhere only in intelligible matter.[102] As a result, the prospects for mixed sciences would look much grimmer.

We must moreover not forget that since on the first interpretation the properties of objects that are treated in mixed sciences are truly mathematical, the objects would have them in virtue of their exemplifying particular mathematical structures, *and not in virtue of their nature*, i.e. their being the kind of things they essentially are. It is clear that even on such a view the mixed sciences would only

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[99] McKirahan 1978, p. 202.

[100] See Lear 1982 for a reconstruction of Aristotle's philosophy of mathematics which makes Aristotle come out as holding a rather sophisticated and attractive view on these matters.

[101] See Mueller 1979 for a defence of this view; Mueller 1990 charts the attribution of this view to Aristotle by the ancient commentators. (In the later publication, Mueller admits that he no longer holds on to this interpretation, leaning instead towards the view expounded in Lear 1982.)

[102] "To say that the mathematician studies a man as solid *is not to say that he studies a man at all*. Rather, it is to say that he studies what is quantitative and continuous in three dimensions." Mueller 1979, p. 102 (my emphasis). For Aristotle's talk about intelligible matter, see *Metaphysics* VII, 10.

be of seriously circumscribed value to Aristotle. As a result, the view generally held among medieval commentators seems to have been that the physical nature of the subject matter in the mixed sciences anyway changes the demonstrative status of its mathematical proofs of particular facts from *propter quid* to *quia*.[103] (It must be noticed that the medieval commentators often seem to have had a slightly different distinction in mind than did Aristotle.[104] Yet the difference is not terribly important for our purpose.) When one applies mathematical proofs to physical objects, one foregoes the possibility of ever attaining any of the object's properties' proper and immediate causes.[105] The properties studied by the mathematician simply never inhere in a physical object in virtue of its nature. All that mathematical demonstrations can possibly teach concerning natural objects must remain on the purely accidental level.

It is important to try to disentangle the two related threads of criticism that are being levelled against the mathematical sciences. On the one hand there is what I propose to call the *problem of idealization*, on the other hand the *problem of abstraction*. The first states that physical objects never exemplify *exact* mathematical structures; the second that mathematical properties are necessarily *accidental* (i.e. what one is left with when everything essential is abstracted away). It seems to me that the latter is the more fundamental problem from an Aristotelian point of view.[106] The problem of idealization apparently derives its strength from its association with the abstractive view on mathematical entities. Seen from this perspective, it is primarily an expression of the *ontological gap* between the mathematical and the physical that finds its origin in the fact that the former supposedly deals with intelligible and completely abstract matter and the latter with sensible and essentially formed matter. As such it boils down to the idea that we should take Aristotle's talk of mathematics as involving an operation of separation quite literarily.[107]

2.1.2 The *Quaestio de certitudine mathematicarum*

The existence of the category of mixed sciences provided some space, although seriously circumscribed, for applied mathematical sciences within an Aristotelian framework. The status one ascribes to these sciences clearly depends on one's views on the nature of mathematical knowledge,

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[103] See Laird 1987, 1997. Thomas of Aquino seems to have been a notable exception.

[104] Compare McKirahan 1978, p. 203, with Laird 1987, p. 168.

[105] As Laird 1987, p. 151, explains for the case of Grosseteste: "That is to say, when a pure mathematician considers a spherical body, he thinks of it as an abstract sphere and demonstrates of it certain purely mathematical properties, such as its being the largest isoperimetric figure. But when a *physicus* considers the same spherical body, he demonstrates that it is spherical using natural principles such as the homogeneity of its material."

[106] Especially if one takes into account the view, defended by Lear, that Aristotle did not believe that idealization posed an insurmountable problem.

[107] The main Aristotelian passages where he speaks about this separation are in *Metaphysics* XIII,3, and *Physics* II,2. But, as already mentioned, see Lear 1982 for a more subtle reading of these passages.

and its relation with the empirical world. During the second half of the sixteenth century there arose an interesting philosophical debate on the nature of mathematics which is immediately relevant to these issues. Although the debate was primarily focused on the status of demonstrations in pure mathematics, it can help us to better ascertain the space of possible positions concerning the prospects and place of a mathematical science of nature that existed around the turn of the seventeenth century.[108]

2.1.2.1 "*Pueri vero non sunt expertes…*"

In 1547 there appeared a treatise entitled *Commentarium de certitudine mathematicarum disciplinarum*. Its author was the Siennese philosopher Alessandro Piccolomini, who had appended the treatise to his equally influential paraphrase of the pseudo-Aristotelian *Mechanical question*s. The title immediately makes clear what was at stake: to ascertain the reasons behind mathematics' supreme certainty. According to a certain tradition this would be due to the nature of mathematical demonstrations. These would actually be *demonstrationes potissimae*, which, following Averroes, were held to be the highest type of syllogistic demonstrations, as these give knowledge both of the fact that something is the case and of the true, proper and immediate cause of this fact (i.e. they are at the same time an explanation *quia* and *propter quid*). Yet, as Piccolomini tries to show, one could doubt whether mathematics really lives up to that standard. After all, which causes would be given in a mathematical demonstration? Certainly no efficient, material or final causes; but, again following Averroes, one could believe that mathematicians were dealing with formal causes. Borrowing from Proclus, Piccolomini offers some examples from Euclid's *Elements* that prove the contrary. If the demonstration is to be *potissima*, the major premise must give an essential definition and the middle term must be the proper, unique and immediate cause of the property proved. Euclid fails on both scores.

There is an alternative way to understand the certainty of mathematics, however. Rather than focussing on the type of demonstration, Piccolomini suggests that we should focus on the peculiar nature of mathematical objects. In fact, quantity is the most universally shared sensible accident: one just abstracts from everything that makes up the particularity of any object until all that one is left with is what Piccolomini prefers to call "quantum phantasiatum", which is nothing but the possibility to acquire specific spatial determinations. And this "omnium sensatorum sensatissimum" is something which is undeniably shared by all our sense experiences. Since we have abstracted from all particularity, this implies moreover that quantity has no intrinsic connection with substantial forms. On the contrary: it is pure receptivity for form; i.e. being associated with prime matter, it inheres in all

[108] My account of the debates on the *Quaestio…* leans heavily on the very useful and erudite synthetic work of De Pace 1993. The other secondary sources that I consulted are Galluzzi 1973, Rose 1975 (chapter 12); Wallace 1984 (chapter 3); Jardine 1988; Baldini 1992 (chapter 1); Mancosu 1992, 1996 (chapter 1); Dear 1995 (chapter 2); Feldhay 1998. Extensive quotations of most of the authors are given throughout the text and in the footnotes of De Pace 1993.

material substances without depending on any particular form. The certainty of mathematics simply derives from its total separation from everything natural. But this immediately implies that mathematics cannot be of much value in trying to understand the true nature of things. Even if mathematical knowledge is being applied to physical objects, as is done in the mixed sciences, this in no way restores its scientific character. Indeed, in these sciences one simply concentrates on the non-essential quantitative characteristics of objects. In fact, Piccolomini explains, while adolescents may have very little expertise, they are very well capable of this kind of mathematical abstraction. Natural philosophy and metaphysics, on the contrary, require long study, much work, and unremitting observations, and are way beyond their ken.[109] We are confronted with a telling mirror image of the criticism that Galileo, and with him many other seventeenth-century philosophers, will level against exactly the kind of philosophy that Piccolomini stands for: that it is too easy to be informative in any sense.

2.1.2.2 *"Nova et non fondata"*

Piccolomini's attack on the status of mathematics did not go unnoticed. Among the people who took up the challenge was Francesco Barozzi, lecturer of mathematics at the university of Padua – the same university that had earlier been attended by Piccolomini, and which would later host Galileo among its staff. Barozzi was especially versed in the writings of Proclus, which had provided one of the cornerstones of Piccolomini's arguments. He immediately received the approval of Daniele Barbaro, the editor of Vitrivius' *Architecture*, who expressed his gratitude for the fact that the opinion of Piccolomini was refuted as "nova et non fondata".[110] Contrary to what we are often led to expect, mathematics was apparently held in high respect in many sixteenth-century university circles.

In his *Opusculum, in quo una Oratio, et duae Quaestiones: altera de certitudine, et altera de medietate Mathematicarum continentur,* published in 1560, Barozzi exploited the fact that Piccolomini had blended what seemed to be a basically Aristotelian position with Neo-Platonic elements. Notwithstanding his highly critical attitude towards the status of mathematical demonstrations, Piccolomini had assumed that mathematics somehow occupied a middle position between philosophy and metaphysics, a position which was reflected in the certainty attached to the objects of mathematics.

[109] "Quaerens ergo Aristoteles in Ethica cur pueri, prudentes, spapientes, aut naturales fieri non possunt, Mathematici vero possunt, statim assignat causam, quia scilicet Mathematicae sunt ab extractione, aliarum vero facultatum principia per experientiam assumuntur. *Pueri vero non sunt expertes, ad abstrahendum vero maxime sunt idonei.* […] Cum igitur principalia naturalia, resque ipsae naturales, et etiam Metaphysicae ex effectibus, longa experiential per sensum perceptis, cognoscantur, hoc autem longo tempore indigent, maximoque labore, et assidua observationone, nil mirum si pueris aditum negant, quipped qui ob aetatem experti non possunt. Res autem mathematicae, cum ex abstractione sint, seipsas penitus, et medullitus sensui nostro praebent, seque totas patefaciunt." Quoted in De Pace 1993, pp. 44-45. (My emphasises.)

[110] De Pace 1993, p. 126, fn. 18.

Such a hierarchy of disciplines according to the nature of their entities was however an essentially Neo-Platonic element, where this was however commonly interpreted as degrees of *perfection*. By reading *medietas* in such a Platonist vein, and neglecting Piccolomini's stress on the disregard of all particularity that comes with the all too easy abstraction, Barozzi can proclaim that it is impossible that the certainty of the objects would not reflect itself in the certainty of its demonstrations.

The Platonist perspective chosen by Barozzi turns Piccolomini's argument exactly upside down. Whereas the latter had argued from the separation of mathematical objects from natural objects to the imperfection of mathematics, Barozzi sees in this separation the ground of mathematics' superiority. The more we avoid the corruptibility and imperfections of the empirical world, the closer we can approach the true nature of reality. The pursuit of mathematical knowledge is thus a necessary step in man's philosophical ascent towards truth.

Barozzi also deals directly with Piccolomini's attack on the status of mathematical demonstrations as not exemplifying *potissima* demonstrations. He rightly points out that all the latter has done is to give *some* examples from Euclid which seem not to live up to the standard. But this is a long way from proving that geometrical demonstrations *in general* do not live up to the standard. One should rather understand these exceptional deviations as being introduced for didactic reasons, to avoid complicating the presentation of a series of theorems too much. Barozzi also stresses the fact that the scientific character of mathematics can also be seen from its perfectly systematic character. It is clear that this character was not really questioned by Piccolomini, but it is important to see that this is a theme that would become increasingly associated with the question on the certainty of mathematics. As a result, it would start to become possible to detach the discussion on the certainty of mathematics from purely ontological questions, although this certainly would have gone counter to the way both Piccolomini and Barozzi understood the topic.

2.1.2.3 *"Alteram Geometricam, reliquam syllogisticam"*

Pietro Catena was another Paduan professor of mathematics (from 1547 until 1576) who entered in the debate on mathematical certainty. In a number of treatises, he argued resolutely for a Platonist view on mathematical entities, and tried to develop a view on demonstration which could both do justice to such a view, and incorporate Aristotelian elements. Whereas Piccolomino had defined mathematical objects as "sensata sensatissimorum," Catena was clear on the basic independence of mathematical objects from everything empirical. If they were solely the result of an abstractive operation, they would lack the exactness which is one of their basic characteristics. Moreover, it is necessary to conceive of these objects as absolutely universal, again something not to be found in any empirical grounds. Alongside these strong objections against Picollomino's views, Catena offered an obvious alternative: pure reason alone is responsible for the existence of mathematical objects. As had already been done by Barozzi, Catena invokes the Platonist doctrine of reminiscence as an essential prerequisite to understand the grounds of mathematical knowledge. Yet, as stressed by Anna De Pace, his focus is significantly different from Barozzi's, and his conclusions consequently point in another direction. Whereas the latter had interpreted this doctrine in a way that precluded possible application of mathematical knowledge to material objects, Catena claims that geometry is not only the science of ideal entities, but also a tool to attain knowledge of concrete sensible things. Mathematics deals with universals, which are known through a rational thought process alone, but particular things can also participate in this universal nature. Mathematical objects do not primarily serve as deliverance from an unstable and imperfect empirical world. They rather allow us to discern universal properties in empirical things, transforming these into scientific objects.

The demonstrative procedures of the mixed sciences are thus the same as those of pure mathematics. But these are not *potissima* according to Catena. This does not necessarily affect their scientific status, however, but can also be seen as a limitation of Aristotle's syllogistic model of demonstration. In this way, Catena turns Piccolomini's critique of the status of mathematical demonstrations towards his own ends. He argues that already Aristotle had made the distinction between on the one hand syllogistic and on the other hand geometrical induction.[111] Both are valid modes of inference, directed towards different goals, and issuing from different kinds of premises, but Catena seems to intimate that geometrical induction has much more heuristic power. Much stress is again put on the systematic character of the body of geometrical knowledge. Geometrical demonstrations indeed do not necessarily involve "an essential definition" or "the proper, unique and immediate cause of the property proved" (as was demanded by Piccolomini), but they do always proceed from the better known or more universal to the less known or less universal. The internal order of relationships established in a geometrical treatment makes it possible to attain clear and valid knowledge of complex properties. But let us not forget that this stress on the rigour of the procedure is still accompanied by an explicitly metaphysical view on the essentially intelligible nature of the mathematical entities which guarantees the universality, exactness, and applicability of every conclusion drawn on their basis.

2.1.2.4 *"Perspicuum est repugnare quantitati"*

The Jesuit professors at the Collegio Romano followed the debate on mathematical certainty with obvious interest. The philosopher Benedetto Pereira unequivocally sided with Piccolomini and tried to purge the latter's position from what he understood to be spurious Neo-Platonic elements so as to strengthen this position (and to counter Barozzi's exploitation of these elements). In his *De communibus omnium rerum naturalium principiis*, first published in 1576, he further develops the

[111] "Verbum hoc *inducens* duas inductiones significat. Alteram Geometricam, reliquam syllogisticam." Quoted in De Pace 1993, p. 212, n. 54.

view that mathematical properties are only true of prime matter, which he conceptualizes as a kind of quasi-substance. Since he thus fully agrees that quantity is completely separated from substance, he again draws the conclusion that mathematics can have nothing to do with the explanation of causes, and thus cannot be truly scientific. As an example he offers the property of a sphere that it touches a plane in one single point, which only holds of the sphere as an abstract and mathematical quantity, but which is false for the sphere as a physical extension, since "si subiectum quantitatis […] est substantia composita, perspicuum est repugnare quantitati".[112] It is of course not the abstractive nature per se of mathematical concepts that is detrimental in the views of this Aristotelian philosopher, but their explanatory impotence. As mathematical reasoning ignores all change and the essentially formed matter, i.e. everything that has to do with the actual existence of substances, it is solely interested in an *ordo cognoscendi*, to the complete exclusion of the *ordo essendi.* Pereira further strengthens the position that natural philosophy is completely independent of mathematics by resolutely attacking the Platonist doctrine of reminiscence: all knowledge acquired by the soul is due to its natural unity with the body and senses. Whereas Piccolomino had attributed the certainty of mathematics solely to the nature of its object, Pereira agrees with many of his opponents that the nature of its demonstrations is to be held primarily responsible. However, the rigour of its procedure remains separated from the nature of its premises, and cannot guarantee its scientific character.

Pereira was chided by Christopher Clavius, the most eminent mathematician of the Collegio Romano, for spoiling his pupils by telling such things as that "mathematical sciences are not sciences, do not have demonstrations, abstract from being and the good etc."[113] The vehement reaction of Clavius brings another side of the discussion to the fore: the institutional struggle between philosophers and mathematicians on the right to treat the natural world.[114] It is important to remember that Clavius was engaged in a program of educational reform within the Jesuit society, in which he tried to push the agenda of mathematics as deserving an elevated status, whereas Pereira can be seen as providing arguments that should reinforce the Jesuit's politics of knowledge, wherein the distinction between mathematics and natural philosophy, and the submission of both to theology, played a crucial role. Ugo Baldini has forcefully stressed how this architecture of the field of knowledge essentially thrived on an Aristotelian ontology, which permeated both questions of regional metaphysics (*physica particularis*) and general methodological issues (where Aristotelian logic was unassailable).[115] This obviously limited Clavius in the possible options he had in answering the attacks on the value of mathematics. The strict division of the fields of knowledge at the same time had given the mathematicians a *de facto* form of relative autonomy, however, as they were supposed to be dealing with their own peculiar subject matter. This allows us to understand how Clavius dealt with the

[112] Quoted in De Pace 1993, pp. 88-89.

[113] Translation in Clavius 2002, p. 467.

[114] On this issue, see Baldini 1992; Dear 1995 (chapters 2 and 6); Feldhay 1995 (chapter 8), 1998.

[115] Especially in chapter 1 of Baldini 1992.

challenge issuing from the *Quaestio…* without tackling the metaphysical and logical issues head on, a strategy that has been analyzed in some detail by both Peter Dear and Rivka Feldhay. Clavius chose to present an appeal to the authority of both Aristotle and Plato concerning the philosophical worth of mathematics, combined with a stress on both the superior certainty of mathematical demonstrations, and its utility in a host of disciplines, including natural philosophy, metaphysics and politics. Maybe the most important element in this respect was the fact that the systematic character of mathematical treatises, which "alone preserve the way and procedure of a science. For they always proceed from particular foreknown principles to the conclusions to be demonstrated, which is the proper duty and office of a doctrine or discipline, as Aristotle, *Posterior Analytics* I, also testifies."[116] Notably absent is any discussion of the thorny issue of the causal status of its demonstrations. Dear summarizes the issue as follows:

> It is important to stress that the issue went beyond the mere making of a few apologetic remarks at the beginning of a treatise before proceeding to the real content. The usual, and most effective, approach was to carry on as if the mathematical discipline in question were obviously and unproblematically a science. The Euclidean theorem form provided a structure already conformable to the ideal of scientific demonstration because it had been Aristotle's own model for a science. Its mere employment therefore went a long way towards bestowing upon its subject matter the mantle of "science." The difficulties lay in persuading the subject matter to fit the formal structure.[117]

Again, we notice the tendency to substitute a purely methodological criterion for the metaphysical worry initially fuelling the discussion. Yet, it is clear that as a philosopher one could not just shrug of this worry, as the suitability of a topic to be treated in a certain way is in the end simultaneously a methodological and a metaphysical question. Maybe it is not perspicuous that empirical objects cannot be treated mathematically, but the converse does not thereby become established automatically.

2.1.2.5 *"For nature in the trunks of trees strives after the figure…"*

That the circle of mathematicians around Clavius must have felt this uneasiness is testified by the contribution of his pupil Blancanus, who in his 1615 *De mathematicarum natura dissertatio* explicitly takes up the metaphysical and methodological problem.[118] He tries to provide for a view of mathematical entities that could underwrite the claims to certainty and scientificity on behalf of mathematical demonstrations. To this end he especially stresses that mathematical definitions are truly

[116] Quoted in Dear 1995, p. 40.

[117] Dear 1995, pp. 41-42.

[118] The *Dissertatio* has been translated as an appendix in Mancosu 1996, to which I refer in the following as *Dissertatio,* with page numbering as in Mancosu 1996.

essential and not just nominal as implied by writers such as Piccolomini and Pereira. (To argue this point he makes the distinction between quantity per se, and quantity insofar as it is delimited, i.e. as making up particular figures and number. Mathematical definitions are essential with respect to the latter perspective on quantity, not with respect to the former, which remains the domain of natural philosophy and metaphysics).[119] As a result, he can argue that mathematical demonstrations do use formal causes. That they moreover also involve material causes follows from his view on quantity as delimited intelligible matter. To settle his case most convincingly, Blancanus adds an appendix in which he analyzes the forty-eight demonstrations contained in the first book of Euclid's *Elements*, showing for each of them the kind of causes involved.

Because of this metaphysical underpinning, Blancanus can now argue that mathematical demonstrations are most perfect and *potissima* in that they do essentially reflect the *ordo essendi* of (mathematical) things.[120] And because of his distinction between quantity per se and delimited quantity, he has made an important move towards a more fine-grained understanding of the applicability of mathematics in the empirical world as well. But at the same time he admits that there still remains a gap between the intelligible realm of pure mathematics and the messy forms of empirical nature. However, from his perspective the important thing is that this does not stain the image of mathematics – it is at most an imperfection of nature.

> We should know that even if these mathematical entities do not exist in that perfection [of absolute exactness], this is merely accidental, for it is well known that both nature and art intend to imitate primarily those mathematical figures, although because of the grossness and imperfection of sensible matter, which is incapable of receiving perfect figures, they do not achieve their end. … Therefore, even though these [perfect mathematical figures] do not exist in the nature of things, since in the mind of the Author of Nature, as well as in the human mind, their ideas do exist as the exact archetypes of all things, indeed, as exact mathematical entities, the mathematician investigates their ideas, which are primarily intended per se, and which are [the] true entities.[121]

It is not far-fetched to recognize important Platonist elements in passages like these.[122] But it is also important not to forget that they always remain inscribed in an overall Aristotelian framework, as was mandatory for all Jesuit thinkers.[123] A little further in his *Dissertatio*, e.g., Blancanus seems to agree that in the end mathematics only deals with accidents. But he also adds the important caveat, "that it is better to get to know innumerable, marvellous truths about an accident, than always to be cast from one side to the other, by the whirlpool of a thousand of opinions and dissensions, especially

[119] *Dissertatio*, p. 195.

[120] Ibid., p. 184.

[121] Ibid., p. 180.

[122] Cf. Galluzzi 1973.

[123] See again Baldini 1992 (chapter 1).

concerning material substance, and hence never to arrive at the cognition of any substance at all."[124] This admission thus carries a double message: there remains an essential difference between natural philosophy and mathematics – but so much the better for mathematics. This is not all, as "in applied mathematics the case is different, where it is not bare quantity, but either the heavenly bodies, or musical sounds, or the modes of vision and deception, or the powers of machines are studied, *with the same ends in mind and with the same scope as in other subjects studied by other philosophers*."[125] Having cleared the opposition against the status of mathematical demonstrations, Blancanus feels free to ascribe a completely unproblematic status to mixed mathematics, which he claims is perfectly capable of giving *propter quid* demonstrations. We have already seen, however, that he also claimed that a gap remained between perfect mathematical entities and natural objects. He is silent on how these two views are supposed to sit together, although there are some hints in his text. The idea that mathematical entities function as archetypes is further clarified as follows:

> For nature in the trunks of trees strives after the figure of the cylinder, in apples and grapes after spherical or spheroid figure, in the cornea of the eye after circle, indeed, the eye itself is most spherical. The sun and other stars are agreed on all hands to be entirely spherical; the surface of the water is globular, and also the earth itself, were it not for the coarseness and diversity of its matter, would obviously take on a round shape. … But art even more obviously follows these figures; since craftsmen endow almost all their artifices with quadrangular or round figures, or with circles or ellipses. Indeed, art itself, not unlike nature which it imitates, is [also] defrauded of its proper end by the coarseness of matter.[126]

Astronomy is immediately exempted from the material corruption, and eminently suitable to be treated mathematically. Both earthly natural and artificially created objects are supposedly still amendable to such treatment, since they "strive" towards these perfect forms. But the remaining divergences constitute the blind spot of Blancanus' peculiar blend of Aristotelian and Platonist elements. He is unable to say anything sensible on their status, and so passes over them in silence. Since he starts from an abstractive view on the nature of mathematics, he must countenance a metaphysical gap between both realms of reality, without being able to see the divergences as possible challenges that can be turned into problems to be solved. At the same time he thus leaves the door open for Pereira's critique to retain its appeal.

[124] *Dissertatio*, p. 202.

[125] Ibid., p. 202. (My emphasis.)

[126] Ibid., p. 180.

2.1.2.6 *"Veritas inspectio non attingatur ab homine sine lapsu Divinitatis in mente nostrum"*

The last sixteenth-century philosopher to whom we now turn occupies a special place, as he was a colleague and friend during Galileo's time as professor of mathematics in Pisa.[127] The authorities of Plato and Aristotle had already been invoked in different respects by the authors participating in the debate on mathematical certainty, but Jacopo Mazzoni took a comparison of both Philosophers as the explicit starting point in his *In universam Platonis et Aristotelis philosophiam praeludia, sive de comparatione Platonis et Aristotelis*, published in 1597. He is not so much interested in reconciling both philosophers, but rather in learning something from them (and from their differences): he compares them with "skilful hunting hounds which accompany the searcher after truth on his traverse of the wide expanse of being."[128]

Mazzoni accepts the irreducibility of mathematical and physical demonstrations. The latter crucially involve the four Aristotelian causes, which are absent in the former. He is willing to admit that it can be claimed that mathematics uses formal causes, but only if it is understood that this is not in the strict Aristotelian sense, as mathematicians are not dealing with the form of substances. Mathematical definitions simply do not state qualitative essences or final ends. In this respect, Mazzoni thus seems to side with Piccolomini and Pereira, but at the same time he reprimands Aristotle for not having paid enough attention to mathematics. He is clear on the fact that he deems mathematical demonstrations to be useful and valid on a number of scores. This judgement is also extended to the mixed sciences, which crucially involve mathematical demonstrations rather than the Aristotelian causes on which they must remain completely silent, as all mathematically derivable conclusions find their origin in the definitions of the employed geometrical figures. The difference with pure mathematics is that mixed mathematics is answerable to the conjunction of reason and sense, rather than to reason alone. Frederick Purnell concludes:

> In consequence, there are, at times, two approaches open to the physical investigator. Within the framework of the traditional Aristotelian natural philosopher, he can approach a problem with an eye to arriving at an explanation according to the old four-causal scheme. Or, with proper training in the mathematical techniques, he can seek to develop a solution based upon mathematically definable characteristics.[129]

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[127] On the relation between Mazzoni and Galileo, see Purnell 1972, which also contains a useful summary of Mazzoni's views on mathematics, as does Galluzzi 1973. But, again, De Pace 1993 (chapter 4) provides the most full-fledged and balanced treatment.

[128] Purnell 1972, pp. 275-276.

[129] Purnell 1972, p. 281.

Now, as this mathematical approach cannot fall back on revealing the qualitative structure of the world as its *raison d'être*, Mazzoni also offers an alternative "Platonist" metaphysical grounding to its procedures, which ultimately leads to the idea that reality can be thought as structured in geometrical forms that are the manifestation of the Divine rationality. At the same time he maintains that only Aristotle believed in the possibility of a science of the empirical world, which for Plato (and, as we saw, Barozzi) was only limited to the world of ideas. The task of revealing these geometrical structures underlying the flux of empirical reality is still the domain of the *physicus*, which again involves the task of abstracting away from all ever-present material impediments.[130] Yet Mazzoni stresses that it will not do to think of matter as defective and playing an entirely negative role, since this would contradict its Divine origin (remember that God created the whole world, including its matter, *ex nihilo*). To counter this corrupted view, he develops an interpretation of matter in which it is thought of as "susceptaculum perfectionis,"[131] which is animated by a natural appetite to receive particular forms. And man, as investigator of the world, is capable of retrieving these forms, because he was created in God's likeness and has the archetypes after which the world was formed impressed in his mind: to separate the ideal forms from the material impediments, he only has to appeal to these God-given ideas within.[132] Mazzoni does not leave the door open for Pereira's critique – God has closed it for him. Of course, Blancanus also seems to hint at the same kind of closure, although he is less explicit on this point (probably because of the Jesuit context in which he was working, which made it less easy to appeal in such overt manner to Platonist ideas).

This does not alter the main point I wish to make, however. This kind of closure is bound to remain highly controversial, and only seems to further the repression of the impediments themselves. They disappear in the fissure that is left between the archetypes and the actual things in the world, and remain fundamentally unthinkable in their own right. In a traditional Neo-Platonic vein, Mazzoni defines all evil as mere privation, refusing to assign it a positive cause.[133] It is in the same movement of thought that the impediments are bound to vanish from sight.

[130] "Ut itaque in ista quaestione inter Platonem et Aristotelem iudicium nostrum interponamus, dicimus meliorem nobis videri Aristotelis opinionem, idest res naturales quoad formam adeptam carere fluxu illo perenni […] et proinde *remoto impedimento sempiternae mutationis eas scientiae subiectum accomodatum esse posse*." Quoted in Galluzzi 1973, p. 70 (my emphasis).

[131] De Pace 1993, p. 280.

[132] "veritas inspectio non attingatur ab homine sine lapsu Divinitatis in mentem nostrum." Quoted in De Pace 1993, p. 305, fn. 92.

[133] "Ex defectu ergo, et privatione malum oritur." Quoted in De Pace 1993, p. 294.

2.1.3 Mathematics and the empirical world

It should be clear that in the foregoing I have not attempted to give a fully balanced historical treatment that could do justice to the often rather subtle positions of the authors involved in the *Quaestio de certitudine*. I have rather been interested in what I take to be revealing structural features of the development of these discussions that should allow us to better assess Galileo's positioning on these issues. In the present subsection, I will try to bring some of these features further into the open, again without pretending completeness in any respect.

To begin with, let me suggest that we take Mazzoni's utterly eclectic position as symptomatic for an important characteristic of the *Quaestio…*. All parties could have recourse to ancient authorities and point to passages in Euclid's *Elements* that were supposed to confirm their own principal theses, and such attribution of their views to these authorities was an important and indispensable element in their argumentative strategies. But it makes no sense to try to capture the full complexity of the sixteenth century views on these matters under simple common denominators such as Aristotelian or Platonist.[134] At the same time, we must not be blind to the fact that these authors themselves often had recourse to these labels. It was common to refer to Aristotle's retort that Plato had been too much enchanted by mathematics to either express one's agreement or disagreement, thus placing one's own view in one of both camps. However, it is clear that these authors were creatively working out solutions to what they took to be pressing problems (one important constraint on any acceptable solution being that it should be able to claim ancient authorities as forerunners). These problems had to do with the contentious relationship between mathematics and the empirical world that had come to the foreground in the sixteenth century because of the steady rise of the use of mathematics in solving empirical question (as we will see in section 2.2). It is no accident that Piccolomini's opening shot in the *Quaestio…* was appended to his paraphrase of the pseudo-Aristotelian *Mechanical questions* which, as we will see, occupied a prominent place in this "Renaissance of mathematics."

The inextricable tangle of methodological and metaphysical questions at first makes it hard to decide what to make of these debates on mathematical certainty. There is nevertheless some common ground that we saw recurring in otherwise diametrically opposed authors. Most important is the idea that mathematical objects are somehow separated from the empirical world – albeit authors as Catena and Mazzoni defend that they nevertheless can be reinjected into it. As a result, the debate presupposes a picture of nature which contains multiple levels of reality. The most pressing problem then was how to either mediate or protect the ontological border between physics and mathematics, and to assign them their proper places. These differentiated ontological levels also seemed to carry their own methodological requirements. An important evolution in the debates on mathematical certainty is the growing tendency to consciously separate both methodologies *as fit to their own ends*. Instead of

[134] See Schmitt 1983 for the generally eclectic character of all Renaissance philosophy.

measuring the scientificity of mathematics by an Aristotelian yardstick, one could also stress the rigor of mathematical procedures, which moreover sits comfortably with whatever metaphysical underpinning – although it is clear that all authors defending mathematics' value felt obliged to provide such underpinning. Piccolomini's attack, and the discussion following thereupon, brought a potentially far-reaching philosophical option into the open: mathematics has nothing to do with the Aristotelian causes, but that is not necessarily a problem.

Of course, philosophers such as Pereira could admit that mathematics has its own methodology, while continuing to stress that this constitutes exactly its problem. And as long as one operated in an environment that was organized around an Aristotelian "world view", such as the Jesuit society clearly was, this was bound to remain a valid complaint.[135] Yet the existence of the category of mixed sciences within the Aristotelian logic of science at the same time provided enough justification for mathematicians such as Clavius to continue employing their proper methodology, and to try out treating as much phenomena on a mathematical basis as they could, whatever the final verdict on its demonstrations might turn out to be. The fact that there was no clear consensus made room for ignoring the issue to a certain extent. But as already noticed, Blancanus' explicitly metaphysical and methodological justification of mathematics proves that this was only true to a certain extent – at least for writers in an academic context. His contribution to the debate is clearly directed at overcoming Pereira's objection: mathematics only appears to have a completely different methodology, but it can actually be completely inscribed within the four-cause syllogistic framework. On this view, the only real difference between natural philosophy and (mixed) mathematics becomes one of subject matter ("with the same ends in mind and with the same scope as in *other* subjects studied by other philosophers"[136]). That such integration could be partly successful proves the flexibility of the Aristotelian philosophy, within which it was consequently possible to recuperate much of the results of the new mathematical sciences.[137]

There are some important things to be noticed about this incorporation of the mathematical sciences within an Aristotelian framework, however. As already argued by Peter Dear, Blancanus' tactic actually depends on a strict surveillance of the boundaries between mathematics and natural philosophy.[138] It does reclaim some traditional physical questions for mathematical treatment, but it does so by being more explicit on the distinctions between delimited quantity and qualitative substances (of course, always accompanied with the important suggestion that the former can be treated with an incomparably higher degree of certainty). The following statement by Mazzoni could have been made by the Jesuit as well: "Cuius dicti ratio est, quia omnes Mathematicae, et etiam mixtae,

[135] Cf. Baldini 1997 for a quick sketch of the development of philosophy and science within the Jesuit society during the seventeenth century.

[136] Cf. the quote in section 2.1.2.5.

[137] This fact has been much stressed since the 1990's by writers such as Peter Dear.

[138] Dear 1995, especially chapter 6.

ut Astrologia et similes, *nullam aliam rei quidditatem agnoscunt*, nisi eam, quae ex definitionibus figurarum Mathematicarum emergit."[139] That Blancanus goes on to stress that these definitions do state the essence of the figures treated, whereas Mazzoni rather stresses the difference with what he understands to be strictly Aristotelian formal causes, then shows that different epistemic strategies could be grafted onto this setting opposite to each other of mathematical properties and substantial qualities. The autonomy of mathematics can be protected by exploiting the existence of the category of mixed science within an Aristotelian framework, or it can be used to override the claims of the natural philosophers completely by simply claiming its superiority (the direction in which Mazzoni is clearly moving – at least when it comes to treating local motion)[140]. The choice obviously depends on the broader context within which one is working.

I already pointed out that these debates were not held in some kind of academic vacuum, but that they were actually fuelled by what these philosophers perceived as a real challenge: the ever more present use of mathematics in a host of different applications. In the next section we will see how the mathematicians gradually had established their right to speak on empirical matters, claiming a distinct form of knowledge for themselves. Significantly, this process was largely independent of explicit metaphysical pictures. Hence, I suggest that we can best understand Mazzoni's and Blancanus' roles as that of important mediators who translate the outcome of this process back into a respectable philosophical idiom. They provide a philosophical discourse which can be used to grant philosophical legitimacy to the burgeoning mathematical disciplines – a discourse which then can be mobilized in both directions indicated. But because this discourse is structured by the basic *difference* between ideal entities and empirical objects, this translation at the same time involves the loss of a relevant feature of the mathematician's knowledge. In chapter 9, we will see how Galileo opts for a differently structured discourse in his attempt to legitimize his mathematical sciences of nature.

2.2 The mathematical science of mechanics in the sixteenth century

2.2.1 The nobility of mathematics

While mathematics' status as an Aristotelian science was being vigorously debated during the sixteenth century, it could already claim to be a respectable discipline in its own right. The humanists' interest in recovering ancient sources of knowledge had not excluded mathematical treatises, and both Euclid and Archimedes went trough new and careful editions during the sixteenth century.[141] As documented by Paul Lawrence Rose, there existed close and important links between leading humanists and mathematicians who often shared the same patrons. The writings of an early figure such

[139] Quoted in De Pace 1993, p. 271.

[140] Cf. chapter 4, section 4.1.1.

[141] See Rose 1975 for a careful study of the recovery and editorial history of mathematical treatises during the Renaissance.

as Regiomontanus already exemplify many of the treats that would become characteristic in the carving of a self-image for a certain class of mathematicians. The German mathematician stresses the pedigree of his own work in a rich and continuous tradition spanning great men and many nations, and he opposes the consensus that holds among mathematicians regarding their conclusions with the perpetual disputes that rage among philosophers.[142] Both aspects would become recurring themes during the sixteenth century, with the former being especially important in establishing the nobility of the discipline, whereas the latter seemed to have had an almost therapeutic value that allured to many minds, as is testified by Regiomantus giving us "the worthy testimony of Giovanni Bianchini, who very recently said, 'Ten years ago I would have lain helpless, deprived of my life, were it not that the sweetness of astronomy maintained my spirit';"[143] or by Baldi's story of Commandino's change from the profession of medicine to mathematics because he had become disillusioned by the uncertainty of medicine after he had lost both his son, wife, and father after sudden illnesses.[144] Many sixteenth century examples of similar oratories of praise, meant to encourage the study of mathematics, could be given, including famous names such as Peter Ramus, John Dee, and Tycho Brahe.

The landscape of sixteenth-century mathematical practice was of course much diversified, and not all mathematicians pursued the same agenda or even shared a set of common skills.[145] But unlike a large number of these practices that could be simply shrugged aside as dealing with lowly human affairs, useful for pragmatic goals at most, such as book-keeping or land surveying, the kind of mathematics that was promoted by men as Regiomontanus and Commandino often seemed to meddle with topics which were commonly taken to be in the domain of natural philosophy. Such was the case with mathematical optics, harmonics, and especially with astronomy. Taking moreover into account the respectable contexts (university and court) in which many of the practitioners of these mathematical sciences operated (among whose ranks were moreover to be counted men of high social status such as Guidobaldo del Monte), their claims to the nobility and certainty of their discipline could amount to an implicit attack on the prerogatives of natural philosophy.

As we saw, the potentially conflicting interests of mathematicians and philosophers were mainly managed by holding on to what was taken to be an Aristotelian division of the different areas of knowledge; a division that was already enshrined in the university curriculum and further developed in e.g. the educational policy of the Jesuits. Such a division seemed both to secure philosophy's primary status, while it left open a wide enough space of knowledge for the mathematicians through the category of the mixed sciences. To many mathematicians this must have been an alluring situation, no doubt also because of the inherent flexibility of the boundaries, which proved great enough to

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142 Rose 1975, pp. 95-97. See also Swerdlow 1993.

143 Quoted in Swerdlow 1993, p. 151.

144 A story recounted in Rose 1975, pp. 187-188.

145 See Biagioli 1989 for a sketch of the landscape of sixteenth-century mathematical practitioners in Italy, as well as for the changes that occurred in that landscape during the century.

accommodate conflicting views concerning their precise import (as should be clear from the preceding section). Yet we will also see how certain mathematicians were willing to overstep the boundaries that were nevertheless clearly imposed, precisely under the aegis of the greater nobility and certainty that they ascribed to their own discipline. Galileo of course was one of them. So let us try to ascertain the import of this nobility just a bit more in detail for our topic of concern: the science of mechanics.[146]

2.2.2 Of Princes and principles

Among the most significant consequences of the Renaissance of mathematics were the recovery, publication, and close study of an ancient corpus of writings on the science of mechanics (as already mentioned in the first chapter primarily meaning the theoretical study of the simple machines such as lever and pulley). Both the *Mechanical questions,* which were generally ascribed to Aristotle (but are now no longer believed to be his work), and Archimedes' *Equilibrium of planes* and *On floating bodies* were among these writings, and they would prove to be very consequential on a different number of scores.[147] In the next chapters, I will bring out some of the important conceptual breakthroughs they would make possible, but in this section I will be primarily interested in the effect of the existence and general availability of these writings on the image of mechanics as a noble science.[148] To add some flavour to my summary discussion of different aspects of this issue, I will in footnotes give some illustrative quotations from the preface to Guidobaldo del Monte's *Mechanicorum liber*, the most respected treatise on mechanics from the second half of the sixteenth century.[149] This at

[146] As already announced in the preface, I focus exclusively on Galileo's science of motion in this thesis. It need not be stressed that the developments sketched here were of course much wider and also especially significant in the field of astronomy. It is clear that this could not fail to have repercussions on the perception of a field such as mechanics (whose practitioners often overlapped, as can e.g. be seen in the case of Clavius and Galileo). As is clear from the pioneering study of Westman 1980 there are important similarities with the process through which the "astronomer's role" became gradually redefined through the sixteenth and the beginning of the seventeenth century (especially the place of court culture played therein). Yet there are also some significant differences which will transpire from the discussion in the next subsection and which have to do with the crucial role of what I will call cognitive control; a role which hopefully will become much clearer in the chapters to follow.

[147] For the history of the pseudo-Aristotelian treatise in the Renaissance, see Rose and Drake 1971 and Laird 1986. For Archimedes, see Clagett 1978 and Laird 1991.

[148] For the following I am much indebted to Rossi 1962; Keller 1972, 1976; Rose 1975; Westman 1980; Moran 1981; Bennett 1986; Laird 1986, 1991; Biagioli 1989; Laird 1991; Vérin 1993; Smith 1994; Cuomo 1997; Long 1998; Henninger-Voss 2000, 2002. Especially the brilliant work of Hélène Vérin contains a wealth of information and insights on the image and practice of the mechanical sciences during the later Middle Ages, the Renaissance, and the early modern times.

[149] The examples will be taken both from Guidobaldo's own preface to his 1577 treatise, and from Filippo Pigafetta's dedication and preface to the Italian translation that he published in 1581. (All translations are taken from Drake and Drabkin 1969, to which the notes will directly refer.) For a fascinating analysis of the differences between both texts, see Keller 1976; Henninger-Voss 2000. These subtle but undeniable differences are relevant to the overall picture I am attempting to draw here, but for reasons of brevity I shall not comment upon them.

once introduces Guidobaldo, to whom we shall return frequently in the next chapters (especially chapters 3 and 5).

The fact that Aristotle himself was believed to have devoted a tract to mechanics could not but elevate its status. Whereas the name of mechanic was often taken to refer to someone engaged in lowly affairs, this most noble predecessor who was moreover "the leader of all philosophers" was enough to dispel all doubts regarding the worth of the discipline.[150] Many other illustrious forerunners of non-suspicious stature were usually added, with special attention for Archimedes. The eminently theoretical and rational character of the latter's writings were moreover often stressed, with a clear bow to the certainty that accrues to all things mathematical – a most noble thing indeed.[151] When considered from the perspective of the traditional division of the sciences, mechanics was moreover clearly a contemplative rather than an operative science.[152] It provides the causes and principles behind the successful operations of machines such as the lever and the pulley.[153] The introduction of the *Mechanical questions* significantly claims that the mathematical speculations allow us to discover "the how" of mechanical problems (whereas "the about what" is known physically).[154] Mechanics clearly allows mathematicians to gain knowledge of reasoned facts – even in the possible absence of a clear view on the status of these reasons, as testified by the debates in the *Quaestio de certitudine.* Another

[150] "And as for certain manipulators of words who deprecate mechanics, let them go and wipe away their shame, if they have any, and stop falsely charging [mechanics with] lack of nobility and lack of usefulness. If they still do not wish to do so, let us leave them, I say, in their ignorance; and let us rather follow Aristotle, the leader of all philosophers, whose burning love for mechanics is sufficiently proved by the acute *Questions of Mechanics* which he gave to posterity." (p. 243.) For the different associations of the name "mechanic" and "engineer" during the Middle Ages, see Vérin 1993 (chapters 1 and 2). For the Greek origins of the term, see the first two chapters in Micheli 1995.

[151] "For if we hold that *nobility is related* both to the underlying subject matter and *to the logical necessity of the arguments (as Aristotle on occasion asserts)*, we shall doubtless consider [mechanics] the noblest of all. It not only crowns and perfects geometry (as Pappus attests) but also holds control of the realm of nature." (p. 241; my emphases.) Notice the stance that Guidobaldo is implicitly taking with respect to the issues discussed in the *Quaestio de certitudinem.*

[152] For the Medieval distinction between contemplative and practical sciences, see e.g. Weisheipl 1965. This distinction of course goes back to the *Nicomachean ethics* of Aristotle.

[153] "Certainly this science is of the highest theoretical value and of subtlest structure, for it deals with that part of philosophy which treats of the elements in general, and of the motion and rest of bodies according to their positions; thus we assign the cause of their natural movements, and thus by machines we force bodies to leave their natural places, carrying them upward and in every direction, contrary to their nature." (p. 248.) Notice the unproblematic assimilation of mechanics to a "part of philosophy." This passage from Pigafetta's dedication of his translation is an almost literal rendering of a passage in Pappus' *Mathematical Collections* (see Pappus 1878, p. 1023), as is the case with much of Pigafetta's dedication.

[154] Aristotle 1963, pp. 330-331. (Hett, inaccurately, has "the method is demonstrated by mathematics" in his translation; see Micheli 1995, p. 24, fn. 13. Micheli quotes the following sixteenth century translations: "porque el como es manifiesto, por las mathematicas, y el de que por las naturals" (de Mendoça); "etenim quod ipsum quomodo ad mathematica pertineat: ipsum vero circa quod, ad Physica, manifestum est" (de Monantheuil); "Quandoquidem mathematicum id certe est: ad quaenam referri possint cognoscere; physicum vero: quidcirca versentur" (Fausto).) This characterization is of course part of the positioning of mechanics as a mixed science (cf. section 2.1.1). See also chapters 3 and 5.

crucial feature that could be borrowed from the pseudo-Aristotelian *Mechanical questions* was the *topos* of curiosity. The author stresses that mechanical phenomena are truly remarkable in nature, especially the fact that a greater weight can be raised by a lesser. The main thrust of the treatise then is to explain rationally how these wondrous phenomena come about, a most philosophical activity, indeed.[155]

The mathematical practitioners were not only eager in setting apart their business from that of the "mere" mechanics, who only possessed know-how but no knowledge of true principles. They also often self-consciously stressed what (together with its higher certainty) separated their knowledge form that of the philosophers: its practical utility.[156] And even better: its practical utility in issues of great concern for those in power.[157] Consider: they could *control* the physical power that can be exercised through the use of machines, so useful both in times of war and peace.[158] This was especially relevant given the new ways of warfare that were the consequence of the introduction of the cannon: among other things this changed the design of fortifications, which from now on required substantial mathematical skills,[159] and this posed as well many problems in transportation.[160] As a result, mathematics and mechanics entered the education of young aristocrats in the sixteenth century.[161] This promise of practical utility nicely dovetailed with the fact that from the fifteenth century onwards, the "practice and representation of rulership came to be closely associated in particular ways with technological power and the mechanical arts."[162] It is important to stress that this was a case of both practice *and* representation: the utility of mechanical knowledge could only be so openly advertised

[155] Compare the passage from the *Metaphysics* I, 982b, where Aristotle famously declares that "it is through wonder that men now begin and originally began to philosophize". Tybjerg 2003 shows how this aspect was already exploited to elevate mechanics to a status on a par with philosophy by Hero of Alexandria. The cognitive category of wonder will be examined more closely in chapter 5.

[156] In this respect they were part of a general humanist movement that was primarily interested in knowledge that could have practical pay-off. On the relations between science (as we now conceive of it) and humanism, see Cochrane 1978 and Long 1988.

[157] Consider especially the opening address of Guidobaldo's preface where he dedicates his work to Francisco Maria II, the duke of Urbino. "There are two qualities, Illustrious Prince, that are usually very effective in adding to men's power, namely utility and nobility." (p. 241.)

[158] "It not only crowns and perfects geometry (as Pappus attests) but also holds control of the realm of nature. For whatever helps manual workers, builders, carriers, farmers, sailors, and many others (in opposition to the laws of nature) – all this is the province [*imperium*] of mechanics." (p. 241.)

[159] Galileo was one of the many mathematical practitioners who taught fortification to young noblemen as an important source of income; Guidobaldo's only paid job was as overseer of the Tuscan fortresses (for only a few years). Others involved in fortification include Albrecht Dürer and Simon Stevin. For more on the topic of fortification and the role of mathematical practitioners, see especially chapter 4 of Vérin 1993.

[160] Keller 1976, p. 24.

[161] Biagioli 1989, p. 45. Moran 1981 discusses the cases of some sixteenth century German prince-practitioners.

[162] Long 1997, p. 3.

and offered to the powerful because it was at the same time noble knowledge.[163] (In this respect, it is relevant to note that Archimedes reputation was as much that of an artificer, involved in the production of war-machines, as that of an abstract mathematician.[164]) The mathematicians claimed that they were able to take the experiential knowledge embedded in the artisan practices and render it universal, certain, and disciplined, thanks to recovery of the ancient science of mechanics. As a result, they could occupy an extremely interesting place, as mediators between on the one hand the low world of labour and practical exigencies, and on the other hand the high world of more lofty exercises of power.[165] This mediation involved crucial questions of control wherein the mathematicians' highly disciplined way of reasoning could find a natural and fruitful place. Their promises of their ability to *calculate* many interesting properties of machines (or of the path of cannonshots, as in Tartaglia's *Nuova scientia*; or of the exposure of city walls to these cannonshots, as in the many treatises on fortification) in anticipation of their actual operation at the same time held promises of a greater efficiency, and of a greater independence of the particular artisans' know-how. This was moreover accompanied by the prospect of being able to offer novel inventions, again as a result of the application of some fundamental mathematical principles.[166] It is only now, after that the mathematical science of mechanics has been restored to its former nobility, that the prospects for some control over

[163]Consider Guidobaldo's addressing the duke of Urbino, to whom his *Liber mechanicorum* is dedicated, "from boyhood you were so inflamed with a passion not only for all studies, but especially for mathematical studies, that you would consider your life bitter and unhappy unless you had embraced them. And then, occupied in the study [of mathematics], you passed the first part of your life in gaining an understanding of the subject, and often raised your voice, as was worthy of a prince, to say that you were especially fond of mathematics for the reason that mathematics in particular can emerge from that domestic and private kind of life into the sun and dust as they say. And, indeed, in clear proof of these [public] interests would be the ardent desire for military skill that you manifested from early youth." (p. 246.)

[164] Guidobaldo significantly has a long passage in which he sums up all of Archimedes' superhuman feats as an artificer, but nowhere does he explicitly extol the rational organization of his writings (which he however does in his *In duo Archimedis…* published in 1588, a writing that is aimed at a rather different audience – at different places Guidobaldo e.g. refers to Archimedes' work as providing the "elements of mechanics", an obvious reference to Euclid; del Monte 1588, pp. 2nd of the unnumbered preface, 19, 20, 21). Pigaffeta, whose preface has an even heavier stress on the practical utility of mechanics, goes as far as calling Archimedes "the best of all craftsmen [*il megliore artefice*]" (p. 250). See Laird 1991 for the changing images of Archimedes during the Renaissance.

[165] Henninger-Voss 2000 is a most exciting analysis of some of the complexities involved in this mediation, discussing the case of Guidobaldo and his translator Pigafetta. See also Smith 1994 (especially chapter 2) for the detailed story of such mediation through the figure of Becher, who himself moved from the position of artisan to that of a mathematician controlling the endeavours of his former co-workers.

[166] "It should be added that … the author has contented himself at present to teach (and he is the first Latin writer to do so) by means of easy and plain demonstrations merely the method of understanding and operating the six mechanical instruments, to which all others may be reduced. For these are basic and fundamental, and there may be compounded in various ways combinations of two, three, or more; thus the windlass may be combined with the pulley, the screw with the windlass or the lever, and so on. *This may be done at will by anyone who can proceed with good judgement in various works*." (p. 258, my emphases.)

material progress are restored.[167] The following statement of Guidobaldo, taken from the preface of his very influential books on perspective which he published in 1600, nicely sums up many of the features that were thought to be characteristic of the mathematical sciences. He tells his brother, the cardinal, to whom he dedicates the book that it very befitting for a noble man to study mathematics:

> This is above all true of those [mathematical arts] such as mechanics and perspective and other pre-eminent operative arts, out of which, as from a copious source so many outstanding works of illustrious men have emanated, who have taken their norm and rule for the construction of their works from those mentioned arts, and who most willingly admit that the palm that they have acquired for themselves for their marvellous inventions, must be rightly ascribed to those same arts and once they have accepted the palm that they must wear it. [168]

2.2.3 The *imperium* of mechanics

From this brief description we can start to understand how the nobility of mechanics could be mobilized to overcome/ignore the hesitations that were shown by philosophers in considering any form of (applied) mathematics a science. And as this legitimizing move was not predicated on any metaphysical grounds, it might start to allow for a complete blurring of the ontological distinctions between the different sciences. As it stands, however, this ignores one crucial fact about mechanics that was commonly stressed by sixteenth century writers: it was not merely presented as a theoretical and mathematical science, but also as the science that dealt with motions and effects outside or even against nature.[169] This testifies to the fact that more was needed before one could really breach the boundaries between this mathematical science and natural philosophy. But something important has already happened to these boundaries. The present analysis has indicated how the autonomy of the mathematical sciences was implicitly but self-consciously asserted by the mathematicians. Any boundaries between their science and natural philosophy thus had to be provided from *what could now be perceived as the outside* – the demarcation of natural vs. unnatural motions being entirely a philosophical issue. To put it differently: the applied mathematical sciences are no longer held to be in a subservient position but are now perceived to stand besides natural philosophy. This relative

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[167] "But with the fall of the Roman Empire and the appearance of the barbarians in Italy, Greece, Egypt, and [places] where arts and letters had prevailed, nearly all the sciences declined miserably and were lost. Mechanics in particular was for a long time neglected." (pp. 251-252.) See Keller 1972 for the sixteenth century sentiments concerning the prospects of material progress that were opened up by the restoration of the mechanical sciences.

[168] "praesertim verò earum, è quibus, veluti uberrimo fonte tot egregia illustrium virorum emanarunt opificia, Mechanicae nimirum, ac Perspectivae, praestantioresque operative artes, quae normam, & regulam in suis construendis operibus ab iis sumpserunt, eisdemque mirabilium suorum inventorum partam sibi palmam meritò adscribendam, acceptamque ferendam libentissimè fatentur." Guidobaldo 1600, p. 2r.

[169] "And mechanics, since it operates against nature or rather in rivalry with the laws of nature, surely deserves our highest admiration." (p. 241.) Cf. Laird 1986 for a summary of some sixteenth century views on the issue. See also chapter 5.

autonomy does imply that an intervention opposite to Piccolomini's, where one judges natural philosophy by the standards of these mathematical sciences, also becomes thinkable.[170] A detailed story behind one particular instance of this inversion will be told in chapters 4 and 5; in the present chapter I am rather interested in the conditions of possibility of this inversion.

It must be stressed that the promised practical usefulness and effectiveness did not play a direct role in cognitively justifying mechanics as a science (taking into account the essential difference between on the one hand *praxis* and *techne* and on the other hand *episteme* which seemed to have been generally hold on to during the sixteenth century) – but it did play a crucial role in carving out a socially and culturally interesting place for mathematical practitioners from which it became possible for them to claim such a status.[171] It is rather the implication that a discipline based on ancient principles and fit for princes could not fail to be a science that is the central element in the process of the legitimization of mechanics as a science during the sixteenth century.[172] And the stress on both principles and Princes was bound together through the central issue of control – control of both the knowledge that was up to then primarily embedded in practice, and of its practical effects. This specific constellation suggests that mathematicians' highly disciplined way of reasoning is not merely a formalistic property, i.e. solely related to a possibly empty *ordo cognoscendi*, but that it also has a direct pay-off in its *systematic* relation to experiential knowledge. The specifics of this systematic relation will be further analyzed in the remainder of this thesis, starting with the next chapter, where we will study Guidobaldo's mechanical writings in much detail. For now, let me just point out that we are confronted with a further displacement of metaphysical considerations from the heart of the picture of mathematical science. The stress on the methodological procedures that we already saw present in the *Quaestio…* is not to be limited to reasoning within the realm of intelligible matte or inborn ideas: its true significance transpires only when these reasoning procedures are *put to real work*. As already announced, it will take the whole of this thesis to really assess the import of this brief statement.

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[170] It is probably precisely to pre-empt the possibility of such inversion that Piccolomini appended his discussion on mathematical certainty to his paraphrase of the pseudo-Aristotelian *Mechanical questions.*

[171] There is moreover a further story to be told (some other time – and possibly by someone else) about how this would more and more start to take over the role of cognitive legitimization. Consider e.g. the ubiquity of variants on the argument of inference to the best explanation in present day philosophy of science.

[172] For those worrying whether this does not place too heavy a focus on social factors: consider what would have happened with its claims to be fit for princes if it would have failed completely in achieving any empirical pay-off. But more importantly, it is important not to put the carriage before the horse: the apparently clear-cut distinction that we can see between external (sociological) and internal (epistemological) factors is the outcome of the kind of process that we are trying to describe here (it is part of a modern *savoir*). It can hardly be expected to have been part of the dynamics of this process itself (unless one would be willing to accept some teleological causation). Also keep in mind that we are dealing with the *legitimization of a certain practice as science*; not with the legitimization of some of its particular factual claims – which presupposes that the practice from which they issue is already conceived to have some authority in making such claims. (Remember also the discussion in section 1.3.2.)

In this way, society provided a place for cognitive legitimization of the applied mathematical sciences which would not have been found within a university context. Of course, universities as part of society would partly absorb this evolution (as testified by a figures as Catena, Mazzoni, and Blancanus), but it could not have arisen there. This insight is also of interest when we try to understand the difference that exists between the medieval situation and the one in the sixteenth century with respect to the status and success of these sciences. Whereas there already existed a wide range of metaphysical options during the Middle Ages (consider figures such as Grosseteste and Bacon),[173] this noble space from which to practice and systematize these sciences was something new with the Renaissance. The image of mathematics met contemporary sixteenth century demands that had not existed before (and certainly not on a comparable scale).

The *imperium* of the mathematical science of mechanics had accordingly come to occupy a singular region by the end of the sixteenth century.[174] On the one hand it was consciously positioned *above* artisanal practices; on the other hand it was thought to be noble enough to occupy a position *besides* natural philosophy. Its practitioners could consequently engage simultaneously in a vertical and a horizontal interaction, and could do so exactly because of the distance that separated them from both the other spheres of knowledge. As they were not doing natural philosophy (and certainly not in a traditional vein), they could try to recuperate the manipulative knowledge embedded in the artisanal practices; and as the resultant knowledge was not to be equated with these practices, they could still open up a conversation with philosophers. Of course, both interactions were not straightforward and involved complex negotiations, which in large part fall outside of the scope of the present thesis.[175] But there is no need to stress that the combination of both dimensions would prove to be explosive.

[173] Cf. Weisheipl 1958, 1965; Grant 1996.

[174] The following is my gloss of the important argument made in Bennett 1986.

[175] I must again refer to some of the previously cited literature (and especially to Biagioli 1993 for the case of Galileo). What I will do in the following section is related to the analysis of such negotiations, but significant differently oriented in focus. I will not attempt any micro-sociological analysis of how these negotiations were embedded in the larger social and cultural context that simultaneously made them possible and constrained them. I will rather limit myself to a description of the kind of discourse that Galileo was able to produce as a result of the availability of the kind of position being described here. In the language introduced in chapter 1, section 1.2.2, I will attempt an archaeology of Galileo's science of motion, not a genealogy.

3 Gravitating towards stability: Guidobaldo's Achimedean-Aristotelian synthesis

Guidobaldo del Monte was one of the most famous mathematicians of the second half of the sixteenth century. He wrote on a number of topics, including perspective and astronomy, but he was and is best known for his works on mechanics. Being of a noble family he was a privileged member of the ducal court at Urbino, and as an early patron of Galileo he secured the latter's appointment as professor of mathematics at the University of Pisa in 1589.

As I will argue in detail in the present chapter, it will not do to portray Guidobaldo as a strict Archimedean who tried to "reconcile" his mechanics with Aristotelian notions, as has often been suggested. This is to forget how inherently problematic is the idea of someone being strictly Archimedean, especially given the abstract character of the latter's writings. Whatever the story behind Guidobaldo's humanistic interest in restoring the ancient science of mechanics, it required a lot of creative and insightful interpretation. Many conceptual choices had to be made which could not be read off from the ancient sources, e.g. how to make sense of Archimedes' proof procedure in his proof of the law of the lever. As will become clear, we cannot simply identify different sources for our and sixteenth-century writers knowledge of (the history of) mechanics with distinct Archimedean (static) and Aristotelian (dynamic) "traditions". Let me accordingly stress the intrinsic interest that the work of Guidobaldo should hold for anyone interested in sixteenth and seventeenth century mechanics. Since this work has not received the detailed attention it deserves, however, I hope that the present chapter can remedy this situation.

The resulting picture of Guidobaldo's science contains many elements that are directly relevant for what I have called an archaeology of Galileo's science of motion. As will become clear in the next chapters, the interplay between empirical and theoretical considerations that characterizes Guidobaldo's exemplary instantiation of the category of mixed sciences will prove to be especially relevant. But the specific conceptual structure that underlies his mechanics will also turn out to be very interesting when we will discuss Galileo's effacement of the Aristotelian distinction between the natural and the artificial in chapter 5.

3.1 Guidobaldo del Monte and the science of mechanics

3.1.1 Traditions in the history of science

Until recently, Guidobaldo del Monte was mainly treated as a transitional figure in the history of science. His contributions in reviving the ancient science of mechanics were often praised, whilst his inability to see beyond the ancients was much deplored. Whereas Pierre Duhem's derisory description of Guidobaldo's oeuvre as "sometimes in error, always mediocre"[176] found its direct echo in the work of the French historians of science Pierre Costabel and René Dugas,[177] Anglo-Saxon historians of science tended to be slightly more positive in their judgement. Yet both Paul Lawrence Rose and Stillman Drake, to name but two of the most prominent ones, did not truly alter Duhem's assessment.[178] They admitted that Guidobaldo's contribution not only restricted the advance of modern science, since he was one of the most influential promoters of a mathematical approach to nature and most importantly an early supporter of Galileo,[179] but they still stressed the many steps he was unable to take which "he would otherwise have been quite capable of making".[180]

It is clear that these negative evaluations of Guidobaldo's mechanical writings are based on a particular historiographical position which favours the vantage point of "classical" mechanics as a norm to judge earlier approaches. As a result it is not surprising that in more recent literature we find important amendments to this picture.[181] By focussing more closely on Guidobaldo's own interests and predicament, these writers have stressed the social position from which he was working, the philosophical and scientific agendas he was pursuing, and especially the interplay between these elements. As a result, we are beginning to have a more nuanced understanding of the reasons why Guidobaldo's mechanics has some of the particular characteristics for which he was so severely criticized by earlier writers.

Much of the (admittedly not very numerous) writings on Guidobaldo's mechanics have been organized around the historiographical categories of scientific traditions or schools. Stillman Drake influentially but controversially distinguished two sixteenth-century Italian schools of mechanics: a Northern group, "conspicuously interested in practical aspects of mechanics", and a Central Italian group that "concentrated its interest on works of classical antiquity and on the rigorous application of mathematics to mechanics".[182] While not questioning the difference in outlook between these groups

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[176] Duhem 1905, p. 226.

[177] Dugas 1950, p. 99; Costabel 1954, p. 10.

[178] Rose 1975 (chapter 10); Drake 1969.

[179] Rose 1975, p. 233; Drake 1969, p. 48.

[180] Drake 1969, p. 46.

[181] Gamba and Montebelli 1988; Biagioli 1989; Bertoloni Meli 1992; Micheli 1995 (appendice II); Henninger-Voss 2000.

[182] Drake 1969, p. 13.

of mathematicians, Mario Biagioli has tried to "uncover the more complex social dimensions of the interaction of these two "schools" and of their quite different conceptual styles".[183] Enrico Gamba and Vico Montebelli take a step further in thoroughly investigating the characteristics and context of the Central Italian group, which was actually organized around the duchy of Urbino. They especially stress Guidobaldo's commitment to the empirical character of mechanics, and link this with the presence of skilled instrument makers in Urbino.[184] Domenico Bertoloni Meli asks us to question the existence of a coherent agenda existing within the Urbino "school", by opposing Commandino against Guidobaldo on a number of central issues.[185] Gianni Micheli points to the fact that Guidobaldo's humanist interest in recovering an ancient science cannot be analyzed separately from his attempts to come to a rational understanding of mechanical phenomena, and vice versa.[186] Mary Henninger-Voss, finally, has paid detailed attention to the ways in which Guidobaldo himself consciously tried to establish a tradition for mechanics, one that at the same time could be based on noble and universal principles, and remain valuable in local artisanal contexts.[187]

By focussing on the notion of a tradition, most of these writers have primarily paid attention to Guidobaldo's conception of what constitutes the identity of the science of mechanics.[188] As such, there are almost no recent extended discussions of the conceptual structure of the science for which Guidobaldo sought to establish an identity.[189] Admittedly, these are two sides of the same coin. But taking this metaphor literally, it might be time to turn the coin and take another look at the bottom side. I will hence try to focus on the actual conceptualizations used by Guidobaldo, and only at the end of my analysis will I refer to his own pronouncements on the nature of mechanics. My primary aim will be to look at the use to which central concepts, such as centre of gravity, are put *within* the confines of Guidobaldo's texts. That is, I am in the first place interested in the coherence that Guidobaldo tried to forge for the domain of mechanics by arguing for a host of relations between different concepts that were somehow connected with the traditional ways of conceiving mechanical phenomena.

3.1.2 Guidobaldo and Galileo

To my mind, there is no doubt that the utility of this kind of exercise in conceptual analysis is partly determined by the position occupied by Guidobaldo as an almost contemporary and at some

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[183] Biagioli 1989, p. 57.

[184] Gamba and Montebelli 1988.

[185] Bertoloni Meli 1992.

[186] Micheli 1995.

[187] Henninger-Voss 2000.

[188] A useful summary of the landscape of sixteenth-century positions on this issue is provided in Laird 1986.

[189] Gamba and Montebelli 1988, part II, provides an exception but as the concept of centre of gravity is not further analyzed there, the author misses an essential part of the fine-structure of Guidobaldo's conceptualization of mechanical phenomena.

point paragon of Galileo. Let me therefore first clarify how the present chapter is situated with respect to the kind of work pioneered by Duhem, which was also focused on conceptual issues. Its prime aim is to investigate Guidobaldo's science as much as possible on its own terms. I will e.g. not posit the existence of a dynamic and a static tradition in mechanics, presumably deriving from Aristotle and Archimedes respectively, as is often done following the lead of Duhem.[190] I will rather try to investigate how Guidobaldo himself interpreted and recuperated the writings of his predecessors. After all, it was only through the work of people like Guidobaldo that such a distinction gradually took a meaningful shape, and in any case it will turn out that it makes no good sense to read Guidobaldo's own writings through such a filter.[191] Yet, in an important sense the work of people like Duhem is still the starting point for my own analysis. Their criticisms did single out some of the most peculiar aspects of Guidobaldo's mechanics. As such they provide some kind of hermeneutic benchmarks from which we can start to reconstitute some of the coherence of Guidobaldo's own conceptualizations of mechanical phenomena.[192]

It is clear that a complete treatment cannot avoid shifting to and fro between the level of conceptual analysis and a broader analysis of the philosophical and social implications of Guidobaldo's "scientific project". Yet by anchoring my analysis as much as possible in a thorough analysis of Guidobaldo's use of certain central concepts, I hope to lay part of the groundwork for a richer understanding of this scientific project than can be attained by focussing primarily on social and philosophical factors. This need not be taken as a sceptical remark towards the previously cited literature. On the contrary, I consider most of the insights reached there as completely compatible with my own analysis.[193] Let me just indicate in what respect I hope to add something substantially to them.

The most central issue surrounding Guidobaldo's scientific project is the relation between on the one hand his adherence to the principles and canons of Archimedean science, and on the other hand his attempts to integrate this within an Aristotelian framework. It is reasonably clear that such a project cannot be understood without taking into account how this was part of Guidobaldo's attempts

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[190] Cf. especially Clagettv 1959, pp. 3-23.

[191] See especially chapters 7 and 8 for the way in which such a distinction gradually became thematized in Galileo's writings. For some further historiographical reflections on the gradual process through which the distinction between statics and dynamics took its present-day shape, see Gabbey 1993.

[192] This is especially true with regard to Duhem's and Costabel's treatments of the status of the centre of gravity (Duhem 1906, chapters 15, 16; Costabel 1954). Their criticisms clearly pinpoint in what sense Guidobaldo's understanding of this notion must differ essentially from a modern understanding. However, this need not be taken as a sign of Guidobaldo's incoherence (as they frequently suggest). It can also be taken as a warning post that if we want to understand the coherence of his science on his own terms, we certainly will have to make sense of these differences.

[193] It is after all the goal of archaeological analyses to see how these different elements *taken together* allow a knowledge to function in the way it does. As explained in chapter 1, section 1.3.1, the present chapter is aimed at understanding some aspects of the internal discursive organization of the mixed sciences by seeing how its concepts are put to use. This is intended to complement the analyses in chapter 2, which are more in line with the secondary literature on Guidobaldo cited in the text.

at forging an interesting socio-professional identity for the practitioners of the “noble science” of mechanics, and it is undeniable that this limits possible choices to be made in developing such a science.[194] However, we need not suppose the interaction to have been one-sided. It is highly plausible to assume that particular conceptual aspects of (Guidobaldo’s interpretation of) both Archimedes’ writings on equilibrium, and the Aristotelian treatise on mechanics helped to shape the particular form this attempted synthesis took. It is the latter suggestion that provides the motivation behind the present chapter.

When I will discuss the traditional Aristotelian distinction between the artificial and the natural as applied to mechanical instruments in chapter 5, we will see how Guidobaldo’s conceptualization of mechanical phenomena nicely fits in this broader philosophical framework. The local discursive organization of his science reflects some crucial elements from the wider discursive context in which it found its place. Yet we will also see how it at the same time contained the crucial elements that would allow someone as Galileo to dissolve this distinction. It occupies a truly pivotal position in this momentous transformation in our understanding of the relation between human agency and objective reality. This only further justifies our paying very close attention to the conceptual fine-structure of Guidobaldo’s mechanics.

One further element of great interest is the kind of interplay between theoretical and empirical considerations that is characteristic of Guidobaldo’s mechanical writings. I think it is time to clear up some serious miscomprehensions concerning Guidobaldo that have been often repeated, especially in the literature on Galileo. Noretta Koertge, e.g., states in a very influential article on Galileo’s use of idealization that Guidobaldo’s work exemplified a “pedantic empiricist program” that counselled “to give up looking for simple ideal laws and try instead to describe actual states of affairs, warts, and accidents and all, in hideous, complicated detail”, but that Galileo “was too good a physicist” to adopt it.[195] William Wallace even goes as far as stating that Guidobaldo “had examined Archimedes’ proof of the balance theorem and had rejected it for its lack of rigor.”[196] It would certainly have outraged Guidobaldo, an admirer of the work of his Greek Master, that someone could ascribe such a position to him. Both Koertge and Wallace are apparently misled by Guidobaldo’s discussion of the complications that arise because of the fact that the lines of descent of weights hanging from a balance are not parallel but actually converge in the centre of the earth. We will see that Guidobaldo’s actual considered position on this matter is much more subtle that anyone has seen up till now. It is only when we are thus freed from ascribing a position to him that he never held that we can properly see how Guidobaldo understood problems that have to do with idealization in developing a mathematical science of mechanics.

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[194] Cf. the description in chapter 2, section 2.2.2.

[195] Koertge 1977, p. 393.

[196] Wallace 1984, p. 241.

3.2 Archimedean elements: Revolving about the centre of gravity

3.2.1 Interpreting Archimedes physically

Guidobaldo's essential contribution to the so-called Archimedean revival of the sixteenth century is beyond doubt. His 1577 *Mechanicorum liber*, which was quickly translated into Italian, incorporated central Archimedean concepts, and in 1588 he published a full-blown paraphrase of and commentary on Archimedes' *Equilibrium of planes*.[197] In this section, I will be primarily interested in Guidobaldo's understanding and analysis of Archimedes' treatise, as it is especially expressed in the latter work.[198]

It is useful to start by reminding ourselves that the extant writings on mechanics of Archimedes provide all interpreters with some serious puzzles, whether these interpreters live in the twenty-first century or in the sixteenth.[199] Most conspicuous is the complete absence of any explicit definition of the notion of centre of gravity, which nevertheless is the most central conceptual element of the *Equilibrium of planes.* Guidobaldo also comments on this in his introduction to his paraphrase of Archimedes.[200] Interestingly enough, his way to deal with this absence parallels the solution of most modern commentators. He has recourse to the definition given by Pappus in the eighth book of his *Mathematical collections*.[201] Of course, Pappus wrote centuries after Archimedes, but as Pappus himself indicates that he is following Archimedes in exposing the principles of mechanics,[202] Guidobaldo could feel secure in claiming that "Pappus does not depart even a nail's breadth from the principles of Archimedes."[203] In a similar vein, most modern writers assume that Pappus had access to lost treatises of Archimedes (Pappus himself quotes at least one such treatise in his *Collections*), which formed the basis for his definition.[204]

However, prefacing Archimedes' treatise with Pappus' definition of centre of gravity is not without consequences. This definition reads as follows:

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[197] del Monte 1577; 1581; 1588. English translation of the first two books, when available, will be given from Drake and Drabkin 1969.

[198] I am not aware of any other detailed study of Guidobaldo's "paraphrasis". The only partial exception is Micheli 1995, which has many references to Guidobaldo's understanding of specific aspects of Archimedes' treatise dispersed throughout the book.

[199] For a sample of the modern literature on Archimedes, see Dijksterhuis 1987; Drachmann 1963; Knorr 1982.

[200] "Cùm itaquè supponat, nos exquisitam habere notitiam centri gravitatis." del Monte 1588, p. 8.

[201] Pappus 1878.

[202] "Haec igitur doctrinae centrobaricae summa esse videtur, cuius elementa ediscas, si Archimedis de aequilibriis libros et Heronis mechanica adieris…" Pappus 1878, p. 1035.

[203] del Monte 1577, unnumbered preface. (Transl. from Drake and Drabkin 1969, p. 244.)

[204] Pappus 1878, p. 1069; cf. Dijksterhuis 1987, Drachmann 1963, and Knorr 1982.

> The centre of gravity of any body is a certain point within it, from which, if it is imagined to be suspended and carried, it remains stable and maintains the position which it had at the beginning, and is not set to rotation by that motion.[205]

This definition brings to attention a set of physical properties which are notable for their absence from the *Equilibrium of planes*. It is indeed surprising how devoid this treatise is of all physical interpretations of its main concepts. Nowhere does Archimedes speak about suspending weights, and even the term "weight" (βαρος) is soon after the introductory postulates dropped for the more neutral "magnitude" (μέγεθος). This is not all, Pappus immediately after giving his definition goes on to explain how we should understand this notion and introduces considerations connecting weight with a tendency for motion towards the centre of the world. Again, Archimedes nowhere gives a hint of any such connection in his treatise. There are even no direct indications of the direction in which the weights or magnitudes are understood to move.

The overall tendency of Archimedes' treatise is thus characterized by a conscious attempt at reaching a level of abstraction as high as possible. Seen from this perspective, the famous law of the lever, stated in propositions 6 and 7, seems to be not so much about physical balancing, but about relating geometrical magnitudes to centres of gravity. And the goal of this exercise becomes clear if we consider the next propositions, which introduce properties of the centres of gravity of parallelograms and triangles. These in turn provide the means for squaring a parabola as is done in the second book of the *Equilibrium of planes*. (After having determined the centre of gravity of these figures, the area of other magnitudes, such as a parabolic segment, can be determined by balancing these figures with the other magnitudes and analyzing the conditions for equilibrium, exploiting the fact that the centre of gravity of the triangle is already known.)[206] The exercise in which Archimedes seems to have been engaged was not so much a mathematization of physics, but a physicalization of mathematics.

Guidobaldo at several points comments on the abstract character of Archimedes' presentation, but he always seems confident to offer a physical interpretation himself. He states e.g. that Archimedes chose to speak about magnitudes because this is a common name for both plane figures and solids.[207] Instead of interpreting this terminology as a sign of Archimedes' desire to avoid physical connotations, Guidobaldo turns it into a means of highlighting these. Indeed, he stresses that the first eight propositions, which form the nucleus of Archimedes' mechanics, are valid both for plane figures and solids (he even goes as far having the accompanying figures in his paraphrase alternatively depict

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[205] del Monte 1588, pp. 8-9. (Transl. from Drake and Drabkin 1969, p. 259).

[206] This is explained in great detail in Archimedes' *Method*, to which Guidobaldo obviously had no access.

[207] "etenim in his semper loquitur vel de gravibus simpliciter, veluti in primis tribus theorematibus; vel de magnitudinibus, ut in reliquis quinque quod quidem nomen tam planis, quàm solidis quibuscunque est comune, ut etiam ij, qui parùm in Mathematicis versati sunt, satis norunt." del Monte 1588, p. 20.

suspended planes and solids).[208] And it is quite clear that he was rather embarrassed by the apparent restriction of the treatise to plane figures, as is testified by his convoluted discussion of the problem as to how we can understand a plane figure, which has no gravity, to have a centre of gravity. His most convincing answer seems to lie in the fact that a solid which has weight, and can be equilibrated by suspension, can be thought to have its point of suspension in its upper plane, whence we can also imagine this plane to be suspended in equilibrium as well.[209] Equilibrium of plane figures is hence made dependent on equilibrium of solids. It is clear that the adoption of Pappus' definition strongly favoured – maybe even necessitated – such a view.

If Archimedes wanted his physicalization of mathematics to succeed, he somehow had to introduce physical elements in his proofs. And indeed, in the proof of the law of the lever we find him implicitly equating "balancing" with being placed around the common centre of gravity. This is a point which Guidobaldo seizes upon to highlight the central role played by the (physical) definition of centre of gravity. In a long introductory section to the proof of proposition 6 (the commensurable case for the law of the lever), he tries to offer an explication of Archimedes' method of proof. Now, this method crucially involves the replacement of a weight (magnitude) on a balance (line) by smaller equal weights (magnitudes), which together weigh as much (have the same magnitude) and are suspended (placed) in such a way that their centre of gravity coincides with the centre of gravity of the original weight (magnitude). It is clear that Archimedes assumes that such replacement does not alter the action of the weights on the balance. (Notice how hard it is not to state Archimedes' procedure in physical terms.) Which of course elicited Mach's criticism that "the entire deduction contains the proposition to be demonstrated, by assumption if not explicitly."[210]

Mach's criticism actually consists of two parts: firstly, Archimedes cannot *prove* that equilibrium is not disturbed if we replace a magnitude by another one with the same weight and centre of gravity, but of different shape; secondly, the actual form of the dependence of the action of the magnitude on its position and weight can only be the linear combination (weight) x (distance), given the actual replacements effected by Archimedes. The second criticism seems rather inappropriate. One can't help but wonder what's wrong with a proof that makes explicit the formal conditions underlying a procedure that is deemed valid on other grounds. The first criticism is implicitly but extensively taken up by Guidobaldo in his explication of the proof method; i.e. he sets out to prove that such a replacement indeed does not disturb equilibrium, and he explicitly states that it is inadmissible to base this proof on the law of the lever.[211] His proof proceeds in three steps, which I will now analyze in

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[208] del Monte 1588, pp. 19-21.

[209] del Monte 1588, 14-16.

[210] Mach 1960, p. 20.

[211] "At verò quoniam demonstrationes ibi allatae indigent, quae Archimedes in sequenti sexta propositione demonstravit, idcirco demonstrationes illae huic loco non sunt oportunae." del Monte 1588, p. 59. (Guidobaldo is referring to demonstrations of some propositions in his *Liber mechanicorum.*)

some detail, as they forcefully reveal how Guidobaldo dealt with the incompletely interpreted formal framework given by Archimedes' treatise by exploiting the physical nature of Pappus' definition. (Incompletely interpreted because the notion of centre of gravity remains undefined, and because many other mathematical elements receive no direct physical interpretation.)

3.2.2 Guidobaldo's physical proof of indifference

The proof procedure under investigation involves the replacement of one weight, say *E*, by two smaller weights, say *B* and *C*, which together weigh as much as *E* and which are placed in such a way that their centre of gravity coincides with the centre of gravity of *E* (see figure 3.1). It has to be shown that both configurations are completely equivalent with respect to equilibrium with a further weight, say *A*.[212]

First, Guidobaldo asks us to imagine that the weights *B* and *C* are suspended *below* the line connecting *A* and *C*. They are connected by a line which in their common centre of gravity is suspended from the line *AC*. Now, since they are suspended from their centre of gravity it follows from the definition of centre of gravity that they will be at rest. As the body composed of the two weights remains at rest, this implies that they are sustained in their centre of gravity by a power which equals their combined weight. Obviously the same power would also sustain the weight *E* if it was suspended from its centre of gravity at the same place. As a result, both the combined weight and the single weight gravitate with their total weight in their centre of gravity.[213]

Next, Guidobaldo places the weights back in the line *AC*. If we now consider on this line the point *D* which is the centre of gravity of the weights *A* and *E*, then it obviously will also be the centre of gravity of the weights *A* and *B* and *C*. Hence the combined weight and the single weight are completely equivalent with respect to equilibrium with the weight *A*.

Apparently not completely satisfied, Guidobaldo moves on to a further consideration. This time he wants to compare the weights *B* and *C* when placed in the line *AC* with the same weights when placed at equal distances around their centre of gravity, but at an angle to the line *AC* (such that the new places, say *F* and *G*, or *H* and *K*, still lie at a straight line going through the centre of gravity – see figure 3.2). Now, since the body composed of both weights when placed in *FG* or *HK* still has the same centre of gravity, which remains stationary, it does gravitate in the same place as it did when placed on the line *AC*. Again the same conclusion follows with respect to the body's capacities for equilibrating the weight *A*.

The crux of the whole line of argument lies in the fact that the complete weight of *any* body can be considered to be concentrated in its centre of gravity. And this replacement is justified through

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[212] del Monte 1588, pp. 55-58.

[213] "Quocumque enim modo eadem gravia sese habent, eodem semper modo in eius gravitatis centro gravitant." del Monte 1588, p. 56.

the definition of centre of gravity due to Pappus. The first and the third step merit some further comments. Peculiar about the first step is the fact that Guidobaldo makes the detour through suspending the weights below the line in which they are actually placed. The reason is that he wants to argue for the equivalence of the two configurations via the equality of the sustaining power, which apparently can be most easily conceptualized if the weights are suspended from above (probably due to the fact that gravity is a natural tendency for motion downwards). This argument is actually the continuation of a line of thought which was already introduced earlier in Guidobaldo's paraphrase, in the preface immediately after the definition of centre of gravity and in the scholium to proposition four.[214] That we have to sustain a body in its centre of gravity if we want to completely stop its natural motion actually betrays a deeper-lying fact about the constitution of the physical world. In an Aristotelian cosmos the natural tendency for downward motion of heavy bodies is due to their striving to be at rest in the centre of the universe. Yet, the definition of centre of gravity teaches us that such a body will only be truly at rest if its centre of gravity coincides with the centre of the universe.[215] But this implies that we can be more specific about this striving of a body: it is the centre of gravity which truly wants to unite itself with the centre of the universe. Which brings us back to the earlier line of argument: if we want to halt a body's natural motion, we have to arrest its centre of gravity, which is the seat of the body's gravitational action.[216]

Even at this place, Guidobaldo is not simply taking over pre-given scholastic metaphysical ideas, introduced to fill in the gap in Archimedes proof procedure. As he is in the first place interested in making sense of this procedure, it turns out that the actual procedure used also shapes the way we have to understand these metaphysical foundations which are accordingly being transformed by their incorporation within this Archimedean context. He is truly trying to forge a synthesis and not merely adding up Aristotelian and Archimedean elements. This becomes clear in a passage in which Guidobaldo raises the worry whether two bodies merely connected by a line can be considered to be

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[214] del Monte 1588, pp. 9-11; 43-44.

[215] Note that this involves a subtle shift of reasoning on Guidobaldo's part. To make this point, he turns to Commandino's definition of centre of gravity, which Guidobaldo always present as completely equivalent to Pappus' definition (he calls it a "descriptionem" of the notion, rather than a definition, presumably implying that Commandino gives a further explanation of how we should understand the actual definition, which is due to Pappus). Commandino's definition, however, nowhere mentions suspension, but only states that the parts of the body on all sides of its centre of gravity will have equal moment ("Centrum gravitatis uniuscuisque solidae figurae est punctum illud intra positum, circa quod undique partes aequalium momentorum consistent" del Monte 1588, p. 9). Pappus' definition, with its emphasis on suspension is rather ill-suited to establish this cosmological connection, since it seems improper to think of the role of the centre of the universe as a point of suspension.

[216] "Quare dum asseritur, grave quodcumque naturali propensione sedem in mundi centro appetere, nil aliud significantur, quàm quòd eiusmodi grave proprium centrum gravitatis cum centro universi coaptere expetit, ut optimè quiescere valeat. … Ex iis omnibus, quae hactenus de centro gravitatis dicta sunt, perspicuum est, unumquodque grave in eius centro gravitates propriè gravitare… Praeterea quando aliquod pondus ab aliqua potentia in centro gravitatis sustinetur; tunc pondus statim manet, totaquè ipsius ponderis gravitas sensu percipitur." del Monte 1588, p. 10.

natural constituents of the physical universe. It turns out to be a sufficient answer that Archimedes considers them as such.[217] If we can ascribe a centre of gravity to any combination of physical bodies, then we can consider them to be appropriately unified. This comes down to: the capacity to be held in equilibrium is what constitutes a body's unity.

We have thus gained a richer understanding of the metaphysical foundations underlying the validity of Archimedes procedure, i.e. the reason why it is appropriate to consider the complete weight of any body to be concentrated in its centre of gravity. A further aspect of this procedure can be brought to light by considering the third step of Guidobaldo's overall argument. This third step crucially involves the fact that Pappus' definition of centre of gravity implies that a body suspended in its centre of gravity will always be in what we now call *indifferent* equilibrium (i.e. no matter what the orientation with respect to that point, the body will remain in equilibrium). It is clear that this has to be supposed for the *de facto* replacement of any body by its centre of gravity to make sense. If this would not be true, then the position in which a body is held would not be indifferent. To stress the relevance of this fact for an appropriate answer to Mach's criticism: if this were not the case, then the form of a body would indeed matter (as made visually clear by the accompanying figure 3.2).

The preceding paragraphs should suffice to show the crucial role played by Pappus' definition in interpreting Archimedes' treatise. It is seen to provide a natural link with an Aristotelian cosmological framework, exactly through the way it functions in making sense of Archimedes' proof procedures. Yet the essentially physical nature of Pappus' definition brings one important weakness for any theory that is built around it: it is hard to give any straightforward existence proof. That is, it is hard to see why it would be necessary at all that a point with these properties actually exists within any physical body. But we have seen that Guidobaldo's interpretation of Archimedes' procedure crucially turns around the existence of a point in which a body can be held in indifferent equilibrium. As the existence of such a point can apparently only be assumed, Guidobaldo's proof seems to be left hanging in the air, suspended from a centre of gravity which might well be non-existent.

3.3 Aristotelian traces: Revolving about the fulcrum

3.3.1 The Aristotelian circle and its centre

The discussion of Guidobaldo's paraphrase of Archimedes' *Equilibrium of planes* made abundantly clear that the latter treatise contains important lacunae from a physical point of view. Guidobaldo had recourse to Pappus' definition of centre of gravity to fill in quite a few of these, but

[217] "Quoniam scilicet recta linea AB eas [magnitudines AB] coniungit ; ideo Archimedes considerat unam tantùm esse magnitudinem… Neque magis una est magnitudo quadrilaterum, pentagonum, cubus, & huiusmodi aliae, quam sit magnitudo, quae componitur ex magnitudinibus AB unà cum linea AB . quòd si est una tantùm magnitudo, ergo unum habet centrum gravitatis." del Monte, p. 43.

there is another important ancient source from which we can find substantial traces in Guidobaldo's mechanics. The pseudo-Aristotelian *Mechanical problems* were widely disseminated and discussed throughout the sixteenth century and it is not surprising that Guidobaldo paid considerable attention to them.[218] In the present section, I will trace some of the general conceptual features of the treatise which found their way into Guidobaldo's mechanics. In the next section, I will take up pseudo-Aristotle's and Guidobaldo's treatment of the stability of a balance.

As with the *Equilibrium of planes*, any interpretation of the *Mechanical problems* faces considerable puzzles. In a sense these go even deeper for the latter work, as Archimedes' work was seen to be rather easily completed by the addition of a definition of centre of gravity. The *Mechanical problems,* rather than giving the impression of being merely incomplete, present some obscure passages, which moreover form the core of its explanatory framework. Rather than trying to unravel their precise meaning, I will be primarily interested in presenting features of Guidobaldo's mechanics which can be seen as bestowing such a meaning, although in many respects it would seem unlikely that this was the meaning intended by the Greek author. [219]

The central organizing principle of the *Mechanical problems* is the reduction of the mechanical properties of the lever (and balance) to the mathematical properties of a circle. And these latter properties are thought to be of a special nature since "the circle is made up of … opposites, for to begin with it is composed both of the moving and of the stationary".[220] A circle is generated through the motion of a line which is fixed in one point (the centre), and of which the endpoint traces the circumference. This motion moreover is of a special nature, since it is actually the result of the simultaneous performance of two movements: one natural and one unnatural. This is thought to explain "why that part of the radius of a circle which is farthest from the centre moves quicker than the smaller radius which is close to the centre, and is moved by the same force".[221] The natural motion of the radius is somehow identified with the movement resulting from a tangentially applied force which is both moving the smaller and the greater radius, and which is thus identical for both.[222] The unnatural

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[218] Rose and Drake 1971.

[219] Micheli 1995, especially chapter 3, is a recent and erudite study aimed at a more precise understanding of the *Mechanical problems*, which moreover pays much attention to Renaissance commentaries on the work.

[220] Aristotle 1963, p. 333. I will use this twentieth century translation, without paying attention to the sixteenth century translations and paraphrases as I don't think that any of the points that I want to make here about the work depend on the differences that exist between these translations. See Micheli 1995, chapter 3, for discussions of some of these differences.

[221] Aristotle 1963, p. 337.

[222] I think it is clear from pseudo-Aristotle's own explanation that this force is not to be identified in general with the action of a weight, but with a tangentially applied force generating the motion of the radius and hence the "nature" of the circle. All this is part of a general investigation of the properties of a circle, not of the behaviour of weights. Only at the end of his explanation, when actually answering the first problem, does pseudo-Aristotle identify the equal force on both a large and a small radius with the weight in a balance. It is of course a conspicuous aspect of a balance that its arms are placed

motion is different for both, however, since it results from the influence exerted by the centre on both endpoints, and this influence is different as both points are situated at a different distance from the centre. (How to understand this "influence" is one of the obscurities I referred to in introducing the pseudo-Aristotelian treatise. At the end of this section we will see how Guidobaldo tries to conceptualize it.) And "because the extremity of the less is nearer the fixed point than the extremity of the greater, being attracted towards the centre in the opposite direction, the extremity of the lesser radius moves more slowly".[223] Having seen why a smaller radius must move more slowly, we can exploit this understanding in explaining some mechanical problems, such as "why is it that small forces can move great weights by means of a lever".[224] The explanation crucially involves the identification of the relevant elements in the lever with the structural properties of a circle:

> [T]here are three elements in the lever, the fulcrum, that is the cord or centre, and the two weights, the one which causes the movement, and the one that is moved… Now the greater the distance from the fulcrum, the more easily it will move. The reason has been given before that the point further from the centre describes the greater circle…[225]

A lesser weight can consequently move a greater weight because it suffers less interference from the centre in making its motion. It is important to keep in mind that the Greek author does not directly identify the greater speed with the cause of the compensation for the lesser weight, but starts from a deeper lying explanation of this greater speed.

It was noted in the previous section that Guidobaldo at several points provided Archimedes' abstract treatise with appropriate physical interpretations. One of the missing physical elements in this treatise is a fulcrum as the fixed point around which a lever and balance can turn. In his scholium to the first Archimedean postulate Guidobaldo immediately posits such a point and goes on to identify it directly with the Aristotelian "centre": "that point, moreover, that Archimedes admits, and from where the distances from which the weights are hung are measured, … Aristotle calls centre".[226] That this was by no means a gratuitous identification for Guidobaldo is testified by his *Mechanicorum liber*. There we find him having recourse to the general Aristotelian explanatory structure, including the crucial role of the centre, when he engages in a polemic with Tartaglia and other proponents of Jordanus' views on positional gravity (this polemic will be further analyzed in the next section).

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horizontally, and that the action of the weight is thus indeed working tangentially. The general properties of a circle can thus be recuperated to explain the behaviour of a balance *near* equilibrium (and this is all the author is interested in at this point).

[223] Aristotle 1963, pp. 341-3.

[224] Aristotle 1963, p. 353.

[225] Aristotle 1963, p. 353.

[226] "Punctum autem illud, quod Archimedes accipit, unde sumuntur distantiae, ex quibus gravia suspenduntur, …, Aristoteles centrum appellat." del Monte 1588, p. 24.

3.3.2 The causal role of the fulcrum

Jordanus, and following him Tartaglia, had posited that a body that is constrained by a rigid bar to move on a circle will move more swiftly as its position is closer to the horizontal diameter, and Guidobaldo reproaches them for having failed to uncover the true cause of this fact. This is shown by him to consist in the different influence the stationary centre of the moving bar exerts on the weight according to the latter's position. Imagine the weight as it rests on the bar while this stands perpendicular on the horizon: as it will weigh down on the bar, and hence on the centre *which cannot move*, the bar will have to resist the body's tendency for downward motion and push back against it. The result is that the body will be deprived completely of its tendency to descend. Now imagine the weight as it is attached to the bar which is held in a position somewhere in between the horizontal and the perpendicular: it will still weigh down on the bar, but the resistance offered by the bar will not be complete, as the direction of the body's tendency for motion and the direction in which the bar can push back against the body no longer coincide. Finally, when we imagine the weight attached to bar as the latter is perpendicular to the direction of the body's tendency to motion, the body will retain its complete tendency for motion.

It is striking how close Guidobaldo in his explanation approaches a modern understanding of the effects of constraint on the motion of bodies, when we identify the push back of the arm with a constraining force in the sense of classical mechanics and the resulting tangential force with the tendency for motion of the partly stustained weight. (Such assimilation would of course require a sophisticated understanding of the composition of forces which we cannot easily ascribe to Guidobaldo.)[227] At the same time it is striking how close Guidobaldo stays to the Aristotelian

[227] Yet it must be noted that it is not by accident that Guidobaldo most probably found the inspiration for his explanation in the *Mechanical problems,* where the parallelogram rule for the composition of motion is expounded and moreover lies at the centre of the explanatory structure. An important difference remains: Guidobaldo would have to consider the tangential force/motion as the resultant of the perpendicular free force (i.e. the weight) and the constraining force, which is normal to the circumference, whereas the Greek author considers the circumference itself as the result of the composition of motions which result from a force directed *towards* the centre and a tangentially applied force.

One reason why one may suppose that Guidobaldo never consciously analyzed the details of such decomposition is that it would almost directly have led him to the correct solution of the inclined plane problem. The main reason why he probably did not take this route, and instead adopted Pappus' treatment of the inclined plane, is that he conceived an inclined plane as a wedge upon which a body is forced to move. As Guidobaldo himself did not include Pappus' proof in his own treatise (it was only added in Pigafetta's translation), as his references to Pappus' treatment are rather sloppy (the balance involved in Pappus' proof has e.g. its fulcrum in the point of contact between the body and the inclined plane, whereas the lever to which Guidobaldo wants to assimilate the wedge has its fulcrum in the tip of the wedge), and as he only uses the qualitative result that more force is needed as the plane is more oblique (conform with his belief that no exact proportions could be given for problems involving motion – see *infra*, section 3.6.2), I think we can safely assume that he did not pay much detailed attention to the conceptualization of the inclined plane problem. The references to Pappus rather seem to be added to justify his inclusion of the wedge and the screw in his mechanical treatise. Accordingly, I will not further treat the inclined plane in explanatory framework, where the resulting speed of motion is also identified with the resultant of the combination of the natural motion of a body with the influence exerted by the stationary centre. Such assimilation becomes even more striking when we find Guidobaldo extending his explanation to the effect of the *length* of the rigid arm on the swiftness of the motion. Yet this extension at the same time shows the limits of this assimilation. Guidobaldo has crucially transformed the Aristotelian explanation by adding a different (almost "modern", we could be tempted to say) understanding of the interaction between weight and centre, based on an action-reaction pair. While this opens up a potentially forceful and coherent understanding of the variations of the dynamic effects of a constrained weight, it is impotent to explain the effect of the length of a lever arm, which was the prime objective of the Aristotelian explanation. If Guidobaldo wants to explain the latter case without straightforwardly reversing (which he nevertheless might give the impression of doing) to the Jordanian idea that it is the straightness of the virtual motion that explains the difference in apparent weight – an idea which he had earlier criticised as not truly demonstrative – then he can only effect this by an implicit reversal to the vague Aristotelian "influence" of the centre on the weight. It is thus the general Aristotelian explanatory structure of stationary centre constraining/influencing the moving weight which keeps together Guidobaldo's own attempts at causal analysis.

By introducing the Aristotelian "centre" as a fulcrum in Archimedes' treatise, Guidobaldo also incorporates the explanatory structure going with it. As a result he provides the abstract treatise with a further physical and causal interpretation. It is thus not surprising that he goes as far as claiming that Archimedes most probably received some of his postulates from the Aristotelian treatise.[228] Given the fact that Guidobaldo had also seized upon Pappus' definition of centre of gravity as a genuine Archimedean element, this need not be "a curious theory of the history of mechanics"[229], as both this definition and the Aristotelian explanatory structure make a lot out of the physical suspension of bodies in a central point. It is moreover precisely the duality of both centres, the centre of gravity and the fulcrum, which provides Guidobaldo with his most powerful explanatory strategy in his *Mechanicorum liber* (as will be seen in the next section). The same can be said about Archimedes' *Equilibrium of planes,* which first assumes that a body will prevail over another one if it is farther from the "centre" than the other one, and then goes on to show what is the general condition for equilibrium by demanding that the "centre" coincides with the centre of gravity of both bodies taken together. Finally, Guidobaldo could have found convincing historical confirmation for his claim in

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the present chapter. This need not detract from the fact that revealing questions can be posed about Guidobaldo's decision to refer to it in his treatise, but these fall outside the limited perspective I have adopted here. (In chapter 6, section 6.1 I will come back to Pappus' treatment when discussing Galileo's solution of the problem, and his criticism of Pappus.)

[228] "Supponit autem Archimedes hoc postulatum respiciens fortasse ad ea, quae Aristoteles in principio quaestionum mechanicarum ostendit, ubi colligit Aristoteles idem pondus celeriùs ferri, quò magis à centro distat…" del Monte 1588, p. 26.

[229] Drake 1969, p. 15

Pappus' reference to a lost treatise of Archimedes in which is ascribed to Archimedes exactly the proposition that greater circles overcome smaller ones.[230]

3.4 Synthesis: Revolving about the centre of the world

3.4.1 The stability of a balance – arguing against positional gravity

Up to now we have encountered two different respects in which Guidobaldo incorporated the Archimedean *Equilibrium of planes* within a broader Aristotelian framework. On the one hand, the physical definition of centre of gravity allowed him to integrate the Archimedean treatment of equilibrium within the general cosmological constitution of the universe. On the other hand, the Aristotelian treatment of the cause of disequilibrium allowed him to supply part of the missing physical structure in the Archimedean treatise. In a convoluted discussion in his *Mechanicorum liber* we can find both strands coming together.

The *Mechanicorum liber* opens with Pappus' definition of centre of gravity, accompanied by the corresponding definition due to Commandino, followed by a few obvious axioms about weight as a magnitude, and three *suppositions*, which read as follows:[231]

1. Every body has but a single centre of gravity.
2. The centre of gravity of any body is always in the same place with respect to that body.
3. A heavy body descends according to its centre of gravity.

The first section of the treatise concerns the stability of the balance. The first propositions introduce propositions concerning the stability of an equal arm balance with equal weights as it is sustained respectively above, under, and in its centre of gravity. All proofs combine a straightforward application of the Archimedean determination of the centre of gravity with the supposition that a body descends according to its centre of gravity (and the implicit acknowledgement that the fulcrum is a fixed point which must remain stationary). If the balance is sustained from above and removed from the horizontal position, the centre of gravity will be raised, and if the balance is released the centre will be able to descend until the balance is again in horizontal position (see figure 3.3.). The two other cases can be treated in a completely similar way (the centre of gravity will be respectively lowered – and will be able to keep on descending – and remain stationary). As a result, we have respectively stable, unstable and indifferent equilibrium.

[230] "demonstratum est enim in Archimedis libro περι ζυγων *sive de stateris* et in Philonis Heronisque mechanicis, a maioribus circulis superari minores circulos, si circa idem centrum conversio eorum fiat." Pappus 1878, p. 1069.

[231] del Monte 1577, p. 1v; 1581, p. 259.

Immediately after the proof of indifferent equilibrium, Guidobaldo enters into a sustained polemic discussion of Jordanus and other writers who want to base mechanics on the notion of positional gravity. This discussion has given rise to quite some miscomprehensions concerning Guidobaldo's own views, as it can be very misleading to consider only parts of this polemic without keeping an eye on the overall argument. In what follows, I will accordingly first try to summarize the different steps in Guidobaldo's argument, especially paying attention to the often criticized focus on the non-parallelness of the lines of descend of weights suspended on a balance.

The occasion which triggers the discussion is the existence of indifferent equilibrium, which was denied by Jordanus, Tartaglia and others (although they did not use that name for the state they assumed to be impossible).[232] According to these authors, a balance would never be in indifferent equilibrium since the weight on a depressed arm is always "positionally lighter" (as they called it) than the weight on the other arm. Hence a balance with equal weights, suspended in its centre, always returns to a horizontal position.

In a first step, Guidobaldo reiterates his proof of proposition four, which states the case of indifferent equilibrium, but with a slightly different emphasis. Instead of giving a direct proof, he reduces the claim that an equal arm balance sustained in its centre would have stable equilibrium to absurdity, by showing that this would imply that the centre of gravity of a given body would not be unique, contrary to the first postulate.

Next, Guidobaldo shows a mathematical error in Tartaglia's and Jordanus' argument concerning the supposedly smallest ratio of angles. This argument was explicitly designed to save a theory based on the notion of positional gravity from some strange consequences, but it could also be used to undercut Guidobaldo's argument. Its main point consists in showing that, although the weight on the elevated arm is positionally heavier than the weight on the depressed arm, the difference in heaviness is always infinitesimally small and consequently can not be offset by adding a small weight to the positionally lighter weight. The relevance of this argument for Guidobaldo's argumentation lies in the fact that this could be used to argue that although the one weight would be positionally heavier than the other, the centre of gravity of both weights would not change and as a result still be unique. The resulting theory would of course have a strange notion of centre of gravity, but Guidobaldo is clearly determined not to leave any room for his adversaries.

Not only is the argument concerning the ratio of angles wrong on its own terms, it also assumes that the lines of descent of the weights at both ends of the balance are parallel, contrary to what Tartaglia states at other places. Guidobaldo thus introduces the convergence of the lines of descent towards the centre of the world into the argument to tackle Tartaglia on his own ground. He immediately deduces that as a consequence of this convergence the weight on the depressed arm

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[232] I will not give references to the places where the relevant passages in Tartaglia, Cardano, and Jordanus can be found, since these are already noted in the translation of Guidobaldo's treatise in Drake and Drabkin 1969.

should always be positionally heavier, and that even stable equilibrium is inconsistent with the theory of positional gravity. (See figure 3.4.)

As a next step, Guidobaldo summarizes the arguments on which grounds the theory of positional heaviness would destroy the possibility of indifferent equilibrium. Firstly, it is assumed that the closer a weight is to the horizontal position, the heavier it will be. This in turn is due to the fact that it will be moving more swiftly because it is farther from the perpendicular erected on the centre of the balance. Secondly, this difference in positional gravity can also be deduced from the different degrees of straightness of the arcs at different places along the circle described by the balance arm.[233]

Guidobaldo agrees that a weight will move swifter if it is closer to the horizontal position, but he also claims that the theory of positional gravity fails to deliver the true reason for this fact. This is proved by showing that the true cause involves an explanation wholly absent in the writings of Tartaglia and Jordanus. This explanation is the one which was summarized in the previous section. It is followed by a long passage in which this explanation is applied to different configurations of the position of the centre of the weight's arm of suspension with respect to the centre of the world. The conclusion is that the position where a body would have the greatest "free" weight changes with this relative position (as the position where the line of descent and the arm of the balance are perpendicular changes – see figure 3.5). This digression does not directly touch on the arguments concerning positional weight. At this point Guidobaldo seems rather to be assessing the possible effect of the convergence of the lines of descent on his own theory.

The explanation grounded in the different curvatures of the arc is first attacked by showing again that it is incompatible with the convergence of the lines of descend. However, this time Guidobaldo seems to agree that this might be taken as mere hairsplitting since this convergence must remain imperceptible. Thus, he goes on to offer a further foundational critique of the notion of positional gravity. Firstly, he argues that the notion is incoherent, since a weight might be assigned different positional gravities depending on the way one considers its position. This is due to the fact that the curvature of an arc depends on the length of the segment one considers.[234] Secondly, the theory contains a crucial ambiguity which renders it unable to correctly assess the stability of a balance. The arguments concerning the impossibility of indifferent equilibrium were all based on a misapprehension of the way the two weights on the ends of the balance should be considered. The potential descent of the one was compared with the potential descent of the other, whereas it should have been compared with the latter's potential ascent since the two weights are always moving on the opposite arms of a balance – i.e. these authors overlooked the essential consequence of the conjunction of weights on a balance.

[233] A third argument described by Guidobaldo does not truly involve the notion of positional gravity.

[234] The cogency of this critique was denied by Duhem, who stresses that according to Jordanus the positional gravity has to be calculated for an arc smaller than any assigned value. (Duhem 1905, p. 215.)

3.4.2 Parallel lines of descent

At this point we are over halfway through the extended discussion appended after the fourth proposition. In the part that follows, Guidobaldo leaves behind the straightforward criticism of the notion of positional gravity, and further expands on the proper way to understand the stability of a balance. This involves two crucial explanatory features, which I will take up in turn as it is here that we can best assess the kernel of Guidobaldo's understanding of the right way to conceptualize mechanical problems.

Firstly, and most conspicuously given the criticisms that were levelled by Duhem and others at precisely this point, Guidobaldo reintroduces parallelness for the lines of descent of the weights suspended on the opposing arms of a balance. Immediately after having criticized Jordanus and Tartaglia for having neglected the effect of the conjunction in assessing stability, Guidobaldo claims that as a further effect of this conjunction the lines of descent will become parallel.

Secondly, Guidobaldo stresses that the different types of stability are governed by the duality between centre of suspension and centre of gravity. He further points out the structural similarity between his explanation and the one offered by (pseudo-)Aristotle in the *Mechanical Problems.* The latter of course did not involve the notion of centre of gravity, but this notion can now be imputed to the Aristotelian author because of this structural similarity. Yet, as Guidobaldo himself notices, "Aristotle poses only two questions [stable and unstable equilibrium] and leaves out the third; that is, the case in which the centre of the balance is in the balance itself."[235] Hence, it is exactly the most crucial case that is missing in the Greek treatise. Guidobaldo is not disturbed by this: "But he left this out as a thing well known, as he usually did omit obvious things. Who can doubt that, if the weight is sustained at its centre of gravity, it will remain at rest?"[236] No-one, of course; that is, no-one who accepts the existence of the centre of gravity as defined by Pappus…

The structural similarity between Guidobaldo's and Aristotle's treatment can only be secured via a not very subtle rhetorical strategy. Yet, as was argued in section 3.3, it is no accident that the duality between the two centres is stressed through a reference to the *Mechanical Problems.* What is interesting is not so much that Guidobaldo unconvincingly attributes a knowledge of barycentric theory to Aristotle, but that he takes over an Aristotelian focus on the physical effects of the stationary character of the point around which the weights move, and integrates this within a barycentric theory. If the fulcrum is e.g. situated above the centre of gravity, then the geometry of the situation immediately shows that the weight on the raised arm of a balance will be more "free" – i.e. less sustained – than the opposite weight, and the balance will have to return to a horizontal position (see figure 3.6). Guidobaldo's explanations at this point can be purely geometrical since he has already

[235] del Monte 1577, p. 26v. (Transl. from Drake and Drabkin 1969, p. 290.)

[236] del Monte 1577, p. 26v. (Transl. from Drake and Drabkin 1969, p. 290.)

analyzed the physics accompanying this geometry. However, as was already indicated, an essential part of this geometry is the fact that the lines of descent of both weights are taken to be parallel. How can this be squared with Guidobaldo's recurrent critique of other authors' neglect of the actual convergence of these lines?

The reason why Guidobaldo returns to parallel lines of descent is clearly indicated by himself: if he did not do this, he would be confronted with the same problem as he had uncovered for the proponents of positional gravity. After all, his own analysis of the differences in "free" weight due to the relative direction of the line of descent of a weight with respect to the arm from which it is suspended, gives the same results as the analyses based on the notion of positional gravity. But he had already shown that from the combination of the latter with the fact that the lines of descent converge in the centre of the world there follows the undesired result that the weight on the raised arm of a balance would have to be positionally lighter than the weight on the depressed arm. All that Guidobaldo offers by way of a direct justification for returning to parallel lines is the following:

> But if the weights *E* and *D* are joined together and we consider them with respect to their conjunction, the natural inclination of the weight placed at *E* will be along the line *MEK*, because the weighing down of the other weight at *D* has the effect that the weight placed at *E* must weigh down not along the line *ES*, but along *EK*.[237]

The line *ES* is the line connecting the weight *E* with the centre of the world *S*, whereas the line *EK* is a line through *E* but parallel with the line connecting the centre of gravity of *E* and *D* with the centre of world (see figure 3.7).

There is no further explanation of how this weighing down is to be understood, which is especially problematic given the fact that Guidobaldo's earlier analysis crucially rested on the fact that the weight at *D* already weighs down on the fulcrum which remains stationary. It might thus seem that Guidobaldo's justification must remain completely ad hoc. There is however one further feature about it which merits closer attention, and which will bring forth a greater coherence in Guidobaldo's conceptualization of this problem than might be apparent at first sight – and one that is certainly greater than acknowledged by Duhem *et alteri*.

The lines of descent are not just posited to be parallel to each other, but also to be parallel to the line connecting their centre of gravity with the centre of the world. This is immediately relevant, because if Guidobaldo has a means to justify this fact, he also has resources which are unavailable to the proponents of a theory based on the notion of positional gravity. As a result, he could at the same time criticize them for neglecting the convergence of the lines of descent and hold on to parallel lines in his own conceptualization. And if we remember Guidobaldo's understanding of the notion of centre

[237] del Monte, p. 20r. (Transl. from Drake and Drabkin 1969, p. 282.)

of gravity as it was evinced in his comments on *On the equilibrium of planes,* it becomes clear that he is not just positing an arbitrary stipulation.

One of the main features of the centre of gravity was that it is connected in a crucial way with the cosmological structure of an Aristotelian cosmos. We have seen that it is the centre of gravity which truly wants to unite itself with the centre of the universe (a fact which is also expressed in the third supposition of the *Mechanicorum liber*, quoted above). The present argument for the parallelness of the lines of descent can be understood as a straightforward extension of this understanding. The figure accompanying the text at this point clarifies this further (see again figure 3.7). A balance in a raised position is shown, as are the lines of independent descent of the two weights and the line of descent of their centre of gravity, all converging in the centre of the world. The balance is also shown with its centre of gravity in the centre of the world, its arms parallel to the original position. If we now draw lines from the weights in their original position to the same weights in this latter position, we have their paths of descent as their centre of gravity descends towards the centre of the world; lines which are parallel with each other, and with the line of descent of the centre of gravity.[238]

3.4.3 The three centres

Drawing all the lines of this discussion together, we can see that Guidobaldo's understanding of the stability of balances is structured by a three-fold organization. The duality between centre of gravity and fulcrum can only play its explanatory role because there also exists an intimate relationship between the centre of gravity and the centre of the world, which gives a balance its required unity so that the lines of descent of the suspended weights have to be considered parallel. In a comment that was introduced by Pigafetta in the Italian translation of the *Liber mechanicorum*, but which was actually due to Guidobaldo,[239] we find him stressing this three-fold structure himself:

> Now our author is the first to have considered the balance in detail and to have understood its nature and its true quality. For he is the first of all to have shown clearly the way of dealing with it and teaching about it, by propounding three centres to be considered in its theory: one is the centre of the world, another the centre of the balance, and finally the centre of gravity of the balance: for in this was a hidden secret of nature. Without these three centres, it is clear that one could not come to a perfect knowledge or demonstrate the various properties of the balance…[240]

[238] It is true that the present explanation introduces some problems of its own; it is especially hard to understand what happens with the bodies' tendencies to descend at the point when their centre of gravity coincides with the centre of the world. This situation reappears in Fermat's discussion of the geostatic question, and shows its problematic character in that context.

[239] See the transcription of a letter of Guidobaldo to Pigafetta in an appendix to Micheli 1995.

[240] del Monte 1581, p. 28r. (Transl. from Drake and Drabkin 1969, p. 294.)

Guidobaldo's conceptualization of mechanical phenomena essentially involves both what he had found in Aristotle and his followers, and what he had learned form Archimedes. Its basic conceptual element, centre of gravity, is of Archimedean origin, but the way it functions is co-determined by an Aristotelian cosmological frame and by the particular Aristotelian understanding of the balance.

There is one further strand running through Guidobaldo's discussion that remains to be taken up. It was already remarked upon that Pappus' definition of centre of gravity is of an essentially physical nature, and that the notion thus can be given no straightforward existence proof. At the same time, we saw Guidobaldo axiomatically holding on to its unique existence in his first criticisms directed against Jordanus and Tartaglia (that the difference in positional gravity of weights on opposite arms would imply the non-uniqueness of the centre of gravity of a balance), on the basis of his first two suppositions, quoted above. This straightforward connection between the possibility of indifferent equilibrium and the existence and uniqueness of the centre of gravity brings to light what is really at stake for Guidobaldo in his polemic with the proponents of the notion of positional gravity. *By denying indifferent stability they take away the well-foundedness of the whole concept of centre of gravity* (hence also Guidobaldo's confidence in claiming that Archimedes seems to have been of the same opinion as him concerning the stability of balances, a topic never mentioned by Archimedes)[241].

If we take a look at the discussion from this perspective, a further significant link with the issue of the parallel lines of descent comes to the fore. Precisely because the convergence of the lines of descent would imply the impossibility of indifferent equilibrium, it would also threaten Guidobaldo's mechanics in its true core. This connection would again become a central issue in the mainly French discussion concerning Jean de Beaugrand's *Geostatice* in the 1630's.[242] It could hardly have escaped Guidobaldo's attention, given the extended discussion he gives of the effects of the relative position of a weight with respect to the centre of the world on its "free" weight when suspended from a balance arm, which would directly imply that the common centre of gravity of weights on the opposite arms of a single balance would change with the inclination of the balance.[243] Yet, it is crucial to Guidobaldo's mechanics that this insight cannot be applied to connected weights, because he holds on axiomatically to the unique existence of a body's centre of gravity. And precisely because he holds on to its existence, he has the resources to argue for the parallelness of the lines of descent.

All this might give the impression that we are trapped in a kind of circularity, which only highlights the coherence of Guidobaldo's position, but has nothing to say about its well-foundedness. There are two reasons why this is not completely true. Firstly, if there is no way to restore the paralleness of the lines of descent, even stable equilibrium will not be possible. Hence, even if one

[241] del Monte 1577, p. 5v. (Transl. from Drake and Drabkin 1969, p. 262.)

[242] Cf. Duhem 1906; Costabel 1954; Roux 2004.

[243] This is especially so if we take into account that he had earlier criticized Tartaglia *et al.* because their arguments concerning the differences in positional gravity would imply a change in centre of gravity with the inclination of a balance.

does not necessarily want to hold on to indifferent equilibrium, there is still a good reason why one would want to be able to argue that the lines should be parallel. But the notion of positional gravity provides no clue whatsoever on this score, whereas the notion of centre of gravity does. Of course, one could decide to ignore the convergence of the lines of descent because it must remain imperceptible. Yet, secondly, Guidobaldo has another argument why his mechanics is truly well-founded. He claims to have been able to construct an empirical balance which shows indifferent equilibrium.[244] In the end, it is thus an empirical proof that secures the existence of the centre of gravity as defined by Pappus, and as a result also shows that Archimedes' proof procedure in his *Equilibrium of planes* is completely legitimate. But the attention for the different types of stability was due to the Aristotelian *Mechanical problems*, which accordingly points the way to the necessary empirical foundations for the abstract Archimedean treatise.[245]

3.5 Of weight and power

3.5.1 The mechanical machines

In the foregoing sections we have seen the intricate ways in which Guidobaldo's conceptual structuring of the science of mechanics revolves around the three centres, and consequently has a truly Aristotelian-Archimedean character. In the next section, a preliminary attempt will be made to reconnect this analysis with some of the issues surrounding Guidobaldo's broadly conceived

[244] del Monte 1581, p. 28r. (Transl. in Drake and Drabkin 1969, p. 295.)

[245] I claimed in section 3.1 (cf. footnote 17 and the accompanying text) that some of the conclusions of Duhem and Costabel could be used as a kind of hermeneutic benchmarks, because they allow us to pinpoint in what respects Guidobaldo's conceptualization of mechanics is essentially different from a modern one. Let me quickly summarize these conclusions, and leave it to the reader to compare them with the foregoing discussions. Both Duhem and Costabel make a lot out the presumed fact that Guidobaldo's conception of centre of gravity had to be incoherent because it involved both the definition due to Pappus, and the one due to Albert of Saxony. The first presumably involves parallel lines of descent (because, as we have seen, this is a precondition for indifferent stability), whereas the second essentially involves the centre of the universe (it is broadly speaking the idea that in any body there is one point which strives to unite itself with the centre of the universe), and hence brings with it convergence of lines of descent. On this ground, they criticize Guidobaldo on two scores: that he does not realize this incoherence, and that he cannot possibly overcome it. According to them, this incoherence could only be overcome by leaving behind the overtly physical connotations of both definitions, and by introducing a purely geometrical definition. Such a definition would allow the centre of gravity (which would become an ill-suited name for the concept) to play its truly fruitful role: to be a centre of dynamical equivalence; i.e. one can *derive* from this geometrical definition that it is the point where one can conceive all the mass of a system of bodies to be concentrated and the geometrical resultant of all the forces on these bodies to be applied. If we take these forces to be forces of weight, and if these are considered to be parallel, *then* it follows that we can always replace the system of bodies by its centre of gravity. That we have indifferent equilibrium if we hold a body in its centre of gravity is merely a physical consequence of this fact, but it is no part of the defining characteristics of the concept.

"scientific project". But before coming to these concluding remarks, it is important to assess some consequences of this way of conceptualizing mechanical phenomena; consequences that can be judged from the other sections in the *Mechanicorum liber* that follow upon the treatment of the balance.

Guidobaldo follows Pappus in reducing the other mechanical instruments to a combination of levers. In a letter to Pigafetta, he moreover states that the lever and the balance operate on exactly the same principles, the only difference being the mode of operating: a balance has weights on both ends whereas to a lever is applied another kind of power at one end.[246] But if we have a look at his way of determining the exact proportions governing the use of a lever, we immediately find him assimilating these applied powers to suspended weights, and as a result effectively transforming a lever into a balance.[247] This allows Guidobaldo to apply the conceptual structure that we discerned in the foregoing sections to the lever: first he demands that the fulcrum should coincide with the common centre of gravity of the weight to be sustained and a weight suspended at the point of application of the force, and only afterwards he sets the force to be applied equal to the weight that is thus determined.

The most important innovation introduced in the section on the lever is that the fulcrum must no longer of necessity lie in between the weight and the applied power/assimilated weight. This will of course be of capital importance in reducing a system of pulleys to a system of levers. Guidobaldo adduces two equivalent way of proving the exact proportions holding between sustaining power and suspended weight for such levers with suspended weight in between the fulcrum and the applied power. Both methods crucially replace powers by suspended weights and then exploit the rational principles that hold for weights on balance. His second method straightforwardly reverts to the balance model by imagining the lever arm to be extended at the other side of the fulcrum where a weight equal to the weight to be sustained is suspended at an equal distance from the fulcrum; a weight which in its turn can be held in equilibrium by a smaller weight suspended from the point at which the power must be applied. His first method is more interesting since it comes close to introducing something akin to the notion of static moment.[248] It exploits the idea that bodies of the same weight ("pondus") can have different gravity ("gravitas") depending on their relative position to the fulcrum, by setting the power equal to the *pondus* of a suspended weight that has as much *gravitas* as the weight to be sustained by that power. Yet the way he determines this *gravitas* is again through a straightforward identification of the position of the centre of gravity with the position of the fulcrum.

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[246] "Riduco le cinque machine alla leva, è vero, ma non però riduco la bilancia alla leva, essendo che esse siano una med.ma cosa e fra loro non vi è altra differenza, se non che con la bilancia si considerano li pesi, e con la leva si considerano la forza e il peso insieme…" Quoted in Micheli 1995, p. 161.

[247] Drake's translation in Drake and Drabkin 1969 skips almost all the proofs of the propositions concerning the lever, hence actually hiding the transformations that govern Guidobaldo's understanding of the lever.

[248] Already in proposition five on the balance does Guidobaldo state that suspended weights ('pondera") have gravity ("gravitate") in proportion to the distance from the fulcrum. The closeness to our notion of static moment is explicit in Commandino's version of the definition of centre of gravity. It is important, however, to keep in mind that static moment not only depends on the length of the lever arm, but also on the direction of the applied force.

3.5.2 The effect of the direction of applied power

Guidobaldo had no other way of understanding the effect of a power than by assimilating it to a weight having a natural tendency downward which could be introduced in arguments involving centres of gravity. In a corollary to the third proposition on the lever he even claims that all the proportions established remain valid if the lever is not held in a horizontal position, since this follows from what was said about the balance[249] – obviously referring to the discussions on indifferent equilibrium. Immediately afterwards he corrects this statement, yet not as we would expect by introducing the effect of the different directions in which a power can be applied, but by analysing the effects of different ways in which the weight to be sustained can be attached to the lever. His lack of attention to the effect of the direction of the applied power can be partly explained by noticing that it plays no role when we are dealing with pulleys, where the powers are always applied vertically.[250] And it is clear, through the sheer weight of exhaustive discussions of different kinds of arrangements, that the section on the pulley forms the main goal of the treatise. As the lever seems to be primarily introduced to explain the workings of pulleys, explicit discussions of the direction of the applied power are not that important. However, contrary to what Duhem claims,[251] Guidobaldo did realize that this could have significant effects.

In a passage on the wheel and axle (see figure 3.8), Guidobaldo discusses the effect of applying the power at different places at the wheel. He notices that if we apply the power to handle *T*, which is situated higher than the common axis of wheel and axle, then we get different results for the necessary sustaining power, depending on whether we "were to apply a living force to sustain the weight …, acting as if it wished to reach the centre of the world, as did the weight applied [there] by its own nature" or if "the handle were pressed by the hand".[252] Guidobaldo again introduces considerations on the relative positions of fulcrum and centre of gravity to justify this difference. The weight *G* will balance the weight suspended from the axle when their common centre of gravity, lying on the line *TB* connecting both points of suspension, is situated perpendicularily above the common centre *C* of both wheel and axle, which functions as a fulcrum. An elementary geometrical calculation shows that this centre of gravity lies closer to the weight when this is suspended from a position that is higher on the wheel; whence a weight must be heavier to sustain the other weight from this position. This special case of what we would call a bent lever is as a result reduced to a balance which is

[249] del Monte 1577, p. 42r; again a passage not included in Drake's translation.

[250] Guidobaldo explicitly notices that in his pulley systems "the power will always move the weight as with a lever parallel to the horizon". del Monte 1577, p. 77r. (Transl. from Drake and Drabkin 1969, p. 311.)

[251] Duhem 1905, pp. 219-223.

[252] del Monte 1577, p. 108r. (Transl. from Drake and Drabkin 1969, p. 318.)

sustained in a point under its centre of gravity.[253] This is a procedure which could be generalized to give a treatment of all kinds of bent levers, as long as the power applied can be assimilated to a suspended weight. The latter limitation is of course highly important: if the lines of force are no longer parallel, the notion of centre of gravity loses all sense for Guidobaldo, and his explanatory scheme breaks down.

Guidobaldo nevertheless also claims that when the power is applied perpendicularly (as pressed by a hand) the position on the wheel makes no difference. This seems to betray a more general analysis of the effect of directionality of forces, and hence would be a ground to attribute an understanding of what we call static moment to Guidobaldo. This attribution could be further strengthened by considering the argument that he actually gives for this indifference. He claims that this follows from the fact that powers applied perpendicularly at both the points *T* and *F* have their inclination along the circumference at the same distance from the centre.[254] This seems to imply that he considers the relevant factor responsible for sustaining the suspended weight to be the component of the force working along the line of motion of the lever, combined with the distance from the fulcrum. If we further connect this with his analysis of the effect of constraint on the force of weight, then a general conception of static moment seems to be completely within Guidobaldo's reach. However, it must be remembered that he wrongly suggested that his analysis of constraint would also explain the effect of the length of the lever arm, which clearly undercuts any arguments that would ascribe to Guidobaldo an understanding of what we call static moment. And most importantly, we cannot ignore the fact that he simply did not take this step – he clearly preferred to ground his analysis as much as possible in the concept of centre of gravity. Nowhere else in his writings are there any discussions of the effects of the directions of applied forces.[255]

Guidobaldo's insight in the differences between powers applied perpendicularly and weights suspended vertically is more nuanced than the simple ignorance ascribed to him by Duhem. Contrary to Henninger-Voss' claim that "Guidobaldo seems to have analyzed all machines from the unstated assumption that they always move according to the manner in which they are employed by workers"[256],

[253] Compare especially with the discussions at del Monte 1577, pp. 29v-30r. (Transl. in Drake and Drabkin 1969, p. 293.)

[254] "tunc eademmet potentia, vel in F, vel in T constituta idem pondus k sustinere poterit; cùm semper in cuiuscunque: extremitate scytalae ponatur, ab eodem centro C aequidistans fuerit, *ac secundum eandem circumferentiam ab eodem centro aequaliter semper distantem perpensionem habeat*." del Monte 1577, p. 109r.

[255] At least, I have not been able to locate other places in Guidobaldo's writings where he would directly apply the insight that it is only the perpendicular component which must be taken account. Proposition five of the section on the lever in the *Mechanicorum liber* is certainly not a case, as is claimed by Montebelli (Gamba and Montebelli 1988, pp. 239-240). One only has to notice that Guidobaldo nowhere considers the projection of the arm on which the power is applied to see the inappropriateness of the figure that is provided by Montebelli (his figure 14). Guidobaldo in this proposition is not discussing the need to project the lines of force on a perpendicular arm, but the place where we should consider the force of the weight to be applied to the lever arm (which need not result in a perpendicular projection).

[256] Henninger-Voss 2000, p. 255.

which was based exactly on Duhem's mistaken argument, we must stress that Guidobaldo analyzed almost all machines from the stated assumption that they are operated as if they were moved by suspended weights. Guidobaldo's mechanics is essentially a science of weights, which always have their natural inclinations, but which can be put to human use through a clever exploitation of the properties of centres of gravity. And this exploitation finds place both at the level of the organization of rational principles, as in his polemic against Tartaglia and Jordanus, as at the level of bringing these principles into operative act.[257]

3.6 The dynamics of stability

3.6.1 Guidobaldo's mixed science

The concept of centre of gravity provides Guidobaldo's mechanics with the necessary conceptual stability. Through its multiple guises, it can play different roles simultaneously. It is both an essentially physical notion, which at the same time connects mechanics with a general cosmological structure and can be found incarnated in all particular mechanical machines, and a mathematical notion, which allows the construction of a deductive theory on its basis.[258] In this concluding section, I will try to bring out some aspects of the part that is played by these roles in shaping Guidobaldo's scientific project.

There is an oft-repeated judgement that Guidobaldo denounced the ideas of Jordanus out of a misplaced homage to ancient authors (and a consequent rejection of medieval writers), and because he held on to an idea of absolute mathematical rigor.[259] The latter aspect is especially taken to be evinced in his insistence on the convergence of the lines of descent. However, we have seen that Guidobaldo only insists on this convergence in a specific context, i.e. in his polemic against Tartaglia and Jordanus. The belief in the reality of this convergence was something he shared with his opponents, but whereas it destroyed the coherence of their arguments, he could evade its undesired consequences. It is thus put to a very specific argumentative use, and nowhere does Guidobaldo suggest that all mechanical explanations should take account of this fact – quite on the contrary. In an almost paradoxical way Guidobaldo introduces this convergence into the discussion to save the possibility of indifferent equilibrium (whereas on first sight this fact would seem to destroy this possibility). It is this possibility which is truly at stake, and with it the well-foundedness of the notion of centre of gravity. Because these authors had argued against indifferent equilibrium, they could in no way possess true science.

[257] I borrow the apt expression "bringing into operative act" from Henninger-Voss 2000, p. 247, which, notwithstanding the confusion just pointed out in the text, is undoubtedly the best analysis of the hybrid nature of this double exploitation.

[258] Cf. especially del Monte 1588, p. 48, where Guidobaldo stresses the fact that centre of gravity is a mathematical notion, defined for mathematical objects, which allows its introduction in the Archimedean proofs of propositions 6 and 7.

[259] Duhem 1905, pp. 209-226; Drake 1969, pp. 44-48; Rose 1975, p. 233.

This is what the long polemic discussion is designed to show.[260] In the same vein it is not so much the notion of positional gravity as such that is criticized (after all Guidobaldo's analysis of the effect of constrainment on the "freedom" of a weight was explicitly designed to give the same results), but its organizing power – without the concept of centre of gravity, one is bound to run into insurmountable troubles.

The empirical proof of indifferent equilibrium was seen to occupy a crucial place in securing the foundations of Guiodbaldo's mechanics. At several places Guidobaldo stresses that it is essential to him that such empirical foundations had to be provided.[261] This focus on the empirical underpinning of the principles of his science allows us to see Guidobaldo's mechanics as an exemplary instantiation of the Aristotelian category of the mixed sciences. As we have seen in section 2.1.1, in establishing a mixed science one has to be able to show that a set of physical objects have some characteristics in virtue of which they are amendable to a mathematical treatment. This treatment then involves giving mathematical explanations of *why* a host of (mathematical) properties hold of these objects. It is evidently possible to give a mathematical *description* of a balance (based on the magnitudes of weight and length), and Aristotle and Archimedes have moreover shown how to exploit this mathematical description to *explain* different properties that hold of a balance *qua* mathematical instrument. This is possible because we can start from some *communes notiones* and *suppositiones* that characterize the mathematical concepts of weight and centre of gravity as holding of any *physical* balance.[262] Based on these properties we can then exploit mathematical reasoning to demonstrate a host of remarkable properties (e.g. the different kinds of stability, or the precise ratio's for the multiplication of force in a system of pulleys). The foregoing discussions have indeed shown how an empirical balance incarnates the essential conceptual features of mechanics in its different kinds of stability; features which only have to be expressed symbolically and ordered methodically by the mathematician. (This also helps understanding how Guidobaldo could have ascribed barycentric theory to Aristotle on account of no more than his treatment of the stability of balances.)

Apparently opposite to Guidobaldo's stress on the need of empirical underpinnings, Tartaglia had claimed that mechanical phenomena could be considered either "in abstraction from all matter", or through material tests and physical arguments, but that we should not confuse these two modes of

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[260] It is noteworthy that in Guidobaldo's own preface to the *Mechanicorum liber,* which stresses both the utility and the nobility of mechanics, he only has a scornful remark for Jordanus' "disastrous errors"; whereas Pigafetta's preface, which is almost exclusively devoted to the utility of mechanics, has a much more friendly reference to Jordanus, "who wrote of the science of mechanics" and "began to resuscitate it somewhat". (del Monte 1577, unnumbered preface; 1581, unnumbered preface; Drake and Drabkin 1969, pp. 246, 252. For an analysis of the differences between the Latin work and its vernacular translation, see Henninger-Voss 2000.) Guidobaldo's gibe occurs in the context of his stressing that he has tried to build up his work "from it foundation to its very top" – the most important problem with Jordanus is clearly not that he had made some easily correctable errors, or that he had introduced different concepts, but that he threatened these essential foundations.

[261] Cf. e.g. the letter to Contarini cited in Gamba and Montebelli 1988, p. 86.

[262] That any body has a centre of gravity; that it descends according to its centre of gravity; etc. (Cf. section 3.4.1.)

consideration.[263] But as Guidobaldo retorts, this actually implies that it becomes completely mysterious why this would still be a mathematical science *of* mechanics; as he famously expresses it: "mechanics can no longer be called mechanics when it is abstracted and separated from machines".[264] To borrow Henninger-Voss' assessment: Tartaglia's science seems to be rather a mixed-up than a mixed mathematical science.[265]

3.6.2 Idealization in Guidobaldo's science

Tartaglia had made his claim in a very precise context, however, i.e. when commenting on the difficulties that everyone is bound to notice when trying to verify theoretically established properties in empirical situations. He concludes that the presence of matter would necessarily hinder the truth of propositions proved mathematically in the abstract. Guidobaldo is of course aware of this problem, as he warns us (through the intermediary voice of Pigafetta) that:

> … in performing this experiment one might not act hastily, for it is an extremely difficult thing … to make a balance which is sustained precisely at the centre of its arms and at its precise centre of gravity. For this reason it is good to remember that, when anyone tries to perform such an experiment and does not succeed, he should not be discouraged, but rather should say that he had not been careful enough, and should try repeatedly until the balance is just and equal and is sustained precisely at its centre of gravity.[266]

The symbolic expression and methodological ordering that are the tasks of the mathematician cannot be attained through a straightforward inductive process. It is rather because Guidobaldo already has the proper rational principles that he is able to teach where we can find their incarnation. The important difference with Tartaglia's pessimistic attitude is thus that Guidobaldo is confident that, given the *right* set of principles, these can always be found to be empirically exemplified.

It is here that we can also find the background to Guidobaldo's claim that a moving force is always greater than a sustaining force, which implies the impossibility of extending the precise proportions established for equilibrium to situations in which the weights are moving.[267] It may be hard to precisely determine the centre of gravity of a physical balance, but *whenever* it is suspended in it, it will exhibit indifferent equilibrium. Yet, no matter how hard one may try to do away with friction,

[263] Tartaglia, *Quesiti et inventioni diverse* (Venice, 1546), 76-78. (Transl. from Drake and Drabkin 1969, pp. 106-7.)

[264] del Monte 1577, unnumbered preface. (Transl. from Drake and Drabkin 1969, p. 245.)

[265] Henninger-Voss 2002, p. 382.

[266] del Monte 1581, p. 28r. (Transl. from Drake and Drabkin 1969, p. 295.)

[267] That this claim is not due to the fact that he "refused to countenance the use of *insensibilia* in mechanics, because they were not susceptible of precise mathematical definition" (as is claimed by Rose 1975, p. 233) is proven by his discussion of the argument concerning smallest angles.

it will *never* be true that the addition of the smallest possible weight sets in motion a balance that was in equilibrium. The intimate connection between rational principles and their material incarnation is only possible for systems in equilibrium. When a balance (or a pulley etc.) is set in motion, friction will always introduce extra factors that are beyond the reach of rational principles. In a letter to Giacomo Contarini we find Guidobaldo expanding a little bit on this. Particularly interesting is the fact that he stresses that although the addition of such a smallest weight does not set the balance in motion, this does not render the balance false.[268] This again betrays the role played by the rational principles: we know that this aberrant situation must be due to impediments such as friction, because we have the rational guarantee that the true cause of equilibrium is equality in weight. *An analogue guarantee is missing for motion*. All that we can absolutely be sure of is that we always need an extra finite force to break situations of equilibrium. Yet, this need not have detracted Guidobaldo that much, since his precise analysis of the conditions of equilibrium is enough to show all the relevant structural characteristics of the machines.

It is important to note that Guidobaldo in the first place refers to the friction introduced by the turning of the machine around a fulcrum; i.e. even if we would accept the possibility of a vacuum, this would not fundamentally alter the situation. But thinking away the friction caused by the fulcrum would (in Guidobaldo's eyes) imply that the latter would no longer be a physical point, and as a result that we would not be dealing with machines anymore – that we would leave the science of mechanics. It is clear from the preceding analyses that it is impossible to abstract from the physical nature of the fulcrum in Guidobaldo's conceptualization of mechanical phenomena (see especially section 3.3.2).

To sum up: it is not that Guidobaldo does not acknowledge the fact that ideally true propositions can be violated through material hindrances, *but that only under precise circumstances these can count as deviations from true principles*; i.e. when these principles already have shown their empirical validity.

3.6.3 The dynamics of stability

It is well known that Galileo was not as disturbed by this lack of exact correspondence between rational principles of motion and empirical situations, and that he resolutely chose to consider situations in which all friction was absent. What sets Guidobaldo apart from Galileo is that he refuses

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[268] "La materia fa qualche resistenza […] la qual [materia] vuol la parte sua ancor lei, e quanto sono più grandi in materia tanto più resiste, sì come si provo tutto il giorno nelle libre che, per picole e guiste che le siano e che habbino pesi da tutte due le bande eguali e giusti, non di meno a un di loro se gli potrà metter sopra et aggiunger un peso di tanto poco momento, come un minimo pezzolino di carta che la bilancia starà senza andar giù da detta parte, né per questo la bilancia sarà falsa ; dove è da considerare che la resistanza che fa la materia lo fa quando si hanno da mover i pesi e non quando se hanno da sostenere solamente, perché all'hora l'instrumento non si move né gira; e con queste considerationi la troverà sempre che l'esperienza e la demonstrazione andaranno sempre insieme." (Quoted in Gamba and Montebelli 1988, p. 76.)

to go to these truly abstract applications of his concepts. Such difference has nothing do with the fact that Guidobaldo would be an adherent to a statical tradition, which eschewed all dynamical notions, whereas Galileo would be the first to truly unite this with a dynamical tradition.[269] Guidobaldo's analysis of equilibrium always has the following form: why is a balance in equilibrium/in motion – because its centre of gravity (the seat of its dynamic tendency) coincides with/differs from the fulcrum,[270] which through its stationary character exerts an opposing force that completely/only partly annihilates the tendency for motion.[271] He even goes as far as commenting on the speeds with which a balance will move to its position of equilibrium, depending on the relative position of its centre of gravity with respect to the fulcrum.[272] It is beyond all doubt that Guidobaldo conceived of equilibrium as the result of the opposition of a dynamic force by another equally strong force. Both the static and the dynamic properties of the centre of gravity are essential to his conceptualizations, as was already clear from his comments on the Archimedean proof procedure for the law of the lever.

This is also why it is highly misleading to construct the difference between the mixed science of mechanics and the natural philosophical theories of motion as a difference between statics and dynamics, as is often done. Some dynamical ideas (i.e. about the causes of natural motion) are necessarily present in mechanics, as these are part of the physical side of this mixed science, but the geometrical ratio's that Guidobaldo is actually explaining are not at all about natural motion.

That Guidobaldo could not have seen a substantial difference between a statical and a dynamical tradition is hence no case of anything like a doctrine of "double truth"[273], but a consequence of the fact that weight functions in the same way in both the contexts of equilibrium and motion, the only relevant difference being the presence of extra friction. The works of Aristotle and Archimedes were too closely interwoven for him to see different traditions,[274] whereas he strongly believed that the work of Jordanus was simply mistaken – the problem about Jordanus is not that he worked with

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[269] But this of course leaves open the question of the grounds on which Galileo nevertheless chose to take the steps that Guidobaldo consciously refused to take. I hope to provide a satisfactory answer in chapter 6, section 6.1.3.

[270] The formulation is a little bit too concise: it is not necessary that the centre of gravity coincides with the fulcrum; it is enough that it lies on a straight line connecting the fulcrum with the centre of the world – this is of course exactly the difference between on the one hand indifferent and on the other hand stable and unstable equilibrium.

[271] The proof of the first proposition in the *Mechanicorum liber*, which is skipped in Drake's translation, provides a nice illustration of this mode of argumentation.

[272] del Monte 1577, p. 24v. (Transl. from Drake and Drabkin 1969, p. 287.)

[273] Biagioli 1989, p. 65.

[274] I already quoted Drake's judgement that this was "a curious theory of the history of mechanics". Knorr 1982, provides convincing arguments for the exciting thesis that this might actually be the best history of mechanics available. He shows how the medieval so-called dynamical treatments of the balance in all probability derive directly from a lost work of Archimedes, pre-dating the *Equilibrium of planes* and the introduction of the concept of centre of gravity, and he adds the suggestion that Archimedes' interest in this kind of problems might have been triggered by the pseudo-Aristotelian treatment (*ibid.*, 100-102). It hence appears that what most historians of science have construed as two entirely different traditions actually have a common root in closely related efforts that took place in one and the same context.

dynamical notions, but that he missed the *proper* dynamics behind the different kinds of stability of a balance.

FIGURES TO CHAPTER 3

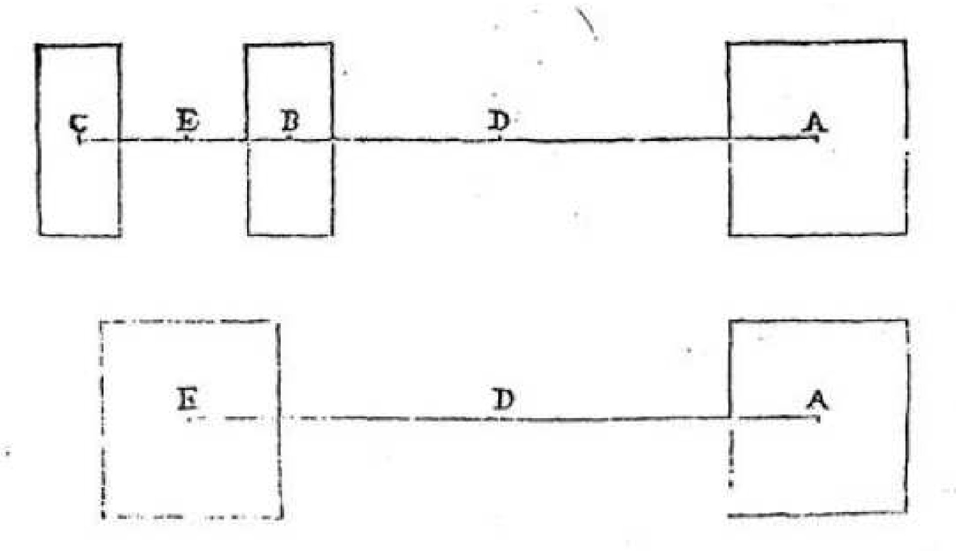

FIGURE 3.1

Replacing the weight *E* with two weights *B* and *C*, which together weigh as much as *E* and which are placed in such a way that their centre of gravity coincides with the centre of gravity of *E*, does not alter the conditions of equilibrium with a further weight *A*. (del Monte 1588, p. 55.)

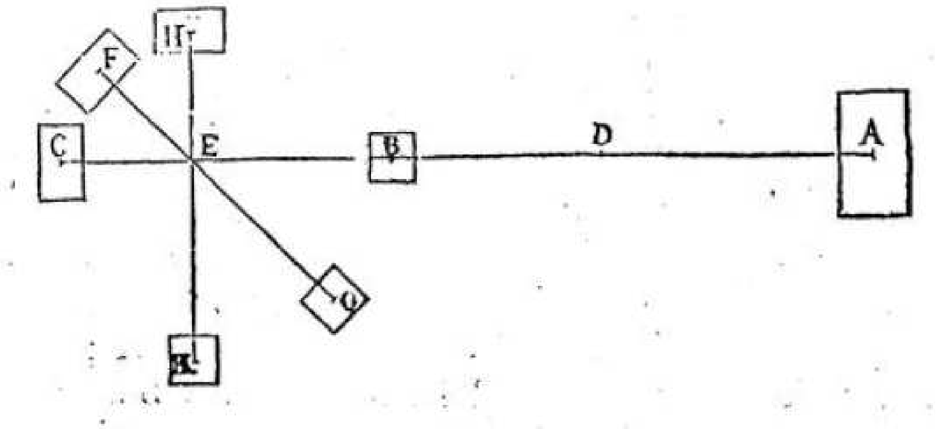

FIGURE 3.2

Placing the weights *B* and *C* at places *F* and *G*, or *K* and *H*, such that *E* remains their common centre of gravity does not alter the conditions of equilibrium with a further weight *A*. (del Monte 1588, p. 57.)

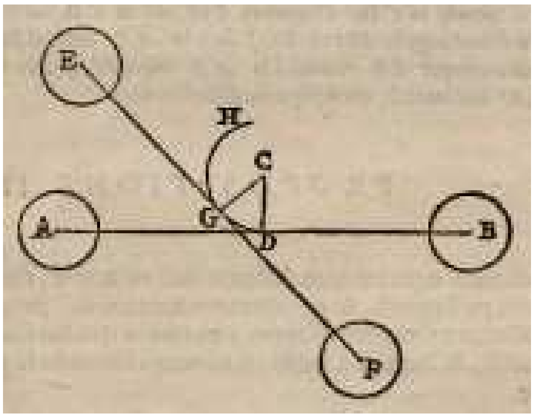

FIGURE 3.3

When a balance is sustained in a point *C* above its centre of gravity it will be in stable equilibrium: if it is moved from position *AB* to the position *EF*, its centre of gravity will be raised from the position *D* to the position *G*; its centre of gravity will naturally descend back to the position *D* which is situated lower; hence we have stable equilibrium. (del Monte 1577, p. 4r.)

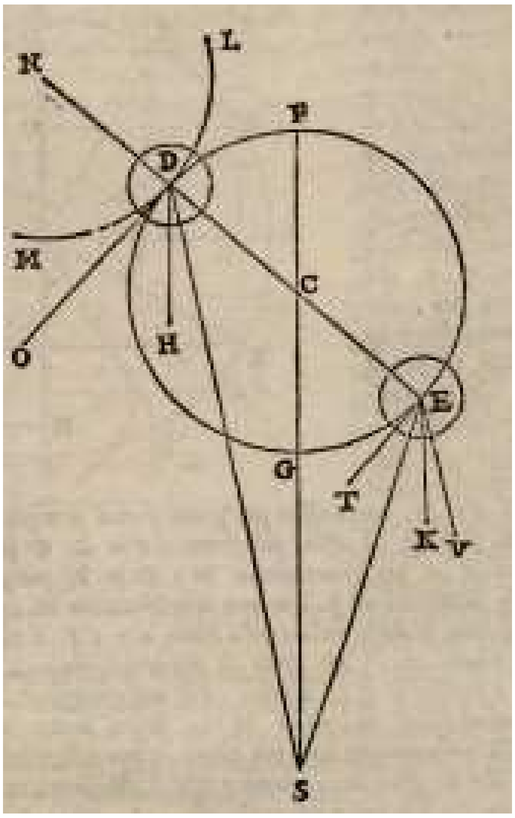

FIGURE 3.4

Since the lines of descent of the bodies at *D* and *E* converge in *S*, the centre of the world, the body at the lower position *E* will always have to be positionally heavier according to the views of Tartaglia and Jordanus since the angle *SEG* is less than *SDG*. It follows that even stable equilibrium would be impossible on these authors' own assumptions. (del Monte 1577, p. 8r.)

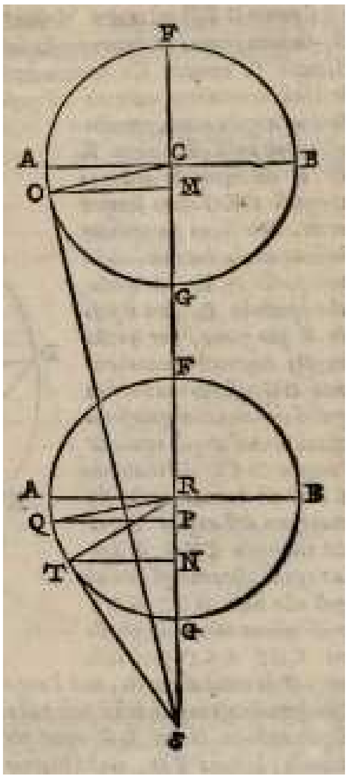

FIGURE 3.5

The position where a body would have the greatest "free" weight changes with the relative position of the balance with respect to the centre of the world *S* as the position where the line of descent and the arm of the balance are perpendicular changes (in this example from position *O* in the upper balance to position *T* in the lower one). (del Monte 1577, p. 12v.)

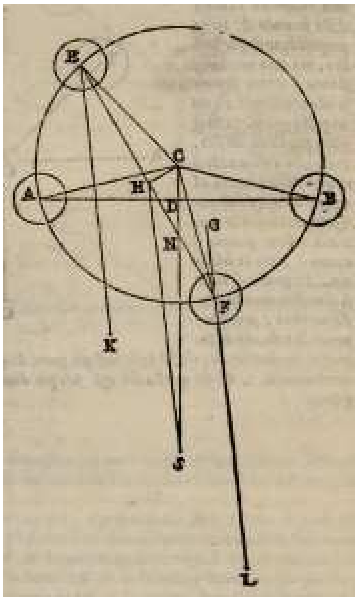

FIGURE 3.6

If the fulcrum *C* is situated above the centre of gravity *H* of the balance, then the geometry of the situation immediately shows that the weight *E* on the raised arm of a balance will be more "free" – i.e. less sustained – than the opposite weight *F*, and the balance will have to return to a horizontal position. (del Monte 1577, p. 23r.)

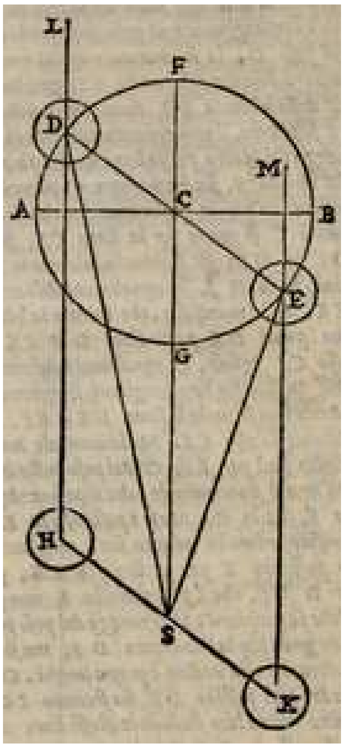

FIGURE 3.7

A balance with weights *D* and *E* is sustained in its centre of gravity *C*. The point *S* represents the centre of the world. The lines of independent descent are *DS* and *ES*, but since the line of descent of the centre of gravity is *CS* the weights are actually constrained to descend according to lines *DH* and *EK*, hence restoring parallelness for the lines of descent. (del Monte 1577, p. 19v.)

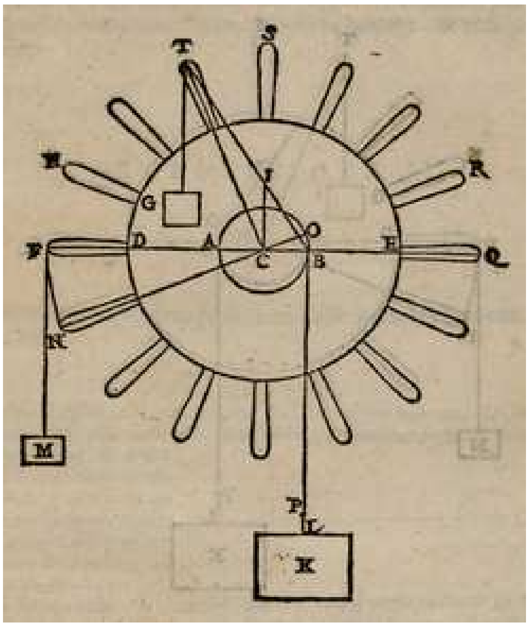

FIGURE 3.8

When we apply a power to handle *T*, which is situated higher than the common axis of wheel and axle, then we get different results for the necessary sustaining power, depending on whether we "were to apply a living force to sustain the weight …, acting as if it wished to reach the centre of the world, as did the weight applied [there] by its own nature" or if "the handle were pressed by the hand". The weight *G* will balance the weight suspended from the axle when their common centre of gravity *I*, lying on the line *TB* connecting both points of suspension, is situated perpendicularily above the common centre *C* of both wheel and axle, which functions as a fulcrum. An elementary geometrical calculation shows that this centre of gravity lies closer to the weight when this is suspended from a position that is higher on the wheel; whence a weight must be heavier to sustain the other weight from this position. (del Monte 1577, p. 108r. Transl. from Drake and Drabkin 1969, p. 318.)

4 *De Motu*: Disciplining a discourse

At the end of the year 1589 the young Galileo Galilei, aged 25, began lecturing as a professor of mathematics at the University of Pisa. During the three years that he held that position, he in all probability composed the different versions of his treatise De motu *which were found in a folder among his manuscript notes that contained his "older notes on motion".*[275] *In this chapter I will analyze these first Galilean attempts to develop a mathematical natural philosophy. Introducing mathematical arguments in treating natural motion went counter to all Aristotelian precepts. I will accordingly be especially interested in the way Galileo positions his mathematical approach with respect to the traditional philosophical discourses on this topic.*

In a first main section, I will describe how Galileo inserts his mathematically structured demonstrations within some of the traditional problems of motion, and how he thus transforms their nature. He could only extend the scope of the mathematical disciplines to previously illegitimate applications in a dialectic movement against the then current authorative way of treating problems of motion. In the second section I will discuss how he follows the structure of the mixed sciences by anchoring his mathematical explanations of phenomena of motion in some kind of basic principle that connects his mathematical framework with physical situations. We will see how Galileo attempts to render some of the characteristics of natural motion intelligible by introducing incontestable experiences with the behaviour of bodies on a balance where everybody can "see" the motive power of natural bodies at work. This then determines how he selects privileged factors, which complications he feels free to disregard, in which kind of mutual relation he places central concepts; in short, it determines the grounds that makes something "problematical."

This central role of the balance as a model of intelligibility brings with it the far-reaching suggestion that the things in the world themselves show their essential characteristics most clearly in our way of interacting with them. The possible grounds behind this idea will be treated in the next chapter. In the last section of the present chapter I will try to connect the kind of position that Galileo is developing in his De motu *with the historical context that was sketched in chapter 2.*

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[275] There have been several discussions about the exact order of composition of the fragments contained in the folder. Giusti 1998 is a recent assessment of the available evidence, which finds agreement with the order that was earlier proposed by Fredette 1969 and Drabkin 1960. Depending on the order one follows, different dating for the individual fragments follows, but there is a more or less general agreement that they all must be dated between 1586 and 1592. (I will without further comment adopt the "standard" chronology, i.e. first the dialogue fragment, then the complete treatise, then the reworking of the first book, followed by the abandonment of the treatise. (I would claim that the change in the nature of the pictures illustrating the hydrostatic demonstrations – see section 2.1.3 – already provides secure enough a ground for placing the treatise version after the dialogue. Cf. also Palmieri 2005.))

4.1 Rethinking the Aristotelian cosmos

4.1.1 Pisa, ca. 1589

Writing treatises on local motion was clearly *en vogue* in late sixteenth-century Pisa. The philosophers Girolamo Borro and Francesco Buonamico published in respectively 1575 and 1591 *De motu gravium & levium* and *De motu libri X quibus generalia naturalis philosophiae principia summo studio collecta continentur* etc. etc. (comprising over 1000 folios!).[276] The topic of course had a long history of philosophical discussions.[277] Indeed, already in antiquity books 5, 6, and 8 of Aristotle's *Physics* were referred to as his work "on motion".[278] More surprising might be that a mathematician such as Galileo was tackling the very same philosophical problem. Yet we know that his immediate predecessor in Pisa, Filipo Fantoni also owned an early manuscript version of Borro's treatise, thus showing a clear interest in the debates.[279] Moreover, when we consider Buonamici's treatise, we find him explicitly arguing "against the mathematicians" in some chapters where he treats Archimedean hydrostatics.[280] The question whether mathematics could be of any direct use in treating questions concerning motion was obviously a live one at this time.

Not only Buonamici, but also Borro took a staunch position against the use of mathematics in natural philosophy.[281] He had even written a brief work devoted to the "causes of our ignorance" among which figured prominently the lack of sufficient experience in natural philosophy. This experience was then contrasted with Plato's use of the mathematical method which was claimed to lead one into error (in this context Borro cites the same fragment as Piccolomini, cited in chapter 2, section 2.1.2.1, about children being experts in mathematics but not in philosophy).[282]

Borro's treatise was very recently published at the time of Galileo's writing. Whereas Buonamici's probably was not yet, Galileo in all probability knew his opinions from his teachings and

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[276] Borro 1575; Buonamici 1591. I learned the little I know about these voluminous and slightly tedious (no doubt, partly due to my unfamiliarity) treatises from the important work of De Pace 1990, and Camerota and Helbing 2000. From the detailed investigation by De Pace, it undeniably follows that Borro's treatise is both the prime source and the prime motivation for Galileo's own *De motu*, rather than the work of the Jesuit philosophers from the Collegio Romano as was argued by William Wallace (especially his 1984).

[277] See Dijksterhuis 1924 for a still enticing overview of the history of the problem; the first two chapters of Clavelin 1968 are also very perceptive. Grant 1964, 1965a,b, and Murdoch and Sylla 1978 provide good introductions to the Medieval treatments. Cohen and Drabkin 1958, pp. 200-224 have collected translations of some of the key passages from different ancient authors.

[278] Cf. Murdoch and Sylla 1978, p. 209.

[279] Camerota and Helbing 2000, pp. 331-332, n. 45. (It was first thought that Fantoni was actually the author of this manuscript, but this has by now been disproved by closer inspection of the work.)

[280] "contra mathematicos" Buonamici 1591, p. 494.

[281] Cf. Schmitt 1972.

[282] Schmitt 1976, pp. 467-468, which in an appendix also contains a transcription of the manuscript.

he might even have held some (public?) discussions with him.[283] It is thus a very clear statement of intent when Galileo provides his own treatment with the following introduction, through the mouth of Domenico, one of the characters of his early version in dialogue form:

> It will be very pleasant to hear your way of thinking on these topics and on similar ones which depend on them: for I know that on this subject you will either say nothing or bring forth something new and very near the truth itself. Now since you have grown accustomed to very reliable [*certissimis*], very clear and also very subtle mathematical demonstrations, as those of the divine Ptolemy and the most divine Archimedes, you cannot in any way give your approval to cruder arguments: and since these things which I have proposed to you are not very far removed from mathematical considerations, it is with eager ears that I expect something beautiful from you.[284]

Although in all probability familiar with the philosophical discussions concerning the possible status of mathematical treatments of philosophical issues, Galileo did not enter explicitly into the familiar topic of the relative worth of Aristotle's and Plato's opinion on the matter. Ten years later, however, his friend Jacopo Mazzoni fittingly answered Borro's attack by arguing that Aristotle had gone wrong because of neglect of mathematics *precisely* in the kind of questions that were also treated in Borro's *De motu*.[285] Even more interestingly, in his own treatment thereof he seems to have been directly influenced by Galileo's treatment in his unpublished *De motu*. There is a letter from Galileo to Mazzoni, written immediately after the publication of the latter's *In universam Platonis et Aristotelis…* where Galileo expresses his satisfaction that the philosopher had changed his mind and had now come to adopt the position which Galileo had defended in their earlier discussions.[286] The context makes it clear that he is referring to the problem of fall which made up the major part of the first book of Galileo's *De motu*.[287] The dialogue version of that treatise also contains one other explicit stab directed at Borro. Immediately after Galileo's alter ego Alessandro has expounded his anti-Aristotelian dynamical scheme, his side-kick Domenico exclaims:[288]

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[283] Cf. Camerota and Helbing 2000, pp. 358, 362-363.

[284] *Opere* I, p. 368. (Transl. from Galilei 2000, p. 115.)

[285] "Aristoteles ob non adhibitas oportunis locis mathematicas demonstrationes, maxime recesserit a vera philosophandi ratione. … Sed si quis voluerit hanc rem diligentius considerare, forsan, et Platonis defensionem inveniet, videbitque Aristotelem in nonnullos errorum scopulos impegisse, quod quibusdam in locis Mathematicas demonstrationes proprio consilio valde consentaneas, aut non intellexerit, aut certe non adhibuerit." Quoted in De Pace 1993, p. 330, fn. 151

[286] "…ha egli a me in particolare arrecata grandissima sodisfazione e consolazione, nel vedere V.S. Eccellentissima, in alcune di quelle questioni che ne i primi anni della nostra amicizia disputavamo con tanta giocondità insieme, inclinare in quella parte, che da me era stimata vera ed il contrario da lei…" *Opere* II, p. 197.

[287] See Purnell 1972, pp. 292-293.

[288] That statements such as these are directed towards Borro is clear from the fact that the latter is the only contemporary defender of Aristotle who is explicitly named in Galileo's dialogue, right at the outset where it is stated that "since this too

> Oh! what a subtle discovery, oh! how beautifully imagined! Let them remain silent, silent, those who assert that they can pursue philosophy without a knowledge of divine mathematics. And will anyone ever deny that only with it as guide can the true be distinguished from the false, that with its aid keenness of mind is stimulated, and that, finally, with it as guide whatever is really known among us mortals can be apprehended and understood?[289]

Galileo's *De motu* must undeniably be inscribed in a philosophical context impregnated by the issues that were discussed in the *Quaestio de certitudine*. Yet it does not contain explicit references to this debate. Nowhere does Galileo discuss the nature of mathematical definitions, or does he enter in any other related metaphysical exercises. So the important question becomes: on which grounds does he nevertheless think he is justified in pushing his approach; i.e. in what respect does he present his "mathematical" demonstrations as superior? In the next subsection I will introduce some of the elements of Galileo's positioning in this respect, but a full-fledged assessment must be postponed to sections 4.2 and 4.3, as we will first have to see some of the differences between his and the traditional philosophical demonstrations at work.

4.1.2 *"Mathematici mei"*

> As we will explain later that all natural motion of translation, whether it be upward or downward, is the result of the proper heaviness or lightness of the mobile, we have thought it in accordance with reason [*rationi consentaneum duximus*] to bring forth for every one to see how it should be said that a thing is lighter or heavier than another, or equally heavy.[290]

Such is the opening sentence of the treatise version of Galileo's *De motu*. The contrast with the rambling style of the dialogue is great. The treatise follows a clear guiding thread which is presented with steady hand. As a result of this disciplined rewriting, Galileo probably became sharply aware of the inner logic which he thought should guide such investigations into the characteristics of local motion. Halfway his treatise, being forced, once again, to correct some Aristotelian teachings, he explains:

> The method which we shall observe in this treatise will be that the things that must be said always depend on those that have been said; and that (as much as this will be possible) I never presuppose as true those that must be made clear. As a matter of fact my masters in mathematics [*mathematici*

has been treated with thoroughness by many, and most thoroughly by Girolamo Borro". *Opere* I, p. 367. (Transl. from Galilei 2000, p. 114.)

[289] *Opere* I, p. 401. (Transl. from Galilei 2000, p. 145.)

[290] *Opere* I, p. 251. (Transl. from Galilei 2000, p. 1.)

mei] have taught me this method: but it is not sufficiently observed by certain philosophers, who quite often, in teaching the elements of physics, presuppose things that have been reported either in the books *De Anima*, or in the books *De Caelo*, and even in the *Metaphysics*; and not only that, but even, in teaching logic itself, they constantly mouth words that have been reported in the last books of Aristotle; so that, while they teach pupils the first rudiments, they presuppose that these pupils know everything, and they hand down their teaching not from things better known, but from things purely and simply unknown and unheard of. Now what happens to those who learn this way is that they never know anything by its causes, but they only believe as by faith, that is because Aristotle has said so.[291]

The message is clear enough: *mathematicians are the true logicians*. It is their stringent way of reasoning that allows them to come up with certain knowledge. They are always able to clearly separate between what is given and what is to be proved, and this is why they cannot accept statements solely on authority: they check proofs to see whether they are wanting or not. Galileo's explicit appreciation of the worth of mathematics is primarily tied to its essential aid in overcoming equivocations by clearly defining terms and reasoning correctly with them.

To fully appreciate the significance of this opposition it is necessary to compare Galileo's *De motu* somewhat more closely with those of "certain" Pisan philosophers. One of the key issues being debated by Borro and Buonamici was the nature of the elements.[292] Borro defended a strict Averroist position, which was attacked by Buonamici who preferred the Greek commentators. This basic opposition structures much of what both authors claim with respect to the free fall of bodies. One of the points of contention was the question whether elements do have weight in their own place, which was denied by Buonamici but defended by Borro. The latter also claimed that his view was corroborated by experience, as he had shown to some interested people by dropping a large piece of wood and a small piece of lead which weighed more or less the same from his window.[293] The wood fell faster. This supposedly confirms the assumption that air has weight in its own region *since* everybody agreed that wood contains mostly air, whereas lead is constituted mainly of earth and water. The air in the wood thus still assists its downward motion. And when we let them down in water, the piece of wood will weigh less than the lead, again because of their respective elemental constitutions

[291] *Opere* I, p. 285. (Transl. from Galilei 2000, p. 36.)

[292] On these debates, see especially again De Pace 1990, Camerota and Helbing 2000; for some of their medieval sources see Grant 1965a. To be judged from a cursory perusal of the significant parts of Borro's and Buonamici's treatises, and the summary treatments in the two quoted secondary sources, their arguments present quite some puzzles in their own right, which I will not even try to raise. I will (of necessity) stay content with introducing some of the broad characteristics of their treatment which hopefully allow us to better understand the import of some aspects of Galileo's presentation of his own alternative treatment.

[293] He states that "ex aspectu coniicere licebat" that they were equal in weight and that it was even judged unnecessary to weigh them on a balance ("neque enim nos ad lancem illa expendere necessarium esse duximus")! (Borro 1575, p. 215.)

(in water the elemental air in the piece of wood no longer weighs down, but the water in the piece of lead still does). In answer, Buonamici stresses that a crucial error lies in the fact that by stating that wood contains more gravities (i.e. water, earth, and air) than lead (which only had two, water and earth), Averroes and his followers overlooked the fact that the gravity of lead was greater "secundum gradum".[294] One might be tempted to see an approach to the concept of specific gravity in this reference,[295] but it must be stressed that Buonamici stays within an Aristotelian framework. Each element has an absolute weight or lightness proper to its constitution. Buonamici only draws attention to the fact that the absolute weight proper to e.g. earth is more intense than that of air.[296] He also significantly pays a lot of effort in arguing against the Archimedean treatment of hydrostatic extrusion of a lighter body (extrusion would be a natural candidate to replace the idea of absolute lightness, as would indeed happen – albeit still not entirely confidently – in Galileo's *De motu*).[297]

Let me now quote the remainder of the paragraph in which Galileo comments on the difference between "his mathematicians" and "certain philosophers":

> There are only a few who inquire whether what Aristotle said is true: for it suffices for them that they will have the reputation of being more learned, the more passages of Aristotle they have at hand. But, leaving this aside, returning to our subject, it must be considered whether air and water really have weight in their proper places: for this question can be explained presupposing only the things that have been reported.[298]

It is exactly this question that divided Borro and Buonamici, who both excelled in embellishing their arguments with the right references to Aristotelian texts and presented their endeavour as one of interpreting Aristotle correctly. Galileo could hardly have chosen a place that would have been better suited to introduce the most explicit methodological remark of his whole *De motu*.

Let me also quote the opening paragraph of Galileo's *De motu* again, but now a somewhat longer part of it:

> As we will explain later that all natural motion of translation, whether it be upward or downward, is the result of the proper heaviness or lightness of the mobile, we have thought it in accordance with reason to bring forth for every one to see how it should be said that a thing is lighter or heavier than another, or equally heavy. Indeed, it is necessary to determine this: for it often happens that things that are lighter are called heavier, and conversely. Thus, at times we say of a large piece of wood that it is heavier than a small piece of lead, even though, purely and simply,

[294] Buonamici 1591, p. 485.

[295] Camerota and Helbing 2000, pp. 338-339.

[296] See also De Pace 1990, pp. 56-57, n. 167; and Grant 1965a, p. 357.

[297] Camerota and Helbing 2000, pp. 358-363.

[298] *Opere* I, p. 285. (Transl. from Galilei 2000, p. 36.)

> lead is heavier than wood; and of a large piece of lead, we say that it is heavier than a small one, even though lead is not heavier than lead. For this reason, in order that we may escape pitfalls of this kind, those things will have to be said to be equally heavy to one another which, when they are equal in size, will also be equal in heaviness.[299]

It is of course no accident that Galileo chooses the example of a large piece of wood and a small piece of lead.[300]

When dealing with the motion of heavy and light bodies it is necessary *first* to give a clear and unequivocal definition of how the terms "heavy" and "light" are to be used. Once this is done, one can safely argue concerning their speeds. Thus Galileo will lead his reader to a series of demonstrations, always building on what was proved earlier, which results, among other things, in a rebuttal of Borro's claims with respect to the weight of elements in their own place. Along the road, he also shows the inadequacy of some of the arguments which were adduced by Aristotle/Borro against a view which made lightness a relative property rather than an absolute one. These arguments fall short because they equivocate on the meaning of "heavier than", exactly the kind of pitfalls that Galileo tries to pre-empt in the opening paragraph of his treatise! As will become clear, Galileo's more secure way of reasoning is due to the fact that his stipulation is actually grounded in his experience with Archimedean hydrostatics.

4.1.3 Elementary mathematics

In the second chapter of the treatise version of *De motu* Galileo immediately prides himself on the fact that he can provide a rationale for the Aristotelian cosmological scheme, whereas other authors could only posit it without further rational foundation. This rationale is based on the geometrical properties of a sphere. If one body is heavier than another, this means (according to Galileo's stipulation) that an equal volume of it weighs more than the other. If it now were true that bodies are heavier when they enclose more particles of matter in the same space, then this would imply that the heavier body contains a greater amount of matter in the same space; or equivalently, that heavier bodies contain the same amount of matter in smaller spaces. Now consider one of the essential properties of a sphere: spaces become narrower as we approach the centre, and larger as we recede from the centre. Wouldn't it then be a rational constitution if the heavy elements should be placed near the centre of the cosmos, and the light ones farther away?

This explanatory scheme was probably suggested to Galileo by his study of Archimedes' treatise on floating bodies, which always demonstrates its propositions concerning equilibrium – whereby the lighter must stay on top of the heavier – on a sphere that represents the surface of a fluid

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[299] *Opere* I, p. 251. (Transl. from Galilei 2000, p. 1.)

[300] Cf. already De Pace 1990, p. 56.

at rest around the centre of the earth.[301] (Figure 4.1 shows a typical illustration from Archimedes' treatise in the popular 1543 edition by Niccolo Tartaglia.) Anna De Pace has shown that this argument must have been especially meaningful to him, as Girolamo Borro had argued that the philosophers who followed Plato in the denial of absolute lightness could no longer account for the *natural order* in the cosmos.[302] At this point Galileo is trying to show that this denial, which follows from his insistence on the relative definition of "heavier than", does not deprive him from explanatory means. On the contrary, he seems to suggest, he can do even better on the Aristotelians' own score.

Let us notice, however, that whereas his argument appears to do justice to the Aristotelian cosmological scheme, it does this only by assuming a strikingly un-Aristotelian conception of matter (which Borro had ascribed to the atomists, the Pythagoreans, and to Plato). Galileo is careful to introduce this as only a possibility, for which "ancient philosophers … were perhaps unjustly refuted by Aristotle"[303]. But it is clear that he is rather taken by the fact that by considering the elements in this way "we will find a certain suitability, not to say a necessity, in such a distribution of the heavy and the light."[304] It is more fruitful to understand Galileo's intervention as showing how one can *rethink* the Aristotelian cosmos from a fundamentally different perspective, rather than as doing justice to it. He retains some of its overall characteristics but fills it out completely anew from the inside by replacing qualitatively differentiated elements with homogenously structured matter.

Galileo's stipulation not only allows him to make "better" sense of the Aristotelian cosmological scheme, it also allows him to infer the "right" dynamics from it (i.e. bodies heavier than a medium move downwards in it, bodies lighter upwards). Herein we see the Archimedean import become even more dominant, yet in a first instance we also retain the general pattern that natural motions are predetermined by natural places. The natural places are the places of Archimedean equilibrium on Galileo's reinterpretation of the rationality behind the Aristotelian cosmos (the "heavier" underneath the "lighter" – always keeping in mind that we have to consider equal volumes). Therefore, natural motion will always be motion towards such equilibrium. Extending this idea to motion through a medium Galileo can prove that bodies lighter than the medium do not descend whereas the heavier do. These proofs, clearly based on Archimedes treatment of the floating of bodies, are always structured as follows: (1) suppose that situation *X* were an equilibrium state; (2) this cannot be so, because of the natural disposition, which is *Y* (the heavier placed underneath the lighter); (3) hence we have motion towards state *Y*. (Figure 4.2 gives an illustration of this explanatory scheme.) (The main difference with Archimedes' demonstrations is that Galileo explicitly interprets them

[301] That Galileo had already closely studied Archimedes' work on hydrostatics by the time of writing his *De motu* is demonstrated by his short tract on the hydrostatic balance, probably written in 1588 (and certainly before starting his work on *De motu*). See chapter 7, section 7.1, for an analysis of this tract and its profound influence on Galileo's subsequent work.

[302] De Pace 1990, pp. 12-13, 19.

[303] *Opere* I, pp. 252-253. (Transl. from Galilei 2000, pp. 2-3.)

[304] *Opere* I, pp. 253. (Transl. from Galilei 2000, p. 3.)

dynamically; i.e. he is interested in the motion towards equilibrium, whereas Archimedes rested content with showing, through a reduction argument, the equilibrium states.)

By demanding (with Archimedes) that we always consider equal volumes, Galileo immediately answers one of Borro's most important criticisms against a position that denies the existence of absolutely light elements. If this were an entirely relative matter, Borro had claimed, then it would follow that a large amount of fire would be heavier than a small amount of air, and it should accordingly be able to descend, whereas we see that fire always rises.[305] If we only be careful enough to use the terminology of "heavier" and "lighter than" in the appropriate sense, Galileo answers, we will see that such absurd results never occur.

This explanatory scheme again exposes how Galileo evacuates the contents of Aristotelian physics from the inside out; i.e. he retains some of its surface characteristics (natural motion towards natural places), but puts them in a radically different kind of internal relation. From an Aristotelian perspective the natural order of the cosmos is both explanatory and ontologically prior to the motion of any element. Remember Aristotle's celebrated definition of motion as "the fulfilment of what exists potentially, insofar as it exists potentially"[306]. When elements undergo natural motion they move towards a state in which they actualize their proper nature. This state only occurs when they are in their respective natural places where all bodies of the same elementary nature form natural unities.[307] All motion necessarily performs an ontological function, which is fixed by the natural order that constitutes the cosmos. The natural tendencies that are exhibited by elementary bodies are thus ontologically posterior to this cosmological order, and the state of rest is conferred upon bodies, not because of statical considerations, but because of the natural unity that comes with their natural place.[308]

Galileo reverses this picture. The cosmological structure *results* from the prior tendency that all bodies have for downwards motion. It is only because different kinds of bodies have a different density that a stable ordered structure arises. The state of rest is conferred upon bodies because there is equilibrium between the tendencies of bodies to move downwards and the medium's resistance against this tendency (a resistance which is due to the fact that the medium also has got a tendency for downwards motion). The unity of a truly Aristotelian cosmos depends on its intrinsic structure, which *ontologically* determines the characteristic properties of the different elements. The unity of Galileo's reinterpreted cosmos is due to the homogeneous property of weight that is shared by all kinds of

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[305] Borro 1575, pp. 38-39.

[306] *Physica* III.1, 201a. (Transl. from Aristotle 1930.)

[307] See also the analysis of what makes something natural in chapter 5, section 5.1.1. It will be argued there that Aristotle's interpretation of "nature" is primarily devoted to understanding the unity of things as the kind of thing they are.

[308] Cf. Machamer 1978b and Matthen and Hankinson 1993 (esp. pp. 425-430) on these issues. See also Wallace 1978 for a typically 16th century Aristotelian position on this matter.

bodies and which *mathematically* determines how these bodies situate themselves relative to each other.

4.1.4 On the speed of fall

When I claimed in the previous subsection that Galileo's explanatory scheme allows him to infer the "right" dynamics, this was restricted to explaining whether bodies move up, down, or remain in rest. Yet, not much further in his treatise, Galileo readily extends this to a quantitative measure for the speeds of such motion. For, as he claims, "for he who assumes motion, necessarily assumes swiftness" and "consequently, swiftness comes from the same thing as does motion".[309] The heavier a body, the greater will be the speed of its motion; heaviness now understood relatively to the medium in which a body moves. Galileo retains the basic dynamic idea that speed is proportional to weight, but he takes account of Archimedes' celebrated seventh proposition in his treatise on floating bodies, which states that a body that is placed in a medium weighs less by an amount that equals the weight of an equal volume of the medium.

At this point, Galileo starts to actually *oppose* Aristotelian physics, rather than to reinterpret it. He follows a long tradition in ascribing to Aristotle a mathematical law for the speed of fall, which is said to be proportional with the weight of the falling body and inversely proportional with the resistance of the medium.[310] Galileo's own dynamical scheme, however, implies that the resistance of the medium must be measured by its weight which is to be *subtracted* from the weight of the body. That is, as he explains, whereas Aristotle had suggested a geometric ratio (a "quotient"), we should actually use an arithmetic ratio (a difference).[311]

Galileo's arithmetic ratio involves an ambiguity which is not really resolved in *De motu*, and which concerns the status of what we would call "specific weight", a concept that is never explicitly defined by Galileo prior to 1612. All secondary literature nevertheless assumes that Galileo refers to it when he talks about subtracting the "weight" of the medium from that of the body, and it is undeniable that this is what he actually believed to be the proper measure for the speed of fall. But, as I will argue in chapter 7, sections 7.2 and 7.3, the transition from absolute weight to specific weight in *De motu* is not at all unproblematic. (Remember that Galileo's stipulation only states that we should consider *equal* volumes of bodies when comparing their weight; to speak about something like specific weight implies that we consider *unit* volumes.) In the following, I will temporarily pass over all that is involved in this issue, and assume that Galileo indeed is talking about something like our concept of specific weight.

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[309] *Opere* I, p. 261. (Transl. from Galilei 2000, p. 15.)

[310] See Gregory 2001 for a sympathetic treatment of the kind of questions to which Aristotle's proportional talk might have been actually directed.

[311] *Opere* I, p. 278.

The most important consequence of this fact is that bodies of the same kind (which have the same specific weight) always fall with equal speeds, whatever their absolute weight, contrary to the Aristotelian teaching that speed of fall is always proportional with weight. But there are some further consequences which show how misguided Aristotle had been, according to Galileo, and which are related with the assumption that speed is inversely proportional with the "density" of the medium. This inverse proportionality implies a number of paradoxes, such as the impossibility of equilibrium. Indeed, when a body is floating e.g. on water, its speed is zero, but according to the Aristotelian ratio this implies that the density of water would be infinite, which is absurd. Since Galileo sets force proportional to an arithmetic ratio instead of a geometric ratio, he can easily avoid this paradox.

We see how Galileo skilfully uses Archimedean hydrostatics to dismantle Aristotelian dynamics. But again, he does so by fundamentally reinterpreting the latter from within his own "mathematical" perspective. Demanding that the state of rest of a body should also be accountable for by a dynamical proportion actually comes down to seeing rest as motion with zero speed. This is a vision that is fundamentally foreign to Aristotelian physics where rest and motion are qualitatively differentiated notions. Aristotle conceptualizes motion as a process towards rest; rest which is conferred upon bodies because they find themselves at their natural place. Galileo on the contrary sees rest as infinitely slow motion, which is caused by the interplay between forces of weight that is similarly responsible for the speed of a body in motion.[312]

4.1.5 "Problems that have to do with motion"

Galileo illustrates his dynamical scheme with a number of elaborated examples, demonstrating all kinds of proportions that hold for the speeds of different kinds of bodies when falling through different media. From these exercises it follows that the proportion between the weights of bodies of a different kind but of the same volume changes as these bodies are weighed in different media. This then brings Galileo to the following claim: "And if they could be weighed in the void, in this case surely, where no heaviness of the medium would diminish the heaviness of the weights, we would perceive their exact heavinesses."[313]

[312] In the 1612 controversy on floating bodies, Galileo's Aristotelian opponents argued that a body's rest while floating on a medium is caused by another factor than is its motion; i.e. shape is a cause *secundum quid* of its rest, while the elemental makeup is the cause *per se* of motion (cf. Biagioli 1993, pp. 190-191). Galileo tellingly answers that "there is only one, true, and proper cause of buoyancy – the one known to me and to others. Distinctions such as *per se* or *per accidens* … cannot be applied to it. Those distinctions brought only to help those who cannot grasp the true, proper, and immediate cause of the philosophical problem they are confronting." *Opere* IV, p. 299. (Transl. from Biagioli 1993, p. 192.) Again, he reformulates the problem by erasing all qualitative distinctions.

[313] *Opere* I, p. 276. (Transl. from Galilei 2000, p. 27.)

Here Galileo is again in clear opposition with Aristotle's physics, where the possibility of motion in a void is denied. The basic argument of Galileo is simply to invoke his own dynamical scheme. This does not suffer the drawback of Aristotle's which would imply that motion in a void (a medium with density zero) would be impossible since the speed of fall would be infinite. As Galileo's own scheme escapes from this absurdity, he can claim that a "in a void also a mobile will be moved in the same way as in a plenum".[314]

Once more, Galileo is *transforming the nature of the problems of motion*. The question whether motion in a void is possible is indeed one the central problems that any philosophical treatment of motion had to deal with; as Galileo himself states: "this problem is one of the things that have to do with motion."[315] He deals with it in a way that renders all former treatments unrecognizable, however. To illustrate this, let us consider some Aristotelian philosophers who apparently came close to giving the same *answer*, but who conceived the nature of the *problem* from within a truly Aristotelian framework.[316]

Many scholastic philosophers, among whom Thomas of Aquino, already defended the position that bodies could fall with finite speeds in a void. The main problem that they had to face goes back to Aristotle and has to do with the fundamental role of resistance in his conception of motion. All motion requires some resistance which not only guarantees the successiveness and hence continuity of the motion, but which also is responsible for the fact that the speed will always remain finite. This resistance seems to absent in a void, however. In the early fourteenth century Thomas Bradwardine, among others, introduced the concept of "internal resistance" as a solution. Composed bodies can fall with a finite speed in a void because they consist of a mixture of heavy and light elements. If this mixture mainly contains heavy elements, then the light elements will function as an internal resistance against the motion that is determined by the heavy elements. This is only possible because they are *absolutely* light, i.e. they also have this property in a void. (The same is true for composed bodies which predominantly contain light elements: the heavy elements now function as internal resistance against the upward motion of the body.)

While Bradwardine accepts the possibility of motion in a void, his reason for believing so is fundamentally different from Galileo's. This is seen most clearly in the restriction to composed bodies: simple elemental bodies cannot fall in a void – they are essential unities and cannot be further subdivided in a motive power and a resistive part.[317] But the distinction in treating composed and

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[314] *Opere* I, p. 282. (Transl. from Galilei 2000, p. 32.)

[315] *Ibid*.

[316] The following is entirely based on Grant 1965a.

[317] It is true that some Aristotelian philosophers denied this and argued that also elemental bodies can be ascribed an internal resistance. This was e.g. the position of Menu, professor at Jesuit Collegio Romano at the end of the sixteenth century, as described by William Wallace: "In defence of his use of intrinsic resistance, Menu distinguishes two kinds of resistance, on that takes place with action, the other without. The resistance that arises from the quantity of an element as it moves through

elemental bodies has lost all sense in Galileo's explanatory scheme. As noticed by Anna De Pace,[318] it is accordingly not without reason that Galileo claims that those "forerunners" who had already defended the possibility of motion in a void had "arrived at the truth by belief more than via true demonstration"[319] – that is, if we understand what is meant by "real proof" along Galilean-Archimedean rather than along Aristotelian lines. The considerations that made the question *problematical* in the first place have shifted completely.

We have already seen in section 4.1.2 how this same Aristotelian explanatory framework still informed the controversy between Borro and Buonamici. Immediately after having cleared the opposition against the possibility of motion in a void, Galileo accordingly moves on and tackles the question whether elements have weight in their own place (yet another classic problem that has to do with motion, we could add). It is clear by now how Galileo will unlock this problem, and how he will again agree with neither position that can be defended from an Aristotelian perspective.[320] A portion of water is neither heavy nor light in its own place *because* it is in a state of Archimedean equilibrium. Yet this does not imply that it would have no weight when considered in itself, "purely and simply and absolutely, regardless of anything else".[321]

Let me finally summarize quickly how Galileo deals with two other topical problems for *De motu* treatises: the question of what moves a projectile that is no longer in contact with its mover; and the cause of the acceleration that we see in freely falling bodies. In response to the first question Galileo seems to opt for the traditional medieval solution which ascribes this to the presence of an impressed force. But, as could be expected by now, rather than taking over this solution, he fundamentally reinterprets it by incorporating it within his overall Archimedean framework.

Galileo concentrates his efforts on explaining what makes a projectile move upwards against its own inclination. The impressed force that is responsible for this forced motion is self-expanding and thus explains how the natural motion of the projected body will finally prevail. The way in which Galileo conceptualizes the interaction between impressed force and intrinsic weight is interesting. A body's "innate and intrinsic heaviness is lost in the same manner as it is also lost when it is placed in

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a void is of the latter kind, and for this it is not necessary that the quantity act in any way; rather it suffices that one part of the quantity not be able to be in the same place as another part in the same time." (Wallace 1984, p. 160.) Also in this case we are moving within a realm of arguments that is no longer considered explanatory by Galileo. (It seems, by the way, as if Menu offers a kinematic argument for the continuity of motion in a void, but tries to let it pass for a dynamic reason for its possibility. That is, he offers a resistance that does not act, but that still has an effect. This possibility would accordingly be denied by other Jesuit philosophers at the Collegio Romano.)

[318] De Pace 1990, pp. 32-40.

[319] *Opere* I, p. 284. (Transl. from Galilei 2000, p. 35.)

[320] Some of the subtleties that are relevant in answering this kind of question from an Aristotelian perspective, and which have to do with the intricacies of Aristotelian causal explanations, can be found in Wallace 1978, which describes the position on this issues of Muzio Vitelleschi, a Jesuit professor at the Collegio Romano at the end of the sixteenth century.

[321] *Opere* I, p. 289. (Transl. from Galilei 2000, p. 40.)

media heavier than itself."[322] We must imagine the projected body as if immersed in an extra medium that gradually becomes rarer up till the point that the body falls according to its "innate and intrinsic" weight. By incorporating the self-expanding impressed force, which indeed is of medieval origin, in his hydrostatical framework, Galileo turns this qualitative notion into a precisely quantifiable and in principle objectively determinable concept.

Galileo's explanation of acceleration is directly grafted on this explanatory mechanism. A falling body always starts from a situation in which it was held up, either by a hand or by something else that prevented it from falling. This implies that a force was impressed on it, exactly opposite to its proper weight. Once the source of this force is removed, the body starts falling as the force gradually diminishes, which diminishment also explains why the body accelerates.

4.2 Structuring phenomena of motion

4.2.1 Mathematics in motion

It has been argued by authors such as Peter Machamer and James Lennox that the tradition of mixed science provided Galileo with his prime model in developing a mathematical science of motion.[323] His recourse to Archimedean explanatory schemes is perfectly in line with this claim. In the present section I will assess some of the consequences of this idea for *De motu*.

Let us for a moment go back to Galileo's proofs of the fact that heavy bodies move down and light ones move up (heavy and light of course being understood relatively). In adducing these demonstrations Galileo cleverly exploits the fluid character of media (faithfully following Archimedes). The body that is immersed in the medium pushes downward *(deorsum permit)* against the part of the medium *besides it*! This is evident from Galileo's own pictures (see again figure (4.2)). When the body *ef* is immersed in the medium, the level of the medium is necessarily raised: hence the body *ef*, in pushing downward, raises the part of the medium *so* (equal in volume to the immersed part), which is the part besides the body. In addition, we see how he conceptualizes the situation in terms of two bodies which are trying to raise the other, and at the same time resist being raised themselves. Both facts are analogous with what happens on a balance, a fact that Galileo brings to the fore in a separate chapter. As he announces himself after having given his hydrostatic demonstrations:

> But, because all these things that have been conveyed in the two preceding chapters can be made clear in a manner still less mathematical and more physical [*minus adhuc mathematice, et magis physice, declarari possunt*], by reducing them to a consideration of the scale pan, I have decided in the following chapter to explain the correspondence that these natural mobiles observe with the

[322] *Opere* I, p. 312. (Transl. from Galilei 2000, p. 64.)

[323] Machamer 1978; Lennox 1986; cf. also Biener 2004.

> weights of an [equal-armed] balance: and the purpose of this is to attain a richer knowledge of the things that will be conveyed and more exact knowledge on the part of my readers.[324]

Galileo's terminology contains an unmistakeable reference to the mixed science, understood as these sciences which are partly mathematical, partly physical. So why does he choose to interpose this reference in introducing the balance analogy? This becomes clear if we read further in the chapter "in which is explained the correspondence that natural mobiles have with the weights of a balance."[325] To see this correspondence we have to represent the naturally moving body by a weight suspended from a balance, and an equal volume of the medium through which it is moving by the counterweight. We now notice that the body indeed moves up, down, or remains at rest, depending on whether it is lighter, heavier, or equally heavy as the medium. After having drawn out this analogy Galileo states:

> Having examined these things in the case of the scale pan, returning to natural mobiles, we can put forward the following as a general proposition: namely, that the heavier cannot be raised by the less heavy. With this presupposed, it is easy to understand why solids that are lighter than water are not completely submerged.[326]

Galileo's obliquely referred to the mixed sciences in introducing the chapter on the balance analogy because it is here that he first explicitly enunciates this general principle which gives his treatise the formal structure of a mixed science.

We have already seen that in establishing a mixed science one has to be able to show that a set of physical objects have some characteristics in virtue of which they are amendable to a mathematical treatment; this treatment then involves giving mathematical explanations of *why* a host of (mathematical) properties hold of these objects as characterized in that way.[327] We have moreover seen how this structure is exemplified by Guidobaldo's treatment of mechanical phenomena.[328] Let me quickly repeat the crucial features. It is evidently possible to give a mathematical *description* of a balance (based on the magnitudes of weight and length of the arms), and Aristotle and Archimedes have moreover shown how to exploit this mathematical description to *explain* different properties that hold of a balance *qua* mathematical instrument. This is possible because we can start from some *communes notiones* and *suppositiones* that characterize the mathematical concepts of weight and centre of gravity as holding of any physical balance. Based on these properties we can then exploit mathematical reasoning to demonstrate a host of remarkable properties (e.g. the different kinds of

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[324] *Opere* I, p. 257. (Transl. from Galilei 2000, p. 9. The translation in Galilei 1960 is rather inaccurate and muddles the meaning of this passage.)

[325] *Opere* I, p. 257. (Transl. from Galilei 2000, p. 10.)

[326] *Opere* I, p. 258. (Transl. from Galilei 2000, p. 11.)

[327] Chapter 2, section 2.1.1

[328] Chapter 3, section 3.6.1.

stability, or the precise ratio's for the multiplication of force in a system of pulleys). We can effect all kinds of geometrical operations on a centre of gravity because it is not only a physical notion but also and simultaneously characterizable as a mathematical point situated on a line.

This is then the function of the general enunciation just quoted: *it shows the physical property of all naturally moving bodies that enables them to become incorporated in a mathematical explanatory scheme*. Weight is a mathematical quantity that stands in all kind of relations to other quantities such as volume, but it also is a physical property of any body that constrains these mathematical relations in a physically meaningful way that is expressed in the general principle. In the completely revised third version of Galileo's treatise, its structure becomes more transparent. The principle is explicitly introduced as an axiom that is necessary for all demonstrations, and it is accordingly placed much earlier in the treatise, before any mathematical treatments of the natural motion of bodies are given.[329] This axiom is now also followed by a lemma in which Galileo proves the crucial mathematical proposition (which he had assumed without proof in the first version) that the parts of a homogeneous body have weight proportional to volume. It is because of these mathematical relations between weight and volume that Galileo's axiom enables him to demonstrate the basic directionality of natural motion. Adding to this the extra postulate that the speeds directly mirror the motion, he can mathematically demonstrate the various kinds of ratios that hold for the natural motion of bodies.

In chapter 2 I quoted the following description of mixed sciences: "The optician studies lines *in sight*, the musician, numbers *in sound*."[330] We could now add: the geometrical philosopher[331] studies ratios *in natural motion*.

Let us keep in mind that here we have a young man who is fascinated by mathematics, and especially by Archimedes' treatment of hydrostatics and mechanics. He has already shown himself adept in manipulating its formal apparatus to solve particular problems, both in his little tract *La bilancetta* and in his treatment of the centre of gravity of solids, which brought him in contact with some of the leading mathematicians of his time, such as Guidobaldo del Monte and Clavius, and indirectly led to his appointment as professor of mathematics at the university of Pisa.[332] And this very same young man has been thoroughly exposed to Aristotelian philosophy during his education in medicine, and has made himself further familiar with some of its intricacies upon his appointment in Pisa.[333] He certainly knew which were the traditional disputes on local motion, as illustrated nicely in

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[329] *Opere* I, p. 348.

[330] McKirahan 1978, p. 202. Cf. chapter 2, section 2.1.1.

[331] The term "filosofo geometra" is used by Galileo in his *Dialogue concerning the two chief world systems* (*Opere* VII, p. 234).

[332] Cf. chapter 7, section 7.1, for an analysis of *La bilancetta* and sections 7.2 and 7.3 for its influence on Galileo's *De motu.*

[333] In the *Assayer* of 1624, Galileo looks back at his education in Aristotelian philosophy in the following terms: "At my age, these altercations simply make me ill [*sento grandissima nausea*], though I myself used to plunge into them with delight

the memoranda which were found attached to his treatise. Among other things they contain a list of problems to be treated, probably written down before Galileo actually started writing his *De motu*.[334] This list could have easily been compiled by browsing other sixteenth-century *De motu* treatises. It also evidently summarize the questions that Galileo felt he could treat on the basis of Archimedean principles. Maybe he had also read Benedetti's earlier attempts to turn the Archimedean principles into principles of natural philosophy,[335] but he would have hardly needed such an inspiration. It was clear for anyone who cared to see; the possibility lay there "exposed to us so openly and manifestly by nature that nothing could be clearer or more open [*nobis a natura adeo aperta et manifesta exponuntur, ut nihil clarius, nil apertius*]."[336]

4.2.2 Looking at motion

Aristotelian physics has often been described as the physics of common sense.[337] It is clearly not experimental physics, but it is empirical physics through and through. It systematizes what we observe around us: that some kind of bodies will always move up out of themselves, whereas other kinds will sometimes move up, sometimes move down, and still other kinds will always move down; that bodies put in motion will always come to a stop; …[338]

Galileo begs to disagree:

> But, heavens!, how, I ask you, are we to believe the chimeras of those people, with which they profess to explain the most hidden secrets of nature [*naturae abditissima arcana*], if in the case of things that are, as it were, completely open to the senses they rashly assert the opposite of the truth?[339]

And he begs to disagree, again, when treating the crucial question whether elements have weight in their own place. This brings us back to what was said in section 4.1.2, where the Pisan controversy concerning this question was first introduced. But now that we have seen the broad outlines of how Galileo exploited what he had learned from "his mathematicians," we are in a better position to assess the import of the distinction he drew between their way of proceeding and that of the philosophers. It

during my childhood, when I too was under a schoolmaster [*sotto il pedante*]." *Opere* VI, p. 245. (Transl. from Drake and O'Malley 1960, p. 198.)

[334] *Opere* I, pp. 418-419.

[335] Cf. the evidence cited in Purnell 1972, pp. 290-293.

[336] *Opere* I, p. 274. (Transl. from Galilei 2000, p. 25.)

[337] E.g. Koyré 1966, pp. 17-18.

[338] The philosopher di Grazia, e.g., in the 1612 controversy on floating bodies, reproaches Galileo that he does use mathematical reasoning where the evidence of his senses should suffice ("egli nelle cose che son sottoposte al senso, e che noi continuamente veggiamo, vuole dimostrarle con matematiche ragioni"). *Opere* IV, p. 87.

[339] *Opere* I, p. 385. (Transl. from Galilei 2000, p. 131.)

will be remembered that the prime virtue that Galileo ascribed to the mathematical way was its methodical character, never assuming as true what must actually be proven, but starting from what is already known by every pupil. At several places he expresses the strong opinion that the philosophers, because of their unmethodical approach, simply forget *to look in the right places*. In a passage quite similar to the one just quoted it is expressed as follows:

> For truth has the property that it does not lie hidden to the extent that many people have believed; but its traces shine brightly in different places, and many are the paths by which one approaches it: yet it often happens that we do not notice things that are nearer and more clear. And we have a manifest example of this at hand: for all the things that have been demonstrated and made clear above in a rather laborious way are exposed to us so openly and manifestly by nature that nothing could be clearer or more open [*nobis a natura adeo aperta et manifesta exponuntur, ut nihil clarius, nil apertius*].[340]

By the "laborious ways," Galileo refers to are the Archimedean-style proofs of the dynamics of solids in a fluid medium. Which are the open and manifest ways? First, Galileo asks us to imagine that we forcefully submerge a body lighter than water, as well as try to draw a body heavier than water upward. It is clear, he states, that in both cases the force that we need to exert will be equal to the force with which the body respectively tends to move upward and downward. It is moreover clear that if the body would weigh just as much as an equal volume of water no force would be needed. It follows that the force needed will be exactly equal to the amount with which the weight of the body differs from the weight of the equal volume of water. Then, secondly, Galileo states that "it is possible to observe the same thing in the weights of a balance".[341] Again, if we have two weights that balance each other, and then add an extra weight to one side, the body on that side will move down "according to the heaviness by which it exceeds the other weight."[342]

In his study of seventeenth-century Jesuit mathematics, Peter Dear has stressed that the basic problem that confronted mixed mathematicians was the establishment of the right kind of empirical principles. In conformity with the Aristotelian ideal, these premises need to command universal assent on account of their evident character. As a result, the mathematicians had to mobilize many literary techniques to certify the basic principles of the mixed sciences with the needed credentials.[343] This also allows us to further understand why Galileo, in the first version of his treatise, stated his general principle only after already having given the mathematical proofs. It is only at this point that he introduces the analogy with bodies moving on a balance and this is exactly the kind of situation from which the principle grounding his "mixed science" derives its *evident* character – it is "exposed to us

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[340] *Opere* I, p. 274. (Transl. from Galilei 2000, p. 25.)

[341] *Opere* I, p. 275. (Transl. from Galilei 2000, p. 26.)

[342] *Ibid*.

[343] Dear 1995, esp. chapters 2 and 5.

so openly and manifestly by nature that nothing could be clearer or more open." This analogy allows him to direct his readers to look in the right places, where the causes of motion can manifestly show themselves. These right places are situations in which *everybody can ascertain for himself* the force with which a body is moving up or down. In interacting with a balance, in trying to draw up a body immersed in water, everybody immediately *feels* the motive forces at work. As Galileo explains in 1634 when discussing these issues, experience need not be confined to the sense of sight, the senses of hearing and touch can also perfectly have it.[344]

Galileo opposes *a different kind of observation* to the Aristotelian philosophers: one that is tutored by mathematical reasoning which imposes specific and precise conditions of observation. A disciplined kind of seeing that is well-known to everybody familiar with a balance: only when one has been careful enough to prepare it in the right way does it show the weight of the bodies placed on it. (Remember Guidobaldo's warning on the care that needs to be taken before one can see the general mathematical principles of a balance incarnated in a concrete balance.)[345] But *whenever* we have taken this care, it is *evident* that it is only the surplus weight that causes motion. Because this emphasis on exact conditions, the Archimedean schemes (as illustrated in figures 4.1 and 4.2) can show the causes of natural motion. Once having learned to look at nature in this way, it becomes almost impossible not to notice the relevant structures.[346]

When Galileo in 1612 for the first time openly enters into a published dispute with Aristotelian philosophers, in the Florentine controversy on floating bodies, he repeats many of the messages already contained in his *De motu*. By this time he has become even more conscious about what sets apart his way of proceeding from that of the philosophers (he significantly chooses Buonamici as the target of his attack on Aristotelian philosophers – although the latter had not been involved in the actual events that led up to Galileo's publication as he had already died a decade earlier)[347]:

> Besides, he who alleges heaviness brings forth a cause well known to our senses, because we can very easily ascertain whether ebony, for example, or fir, is heavier or less heavy than water; but who will make manifest to us whether the element of earth, or that of air, has predominance in them? Certainly there is no better experience of this than to see whether they float or go to the bottom. So that whoever does not know that such a solid floats unless he [first] knows that air predominates in it, does not know that it floats until he sees it float. For he knows it floats when he

[344] *Opere* VII, p. 724. This is a quote from the *postils* to Rocco, to be discussed in chapter 7, section 7.5.

[345] Cf. chapter 3, section 3.6.2.

[346] The role of this kind of diagrams is a very interesting topic in its own right. I will sadly enough remain almost completely silent on it. But see chapter 5, section 5.2, for some striking examples of Galileo's visual reasoning on geometrical diagrams.

[347] For these circumstances, and Galileo's choice to take Buonamici as foil, see Biagioli 1993, chapter 3.

knows air has predominance, but he does not know that air predominates except when he sees it float, and therefore he does not know that it floats except after having seen it float.[348]

Borro and Buonamici also had crucial recourse to experience in arguing on the question whether bodies have weight in their own place. Yet according to Galileo they simply did not know how to look correctly at what they saw. They claimed that the behaviour of bodies in fall showed them something about the question whether the elemental air in the bodies still weighed down in air or not. But as Galileo reproaches them, this actually presupposes that they already now that they are predominantly constituted from air – and how do they know *that*? In *De motu* Galileo had already ridiculed Aristotle's contention that earth is the heaviest of all substances: notwithstanding the Philosopher's posture that he always starts from what everybody sees, he actually must assume what he pretends to see – unless he would have "the eyes of Lynceus".[349] Galileo now further explains that the difference with his mathematically disciplined way of looking is that he only assumes facts about heaviness to which we have *independent access* – facts which *can be ascertained by anyone*. This why he did boast that "I never presuppose as true those that must be made clear."[350]

4.2.3 Representing motion

In the case of bodies on a balance, no-one doubts that the lighter body is moving up *because* it is lighter – but that it nevertheless still has weight. Everyone readily notices that motion downward also happens in the absence of a counterweight, whereas motion up can only happen in its presence; i.e. only motion downwards has an internal cause, and thus deserves to be called natural. Some further consequences of the relative definition are immediately laid bare through the mediation of the balance.[351] It is because of the apparent undeniability of this kind of shared experiences that Galileo

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[348] *Opere* IV, p. 87. (Transl. from Drake 1981, p. 72.)

[349] *Opere* I, p. 292. (Transl. from Galilei 2000, p. 43.)

[350] *Opere* I, p. 285. (Transl. from Galilei 2000, p. 36.) Cf. section 4.1.2.

[351] See especially *Opere* I, p. 259. It is of course an overstatement to say that this happens "immediately". As Raymond Fredette has driven home on me, we should never forget that *De motu* as we know it is a large amount of work*s* in progress. They show Galileo rethinking the issues over and over again. This is especially clear with regard to the question of the status of upwards motion, which is phrased differently in the different versions: in the revised version of the treatise, Galileo is much more explicit on calling this forced motion, but at the same time he dropped all references to the balance in arguing for this view (*Opere* I, pp. 361-366). Yet I believe it is highly relevant that he *started* his rethinking from the balance. This teaches us something relevant about the ways in which Galileo, through the act of writing and rewriting, tried to impose coherence on his own thoughts; thoughts which might have still been wavering on a lot of issues. The balance thus plays a pivotal role in that it provides a fixed point in his thinking, about which he can leverage the problematical terminology he inherited from the Aristotelian way of phrasing the problems. Once he has turned over his terms in this manner, the balance indeed *immediately* shows how to understand some of the problems of forced motion.

can claim that his mathematical demonstrations are really *about* the motion of natural bodies – it's after all physical experience that proves the "about-what" (*quia*) of any mixed science.

It is important to be clear on the difference with Aristotelian philosophers, who also invoked everyday experiences in arguing for their positions (as we have already seen with Borro and Buonamici). It was e.g. common to refer to the experience of a swimmer underwater when treating the question whether water weighs in its own place, a reference that recurs in Galileo's treatment of the question.[352] But the important difference is that these experiences are invested with a completely different kind of evidential role. For the Aristotelian philosophers this role is determined by the way these experiences can be integrated within a hierarchically organized structure of knowledge that reflects the basic ontological categories. For Galileo they generate evidence through the way that they can be understood on the analogy of the balance that structures his mixed science. It is this analogy that organizes the relevant similarities that can be noticed in different kinds of empirical situations.

We see this reflected in a passage where Galileo reproaches Aristotle that he used a false analogy. The analogy is the following: "Just as earth does not go up in the small cupping glasses of physicians because it is very heavy [*gravissima*], so fire will not go down because it is very light [*levissimus*]."[353] But, as Galileo retorts, "the ratio [*proportio*] has no worth: for it is not because earth is very heavy, that it does not go up, but because it is not fluid; for neither would wood go up, although it is lighter than water, which does go up; but mercury would go up, although it is heavier than earth because it is fluid; and thus fire would go down, because it is held to be not solid but flowing."[354] Because we cannot comprehend these experiences as analogous with what happens on a balance, they cannot tell us anything about the heaviness or lightness of fire. From now on all evidential reasoning concerning natural motion is constrained by the balance.[355]

Galileo repeatedly comments that he is ignoring accidental causes which make his theoretical ratios unobservable in practice.[356] Here he again follows the same logic. His theoretical model distinguishes the essential from the accidental factors. (Remember Guidobaldo's claim that the fact

[352] *Opere* I, pp. 288-289.

[353] *Opere* I, p. 292. (Transl. from Galilei 2000, p. 43. The translation in Galileo 1960, inaccurately, has "absolutely heavy", p. 59.)

[354] *Opere* I, pp. 292-293. (Transl. from Galilei 2000, pp. 43-44.)

[355] Another nice example is the analogy used by Galileo and Buonamici to understand the diminishment of an impressed force in a body: they both compare it with the way in which heat is gradually lost in a body that no longer is in contact with the fire that heated it. (See Koyré 1966, p. 37, for Buonamici.) The relevance of this analogy in an Aristotelian framework resides in the way it can throw light on how to understand an impressed force as a quality that interacts with the natural quality of gravity; whether it e.g. "is reducible to a disposition of the first species of quality" (Wallace 1984, p. 195). For Galileo its relevance is due to the fact that we can see how this would affect the weight of the body in which the force is impressed as it would be ascertained on a balance – it is as if the body is immersed in an extra medium (cf. section 4.1.5), but this obviously does not take away of the body's natural weight *just as* "white-hot iron is deprived of cold; but after the heat [is used up], it resumes the same coldness that is its own." *Opere* I, p. 311. (Transl. from Galilei 2000, p. 63.)

[356] See e.g. *Opere* I, pp. 266, 273, 298, 301, 302, 306, 307.

that the addition of a smallest weight to one side of a balance in equilibrium does not set the balance in motion, does not render the balance false.[357] This is then due to what Galileo would have called an accidental cause.) Most famously, Galileo claims that the acceleration of freely falling bodies is accidental.[358] There is clearly no reason for this claim other than the fact that his Archimedean models have no room for such a variable effect. Equally interesting is the way in which Galileo resolves the fact that acceleration is nevertheless universally present. He claims that he will use a "resolutive method" to "track down what we believe to be the true cause of this effect"; a method which proceeds as follows:

> Since, then, a heavy mobile … in going down is moved more slowly at the beginning, it is therefore necessary that it be less heavy at the beginning of its motion than in the middle or at the end; for we know with certainty, from the things demonstrated in the first book, that speed and slowness follow heaviness and lightness. If, then, it is found out how and why a mobile is less heavy at the beginning, the cause for which it goes down more slowly will certainly have been found. But the natural and intrinsic heaviness of the mobile is certainly not diminished, since neither its size nor its density is diminished: it remains, therefore, that that diminution of heaviness is against nature and accidental.[359]

After this preparatory stage he introduces his explanation involving the self-expanding impressed force.[360] But what is again revealing is that he explicitly calls this "the true cause of the acceleration of motion"[361] which is to be opposed to the philosophers who are not looking for "a cause per se of the acceleration of motion," but instead "only bring up an accidental cause."[362] Also this accidental phenomenon can thus be given an essential explanation, because it can be fitted into the general explanatory scheme determined by the balance (remember that it was already noticed that the effect of Galileo's impetus was also modelled on the balance)[363]. We can discern multiple layers of intelligibility in the phenomenology of falling bodies, instead of an undifferentiated complex of causes operating simultaneously that would be intractable.

All this implies that the balance has gained a special kind of representative power. This is an important aspect of what Peter Machamer has called the function of the balance as a model of intelligibility.[364] It is due to this representative function that the model allows for the generation of

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[357] Quoted in chapter 3, section 3.6.2.

[358] Cf. already section 4.1.5

[359] *Opere* I, p. 318. (Transl. from Galilei 2000, p. 69.)

[360] This explanation was summarized in section 4.1.5.

[361] *Opere* I, p. 319. (Transl. from Galilei 2000, p. 70.)

[362] *Opere* I, p. 317. (Transl. from Galilei 2000, p. 68.)

[363] Cf. section 4.1.5.

[364] Machamer 1998. Cf. chapter 1, section 1.3.1.

evidence for a mathematically developed theory of natural motion. It can play this function because it allowed Galileo to introduce some shared experiences where everybody can incontestably notice the motive power of natural bodies at work. And because these experiences provide the physical axioms for the mathematical explanations of his mixed science, the latter are constrained in a physically intelligible way. They are truly about natural bodies.

But this is of course blatant nonsense. If the balance is representative of something, it is certainly not of natural motion. Mechanical instruments *qua* exemplifying mathematical structures were generally understood to be intrinsically related to human agency.[365] These mathematical structures merely show how *we* can exploit certain properties of natural objects, they do not show what these natural properties are; that is, they are best computational devices. The philosopher studies moving bodies *qua* essentially formed entities, not *qua* exemplifying mathematical structures – the geometrical philosopher is a clear *contradictio in terminis*. A philosopher worries how bodies are led from potency to act, and about the distinction between first act and second act. He tries to see how one can coherently conceptualize motive qualities as instruments of substantial forms. *Etcetera.* In short, he tries to understand how the phenomenon of local motion can be integrated within an ontological framework that guarantees the essential unity of the natural world.[366]

In claiming to be treating "natural" motion, Galileo is overstepping all boundaries that were imposed on a mixed science. A mixed science *abstracts* from nature, whereas it was abundantly shown how Galileo's science actually *reinterprets* nature.[367] However, "nature" functions discursively as a normative instance that regulates the kind of claims that can be scientifically made about objects under study.[368] This is exactly how it functions in the Aristotelian explanations, through the ontological function of the cosmos. But it is not clear how it plays this role in Galileo's explanations. His way of engaging with the objects of his study seems too much tied to human agency, rather than that it would allow the objects themselves to show what makes them the kind of things they are. We can see *how* the balance might function as a model of intelligibility, that is what Galileo shows in his *De motu*, but this does not yet show the grounds on which it could acquire its representative power; i.e. *why* would it function as a model of intelligibility? To answer this question, we will have to investigate the way in which "nature" functions normatively within Galileo's new sciences, which will be done in the next chapter.

[365] This will be argued in closer detail in chapter 5.

[366] For a taste of the kind of problems that worried Jesuit philosophers at the end of the sixteenth century, see the interesting chapter 4 of Wallace 1984. (Cf. also Wallace 1978.)

[367] Cf. respectively chapter 2, section 2.1, and *supra* section 4.1.

[368] Cf. chapter 1, section 1.3.1.

4.3 Disciplining a discourse

4.3.1 *"Cogimur, velimus nolimus"*

In *De motu* Galileo's explicitly expressed appreciation of the worth of mathematics is tied to its essential aid in overcoming equivocations by clearly defining terms and reasoning correctly concerning them. We have by now uncovered many different aspects of what this aid consists in. The most crucial stipulations concerning weight are introduced as if they merely concern correct language use.[369] The fundamental principles which are at the basis of his mathematical treatment are also presented as equally undeniable. They just regiment what every "pupil" knows already or can ascertain independently, and are accordingly not at issue. Hence, the real problem is apparently that Aristotle and his followers simply do not know how to reason correctly. In *De motu* we repeatedly find statements such as: "one must reason about downward motion in the same manner"[370]; "concerning fire one must reason in the following way…"[371]; "we are compelled, whether we like it or not to say that earth is the heaviest, in comparison with other things, because it stands under all other things."[372]

At this early stage of his career, Galileo stays far from any metaphysical arguments in justifying the use of mathematics in natural philosophy. Instead, he exploits the characteristic that had become one of its culturally most distinctive traits. Mathematical reasoning not only controls labour and craft knowledge, it also controls natural philosophy – at least when the latter wants to treat a certain class of subjects. One cannot talk about motion and its ratios, without knowing some elementary geometrical truths concerning ratios in general.[373] One cannot talk about the effects of a body's weight without knowing about the mathematical science of weights. This kind of posture could only have become possible because of the simultaneous development of the evolutions described in chapter 2. Philosophers had started to think about the differences between mathematical proofs and philosophical demonstrations, and in doing so they almost unanimously stressed the rigor of the former. At the same time, mathematicians had started to cultivate the worth of this rigor as something of the greatest interest because it could be exploited in controlling knowledge. Galileo's move in *De motu* is grounded in the conjunction of both these processes.

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[369] Galileo frequently uses expressions such as: "non est *dicendum* aequa grave", "tunc certe … gravius … merito *asseremus" Opere* I, p. 251.

[370] "Pari rationi de motu deorsum est ratiocinandum." (Transl. from Galilei 2000, p. 25.)

[371] "Sic de igne est ratiocinandum:…"*Opere* I, p. 292. (Transl. from Galilei 2000, p. 43.)

[372] "…cogimur, velimus nolimus…" *Opere* I, p. 293. (Transl. from Galilei 2000, p. 44.)

[373] "That Aristotle was little versed in geometry appears in a number of places of his philosophical work … Aristotle was ignorant, not only of the profound and more abstruse discoveries of geometry, but even of the most elementary principles of this science." *Opere* I, p. 302. (Transl. from Galilei 2000, p. 53.) These most elementary principles that Galileo refers to concern the definition of ratios.

In this respect Galileo had been preceded by Giovanni Battista Benedetti, as the latter had also claimed to correct Aristotle's errors in mathematical reasoning concerning the motion of bodies. Benedetti had been connected as mathematician to the courts of Parma and Turin most of his career, whereas Galilo, in contrast to Benedetti, was employed at university while writing *De motu*. It must be kept in mind, however, that his position there had been secured through the protection of Guidobaldo del Monte, who besides a mathematical scholar was an influential nobleman. Galileo had even never graduated at university and had learned his mathematics from the Medici court mathematician Ostilio Ricci. It is again mainly through the same Guidobaldo that Galileo could significantly ameliorate his position by attaining a professorship of mathematics in Padua in 1592. From early on, Galileo's self-identity was thus being shaped through a patronage system that to a large extent was centred on court culture.[374] His move to the Medici court in 1610, where he significantly assumed the title of both court mathematician *and* philosopher, is only a further step in a *parcours* that had been prepared a long time – a *parcours* that seems to include the attempt to write a treatise such as his *De motu*. By putting himself in clear opposition to the established Pisan philosophers, Galileo achieves two things simultaneously: he associates himself with them through posing as a discussion partner, a philosopher among others, as is testified by his treatment of the traditional *topoi* concerning motion; but he keeps his distance, as he is able to pass judgement on the traditional philosophers from a perspective that is not theirs – and he can do this because *as a mathematician* he has a legitimate position to speak from as well.

However, this legitimate position did not authorize Galileo to discourse on *natural* motion. One only has to consider the outraged reactions of the philosophers in the 1612 dispute on floating bodies to see the intransigence with which the inappropriateness of his posture was pointed out.[375] There is one revealing passage where Galileo tries to anticipate these objections in this same dispute:

> Here I expect a terrible rebuff from some of the adversaries. I already seem to hear somebody shouting in my ears that it is one thing to treat things physically and another to treat them mathematically, and that the geometers should remain among their spinning tops without bothering with philosophical matters, whose truths are different from mathematical truths – *as if truth could be more than one*.[376]

He continues by claiming that, as a result, there is nothing contradictory about being both a philosopher *and* a mathematician. In this way he actually effaces the complex metaphysical picture that we saw structuring the *Quaestio de certitudine mathematicarum*, which lies behind the

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[374] For Galileo's patronage strategies, see the highly fascinating analyses in Biagioli 1993, which also contains information on Galileo's early career moves and the role therein of Guidobaldo.

[375] Biagioli 1993, chapter 3, provides a nice sampling of some of these reactions and their background.

[376] *Opere* IV, p. 49. (Transl. from Biagioli 1993, pp. 221-222. My emphases. Drake's translation in Drake 1970, p. 168, is rather inaccurate.)

philosophers' reactions. In chapter 9 we will see how at the end of his career, in his *Dialogue concerning the two chief world systems,* Galileo positions himself with respect to some of the issues that surrounded the *Quaestio*. We will then see what place is left for the related problems of idealization and abstraction. But at this early point in his career they are simply put aside as not relevant.

The theme of the uniqueness of truth was already present in *De motu*, albeit yet less outspoken (as is often the case).[377] In one of the memoranda, Galileo claimed: "There will be many who, after they have read my writings, will turn their mind, not to consider whether the things I have said are true, but only to seek in what way, whether rightly or wrongly, they could undermine my opinions."[378] His trespassing in the field of philosophy is conducted under the aegis of the truth; i.e., if there is some contradiction between his views and that of the philosophers, one of both parties must be in error, rather than that he would be behaving inappropriately.[379] According to Galileo, truth has the essential property that once noticed it cannot possibly be denied, whatever the prior opinions on the right ways of proceeding.[380]

At the end of our long analyses it need not be stressed that Galileo is imposing his criteria for what it takes to be a true statement on the philosophers; that he is trying to draw them into *his* domain of truth. And *we* cannot think otherwise than that he was right to do so; that the way truth functions for him is much more sensible (and fruitful) than what the philosophers had on offer (cf. his stress on the possibility of independent access to basic facts). But this does not preclude us from further asking what it is that grounds his domain of truth. To repeat the question which closed the previous section: what is so peculiar about a balance that it has the power to structure the truths that we can notice about phenomena of motion? The answer that I will propose in chapter 5 is again closely tied to the issue of cognitive control.

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[377] It is not accidental that I have frequently referred to the 1612 dispute in my analysis of *De motu.* This dispute provided Galileo with an occasion to make public many aspects of what he still considered valuable about his earliest attempts at developing a mathematical natural philosophy. By this time his thinking had crystallized in many respects, which allowed him to express his views still more efficiently and polemically.

[378] *Opere* I, p. 412. (Transl. from Galilei 2000, p. 156.)

[379] Cf. e.g. the following passage: "This is Aristotle's demonstration: to be sure it would have concluded very much to the point and from necessity, if Aristotle had demonstrated the things he took for granted, or, if they had not been demonstrated, if they had at least been true; but he has been deceived in this, that these things, which he took for granted as well known axioms things which are not only not manifest to the senses, but have never been demonstrated, and are further not demonstrable, because they are totally false." *Opere* I, pp. 278-279. (Transl. from Galilei 2000, pp. 28-29.)

[380] Cf. the following expressions: "[truth's] traces shine brightly in various place"; "the force of truth"; "if [the truth] had once been found by someone, immediately and without controversy, being what it is by its nature, it would have allowed itself to be seen and known by all"; "This objection is surely of great importance; but nevertheless it is not so powerful that it can obscure the splendor of the truth." *Opere* I, pp. 274, 284, 294, 335. (Transl. from Galilei 2000, pp. 25, 35, 45, 85.)

4.3.2 Searching for objects

As Galileo never published or even circulated the manuscript of *De motu* we can safely assume that he was not entirely satisfied with the resulting scheme. The reappearance of many of its characteristics in his 1612 *Discourse* shows that he nevertheless believed that it contained many valuable insights. This is further corroborated by the fact that he kept the folder with the manuscripts with him while composing the 1638 *Discourses and mathematical demonstrations concerning two new sciences pertaining to mechanics and local motions*.

One of the major sources for Galileo's dissatisfaction must have been that none of the theoretical claims were actually corroborated by experience, as he showed himself perfectly aware.[381] At this point, Galileo had a language to speak (geometry), he was forging himself a position to speak from (a geometrical philosopher), he had problems to address (the topical problems of motion), but it is not clear whether he actually had objects to speak about! With hindsight we can ascribe this to two major insights that he still missed at that time, but which he would acquire not long after writing *De motu*: the fact that the acceleration of freely falling bodies follows exact mathematical proportions; and the fact that all bodies, regardless their specific kind, fall with equal speeds (at least in a void – but it is there that Galileo had already proclaimed that one should search for their true speeds).

In chapters 6, 7, and 8, I will sketch parts of Galileo's search for the objects of his theory, and the consequent re-elaboration of the theory. It will be seen how this search process was directed by his prior structuring of the phenomena of motion as described here, and further regulated by the notion of nature as described in the next chapter. Let me in closing stress how Galileo in his earliest work had already shown a high degree of reflexivity concerning the status of the explanations that he offered. He was quite clear about what constrained possible answers to the problems he was investigating, and he was consciously exploiting his model of intelligibility to select privileged factors and neglect accidental circumstances. This methodological awareness, which is in large part due to his familiarity with both Aristotelian natural philosophy and the mixed sciences, goes a long way towards explaining why it was Galileo rather than someone else who achieved the important breakthroughs that he did.

[381] Cf. section 4.2.3.

FIGURES TO CHAPTER 4

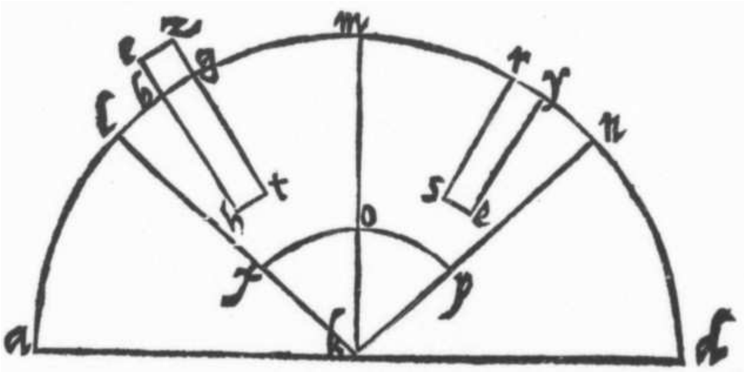

FIGURE 4.1

A typical diagram from Archimedes' treatise on floating bodies, taken from Tartaglia's 1543 *Opera Archimedis Syracvsani philosophi et mathematici ingeniosissimi*, p. 32v; *amd* is the surface of a fluid at rest, with *k* the centre of the earth; the solid body *ezht* which weighs the same as an equal volume of the fluid will be completely immersed but at rest, for if it wouldn't be (as illustrated on the figure) than the pressures on *fo* and *op* would be unequal, which would result in disequilibrium.

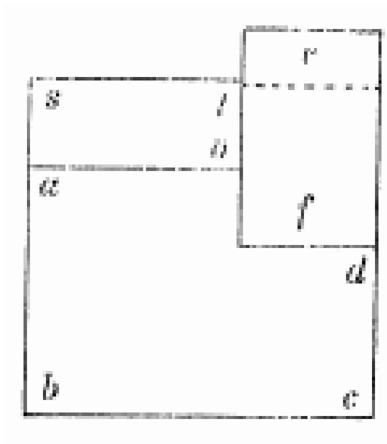

FIGURE 4.2

Diagram illustrating Galileo's demonstration "that bodies of the same heaviness as the medium move neither upward nor downward", from the treatise version of *De motu* (*Opere* I, p. 255); if the solid body *ef* would not be completely immersed, then, since the volume *so* of the water that is raised because of the immersion of the solid body equals the immersed part *f* of it, and the weights of *so* and *f* as a result are also equal, the solid body *ef* will be trying to move downwards with more pressure than the water *so* can resist, and we will have motion towards the state were there is equilibrium; i.e. the solid body completely immersed.

5 Artificial machines, natural motion

That the Aristotelian distinction between the natural and the artificial underwent profound changes at the beginning of the seventeenth century is a commonplace of the history of science and philosophy. Among the most significant consequences seem to be the effacement of an ontologically differentiated picture of the natural world, which gets replaced by a world ruled by one uniform set of laws of nature; and the opening that is thus created for an experimental way of doing science, as human interventions are no longer in se opposed to the natural order. As a result it also became thinkable to subsume all of physics under the title of mechanics, which up till the middle of the seventeenth century mainly referred to the theory of machines.

Most discussions of these transformations focus on Francis Bacon and René Descartes, who both clearly stated that there exists no difference in principle between artificial and natural things. It is clear that both men indeed played crucial roles in the overthrow of the hegemony of Aristotelian philosophy, but it must also be noticed that neither of them paid a lot of attention to the science of (artificial) machines that was already well established by the end of the sixteenth century.[382] *But as we have seen in the preceding chapter, it is exactly in this well-circumscribed context that the issue about the status of the artificial arises for someone as Galileo, through the question whether an instrument such as a balance can provide the principles for natural philosophy. It is clear that this can only be answered affirmatively once the place of machines in the "natural" world has become radically rethought.*

In this chapter, I will accordingly analyse two especially significant sixteenth century writings on the science of mechanics with an eye to how their authors construe the domain of mechanics. (As could have been easily surmised, these writings are respectively Guidobaldo's and Galileo's.) It is well known that the introduction of mechanical treatises contains a wealth of information on how the authors tried to position their knowledge within a broader field of knowledge and practice.[383] *However, the discursive organisation of the content of the treatises itself is as at least as revealing.*[384] *It is only by paying sufficient attention to the structure these authors impose on their knowledge that we can fully ascertain the often subtle ways in which they construe the coherence of the domain of their science. So my focus will be on the following question: what do their theories of the* working *of machines betray about the relation between the artificial and the natural?*

[382] This is not completely true with respect to Descartes who was interested in, and wrote (although really not much) about mechanics. But then the fact remains that the kind of experience that he had of this science was exactly shaped by the prior intervention of people such as Galileo; which only provides further reason to focus attention on how the latter restructured the relation between this science and nature.

[383] Cf. already chapter 2, section 2.2.2.

[384] Cf. chapter 3.

5.1 Aristotelian teachings

5.1.1 Aristotle on the artificial and the natural

The distinction between the artificial and the natural is one of the central organizing themes of the second book of Aritstotle's *Physics*. In this book, Aristotle tries to get a grip on what makes us say that some things exist naturally by analyzing what these things share with artificial things, and what nevertheless sets them apart. He immediately brings up the well known answer that natural things have an internal principle of change; or, that "nature is a source or cause of being moved and of being at rest in that to which it belongs primarily, in virtue of itself *(per se)* and not in virtue of a concomitant attribute *(per accidens)*."[385]

If *we* want to get a grip on Aristotle's answer, it is important that we try to get some feel for the kind of question he was trying to answer.[386] Put somewhat bluntly, he is trying to make sense of the fact that a tree is essentially *that*: a tree. Put a little bit more circumspectedly, he is trying to analyze what makes for the *unity* of the individual things in the world; what it is that constitutes this unity. And this is where the comparison with artificial things becomes relevant. *Being a bed is being recognized as being the kind of thing that was produced to that end.* Its principle of change is the human know-how in producing it, which is also what guides our recognizing it for what it is. Know-how, or *techne,* is accordingly the source of being moved and being at rest in artificial things. Remember that Aristotle defines motion as "the fulfilment of what exists potentially, insofar as it exists potentially"[387].

Hence, some things are what they are because we know what to do with them. And this know-how also enables us to give the right kind of shape to some material that is appropriate to that end. This is then what constitutes these things' identity. But other things are what they are, not because of what we can do with them, but because of what they do (or don't) out of themselves. They appear as unities because they actualize a set of characteristic properties in matter; and they do so without any human intentions intervening. Their identity is *given* to us in experience, and not imposed by us. And that they are true unities is shown by the fact that in actualizing their form they go through changes in which they keep their identity throughout. In Aristotle's words: "We … speak of thing's nature as being exhibited in the process of growth by which its nature is attained"[388] – just as in the production of a bed all steps are directed to that end; *an end which shows itself through the production.*

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[385] *Physica* II.1, 192b. (Transl. from Aristotle 1930.)

[386] For an exciting, although somewhat idiosyncratic interpretation of book II of Aristotle's *Physics*, see Heidegger 1967. See also Weisheipl 1982.

[387] *Physica* III.1, 201a. (Transl. from Aristotle 1930.)

[388] *Physica* II.1, 193b. (Transl. from Aristotle 1930.)

There are things and processes that can be accounted for as the work of man, and there are things and processes that have their own form of work. In short: there are artificial things and natural things, but there exists a strong analogy between both cases – "If, therefore, artificial products are for the sake of an end, so clearly also are natural products. The relation of the later to the earlier terms of the series is the same in both."[389] It is in the same sense that Aristotle famously claims that art imitates nature. Both are strongly goal-directed: "each step in the series is for the sake of the next." *A thing's unity is always constituted by the interplay between matter's appropriateness and form's purposefulness*. But it is precisely because of this that also artificial things do not stand outside the order of physical necessities.

> Similarly in all other things which involve production for an end; the product cannot come to be without things which have a necessary nature, but it is not due to these (except as its material); it comes to be for an end. For instance, why is a saw such as it is? To effect so-and-so and for the sake of so-and-so. This end, however, cannot be realized unless the saw is made of iron. It is, therefore, necessary for it to be of iron, if we are to have a saw and perform the operation of sawing.[390]

A thing's ontological identity is thus determined by its end (whether this is artificial or natural), but its existence (or what is the same, its proper functioning) is dependent on the presence of appropriate material stuff. Both physical investigations and investigations in the workings of artificial things are as a result directed towards uncovering the qualitative causal nexus that underlies the teleological organization of these things.

Such investigations can often be directed towards the same object, but from complementary perspectives. A bed can be considered insofar as it is *that*: a bed, a product of human art. But it can also be considered insofar as it is *wooden*, that is, made of particular natural stuff. Yet, in the latter perspective, we are not dealing with the nature *of* the bed, but with the nature of all things wooden. This distinction was of course already signalled in the extra clause in Aristotle's definition of nature as "a source or cause of being moved and of being at rest in that to which it belongs primarily, in virtue of itself *(per se)* and not in virtue of a concomitant attribute *(per accidens)*." A piece of wood is only accidentally a bed, but it is wooden in virtue of its nature.

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[389] *Physica* II.8, 199a. (Transl. from Aristotle 1930.)

[390] *Physica* II.9, 200a. (Transl. from Aristotle 1930.)

5.1.2 The Mechanical Problems

Somewhere in the third century before Christ someone who was familiar with Aristotelian philosophy composed a treatise on mechanical problems.[391] It treats many simple devices, such as the lever, and offers a theoretical analysis of a host of practical problems. Because it was generally ascribed to Aristotle himself, the influence of the treatise on sixteenth century mechanics was enormous, especially for its opening paragraph which offers a brief characterization of the science of mechanics.

> Remarkable things occur in accordance with nature [*kata physis*], the cause of which is unknown, and others occur contrary to nature [*para physis*], which are produced by skill [*techne*] for the benefit of mankind. For in many cases nature produces effects against our advantage; for nature always acts consistently and simply, but our advantage changes in many ways. When, then, we have to produce an effect contrary to nature, we are at a loss, because of the difficulty, and require skill [*techne*]. Therefore we call that part of skill which assists such difficulties, a device [*mechanè*]. … Of this kind are those in which the less masters the greater, and things possessing little weight move heavy weights, and all similar devices which we term mechanical problems. These are not altogether identical with physical problems, nor are they entirely separate from them, but they have a share in both mathematical and physical speculations, for the "how" [το ώς] is known by mathematics, the "about-what" [το περι ό] by the science of nature.[392]

A few important themes come together in this short paragraph, which would exercise many sixteenth century writers.[393] Mechanics starts from the consideration of marvels such as fact that light bodies can lift heavier ones. These are things that happen "para physis" but according to *techne*. And the science that studies these marvels gives mathematical explanations. I won't go discuss the mathematical character of these explanations here. Let me just remind you that this was of the utmost importance in setting apart the Renaissance mechanical treatises from mere handwork and craft knowledge.[394] In the present section I want to focus on the other three closely related topics: the import of the marvellous

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[391] Cf. chapter 3, section 3.3.1. See also chapter 2, section 2.2.2.

[392] Aristotle 1966, p. 330-331. I have amended the translation of the last sentence as Hett, inaccurately, has "the method is demonstrated by mathematics" in his translation; see Micheli 1995, p. 24, fn. 13. Micheli quotes the following sixteenth century translations: "porque el como es manifiesto, por las mathematicas, y el de que por las naturals" (de Mendoça); "etenim quod ipsum quomodo ad mathematica pertineat: ipsum vero circa quod, ad Physica, manifestum est" (de Monantheuil); "Quandoquidem mathematicum id certe est: ad quaenam referri possint cognoscere; physicum vero: quidcirca versentur" (Fausto).

[393] Cf. Micheli 1995 for an erudite treatment of the problems that surround some aspects of this paragraph, and for some aspects of its reception in the sixteenth century. See also Rose and Drake 1971; Laird 1986; and Festa and Roux 2001.

[394] Cf. chapter 2, section 2.2.

character of mechanical problems, the meaning of the expression "para physis", and the purposefulness of *techne*.

Let me point towards some complexities inherent in this paragraph. On the one hand mechanics is claimed to be about effects occurring "para physis". On the other hand its objects are said to be known physically. This double nature is of course completely in line with the Aristotelian discussions in the second book of the *Physics* as we saw in section 5.1.1. A house is built by exploiting the physical characteristics of its material, but its properties as a house are not according to nature. Most of the Renaissance commentators show themselves perfectly aware of the intricate character of this double characterization. They accordingly translate "para physis" most often as "praeter naturam" – outside nature, or above nature, rather than simply against nature. "Praeter naturam" was also the denomination for a host of other marvels: monsters, comets, prodigies. As a result, mechanical phenomena could find a natural place in the fine-grained catalogue of kinds of things between heaven and earth, so compellingly described by Loraine Daston and Katherine Park in their book *Wonders and the order of nature*.[395]

Let us in this respect have another look at the first sentence of the pseudo-Aristotelian treatise. Things that happen according to nature cause wonder when we don't know their causes, but phenomena that are "praeter naturam" cause wonder *tout court*. Coming to know the causal story behind their operation does not remove the wonder. This is also reflected in the explanatory structure of the *Mechanical problems*, which its author summarizes as follows:

> Now the original cause of all such phenomena is the circle; and this is natural, for it is in no way strange that something remarkable should result form something more remarkable, and the most remarkable fact is the combination of opposites with each other. The circle is made up of such opposites…[396]

I will not spell out the details of the full explanatory scheme,[397] but let it suffice to point out that this is no gratuitous rhetorical talk: the remarkable properties of a lever are indeed referred back to the remarkable properties of the circle. We have a *displacement* of the wonder but not a removal.

But there is more to be said about this wonder that inheres in things mechanical, and this is again connected with the broader category of "praeter naturam". Let me first quote Guidobaldo del Monte:

> For whatever helps manual workers, builders, carriers, farmers, sailors, and many others (in opposition to the laws of nature [*repugnantibus naturae legibus*]) – all this is the province of mechanics. And mechanics, since it operates against nature [*adversus naturam* …] or rather in

[395] Daston and Park 1998; cf. also Daston 1998.

[396] Aristotle 1963, pp. 332-333.

[397] See however chapter 3, section 3.3.1 for a (very) short description.

> rivalry with the laws of nature [*vel eiusdem emulate leges exercet*], surely deserves our highest admiration.[398]

I will come back to this passage in section 5.1.3. For now, I just want to point out how the fascination that went along with the category of "praeter naturam" was often directed towards the human ingenuity involved. As is well-known, the Greeks already associated mechanics with *mètis*, cunningness.[399] By not taking the straightest road, but instead operating through a detour, man can overcome some of his natural deficiencies. As suggested by Guidobaldo, this is why this kind of *techne* must be highly praised. But this also implies that we cannot detach the human purposes from the objects of our wonder. It is precisely because they incarnate these purposes that they deserve our special theoretical attention. This brings us back full circle to the Aristotelian discussion in the *Physics*. Artificial things have their ends imposed on them by us; *take away this intentionality and they become utterly unintelligible*. A lever is what it is because we use it to lift heavy weights – that's what constitutes its basic unity.

5.1.3 The artificial and the natural in Guidobaldo's mechanics

Guidobaldo's *Mechanicorum liber,* published in 1577, is maybe the most influential Renaissance treatise on mechanics. As I argued in chapter 3, the subtleties of Guidobaldo's writings on mechanics have not always been sufficiently grasped. It was seen how he presented an utterly original synthesis between Aristotelian elements, as are found in the *Mechanical problems*, and the Archimedean treatment of the equilibrium of bodies, which centrally involves the notion of centre of gravity. By bringing in Aristotelian elements, Guidobaldo was able to provide the highly abstract Archimedean scheme with a concrete and sensible interpretation, which moreover enabled him to incorporate the resulting theory nicely within a broader Aristotelian framework.

But before seeing what this signifies for the issues discussed in the present chapter, let me first come back to this earlier quoted passage:

> For whatever helps manual workers, builders, carriers, farmers, sailors, and many others (in opposition to the laws of nature [*repugnantibus naturae legibus*]) – all this is the province of mechanics. And mechanics, since it operates against nature [*adversus naturam* …] or rather in rivalry with the laws of nature [*vel eiusdem emulate leges exercet*], surely deserves our highest admiration.

[398] del Monte 1577, unnumbered preface. (Transl. from Drake and Drabkin 1969, p. 241.)

[399] Cf. Micheli 1995, chapter 1; Vérin 1993, chapters 2, 3. As Vérin recounts, part of the suspicions concerning its moral character that surrounded mechanics and its practitioners during the Middle Ages was also connected with this association.

By inaccurately translating *emulate* by “in rivalry with”, whereas it can also be rendered as “in imitation of,” Stillman Drake has obscured the perfect Aristotelian sense of this passage. Remember our discussion of Aristotle’s *Physics*: both nature and art organize their objects according to a similar logic. Just as a bed can be considered from two perspectives, this is also true for all mechanical devices. If a heavy body is lifted, it is made to undergo a motion that is contrary to *its* nature, but this feat of art is achieved by cleverly exploiting the natural characteristics of the material out of which the machine is constructed. Again, for a saw to perform *its* function, it is necessary that it is made from a material such as iron that has some natural properties of its own, but performing its function as a saw in no way is part of iron’s nature.

One striking un-Aristotelian element in this passage is the recurrent use of the expression “law of nature”. This is not unique to Guidobaldo: other Renaissance writers used the same expression in exactly the same context.[400] Yet, although this use certainly does not go back to Aristotle, and its occurrence does pose some interesting questions of its own, we must also be careful not to read too much into it. The fact that mechanical events are said to be in opposition to these laws *by someone who is very eager to elevate mechanics to the status of a true and noble science* signals the distance between this use and a later understanding of the expression. As Guidobaldo claims, it certainly deserves our highest admiration that we can make objects do things against the laws of (their) nature, but this does not throw the least doubt on the validity of the ascription of this nature to them, nor does this render these human acts impossible. The notion of laws simply refers to the general order of nature, which shows itself in what happens normally, but not invariably – Aristotle’s nature is a nature with room for exceptions.[401] In contradistinction to modern scientific laws, Guidobaldo’s laws are true laws that can be transgressed.

Let us now try to see how mechanical devices operate against nature by exploiting natural properties. We have already seen in the third chapter how Guidobaldo’s conceptualization of mechanical phenomena was essentially structured around the interplay between the three centres. In his own words:

> Now our author is the first to have considered the balance in detail and to have understood its nature and its true quality [*intenderla dalla natura e dal vero esser suo*]. For he is the first of all to have shown clearly the way of dealing with it and teaching about it, by propounding three centres to be considered in its theory: one is the centre of the world, another the centre of the balance, and finally the centre of gravity of the balance: for in this was a hidden secret of nature. Without these

[400] E.g., both Vittore Fausto and Leonico Tomeo use the expression, as can be seen in the quotes in Festa and Roux 2001, p. 243.

[401] Cf. especially *Physics* II.5 and II.8, where it is also explicitly stated that “mistakes are possible in the operations of nature” 199a-b. On this characteristic of Aristotelian nature, and part of its aftermath in the seventeenth century, see Dear 1990.

three centres, it is clear that one could not come to a perfect knowledge or demonstrate the various properties of the balance…[402]

To quickly recapitulate the explanatory scheme: why is a light body *B* able to lift a heavy body *A* (see figure 5.1)? Because their common centre of gravity *C* lies to the right of the centre of the balance, the fulcrum *D*, and this centre of gravity has a tendency to move towards the centre of the world whereas the fulcrum must remain stationary. As a result of the interplay between these three centres, *A* moves down and *B* moves up. We can conclude, as Guidobaldo himself states at another place, in his 1588 *In duos libros…,* that the weight ascends contrary to its proper nature *but still naturally*.[403] So, what is it that art brings about? Nothing more than that it suitably places things with respect to each other, after which it just lets nature run its course.

Guidobaldo's conceptualization of mechanical phenomena nicely and exemplary brings out *how being-a-machine depends on being-composed-of-natural-material*. That is, how the human intentionality is not so much freely imposed on matter, but cunningly exploits the natural teleological constitution of all things natural – how art imitates nature. We see how the different ways of considering the same thing, as an artifact and as an object made out of natural constituents, intermesh in the case of machines. The abstract notion of a centre of gravity is obviously a crucial element in Guidobaldo's explanatory strategy. It was already analyzed in sufficient detail in chapter 3 how this notion was crucially linked with the cosmological constitution of the world for Guidobaldo. It is this notion that enabled him to link the "how" of mathematical demonstrations with the "about what" of its empirical instantiations.[404] It is mainly as a result of this intimate link between the mathematical and the physical part of his science that Guidobaldo leaves open no other way of understanding the effect of power than by assimilating it to a weight having a natural tendency downward, which can be introduced in arguments involving centres of gravity. All machines must be reduced to situations where human or animal power is conceptually replaceable by freely hanging weights. Being a machine depends on having parts with well determined centres of gravity – *all other conceptualizations would threaten its place as an artifact in the physical world*.[405]

[402] del Monte 1581, p. 28r. (Transl. from Drake and Drabkin 1969, p. 294.)

[403] "pondus A contra propriam naturam naturaliter ascendet" del Monte 1588, p. 3.

[404] See especially chapter 3, sections 3.2.2, 3.4.2, and 3.6.1.

[405] This explains why Guidobaldo, notwithstanding the fact that he showed himself capable of understanding the effect of the directionality of an applied power, chose not to incorporate this explicitly into the conceptual structure of his mechanics, and instead preferred to reduce all problems to considerations of centres of gravity (cf. chapter 3, section 3.5).

5.2 Galileo's conceptualization of mechanics

5.2.1 The conservation of *momento*

It was already mentioned that Guidobaldo was one of the earliest and most important patrons of Galileo. Both men also corresponded on scientific matters and did some experiments together. It is therefore no surprise that we find important elements of Guidobaldo's mechanics recurring in Galileo's mechanical writings. But this also implies that the important conceptual differences that nevertheless exist must be considered significant. They clearly signal that Galileo was quite consciously trying to do something else in his conceptualization of mechanical phenomena.

There exist two different versions of Galileo's treatise on mechanics, which he used during the 1590's for courses, the second of which was first published in 1634 in a French translation by Mersenne.[406] One of the most conspicuous differences between both versions is the introduction that is only appended to the most extended version, which is in all respects a rather drastic reworking of the first version (and which is also the one translated by Mersenne). I will first discuss the body of the work and the most important aspect of the conceptualization of mechanical phenomena as it is presented there, and only then comment on this remarkable introduction – this is in all probability also the route taken by Galileo: it is the conceptualization that provided the elements for the introduction, and not vice versa.

I already explained how the Archimedean notion of centre of gravity played a crucial organizing role in Guidobaldo's mechanics. In the extended version of his treatise, Galileo also opens his explanation of mechanical phenomena by introducing a proof of the law of the lever which is based on Archimedes' proof. His proof contains many traces of Guidobaldo's earlier explanations: the attention for the interplay between centre of gravity and point of suspension, and the relation between centre of gravity and tendency towards the centre of the world. Let me quickly summarize the proof. (I will gloss over many important points to focus attention on what is of most interest to the present discussion).

A uniform solid is suspended at its endpoints from a line *AB* which at its turn is suspended at the point *G* exactly in the middle (see figure 5.2). It will be in equilibrium. Now divide the solid in two unequal parts, and add an extra string at the point of division. It remains in equilibrium, as it will also if we now hang it from two other strings right above the parts' respective centres of gravity and cut the other strings. At this point follows a geometrical proof of the fact that the ratio of the weights of the two unequal parts equals the ratio between the distances from which they are respectively suspended. Galileo then comments as follows:

[406] Galilei 2002 contains a critical edition of both versions. See Mersenne 1966 for his translation of the treatise.

> And from what has been said it seems to me clearly understood not only how the two unequal bodies *CS* and *SD* weigh equally when hanging from distances inversely proportional to their weights, but moreover how, in the nature of things, this is the same effect as if equal weights were suspended at equal distances, since in a certain sense the heaviness of the weight *CS* virtually spreads out beyond the support at *G*, and that of the weight *SD* shrinks back from it, as any speculative mind can understand by examining closely what has been said about the present diagram.[407]

An argument which he summarizes as follows a few lines further:

> Having shown how the *moments* of unequal weights are equalized by being suspended inversely at distances having the same ratio…[408]

This gloss is of course only comprehensible given the definition of "momento" which was introduced earlier in the treatise:

> Moment is the tendency to move downward caused not so much by the heaviness of the moveable body as by the arrangement which different bodies have among themselves.[409]

I will not go into the complex history of this term, neither comment on its multiple meanings which play an important role in the development of Galileo's further scientific writings.[410] My focus here is on the use to which Galileo puts this novel concept in his theory of the simple machines. But it is important to recall that this notion was first introduced in the same context by Commandino who had defined centre of gravity as that point around which the parts of a body have equal moment. Guidobaldo evidently knew this definition, but never really made much use of this notion (cf. chapter 3). As we will see, Galileo's use of it is much more far-reaching than Commandino's who never takes it beyond a strictly Archimedean context.

Now let us go back to the conclusion that Galileo drew from his proof of the law of the lever. In a striking piece of visual reasoning he teaches his readers *to see* what makes for equilibrium in mechanical situations: one can see how the relative positions of the respective centres of gravity are responsible for the fact that the effect of the separate bodies' weights are distributed over space in such a way that they are conceptually reducible to a situation where a single body is hanging from its two end points. In this way one can *see through the apparent marvelousness of this kind of situation* and *perceive the underlying and inherently stable configuration.* This is then brought out explicitly by the

[407] *Opere* II, p. 163. See also Galilei 2002, p. 52. (Transl. from Galilei 1960, p. 155.)

[408] *Ibid.*

[409] *Opere* II, p. 159. See also Galilei 2002, pp. 48-49. (Transl. from Galilei 1960, p. 151.)

[410] Cf. chapters 7 and 8 for some aspects of this story. Settle 1966 and Galluzzi 1979 are main sources for analyses of this concept in Galileo's thinking.

introduction of the abstract concept of "moment". In this move Galileo reasons himself from Guidobaldo's understanding of mechanical phenomena as essentially caused by the relative position of centres of gravity with respect to a fixed point to a still more abstract stage. He sees that all equilibrium situations can be characterized by the fact that *in a sense they are all the same*.

Immediately after his Archimedean-style proof, Galileo introduces another way of considering the same situation. In this passage he offers a proof of the law of the lever based more closely on the proof procedure in the pseudo-Aristotelian *Mechanical problems*. This time he asks to consider what would happen if the two bodies *A* and *B*, situated at different distances on a balance, would start to move (see figure 5.3). Since they would move on circles with a different radius but a common centre, the speed of the body farthest from this centre would be proportionally faster. He concludes that

> [It is no] wonder that the weight *A* cannot be raised to *D*, though slowly, unless the other heavy body *B* is moved to *E* swiftly; and it is not foreign to the arrangement of nature that the speed of the motion of the heavy body *B* should compensate the greater resistance of the weight *A* when this moves more weakly to *D*, and the other descends more rapidly to *E*.[411]

After which he again concludes that

> From this reasoning we may arrive at the knowledge that the speed of motion is capable of increasing moment in the moveable body in the same proportion as that in which the speed of motion is increased.[412]

There are again much more subtleties involved in this proof, but it is clear that there is one vision dominating Galileo's understanding of mechanical phenomena: whilst it may seem as if we are always dealing with unequal bodies, one heavy and strong and the other weak, this is because we do not fully comprehend the invariancies that actually underlie these situations; invariancies which can only be discovered by geometrically analyzing the appropriate diagrams. Mechanical devices are characterized by the conservation of moment. This is the common core he retracts from both the Aristotelian and Archimedean treatments of mechanics.

5.2.2 The transformation of *momento*

Equipped with this understanding Galileo then analyzes all simple machines. He closely follows Guidobaldo's reduction of the pulley to a combination of levers, but there is one important difference: the complete disappearance of considerations of centres of gravity. Moment has become

[411] *Opere* II, p. 164. See also Galilei 2002, p. 53. (Transl. from Galilei 1960, p.156.)
[412] *Ibid.*

the commanding concept.[413] The importance of this transformation becomes clear if we consider how Galileo puts the concept to work in analyzing the *working* of a simple instrument such as the lever. In this analysis he goes a step further than he did in his arguments leading up to the law of the lever, which were aimed at justifying the concept of moment rather than at using it to further ends, which is exactly what he does now.

He starts from a diagram similar to the one used earlier, where the distance *CD* is assumed five times the distance *CB* which equals *CL* (see figure 5.4). A body placed at *D* will have the same moment as a body five times as heavy that is placed at *B*. So the body at *B* can be moved to *G* by such a body, if we assume that an infinitesimal weight added to this body is enough to set the lever into motion.[414] But considered from the perspective of conservation of moment, this is exactly the same thing as saying that a body five times lighter than the body at *B* can also be moved by the same body if we place it at L, since the proportionality that is expressed through the equality of moment remains invariant. And if we repeat this action five times, we can move the complete body that was placed at *B* to *G*.

> But to repeat the space *ML* is certainly nothing more nor less than to traverse a single time the interval *DJ*, five times this *LM*. Therefore to transfer the weight from *B* to *G* requires no less force and no less time or any shorter travel at *D*, than what is required when applied at *L*. And to sum up, the advantage acquired from the length of the lever *CD* is nothing but the ability to move all at once that heavy body which could be conducted only in pieces by the same force, during the same time, and with an equal motion, without the benefit of the lever.[415]

In this further analysis Galileo moves from a consideration of *conservation* of moment to a more fine-grained analysis of the *transformation* of moment that is effected through a mechanical machine. A machine is a device for redistributing moment over space. Instead of cutting up a heavy body in parts which could be transported by a given force without a machine, it allows one to transport the whole body by making the moving force traverse a proportionally larger distance.

It's now time to move to the introduction of Galileo's treatise. Let me first quote the first paragraph in full:

> It has seemed well worthwhile to me, before we descend to the theory of mechanical instruments, to consider in general and to place before our eyes, as it were, just what the advantages are that are

[413] Among other things, this allows for an easy inclusion of other forces than weight within his conceptual structure, and Galileo accordingly can easily avoid the problems that Guidbaldo had to conceptualize the effect of the direction in which a power is applied to a machine.

[414] This condition of course neglects friction at the fulcrum, and hence oversteps what Guidobaldo thought to be permissible in mechanics (cf. chapter 3, section 3.6.2). In chapter 6, section 6.1.3, I will discuss what lies behind this profound difference in vision between Galileo and his patron.

[415] *Opere* II, p. 167. See also Galilei 2002, p. 56. (Transl. from Galilei 1960, p. 159.)

drawn from those instruments. This I have judged the more necessary to be done, the more I have seen (unless I am much mistaken) the general run of mechanicians deceived in trying to apply machines to many operations impossible by their nature, with the result that they have remained in error while others have been likewise been defrauded of the hope conceived from their promises. These deceptions appear to me to have their principal cause in the belief which these craftsmen have, and continue to hold, in being able to raise very great weights with a small force, as if with their machines they could cheat nature, whose instinct – nay, whose most firm constitution – is that no resistance may be overcome by a force that is not more powerful than it.[416]

Whilst it is true that small weights may raise greater weights through the use of machines, this does not imply that a smaller *force* has overcome a greater resistance. Moment, which gives a measure for the force which is *actually exercised* through a machine, is always equal at the sides of the moving force and the resistance.

The contrast with the introduction to the earlier short version of his treatise is striking; this introduction actually consisted of one sentence, entirely traditional in the delineation of its subject: "The science of mechanics is that faculty which teaches the reasons and shows the causes of miraculous effects concerning the moving and lifting of great weights with little force that we see done with diverse instruments."[417] In his later version Galileo will correct himself and state that this is *not done* with little force, although only a little force is used – but it is used over a long path; it is put to a lot of "work".

5.2.3 From Guidobaldo's to Galileo's conceptualization

At one point in his *Mechanicorum liber*, Guidobaldo had also already stated the following corollary:

> It is also evident that the more easily the weight is moved, the greater will be the time; and the greater the difficulty with which the weight is moved, the shorter the time; and conversely.[418]

Yet nowhere does he enunciate this as a general principle upon which to build the science of mechanics. It is clear that he judged the dynamics between the three centres, the centre of gravity, of the balance, and of the world, as much better suited to play this role. This is probably due to a general humanistic project aimed at restoring the ancient science of mechanics, and as became especially clear from our analyses in chapter 3, it is crucially connected with the dispute over the possibility of

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[416] *Opere* II, p.155. See also Galilei 2002, p. 45. (Transl. from Galilei 1960, p. 147.)

[417] Galilei 2002, p. 5

[418] del Monte 1577, p. 105v. (Transl. from Drake and Drabkin 1969, p. 317.)

indifferent equilibrium in which he was engaged. It is clear that to his mind centre of gravity was *the* crucial element for a rational organization of mechanics.

In the first version of Galileo's treatise practically the same statement also appeared as a corollary:

> But it must be remarked that so much as we make it easier on ourselves using a lever, that much more time will we have to take; and that so much as the force will be less than the weight, that much larger will be the distance over which the force travels than the distance over which the weight travels.[419]

Somewhere in between this first version and the composition of the second version, i.e. during the 1590's, Galileo must have realized the potential of this enunciation as a general mechanical principle. Rather than thinking of a machine as an instrument to shift the centre of gravity of bodies, he starts to think of it as an instrument to redistribute moment. As we saw, all machines will now be characterized by the fact that they conserve an abstract quantity. And whereas it was commonly stated that mechanics brings about phenomena that are "praeter naturam", Galileo now reproaches "the general run of mechanicians" that they talk as if they could *cheat nature.* It is very probable that the transition to this new way of framing the problem was suggested to Galileo by his reformulation of his treatment of mechanical phenomena by means of the concept of moment. One of the main attractive features of this principle must have been the possibility that was thus opened of clearly delineating the objective limits of what could be achieved through the use of machines. But, as I will now explain, in this move he radically transforms the meaning of nature.

5.3 The nature of Galileo's nature

5.3.1 Against the general run of mechanicians

Let me first go back to some of the crucial passages in which Galileo introduced the idea of the conservation of moment in his treatise:

> And from what has been said it seems to me clearly understood not only how the two unequal bodies *CS* and *SD* weigh equally when hanging from distances inversely proportional to their weights, but moreover how*, in the nature of things* [*in rei natura*]*,* this is the same effect as if equal weights were suspended at equal distances...
>
> *[It is no] wonder* [*non sarà maraviglia*] that the weight *A* cannot be raised to *D*, though slowly, unless the other heavy body *B* is moved to *E* swiftly; *and it is not foreign to the arrangement of*

[419] Galilei 2002, p. 7.

> *nature* [*né alieno dalla costituzione naturale*] that the speed of the motion of the heavy body *B* should compensate the greater resistance...

In both passages we see how Galileo equates conservation of moment with what is natural. The fact of this conservation is moreover enough to remove all wonder from this kind of mechanical phenomena. By linking this idea of conservation with the arrangement of nature, Galileo obviously changes what it might mean to do things that go against or lie outside this arrangement.

When writers like Guidobaldo claimed that mechanical phenomena were outside nature, or that artisans worked in opposition to its laws, they were evidently not claiming that they were able to overstep the boundaries of what was possible; they merely showed their awareness of the Aristotelian way of identifying objects by the origin of their principles of coming into existence and of organization. Yet when Galileo states that it is impossible to achieve any effects that are "outside the constitution of nature" he is trying to ascertain the boundaries of the possible and the impossible. Galileo's awareness that he is doing something else is testified by the fact that all explicit references to the traditional *topoi* from the introduction to the pseudo-Aristotelian *Mechanical problems* have disappeared from his own introduction. The fact that he instead chooses to attack the idle illusions of what he calls "the general run of mechanicians" is also very revealing and significant. Through this move he is shifting the legitimization for the science of mechanics from the topic of wonder to that of cognitive control.

A striking parallel is to be found in Salomon de Caus' *Les raisons des forces mouvantes*. This author introduces a very sceptical message with respect to his predecessors, whom according to his judgement only knew how to invent on paper.[420] Let me quote one very revealing passage from the dedication of his book to the French king, where he warns him that

> Les Princes sont souvent solicitez de tels Architectes & ingenieurs (plustost remplis de vaines imaginations que de bons fondements) pour leur faire entreprendre des ouvrages lesquelles ne peuvent aporter aucune utilité ni plaisir…[421]

As is clear from a passage from the short introduction to his work, the main thing he reproaches these engineers is that they do not realize that time is also an important factor that must be accounted for in the operation of mechanical instruments.

It is clear that this places Galileo's introduction in an interesting light; and even more so if we add the following quote from the closing paragraph of his own introduction:

[420] He refers explicitly to Jacob Besson and Augustin Ramelli, authors of two of the most well-known so-called "Theaters of machines", a genre to which de Caus' book can also be taken to belong. He clearly tries to distance himself through his introduction, however, and his treatment is indeed more theoretically oriented. On this genre, see the next section 5.3.2.

[421] de Caus 1615, unnumbered dedication.

> These, then, are the utilities that are drawn from mechanical instruments, and not those which, to the deception of so many princes and to their own shame, engineers of little understanding go dreaming about when they apply themselves to impossible undertakings.[422]

It is of course tempting to speculate on the possibility that de Caus, who had spent some time in Italy, knew Galileo's treatise (which seems to have been rather widely distributed); but I am in the first place interested in the fact that, whatever the source of his inspiration, he thought it was interesting and possibly rewarding to take this kind of stance in his dedication. This recurring feature seems to testify to the reality of the experience that rulers were often confronted with engineers who were unable to bring into practice the splendid projects they had promised; or at least that this could be perceived as a possible threat to the dignity of a ruler. It is a well known story that Galileo got himself into troubles early in his career by unfavourably judging the possibility of a project designed by Giovanni de' Medici, the natural son of Grand Duke Cosimo. And after he became court mathematician in 1610, on different occasions he was called upon to judge the quality of proposed projects and new inventions.[423] There seems to have been an institutional place for someone who claimed to be able to discipline the ambitions of the general run of mechanicians by passing judgements on the possibility or impossibility of their proposals, which of course confirms the general analyses of chapter 2, section 2.2.

5.3.2 Shifting *topoi*

Within an Aristotelian framework there was nothing paradoxical about the fact that one could achieve effects that were "praeter naturam", but it was in no way made into a theme whether there were any limits on these effects. This question was rather to be relegated to moral considerations, because of the close link with cunningness.[424] But the latter category changed some of its moral connotations during the Renaissance, as the appearance of the genre of the theatres of machines at the end of the sixteenth century bears striking witness.[425] These collections of engravings of mechanical inventions, mostly accompanied by very brief descriptions, enjoyed a wide popularity, but it is important not to misjudge the relation of these magnificent books to the actual artisanal practice. Rather than providing blueprints for actual machines or codifying elements of practice they seem to be

[422] *Opere* II, p. 158. See also Galilei 2002, p. 48. (Transl. from Galilei 1960, p. 150.)

[423] Cf. e.g. Westfall 1989 for one such occasion. Another example is to be found in *Opere* VIII, pp. 571-581. Cf. also *Opere* IV, p. 32, referred to *infra*, in section 5.4.1.

[424] Vérin 1993, chapter 1, traces some of these moral question that surrounded the cunningness of mechanics. For some of the moral connotations that went together with the general category of "praeter naturam" and its transformations in early modern Europe, see Daston 1998.

[425] For some considerations on these still rather ill-understood works, see Keller 1978; Séris 1987, chapter 1; Vérin 1993, chapter 3; Vérin and Dolza 2002. Cf. also Popplow 2004.

intended primarily to display the ingenuity of their authors, who in this way advertise their capacities as engineers, capable of thinking out new projects. The depicted machines function as the incarnation of the engineer's *ingenium*, more or less along the same lines as Renaissance art theorists identified *disegno* – design – both with the artist's conception and the realization thereof in drawing. These engineers claimed to be people who knew how to transform very specific needs into projects that were capable of overcoming the many natural obstacles against the fulfilment of these needs. Cunningness had become something to be paraded.

I propose that we consider on the one hand the theatres of machines, and on the other hand writings such as Galileo's treatise and those of his followers, as two different discursive practices that were grafted upon the same artisanal practice. Whereas the theatres suggest a free play of the imagination, only constrained by the intentions that must be put into practice, Galileo is exactly regimenting this play of the imagination through his abstract analyses; he tellingly stresses that certain things are "absolutely impossible to accomplish with any machine imagined *or imaginable*."[426] These two discursive practices present us with two very different modes of carving out a space of possibilities. But the limits that Galileo draws are not the limits of practical feasibility or of efficiency; the limits to what we can do with machines are the limits that are imposed on us *by nature*.

It is revealing to see how the *topoi* from the introduction to the pseudo-Aristotelian treatise are still implicitly structuring Galileo's introduction, but in a profoundly transformed configuration. Wonder has ceased to be a central cognitive category, but it is immediately replaced by control. And whereas the former category was intimately linked with the issue of what it meant to be *praeter naturam*, the centrality of the latter category is due to the fact that nothing or nobody can overstep the *boundaries* of nature. But this reconfiguration is bound to have far-reaching consequences for the place of human agency in the discourses on machines, as we have seen this to be also inextricably connected with the topic of wonder.

This allows us to understand how an otherwise mysterious process can take place.[427] As "nature" functions as a regulative normative instance within any discursive practice, a change in its import cannot simply take place *on account of* nature. That is, as long as it is not yet present, nature itself cannot force changes in its presence. But this implies that some kind of strong form of idealism or solipsism would seem to be the only options if we want to hold on to the idea that there have been, and still can be, upheavals in historical configurations that have as effect that "nature" takes on a whole new mode of functioning. This is of course one of the classic objections that have been levelled against attempts to develop a relativized Kantian position (as witnessed e.g. by the strong reactions that were provoked by Kuhn's *Structure of scientific revolutions,* especially when it comes to the

[426] *Opere* II, pp. 156-157. See also Galilei 2002, p. 46. (Transl. from Galilei 1960, p. 148. My emphases.)

[427] Cf. already chapter 1, section 1.3.1.

world-change talk)[428]. It is at this point that an archaeological approach can be especially fruitful. By taking into account that "nature" never functions in isolation, and that we are always dealing with a complex configuration that first allows the interdependent factors to play their particular functions, there is more room to understand how change can take place. In the present case, we can see how it is possible for Galileo to install a new way of functioning for "nature" by exploiting an interlocking complex of elements that had an undeniable presence. (This is why it is not solipsism or idealism: it starts from presence.) He is able to introduce nature as a new kind of presence by holding fixed some of the structural relations that hold between a legitimizing cognitive goal (or if one wants a "sensibility"), respectively wonder and control, and the functioning of nature; by grounding this goal in a social reality which can give it its legitimacy; and by showing how this can be seen to actually structure the working of machines, i.e. by exploiting an abstract mathematical structure that was already noticed by Guidobaldo but which had not been accorded any special significance by him.

5.3.3 Science and technology

The disappearance of wonder as a central category and its replacement by control is again nicely brought out by comparing the theatres of machines with Galileo's treatise. Whereas the former stress the *functions* of the depicted machines with all the pictorial and textual means that are at their disposal, the specific uses of the machines have completely disappeared from Galileo's treatment. Rather than being expressions of human agency, machines have become exemplifications of the inviolable principles which constrain this agency. The *identity* of a machine no longer lies in its functional organization of material to a specific end, but in the fact that it is a closed system that conserves the amount of moment that is put into it. It is the unity of nature rather than the intention of men that constitutes their ontological character. The disappearance of wonder as a central category goes together with the fact that function becomes external to a machine's identity.

We must be careful not to misconstrue the consequences of this disappearance of functional analyses from the body of Galileo's text. It is not that the machines are no longer considered to be useful tools to attain certain ends, but this *purpose has become something extrinsic to their functioning*. At the same time this opens up an independent space for pragmatics as distinct from the considerations on the internal constitution of the machines. This space is already prominently present in Galileo's introduction, which is tellingly titled "On the utilities that are derived from the mechanical science and from its instruments." As already explained, this utility has nothing do with the fact that mechanicians would be able to work *against* nature. No, this utility is derived from the fact that machines transform the moment that we put into them, thus enabling us to accomplish all of the following useful effects:

[428] Admittedly, this talk *has* often been confusing; I nevertheless submit that perfect sense can be made of it by developing a more sophisticated neo-Kantian position than Kuhn initially had at his disposal. Some parts of the present thesis are intended to contribute to such a position, but I can at this place only gesture towards this possibility.

- We can move a great weight without having to divide it into pieces.
- We can adapt their components to the circumstances in which they must work.
- Through their mediation we can use the force of several non-human sources of power, such as animals, water and wind.

In none of these cases there is extra force created, but in all of them the available force is put to greater utility. It is the task of engineers to implement these pragmatic advantages in concrete situations, but the *ends* which can be reached in this way have become plain *effects* rather than "principles of motion". Dealing with human intentions becomes the task of what can now become "technology".

I cannot go into the constitution of something like technology as an independent field of knowledge and practice. Let me just point out that it will be situated somewhere in between the two discursive practices that I have outlined here: it will share the attention for particular circumstances with the theatres of machines, but it will be guided by a logic of rational control on what is within its power to achieve.[429] And this control will make it possible to firmly anchor its attention for the particular within an economical logic, where it becomes of prime interest to calculate advantages and disadvantages.[430] Rather than ascertaining the boundaries of the possible, it will be eager to exploit the potentialities that lie within them. At this point it also becomes possible to think of technology as applied science – which however is not to say that it becomes applied science.

As the artificial ceases to be a distinct category in this new way of conceiving nature, it might be claimed that Aristotle's thinking was much closer to the artisans' world, which is clearly organized around a functional perspective on objects – we only have to recall his attention for the analysis of what makes a saw a saw. In a sense this is obviously true and makes for part of what is sometimes perceived as the poverty of a mechanical world view. Yet, there is another aspect of artisanal experience which completely escapes Aristotle's attention, and this is the awareness of frustrated expectations. It is clear for anyone who tries to construct such functional objects that not anything will work, that there are cases of persistent failure which cannot be immediately remedied. It would be foolish to claim that Aristotle was not aware of this fact; yet nowhere does he make these limitations on human agency in an independent object of knowledge.

Let me at this point also stress the important differences between the kind of process that I have been describing here and the point of view, which has been stressed by some authors since the 1980's, that highlights the importance of alchemy in breaching the boundaries between the artificial and the natural.[431] I don't want to deny the importance of this tradition, which had close links with scholastic philosophy and apparently had a major influence on Bacon. As William Newman states:

[429] Cf. especially Séris 1987 and Vérin 1993 on these issues.

[430] This is of course perfectly in line with Galileo's talk about the impossibility of cheating nature. The book of nature has become a bookkeeper's affair. (For Galileo's use of the metaphor of the book of nature, see chapter 9, sections 9.1 and 9.3.)

[431] See especially Newman 1989, 1998.

"alchemical texts from the high and late Middle Ages already were enunciating an attitude toward the art/nature division that was strikingly similar to the operative view of nature held by Francis Bacon and others in the Scientific Revolution."[432] What I do want to point out is that what I have isolated as particularly crucial in the new conceptualization of the identity of a machine, the setting of inviolable limits to their operation, seems to be completely missing in this tradition. Both traditions had their share in what we now isolate as *the* Scientific Revolution, but it is important not to loose sight of the important divergences that existed within these processes that happened more or less simultaneously.

Related to his stress on the alchemical tradition is the close association that had existed between mechanics and magic.[433] This is a coupling that exists until well into the seventeenth century (through people like Athanasius Kircher and Gaspar Schott). I take it that Galileo's stress on the inviolable limits imposed by nature, and the consequent disappearance of wonder as a central sentiment, can be seen as an important step towards the demise of this tradition. Technology is not something that has apparently limitless power; it has power exactly because it knows the limits of nature. Maybe the most important consequence of this process was the change in the moral status of man's knowledge and exploitation of machines. The image of an earnest, sober, calculating and *hence* objective investigator will come to replace the exalted magus. But all this actually deserves a separate and better documented treatment, so I will leave it at this.[434]

5.4 Galileo's new sciences of nature

5.4.1 Agency in nature

As Galileo starts to identify nature with this conservation principle, that is, with a principle that puts a limit on what is humanly possible to achieve, *nature becomes identified with what is beyond human will*. This will of course become an important rhetorical tool in his battles over the Copernican system, but I think it is important to realize that it finds it origin here in Galileo's thinking on machines.[435] It is also clear that it is not a terribly big step from this point to a picture of nature as governed by one uniform set of mathematically structured laws. In this respect it is very telling that Mersenne translates Galileo's introduction as follows:

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[432] Newman 1998, p. 87.

[433] Cf. e.g. Eamon 1983.

[434] See Daston 1995 for a particularly interesting historiographical framework from which to investigate these kinds of processes. It is clear that a complete archaeological investigation could not neglect these processes in which the possible positions for a scientific subject are changed together with its possible objects.

[435] Cf. chapter 9, section 9.XX.

> … que les artisans ne croyent pas qu'ils puissent servir aux operations don't ils ne sont pas capables, & que l'on puisse lever de grands fardeaux avec peu de force: car la nature ne peut être trompée, *ni ceder à ces droits…*[436]

His dedication to his translation contains even some more revealing passages:

> Mais j'estime que l'ordre & le règlement admirable que la nature observe dans les forces mouvantes, vous donnera encore plus de plaisir, parce que vous y verrez reluire une équité, & une justice perpétuelle qui se garde, & que l'on remarque si justement entre la force, la résistence, le temps, la vitesse & l'espace, que l'un recompense tousjours l'autre …
> Je croy que si la Justice pouvoit parler qu'elle confesseroit ingénuement qu'il n'y a nulle science naturelle qui luy soit si semblable, que celles des Mechaniques.[437]

It is of course needless to recall that Mersenne was one of the prime movers in the birth of a mechanical philosophy.[438]

These are the lines along which we must answer the question about the grounds for the representative power of the balance. As a machine is now thought of as exemplifying inviolable invariancies, and human intentions have become extrinsic to their ontological identity, the idea of using mechanical tools in investigating natural principles also looses much of its paradoxical character. We can formulate this even more strongly: *as the basic principles of this new mechanical science express the limits of our manipulative capabilities, it becomes natural to investigate these exactly through manipulations*. It is through our way of interacting with it that nature now can first truly show itself. This allows mathematical instruments that had been primarily practical problem-solving tools now also to function as investigative tools.[439]

It is this background that explains the novelty of Galileo's causal reasoning when compared with the Aristotelian one. We already noticed in chapter 4 how Galileo actually evacuated the Aristotelian cosmos from its causal and qualitative organizing structure, and replaced it by an organization structured around the model of a balance. Galileo remained silent on how to understand the causal structures responsible for this organization. This of course left the door open for the main complaint that the Aristotelian philosophers levelled against the mathematical sciences, i.e. that they are not scientific because they give no knowledge of causes.[440] We indeed have seen how a philosopher such as Mazzoni, who was close to Galileo and who had a very sympathetic attitude

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[436] Mersenne 1966, p. 23 (my emphases).

[437] Mersenne 1966, pp. 13-14, 16.

[438] Cf. Lenoble 1943, who pays especially attention to Mersenne's battle against Renaissance naturalism, a battle in which the inviolability of nature's prerogatives and the concomitant disappearance of the category of "praeter naturam" played a crucial role.

[439] I borrow the distinction between problem-solving and investigative tools from Bennett 1986, p. 2.

[440] Cf. chapter 2, section 2.1.2.

towards the mathematical sciences, basically agreed with this fact. That is, although it might seem plausible to assume that mixed sciences also involve formal causes (mathematical definitions), this can only be upheld if one is aware that one actually substantially changes the import of what makes for a formal cause, because the Aristotelian notion of substantial form simply plays no role within mathematical demonstrations.[441]

The 1612 *Discourse* on floating bodies provides some clear illustrations of how Galileo's own causal talk is directly linked to nature's new way of functioning as a regulative normative instance.[442] After having given an explanation that is entirely based on the conservation of moment in all instances of equilibrium, Galileo comments: "It appears to me that up to this point there has been sufficiently described and opened a road to the contemplation of the true, intrinsic, and proper cause of the diverse motions and of rest of different solid bodies in various mediums…"[443] In dismantling the Aristotelians' view that the shape of the bodies is the cause of floating, he moreover skilfully uses experimental models that allow him to vary one element at a time.[444] But this method of causal variations is of course only sensible given the regulative goal of establishing invariancies that hold under a well-circumscribed set of conditions.[445] (He even goes as far as claiming, although not in print, that "cause is that which put [placed], the effect follows; and removed the effect is removed."[446] This must – implicitly – be understood to hold true only under very specific circumstances that are held fixed.)

In the *Dialogue concerning the two chief world systems* Galileo gives the following general characterization: "Thus I say that if it is true that one effect can have only one basic cause, and if between the cause and the effect there is a fixed and constant connection, then whenever a fixed and

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[441] Galileo perversely demonstrates this in his Archimedean explanation of the Aristotelian cosmological structure in *De motu* (see chapter 4, section 4.1.3). When claiming that it was natural that the heavier elements should occupy smaller spaces because of their greater density, he expresses this as follows: "that the *form* of earth caused its matter to be concentrated in a very narrow place" (*Opere* I, p. 253. Transl. from Galilei 2000, p. 3; my emphasis.) But this "form" has now taken on an entirely geometrical character!

[442] It is interesting to note that in justifying his participation in the controversy on floating bodies in a letter to his patron, the grand duke, Galileo draws attention to the fact that because of exactly the same kind of theoretical considerations concerning the true causes of floating "your Highness well recalls how, four years ago, *I happened in your presence to contradict some engineers*, otherwise excellent in their profession". *Opere* IV, p. 32. (Transl. from Drake 1970, p. 164; my emphases.)

[443] *Opere* IV, p. 79. (Transl. from Drake 1981, p. 59.)

[444] *Opere* IV, pp. 88-89. Galileo cleverly chose to use wax as his central material, "since besides its receiving no sensible alteration from impregnation by water, it is tractable, and the same piece is very easily brought to any shape; while being very little less heavy than water, it can be brought to very nearly equal heaviness therewith by imbedding in it a few lead filings." (Transl. from Drake 1981, p. 75.)

[445] "La pensée préscientifique ne s'acharne pas à l'étude d'un phénomène bien circonscrit. *Elle cherche non pas la variation, mais la variété*." Bachelard 2004 [1938], p. 36.

[446] *Opere* IV, p. 22. (Transl. from Drake 1981, p. 217.)

constant alteration is seen in the effect, there must be a fixed and constant variation in the cause."[447] *The mathematization of nature has become thinkable for Galileo because he construes causal relations* as *relations*; i.e. they are expressible through constant ratios and this is why they can easily be integrated within mathematical demonstrations (which for Galileo comes down to manipulating them through his geometry of proportions).[448]

It is because nature never transgresses certain inexorable boundaries that Galileo's move in *De motu* and later in the *Discourse* on floating bodies cannot be transgressive either. Covered under the "protecting wings of the superhuman Archimedes"[449] he can fly freely over what the Aristotelians arbitrarily had posited as boundaries, constrained only by nature itself. In the *Assayer* Galileo answers the Aristotelian philosopher Sarsi, which was actually a pseudonym for the Jesuit Grassi, as follows: "Sarsi perhaps believes that all the hosts of good philosophers may be enclosed within walls of some sort. I believe, Sarsi, that they fly, and that they fly alone like eagles, and not like starlings."[450]

5.4.2 Towards Galileo's new sciences

The foregoing analysis teaches us something extremely important about the kind of sciences that Galileo is trying to develop. Instead of focussing on the empirical world in its full complexity, he directs his attention to isolated subsystems that allow invariancies to show themselves in the stable behaviour of these systems. But to achieve this, *one has to choose the right level of abstraction*. It is only under a very specific set of conditions that such stability is achieved, and demarcating these conditions takes a lot of hard work. (Consider e.g. the complete failure that Galileo met in trying to analyze magnetic phenomena.) It is only when one has found out the right way of describing things in the world, and at least as importantly, interacting with them, that it becomes possible to offer the kind of explanations that Galileo is searching for. This also automatically brings the problem of idealization to the fore, as any actually realizable physical system will at best be only approximately isolated from disturbing influences. The next chapter will be focussed on showing how Galileo was able to achieve some level of success in isolating some appropriately closed systems (i.e. closed off from disturbances in such a way that the system shows a relevant kind of stable behaviour – if analyzed at the right level of abstraction), which is a task preliminary to the development of full-fledged mathematical theories. The kind of stability that Galileo tries to uncover involves the fact that appropriately defined parameters are known to be always proportional to each other. The idea of functional dependence that is so crucial in modern physics is no part of Galileo's mathematical apparatus, but his geometrical

[447] *Opere* VII, p. 471. (Transl. from Galilei 2001, p. 517.)

[448] Cf. also Mertz 1980.

[449] *Opere* I, p. 300. (Transl. from Galilei 2000, p. 50.)

[450] *Opere* VI, pp. 236-237. (Transl. from Drake and O'Malley 1960, p. 189.)

framework does have place for a similar kind of predictive closure. The empirical stability should thus be reflected on the conceptual level.

This way of proceeding actually installs the specific interplay between universality and locality that has become so specific for modern physical sciences. If one has been able, in a very specific and local situation, to isolate a sufficiently closed system that shows some stable behaviour, one can transfer the lessons learned from this behaviour to all similar situations.[451] *And one can do this exactly because this stability expresses what lies outside our manipulative capabilities and hence must be ascribed to nature*. This answers the question posed at the end of the previous chapter, why the balance has acquired the possibility to act as a model of intelligibility. But we can now add the important caveat that it can only play this function for situations that are sufficiently similar in all relevant characteristics. Chapter 7 and 8 will recount how this caveat actually led to the demise of the centrality of the balance model within Galileo's science of motion, and simultaneously prepared the way for a new approach, as it turns out that the conditions under which a balance exemplifies relevant invariancies were not directly transferable to the case of free fall.

[451] Let me add the important observation that Galileo at times uses the term "proportio" where we would use something like "analogy" (cf. e.g. *Opere* I, p. 292: this is a passage that was quoted in chapter 4, section 4.2.2, when I was discussing how the "analogy" with the balance determines relevant similarities for Galileo.). *Similar situations are proportional situations, i.e. situations where the same kinds of ratio's can be observed.*

FIGURES TO CHAPTER 5

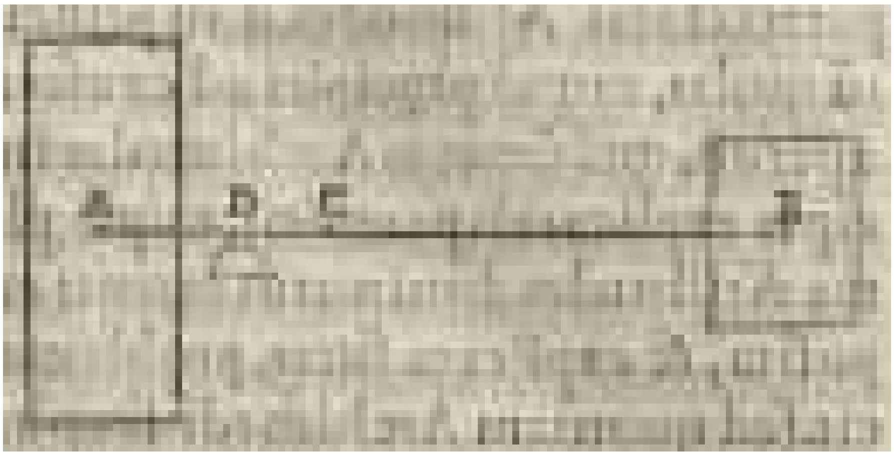

FIGURE 5.1

The lighter body *B* is able to lift the heavy body *A* because their common centre of gravity *C* lies to the right of the centre of the balance, the fulcrum *D*, and this centre of gravity has a tendency to move towards the centre of the world whereas the fulcrum must remain stationary. (del Monte 1588, p. 3.)

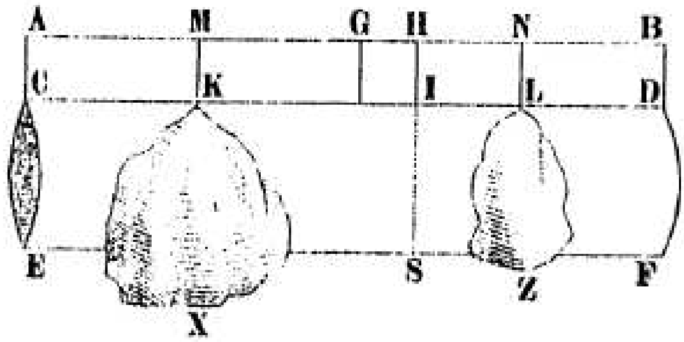

FIGURE 5.2

The uniform solid *CF* is suspended at its endpoints from a line *AB* which at its turn is suspended at the point *G* exactly in the middle. It will be in equilibrium. Now divide the solid in two unequal parts *CS* and *DS*, and add an extra string at the point *I*. It remains in equilibrium, as it also will if we now hang it from two other strings right above the parts' respective centres of gravity at *K* and *L* and cut the other strings. It can easily be geometrically proven that the ratio between the distances *MG* and *GN* equals the ratio of the weights of the respective unequal parts. (*Opere* IV, p. 161.)

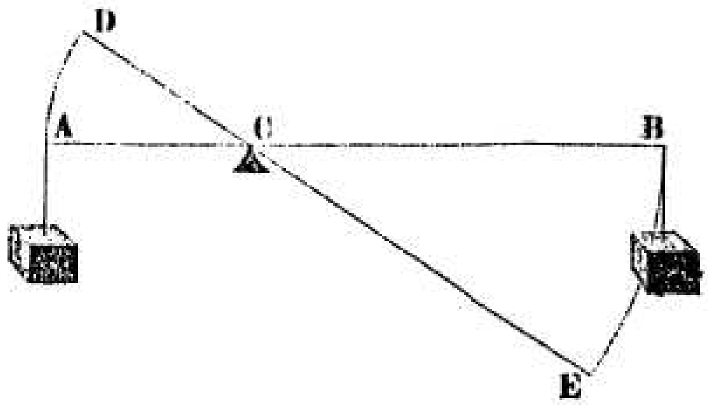

FIGURE 5.3

Since the two bodies *A* and *B*, situated at different distances on a balance, would move on circles with a different radius but a common centre, the speed of the body farthest from this centre would be proportionally faster. (*Opere* II, p. 163.)

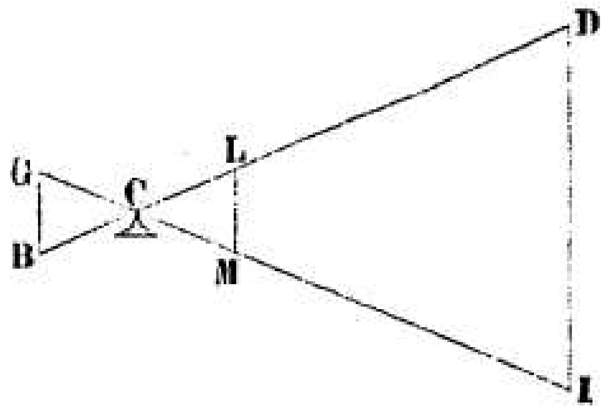

FIGURE 5.4

The distance *CD* is assumed five times the distance *CB* which equals *CL*. A body placed at *D* will have the same *momento* as a body five times as heavy that is placed at *B*. So the body at *B* can in principle be moved to *G* by such a body, if we assume that an infinitesimal weight added to this body is enough to set the lever into motion. Considered from the perspective of conservation of moment, this is exactly the same thing as saying that a body five times lighter than the body at *B* can also be moved by the same body if we place it at *L*, since the proportionality which is expressed through the equality of moment remains invariant. And if we repeat this action five times, we can move the complete body that was placed at *B* to *G*, by cutting it in five equal pieces. (*Opere* II, p. 166.)

6 Inclined planes and pendulums: Abstraction and idealization

In concluding chapter 4, I claimed that Galileo by 1591 had a language to speak (geometry), was forging himself a position to speak from (a geometrical philosopher), and had problems to address (the topical problems of motion), but that it is not clear whether he actually had objects to speak about. In the present chapter, I will analyze some crucial features of Galileo's search process aimed at remedying this situation. This led him to introduce novel empirical facts, which we still acknowledge as valid, as crucial elements into his mathematical theory of motion. In the first section I will analyze the grounds behind his proto-inertial principle, which led to a mathematical treatment of projectile motion and indirectly made possible the formulation of exact mathematical proportions characterizing the acceleration of free fall. In the second section I will focus on Galileo's insight in the independence of speeds of fall from specific gravity, at least for fall in a void.

In both cases it is clear that these novel facts could only be introduced as idealizations, not directly observable in empirical situations. The analyses of chapter 5 will be crucial to understand the status that such idealizations had within Galileo's thinking. The stress on nature as "that which lies beyond human will" allows us to see why he would understand these novel facts to be valid idealizations in building a science of *nature. As a result, we will also be in a position to understand the difference that separates Guidobaldo's and Galileo's mechanical investigations. It will become clear how Galileo's focus on appropriately closed systems lies behind both his interest in his proto-inertial principle, and the role that the pendulum could play in investigating the properties of free fall.*

In a third section, I will take a rather different perspective on this same search process, now focussing on the kind of bodily manipulations that are required in bringing it to a successful end. I will argue that we need to take into account what I call "performative reason." This disciplined way of engaging instruments such as a pendulum is an essential element in the possibility of building up abstract mathematical representations of concrete physical events. Highlighting this performative component will allow us to gain a more complete picture of the different levels at which a model of intelligibility functions.

6.1 Galileo's proto-inertial principle

6.1.1 Abstracting indifference from the balance

In *De motu* Galileo not only treats motion through media, in a separate chapter he also treats the problem of motion on an inclined plane. This problem can also be considered topical, but for a different tradition, i.e. the mixed science of mechanics. Galileo accordingly claims that the question "has been treated thoroughly by no philosopher, as far as I know", but that he nevertheless chose to include it because it "concerns motion."[452] (He was not claiming that it had not yet been treated by any mathematician.) Medieval authors working in the Jordanus tradition typically dealt with this problem, and had already found the correct solution.[453] Pappus also tackled the problem, and as we have seen, Guidobaldo included Pappus' erroneous solution in his *Mechanicorum liber*.[454] When repeating his own analysis in *Le mecaniche* Galileo will explicitly refer to Pappus' attempt to criticize it.[455]

The problem as Galileo presents it is to find out how much the speed of a body moving down on an inclined plane is diminished as the slope of the plane becomes more horizontal. As a starting point he repeats a claim that he had already made in making intelligible his dynamical demonstrations, i.e. "that it is manifest that what is heavy is carried downward with as much force, as would be necessary for pulling it upward by force; that is, it is carried downward with as much force as that with which it resists going up."[456] To solve the inclined plane problem Galileo will try to find out the magnitude of the weight which suffices to equilibrate a body of a given weight on an inclined plane of a given slope, and then set the speed proportional to this weight. The similarity with his treatment of motion through a medium is clear: finding out the effective weight of a body in a specific situation is again the clue to Galileo's method.[457]

To determine the effective weight of a body on an inclined plane Galileo cleverly exploits the properties of a bent lever (see figure 6.1). When an equal arm balance is in horizontal position, a body hanging from the end of its arm at point *d* will be equilibrated by a counterweight that is as heavy as the body itself. When this arm is pivoted around the fulcrum, while the other arm holding the counterweight remains in horizontal position, the counterweight will have to be less heavy due to the properties of a bent lever. The farther we turn the arm of the balance holding the body, the lighter the

[452] *Opere* I, p. 296. (Transl. from Galilei 2000, p. 46.)

[453] See Moody and Clagett 1960 for the main treatises that make up the medieval science of weights; Brown 1978 is a convenient introduction.

[454] Cf. chapter 3, section 3.3.2.

[455] See *infra* section 6.1.2.

[456] *Opere* I, p. 297. (Transl. from Galilei 2000, p. 47.) Cf. chapter 4, section 4.2.2.

[457] There is a fragment among Galileo's notes, in the hand of Mario Guiducci who was his assistant for a time after 1618, where the two phenomena are explicitly stated to be completely analogous with respect to each other (*Opere* VIII, p. 377).

counterweight has to be. But at any of the positions to which we can thus turn the body, it will have the same tendency towards motion as it would have if it were on the inclined plane that is tangent to that point on the circle traced by the bent arm. Hence, in figure 6.1, a body hanging at the point *s* would have the same tendency as if it were on the inclined plane *gh*. The geometry of the situation and the proportions characterizing the bent lever imply that the tendency towards motion on the inclined plane has the same ratio to the tendency to descend vertically as the vertical height of the inclined plane has to the path of oblique descent. (Or equivalently, that the force required to overcome a body's weight on an inclined plane has this same ratio to the force required to overcome its weight along the vertical).

Galileo, after having given this demonstration, immediately adds the warning that "it must be understood of this demonstration that there exists no accidental resistance (roughness either of the mobile or of the inclined plane; or because of the shape of the mobile)."[458] But it immediately follows that *if* this assumption were to be satisfied, "any mobile on a plane parallel to the horizon will be moved by a minimal force, indeed by a force smaller than any given force."[459] (The vertical height over which the body is to be moved is zero, so the force required to overcome its resistance against motion is also zero.) Galileo apparently understands this to be quite a momentous conclusion, as he continues: "And this, since it seems quite difficult to believe, will be demonstrated by the following demonstration."[460]

We are dealing with the first steps towards some kind of inertial principle, but it is telling that Galileo immediately returns to the balance in an attempt to clarify it further. He now presents figure 6.2 which abstracts the situation as presented in figure 6.1 one stage further, by stressing the essential *properties of the balance* as represented by a circle. The property that interests Galileo is the fact that any body hanging from point *d* can be moved by any force whatever at point *b*. But this smallest force would even suffice to *raise* the body, so "what wonder is it, that the same weight *d* should be moved, on a non ascending plane, by the same force or even a smaller one, than the force at *b*?"[461]

Seen from our own vantage point, Galileo is exploiting the fixed nature of the fulcrum that takes away force from the free weight of the body to model the constraining effect of the inclined plane; i.e. he is decomposing the force of weight in a component that is annulled by the constraining force and a resulting net force in the direction of motion. It is moreover immediately clear that the body hanging perpendicular under this fixed point has zero effective weight. We have seen that this

[458] *Opere* I, p. 298. (Transl. from Galilei 2000, p. 48.)

[459] *Opere* I, p. 299. (Transl. from Galilei 2000, p. 49.)

[460] *Ibid.*

[461] *Ibid.*

was already explicit in Guidobaldo's conceptualization of mechanical phenomena,[462] but Galileo remains almost completely silent on the physical role of the fulcrum.

Galileo adds some further considerations, this time completely detached from the balance:

> Furthermore: a mobile, having no extrinsic resistance, will go down naturally on a plane inclined no matter how little below the horizon, with no extrinsic force applied; as is evident in the case of water: and the same mobile does not go up on a plane erected no matter how little above the horizon except violently: it therefore remains that on the plane of the horizon itself it is moved neither naturally nor violently. Now if it is not moved violently, hence it will be able to be moved with the minimum of all possible forces.[463]

Galileo is here connecting the conclusion that he first drew from the geometrical proportions characterizing a balance with the natural constitution of the universe. This is a possibility that must have been immediately clear to him, as he had already commented on the non-exhaustiveness of the Aristotelian dichotomy between natural and forced motion in the dialogue version of the treatise. In this context he tried to assess how to understand the circular motion of a marble sphere situated at the centre of the universe.

> Thus if there were a marble sphere at the center of the world, so that the center of the world and the center of the sphere were the same, and then a beginning of motion of the sphere were given by an external motor, perhaps then the sphere would not be moved by a violent motion but by a natural one; since there would be no resistance of the axes, and the parts of the sphere would neither approach nor recede from the center of the world. Now I have said, perhaps: because if such a motion were not violent, it would endure forever; but that eternity of motion seems far removed from the nature of earth itself, to which rest seems to be more pleasant than motion.[464]

At this point Galileo is merely trying to explore the boundaries of the Aristotelian classificatory apparatus. His stance is uncommitted, and when he repeats this analysis in a separate chapter in the treatise version of *De motu* the uncertainty about the eternity of the motion remains. He presents the question whether the motion should endure or not, but nowhere gives an answer. For our purposes, the most important thing about these discussions is that Galileo is very clear about the fact that the indifference towards motion (an expression first used in *Le mecaniche* to characterize this situation) that was demonstrated geometrically in the discussion of inclined planes can also be understood as due to the fact that some bodies neither approach to, nor recede from the centre of the universe.

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[462] Cf. chapter 3, section 3.3.2. I already noted in a footnote in that section how this actually provided Guidobaldo with all the necessary tools to solve the inclined plane problem in a correct way.

[463] *Opere* I, p. 299. (Transl. from Galilei 2000, p. 49.)

[464] *Opere* I, p. 373. (Transl. from Galilei 2000, p. 120.)

6.1.2 A proto-inertial *principle*

In *Le mecaniche* Galileo returns to the problem of the inclined plane. Although his actual derivation of the proportions characterizing equilibrium on an inclined plane follows the lines of *De motu*, the overall presentation of the problem is significantly different. In the earlier presentation the indifference to motion of a body on a horizontal plane was presented as a *consequence* from the proportions that characterize equilibrium (and hence motion) on an inclined plane. In the recapitulation in *Le mecaniche* it is presented as "an indubitable axiom"[465] that *precedes* the geometrical demonstrations. Its proper grounds are now sought in the considerations that were adduced in *De motu* as further confirmation of this remarkable conclusion.

Galileo repeats the analysis of bodies neither approaching nor receding from "the common centre of heavy things."[466] He also provides some further semi-empirical examples, referring not only to the earlier mentioned motion of water (which is now specified to run through a river bed that is very little slanted) but also to a motion on a "surface of a frozen lake or pond". We shouldn't forget that water management was one of the prime occupations of sixteenth and seventeenth century Italian engineers: it was no doubt a very significant fact to them that even the slightest slant was enough to make the water flow.[467]

Galileo further adds that Pappus had already attempted to treat inclined planes, but that in his opinion "he missed the mark, being defeated by the assumption which he made when he supposed that the weight would have to be moved in the horizontal plane by a given force."[468] Pappus had indeed stipulated that a force would be needed to put a body on a horizontal plane in motion, and had tried to find out which extra force would be needed on an inclined plane. As already indicated in the notes to the translation of his treatment in Cohen and Drabkin (1958), this is actually not the source of his error – after all, there is nothing wrong in principle with assuming the presence of friction.[469] Whereas Pappus also had tried to find out the force needed to equilibrate a body on an inclined plane by exploiting a balance model, his model actually did not result in a correct decomposition of the force of the weight of the body. This becomes immediately clear if we take a look at the figure illustrating it (see figure 6.3 and accompanying explanation). His model implies that an infinite force would be needed to draw the body vertically upward, as in this case the force would be applied on a lever arm which has zero length. That Galileo had noticed this absurdity is revealed by his introduction of his

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[465] *Opere* II, p. 180. (Transl. from Galilei 1960, p. 171.)

[466] *Opere* II, p. 160. (Transl. from Galilei 1960, p. 152.) It is interesting to note how Galileo now avoids talking about the centre of the universe in this respect.

[467] Cf. Westfall 1989 for an occasion when Galileo acted as consultant for a large scale project that involved a plan to change the path of the river Bisenzio close to Firenze.

[468] *Opere* II, p. 181. (Transl. from Galilei 1960, p. 172.)

[469] Cohen and Drabkin 1958, p. 196, fn. 3.

own demonstration in the following words: “It will be better, *given* the force that would move the object perpendicularly upward (which would equal the weight of the object), to seek the force that will move it on the inclined plane.”[470] Pappus’ error is thus to be avoided by starting from this boundary condition which will allow one to obtain physically sensible solutions. (This is in all probably also the way in which Galileo first saw how to correctly model the forces in the problem through a balance: consider the balance from figure 6.3, but now start by hanging two equal weights from both its arms when the circle, which now represents the balance rather than the body, touches a vertical plane.)

The main innovation in the actual demonstration of the proportions characterizing equilibrium on an inclined plane is that Galileo now formulates it in terms of “moment of weight” rather than in terms of weight as he had done in *De motu*. This time he is also much more explicit about the fact that he is actually modelling the effects of constraint, as is testified by the following passage (cf. again figure 6.1):

> But to consider this heavy body as descending and sustained now less and now more by the radii *ar* and *as*, and as constrained to travel among the circumference *dsr*, is not different from imagining the same circumference *dsrb* to be a surface of the same curvature placed under the same movable body, so that this body, being supported upon it, would be constrained to descend along it. For in either case the movable body traces out the same path, and it does not matter whether it is suspended form the center *a* and sustained by the radius of the circle, or whether this support is removed and it is supported by and travels upon the circumference *dsrb*.[471]

It is interesting to notice already how suggestive this is of the pendulum as a further model for this kind of situation.[472]

After having derived the basic proportionality that characterizes equilibrium, Galileo goes on to illustrate how this explains the working of a screw. At the end of this analysis he introduces an extremely important discussion: “Finally one must not ignore the consideration which from the beginning has been said to hold for all mechanical instruments, that is, that whatever is gained in force by their means is lost in time and speed.” At first sight this might not be apparent in the present case. For if we consider a heavy body *E* being hauled up on an inclined plane by a lighter body *F* which can move down perpendicularly, and which is connected to *E* by a cord *EDF,* both bodies always move over the same distance in the same time (cf. figure 6.4). However, the important point to notice is that

> …heavy bodies do not have any resistance to transverse motions except in proportion to their removal from the center of the earth, then the movable body *E* not being raised more than the

[470] *Opere* II, p. 181. (Transl. from Galilei 1960, pp. 172-173; my emphasis.)

[471] *Opere* II, p. 181. (Transl. from Galilei 1960, pp. 173-174. I changed the lettering to make it consistent with figure 6.1.)

[472] Cf. *infra* section 6.2.

distance *CB* in the whole motion *AC*, while *F* has dropped perpendicularly as much as the whole length of *AC*...[473]

We have to compare the forces with respect to these unequal *vertical* distances. These are indeed in proportion to the weights, if we take account of the proportions characterizing equilibrium on inclined planes.

We can now understand why the indifference to motion of a body on a horizontal plane has changed status for Galileo and has become a general principle that is placed before introducing the actual derivations. It is this principle that allows him to decompose the motion of a body on the inclined plane in two components, and, as a result, to discern the general conservation of moment that should characterize any mechanical instrument. (The reformulation in terms of moment also immediately gains in significance.) The principle is still a mathematical consequence of the proportions that are derived for the inclined plane, but only because it now actually constrains which are the physically possible proportions. *Any measure which does no justice to this constraint would threaten to venture into the physically impossible*. It does indeed express something basic about the "constitution of nature with respect to the movements of heavy bodies."

When forty years later, Galileo returns to the problem of the inclined plane after having published his *Discorsi* in 1638, he takes the by now logical next step: he explicitly bases his proof of the characteristic proportions of equilibrium on the conservation of moment.[474] The balance has completely disappeared from the picture, and the inclined plane has become a closed system in its own right, thanks to the proto-inertial principle.

6.1.3 A fraction too much friction

It is important to note that Galileo's first steps towards the proto-inertial principle pivot around the balance's fulcrum. (The preceding sentence should be read in its literal sense.) We have seen that it was precisely what Guidobaldo conceived to be the essential role of the fulcrum that made it impossible for him to abstract from the friction that it necessarily introduces when one tries to put a body on a balance in motion.[475] That Galileo chooses to neglect this friction signals a significantly different way of conceptualizing physical problems. His next steps transferred the role of the fulcrum in annihilating a body's weight to the plane on which the body is moving. But again, he focuses on the relations that hold between the forces exerted by the body and the constraining instance and neglects frictional forces as accidental.

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[473] *Opere* II, p. 186. (Transl. from Galilei 1960, pp. 177.)

[474] Cf. chapter 8, section 8.3.2.

[475] Cf. chapter 3, section 3.6.2.

How should we understand the difference that separates Galileo from Guidobaldo? It was already concluded that Guidobaldo does acknowledge the fact that ideally true propositions can be violated through material hindrances. However, these can count as deviations from true principles only under precise circumstances; i.e. when these principles already have shown their empirical validity. This was the main reason why he refused to give exact proportions for machines in motion. Of course, he still could have made the thought experiment of mentally abstracting from all friction (as *we* understand it, all that is essential about the fulcrum is its fixed nature); but this would have made no sense, given the way in which he was consciously positioning himself as a practitioner of the mixed sciences. (I don't see any reason to doubt Guidobaldo's capacities as a theoretical mathematician, and hence his capacity of making such abstraction if he would have seen any sense in doing so.) This requires him to posit only basic principles that can be found exemplified in material instances. This is why Guidobaldo can see sense in idealizing physical situations to introduce exact measures (after all, even the most precise balance will never be mathematically exact), but still refuses to introduce an idealized balance which would have a frictionless fulcrum.

When we move to Galileo, the conditions under which something can count as a deviation from true principles apparently changed. I propose that we understand this as follows: *what for Guidobaldo was an invalid abstraction becomes an innocuous idealization for Galileo*. In making this distinction abstraction is tied to the *scope* of a model, whereas idealization has to do with its *precision*. Thinking away the physical nature of the fulcrum alters the scope of the theory for Guidobaldo, as it implies that we are no longer dealing with mechanics, but rather with pure mathematics; Galileo sees a continuum from a fulcrum with friction towards an ideal fulcrum which only alters the way in which the precise relations show up in empirical reality.[476]

But this leaves the important question open: what lies behind this change? A whole lot, as it is "nature" that has changed in the meantime – which of course immediately alters the valid scope of a theory. The importance of Galileo's conservation principle in his mechanical treatise was sufficiently stressed in our analyses in chapter 5. One fact about it remains to be noticed, however, and that is that Galileo neglects friction in all his treatments of the different machines. We have seen how he introduced the conservation of moment by both the Archimedean and the pseudo-Aristotelian proof of the law of the lever. In the latter proof he is setting the balance in motion, and this is again repeated in his example illustrating the transformation of moment.[477] However, if we take into account the status

[476] In discussing the rotation of the marble sphere, discussed at the end of section 6.1.1, Galileo is very explicit on this when he comments on the necessity of supporting it at its axis: "But the more the ends of the axis are polished and thin, the less they will suffer resistance: so that, if we imagine them to be indivisibles, then no resistance will develop from them." *Opere* I, p. 307. (Transl. from Galilei 2000, p. 59.) This extrapolation is still absent in the dialogue version of *De motu*, where it is simply asked whether there is "not always in such motion the resistance of the axes, which … resists the motion?" *Opere* I, p. 373. (Transl. from Galilei 2000, p. 120.)

[477] Cf. chapter 5, sections 5.2.1 and 5.2.2.

that Galileo accorded to the proportions thus determined, the complete disappearance of friction becomes perfectly well-suited. As these proportions now express the boundaries of what can be effected with machines, it is only sensible to think away all friction: the frictionless situation sets the upper limit to what kinds of motion can be actually realized. *Guidobaldo was interested in the possible, Galileo in the boundaries of the possible*. This is what has changed an invalid abstraction in an innocuous, nay necessary, idealization.

Stillman Drake analyzed the difference between Guidobaldo and Galileo in the following terms: Galileo had been able to derive mathematical laws for motion from mechanics because he had understood that the addition of an insensible weight to a balance in equilibrium would suffice to put it in motion, but Guidobaldo "for lack of [this] simple bridge between statics and dynamics, is unable to formulate quantitative laws for the latter." He goes on:

> Experience bore Guido out in a sense, as some power *is* lost in actual simple machines; … Yet Guido was in the habit of showing side by side material machines and schematic figures of them, and as a mathematician he should have been able to see the idealized truth. The fact that he did not is strong evidence that it is simpler for us to see this than it was for Galileo, who was the first to do so. Nor is this surprising; it was he who made it simpler for us.[478]

To repeat my argument: as a pure mathematician Guidobaldo was probably capable of seeing what Drake calls "the idealized truth", but as a mixed mathematician he refused to see the "truth" in it. The way in which truth functions as a normative instance for him is significantly different from how it works for Galileo.

Nature expresses what lies outside our manipulative capabilities. We can reduce friction more and more by fabricating ever more polished surfaces (and Galileo was certainly doing this, not only by referring to frozen lakes, but also by working on the material in his workshop): this lies within these capabilities. Not long after Galileo's death we will even find out how to produce artificial voids: idem. But what we cannot change is what *all* bodies will do when put in motion on a frictionless surface and in a void. (Of course we could manipulate this behaviour by interacting directly with any single body; but in this way we would merely be reintroducing "external" disturbances that could be eliminated *at will*.) And this is the behaviour that our basic principles must express. In the present case, this behaviour is seen to follow from the basic properties characterizing weight, which all bodies posses.

There is a famous letter from Galileo to Guidobaldo, written in 1602, in which we find the first written traces of Galileo's occupation with isochronous motion. In the same letter he also enters into the problem of idealization in the closing paragraph of the letter.

[478] Translator's footnote in Galilei 1960, pp. 166-167, fn. 24.

> Regarding your question, I consider that what your Most Illustrious Lordship said about it was very well put, and that when we begin to deal with matter, because of its contingency the propositions abstractly considered by the geometrician begin to change: since one cannot assign certain science to the [propositions] thus perturbed, the mathematician is hence freed from speculating about them.[479]

We don't have Guidobaldo's letter to which Galileo is answering, so we cannot be entirely sure about what both men were discussing. Guidobaldo must in all probability been complaining about Galileo's occupation with deriving geometrical proportions characterizing motion, which could never be borne out in experience because of phenomena of friction. This is confirmed by the fact that earlier in the letter Galileo comments on Guidobaldo's failed attempts to confirm isochronity for motion of balls in a hoop. In closing his letter, Galileo reassures his patron that he is well aware that these perturbed phenomena are beyond the scope of mathematical theories. What is left open, however, is what this tells about the status of the idealized proportions themselves. It is telling that Galileo remains silent on exactly this crucial point, which separates him from Guidobaldo's own endeavours.

Let me in closing come back to Pappus' "erroneous principle". We can now see that from Galileo's perspective there is nothing wrong in principle with assuming that we need an extra force to counter friction forces in treating problems of motion. However, it is important that this force is introduced at the right place. That means: not in setting up the terms of the problems. Friction merely disturbs the precise relations that are determinable in its absence. *It belongs to the outside of the closed physical system*: it can explain why moment is actually lost rather than conserved.

6.1.4 Kinematic interpretation of the principle

We have seen how Galileo already in the first version of *De motu* posed the question whether a homogeneous marble sphere placed in the centre of the universe would persevere in an imparted rotational motion. As this question arose within his attempts to ascertain the boundaries of the Aristotelian classification of types of motion, it seemed impossible for him to give an unambiguous answer – simply because the kind of motion that the sphere would have was ambiguous in itself (when considered from within an Aristotelian typology). When introducing his proto-inertial principle in *Le mecaniche,* Galileo still remains silent on the precise characteristics of the motion we are dealing with. It is enough for his purposes to see that a body would simply be non-resistant to motion along the horizontal.

Shortly after having completed the final version of *Le mecaniche,* Galileo started a research program that would further investigate motion on inclined planes. Most importantly, as he announced in the earlier quoted letter to Guidobaldo, he had found out that he could mechanically derive that

[479] *Opere* X, p. 100. (Transl. from Renn *et al.* 2000, p. 405. The translation in Drake 1978, p. 71 is rather inaccurate.)

motion on chords inscribed in a circle would always take the same time (see figure 6.5). This law of chords, which would occupy a central position in the 1638 *Discorsi* as theorem VI, seemed to carry the promise of opening up the possibility to demonstrate the isochronity of pendular motion, which he also announced in the same letter. He never succeeded, however. The closest he came was his scholium on brachistochrone motion to theorem XXII in the *Discorsi,* which he consciously did not present as a rigorous proof. It would be left to Christiaan Huygens to carry this research program to successful completion.

This stage in Galileo's thinking signals a much greater awareness of the role of time as a parameter in the phenomena he is studying. As a result, he also pays closer consideration to speed as an object of his study in its own right (remember that in *De motu* Galileo had merely claimed that "he who assumes motion, necessarily assumes swiftness" and "consequently, swiftness comes from the same thing as does motion").[480] I won't go into any of the conceptual problems that confronted Galileo in this respect, nor will I try to trace his attempts to integrate his newly acquired insights in a coherent deductive theory, which will finally result in days 3 and 4 of his *Discorsi.*[481] I only want to draw attention to the fact that precisely at this stage of Galileo's thinking does his proto-inertial principle start to function as a kinetic principle as well.

This period in Galileo's research is of course connected with his investigations in the precise proportions that hold for the acceleration of falling bodies. Jürgen Renn and collaborators have recently shown that we have every reason to accept that Galileo already was aware of the parabolic shape of projectile motion in 1592, following a set of experiments that he did together with Guidobaldo.[482] They in all probability threw inked balls along an inclined roof and recorded the trajectory followed by the balls (see figure 6.6). Guidobaldo wrote down in his notebook that the ball "will take the same path in falling as in rising, and the shape is … a line which in appearance is similar to a parabola and hyperbola"[483]. It appears that Galileo was at first primarily interested in the fact that the projectile clearly followed a symmetric path, which would certainly not be expected from an Aristotelian viewpoint. It also clearly belies the figures he had included in his *De motu* when discussing projectile motion (see figure 6.7), which followed Tartaglia in distinguishing a first part that was entirely straight, due to the impressed force, a middle part that is curved, which results from the mixture of the violent impressed force and the body's natural tendency, and a final part where the body falls down perpendicularly, due to its own weight (this last part is not discussed by Galileo). But,

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[480] *Opere* I, p. 261. (Transl. from Galilei 2000, p. 15.) Cf. chapter 4, section 4.1.4. On Galileo as inheritor of a scholastic conception of velocity, see Souffrin 1992; Damerow *et al.* 2004, chapter 3.

[481] On this topic, see especially Wisan 1974; Damerow *et al.* 2004, chapter 3.

[482] Renn *et al.* 2000. (See also Damerow *et al.* 2004, pp. 158-164.) Fredette 1969, pp. 154-159, seems to have the first to draw attention to Guidobaldo's discussion of these experiments in his notebook in connection with Galileo's obvious interest in the parabolic path of projectiles.

[483] Quoted in Renn *et al.* 2000, p. 314.

from a backward looking perspective, the most suggestive fact about this discovery of the parabolic path is that a trained mathematician such as Galileo would have had no problem recognizing that this trajectory implied a times squared law for free fall *if* he would assume that the motion could be decomposed in a vertical accelerated and a horizontal uniform motion.

Renn *et al.* ascribe the fact that Galileo did not immediately stress the latter conclusion to what they call his practical turn in between 1592 and 1602. In this period he devoted most attention to practical problems, and did not pay much attention to purely theoretical issues.[484] This is certainly part of the answer, but I think it is also important to take into account what was shown above: how the indifference to motion of a body on a horizontal plane only became something like a proto-inertial *principle* around 1600. This happened precisely in his writing on mechanics which Renn *et al.* would link with his practical interests, but the analysis in chapter 5 gives reason to prefer a more complex picture of Galileo's relation to practical traditions. This is directly reflected in the status that this principle could have within his thinking.

It is only in *Le mecaniche* that Galileo presents his proto-inertial principle as an "indubitable axiom" that follows from "the constitution of nature with respect to the movements of heavy bodies", whereas in *De motu* it had primarily served as a means to destabilize an Aristotelian framework. Even more importantly, it is only in the mechanical treatise that he explicitly treats motion on an inclined plane as composed of a horizontal component which requires no force and a vertical component that was forced. This is of course the kind of decomposition that could then lead to the law of fall – but Galileo only explicitly started considering it when thinking about how to understand the inclined plane as a closed mechanical system. As I already pointed out at the end of chapter 5, it is not evident to find out the right level of abstraction to describe systems in such a way that they enable one to observe interesting stable phenomena.[485] Seeing the precise proportions characterizing *natural* acceleration in the path of a projectile requires one to understand both the decomposition and the horizontal component as natural in their own right.

Applying this decomposition to the parabolic path of projectiles would have taught Galileo something else of prime importance: that it was fruitful to think of his proto-inertial motion as *uniform* motion. (It is only this assumption that he can give a proportion characterizing the relation between distance fallen and time passed.) Its implication in this new set of phenomena thus further sharpens its characteristic properties, which had to remain ambiguous when seen from an earlier perspective. Once this further step is taken, Galileo can exploit the principle to link accelerated motion on an inclined plane with uniform horizontal motion through his so-called double distance rule. This in turn will become an extremely important conceptual tool in mathematically handling accelerated motion.[486]

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[484] It is questionable, though, how far this can be ascribed to the influence of Guidobaldo's example, as Renn *et al.* suggest.

[485] Cf. chapter 5, section 5.4.2.

[486] Cf. Wisan 1974, pp. 205-206; Damerow *et al.* 2004, pp. 175-179.

It is well documented that Galileo kept on experimenting with accelerated motion on inclined planes throughout his Paduan period, but the remaining evidence is often too scarce to enable us to be really sure about the kind of experiments he was performing to what ends. Let me refer to a recent article by Alexander Hahn that studies many of the reconstructions that have been offered and comes to the conclusion that primarily the folio 116v experiments stand out as a successful test of Galileo's principles.[487] In these experiments, Galileo rolled bodies from different heights on an inclined plane and after a short horizontal run with the speed collected on the inclined plane they were projected from the table on which the plane was mounted (cf. figure 6.8 and accompanying explanation). The distances at which the bodies hit the ground are recorded and compared with the heights of the inclined planes on which they collected their speeds. Particularly interesting is the conclusion that Hahn draws:

> The discussion … shows that the experiment tests none of Galileo's insights independently, but that it in fact tests Galileo's account of motion as a whole. Therefore, it tests neither his law of fall nor his principle of inertia directly. Whereas the test of either the law of fall or the principle of inertia necessarily involves the measurement of time, the experiment of 116v bypasses any need to measure this elusive variable.[488]

This precisely mirrors the way he probably found out simultaneously both his principle of inertia as a kinetic principle and the law of fall.[489] *Time only enters essentially into the mathematical proportions that bind together the principle and the law*. But as we will see in section 6.3, this doesn't imply that for Galileo time had not also a more concrete reality as a physical presence and a mathematical object.

6.1.5 Circular inertia and all that

Let me briefly comment on some much discussed issues concerning Galileo's proto-inertial principle. These have to do with its relation to on the one hand Newtonian inertia, and on the other hand something like a cosmological principle of circular inertia.[490] A body lying on a perfectly horizontal plane is indifferent to motion according to Galileo, not because there are no forces, but precisely because there is equilibrium of forces. It is important, but in a sense only accidental, that these equilibrium situations primarily show in circular motion around the centre of the earth.[491] The

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[487] Hahn 2002.

[488] Hahn 2002, p. 358.

[489] Drake 1973 believed that the tests were aimed at testing the principle of horizontal inertia; Naylor 1974 suggested that it was the times-squared law. See also Hill 1988.

[490] Drake 1970, chapters 12 and 13, offers a good introduction to some of the problems that arise.

[491] Chapter 8, section 8.2.2 will show an example where Galileo extended the idea to non-circular, and even non-horizontal, "inertial motion."

most important thing is that strictly speaking *"inertial states" are only thinkable for Galileo in the presence of forces* – in complete opposition to the classical viewpoint. This is of course due to Galileo's conviction that gravity is something internal to matter, responsible for its essential tendency toward downward motion. If we follow the logic explained at the end of section 6.1.3, it is clear that we cannot directly intervene on the weight of a body: this is something that lies outside our manipulative capabilities. It is only when we start thinking of weight as due to an attractive force that it can properly be understood as an *external* property and that we can further abstract the description of inertial motion.

This situation poses serious problems for Galileo's attempts at treating projectile motion mathematically.[492] If he wants to prove that *all* projectile motion is parabolic, than he should assume that the motion in the direction in which the projectile is launched is inertial. But strictly speaking he can do this only for horizontal projection (as e.g. from a tabletop, as in most of his experiments). There is manuscript evidence of Galileo grappling with this problem, but in his final presentation in the *Discorsi* he passes over it in silence and merely presents the case of horizontal projection with no indication of its possibly limited nature. His disciple Torricelli first stated the case in its full generality and thus extended the inertial principle to arbitrary directions by adding the force of gravity as external to inertial motion.[493]

In the *Discorsi* there is nevertheless a passage where Galileo refers to motion upwards on an inclined plane as a "kind of mixture of *equable ascending* and accelerated descending motion."[494] Galileo at times seems to be wavering on how to proceed best.[495] But this does not so much betray a wavering between circular and rectilinear inertia, as it is often stated, as between gravity as internal and essential to a body, and gravity as somehow to be ascribed to an external force. As we know, this is a truly important problem that would exercise much of seventeenth century natural philosophy.

I will leave it at this. Let us just keep in mind how much Galileo's physical thinking remains in flux. He is groping towards a set of satisfactory principles and he achieves some partial successes. But we should do him injustice by imposing a coherence that would be too neat to allow us to see the complexities with which he was actually confronted.[496]

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[492] These problems are extensively discussed in Damerow *et al.* 2004, pp. 208-236. See also Koyré 1966, pp. 273-276.

[493] See Damerow *et al.*, pp. 284-286. This was also already discussed by Koyré 1966, pp. 298-299.

[494] *Opere* VIII, p. 244. (Transl. from Galilei 1974, p. 198; my emphases.)

[495] Another example is the treatment of extrusion in the 1633 *Dialogue*.

[496] The irrepressible Stillman Drake has the tendency to find this kind of coherence everywhere; hence also in the case of inertia, where he ends up with explaining away anything that might refer to genuine incertitude on Galileo's part under the denominator of something like the well-heeded caution of a modern physical scientist who "refused to generalize beyond the reach of our available experimental evidence" (Drake 1970, p. 255).

6.2 The pendulum as experimental system

6.2.1 The pendulum, ca. 1602

According to Viviani, Galileo's first biographer, Galileo had already empirically observed the isochrony of pendulums in the 1583 as a student in Pisa.[497] The fact would have first struck him while attending mass in the Duomo of Pisa and noticing that a swinging lamp kept pace with the music, even when the amplitude of its sings was noticeably diminishing. We shouldn't forget that Galileo was a schooled musician and son of a professional musician and musical theorist, which does lend quite some credibility to this mode of discovery. Whatever the historical truth behind this story, when Galileo wrote his *De motu,* he remained completely silent on any precise properties that would characterize pendulum motion, nor do we find any other references to it before his 1602 letter to Guidobaldo, referred to earlier.[498] There is only one explicit reference to pendulum motion in *De motu.* Galileo introduces the fact that a pendulum with a wooden bob comes quicker to rest to one made of lead to illustrate his views on the dissipation of impressed force.[499] This places him squarely within a scholastic tradition, as Oresme had already introduced the pendulum as one of the prime examples to make visible some of the properties of impetus theory.[500] It also makes clear that whatever the precise nature of his observations, Galileo was aware of the potential of the pendulum to illustrate physical phenomena.

Whether he had already made significant observation on pendular motion or not, the sudden appearance of its precise properties around 1602 can be traced to the hope of integrating them within his developing mechanical treatment of motion on inclined planes.[501] We have already seen how Galileo explicitly stated in *Le mecaniche* that it doesn't make a physical difference whether a body is

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[497] Settle 1995 reproduces the relevant parts of Viviani's story, and adds a charming reconstruction of his own.

[498] Cf. sections 6.1.3 and 6.1.4

[499] *Opere* I, p. 335. Cf. also p. 413.

[500] Cf. Hall 1978.

[501] Some authors, most notably Wisan 1984b and Naylor 2003, have argued that this awakened interest in the properties of pendular motion is intrinsically connected with Galileo's Copernicanism and the consequent significance of circular motion (an interpretation that has its pedigree in Koyré's work). I agree with them that Galileo's intriguing argument on "circular fall" in the 1633 *Dialogue* in all probability is an early treatment of fall that might well date back to the 1590's, especially given the similarity with the argument in the dialogue version of *De motu* on acceleration as an optical illusion. (It is moreover clearly incompatible with his more mature views on free fall, as Galileo was quickly to admit, calling it "a poetic fiction" in a letter written after the *Dialogue*; cf. *Opere* VII, p. 89; transl. in Drake 1978, p. 377.) But I don't believe that Galileo's interest in Copernicanism offers a better explanation for the attention that he starts devoting to pendular motion than does the direct link with his treatment of the inclined plane that I point out in the text; after all, Galileo is in the first instance interested in the natural motion *downwards* in the pendulum, which simply doesn't fit a Copernican natural circular motion.

constrained by an underlying plane or by a radius of a circle fixed in its centre.[502] The latter situation is of course very suggestive of pendulum motion. It is equally suggestive to have a look at folio 151r (see figure 6.9) where Galileo in all probability offers the first proof of his law of chords, announced in his letter to Guidobaldo.[503] This proof is directly obtained by transferring the tangent plane of his demonstration of the law of the inclined plane *inside* the circle where it becomes a chord. It is bound to remain a moot question whether Galileo had first observed the isochrony of circular motion and immediately hypothesized that his treatment of inclined planes might offer a physical proof for it; or whether he had first noticed the peculiar property of motion along the chords of a circle theoretically and then decided to check empirically whether it might be a clue to a more general property of motion along circles. The fact is that only at this point does he have a place for the pendulum to take on a specific evidentiary role within his mechanical theory.

It is no accident that Galileo became engrossed with the pendulum when finishing, or just having finished, the revised version of his *Mecaniche* which stressed the idea of mechanical machines as closed systems. One of the most striking characteristics of the pendulum is that the swinging ball always regains the height from which it started, thus immediately suggesting the idea of conservation of moment.[504] Another property that becomes especially meaningful from this perspective is the strict dependence of a pendulum's period on its length. As Galileo explains in his 1633 *Dialogue*:

> … the vibrations of … a pendulum are made so rigorously [*con tal necessità*] according to definite times, that it is quite impossible to make them adopt other periods except by lengthening or shortening the cord. Of this you may readily make sure by experiment [*esperienza*], tying a rock to a string and holding the end in your hand. *No matter how you try, you can never succeed* in making it go and back forth except in one definite time, unless you lengthen or shorten the string; you will see that it is absolutely impossible.[505]

In the *Discorsi* the same property is expressed as follows: "it is necessary to note that each pendulum has its own time of vibration, so limited and fixed in advance that it is impossible to move it in any other period than its own unique and *natural* one [*l'unico suo naturale*]."[506] It suffices to recall the conclusion of chapter 5, that "nature" in Galileo's thinking became identified with what is beyond human will, to see *how well-suited the pendulum was to be exploited as an investigative instrument*.

The most peculiar property of the balance is its already mentioned isochrony; i.e. whatever the amplitude given to a swing, the time it takes remains unchanged. As a consequence, the pendulum

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[502] Cf. section 6.1.2.

[503] Cf. Wisan 1974, pp. 163-164.

[504] As we will see in chapter 8, section 8.3.1, this property will occupy a central place in Galileo's presentation of his science of motion in the *Discorsi.*

[505] *Opere* VII, p. 475. (Transl. from Galilei 2001, p. 522; my emphases.)

[506] *Opere* VIII, p. 141. (Transl. from Galilei 1974, p. 99; my emphasis.)

seems to be a particularly interesting closed system, *comparable with*, but at the same time *interestingly different from* a balance. Most importantly, since any swing always starts from zero speed, isochrony is only intelligible if we take into account that any downward motion is accelerated, and that this acceleration moreover obeys precise proportions which make the times always come out equal. No matter what the precise historical chronology between his empirical discovery of isochrony and his mathematical derivation of the law of chords, it is clear that once he had realized this connection, he was determined to see what could be learned from it concerning the proportions characterizing all natural accelerations.[507]

We know, and seventeenth century natural philosophers were quick to find out, that the simple pendulum is not truly isochronous. The equality of times only holds true for relatively small arcs of swing. Galileo never explicitly mentions this limitation, and at different places he even stresses that it is also supposed to hold for large amplitudes. This is one of these facts that exercised historians of science eager to find out how much Galileo had been truly experimenting, and how much he recurred to fictitious experiments.[508] We can safely follow Naylor's recent assessment of this issue with respect to the pendulum. Galileo undoubtedly did numerous experiments that established that the properties claimed by him hold for small angles, and he certainly would have been aware that some discrepancies arise for large amplitudes. But he would have noticed the same when experimenting with inclined planes, where the isochrony of chords can only be experimentally established for planes that aren't too much inclined (for larger inclinations the motion is not smooth enough). In the latter case, Galileo had excellent theoretical reasons to believe that it nevertheless *should* have been true, and that the discrepancies thus had to be ascribed to accidental disturbances. The latter justification would then probably have been transferred to the case of pendulums as well, in the expectation that there was an essential correspondence between both cases.[509]

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[507] Machamer and Hepburn (2004) have recently provided a suggestive argument that the pendulum also provided the essential clue in Galileo's search for the proper definition of uniform acceleration: with respect to space or to time? Central in their reconstruction stands, again, a diagram relating an inclined plane and pendulum through a circle – the prevalence and importance of this kind of diagrams in the development of Galileo's science of motion is truly striking and merits a detailed study in its own right.

It has also been argued that Galileo only looked for his times-squared law because he needed the proportion of time fallen with distance on an inclined plane to complete his search for the brachistochrone (cf. Wisan 1974, p. 175). If we assume that his *Discorsi* postulate was one of his earliest findings, based on the inclined plane theorem of *Le mecaniche*, it could even have directly led to the times-squared law. The dynamical proof of the postulate, discussed in chapter 8, section 8.3.2, presupposes the law of fall, but this implies that by presupposing the postulate, the times-squared relation (in its mean proportional form of course) could have been derived.

[508] For the discussion concerning the pendulum experiments, see Ariotto 1968; Drake 1975; MacLachlan 1976; Naylor 1976, 1977, 2003; Erlichson 1994; Settle 1995.

[509] Naylor 2003, p. 180. This correspondence was so central to Galileo's science of motion that Descartes could state that the whole third day of the *Discorsi* seemed to have been written to prove isochronism. Cf. Drake 1978, p. 391.

6.2.2 Extrapolating fall through media, ca. 1615

One other property of pendulums that could have been easily noticed by Galileo is that the material of the bob does not make any difference on the period of a pendulum, although it is true that lighter materials will slow down much more quickly. But this poses important problems for Galileo's claim in *De motu* that speed of fall is determined by a body's specific weight. As a result, we would expect that experimenting with pendulums should have convinced him of the untenability of this earlier view. Surprisingly enough, we find him repeating it in his 1612 *Discourse* on floating bodies. This might be due to the observed difference in the rate of change of the amplitude which somehow could have suggested that the lighter bodies have an intrinsically slower motion (a suggestion that Galileo will dismantle in his *Discorsi,* as we will see below).[510] Or it might be that he simply chose to ignore this for tactical reasons in the 1612 controversy, because he was not yet entirely sure about how to square this fact with a hydrostatic framework that contained so many valuable insights.

The publication of Galileo's 1612 *Discourse* was followed by several published replies by Aristotelian philosophers. Together with Benedetto Castelli, a former pupil, Galileo prepared a set of answers to some of these, which were published in 1615. They contain the typical scathing remarks and repetitions of earlier arguments, but hidden in the train of one line of argument is presented a remarkable new argument.[511] Drop a ball of ebony and one of lead into water: one will observe that their speeds differ considerably. Now let the same balls fall through air: one will observe that their speeds differ only to a very small degree. As a result we can conclude that it is very likely that if we would further rarefy the medium until we would reach a void, the speeds would be equal. Galileo stresses that the conclusion is valid for bodies of different specific gravity.

In *De motu* Galileo already gave an extrapolation argument for the effective weights of different kinds of bodies: the rarer the medium the smaller will be the ratio of the effective weights of two bodies a and b of equal size (i.e., with c, d, representing the weight of successively rarer media, $(a - c)/(b - c) > (a - d)/(b - d) > \ldots > a/b$).[512] That is, the denser the media, the greater will be the accidental differences between the weights of the bodies. Following the dynamical theory of *De motu*, this implies that the differences between their speeds will show the same properties. All that Galileo now adds in 1615 is the empirical observation that the differences between the speeds in a medium like air are already very small. By looking at the speeds directly, instead of only considering their presumed causes, i.e. the weights, Galileo lets experience overrule his earlier theoretical model.

This argument would of course have momentous consequences for the understanding of free fall, but these are not stressed at all in 1615. Again, it looks as if Galileo was not yet sure about what to do with the new insight. By the time he repeats the argument in the first day of the *Discorsi* he has

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[510] Cf. *infra* section 6.2.3

[511] *Opere* IV, p. 659.

[512] *Opere* I, pp 294-295.

apparently gained confidence in this conclusion which signals the (partial) breakdown of his hydrostatic understanding of the phenomenon of fall. In chapters 7 and 8, I will recount part of the story behind Galileo's attempts to make sense of this fact. In the present chapter I will pursue how he deals with the specific problems of idealization and abstraction that are engendered by the conclusion of his extrapolation argument in the first day of the *Discorsi*.

The extrapolation argument leads to the same delineation of the proper domain for Galileo's science of motion as he had already introduced in *De motu,* where it was claimed that "the true and natural differences of speeds … occur in the void only."[513] An important question necessarily resurfaces at this point: is this an idealization that allows us to observe the phenomenon under study in its ideal circumstances where precise ratios can be discerned; or are we rather dealing with an illegitimate abstraction where we surreptitiously alter the scope of the theory (from natural to fictitious situations)? Why would it teach us something valuable about free fall of bodies to claim that in a void they would fall with equal speeds, when we see arising clear differences in their speeds in all actual instances – aren't we just dealing with a different kind of phenomenon? We have seen that Galileo's proto-inertial principle followed from the basic properties characterizing weight. In the present case, Galileo can no longer exploit any theoretical models to render the idealized behaviour fully intelligible and thus plausible. (Although we will see in chapters 7 and 8 that he came close to offering such a back-up.)

The extrapolation itself is of course supposed to offer an argument for the claim that we are dealing with a justified idealization. After all, it follows the logic of causal analysis that we saw to be guiding all Galileo's investigations into causal structures. He is ascertaining the effect of varying one variable (density of the medium) on another variable (speeds of fall) while holding fixed a well-circumscribed set of conditions (specific gravity of the falling bodies). But Galileo himself mentions a possible problem with the argument, which has to do with the question whether it couldn't be true that it is only valid under (too) limited conditions – which would indeed turn its conclusion into an unwarranted abstraction. The suspicion is that this kind of extrapolation only holds true when one observes fall over small distances, whereas there would remain an irreducible difference between heavy and light bodies when they fall over long distances. As we will now see, Galileo undercuts this possible objection through a clever exploitation of the properties of pendulum motion which allows him to show that the situation claimed by him to obtain in a void is indeed truly relevant for understanding fall in media.

[513] *Opere* I, p. 296. (Transl. from Galilei 2000, p. 93.)

6.2.3 The first day experiments, 1638

The first step taken by Galileo in closing the gap that separates the phenomenon of fall in a void from actually observable fall is to take account of a buoyancy effect. He had already done this in *De motu*, but the important difference is that he now starts from a situation in the void where all bodies fall with equal "absolute speed"[514]. If a body falls through a medium it inevitably happens that "the heaviness of the medium detracts from the heaviness of the moveable" which supposedly alters the speeds since this "heaviness is the instrument by which the moveable makes its way, driving aside the parts of the medium."[515] Galileo makes this effect clear by a number of examples. Suppose that lead is 10,000 times as heavy as air, while ebony is only 1000 times as heavy, and let water be 800 times as heavy as air. The effect on the alleviation of lead, in going to a denser medium such as water, will be negligible compared with the effect of the denser medium on the specific gravity of ebony. Although they have the same "absolute speed", the speeds of ebony and lead in dense media will differ considerably, due to the greater difficulty suffered by ebony in overcoming the obstacle posed by the medium. This buoyancy effect of the medium would be calculable in principle, provided all the absolute specific gravities were known, i.e. the specific gravity measured with respect to vacuum, and not with respect to air.[516]

Thus far, Galileo has merely followed the more than 40 years old lead of *De motu.* But he now adds an interesting complication, which gives his treatment of fall in media a much greater subtlety. As he notices, a medium not only alleviates, it also has a frictional effect, which is dependent on the speed of the falling body. "There is an increase of resistance in the medium, not because this changes its essence, but because of change in the speed with which the medium must be opened and move laterally to yield passage to the falling body that is successively accelerated."[517] (As a result any accelerating body will at a certain point reach a terminal velocity which will remain uniform.) At this stage Galileo no longer has a theoretical model which would allow him to calculate the difference a medium makes on the fall of different kinds of bodies. However, he will show how *to isolate experimentally what differentiates the behaviour of these bodies*.

Galileo is in particular interested in the differences that might arise between dense and light bodies when they fall over long distances, as he suggests himself that this might pose a problem for his

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[514] *Opere* VIII, p. 121. (Transl. from Galilei 1974, p. 80.) In the *postils to Rocco*, written shortly before Galileo started composing the *Discorsi*, Galileo speaks about the "natural velocity" of bodies in a void (*Opere* VII, p. 742).

[515] *Opere* VIII, p. 119. (Transl. from Galilei 1974, p. 78.)

[516] The formula with which Galileo is implicitly operating is of the form: $v = v_0\,[w(body) - w(medium)]/w(body)$ (with '$w()$' the specific gravities, 'v_0' the absolute speed, and 'v' the speed in a medium). In chapter 8, section 8.1.2, I will try to dispel the suspicion that the recurrence of this hydrostatic way reasoning would be inconsistent with the insight that weight does not cause the speed of fall in a void.)

[517] *Opere* VIII, p. 119. (Transl. from Galilei 1974, p. 78.)

hypothesis of equal absolute speeds. In these circumstances, and *even in a rare medium such as air*, dense bodies will outstrip the light ones with considerable distances. Since such an observation poses practical problems, Galileo suggests an ingenious experimental setup, mimicking this situation.

> So I fell to thinking how one might many times repeat descents from small heights, and accumulate many of those minimal differences of time that might intervene between the arrival of the heavy body at the terminus and that of the light one, so that added together in this way they would make up a time not only observable, but easily observable.[518]

The experimental device standing in for fall over great distances is a pendulum, and the assumed isochrony of the pendulum swings will be the clue to Galileo's analysis.

When two balls, one of lead and one of cork, are made to swing on identical pendulums, two facts may be observed, Galileo claims. The swings of the different balls remain isochronous with each other, while the amplitude of the cork ball will diminish much more swiftly. That the swings remain isochronous implies that *whenever* the two balls traverse equal arcs, they do so in equal times: the greater retardation of the lighter body cannot be due to an inferior natural speed. Hence, there can be no direct correlation between the different specific gravities of the bodies and the different speeds if they fall over long distances. All differences that do arise must be due to the effect of the medium on the bodies, and this effect can thus be shown present in the (differing rate of) diminution of the amplitudes. Since the buoyancy effect is only dependent on the ratio between the specific gravities of the falling body and the medium, which is constant and thus cannot be responsible for a diminution of speed (as witnessed by the shrinking amplitudes), the friction effect must be the cause of the change in speeds. The fact that the rate of diminution is different for bodies of different specific gravity can then be explained hydrostatically.

In 1634 Galileo wrote down a long reply to a book by the Aristotelian Antonio Rocco. The latter had offered numerous criticisms of Galileo's views as exposed in his 1633 *Dialogue*. Galileo's replies were never published during his lifetime, but they contain many discussions that will reappear in the first day of the *Discorsi*.[519] He also gives a long treatment of the effect of a medium in differentiating the speed of fall of different kinds of bodies. A the end of this discussion, he mentions that when these bodies fall over short distances their speeds will almost be completely equal, which implies that the differences that arise over longer distances cannot be due to their different specific gravity (which after all doesn't change with the distance over which they fall), and as a result must be ascribed to the impediments of the medium.[520]

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[518] *Opere* VIII, p. 128. (Transl. from Galilei 1974, p. 87.)

[519] A particularly striking case will be analyzed in chapter 7, section 7.5, and chapter 8, section 8.1.1.

[520] *Opere* VII, p. 744. He brings up the question of fall over long distances as this was one of the criticisms that Rocco had levelled against him.

In the period between writing this general rehearsal of his first day and its final composition, Galileo must have realized that this argument could be considerably strengthened by exploiting the properties of a pendulum. The interesting twist that Galileo can give to his argument by introducing the pendulum is that it *combines* the effects seen in fall over short and over long distances into one motion. Even when the pendulums have already been swinging for a long time, and the lead and the cork body are consequently moving at different speeds, the isochrony still remains. As a result, we are still assured that their natural speed of motion is the same (they have lost a lot of moment through the effects of friction, but when they fall through new swings from ever lower heights they show exactly the same acceleration). There are of course some physically significant differences between the cases of fall on a pendulum and free fall. Most importantly, in free fall the speeds keep on augmenting until a body reaches its terminal velocity due to friction effects, whereas on a pendulum a body will progressively slow down; a slowing down which moreover is the quickest at the beginning of the motion. But this in no way diminishes the pendulum's value as a model for the kind of effects that Galileo is interested in: it shows both the differentiation between different kinds of bodies (in the long run) and their equal unhindered speeds (in the short run).

6.2.4 The pendulum as a laboratory

Let us take stock of what Galileo has achieved with these discussions presented in the first day. He has shown that the proper domain to model free fall mathematically is fall in a void – since in this case *all* bodies will exhibit the same behaviour, independent of any other factors. Notice that he has not yet established the exact relations constituting such models: this will only be done in the third day where the times squared relation will find its place in an elaborated deductive structure built on the supposition of uniform acceleration. *That the models thus constructed will still be relevant for all actually occurring instances of free fall, is secured by his particular experimental procedure, guaranteeing that the case of fall in a void is not merely the simplest case, but the most general.* There can thus be no question of an invalid abstraction. By isolating all that actually differentiates different kind of bodies with respect to the phenomenon of free fall, it becomes possible for Galileo to attribute the presence of the "pure phenomenon" to actually occurring instances of free fall, even if these might show considerable deviations from the theoretical models.

In a sense Galileo is able to recover what could not be abandoned by Guidobaldo: the requirement that one needs a concrete exemplification of the basic principles of a mixed science. It has become considerably more complex to actually observe this exemplification, but the pendulum *shows* that the principle of equal acceleration really expresses a basic fact about all natural bodies. That Galileo considered this to be the function played by his experiments is demonstrated by the fact that he has Sagredo explicitly comment on their results, that it is "the most admirable and estimable condition of the demonstrative sciences that they arise and flow from well-known principles, understood and conceded by all."[521]

Later in the fourth day Galileo repeats his earlier claim that:

> …no firm science can be given of such events [*accidenti*] of heaviness, speed, and shape, which are variable in infinitely many ways. Hence to deal with such matters scientifically, it is necessary to abstract from them. We must find and demonstrate conclusions abstracted from the impediments, in order to make use of them in practice under those limitations that experience will teach us.[522]

To this end he then tries to estimate the effect of air friction on different kind of bodies and under different conditions (again using the pendulum as an investigative tool).

In the first day, Galileo is doing something strikingly new, however. He is learning something about the ideal case *from* the way in which it is disturbed by the presence of a medium. Although he is still not giving a scientific treatment of the disturbances themselves, *he shows how to exploit their presence to epistemic ends*. In a more contemporary language, Galileo shows how to retract a meaningful signal from the noisy actual behaviour by looking at how signal and disturbances interact with each other. In his fascinating book on how this task is achieved in twentieth century laboratory science, Peter Galison fittingly illustrates this process through a reference to that other Florentine giant:

> Michelangelo was once asked how he had carved his marble masterpiece. The sculptor apocryphally responded that nothing could be simpler; all one needed was to remove everything that was not *David*. In this respect the laboratory is not so different from the studio. As the artistic tale suggests, the task of removing the background is not ancillary to identifying the foreground – *the two tasks are one and the same*.[523]

It is interesting to note that Galileo's pendulum shares some further characteristics with a modern scientific laboratory. As stressed by Bruno Latour, two of its most defining features are the change in scale and the change in the variability of the systems studied.[524] Both are essential in its task to make significant patterns discernible. Both are of course crucial aspects of Galileo's recourse to the pendulum.

In their discussions of Galileo's pendulum experiments, Roland Naylor and David Hill present these as serving as a "didactic device" and as "a means of shoring up soft spots in his geometrical exposition".[525] It is clear that contrary to these authors' claims, these experiments play an epistemologically deep role. It is not accidental that the pendulum would continue to play an

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[521] *Opere* VIII, p. 131. (Transl. from Galilei 1974, p. 90.)

[522] *Opere* VIII, p. 276. (Transl. from Galilei 1974, p. 225.)

[523] Galison 1987, p. 256.

[524] Latour 1983.

[525] Naylor 1976, p. 399; Hill 1988, p. 666.

extremely important role in the further development of seventeenth century natural philosophy in the able hands of men as Huygens and Newton. I need not stress the important steps that both men will take beyond Galileo in their use of the pendulum. Not only do they offer a correct mathematical treatment, but, more importantly for the kind of issues I am discussing here, they use it to obtain accurate measures of the gravitational constant. This of course becomes an important element in the Newtonian style, as it allows the introduction of very severe constraints in their developing theory.[526] Yet it is useful to stress that notwithstanding the absence of this interest in ascertaining parameter values to further epistemic ends, Galileo is already exploiting the systematic nature of some deviations. He thus introduces the disturbances themselves into the picture as potential sources of knowledge about the pure phenomenon itself.

6.3 Performative reason and scientific representation

6.3.1 Performing an experiment

In the present section I want to bring an aspect to the fore that is inextricable linked with Galileo's experiments with inclined planes and pendulums, but that was passed over in silence in the two preceding sections. Let us try to imagine Galileo on the track of the peculiar properties of the pendulum. He is coming home after having attended mass in the Duomo in 1583 – he has just proven isochrony of inscribed chords in 1601 and is determined to see whether he is on to some really general and crucial property of phenomena of fall – he is working on his theory of the tides in 1595 and has become curious to learn about the behaviour of other oscillating systems – he is doing experiments on the properties of lute-strings together with his father in 1586 and starts playing around with one of these strings hanging down with a weight attached to it – whatever the precise occasion, we can easily guess how the process of investigation more or less must have taken place.[527] Galileo would have first tried to ascertain whether a single pendulum really has a constant period independent of its amplitude. But what kind of time-keeper would have been precise enough to that end? Certainly none that were available to Galileo. But of course, if the hypothesized property would really hold, then this could be checked by seeing whether other equal pendulums swinging with different amplitudes would remain synchronous with the first pendulum (i.e. establishing isochrony through a wide range of synchronous relations). Hitting on this idea would have really started off Galileo's investigations. Let me quote from Thomas Settle's neat reconstruction:

[526] Cf. chapter 1, section 1.1. Schliesser 2005 stresses this essential difference between on the one hand Galileo's and on the other hand Huygens' and Newton's use of the pendulum as an investigative tool. Kuhn 1961 already stressed this important role for measurement in physical science.

[527] Cf. Settle 1995, pp. 26-28.

> Start with two pendulums of equal length. First set them in motion in equal arcs; then on arcs with different excursions; then set one in motion and, a few seconds later, set the other in motion while the first is still swinging; as [*sic*] so on. In whatever sequence or configuration one can think of, the result that is most impressive is that in each case the pendulums keep pace with one another. With a little reflection there would be no other conclusion to draw: by their inherent nature pendulums of a given length beat equal intervals of time, no matter what the lengths of the excursions.
>
> Then having taken this first step, the rest is relatively easy. By substituting bobs of different weights and density one learns that the period is independent of those variables.
>
> Finally, by setting two pendulums in motion, one of which, say, is four times the length of the other and watching them swing in a sort of syncopated harmony, one discovers the proportionality between the length of a pendulum and its period.
>
> If we recall that in the 1580s there had been no previous discussion of these properties and no theoretical basis for even imagining their existence, the only way that Galileo could have discovered them was through some sort of empirical exploring, culminating, in effect, in performing the above steps.[528]

But let me also summarily draw your attention to all that necessarily falls outside this discursive description of an explorative performance.[529] Galileo would have needed to look for the right room to hang his pendulums (he often speaks about pendulums measuring over four braccia, i.e. longer than two meters). He would have needed the right kind of strings that wouldn't stretch too much (especially in trying to find out the relation between length and frequency). He has to learn to release the balls of two pendulums at exactly the same time, or in such a way that they will swing in counter-beat, etc. In short, he has to know to handle his pendulums in the "right" way, involving all the small situational adjustments this may require. Anyone who has gone through the disciplinary exercise of an undergraduate physics lab will know that it may take quite some time before one masters seemingly simple gestures sufficiently to obtain truly stable results with an instrument such as the pendulum.

It is important not to loose sight of the fact that even the synchrony of two equal pendulums is something that first must be *achieved*. The pendulum can only play its role as an interesting closed system if it is approached through a repertoire of disciplined bodily gestures. It is as much this approach that makes it into a proto-laboratory as the conceptual relations it can be taken to express. Any potential researcher must first learn how to interiorize the proper way of engaging with material things such as a pendulum before he can start exploiting it as an investigative instrument. But of course, what it means to be "the proper way" depends on what kind of thing we take it to be in our engagement towards it. This in its turn depends on what we hope to disclose through our manipulations of this investigative instrument. And this finally leads us back to nature's function as a

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[528] Settle 2001, p. 844. See also Settle 1995, pp. 26-27.

[529] See also Bjelic 2003, chapter 6.

regulative and normative instance. The proper way to interact with the pendulum is the way in which we can hope to discern the way in which it constrains exactly this interaction. But the dependence is mutual. Galileo's nature can only function as normative instance given the presence of this kind of what I would like to call *performative reason,* which is necessarily embedded in locally situated practices.[530]

It is precisely because of this co-dependence, that this performative reason can be left out of any explicit picture. It is a situatedness that makes possible its own effacement, exactly because it is aimed at establishing "natural" facts. Once this goal is reached, the particular local circumstances which led up to it automatically dissolve in an unarticulated background.[531] But, and this is of course an extremely important caveat, these natural facts couldn't have been present if it wasn't for this performative reason that allows them to show up. (To avoid misunderstanding: I wouldn't want to claim that natural facts are dependent on any *specific instantiations* of this performative reason for their factuality. What I do want to claim is that they are dependent on a particular regime of such reason.)[532]

Let us have another look at this earlier quoted passage:

> … the vibrations of … a pendulum are made so rigorously [*con tal necessità*] according to definite times, that it is quite impossible to make them adopt other periods except by lengthening or shortening the cord. Of this you may readily make sure by experiment [*esperienza*], tying a rock to a string and holding the end in your hand. *No matter how you try, you can never succeed* in making it go and back forth except in one definite time, unless you lengthen or shorten the string; you will see that it is absolutely impossible.[533]

It is obvious that anyone can make the rock go and back forth in many other ways than in one definite time – but none of these will be proper since they are disruptive with respect to the kind of behaviour that Galileo is interested in. The text presupposes that the reader knows this; that he is aware that not any way of engaging with the rock will do. (The hand in which the string is held should remain as quiet as possible; one should be standing still; one should see to it that the rock is swinging smoothly; …) This little "esperienza" of course only describes a first step, not yet aimed at

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[530] There has been a growing number of studies of these kinds of experimental practices since the 1980's, both from sociological and philosophical perspectives. Cf. e.g. Hacking 1983; Collins 1985; Shapin and Schaffer 1985; Rouse 1987; Radder 1988 for but a very small number of the many interesting monographs that have been devoted to this topic. Pickering 1992 is a collection of essays that provides a very nice sampling of many of the approaches that can be found.

[531] This is a topic that has received some attention in sociologically inspired analyses of science; cf. e.g. Shapin 1989, who speaks about the invisible technician; and Schaffer 1994, who analyzes some of the techniques by which the gestures of demonstrators operating physical demonstration devices were rendered tacit in eighteenth-century rational mechanics.

[532] Cf. already chapter 1, section 1.3.2.

[533] *Opere* VII, p. 475. (Transl. from Galilei 2001, p. 522; my emphases.)

establishing any precise ratios. Investigating further properties only adds further layers of performative complexity, demanding further skills that much be exercised in appropriate ways.

A nice example is Galileo's famous experiment with the inclined proof as described in the third day of the *Discorsi* (to test the law of fall by timing the motion of a body rolling down an inclined plane). This is the experiment that brought Koyré to the infamous conclusion: "It is obvious that the Galilean experiments are completely worthless: the very perfection of their results is a rigorous proof of their incorrection."[534] In 1961 Thomas Settle experimentally disproved Koyré by performing the experiment as described by Galileo and actually achieving reasonably accurate results.[535] In this experiment Galileo used a water clock to measure time (a pendulum would be ill-suited since it doesn't allow a continuous measure of time), which he could have calibrated against a pendulum. This calibration is already a complicated operation, involving the simultaneous operation of the water clock and the pendulum, coupled with an accurate observation which can only take place in the right kind of observational circumstances (it must be made sure that the operation of the water clock is synchronized with, as exactly as possible, the end of any swing). But let me also quote from Settle's narrative reconstruction of his own experience in performing the experiment, which nicely brings out what I have been discussing above:[536]

> There are two crucial aspects [to the measurement of time]: the flow from the [water]pipe has to be uniform for at least the period of our longest readings, and the operator has to be trained so that he can release the ball and the flow of water at the same time and then stop the flow of water at the strike of the ball without anticipation or delay. In fact this second requirement is a most interesting one. When I first ran the experiment … it took a little while to get the feel of the experiment. And I sensed at the time that part of what I was doing was training myself to be an integral piece of the apparatus. I very definitely had the impression that there was a rhythm to the experiment, that what I was doing was training a set of monitored reflex reactions analogous to what I imagine a musician must be training as he begins to practice a new piece of music. The basic problem is learning to be able to replace the finger on the pipe at the strike of the ball as it hits the block somewhere down the slope of the plane, and this in such a way that the action takes place without conscious decision. … I have found that it is difficult to have other people come in cold and start doing the work well immediately. In fact my own early work started poorly. The point is that poor early results should not be regarded as conclusive. One should emulate Galileo and repeat the experiment "many, many times."[537]

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[534] Koyré 1968, p. 94.

[535] Settle 1961.

[536] The parallel with some of Settle's phrasings with Polanyi's coeval analysis of tacit knowledge is striking (cf. Polanyi 1974 [1958]).

[537] Settle 1966, p. 85.

The reference to a musician's experience is of course not accidental: this was Galileo's own background, as he was the son of a professional musician and musical theorist, had a brother that also was a professional musician, and was himself an accomplished lute player. He was thus intimately familiar with precisely timed operations. He could have put this to good use in operating instruments aimed at timing other phenomena. Stillman Drake has even put forward the charming hypothesis that Galileo would have tested his law of fall on inclined planes by measuring time through singing to the motion.[538] Whatever the worth of this suggestion, uniform time was clearly not merely an abstract geometrical quantity for Galileo; it was inextricably bound up with the breathing (and, why not, singing) body of the skilled experimentalist who divides time through his trained gestures.

This disciplined way of engaging material objects is part of what I called the historical a priori. The pendulum can only function as an exemplar because it also embodies a specific performative reason. This reason is thus also an essential object for an archaeology of Galileo's science of motion. I characterized critical philosophy in the Kantian tradition by its stress on an anthropocentric model of knowledge, on which objects are *taken as given*.[539] (What is given to us is cognized only on taking it in.) As we now see, this should also be read quite literally. Since this taking takes place according to its own reason, it is no way capricious. It moreover depends on specific historical constellations in which it can find its place, as it needs the right kind of conditions of education and transmission.[540]

Again, this particular form of reason not only determines the kinds of possible objects for a particular knowledge, it simultaneously constitutes a particular kind of correlative subjectivity. The disciplined action always stands *in between* subject and scientific object.[541] The scientific subject will have its own desires and its own bodily policies, which always hang together with the kind of objects that are being studied. In the previous chapter, I already signalled the very different sensibility with regard to the marvellous that underlies Galileo's new sciences. A good way to characterize the disciplined gesturing that was analyzed in the present section, would be by stressing the necessary *patience* that goes with it (and that can be so nauseating annoying to the undergraduate student locked into an "impersonal" physics lab on a sunny afternoon in spring, trying to master the deceptively simple instrument in front of him and counting the swings on innumerable trials).

> Donner et surtout garder un intérêt vital à la recherche désintéressée, tel n'est-il pas le premier devoir de l'éducateur, à quelque stade de la formation que ce soit ? Mais cet intérêt a aussi son histoire et il nous faudra tenter, au risque d'être accusé de facile enthousiasme, d'en bien marquer

[538] Drake 1975.

[539] Cf. chapter 1, section 1.2.2,

[540] Because it succeeds in mobilizing interesting forms of power, however, it can start imposing these conditions on its historical context. This is a line of investigation that I will not further pursue in the present thesis.

[541] "Dans la pensée scientifique, la méditation de l'objet par le subjet prend toujours la forme du projet." Bachelard 2003 [1934]. "The art of knowing is seen to involve an intentional change of being." Polanyi 1974 [1958], p. 64.

la force tout au long de la *patience* scientifique. Sans cet intérêt, cette patience serait souffrance. Avec cet intérêt, cette patience est une vie spirituelle.[542]

6.3.2 Representing phenomena of motion

In section 6.2 we have already seen how the pendulum embodies some crucial theoretical principles of Galileo's science of motion. We can now add that this is only possibly because it simultaneously embodies some kind of implicit performative reason. Only the combination of both aspects allows it to function as an exemplar for further research. In part this is clearly an inheritance of the mixed science tradition (it was already noticed a few times that the balance must be manipulated in a highly disciplined way), but I think we should not loose sight of the much greater open-endedness that is introduced in Galileo's search for appropriately closed systems, as analyzed in sections 6.1 and 6.2.

The meaning of modern scientific concepts is neither fully determined by the conceptual structure of which they are a part, nor by the empirical objects/properties/… to which they are supposed to refer. It is only the way these aspects are put together by experimental means that gives these concepts their full meaning. At the same time, the character of the situations thus described takes on a new dimension. Similarly, as a result of Galileo's experimental analysis it becomes possible for him to attribute the presence of the pure phenomenon to actually occurring instances of free fall, transforming the character of the latter through this attribution.[543] From now on, it will thus become possible to speak meaningfully about the velocity and the acceleration of actually falling objects, and especially about the (mathematical) relations obtaining between them, as defined and analysed at the theoretical level of the new science. At the same time, the meaning of the abstract concepts of velocity and acceleration will be co-constituted through this attribution. The experiments with the pendulum and the inclined plane are essential to all this for Galileo, because they secure the reference of the pure phenomenon in non-pure situations. Without their intermediary his theory would remain a purely hypothetical mathematical scheme. In Bachelard's terminology, they signal the transition from a phenomenology to a "phenomenotechnique" as the essential basis of science.

> Dans l'expérience, [la conceptualisation scientifique] cherche des occasions pour *compliquer* le concept, pour *l'appliquer* en dépit de la résistance du concept, pour réaliser les conditions d'application que la réalité ne réunissait pas. C'est alors qu'on s'aperçoit que la science *réalise* ses objets, sans jamais les trouver tout faits. La phénoménotechnique *étend* la phénoménologie. Un

[542] Bachelard 2004 [1983], p. 12.

[543] In this section I will resolutely opt for an analytic perspective on Galileo's science that doesn't take into consideration what could have been (and could not have been) his own way of understanding his undertaking. In chapter 9 I will pay more attention to his own discursive positioning in this respect.

concept est devenu scientifique dans la proportion où il est accompagné d'une technique de réalisation.[544]

This phenomenotechnique is what makes possible a mathematical science of nature. It is only through a disciplined way of engaging with material objects that we can start to discern stable relationships (*if* these are to be found – this is of course never guaranteed) that can be modelled mathematically as constant ratios. The mathematical closure that we strive for must thus be reflected at the level of gestural and observational management.

In this way we can start to add a further element to the question concerning an instrument's mode of functioning as a model of intelligibility. We already introduced the idea that a balance has some kind of representative power, on account of which it can be taken to exemplify principles of *natural* philosophy and thus generate evidence for our physical theories. The same can be noticed about the way in which the pendulum functions in the experiments described in section 6.2.3 where the swinging bobs are taken to be representative for all falling bodies. As was argued, in both cases the grounds for this representative power must be sought in the discursive function of nature that we analyzed in chapter 5. But there is not only the question of their representativeness. There is also a further question why these concrete material objects can in turn be represented on an abstract level through mathematical structures exemplified in geometrical diagrams, which is equally crucial for their role within Galilean science.

Recently, philosophers of science in the analytical tradition have started thoroughly discussing the issue of scientific representation: what is it that enables one thing to represent another and as a consequence convey scientific knowledge about that other thing?[545] As quickly becomes clear from these discussions, we cannot simply see representation as a two-place relation between two structures. This is basically so for two reasons: the representational relation is unidirectional;[546] and the target system, supposedly a part of the natural world, will always be so rich in potential structures that we must first select one of these – but how does this *selection* finds place if it is not through *representing* the target system as having a particular structure, which of course seems to push the problem just one level back.[547] Both problems can apparently be solved by bringing particular contexts of inquiry in the picture as a third element that both can anchor the representational direction in a notion of intended use, and can bring about the necessary prior selection of structures in a non-representational way. The latter point, as argued by Bas van Fraassen, is rather subtle: it involves the insight that within a context of investigation the relevant structure of a phenomenon under study is fixed through an *indexical* statement that links the structured representation of the phenomenon (in something like a data model)

[544] Bachelard 2004 [1938], p. 75.

[545] See Frigg 2003 for an overview of the issues.

[546] Suarez 2003.

[547] van Fraassen (forthcoming).

to the phenomenon. For us (the investigators) it comes down to exactly the same to claim that (a) this is the phenomenon, and claiming that (b) this is the phenomenon as represented *by us* (in this data model); this is a pragmatic tautology, there is no room for denying one of both claims while holding on to the other, which actually means that the representational relation drops out of the picture – in the context of investigation.[548] *An abstract mathematical structure can thus represent a concrete physical phenomenon because in the context of any investigation the latter already presents itself in a structured way.* But this is exactly what we have seen to be dependent on the exercise of performative reason. The fact that we can describe a phenomenon and summarize some of its characteristics in something like a data model is only possible because *we* engage the material things around us in a structured way. This is why performative reason is an essential ground for the representational power of Galileo's geometrical diagrams. Remember our description of the investment of Galileo's breathing and singing body in making possible the representation of time through an abstract mathematical quantity.[549]

Let me in closing try to sketch the multilayered picture that we can now see emerging around the idea of a model of intelligibility. To begin with, we have a relation between a mathematical *representation* and concrete material things that is made possible because the relevant behaviour of the latter is selected and stabilized through a set of disciplined manipulations. As a result these material things can be understood to constitute something like an experimental system. But we can also take this experimental system as *representative* for natural behaviour because these disciplined manipulations are regulated by the goal of finding out what constrains all possible manipulations. An instrument such as the balance or the pendulum accordingly introduces intelligibility on two levels. On the one hand it provides the abstract mathematical structures with concrete instantiations. On the other hand it also simultaneously gives structure and intelligibility to nature itself. But we must now also stress that it is not merely the instrument that plays this function, but rather our ways of dealing with it.

[548] *Ibid.*

[549] A similar question: why can we represent the physical and lived space by a mathematical space? This is not to be sought in our cognitive architecture, as Kant thought, but in a disciplined way of engaging with our environment that we all learn to interiorize from a very early age on.

FIGURES TO CHAPTER 6

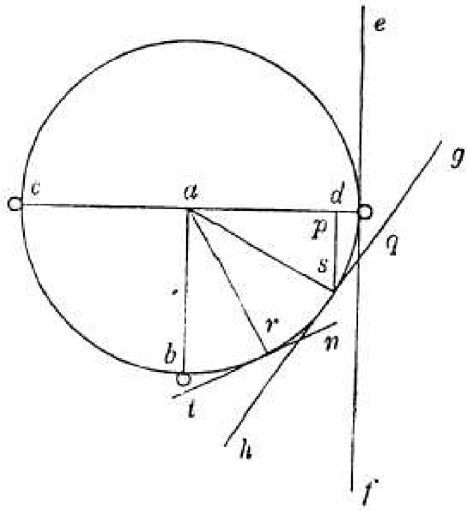

FIGURE 6.1

When an equal arm balance is in horizontal position, a body hanging from the end of its arm at point *d* will be equilibrated by a counterweight at *c* that is as heavy as the body itself. When this arm is pivoted around the fulcrum *a*, while the other arm holding the counterweight remains in horizontal position, the counterweight will have to be less heavy due to the properties of a bent lever (the body at *s* weighs as if it were at the position *p,* etc.); and the farther we turn the arm of the balance holding the body, the lighter the counterweight will have to be. But at any of the positions to which we can thus turn the body it will have the same tendency towards motion as it would have if it were on the inclined plane that is tangent to that point on the circle traced by the bent arm (a body hanging at the point *s* would have the same tendency as if it were on the inclined plane *gh,* etc.). It follows from the proportions characterizing the bent lever and the geometry of the situation that the tendency towards motion on the inclined plane is to the tendency to descend vertically as the vertical height of the inclined plane is to its path of oblique descent. (*Opere* I, p. 297.)

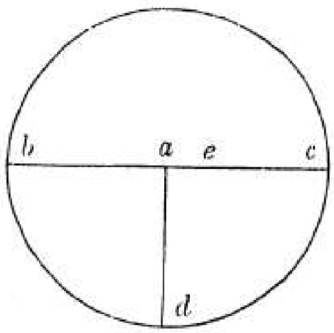

FIGURE 6.2

The balance from figure 6.1. Any small weight at *b* would suffice to equilibrate the body at *d* when it would be hanging at *e*, close enough to *a*. But this implies that this small weight would always be able to raise the body when it would hang from *d*.

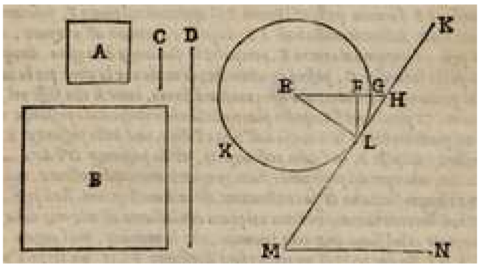

FIGURE 6.3

The inclined plane of Pappus' proof (taken from Pigafetta's translation of Guidobaldo's *Mecaniche* where it was included; del Monte 1581, p. 121r). The body on the inclined planes has weight *A*, the weight needed to equilibrate it on the inclined plane has weight *B*, which needs to be determined. Pappus proposes to consider the balance *EG* with fulcrum *L*. The weight *A* hangs from the point *E*, the weight *B* that must hang from the point *G* to equilibrate the body can now be found out by the law of the lever and the geometry of the situation. If we consider what happens if the body is to be equilibrated along a vertical plane, we immediately notice that *F* and *L* coincide and that the arm *HF* has zero length, which makes the weight necessary to equilibrate the body infinite according to Pappus' model.

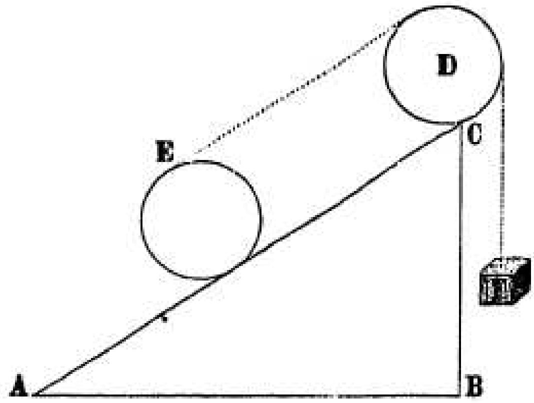

FIGURE 6.4

The heavy body *E* can be hauled up the inclined plane *AD* by the lighter body *F* falling perpendicularly, because the spaces traversed in vertical direction will be respectively *BC* and *AC*. (*Opere* II, p. 187.)

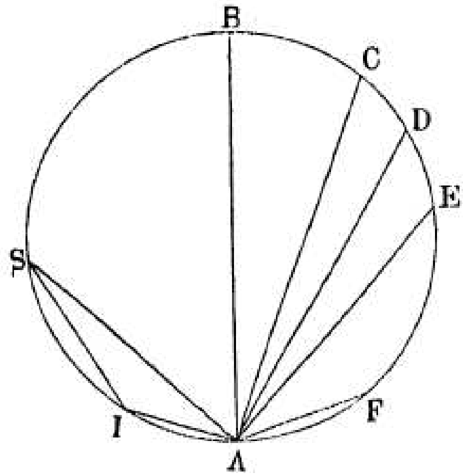

FIGURE 6.5

Galileo in 1602 announced his law of chords in a letter to Guidobaldo. A body descending on any of the chords *FA*, *EA*, *DA*, *CA* or even *BA* will reach the point *A* in the same time. It can also be demonstrated that the journey *SIA* will be completed faster than the journey *SA* (which is of course only intelligible given the accelerated character of the motion). The latter fact also opens up the search for the brachistochrone (the path of swiftest descent), which Galileo hypothesized to be circular. (*Opere* X, p. 99.)

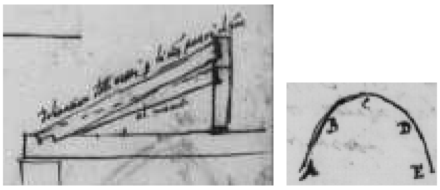

FIGURE 6.6

The roof along which Guidobaldo and Galileo threw inked balls, and Guidobaldo's sketch of the trajectory taken by these balls. (Renn *et al.* 2000, p. 313.)

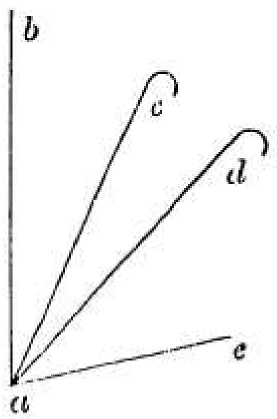

FIGURE 6.7

The figure accompanying Galileo's discussion of projectile motion in *De motu.* (*Opere* I, p. 340.)

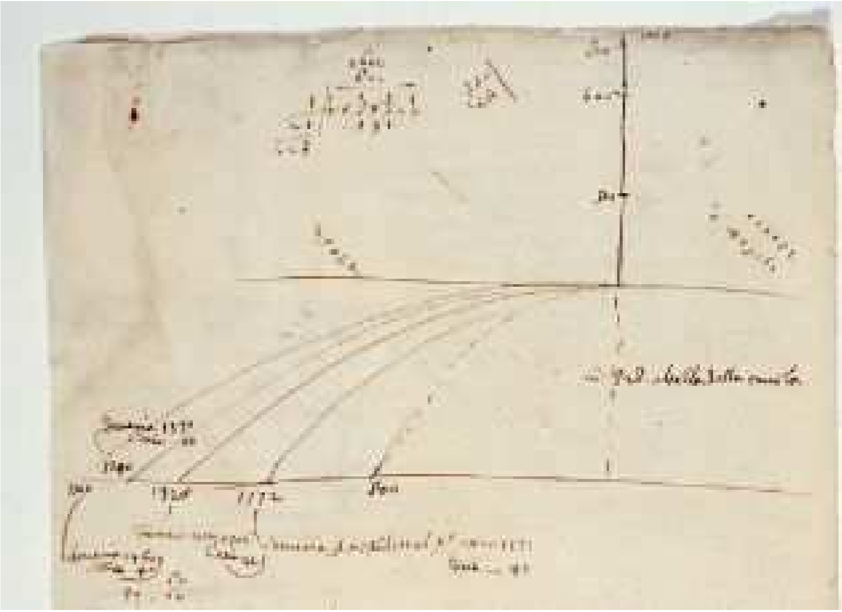

FIGURE 6.8

The experiment of folio 116v. Balls are released from different heights *h* on an inclined plane that is placed on a table (the middle horizontal line in the drawing). After a time *t* they will be deflected on the table, and after a short run on the table the balls are projected from the table with their speeds *v*. The balls hit the ground at a distance *R*. Since whatever the speed the ball had at the point it is projected from the table, it will always hit the ground after an equal time, *R* is proportional to *v* (principle of superposition and inertial horizontal motion). Because of the definition of uniform acceleration, *v* is moreover proportional to *t*. The law of fall gives t^2 proportional to *d* for the motions along the inclined plane. Finally *d* is always proportional to the vertical height above the table *h*. As a result Galileo can check whether *h* is proportional with R^2, as recorded by the measures on the folio. (Galileo's notes on motion can be consulted online at http://www.mpiwg-berlin.mpg.de/Galileo_Prototype/MAIN.HTM)

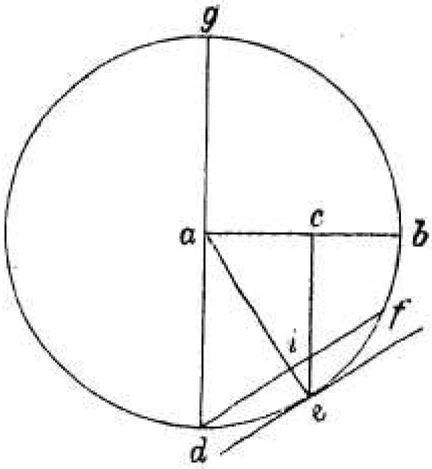

FIGURE 6.9

Folio 151r in all probability contains the first derivation of the law of chords (limited to the special case where motion along one chord is compared with fall along the perpendicular). The moment on *fd* is the same as the moment of the inclined plane tangent to the circle in *e*; but the latter is known to be to the body's "total" moment of fall along the perpendicular as *ca* is to *ab* (by the proportions characterizing the inclined plane). The geometry of the diagram then teaches that the moment of the body along *fd* is to this total moment along *gd* as the line *fd* is to the line *gd*. But this implies that motion along *gd* will take the same time as motion along *fd*. (*Opere* VIII, p. 378.)

7 Weighing falling bodies

"Surely I won't loose my head to such an extent that, while falling, I wouldn't study the laws of free fall."[550]

In this chapter I will take up an issue that was already mentioned in chapter 4: the ambiguous status of weight in Galileo's explanatory scheme in De motu. *I will try to uncover some aspects of the ways in which Galileo deals with what we would call absolute and specific weight. It will be seen that whereas the clear and evident principles which should ground his science of motion are based on experiences with the absolute weight of bodies, he nevertheless believed that something like specific weight provides a better measure for the speed of fall. These two facts sit uneasily together within* De motu. *To fully comprehend the background to this problem and Galileo's way of dealing with it, it will be necessary to start with a detailed analysis of his tract on the hydrostatic balance.*

In De motu *Galileo showed no signs of consideration with the problem that I sketch here. He believed that he could bridge the gap between the two concepts of weight through his famous thought experiment on the speed of falling bodies. We will see how it actually plays the role of a surrogate model of intelligibility. In this role it would continue to play an important role within Galileo's thinking. It is by rethinking his though experiment in 1634, that Galileo explicitly lays bare the gap that existed within his earlier theory. Consequently he is also able to see what was responsible for that gap, and how it could be avoided. In the next chapter we will see how he exploits this insight in some fragments that postdate the* Discorsi, *to come to a more satisfying understanding of the dynamics behind free fall.*

The development in Galileo's dynamical thinking that will be sketched here leads to the demise of the balance as the central model to understand phenomena of motion. Galileo comes to understand that the conditions under which the balance functions properly are not transferable to the situation of falling bodies. It accordingly looses its representative power. The closure that characterized the balance as a particularly interesting system turns out to be irrelevant for understanding the behaviour of falling bodies.

[550] The dadaist Hugo Ball, quoted in Safranski 1998, p. 115.

7.1 *La bilancetta*: Understanding mixtures and transforming gravities

7.1.1 The crown problem

Archimedes jumping out his bathtub is one of these images that have captured popular imagination. Historians of science are of course quick to point out how this is part of a romanticized image of science. It seems to have been no different at the end of the sixteenth century. The story was well known throughout the renaissance, through the numerous editions of Vitrivius' books on architecture. Vitrivius recounts how Archimedes exposed the deceit of a goldsmith who had stolen part of the gold that he had received to make a crown for king Hiero and had replaced it by silver.[551] It must have appealed enormously to mathematicians trying to secure their social position. After all, it was only Archimedes, through his knowledge of the principles of hydrostatics, who had been able to protect the highest authorities from being swindled by a mere artisan. However, the ones who were most self-conscious about their status as having a privileged understanding of mechanical principles were prone to be dismissive of Vitrivius' account. The method attributed by him to Archimedes falls short of the certainty and exactness of which they were capable, and which they had learned from Archimedes himself.

And so we find Galileo at age 22 tackling the problem of Hiero's crown in *La bilancetta*, a short tract devoted solely to this problem.[552] He prides himself on having reinvented the true method that must have been used by Archimedes, having all the exactness required by the true mathematician. His solution is based on a hydrostatic balance, a device that had been used earlier to tackle this problem.[553] It is often claimed that the main interest of Galileo's manuscript lies in the technical innovations proposed with respect to the balance used.[554] Nevertheless, the theoretical treatment offered of the balance provides us with an invaluable picture of the young man attempting to gain full mastery of Archimedean hydrostatics; a mastery that he soon will be trying to exploit in building a natural philosophical treatment of motion on its basis, as we already have already seen in chapter 4. Crucial in this respect is the behaviour of mixtures of pure metals that lies at the heart of the solution to the crown problem. Of particular interest are Galileo's peculiar handling of weight, and his analysis of the effect of a medium on a body's weight.[555]

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[551] Clagett 1978, pp. 1066-1068, n.2 sketches the diffusion of the work in the renaissance; *ibid.*, pp. 1066-1085 is a useful account of the occurrences of the crown problem during the renaissance.

[552] *Opere* I, pp. 210-220.

[553] Cf. Napolitani 1988, pp.163-164.

[554] Cf. e.g. Drake 1978, p. 6; Wallace 1984, p. 221.

[555] Most discussion's of Galileo's early work contain passing references to *La bilancetta*, but a detailed analysis of Galileo's actual proof procedure has not yet been provided. All more or less detailed expositions of Galileo's method that I know of translate it into modern terms and e.g. use algebraic methods.

7.1.2 Solving the crown problem

How can we detect whether a crown of a given weight is fully made up of gold or of a mixture of gold and silver; and if a mixture, in what ratio? If we sink a body in water, it will loose weight by an amount equal to the weight of an equal volume of water (by the 7th proposition of Archimedes' first book on floating bodies). Hence, the smaller the difference between the specific weight of a metal and that of water, the more the metal will suffer a loss of weight. It is this proportionally different behaviour that Galileo wishes to exploit in determining the proportion of two different metals in one mixture. Take a sample of gold and one of silver, weigh them both in air and subsequently in water. By recording the weight-loss, one can determine the respective proportions in which gold and silver are alleviated, and, as a result, their specific weights. Now weigh the crown in air and water, and determine the proportion in which it is alleviated. This last proportion can be related to the earlier determined proportions for the pure metals, fixing the proportion of gold and silver in the crown. Such is the broad outline of Galileo's method, in which he seems to follow the lines of earlier attempted solutions to the crown problem. Here is Galileo's own description:

> Let us suspend a [piece of] metal on [one arm of] a balance of great precision, and on the other arm a counterpoise weighing as much as the piece of metal in air. If we now immerse the metal in water and leave the counterpoise in air, we must bring the said counterpoise closer to the point of suspension [of the balance beam] in order to balance the metal. Let, for instance, *ab* be the balance [beam] and *c* its point of suspension; let a piece of some metal be suspended at *b* and counterbalanced by the weight *d*. If we immerse the weight *b* in water the weight *d* at *a* will weigh more, and to make it the same we should bring it closer to the point of suspension *c*, for instance to *e*. As many times as the distance *ac* will be greater than the distance *ae*, that many times will the metal weigh more than water. Let us then assume that weight *b* is gold and that when this is weighed in water, the counterpoise *d* goes back to *e*; then we do the same with very pure silver and when we weigh it in water its counterpoise goes in *f*. This point will be closer to *c* [than is *e*], as experience shows us, because silver is less heavy [*men grave*] than gold. The difference between the distance *af* and the distance *ae* will be the same as the difference between the gravity [*gravità*] of gold and that of silver. But if we shall have a mixture of gold and silver it is clear that because this mixture is in part silver it will weigh less than pure gold, and because it is in part gold it will weigh more than pure silver. If therefore we weigh it in air first, and if then we want the same counterpoise to balance it when immersed in water, we shall have to shift said counterpoise closer to the point of suspension *c* than the point *e*, which is the mark for gold, and farther than *f*, which is the mark for pure silver, and therefore it will fall between the marks *e* and *f*. From the proportion in which the distance *ef* will be divided we shall accurately obtain the proportion of the two metals composing the mixture. So, for instance, let us assume that the mixture of gold and silver is at *b*,

balanced in air by *d*, and that this counterweight goes to *g* when the mixture is immersed in water. I now say that the gold and silver that compose the mixture are in the same proportion as the distances *fg* and *ge*.[556]

To our modern eyes, the absence of any explicit reference to the concept of specific weight is conspicuous. At the same time, we easily interpret Galileo's reference to "gravità" as pertaining to it. After all, this is exactly what a hydrostatic balance does: it measures differences in specific weight. And if specific weights can be measured, Hiero's crown problem is solved. The absence of the concept might seem even stranger when we take into account that the term was used from the Middle Ages on. However, there are good reasons for this absence.[557] For one thing, Archimedes himself never uses the concept – so if Galileo really wanted to claim that he could provide the original method used by his paragon, he should be able to do without it. But more importantly, it is absent in Archimedes for good reasons. Within the confines of classical proportion theory, as expounded in book five of Euclid's *Elements*, it is impossible to define the concept as the ratio of weight to volume, since ratios are only defined between magnitudes of the same kind.[558] There is no doubt that Galileo always regarded the mathematical instrument of proportional theory as regulative for his theorizing. That he consciously tried to evade the concept of specific weight is further corroborated by the belated introduction of it in the 1612 controversy on floating bodies. By that time he has discovered a flaw in his earlier analysis of the relation between a body and the medium in which it is immersed. It is only at this point, when no other routes are open to him, that he explicitly defines *"gravità in ispecie"* (which immediately forces him to belabour an extension of Euclidean proportion theory, analogous with the way in which he defines uniform speed).[559] I will come back to this in section 7.4. Let us first see in more detail how he tries to analyze the hydrostatic balance within the framework set by classical proportion theory.

When hanging a sample of gold from the balance at point *b* (see fig. 7.1), and weighing it first in air by hanging a counterweight at point *a*, and then in water by readjusting the position of the counterweight until at point *e* it anew equilibrates the sample, the law of the balance gives us for the ratio of the weight of gold in air to its weight in water:[560]

(gold : gold in water) :: (*ac* : *ec*).

[556] *Opere,* I, pp. 217-218. (Transl. from Fermi and Bernardini 1961, pp. 115-116.)

[557] See Napolitani 1988 for much more on this issue, although he pays surprisingly little attention to Galileo's procedure in *La Bilancetta*.

[558] Grattan-Guinness 1996 offers a short and useful overview of the status of ratios and proportions within Euclid's *Elements*. There are some further potential problems with introducing the concept of specific weight as a quantity that can have a ratio, as explained in Napolitani 1988, pp. 190-196.

[559] This analogy is spelled out in detail in Napolitani 1988.

[560] I follow the common practice of representing the ratio of *a* to *b* as (*a* : *b*) and the proportionality of two ratios (*a* : *b*) and (*c* : *d*) as (*a* : *b*) :: (*c* : *d*)

Since we know that the weight of gold in water is equal to the difference of the weight of gold in air and the weight of an equal volume of water, we can transform this proportion in the following:[561]

$$(\text{gold} : \text{water}_{\text{gold}}) :: (ac : ae), \qquad (1)$$

where the subscript "gold" refers to the fact that we are dealing with the weight of a volume of water equal in volume to the sample of gold. Equivalently we have:

$$(\text{silver} : \text{water}_{\text{silver}}) :: (ac : af), \qquad (2)$$

with *f* the position of the counterweight when the sample of silver is immersed in water; and (again with *g* the second position of the counterweight):

$$(\text{mixture} : \text{water}_{\text{mixture}}) :: (ac : ag). \qquad (3)$$

Commenting on (1) and (2), Galileo claims that it follows that the difference between *af* and *ae* is the same as the difference between the "gravity" of gold and the one of silver. What can this mean, and how does it follow?

It is clear that by "gravity," Galileo can only be referring here to something like what we would call specific weight. Nevertheless, he did start by measuring absolute weights, and applying the law of the lever to these. The transformation from absolute to "specific" weight is made possible by the physics of the situation, which seems to demand that the volume of water is always equal to the volume of the metal. Notwithstanding the fact that we are dealing with absolute weights in the first ratios of proportions (1)-(3), these proportions are valid regardless of the volume of the weighed bodies. This implies that *physically speaking* Galileo can consider the volumes of water mentioned in proportions (1) and (2) to be equal to each other, and by then applying the rule *ex aequali*[562] derive that (gold : silver) :: (*af* : *ae*), or equivalently[563] that (gold - silver : silver) :: (*af* - *ae* : *ae*), where gravity now must be understood as the weight of an unit volume of the metal.

Physically speaking, but not mathematically! As Galileo does not see weight as the product of specific weight and volume, there are no volumes for him to cancel out in the mentioned proportions (which cancelling out, moreover, only makes sense from an algebraic point of view – and proportion theory is not algebra). And surely, the samples being weighed are not presumed to be equal in volume – as Galileo is attempting to reconstruct Archimedes' reasoning in solving the problem of Hiero's crown, this would not have made any sense: if the volume of the crown had been known, no hydrostatics would have been needed to expose the treacherous artisan.

We find Galileo reaching his result by equivocating: from the fact that the metal will always be opposed by an equal volume of water, he goes on to reason as if this equal volume was a unit volume, while his terminology proved flexible enough to cover up possible ambiguities. As mentioned, physically speaking he is justified in making these shifts from equal to unit volumes – and undoubtedly he realized this. However, only a few years later we will find him equivocating on

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561 By the rule *convertendo* which states that from (*a* : *b*) :: (*c* : *d*) one can derive (*a* : *a* - *b*) :: (*c* : *c* - *d*).

562 From (*a* : *b*) :: (*d* : *e*) and (*b* : *c*) :: (*e* : *f*) derive that (*a* : *c*) :: (*d* : *f*).

563 By the rule *dividendo,* which states that from (*a* : *b*) :: (*c* : *d*) one can derive (*a* - *b* : *b*) :: (*c* - *d* : *d*).

exactly the same point, yet this time without having the same means to justify it. But before we come to that episode, let us return to Galileo's understanding of mixtures. This will prove crucial in his attempt to cover up the problems caused by that equivocation, through the introduction of his thought experiment.

Starting from proportions (1)-(3) it is possible to derive the following two proportions:[564]

(gold : gold - mixture) :: (*ag* : *ag* - *ae*),

(mixture - silver : silver) :: (*af* - *ag* : *ag*),

which can be compounded:[565]

(gold : gold - mixture) • (mixture - silver : silver) :: (*af* - *ag* : *ag* - *ae*).

Since the gravity of the mixture "has part of the silver" and "part of the gold", the ratio (gold - mixture : gold) can be taken as a measure for the amount of silver contained in the mixture (assuming the mixture to be homogenous); and equivalently for the second ratio on the left. From which the desired conclusion follows. A mixture of two elements will always be "in between" these elements with respect to its "gravity."

7.1.3 Balancing mixtures and speeds

The hydrostatic balance and its schematic representation function as a powerful embodiment of Galileo's knowledge about the relation between the "gravities" of a mixture and its component elements. At the same time, the balance also embodied a rich tradition of thinking about the relation between weight, velocity, and mechanical effects. When these two aspects are put together a very suggestive picture emerges.

Let us first have another look at figure 7.1. It follows from Galileo's analysis that the lengths *ae, af,* and *ag*, stand for respectively the distances at which one counterweight must be hung to keep in equilibrium a body with more gravity, with less gravity, and a mixture of these (distances which can be related in exact proportion to the gravities, which are the same whatever the volume of the bodies). Let us now have a look at figure 7.2, which illustrates the main tenets of one influential way of understanding mechanical problems, which stretches back to the pseudo-Aristotelian *Mechanical problems* (written probably around 3thC BC) and which Galileo will incorporate in his *Mecaniche*.[566] As we saw, central to this view was an understanding of the law of the lever which crucially used the speeds of the bodies on a balance, and which was based on the geometrical properties of the circle. A

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564 By the rules *invertendo* which states that from (*a* : *b*) :: (*c* : *d*) one can derive (*b* : *a*) :: (*d* : *c*), together with *convertendo* and *dividendo*.

565 The symbol '•' is used to denote a ratio compounded of two ratios, which is not to be confounded with multiplication, although in the present case the results are the same.

566 We have already seen in chapter 3 how Guidobaldo's recuperated this kind of demonstration, and in chapter 5 how Galileo integrated it in his *Mecaniche.*

body hanging in *A* can be held in equilibrium by a lighter body hanging at the point *B*. For consider what would happen if the bodies would start to move: since they are constrained by the balance they will move at the circumference of a circle; now, since they will always have moved over distances *AD* and *BE* in an equal time, the lighter body, which will have moved a over a longer distance, will have travelled faster. We can understand that bodies of different weight can give rise to the same mechanical effect (i.e. equilibrium), by seeing that they also differ with respect to another crucial factor: speed, which can offset the differences in weight – associated with all points on the arm of a balance comes a different speed.

Both figures show *how multiple explanatory schemes are embodied in one instrument*: the balance. If we now mentally conceive the superposition of these pictures, since both refer to the same instrument, a suggestion emerges that maybe was too hard to resist: there is a different speed associated with every different ("specific") "gravity" – and this speed is independent of the volume of the bodies.

That the encounter with the hydrostatic balance indeed proved to be very enlightening for Galileo is testified by a fragment from the aborted dialogue version of *De motu*:

> I am, at last, unable to avoid demonstrating to you some theorems, from the comprehension of which you will understand most clearly not only what you are asking for, but also what ratio bodies have, both heavy ones and light ones, with regard to the swiftness or the slowness of their motion, as well as what the ratio is of the heavinesses and lightnesses of one and the same body, if we were to weigh it in different media: all these things had to be demonstrated when I tried to find the real reason by which we could, in a mixture of two metals, assign to each individual metal a very precise share.[567]

7.2 *De motu*: Understanding weight as a dynamic factor - ambiguities

7.2.1 The dynamics of *De motu*: effective or specific weight?

We have already encountered of the main features of Galileo's explanatory scheme in *De motu* in chapter 4. It was also pointed out that his measure for the speed of fall contained an ambiguity which has to do with the status of his concept of "weight."[568] I will now elaborate a bit on this problem, which has received surprisingly little attention. Let me begin with quoting a few passages in which Galileo illustrates his basic dynamical scheme.

[567] *Opere* I, p. 379. (Transl. from Galilei 2000, p. 125.)

[568] Cf. chapter 4, section 4.1.4.

> Let us show concerning upward motion, that solid magnitudes lighter than water, having been impelled into water, are carried upward with as much force, as that by which a quantity of water, whose size is equal to the size of the submerged magnitude, will be heavier than that magnitude [*tanto vi, quanto aqua, cuius moles nequetur moli demersae magnitudinis, ipsa magnitidine gravior erit*].[569]

The Archimedean inspiration is clear: the force upon a body is measured by the difference in weight between body and medium. But when we have a look at the way in which speeds are related to these forces, an Aristotelian aspect becomes obvious as well:

> If then this piece of wood, for example, whose heaviness [*gravitas*] is 4, is carried upward in water, and the heaviness [*gravitas*] of an amount of water as great in size as the size of the wood is 6, then the wood will be carried with a swiftness of 2 ...[570]

Galileo replaced the Aristotelian geometric ratio with an Archimedean arithmetic ratio as a measure for the force of motion, but he retains the basic Aristotelian idea that speeds and forces are proportional.

We have seen how Galileo made crucial use of the balance to justify his dynamical explanatory scheme.[571] Experiences with a balance provide the *basic physical facts,* commanding general assent, about natural bodies. This makes it possible for these bodies' motive force to become integrated into a mathematical explanatory scheme. Weight is thus not only a mathematical quantity that stands in all kind of relations to other quantities such as volume, but it is also a physical property shared by all bodies that constrains these mathematical relations in a physically meaningful way. This constraint is expressed in the general principle that Galileo borrowed from experiences with the balance, i.e. "that the heavier cannot be raised by the less heavy."[572]

Both Raymond Fredette and Paolo Galluzzi have stressed that Galileo, upon revising the first book of his treatise, discarded the chapter in which he introduced the balance analogy.[573] Fredette ascribes this primarily to the tensions arising because of the asymmetry between upward (forced) and downward (natural) motions. Galluzzi, however, sees another reason why Galileo might have judged the analogy to be improper. He claims that Galileo's Archimedean explanation of the causes for downward and upward motion is based upon the specific weights of the bodies and the media, whereas the balance only measures absolute weights. *We* can indeed easily see that these quantities are dimensionally incommensurable, but it should be clear from Galileo' treatment of the hydrostatic

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[569] *Opere* I, p. 269. (Transl. from Galilei 2000, p. 21.)

[570] *Opere* I, p. 270. (Transl. from Galilei 2000, p. 22.)

[571] See chapter 4, section 4.2.

[572] *Opere* I, p. 258. (Transl. from Galilei 2000, p. 11.)

[573] Fredette 1969, p. 272; Galluzzi 1979, p. 190.

balance in *La bilancetta* that for him this distinction was not at all clear-cut. Remember that his treatment of this balance also starts from absolute weights and then implicitly transforms these in what we would call specific weights. That something similar could be going on in *De motu* is clear when we have another look at the two last quoted passages. Both do reveal a crucial fact about Galileo's dynamical thinking in *De motu*. He is undeniably reasoning with the *actual volumes* of the moving bodies, and measuring the force by the difference in (absolute) weight for *these* volumes.[574] That is, in modern parlance, the commanding concept seems to be effective weight rather than specific weight. But this means that also within this hydrostatic context, a balance measures a body's tendency to downward motion. This direct identification is clearly illustrated by yet another quotation:

> We are said to be weighed down [*gravari*], when a certain weight [*pondus*] which tends downward by its heaviness [*pondus*] rests on us, and we need to resist by our force [*vi*] in order that it does not go down any further; now this resisting is what we call being weighed down [*gravari*].[575]

A body's tendency to motion is directly responsible for its experienced weight, which is measured by the force that is necessary to resist that motion. We can see how fundamental this kind of force-resistance pair is in Galileo's thinking by remembering the central role it played in his examples that were supposed "to lay clearly in the open" the nature of the phenomena of free fall.[576] (In these examples, he asked the reader to imagine drawing up a body through a medium, or breaking the equilibrium of a balance by adding a small weight.) Both this quotation and the two earlier ones come from chapters which are completely retained in the revised version of the first book.[577] Galileo's experience with balances is still implicitly structuring his thinking, even after he has discarded the explicit analogy with a balance.[578] He has no other way to introduce the motive power of a physical body into his mathematical framework.

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[574] Further confirmation for this identification can be found in the second book of *De motu.* When discussing the possible cause of acceleration, Galileo first claims that "we know with certainty, from the things demonstrated in the first book, that speed and slowness follow heaviness and lightness." (*Opere* I, p. 318. Transl. from Galilei 2000, p. 69.) Since Galileo uses *"gravitatem"*, this might still be taken as ambiguous between absolute and "specific" weight (lightness – *"levitatem"* – must obviously be read as relative, as taught by the first chapter of the first book). Galileo however continues by asking what could cause the change in weight that is responsible for the acceleration, and he adds that "the natural and intrinsic heaviness of the mobile is certainly not diminished, since neither its size nor its density [*nec … moles nec densitas*] is diminished: it remains, therefore, that that diminution of heaviness is against nature and accidental." (*Ibid.*) It is clear that here the "naturalis et intrinseca mobilis gravitas" refers to an absolute weight, as it could also be changed by a diminution of volume.

[575] *Opere* I, p. 288. (Transl. from Galilei 2000, p. 39.)

[576] Cf. chapter 4, section 4.2.2.

[577] See Fredette 1969, chapter 4.

[578] That he kept equating this principle with the balance model is clear from the following passage in the 1633 *Dialogue*: "SALV. Do you not believe that the tendency of heavy bodies to move downward, for example, is equal to their resistance to being driven upward? SAGR. I believe to be exactly so, and it is for this reason that two equal weights in a balance are seen to remain steady and in equilibrium." *Opere* VII, 240. (Transl. from Galilei 2001, p. 248.)

That Galileo is undeniably reasoning on actual volumes, and that, as a result, he sets the force equal to a difference in absolute weights might come as a surprise to many, given that it is always stated in the secondary literature that in *De motu* Galileo sets the speed of a falling body proportional to the difference of the specific weights of body and medium.[579] It is undeniable that this is indeed how we would interpret the actual proportions that he at different places assigns to the speeds of different bodies. The central question to a satisfactory understanding of Galileo's *De motu* becomes: how and why does he make this transition?

7.2.2 From equal volumes to unit volumes

Let me first quote a crucial passage in which Galileo makes exactly this transition:

> If … mobiles differ in size [*mole*] and in heaviness [*gravitate*], having taken hold, from the larger, of a part that is equal to the smaller mobile, we will again have two mobiles which differ in heaviness and not in size; and this part will observe the same ratio with the other mobile in its motion, as the whole of the other intact mobile (for … *it is with the same swiftness that the part and the whole of mobiles of the same species are moved*). Thus is it evident how, if the ratio of the motions of those mobiles that differ only in heaviness and not in size is given, the ratios of those that differ in any other way are also given.[580]

Notwithstanding the fact that Galileo is reasoning on weights of equal volumes, *he claims that he can always generalize his results by pretending that these equal volumes were unit volumes*. The clue to the transition from absolute to "specific" weights thus lies in the equality of the speeds of bodies of the same material. As we will now see, he tries to justify this equality of speeds precisely on the basis of an argument starting from the absolute weights of the bodies. This further testifies to the fact that it is

[579] Wisan 1978, p. 7, e.g. states that it follows from Galileo's natural philosophy that "'natural' motion is caused by relative heaviness and lightness" and immediately adds between parentheses: "Galileo intends relative density". *I propose that we be more careful with ascribing intentions to Galileo and pay attention to the actual ambiguities with which his texts present us.* Even Westfall, who is unusually careful in stating that Galileo claims that the force of a body in a medium equals the "amount by which its weight exceeds that of an *equal* volume of water", also states on the same page that "when Galileo said that speed in a void depends on the total weight of a body, *he meant* its specific weight", without explaining how such a transition would be effected. (Westfall 1971, p. 15; my emphases.) (Clagett 1978, p. 577, also shows a similar and revealing shift in discussing Benedetti's similar theory, when he first states: "Therefore, the greater the excess of the specific weight of the body over that of the medium [the greater the effective weight of the body over the medium *and thus*] the greater the speed of fall" (my emphasis), but in the rest of his presentation consequently talks about the "proportionality statement connecting speeds with the excesses of specific weight" without commenting on how to make this transition – which would be necessary given his own stress on the causal role of effective weight rather than specific weight.)

[580] *Opere* I, p. 267. (Transl. from Galilei 2000, p. 19. My emphasis.)

the latter property that is really basic in Galileo's thinking. The transition to specific weight is then supposed to follow from purely mathematical considerations.

It is at the beginning of the 8th chapter, "In which it is demonstrated that different mobiles moving in the same medium observe another ratio than the one attributed to them by Aristotle," that Galileo tries to establish the equal speeds for all bodies of the same material. He begins by asking whether it wouldn't be ridiculous to imagine a direct proportionality between volume and speed for bodies of the same kind, but immediately goes on "make more use of reasons than of examples (for we are seeking the causes of effects, which are not reported by experience)."[581] And this reasoning goes as follows:

> Thus, if we conceive in our mind that the water, on which a beam and a small piece of the same beam float, becomes imperceptibly and progressively lighter, in such a way that in the end the water gets to be lighter than the wood and the pieces of wood start slowly to go down, who would ever say that the beam would go down first or more swiftly than the small piece of wood? For although a large beam may be heavier than a small piece of wood, the beam must be put into relation with the great quantity of water that must be raised by it, and the small piece of wood with the small quantity of water [that must be raised by it]: and since an amount of water as great in size as the beam itself must be raised by the beam, and similarly for the small piece of wood, these two amounts of water, namely those that are raised by the pieces of wood, will have the same ratio in heaviness to one another as their sizes have [*eandem inter se in gravitate proportionem habebunt quam suae moles habent*] (for the parts of homogeneous things are to one another in heaviness as they are in size, something which should be demonstrated), that is, the ratio that the sizes of the beam and the small piece of wood have to one another: hence the heaviness of the beam will have the same ratio to the heaviness of the water that must be raised by it as the heaviness of the small piece has to the heaviness of the water that must be raised by it: and the reluctance of the large quantity of water [to be raised] will be surpassed by the large beam with the same facility as the resistance of a little water will be overcome by the small piece of wood.[582]

It seems that Galileo is claiming that the equal speeds follow from Archimedean considerations. But this does not really make sense. What he actually proves is that W_{body}/W_{medium} is invariant for bodies of the same material, but to conclude from this that the speeds are equal implies that he would be employing an Aristotelian geometric ratio (with the resistance of the medium measured by its weight) instead of the Archimedean arithmetic ratio which he explicitly favours as the central dynamical formula. Given Galileo's dynamical scheme and the fact that $W_{body}/(W_{body} - W_{medium})$ also is invariant, all that we can conclude is that for any two bodies of the same material, there is a constant ratio between the speeds of these bodies in void and in a medium. This only implies that the speeds of these

[581] *Opere,* I p. 263. (Transl. from Galilei 2000, p. 16.)

[582] *Opere* I, p. 264. (Transl. from Galilei 2000, p. 17.)

bodies are diminished in the same proportion by a medium, not that they are the same.[583] Only upon the supposition that the speeds of all bodies of the same material are the same in the void would the equality of their speeds in a medium follow. But why would these speeds in the void be the same? This in no way follows from Galileo's Archimedean-Aristotelian dynamical scheme – it is even in explicit opposition to it. It seems that he is left without a way of rendering this fact intelligible.

It is clear that the proportional alleviation effect of a medium cannot account for the equal speeds of bodies of the same material – unless one is willing to reverse to an Aristotelian reading of the "facilitate" with which a body can overcome a medium's "repugnantia", a view against which Galileo vehemently argues at other places in the same treatise. Strictly speaking, Galileo cannot make the transition from absolute to specific weights. This raises the further question: why does he nevertheless want to make it? After all, he could as well have developed a theory which is directly based on his Archimedean-Aristotelian scheme, and set $v \sim W_{body} - W_{medium}$.

A first clue to a possible answer is given by Galileo himself, when he raises empirical objections against a direct proportionality between speed and weight which he dubs "ridiculous." Moreover, when he will recount his own development in the 1630's, he again stresses these considerations as the first to have raised his suspicion against Aristotle's explanations.[584] In doing so, he (implicitly) also dismisses the proportionality with an alleviated weight, which however would be less ridiculous (since the differences would be smaller). It is nevertheless quite possible that he was convinced that also these differences in speed would be too large to be empirically credible. But, as we have seen, it is also true that in the same *De motu*, he is quite willing to invoke seemingly *ad hoc* explanations to account for the striking differences between the accelerated character of the motion of all actually falling bodies and the uniform character of the motion of his theoretical models. In this case he did let his theoretical model overrule the empirical observations. It seems that there must be a hidden motivation behind his choice which cannot be traced back solely to its empirical plausibility.

I submit that Galileo's experience with the hydrostatic balance provides the most important clue for understanding this tension in his dynamical thinking in *De motu.* It was crucial to the strategy used to solve the crown problem in *La bilancetta* that the behaviour of a sample in a medium was independent of its volume. It is the hydrostatic balance which had shown him that all bodies of the same material are equally affected by a medium. Moreover, it was already pointed out that the properties of bodies on a balance were closely linked with their "speeds" on the balance.[585] Galileo's argument in *De motu* should be seen as a failed attempt to mimic the cogent reasoning behind the irrelevance of volume for a hydrostatic balance, with the results now translated to speeds.

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[583] This is easily seen when we translate the situation in modern terms: that speed v is proportional with $W_{body} - W_{medium}$, implies that $v \sim (density_{body} - density_{medium}) \times volume$; this implies that for bodies of the same material but of different absolute weight, their speeds in the same medium will be proportional with their respective volumes.

[584] Cf. *infra* section 7.5.1.

[585] Cf. *supra* section 7.1.3.

7.3 *De motu*: Introducing the thought experiment

7.3.1 A hidden assumption

"But it is pleasing [*sed libet*] to confirm this by another argument."[586] Such is Galileo's own introduction to his famous thought experiment in *De motu*. This other argument for the equality of the natural speeds of bodies of the same material has received much more attention than the confused attempt based on the proportional alleviation effects of the medium. This is undoubtedly due to a fascination for the cleverness of the argument, but it may also result from the simple fact that this argument does seem to reach its goal cogently. I agree that the argument is indeed unassailable, but it remains to be pointed out that the premises are not as innocent as they might look. We will see how Galileo's presentation of the thought experiment provides further indications of the far-reaching repercussions of his earlier encounter with the hydrostatic balance.

Let us first consider Galileo's own presentation of his thought experiment.

> And first, let the following be presupposed: namely, if there are two mobiles, one of which is moved faster than the other, the combination of the two is moved more slowly than that part which was moved faster than the other, but more swiftly than the remaining part, which, alone, was carried more slowly than the other…
>
> This having been presupposed, I argue as follows: by proving that mobiles of the same species, of unequal sizes, are carried with the same swiftness.
>
> Let there be two mobiles of the same species, the larger *a*, and the smaller *b*; and, if it can be done, as our adversaries hold, let *a* be moved more swiftly than *b*. There are then two mobiles one of which is moved more swiftly than the other; hence, according to what has been presupposed, the combination of the two will be moved more slowly than the part, which alone, was moved more swiftly than the other. If then *a* and *b* are combined, the combination will be moved more slowly than *a* alone: but the combination of *a* and *b* is larger than *a* alone: hence, contrary to our adversaries' view, the larger mobile will be moved more slowly than the smaller; which would certainly be unsuitable [*inconveniens*]. What clearer indication do we require of the falsehood of Aristotle's opinion?[587]

The argument inevitably leads to its conclusion: bodies of the same material have the same speeds in free fall. Following Gendler's neat reconstruction we can summarize the argumentative structure as follows:[588] (1) natural speed is mediative (the natural speed of a combined body will fall between the

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[586] *Opere* I, p. 264. (Transl. from Galilei 2000, p. 17.)

[587] *Opere* I, pp. 264-265. (Transl. from Galilei 2000, pp. 17-18.)

[588] Gendler 1998, p. 404.

natural speeds of the component bodies); (2) weight is additive (the weight of a combined body will be the sum of the weights of the component bodies); hence (3) natural speed is not directly proportional to weight; and, moreover the only way to hold on to (1) – (3) simultaneously is by asserting that (4) natural speed is independent of weight.

The crux of the argument seems to lie in premise (1). One could wonder how Galileo can claim to know that this is a valid assumption. A first possible answer is provided by the following note which he wrote in a margin in the original manuscript: "Aristotle makes this same assumption in the solution of the 24th Mechanical Problem." Now, this is a little bit of a stretch on Galileo's part. The 24th Mechanical Problem deals with the famous paradox of Aristotle's wheel, not at all with the natural speeds of falling bodies. The importation of that assumption, in the context of the thought experiment would require a much more substantial argument. It is not at all obvious that rolling wheels and falling bodies partake in the same principles. Moreover, if this assumption were accepted only on Aristotle's authority, then it might well function in a reduction of the Aristotelian theory, but not in an argument which seeks to establish an alternative theory. For the conclusion (4) to hold generally, independent grounds for accepting premise (1) must be present. However, such grounds are provided by Galileo:

> As, for example, if we understand two mobiles, such as a piece of wax and an inflated bladder, both of which are carried upward from deep water, but the wax more slowly than the bladder, we ask that it be conceded, that if they are combined, the combination will go up more slowly than the bladder alone, but more swiftly than the wax alone. Indeed this is very clear: for who doubts that the slowness of the wax will be diminished by the speed of the bladder, and, on the other hand, that the speed of the bladder will be retarded by the slowness of the wax, and that a certain motion intermediate between the slowness of the wax and the speed of the bladder will result?[589]

The same argument is then repeated for a piece of wood and an inflated bladder falling downward in air. These are of course very revealing examples. The first thing to notice is that they involve bodies of *different material*. Now, since Galileo wants to conclude that for bodies of the same material the speed of fall is equal, it would have been clearly self-defeating if he could have adduced empirical examples of this kind to illustrate his assumption. But this also points toward the fact that *Galileo considered his assumption to be an empirical fact of the matter*, possibly following a theoretical principle, but surely recognizable without such a principle at hand. Secondly, the provenance of this empirical fact of the matter is easily recognizable. Take two bodies of different material and compare their behaviour with the behaviour of a mixture of these materials…

Once again Galileo translates the situation of *La bilancetta* by having natural speeds mirror the positions of the counterweight on the hydrostatic balance. These positions on the balance arm had

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[589] *Opere* I, p. 265. (Transl. from Galilei 2000, p. 18.)

indeed undeniably shown that "specific weight" is mediative. But this implies that the proportionality of speed with "specific weight" is a hidden assumption of his thought experiment. The thought experiment thus accomplishes the transformation from absolute to "specific" weights by presupposing the latter.

7.3.2 The dynamical conundrum

Once that the conclusion of the thought experiment is reached, it becomes impossible to hold on to a proportionality between speed and absolute (effective) weight. However, this leaves Galileo without any intelligible dynamics, as the balance is his paradigm case of a situation in which the motive force of a body can be noticed. In *La bilancetta,* he had been able to take these motive forces, as measured by absolute weights, as the starting point for analyzing specific weights, by exploiting the fact that any body is always opposed by an equal volume of water in a hydrostatic balance. At this point he thus did also not consider specific weights as giving rise to forces directly. That he still holds on to this indirect relation in *De motu* is clear if we remember that at several places (after already having presented the thought experiment), Galileo does set speeds proportional to forces which are measured by differences in absolute weights – differences which then can be transformed into differences of "specific" weights by pretending (on the basis of the thought experiment) that the results hold independently of the volumes. But if we are not mistaken in imputing to Galileo a dynamics which still refers back to experiences with absolute weights, then the conclusion of the thought experiment must have presented a potential conundrum for him.

The absence of an explicit concept of specific weight undoubtedly helped to mask the dynamical problem. By not explicitly thematizing the dimensional differences within the undifferentiated concept of "grave", the conundrum might have seemed less pressing (and indeed seems to have been largely ignored by most Galileo scholars). There was of course also the attempt at explaining the equality of speeds by considering the alleviation effect of a medium, which might have eased Galileo's mind at this point – provided he did not realize himself that the argument was incoherent with what he claimed at other places. But it must anyway have been clear to him that this was insufficient. This can be seen from the fact that after that he has established the possibility of motion in a void, he proclaims that the thought experiment must also be valid in this situation.[590] Given that the argument is supposed to remain precisely the same, it is clear that the effect of the medium can not be operative in reaching the desired conclusion.

This helps us to pinpoint the gap that remains in Galileo's dynamical conceptualization of motion more precisely. As the transformation procedure which he used to such great effect in *La bilancetta* completely breaks down in the void, he is left without any way to connect his mathematical

[590] *Opere* I, pp. 283-4.

scheme with the shared experiences that had to secure its applicability to the motion of physical bodies. What he offers instead is his thought experiment, *which supposedly can provide for an equally incontestable experience that could possibly anchor his explanatory scheme* – albeit it does this, as we saw, by actually presupposing further experiences which go back to phenomena involving dense media. That it is indeed supposed to render the dynamics of free fall immediately intelligible is further proved by the following passage, which follows almost directly after the presentation of the thought experiment:

> But, I ask, who will not recognize the truth of this on the spot [*veritas non statim cognoscitur*], when he examines it in a pure and simple and natural way? For if we presuppose that the mobiles *a* and *b* are equal and that they are very near each other, then, by the consensus of all, they will be moved with equal swiftness: and if we understand that while they are being moved, they are joined, why, I ask, will they double the swiftness of their motion, as Aristotle held, or increase it?[591]

The question is to the point, and it will be the starting point for a successful solution of the conundrum in the *postils to Rocco*, but at this point it must remain a rhetorical question. If a balance does indeed measure a body's tendency for downward motion, as repeatedly implied by Galileo in *De motu*, then the only natural response to the question would be: why not? This is not to deny that Galileo was convinced that they do not: he clearly believed that specific gravity provided a much better measure for the speed of fall. But it is the argumentative structure of *De motu* itself that leaves a gap at exactly this point: the central empirical principle that should ground his mixed science derives its evident character from experiences involving a body's absolute weight.

One might wonder whether it is really justified to call this gap a "conundrum", as there is no sign that Galileo was puzzled by it in any significant respect.[592] As far as *De motu* goes, this might be true, but as will become clear in section 7.5, at a later time Galileo indeed began to wonder about how to connect the behaviour of the bodies in his thought experiment with their behaviour on a balance. At this point he has clearly become aware of the gap that exists between his full explanatory scheme and the basic experiences that were first thought to ground its applicability. If we would not be allowed to think of this gap as a conundrum, we might as a result loose the means to understand the dynamics behind Galileo's thinking, as it seems that it really did trigger Galileo's rethinking of the thought experiment in a fundamental new way. As was already noticed, once the gap is perceived as a conundrum, the crucial question becomes why bodies of the same material would have to move with the same speed *in the void*. In this situation the empirical examples which were adduced by Galileo to justify the first premise of his thought experiment loose their intuitive plausibility, which was based on the experience with the behaviour of mixtures in dense media. This shows that, although he does not

[591] *Opere* I, p. 266. (Transl. from Galilei 2000, p. 18.)

[592] I have to thank Paolo Palmieri for pushing me on this point.

need to change the argument itself, he would need some other kind of justification for the mediative character of natural speeds. In the later presentations of the thought experiment *exactly such a justification will be provided*, which will be explicitly dynamical in character.[593] As we will see, once that he has provided this justification for the first premise, Galileo will also be in a better position to solve the conundrum raised by the conclusion.

Recapitulating our analysis of Galileo's thought experiment in *De motu*, we can say that it plays a crucial role therein in at least two respects. It enables him to make the transition from absolute to "specific" weight as the relevant factor for the natural motion of bodies, without having to define the latter explicitly. At the same time, it covers up the fact that Galileo by his own standards misses a fully intelligible dynamics for free fall. This transition from absolute to "specific" weight cannot be based on the effect of a medium on the weight of bodies, while Galileo nowhere gives a hint of how to understand "specific" weight as a primordial and immediately intelligible dynamical factor: the only model which he possesses for understanding motive forces is the balance which measures absolute weights; and all his dynamical thinking is based on the idea that speeds are caused by such forces.

7.4 *Discorso*: The impotence of specific gravity as a dynamic factor

7.4.1 Moment and absolute weight

Galileo never published or even circulated the manuscript of *De motu*. As a result, we can safely conclude that he was not convinced of the resulting natural philosophy, whatever the precise reasons for his own dissatisfaction.[594] However, throughout his career he kept returning to topics and concepts which were already introduced within *De motu*. We will have a brief look at one context in which he further developed and articulated some aspects of his dynamical thinking. This will further corroborate the analysis of the argumentative gap that is left in *De motu*.

In 1610 Galileo moved to Florence to become court mathematician and philosopher of the grand duke of Tuscany, where he almost immediately became invested in a controversy on the reason why bodies stay atop on water.[595] In the course of these discussions he realized the need to define specific gravity explicitly, an event which will further clarify the fundamentally limited status of this concept within his dynamical thinking. But before discussing this episode, it is necessary to briefly recapitulate some well-known basic facts about Galileo's conceptualization of mechanical effects.[596]

In the most extended version of *Le mecaniche*, Galileo introduces a set of definitions for his basic concepts. The first is immediately very interesting:

[593] To be discussed in section 7.5.1 and in chapter 8, section 8.1.1.

[594] See already chapter 4, section 4.3.2.

[595] This controversy was already frequently referred to in chapter 4

[596] See also chapter 5, section 5.2

> We call *heaviness* [*gravità*], then, that tendency to move naturally downward which, in solid bodies, is found to be caused by the greater or lesser abundance of matter [*materia*] of which they are constituted.[597]

Weight is here undeniably taken absolutely, and is still indissolubly connected to a tendency for downward motion. What is added is the specification that the more matter a body contains, the more heaviness and thus tendency for motion downward (a specification which was already implicit in *De motu*)[598]. But the real innovation of the mechanical treatise is the next concept to be introduced:

> *Moment* is the tendency to move downward caused not so much by the heaviness of the movable body as by the arrangement which different heavy bodies have among themselves. … Thus *moment* is that impetus to go downward composed of heaviness, position, and of anything else by which this tendency may be caused.[599]

As we have seen in chapter 5, this proved to be a very fruitful concept, which allows Galileo to give his mechanical treatise a clear and powerful structure. To our present purposes, one aspect of Galileo's treatment of a body's moment is crucial: its measurement. As witnessed by the expression "moment is that impetus to go downward," moment is intimately related with dynamical effects, yet it is always measured by a resisting counterweight. If we e.g. consider Galileo's analysis of motion on an inclined plane, we see that each body's impetus to go downward on such a plane is measured by the *weight* of a body keeping it in equilibrium, attached to it by a balance with bent arms, suspended above the plane.[600]

We can immediately learn two crucial facts about Galileo's dynamical thinking at this stage. Firstly, dynamical forces are measured by (static) weights. The balance remains the one and only instrument to understand force. The transition from the static measure to the dynamical effect is then made by the principle that the addition of "an insensible weight"[601] is sufficient to set in motion a weight that is held in equilibrium on a balance or an inclined plane. Secondly, moment as the cause of these dynamical effects arises from the modification of absolute weight. Although there is a clear

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[597] *Opere,* II, p. 159. (Transl. from Galilei (1960), p. 151.)

[598] See chapter 4, section 4.1.3

[599] *Opere* II, p. 159. (Transl. from Galilei 1960, p. 151.)

[600] Galileo's discussion of the inclined plane in *Le mecaniche* is an expansion of an earlier discussion in the second book of *De motu*. As was already remarked by Damerow *et al.* 2004, p. 147, n. 39, the presence of this discussion in the latter work gives rise to an incoherence, as the speed of the motion is measured by its "moment" (a term not yet introduced in *De motu*) and accordingly is proportional with the body's absolute weight (modified by the inclination of the plane). This is actually an instance of the dynamical conundrum that threatens the whole of *De motu.*

[601] *Opere* II, p. 163.

broadening of Galileo's dynamical framework through the introduction of *momento*, it is still indissolubly tied to absolute weight. Specific weight appears impotent to cause any effects.

Paolo Galluzzi has stressed that Galileo is cautious to remain silent on any link between moment and the resulting speeds in *Le mecaniche*.[602] As the treatise is devoted to mechanics, and as an investigation into precise measures of speed as a result falls outside its scope, it is hard to decide what to make of such silence. Anyway, for our present purposes it is enough to notice that absolute weights remain the paradigm cases of forces; and if Galileo possibly did no longer hold on unequivocally to a proportionality between forces and speed (although, as we will see, there are passages in the later *Discourse* on floating bodies which suggest that he had not yet let go this idea), he certainly has not found a way to make sense of any other possible connection.

7.4.2 Moment and specific weight

That specific gravity cannot unproblematically function as a measure for force emerges most clearly from Galileo's *Discourse on bodies that stay atop of water, or move in it* from 1612. The *Discourse* was an outcome of Galileo's involvement in a public dispute concerning the reason why ice floats on water.[603] The opening sections of the work are of particular interest to us, since Galileo starts by reconsidering the foundations of Archimedean hydrostatics. As was pointed out by William Shea, Galileo started a first draft of the work by repeating the analyses of floating, sinking, and rising of bodies in a medium as they were already presented in *De motu.* Subsequently, he discovered that these were insufficient because they are not generally applicable, a discovery that forced him to work out an original new approach to hydrostatics.[604]

The complication that arose for Galileo's former treatment of hydrostatics is that he realized that a body immersed in water is not always opposed by an equal volume of water. (Just imagine the case of a large body immersed in a very narrow vessel.) It is clear that this had profound implications for Galileo's understanding of hydrostatic phenomena. This vitiated his strategy of transforming differences in absolute weights to differences in (unconceptualized) "specific weights". Furthermore, how could he furthermore understand cases of equilibrium in such situations – when the absolute weights of an immersed body and a much smaller amount of water can differ greatly, although both being equal in "specific weight"?

[602] Galluzzi 1978, p. 219. Galluzzi ascribes this caution to Galileo's realization that any straightforward relation between moment and speed would be unable to account for the acceleration along an inclined plane.

[603] For an account of the circumstances surrounding the publication of the *Discourse*, see Biagioli 1993, chapter 3, which also contains interesting discussions on some other aspects of its contents.

[604] Shea 1972, pp. 18-20. Besides Shea 1972 and Biagioli 1993, other extended analyses of this approach, and Galileo's path leading up to it, are Galluzzi 1979, pp. 227-246, and Palmieri 2005a.

The first and foremost thing to notice is that Galileo presents this as an "admirable and almost incredible event"[605] which stands in need of an ingenious explanation. Although he will go on to give, for the first time, an explicit definition of specific weight (by stating that "the absolute weights of solids have the compounded ratios of their specific weights and their volumes"[606]), *he clearly does not see it as immediately explanatory to claim that the body and the medium have equal specific weight*. Once again, we find further corroboration for the fact that Galileo did not consider specific weight as a primordial explanatory factor. He nevertheless had to introduce it explicitly in the *Discourse*, for reasons that I will now briefly discuss.

Galileo's explanation, which is ingenious indeed, for this admirable event is based on his concept of mechanical moment. The general cause of equilibrium is equality of moments, not equality of absolute weights (which is only a special case of the former). The truly central model for understanding natural phenomena is the balance with unequal arms, where we can notice equilibrium obtaining between bodies of different absolute weight. One of the possible factors making up a body's moment is the speed of its motion.[607] Galileo will now also introduce this factor in his discussion of hydrostatic phenomena by taking into account the reciprocal motions of a body and the medium in which it is immersed. To this end he proves some geometrical theorems relating the volumes of the body and the medium with the path over which they respectively ascend and descend when the body is raised by hydrostatic pressure. When a body that is immersed in a very narrow vessel is expelled from the medium, the medium will descend over a proportionally much larger distance than the body will ascend. One can see this intuitively by noticing that the level of the medium will be lowered considerably more by the expulsion of the body (see figure 7.3). If the proportion between the lengths over which body and medium move are known, the proportion between the speeds is known as well, since both motions take place in the same time. This theorem, together with the explicit definition of specific weight allows Galileo to analyse all cases of immersion, emersion, and floatation. If the ratios of the specific weights of a body and a medium are given, the ratios of their absolute weights can be compared with the ratios of their volumes due to the definition of specific weight. The ratios of the volumes then can be transformed into a ratio of speeds due to the geometric theorem. As a result, the ratios of absolute weights can be compared with the ratios of the speeds, and the respective moments can be evaluated (resulting in equilibrium or disequilibrium). As an extra gain, Galileo now can also give a quantitative determination of the exact conditions of equilibrium, i.e. how much of a floating body will be immersed in the medium before it comes to a rest.

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[605] "accidente ammirando e quasi incredibile" *Opere* IV, p. 67. (Transl. from Drake 1981, p. 26.)

[606] *Opere* IV, p. 74. (Transl. from Drake 1981, p. 44.)

[607] This follows from the pseudo-Aristotelian proof of the law of the lever.

Once again, we see that absolute weights remain the primordial dynamical factor through their participation in a body's moment.[608] A body's specific weight merely expresses some proportionality between this absolute weight and the body's volume. This proportion then controls the specific proportion between the moments of the body and the medium in which it is immersed. As a result, specific weight can function as a kind of mathematical measure for the behaviour of a body in a medium, but it cannot be said to cause this behaviour in any unproblematic way. (It belongs to the mathematical part of his mixed science, not to the physical.) And if we consider the situation in a void, specific weight again loses all relevance. It is only when analyzing the interaction between a body and a medium that it functions as a relevant concept, as witnessed by close attention to Galileo's explanatory scheme.

In the concluding section of the *Discourse,* we find Galileo writing that the "heaviness [*gravità*] of the medium must be compared with the heaviness of the moveable" and "that is the single, true, proper, and absolute cause of swimming above or going to the bottom."[609] We are confronted with an apparent return to the original Archimedean scheme where the concept of moment does not occur. The extension of Galileo's explanatory scheme with that concept is only needed in those situations where the hydrostatic paradox can arise. However, the preceding pages of the treatise give the impression that Galileo might really have had specific weights in mind when writing this sentence – and many readers have understood him exactly that way.[610] He claims there that "it is not the greater absolute heaviness, but greater specific heaviness, that is the cause of greater speed, nor does a ball of wood weighing ten pounds descend more swiftly than one of the same material that weighs ten ounces."[611] The presence of this old *De motu* theory, but now formulated explicitly in terms of specific weight, in his *Discourse* testifies that Galileo had not yet found a way to fill in the gap introduced into his natural philosophy by the absence of any fully intelligibly dynamics for natural motion. Although the latter treatise is not focussed on the problem of explaining natural motion, the dynamical ideas which are introduced in it cannot help to make sense of the equal speeds of bodies of the same material. Indeed, when we consider the motion of bodies in a medium that is not enclosed in a vessel, as is the case for natural motion, the speeds of the body and the medium will always be equal, and the moments again reduce to the *absolute* weights.

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[608] Another way to state this would be that Galileo's conceptualization still starts from the balance as its model of intelligibility, but that he now has generalized this model to include the case of an unequal arm balance. (A similar move had already been made with his treatment of the inclined plane; see chapter 6, section 6.1.1 and 6.1.2.)

[609] *Opere* IV, pp. 139-140. (Transl. from Drake 1981, p. 194.)

[610] Stillman Drake, e.g., adds in his translation the following note to the passage just quoted: "Galileo considered his three kinds of floating to have been reduced to a single cause, the lesser specific weight of the floating object in comparison with water." (Drake 1981, p. 231). Cf. also Wallace 1983, p. 619: "he feels that he has successfully determined the true, natural, and primary cause of a body's floating or sinking, namely, its specific gravity relative to that of the medium in which it is immersed."

[611] *Opere* IV, p. 133. (Transl. from Drake 1981, p. 180.)

7.5 *Postille a Rocco*: Rethinking the thought experiment

7.5.1 Re-presenting the thought experiment

Galileo worked on his *Dialogue concerning the two chief world systems* mainly during the 1620's, and finally saw them to press in 1633. Dispersed throughout the work are allusions to the new science of motion discovered by the "Academician." For many seventeenth century philosophers, this was the only first hand knowledge they had of Galileo's work on natural motion. In one of these digressions, Galileo has Salviati state that Aristotle was mistaken in claiming that speed of fall is proportional to the weight of the falling body. He does not adduce any arguments for his statement, except for the empirical implausibility of such proportionality, but he does limit his remarks to bodies of the same material.[612]

It is of course an understatement to claim that the *Dialogues* spurred some debate. One of the philosophers who took up Galileo's challenge and tried to stand up in Aristotle's defence was Antonio Rocco, who in 1634 published his *Esercitationi filosofiche* in response.[613] Among the many things for which he took Galileo to task was his ignorance of the true reasons behind the phenomenon of free fall. As Galileo was not the man to let criticism that he considered misdirected easily pass, he prepared some notes (never published during his lifetime) in which he had his usual sarcastic fun with Rocco, and in which he gave the arguments which he had omitted from his *Dialogues.* It is at this point that he finally faces the gap that he was left with in *De motu.* How can he understand weight as a dynamic factor without thereby having to claim that speed of fall must be proportional with it?[614]

One of the remarkable things about Galileo's *postils* is their unusually direct style. Galileo seems not so much to be trying to convince Rocco, as that he is rehearsing his arguments for himself. He moreover introduces the central and most interesting part of his arguments by claiming that he will now be presenting the reasons by which he convinced himself of the falsity of Aristotle's teachings. We always have to be careful with such autobiographical reconstructions, but they undeniably give an invaluable insight in Galileo's thinking at this stage – if not necessarily in his earlier thoughts. Such an exercise in reconstruction forces him to think through the problem again, consciously trying to unravel

[612] *Opere* VII, pp. 249-250.

[613] Rocco's *Esercitationi* were reprinted by Favaro in his edition of Galileo's works (*Opere* VII, pp. 567-712).

[614] I owe the suggestion that I should have a look at Galileo's *postils to Rocco* to Paolo Palmieri. These *postils* have up to now not received much attention; Drake translates some passages in his *Galileo at work* (Drake 1978, pp. 361-367); and Shea 1972 and Galluzzi 1978 pay passing attention to some passages (see the respective indexes), as does McMullin 1978, p. 226. Palmieri 2005b provides a first more detailed analysis of these *postils* (which are strictly speaking much more than mere postils). Cf. already chapter 6, section 6.2.3, for some further aspects.

the most central aspect of it, which could then lead to a natural and gradual dawning of insight. It is as if in this place he is practicing his favourite Socratic questioning on himself.

First, Galileo claims, he "immediately felt repugnance" in his intellect upon reading Aristotle's texts, for "how could it be that a body ten times or twenty times heavier than the other should fall downwards with ten times or twenty times the speed"?[615] Taking this as his starting point, he then "formed an axiom that could not be doubted by anyone," i.e.:

> that any heavy body [*corpo grave*] that is descending has in its motion degrees of speed, limited by nature and so predetermined, that to alter them, by increasing the speed or diminishing it, could not be done without using violence against it in order to retard it or to prevent its abovementioned limited natural course.[616]

This axiom will serve as a justification for the crucial premise of his thought experiment. It will be remembered that in his initial presentation of the thought experiment in *De motu,* this premise was justified on grounds of the empirical plausibility of the mediative character of natural speeds. The fact that the new justification introduces explicitly dynamical considerations already testifies to the fact that Galileo has gained confidence in his understanding of the dynamics behind the thought experiment.

Next, Galileo introduces not the full blown thought experiment, but the limited version for two equal bodies that are falling with the same speed. In *De motu* this version came after the general thought experiment, and it served there to hide the absence of a fully intelligible dynamics behind the thought experiment. Having now started by laying out a dynamical principle, Galileo will use the same limited situation to show what this principle plays in the case of these falling bodies being tied together. The interesting fact about this situation is that no one would doubt that two equal bodies do fall with the same speed. But if the body that results from their being tied together would have a different speed, Galileo now asks "which one of them [original bodies] will be the one which, adding impetus to the other, will double its speed"? Whereas in *De motu,* he rested content with claiming that such a doubling of the speed would be unintelligible, *he is now trying to come to grips with this unintelligibility*. Given his dynamical principle, it is clear that at least in this situation none of the bodies will exercise a force on the other.

After this preparatory stage, Galileo presents the thought experiment. Again conspicuous is the explicitly dynamical formulation with which he describes the set-up:

> Assume now, mister Rocco, that these assumptions are true, which I don't think you are able to doubt. Thus, every descending weight [*grave*] has degrees of speed determined by nature, and that those degrees cannot be increased if not by violating its abovementioned natural constitution.

[615] *Opere* VII, p. 731.

[616] *Opere* VII, p. 731.

> Consider the two moving bodies *A,* the major, and *B,* the minor, of which, if it is possible, *A* is naturally faster and *B* less fast. Since, given the above, the natural speed of *B* can only be increased by violence, if we would want to increase it by attaching the faster *A* to it, it will be agreed that the speed of that body *A*, in violating *B*, would diminish partially, since there is no more reason that the bigger speed of *A* operates in the minor speed of *B*, than that the slowness of *B* reoperates in the velocity of *A*.[617]

The reduction argument then follows as before.

Not only is the formulation of the thought experimental set-up explicitly dynamical, it also betrays the origin of these dynamical ideas. I already stressed how the balance model shaped Galileo's understanding of forces, and that one of the central facts about this model was the presence of force-resistance pairs. This clearly surfaces in the passage just quoted, but even more importantly, it is now transformed into a true action-reaction pair (which from our vantage point is not strictly speaking the same as the equilibrating forces on a balance, which both exert their force – actually their moment – on a third body, the balance). If the faster body exerts a force on the slower, the slower will also have to exert an opposite force on the faster. This explicit recognition of the presence of a reaction for every action, at least in this kind of situation, will prove to be of the utmost importance in shaping Galileo's further dynamical thinking.

True, in the *De motu* presentation of the thought experiment Galileo had already stated: "who doubts that the slowness of the wax will be diminished by the speed of the bladder, and, on the other hand, that the speed of the bladder will be retarded by the slowness of the wax."[618] Nonetheless, the explicit insight that this mutual retardation and acceleration is the effect of interacting forces is conspicuously missing.[619] Most importantly, he does not think through its possible consequences for what happens in the thought experiment – as is testified by the very different treatment of the case of the two equal bricks. Considerations of empirical and intuitive plausibility seem to do most of the work in this early version. The true innovation of the *postils* lies in the attempt to uncover the grounds behind these judgements.

Immediately after the formal presentation of the thought experiment follows the most interesting passage of the *postils* – and, I would add, one of the most fascinating pieces of writing ever produced by Galileo. I will quote in full:

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[617] *Opere* VII, p. 732.

[618] *Opere* I, p. 265. (Transl. from Galilei 2000, p. 18.)

[619] It is perfectly possible (and I tend to believe: true) that at the time of *De motu*, Galileo understood the effect of combining the wax with the bladder (and vice versa) purely in terms of the effect on their "specific" gravity (in perfect analogy with what happens with the alloys of the king's crown), which is then only indirectly reflected in the speeds. (Remember that he is talking about what happens if we bring together bodies of a different specific gravity.)

These are mathematical advances, mister Rocco. They are consequences that, as far as I can ascertain, were not expected by you. And since I am certain that you persist in believing that once the gravity in *A* is increased by the addition of *B,* its velocity should also increase, if not proportionally to the weight [*peso*] as you required up to now with Aristotle, then at least in some way; how much would it not surprise you if I would show you that the addition of *B* does not increase the gravity of *A* with one hair, nor would the addition of a thousand *B's* increase it, and that given that is doesn't grow in weight [*peso*], by consequence its speed doesn't grow either, thus making you touch with your own hand how you are totally misled in this matter! So you will say: how could it be true that, *A* and *B* being two pieces of lead, the one put on top of the other, it will not increase its gravity? And I would add that even if *B* was made of cork the weight [*peso*] will increase, and I agree with you in admitting that *A*, placed on a balance, will weigh [*peserà*] more with the addition of *B*, even if it was not of cork, but a flake of cotton wool or one leaf of flax; and if *A* would weigh [*pesasse*] a hundred pounds, and *B* an ounce of plumes, on the balance their compound will weigh [*peserà*] a hundred pounds and one ounce. Yet to take advantage of this experience in reference to what we are concerned with is a useless and irrelevant matter. But at any rate, mister Rocco, if you put the palm of one hand under a cannonball weighing a hundred pounds [*100 libbre di peso*], which is suspended and supported by a rope, and you would only touch it, tell me whether you would feel weighed down [*aggravarvi*]? I know that you will answer no, for its weight [*peso*] is supported by a rope, and its descending is entirely prevented. When the rope is cut, and you would interdict this effect by the strength of your arm, you would indeed feel a burden [*gravarvi*] on your hand, which [hand] should do the job of the rope by prohibiting to the ball its natural descent. But when you would not oppose the ball which has been let free, but you would give in to its impetus by lowering the hand with the same speed at which the ball would descend, tell me anew if you, apart from touching it, would feel yourself weighed down by its weight [*dal suo peso gravarvi*]? It is absolutely necessary to reply that this is not the case, because you don't offer any resistance to the pressing [*premura*] of that weight [*peso*]. Conclude now from this clear and brief reasoning, since it is not possible to define being weighed down [*aggravato*] if not as that opposition to a weighing body that is descending, that by the addition and superimposition of the abovementioned bricks the one to the other, which even you will allow to be descending with equal velocity because they are the same, the gravity of the one is not increased by the other. Hence, also the velocity is not increased.[620]

"Yet to take advantage of this experience in reference to what we are concerned with is a useless and irrelevant matter." In this one sentence is contained the resolution of the conundrum. In one master stroke Galileo restructures the whole of his natural philosophy. *By asking Rocco to imagine using a falling balance, he shows its inapplicability as a model for a very central class of natural phenomena.* As a result, the balance loses the centrality which it always had within his philosophy. He now urges

[620] *Opere* VII, pp. 732-733.

that if we want to understand the dynamics of falling bodies, we should not be misled by what happens on a balance!

The way Galileo establishes this limitation of his original model of intelligibility merits closer attention. The most important step in his attempts to convince Rocco (and himself, I would suggest) occurs when he substitutes the hand and arm for the previously assumed balance. This substitution enables him to physically grasp the absence of action-reaction pairs in the case of the falling body and the hand moving down with the same speeds. Indeed, everybody can feel this for himself – even the illustrious signor Rocco could do so. The hand and arm are moreover easily assimilated to a second body falling along with the first body. And in the absence of any interaction, it then makes no sense to speak about the falling bodies weighing more or less. This latter conclusion is of course justified through the claim that "it is not possible to define being weighed down if not as that opposition to a weighing body that is descending." At first sight it might seem that Galileo is reverting to some kind of subjective notion of weight by placing this stipulation at the centre of his explanations.[621] Yet on this interpretation we would lose sight of the essentially interactive aspect of the action of the force of weight which he is laying bare here. His terminology makes clear that he is interested in the two sides of this interaction: there is no "pesare" of the body without the experience of being burdened ("aggravato"), which in turn finds its origin in the counter-force we have to keep on exerting on the body. Galileo is able to extract something fundamental about the property of weight from our way of experiencing it: *a body's gravity gives rise to "peso" only if it is opposed by a continually (re)acting resisting force*.

7.5.2 *De motu* revisited

As we have seen above, in *De motu* Galileo had given the following definition:

> We are said to be weighed down [*gravari*], when a certain weight [*pondus*] which tends downward by its heaviness [*pondus*] rests on us, and we need to resist by our force [*vi*] in order that it does not go down any further; now this resisting is what we call being weighed down [*gravari*].[622]

It is important to ask why this definition had not already in this early work led up to the conclusions which are now shown to follow from it. In the first place it is important to note that Galileo had introduced this definition of being weighed down in *De motu* to back up his claim that elements have no weight in their own place. Since elements simply do not tend downward anymore when they are in their natural place, this situation is considerably more straightforward than when one is dealing with

[621] Palmieri 2005b, p. 232, n. 26, speaks of a "'psychological' definition of weight".

[622] *Opere* I, p. 288. (Transl. from Galilei 2000, p. 39.)

falling bodies. These do have a tendency for downward motion, and the balance would thus have seemed eminently applicable.

Most importantly, *the balance itself serves to hide the necessary action-reaction pairs in the measurement of weight.* After all, the seemingly crucial elements for such measurements are the weight and counterweight and their respective distances from the fulcrum. The physical role of the fulcrum itself is often passed over in silence, although it is precisely the fixed nature of the latter which enables the measurement. The counterweight can only resist the downward motion of the weight because the fulcrum introduces a reaction force on the combined action of both weights into the system. (If the bodies weren't continually weighing down on the fixed point this reaction force would not arise, and the system would simply fall down.) Yet, the confusion easily arises that it is the counterweight which plays the resistive role of the given definition, which would make the non-sense of using a falling balance less obvious.

This comes out clearly in the revised version of the chapter dealing with the question whether an element has weight in its own place. Galileo stresses still more emphatically than in the first version that we cannot say that the elements have weight in their place because "heavy bodies cannot always exert their weight [*gravitatem*]" The reason is obviously that the parts of the medium "resist with as much weight as is exerted upon them".[623] But we can remember from chapter 4 that this situation was immediately assimilated to a balance with a counterweight acting as the resistive force. The role of the fulcrum simply cannot be thematized as long as Galileo holds on to this direct analogy! In this respect it is suggestive to note, as has been done by Paolo Palmieri, that in *De motu* Galileo had presented the two equal bricks as falling adjacent to each other, while in the *postils* he is considering bricks which are put upon each other.[624] This seems to be exactly what is needed to bring the interactive character of weighing down to the fore, whereas the former presentation was still very much tied to the image of a balance.[625]

That it is furthermore precisely the interactive aspect which is still missing at the time of *De motu* is proved by a passage in which Galileo seems to come close to the insights which he reached only here in the *postils*. In offering an explanation of the accidental acceleration of free fall, he already stressed the fact that when a "stone is at rest in someone's hand, one must not say that in that case he who holds it impresses no force on the stone: for since the stone exerts pressure downward by its

[623] *Opere* I, p. 365. (Transl. from Galilei 1960, p. 122.)

[624] Palmieri 2005b, p. 232.

[625] Guided by this image it might even have appeared as if the two falling bodies were keeping each other in equilibrium, hence mutually weighing down on each other (this is the kind of image which Galileo will repudiate in the fragment on the law of the lever, referred to at the end of the present section). This is moreover precisely the image which guided Benedetti in presenting his version of the thought experiment, since he suggests that four equal bodies fall down with the same speed as the body that is composed by their conjunction because the separated bodies will together be able *to equilibrate* the body composed of them *during their fall*; cf. his *Resolutio…* from 1553 (translated in Drake and Drabkin 1969, see especially pp. 150-151).

heaviness, it is necessary that it be impelled upward by the hand with an equal quantity of force, neither larger nor smaller."[626] Yet when the stone is let go, the force of the hand remains for some time with it, although continually diminishing in strength.[627] A few pages earlier, Galileo had already explained how we should conceptualize such impressed force. The body in which it is impressed retains its natural and intrinsic weight, but it assumes a preternatural lightness "in the same manner as [its own innate and intrinsic heaviness] is also lost when it is placed in media heavier than itself."[628] And the first book of *De motu* had made abundantly clear how we should model this effect of a medium. The idea of impressed lightness actually becomes an attempt to have the balance model transferred *into* the body. To put the situation graphically: Galileo imagines the body during its fall as if it is continually in a balance with as counterweight the impressed force which is gradually diminishing, causing the body to become heavier in fall and speeding up. He had not yet freed himself from the falling balance; and the resisting force was indeed assimilated to a counterweight.

That Galileo brought precisely these features to the focus of his attention *after* having rethought his thought experiment is testified by a dialogue fragment which was probably intended for inclusion in either the first or the second edition of the *Discorsi*, but which remained in manuscript form.[629] In this fragment, Galileo expresses doubts about the conclusiveness of the pseudo-Aristotelian proof method for the law of the lever (he had always significantly refrained from granting it the status of a demonstration in his writings). Instead he offers a more satisfactory proof; a proof which also differs from the Archimedean proof that was given by Galileo both in *Le mecaniche* and in the second day of the *Discorsi* (presumably because Galileo sought a more physically appealing proof). If one puts two weights on a balance, and then let it go freely, it will fall perpendicularly along the line connecting the common centre of gravity of the two weights with the centre of heavy things. But if we fix the balance in this common centre of gravity, there will be no motion and the balance will be in equilibrium (and if this fulcrum does not coincide with the centre of gravity, the arms of the balance will respectively move up and down). Now, this proof was not original with Galileo, as it faithfully recapitulates the teachings of Guidobaldo del Monte (without mentioning the latter).[630] However, the fact that we find Galileo reversing to exactly this kind of explanation is significant. In the *Dialogue concerning the two chief world systems* of 1633, he had still presented the pseudo-Aristotelian proof method without any sign of dissatisfaction (but with the usual caution in not calling it a demonstration, but referring to its confirmation by many experiments "*con molte esperienze*").[631] But now, after

[626] *Opere* I, p. 320. (Transl. from Galilei 2000, p. 70.)

[627] Cf. chapter 4, section 4.1.5.

[628] *Opere* I, pp. 311-312. (Transl. from Galilei 2000, p. 64.)

[629] *Opere* VIII, pp. 438-440.

[630] See chapter 3 for Guidobaldo's mechanics. Micheli 1995, pp. 150-151, points out the similarity between Guidobaldo's and Galileo's treatments, yet in a slightly different context.

[631] *Opere* VII, pp. 241-242

having rethought his thought experiment, he apparently comes to prefer an explanation which explicitly singles out the necessity of a fixed fulcrum. To put it a little bit more suggestive: his new method of proof is designed to show that a falling balance is no longer an instrument for the measurement of weight.

7.6 Understanding weight and motion

7.6.1 The role of Galileo's thought experiment in his dynamical thought

Philosophers of science in the second half of the twentieth century have been mainly interested in an analysis of the structure of scientific explanations, and tended to be rather critical about the notion of understanding which was often deemed to be too subjective to be of any real interest.[632] This is not the time and place to enter into a critical re-evaluation of these views, but let it suffice to point out that any view on the nature of explanation has to account for the status of certain basic brute facts which are apparently not in need of further explanation and can serve as explanatory bedrock for other phenomena. It seems that we have to take serious the idea that for any broadly conceived explanatory framework there is always something about the proffered explanations that is responsible for them "making sense." (This feature comes especially to the foreground in periods where competing frameworks struggle for the right to speak about a class of phenomena; periods where the allegation of unintelligibility is often levelled in both directions.) The sense of intelligibility is not merely a subjective feeling accompanying explanations, but refers to a basic way of going about in offering and receiving them; a basic way which can be shared by a large group of people and which most importantly can have a clear normative force.[633]

This is directly connected with the status of the empirical principles that should ground Galileo's mixed science. These should command universal assent because of their evident character.[634] They are supposed to express *what the things in the world themselves show.* In this way the mathematician can assume a set of facts that need not be further explained and as a result open up the possibility of explaining further phenomena. What is most important for our purposes is not so much the existence of such a set, however, but the grounds on which it is selected.

Galileo wants to reduce phenomena to shared experiences which are incontestable for "every man of ordinary intelligence"[635] *when* the latter is interacting with an instrument such as the balance. As explained, this implies that nature's discursive function as a regulative instance has been crucially transformed. As we have also seen, this interaction has to be guided by an implicit form of

[632] See Hempel (1965), p. 413, for an exemplary and influential statement of this view.

[633] Cf. chapter 1, section 1.2.2.

[634] Cf. Dear 1995, p. 42. See also chapter 4, section 4.2.2.

[635] *Opere* VII, p. 183. (Transl. from Galilei 2001, p. 183.)

performative reason. But against this background the balance can function as a model of intelligibility. This makes it possible that the phenomena present themselves in a structured and thus intelligible way, which in turn implies that Galileo's mathematical explanations of the properties of natural motion can also make sense.

But it is of course one thing to have a model of intelligibility which in principle makes it possible to anchor mathematical explanations in shared and incontestable experiences, and another thing to put it fruitfully to work. This supposes that these experiences can be seamlessly integrated within the explanatory scheme. However, the latter also has its own exigencies that at times potentially drive it towards another road leaving a gap between scheme and basic experiences. This is the natural result of the fact that the scheme is always supposed to explain a different and richer set of phenomena. We have now seen that this is what happened in Galileo's first attempts to come to grips with the dynamics of free fall.

It is precisely in an attempt to cover up this gap that Galileo introduces his thought experiment for the first time. It is primarily *intended to restore intelligibility to his explanatory scheme*, rather than to provide independent empirical confirmation thereof.[636] It is in this function that it continued to play a crucial role in Galileo's dynamical thinking. Galileo remained deeply concerned with the connection between on the one hand mechanical instruments such as the balance, and on the other hand the phenomenon of free fall; and it is exactly the thought experiment that allowed him to mediate between both sets of phenomena. It is through *rethinking* the thought experiment that he was able to uncover the crucial facts that were responsible for the gap that – with hindsight – had to exist within his first attempts at natural philosophy.

It is probably no accident that it was precisely a thought experiment that lay behind much of the dynamics of Galileo's thinking. Its seemingly paradoxical character still has the power to fascinate many people and the act of rethinking the thought experiment was probably stimulated by exactly this paradoxical character – with as effect that in unravelling the paradox Galileo was able to forge profound changes in his conceptual framework.[637] But the effect of this rethinking must remain hidden as long as we ignore the subtle but profound differences that exist between the different presentations

[636] Galileo's thought experiment has been the topic of some recent philosophical debates, but these primarily focussed on its epistemological status, i.e. the kind of confirmation it can provide for his empirical claim on the independence of speed of fall from weight, and not so much on its role within Galileo's dynamical thinking. (The conclusions reached in the present chapter are relevant for these debates, but I won't spell this out.) Koyré 1968 and Westfall 1966, 1971 are among the few authors who explicitly consider this role, as was already done by Mach 1960, p. 251. However, these authors also remain silent on the crucial role played by Galileo's *rethinking* of this thought experiment during different stages of his career. That is, they assume that Galileo could draw some important lessons from the thought experiment, but they do not treat the question how Galileo came to see that it implied these lessons.

[637] Paolo Palmieri has recently stressed the important cognitive role that paradoxes played for Galileo, both within his own thinking and in the presentation of his ideas (Palmieri 2005a).

of his thought experiment and especially the different justification for its crucial premise, as has been done up to now.

7.6.2 Towards a new model of intelligibility

It is interesting to note how Galileo establishes the limitation of the balance as a model of intelligibility by exploiting some of its particular properties. In the *postils* to Rocco he leads his reader through a number of steps that make clear which are the conditions under which the balance can function to ascertain the properties of natural bodies. To this end he asks the reader to imagine engaging in a particular set of bodily interactions with a heavy body, which taken together show that it *makes no sense* to conceive of the motive power of a moving body as a "static" weight. Because the balance must be used in a highly disciplined manner, it is possible to for Galileo to show in a very precise way some of its inherent limitations.

Galileo started his endeavours in natural philosophy with the firm belief that weight was the characteristic property of all natural bodies that allowed them to become integrated in a mathematical explanatory scheme. He now discovers that the closure that characterized the balance as a particularly interesting system is irrelevant for understanding falling bodies. As a consequence, it turns out that weight is not the right characteristic to introduce as the central property of bodies in building a mathematical science of motion.

As a result of this deconstruction of the balance model (as applicable to falling bodies), Galileo can now uphold seemingly conflicting theses. Weight is indeed a force, and if a body has more matter (and as a result more gravity), it exerts a greater force that can be measured using a balance. *Exerting* more force does moreover result in an increase of speed. And yet, speed of free fall can be independent of gravity, the reason being that falling bodies do not necessarily weigh down more by the addition of more matter – or to say the same thing, that this extra added matter exerts an extra force on the body which would cause it to speed up.

Galileo also must have felt the uneasiness that anyone feels who is first confronted with this insight. After all, as was already repeatedly claimed in his earlier writings, weight as measured by a balance is caused by the body's gravity, which is a tendency to move naturally downward. In his *postils to Rocco* Galileo stresses that this still holds true,[638] but he also warns Rocco that it does not necessarily follow that this greater tendency causes a greater speed, only that the body "has to tend more downwards."[639] It is true that Galileo does not yet give an explicit explanation of how we *should* understand the precise link between this tendency and the resulting speed, but we will see that there are some clear hints in his latest thoughts on natural motion.

[638] *Opere* VII, pp. 722, 725.

[639] *Opere* VII, p. 722.

As Winifred Wisan once aptly stated, "Galileo … lived long enough and maintained sufficient mental prowess to become in effect his own best disciple".[640] The fascinating creative process that lay behind the development of Galileo's dynamical thinking – a process that spans a period of more than fifty years – bears striking witness to this fact. This will be further illustrated in the next chapter where we will discuss some aspects of the *Discorsi* and some fragments that postdate its publication. At this point he will exploit the thought experiment to find a way in which he can reintroduce the motive power of bodies in his mathematical science of motion. That is, the thought experiment will start to function as a model of intelligibility in its own right.[641]

Postscript to chapter 7

De motu revisited, revisited

Writing chapter 4, which was written long after the present chapter, I reread the memoranda attached to *De motu*. To my great discomfort I came across the following note that Galileo had already written down at the time of working on *De motu*:

> The definition of the heavy and the light through motion handed down by tradition is not a good one: for when a heavy or light thing is being moved, it is neither heavy nor light. For that thing is heavy which exerts weight on something; but what exerts weight on something else is resisted by that thing; hence a heavy thing, when it exerts weight, is not moved: as is evident if you have a stone in hand, which then will exert weight when the hand resists its heaviness; but if it is moved downward with the stone, the stone will not then exert weight on the hand. Hence the definition will better be: That thing is heavier which remains under things that are lighter.[642]

Now, let me first point out that Galileo is again treating a topical problem having to do with motion. The Jesuit philosophers at the Collegio Romano, e.g., did discuss whether "the definitions of light and heavy that are given in terms of rest, that is, standing above and below, are to be preferred to those given in terms of motion."[643] It is also clear that the way Aristotelian philosophers tried to arbitrate this question was again very different from Galileo's proposed answer; the former e.g. tried to assess whether the perfection of the nature of the elements consists more in rest or in motion.[644]

The fact remains that the presence of this passage in the memoranda seems to go counter to the above analyses. This may in the first place stand as a methodological warning post that as

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[640] Wisan (1984), p. 271.

[641] See chapter 8, sections 8.1 and 8.2.

[642] *Opere* I, p. 413. (Transl. from Galilei 2000, p. 157.)

[643] Wallace 1984, p. 169.

[644] Wallace 1984, p. 169.

historians of science we are extremely dependent on the sources that survived the dust of time, and that any of the conclusions that we can reach on their basis are bound to remain highly conjectural. (Admittedly, in the present case this was primarily due to my own unaccountable neglect of an important source that was easily consultable, but the general point may stand.) The historian that studies *De motu* is presented with some further complications, as we are dealing with a rich set of traces of Galileo's attempts to construct a mathematical natural philosophical treatment of motion, yet without a finished produced that is singled out by himself as his considered view of the matter.

So what do we have to make of this passage from the memoranda? Fact is that Galileo did not include it in any of the versions of his treatise. But he does define heaviness as that property of bodies "to remain under lighter ones,"[645] in agreement with the conclusion reached in this passage. Was it just his stab at the topical question treated by the Aristotelians, and did he finally decide that it was not important enough to include it? Or maybe, did he realize while working on his treatise that he should try to justify the fact that he had opted for this characterization? In any case, and I take this to be the most important, as far as we can judge it stands completely unconnected with any of the discussions on the speed of motion.

The most careful conclusion to draw is that Galileo at the time of *De motu* already had all the elements at his disposal that would later allow him to unravel the dynamical conundrum, but that there is no evidence that he brought these elements together at that time. That is, he does not use the situation as described in the above passage to make intelligible the dynamics behind the thought experiment, as he would do in his *postils to Rocco.* As a consequence, I don't think that the presence of this passage among the memoranda necessarily invalidates my analysis of the dynamical conundrum. It only highlights the complexity of the writings that taken together make up Galileo's "older notes on motion". It is furthermore not implausible to suggest that Galileo first realized that he could unravel the paradoxical situation presented by the thought experiment by exploiting the insight contained in the above passage as he was browsing through the folder that contained these "older notes on motion" while planning to write a rebuttal of Rocco's criticisms. (That the analysis in the *postils* is inspired by this passage is undeniable.) After all, the discussions in his *postils* recapitulate many messages from *De motu*, so it is highly probable that he had this folder close to him at that time.

Another, more far-reaching possibility is to conclude that this passage shows how Galileo already became aware of the inapplicability of the balance model while writing *De motu.* The disappearance of the chapter explicitly spelling out the analogy in the revised version could reflect this insight.[646] (But it must be stressed that whereas in the *postils* Galileo explicitly likens the situation of the hand holding the body with that of the body lying in a balance, this association is not yet made in the memoranda fragment.) Yet even in this revised version Galileo still sets the speed of motion equal

[645] *Opere* I, p. 253. (Transl. from Galilei 2000, p. 3.)

[646] Cf. *supra* section 7.2.1.

to the difference in absolute weight between the body and the medium through which it is moving. Seen from this light, it becomes possible that this was the main reason for abandoning the treatise without even ever circulating it. But even if we want to opt for this interpretation, the analyses in the present chapter can still stand; we would only be forced to push back the chronology almost forty years in time.

So maybe the most important thing that we can learn from this passage is that from the very beginning, Galileo was conscious of the fact that measuring physical quantities is a complex operation, which demands very specific circumstances to be carried out validly. It may stand as a testimony of Galileo's intuitive agility that he had already introduced the situation of the hand moving down with the body while he was still assessing how far an Archimedean scheme could be pushed wherein the body supposedly still exercises its weight when moving downward (as it is this weight that causes it to have the speed it has).

FIGURES TO CHAPTER 7

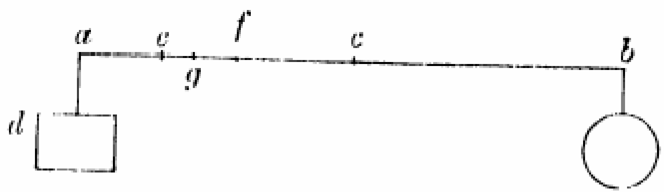

FIGURE 7.1

The hydrostatic balance from *La bilancetta*. A sample of respectively gold, silver, and a mixture of both are first weighed in air from the point *b* with counterweight in *a.* When the samples are now weighed in water, the counterweight will have to be shifted to the respective positions *e*, *f*, and *g*. Associated with each different kind of body is a position on the balance, independent of the volume of the bodies. (*Opere* I, p. 217.)

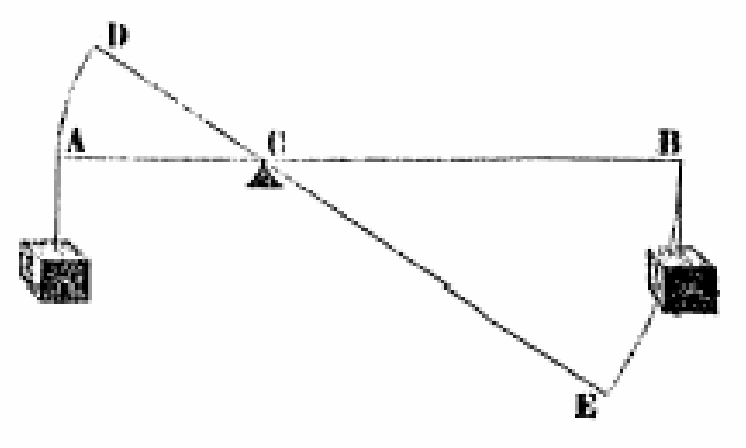

FIGURE 7.2

The "pseudo-Aristotelian" proof of the law of the lever. The lighter body at point *B* will be able to equilibrate the heavier body at *A* because in moving from *B* to *E* it moves faster than the other body in moving from *A* to *D* (since both motions take place in equal times). Different positions on the balance are associated with different speeds. (*Opere* II, p. 163).

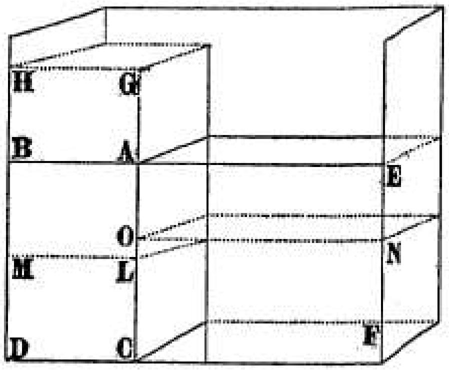

FIGURE 7.3

A solid *ABCD* is immersed in a vessel with water filled up till the level *AE*. When the solid body is raised from the water up till it is in the position *GHLM* the water will fall over a distance *AO*. Galileo easily proves that “the descent of the water, measured from the line *AO*, has to the ascent of the prism, measured from the line *GA*, the same ratio that the base *GF* of the solid has to the surface of water *NO*.” In a very narrow vessel, the water will accordingly fall over a proportionally larger distance than the body will rise. (*Opere* IV, p. 72. Transl. from Drake 1981, p. 40.)

8 Dynamical principles for a science of motion

It was seen in the previous chapter how Galileo's thought experiment led to the demise of the balance as the central model for his theory of motion. In the present chapter, I will show how Galileo tried to render the phenomenon of free fall intelligible in a new way. It will be seen how the thought experiment takes over the role of the balance as a model of intelligibility directing Galileo's dynamical thinking, by drawing particular facts about the relation between weight, free fall, and equilibrium to his attention.

A large part of my analysis will be based on some fragments that postdate the publication of the Discorsi. *These primarily involve Galileo's attempts to come to grips with the phenomenon of percussion, which he had already unsuccessfully grappled with in* Le mechaniche. *In trying to understand this phenomenon, Galileo became fully aware of the special role played by time within the dynamics of falling bodies. As we will see, this made it possible for him to understand both weight and acceleration as common effects of a deeper-lying cause, a body's moment of gravity. In this way, he actually separated what could from now on be understood as statics and dynamics.*

In the last section, it will be seen how this enabled Galileo to close an important gap that existed in the formal structure of the Discorsi, *where he had been forced to introduce a postulate on the speeds that bodies acquire on differently inclined planes of equal height. However, this goes counter to his insistence on the fact that the basic physical principles that constrain a mathematical science must be evident for all. His renewed engagement with the phenomenon of percussion offered Galileo the required insights to understand the dynamics underlying the validity of his postulate. This then allowed him to anchor his mathematical theory of motion anew in a suitable principle that expresses a basic property of all bodies.*

At this point we have come full circle in our investigations of the grounds of Galileo's science of motion. The balance provided his starting point but we have seen how it was quickly joined by the inclined plane and the pendulum. The thought experiment made him aware of the limitations that accrue to all constrained systems as models for phenomena of motion. Yet it looks as if this constraint at the same time is what makes possible the kind of closure that interested Galileo so much. His latest fragments on motion, analyzed in the present chapter, show how Galileo started broadening his conceptual framework in a way that could allow one to discern a relevant kind of closure also in free dynamical systems. This study of Galileo's attempts at stabilizing his conceptual apparatus thus complements the study of his stabilization of empirical situations in chapter 6. The stabilization of concepts cannot be independent of the stabilization of the empirical situations, as the former are supposed to represent the latter. And both are dependent on a prior stabilized field of knowledge as analyzed in chapter 5.

8.1 *Discorsi*: Presenting the thought experiment

8.1.1 From the model to the world

Almost immediately after his fateful encounter with the Roman inquisition that followed upon the publication of the *Dialogue*, Galileo began preparing a work in which he would finally expound his theory of motion. The work, which would go to press in 1638 as the *Discourses and mathematical demonstrations concerning two new sciences pertaining to mechanics and local motions,* was essentially a continuation of many earlier researches on both natural motion and the strength of materials. As we have seen, at the time of composing the *Discorsi* Galileo also wrote down his *postils to Rocco*, so it is not surprising to find much of its contents reappearing in the book.[647] However, there are also some minor but relevant changes in the presentation which I will comment upon in the present subsection.

Galileo's refutation of Aristotle's teachings on free fall is one of the many topics treated in the first day. It follows almost exactly the more than forty years old lead of *De motu*. He first attacks the idea that the speed of fall is proportional to the weight of the bodies by stating that such proportionality is simply ridiculous, since empirically wildly implausible. Thereupon follows the thought experiment, explicitly restricted to bodies of the *same* specific gravity.[648] The presentation of the thought experiment itself is clearly modelled on the earlier recapitulation in the *postils to Rocco*. However, it is no longer preceded by the limited argument for two equal bodies. Apparently, Galileo had become so confident in his understanding that he no longer thought that he needed this preliminary situation, which had served him so well to unravel the conundrum. The argument itself is also presented in a tighter form, apparently the result of a conscious rewriting, but the crucial premise on the mediativity of natural speeds is again introduced on the basis of exactly the same explicitly dynamical considerations.

After the presentation of the reductio argument follows a discussion between Simplicio and Salviati, which Galileo uses to convey the same crucial message as in his earlier reprimand against Rocco. The presentation is again much more streamlined, thereby loosing some of its earlier forcefulness, but there is an interesting novel feature, which I have emphasized in the text:

> SIMP. I find myself in a tangle, because it still appears to me that the smaller stone added to the larger adds weight [*peso*] to it; and by adding weight, I don't see why it should not add speed to it, or at least not diminish its speed in it.

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[647] It is interesting to note that also many of the passages on infinity in the first day of the *Discorsi* were already contained in these *postils*, which as a result provide a fairly extensive sketch of the discussions in this first day.

[648] This limitation is explained in section 8.1.2.

SALV. Here you commit another error, Simplicio, because it is not true that the smaller stone adds weight [*peso*] to the larger.

SIMP. Well, that indeed is quite beyond my comprehension.

SALV. It will not be beyond it a bit, when I have made you see the equivocation in which you are floundering. *Note that one must distinguish heavy bodies [*gravi*] put in motion from the same bodies in a state of rest.* A large stone placed in a balance acquires weight [*peso*] with the placement on it of another stone, and not only that, but even the addition of a coil of hemp will make it weigh [*pesar*] more by the six or seven ounces that the hemp weighs [*peserà*]. But if you let the stone fall freely from a height with the hemp tied to it, do you believe that in this motion the hemp would weigh on [*graviti sopra*] the stone, and thus necessarily speed up its motion? Or do you believe it would retard this by partly sustaining the stone?

We feel weight [*sentiamo gravitarci*] on our shoulders when we try to oppose the motion that the burdening weight [*peso*] would make; but if we descended with the same speed with which such a heavy body would naturally fall, how would you have it press and weigh on us [*graviti sopra*]? Do you not see that this would be like trying to lance someone who was running ahead with as much speed as that of his pursuer, or more? Infer, then, that in free and natural fall the smaller stone does not weigh upon [*non gravita sopra*] the larger, and hence does not increase the weight [*peso*] as it does at rest.[649]

First notice the complete reversal with respect to the earlier presentation of the thought experiment in *De motu*.[650] There the reductio argument was immediately followed by the question: why would the bodies change speed on being tied together? Here we are confronted with the opposite question: why wouldn't they? But most importantly, the question is now followed up with an answer. It seems that it is only now, when he is in the position to dismantle the conundrum, that Galileo dares to bring it fully into the open. Now he can play his favourite argumentative game of first completely destabilizing his opponent's prior convictions by making him admit what he seemingly has to deny, followed upon by the presentation of his own alternative view which enables him to restore coherence in at least the reader's mind (if not necessarily the opponent's).

The innovation with respect to the treatment in the *postils to Rocco* is subtle but of the utmost importance.[651] Whereas in the earlier exposition, Galileo merely claimed that the balance could not be used to measure the weight of falling bodies, he now sees a distinction *within* these bodies themselves.

[649] *Opere* VIII, p. 108. (Transl. from Galilei 1974, pp. 67-68.)

[650] Cf. chapter 7, section 7.3.2.

[651] Another innovation introduced in the *Discorsi* is the example of the lance, which seems to open up Galileo's insight in action-reaction to a more general treatment of impact. Interestingly enough, Galileo indeed takes up the very same example later in the fourth day when he discusses the differences in impact of projectiles depending on the state and characteristics (elastic vs. inelastic) of the thing struck. At the same time, this treatment clearly shows the limitations of Galileo's understanding of the generality of action-reaction, as in this context he remains almost completely (but only almost!) silent on the effect that the impact has on the motion of the projectile itself. (*Opere* VIII, p. 291.)

That is, he explicitly moves from a limitation in the model to an essential difference in the target system. *We* would say: either a body's weight is used in accelerating it, or in pressing down on the balance which resists its motion, but it cannot do both things simultaneously. We will see in below how we can impute to Galileo something rather similar on the basis of his treatment of fall in a dense medium.[652]

At this point, we witness how a peculiar feature of a model of intelligibility (its inapplicability) is transferred to the world. This feature can now become one of the immediate characteristics that the things in the world "show themselves." Of course, one first has to be taught to see (or feel) this fact – through thinking through the thought experiment – but once one has learned to notice it, it becomes one of these incontestable experiences that can back up explanations of more complicated phenomena. This is of course not to deny that learning how to exploit this fact in explaining further phenomena takes a lot of hard work, which it finally would take someone of the stature of Newton to fully accomplish. Yet, we will see in sections 8.2 and 8.3 how Galileo himself already made some preliminary attempts in such a direction.

8.1.2 The function of the thought experiment

One thing that has puzzled some scholars, such as Alexandre Koyré, is Galileo's explicit restriction of the thought experiment to bodies of the same material.[653] After all, he wants to assert that the conclusion should be valid for all kinds of bodies, and apparently there is nothing in the thought experiment which seems to necessitate such restriction. Instead he only removes this limitation further on in his discussions, upon introducing the extrapolation argument for bodies falling through media of ever greater rareness.[654] So why not use the thought experiment to reach his intended goal at once? There are a few possible lines an answer might take. One of these stresses the historical development of Galileo's own ideas.[655] The chosen order of presentation in the *Discorsi* could be seen as a simple recapitulation thereof. As we have seen, this is the way in which the presentation in the *postils* was fashioned. Since we can find more or less the same structure of presentation in the *Discorsi,* it seems that this could be at least part of the explanation. However, Salviati explicitly stresses that the conclusion of the thought experiment is *only valid* for bodies of the same specific gravity.[656] Apparently, Galileo didn't see this as merely a historically contingent limitation. If we can understand the reason behind this limitation, we would be in a much better position to understand the status of Galileo's thought experiment within his own thinking.

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[652] Cf. section 8.1.3.

[653] Cf. Koyré 1968, p. 49.

[654] Cf. chapter 6, section 6.2.2.

[655] This is the answer given by Koyré himself.

[656] *Opere*, VIII, p. 109.

It is useful to go back for a moment to some of the discussions in chapter 7. In commenting on Galileo's failed attempt at justifying the equal speeds of fall of bodies of the same material, I noted that such equality would only follow if it were assumed that these bodies would already have the same speed of fall in the void – a fact which could not be proven by Galileo's hydrostatic considerations, but which could by the thought experiment.[657] Given that the thought experiment can prove this equality of speeds in a void, Galileo's hydrostatical analysis of the effect of a medium shows that its conclusion is still valid in a medium.[658] And Galileo still uses hydrostatics as a means to analyze the effects of a medium in the *Discorsi*.[659] It is thus undeniable that on Galileo's own understanding of the situation the conclusion of the thought experiment is valid for all kinds of media (dense or vacuum) *only if* the bodies have the same specific gravity. His argumentative strategy moreover does not allow him to single out the void before he has proven its possibility, and he will do this only further on in the first day. To summarize: Galileo limits his thought experiment to bodies of the same specific gravity, because he knows that only then the conclusion of the thought experiment is valid in all contexts.

It could be retorted that nothing in the thought experiment itself justifies such limitation. This is true, due to the negligence of the effects of a medium in its set-up. But again, this is explainable if we take into account Galileo's argumentative strategy, which consists in first analysing the dynamical role of the weight of a falling body, and only afterwards the role of the medium. However, it is clear that he himself knew very well what the effects of a medium were. Hence, without having the aims to justify this at that point of his presentation, he was conscious of the need to limit his thought experiment to bodies of the same specific gravity.

This analysis clearly shows that Galileo's thought experiment does not function in an argumentative vacuum. Some factors that are not thematized in the thought experiment itself remain operative in limiting its scope. That these factors are not thematized, and that yet Galileo is clearly conscious of their relevance for the situation, demonstrates the function the thought experiment had for him. He is not so much interested in proving semi-empirical regularities, since that would imply that he should have taken into account all factors known to be relevant, as that he is concerned about understanding how weight functions as a dynamical factor. And the latter analysis is most perspicuous in the case of bodies of the same specific gravity, since for them the effect of the medium can be neglected. (When the bodies would differ in specific gravity, their weight will no longer be affected in a proportionally similar way by a medium, which would complicate what would happen when they were to form a compounded body.)

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[657] Cf. chapter 7, section 7.2.2 and 7.2.3.

[658] This implies that the thought experiment is not rendered superfluous by the hydrostatical analysis, as is claimed by Clavelin 1968, p. 334, n. 12. At this point of the discussions, without taking into account the extrapolation argument, it is still essential to guarantee the equality of speed of fall in the void.

[659] This use might seem problematic from some perspectives, and it could even be claimed that it is outright inconsistent – I will discuss this issue in section 8.1.3.

8.1.3 Understanding the effect of a dense medium

After having presented the thought experiment and his claim that in a void *all* bodies would fall with the same speeds, Galileo goes on to explain why we do not observe this equality in dense media.[660] The explanatory scheme is immediately recognizable: the primary effect of a medium is to subtract from the weight of an immersed body, following Archimedean hydrostatics.[661] The re-emergence of this framework within the *Discorsi* raises some problems for Galileo, which he nowhere explicitly tackles, but which he tries to circumvent in his presentation.

The guiding idea behind Galileo's explanation is simple. Assuming the empirically suggested equality of speeds in a void, the alleviation effect of a medium serves as a measure for the way in which this speed is affected by the medium. The only innovation with respect to *De motu* thus seems to be the assumption of equal speeds in a void. But why would the ratio between a body's weight and an equal volume of the medium's weight serve as a measure for the way the body's speed is affected, if this speed is not caused by the body's weight in the first place? How can Galileo justify this reappearance of weight as a dynamic factor after having discarded its role? The *De motu* explanation of the effect of a medium sits uncomfortable within the *Discorsi*.[662]

It is very improbable that Galileo would not have noticed the tension within his discussions in the first day. That he nevertheless extensively discusses this analysis of the medium's effect testifies to the fact that he must at least have been satisfied with its empirical plausibility. In introducing this analysis he moreover briefly touches on this problematic issue:

> If we then assume the principle that in a medium no resistance exists at all to speed of motion, whether because it is a void or for any other reason, so that the speeds of all moveables would be equal, we can very consistently assign the ratios of speeds of like and unlike moveables, in the same and in different filled (and therefore resistant) mediums. This we shall do by considering the extent to which the heaviness [*gravità*] of the medium detracts from the heaviness [*gravità*] of the moveable, which *heaviness is the instrument by which the moveable makes its way*, driving aside the

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[660] Cf. already chapter 6, section 6.2.3.

[661] Galileo nowhere explicitly differentiates between buoyancy and a medium's frictional effect. Nevertheless, Clavelin 1968, pp. 331-353 (especially pp. 342-343), has claimed that it is possible to discern a coherent distinction between these effects in Galileo's treatment of them. In the present paper, I will not try to decide the hard question whether this is truly possible and justified (which would also involve a careful reading of the treatment of the effect of a medium as it is presented in the *postils to Rocco*). I will rather take a necessary first step towards a satisfactory answer: to ascertain the different status (with respect to the earlier *De motu* treatment) that attaches to the buoyancy effect after Galileo reached his new understanding of weight as a dynamic factor through rethinking the thought experiment.

[662] The tension was already eloquently summarized by Dijksterhuis 1922, p. 233: "by subtracting the upward pressure, the effect of the medium has been taken into account; it is now as if we were again in a void, but with a lighter body. But this is supposed to fall as fast as the heavier…" (my translation).

> parts of the medium. *No such action occurs* in the void, and therefore no difference in speed is derived from different heaviness.[663]

The description is suggestive, but a little too cryptic to impute to Galileo a definite solution to the tension. Yet, if we remember how he made intelligible the non-operativeness of absolute weight in free fall, we can see how this is already constraining his attempts at such a solution. It is entirely coherent to assume that weight again becomes operative at the moment that a body encounters a medium in its fall, since the parts of the medium are at rest and as a result truly resistive – *there is something for the body at which it can weigh down*.[664]

The remaining puzzle resides in the fact that this should have an effect at the body's speed. This does suggest that whatever it is that is operative in giving a body its downward motion, it is somehow intimately related to weight without being identical with it. Either it is giving a body its downward motion (and in such a way that all bodies receive the same speeds), or it is giving it weight by which it can push aside the parts of the medium. In the latter case, the fact that it gives the body weight also implies that it gives the body less of its downward motion.

> The medium … opposes that transverse motion now with less, and now with greater resistance, according as it must be slowly or swiftly opened to give passage to the moveable … . This means some retardation and diminuation in the acquisition of new degrees of speed… .[665]

Apparently, a body can also be at rest and in motion at the same time. The distinction between moving bodies and bodies at rest is only an absolute distinction when we neglect the presence of a resisting medium. But this latter conclusion remains unsaid in the *Discorsi*.

8.1.4 Understanding the accelerated character of free fall

One striking fact about Galileo's presentation in the first day remains to be mentioned: the almost casual treatment of the accelerated character of free fall. It is true that when treating the frictional effect of a medium he cleverly exploits this acceleration, but the overall impression is undeniably that Galileo seems more concerned about the fact that in a void all bodies have the same speeds, i.e. that there is no direct correlation between (specific) weight and natural motion, than he is about the accelerated character of that motion.

[663] *Opere* VIII, p. 119. (Transl. from Galilei 1974, p. 78. My emphases.)

[664] Damerow *et al.* 2004, pp. 269-70, claim that Galileo simply takes over the older *De motu* theory with the addition of the proposition that in a vacuum all bodies fall with the same speed – implying that his dynamical thinking has remained basically unchanged in between the two treatises. It is clear that I cannot accept such a conclusion.

[665] *Opere* VIII, p. 119. (Transl. from Galilei 1974, p. 78.)

It is important to remind ourselves of the fact that from the beginning Galileo was presented with different challenges in his attempts at developing a new science of motion. As was already mentioned in chapter 4, Galileo's hydrostatic model seems to have room only for uniform motions. The tension is created by the idea that causes and effects must be proportional.[666] That a body's speed changes during natural motion, whereas its weight remains constant, further complicated Galileo's attempt at understanding weight as a dynamic factor; i.e. it is another fact that sits very uneasily with his original model of intelligibility.

Of course, by the time of the *Discorsi,* Galileo had abandoned his hydrostatic model for free fall, and he was strongly convinced of the fact that acceleration was an essential characteristic of natural motion. This conviction seems to have been mainly the result of the discovery that he could give an exact mathematical description of this acceleration.[667] Winifred Wisan and Paolo Galluzzi have shown how Galileo at first tried to come to grips with this acceleration through the exploitation of his understanding of motion on an inclined plane.[668] Such an attempt had appeared destined to fail however, because it seemed that it could not accommodate the fact that this acceleration should be independent of weight.[669] By the time of the *Discorsi,* he had not come up with a satisfactory understanding of acceleration, and it seems to be accepted there without further ado as a basis fact of nature:

> A heavy body has from nature an intrinsic principle of moving toward the common center of heavy objects (that is, of our terrestrial globe) with a continually accelerated movement, and always equally accelerated, so that in equal times there are added equal new momenta and degrees of speed.[670]

In the introductory discussions of the third day, Galileo moreover has Salviati famously declare that "for the present, it suffices our Author that we understand him to want us to investigate and demonstrate some attributes of a motion so accelerated (whatever be the cause of its acceleration)…"[671] That Galileo truly saw this only valid "for the present," and remained concerned about providing causal analyses of natural phenomena – albeit having changed the criteria about what counts as a successful analysis – will become clear when in sections 8.2 and 8.3 we discuss some fragments that postdate the *Discorsi* and where Galileo explicitly engages in such causal analysis. This is further proven by the fact that Galileo at times also tried to indicate that his constant acceleration (at least) didn't have to be in contradiction with the proportionality between cause and effect.

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[666] Which is a crucial part of Galileo's causal analyses, as we have seen in chapter 5, section 5.4.1.

[667] Cf. chapter 6, section 6.1.4.

[668] Wisan 1974, pp. 222-229; Galluzzi 1978, especially chapter 4 of the second part.

[669] Cf. *infra* section 8.3, to see how Galileo returned to this line of research after having completed the *Discorsi.*

[670] *Opere* VIII, p. 118. (Transl. from Galilei 1974, p. 77.)

[671] *Opere* VIII, p. 202. (Transl. from Galilei 1974, p. 159.)

> When I consider that a stone, which falls from some height starting from rest, constantly acquires new increments of velocity, why should I not believe that these additions are made in the simplest and easiest manner of all? The moveable remains the same, as does the principle of motion. Why should the other factors not remain equally constant? You will say: the velocity then remains the same. Not at all! The facts establish that the velocity is not constant, and that the motion is not uniform. *It is necessary then to place the identity*, or if you prefer the uniformity and simplicity, not in the velocity but *in the increments of the velocity*, that is, in the acceleration.[672]

The constant effect shows in the acceleration, not in the velocity of natural motion. The constant cause somehow lies in the falling body and is connected with the effect as a "principle of motion". That the cause lies in the falling body irrevocably brings to mind the body's matter – certainly if we take into account Galileo's scorn for explanations through "occult" properties. And as we already have seen, at the beginning of his *Mecaniche* Galileo defined a body's weight to be caused by its matter.[673] Again, there seems to be "something" about the body that is both responsible for its natural motion downward and for its weight – but as the thought experiment has by now taught, without being simply identifiable with the latter.

Both the accelerated character of free fall and the interaction with a dense medium inevitably bring a question to the fore that was left unanswered by the thought experiment: granted that it is intelligible that natural motion is not determined by weight, it is only natural to further inquire into what it is that *does* determine its character. At first sight the thought experiment could not offer any further help on this score. It had anyway always been presented without taking into account acceleration. Yet in the next section we will see how it played a role in Galileo's final efforts, in which he came close to a satisfactory understanding of the phenomenon of free fall.

8.2 *Intorno a due nuove scienze*: The thought experiment after the *Discorsi*

8.2.1 Measuring the force of percussion

At the closing sections of the *Discorsi,* Galileo repeats a promise which he had already expressed earlier in the fourth day: that he will also discuss the phenomenon of percussion, by which a moving weight exerts a much greater power on any resistance than does a body which is merely weighing down.[674] On this topic Galileo admits, through the intermediary voice of Salviati, to have "long remained in … shadows", and only "after he had spent thousands of hours during his life in

[672] *Opere* II, p. 262. (Transl. from Westfall 1971, p. 5. My emphases.) This is a passage from a first draft of the third day of the *Discorsi*, which is commonly dated around 1609.

[673] *Opere* II, p. 159. Cf. chapter 7, section 7.4.1.

[674] *Opere* VIII, pp. 292-293, 312-313.

theorizing and philosophizing about this, he had arrived at some ideas very distant from our first conceptions".[675] Galileo however was never able to complete the projected fifth day of the *Discorsi* in which he would live up to that promise, but among his manuscripts are contained a dialogue which was intended to that end as well as some further notes on the topic.[676]

That these attempts to come to grips with the phenomenon of percussion in part postdate the publication of the *Discorsi* implies that Galileo could tackle the problem starting from the dynamical insights which he had already reached within the *postils to Rocco* and the first day of the *Discorsi*. As we will see, by bringing together the problems treated in these works with the problem of percussion, he was able to come very close to a more or less satisfactory solution to the remaining puzzles within his understanding of free fall. These leads would afterwards be further taken up by Evangelista Torricelli, who had assisted Galileo in the final months of his life. In the first two subsections, I will first offer a summary of an important conclusion that Galileo reaches in his notes on percussion and then provide a new interpretation of how this conclusion became integrated in Galileo's attempts at developing a satisfactory dynamics for free fall. In the third subsection, I will finally show how we can see the thought experiment still driving these very last investigations undertaken by Galileo.[677]

The first traces of Galileo's involvement with the problem of percussion date from the time of *Le mecaniche*. When he comes back to the problem at the end of his life, he still tries to subsume it under his analysis of the mechanical machines. This implies that he tries to understand the mechanism by which the force of the weight of the body is multiplied so that it can give rise to potentially useful effects by means of the concept of (mechanical) moment. It will be remembered that a body's moment expresses its tendency for downward motion, and that it arises from its heaviness combined with either its relative position (with respect to the fulcrum of a lever), or with its velocity.[678] It is clear that in the case of percussion velocity will be the relevant parameter.

The main part of the dialogue on percussion consists in the exposition of several possible ways of measuring the moment of percussion of a falling body. The recurring theme during these discussions is the *infinity* of this moment. One proposal is to take as measure the static weight that drives a pole as far in the ground as does the blow of a percussant body. Galileo explicitly uses the term "dead weight" for this measuring body which operates through its heaviness alone. The problem with this proposal is that the measure is dependent on the resistance of the pole – the more resistant it

[675] *Opere* VIII, p. 293. (Transl. from Galilei 1974, p. 242.)

[676] See Drake 1978, chapters 20 and 21, for the historical circumstances surrounding both the announcement and non-delivery of the fifth day.

[677] Galileo's theory of percussion has not received much attention in the literature. Yet both Westfall 1971, chapter 1, and Galluzzi 1979, chapter 7 of the second part, contain very useful and insightful discussions, as do Moscovici 1967 and de Gandt 1987 who both also discuss Torricelli's exposition of this theory. Torricelli might provide an important link between these latest thoughts of Galileo and the further development of seventeenth-century mechanics, but this topic falls outside the scope of the present thesis.

[678] Cf. chapter 7, section 7.4.1.

is, the proportionally heavier the dead weight must be to have the same effect as the falling body. Yet, although this procedure is not appropriate as a uniform measure for the moment of percussion, it already teaches Galileo something important. If a body has fallen on the pole and driven it a certain distance in the ground, and if we then let it fall again on the pole, its second blow will drive it still further in the ground (although a smaller distance). The same is obviously not true of the dead weight: it operates by pressing, which effect can not be accumulated once the pole has been driven a certain distance. No matter how long it will lie on top of the pole, its effect is already completely exhausted. This implies that the effects of percussion and of a dead weight are truly incomparable. Any resistance which is not infinite will always give way to a blow of a percussant body, which thus can be said to have an infinite moment.

Another proposal to measure the moment of percussion is to use a system consisting of two weights connected by a rope over a pulley, one weight lying on an inclined plane, the other hanging freely along the vertical side of the plane. By letting the free body fall over a certain distance until it pulls the other body through the rope, the moment of its percussion can be measured by determining the distance over which the resisting weight is moved on the inclined plane. The necessary conclusion is again that any weight will be lifted by a falling body, since the counterweight is initially at rest, and thus has a moment which is zero compared to that of the moving body.

Both instances make clear that the infinity of the moment of percussion is actually the result of the incommensurability of the effect of a falling body with the effect of a dead weight. This incommensurability can be understood by considering the role played by time. As was already clear from the case with the dead weight pressing on the pole, the effect of its moment is exhausted in a single instant. The same is obviously not true of the falling body, which can accumulate its moments of gravity before actually hitting the pole.[679] In one of the fragments attached to the dialogue, we find the following summary of the situation by Galileo, where he discusses the differences between a body that presses against another and a body that strikes it:

> …the one that moves [a thing] by pressing without striking, and the other that acts by striking. The mover that operates without impact moves only a resistance which is less, though [it may be] only insensibly [less], than the power [*virtù*] of the pressing heaviness; but that will move it through an infinite distance, accompanying it always with its same force. That which moves by striking moves any resistance, thought [this may be] immense; but [moves it only] through a limited distance.
>
> Hence I consider these two propositions true: that the percussent moves an infinite resistance through a finite and limited interval, while the pressing [force] moves a finite and limited

[679] The possibility of such accumulation is a belated consequence of Galileo's initial choice to conceptualize moment as the *combination* of the effects of weight and speed; i.e., moment is not merely a restriction that is placed on the effect of a constrained weight but something that adds to its effect. The former possibility could also have sufficed to make sense of the pseudo-Aristotelian proof of the law of the lever as given in *Le mechaniche*, but it would have excluded the possibility of including percussion as a mechanical effect.

resistance through an infinite interval; hence to the percussent, the interval is proportionable, and not the resistance, while to the pressing [force] the resistance, and not the interval [is proportionable]. These things make me doubt whether Sagredo's question has an answer, as one that seeks to equate things that are incommensurable; for such I believe are the actions of percussion and of pressing.[680]

Hence, time is a potential measure for the moment of percussion, but (static) weight is not, whereas weight is a measure for the moment exercised by gravity alone, but time is not.

8.2.2 Moment of gravity and acceleration

The intimate relationship between moment of percussion and time is a conclusion of potentially great moment.[681] Galileo in his definition of naturally accelerated motion had already proclaimed that since "the closest affinity holds between time and motion," the uniformity of acceleration had to be understood as the fact that "in any equal times, equal additions of swiftness are added on."[682] Obviously, Galileo also reflected on the relationship between his analysis of the moment of percussion and his work on naturally accelerated motion. In another note appended to the dialogue on percussion, we find the following passage:

> The moment of a body in the act of percussion is nothing but a composite and aggregate of infinitely many momenta, each of them equal only to a single moment, either internal and natural *per se*, as is that moment of its own absolute weight [*gravità assoluta*] which it eternally exercises when placed on any resistant body, or else extrinsic and violent, as is that moment of the moving power. Such momenta go accumulating during the time of motion of the heavy body from instant to instant with equal increments, and are stored therein, in exactly the way that the speed of a falling body goes increasing; for as in the infinitely many instants of a time, however short, a heavy body goes ever passing through new and equal degrees of speed, always retaining those acquired in the previously elapsed time, so also in the moveable those momenta (either natural or violent, conferred on it by nature or by art) go conserving themselves and compounding from instant to instant, etc.[683]

As has been stressed by Paolo Galluzzi, Galileo refrains here from explicitly stating that we are dealing with a direct causal relationship between the accumulation of the momenta (which must be here understood as momenta of gravity, as indicated by Galileo himself) and the acceleration of the motion.[684] He "merely" points out a striking analogy between both phenomena. According to Galluzzi

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[680] *Opere* VIII, p. 343. (Transl. from Galilei 1974, pp. 303-304.)

[681] Despite my different conclusion, for the following I am much indebted to the discussions in Galluzzi 1979.

[682] *Opere* VIII, pp. 197-198. (Transl. from Galilei 1974, p. 154.)

[683] *Opere* VIII, p. 344. (Transl. from Galilei 1974, p. 304.)

[684] Galluzzi 1979, p. 403.

this must be attributed to the independence of acceleration from weight – how could this fact have possible been squared with such a causal relationship?

Since the notes we are discussing here are among the last of Galileo's life, it is possible that he had no time left to think this problem through, and was forced to end with the cautionary tone that is discerned by Galluzzi. Given his earlier analyses of the thought experiment, he nevertheless had all the elements at his disposition to come up with a solution. It was already concluded there that adding extra matter does not press on a falling body, and that therefore no extra speeds are added – although such a body with greater gravity will have to "tend more downwards". Seeing the thought experimental situation through the mechanical conceptual apparatus which Galileo is exploiting in his analysis of percussion, it is clear that this extra matter *does* add moment of gravity. This extra moment will then also be accumulated during the time of fall. And it is indeed undeniable that a heavier body will have a greater moment of percussion at the time it meets a resistance. What remains is the question why the greater moment of gravity has its effect in a greater percussion, but not in a greater increment of speed. That Galileo knew how to understand this, is evidenced by the following fragment, again from the notes appended to the dialogue on percussion. I will quote a long part, to give a taste of Galileo's knack of extracting physical insight from everyday phenomena.

> He who shuts the bronze door of San Giovanni will try in vain to close them with one single push; but with a continual impulse he goes impressing on that very heavy movable body such a force [*forza*] that when it comes to strike and knock against the jamb, it makes the whole church tremble. From this one sees how there is impressed in moveables – and the more, the heavier [*più gravi*] these are – and how there is multiplied and conserved in them the force [*forza*] that has been communicated to them over some time.
>
> A similar effect is seen in a great bell, which is not set in strong and impetuous motion with a single pull of its rope, nor with four, or six [pulls], but [is] with a great many. These being long repeated, the final [pulls] add force [*forza*] to that acquired from the preceding pulls; and the thicker and the heavier [*grave*] the bell shall be, the more force [*forza*] and impetus it acquires, this being communicated to it in a longer time and by a larger number of pulls than are required for a small bell, into which impetus is readily put, but from which it is also readily taken away, this [small bell] not drinking in, so to speak, as much force [*forza*] as the larger one.[685]

If we are allowed to translate this insight to the case of falling bodies, we finally reach a completely coherent understanding of the phenomenon of free fall. The body's gravity is continually pulling/pushing the body down, adding increments of speeds, yet the heavier the body, the stronger the

[685] *Opere* VIII, pp. 345-346. (Transl. from Galilei 1974, pp. 305-306.) A comparable passage, also dealing with the sounding of a great bell is found in the first day of the *Discorsi* (*Opere* VIII, p. 141). The example of the bell actually goes back to *De motu*, where it was used to argue that it is not mysterious that a motive quality can be imparted into a body where it resides for some time.

pulls/pushes shall have to be. More matter adds more moment, but not more speed, since now there is also more matter that must be put in motion. Are we allowed to translate this insight? I would urge that Galileo was moving towards a position in which this made perfect sense. Have another look at the previously cited fragment in which the analogy between the accumulation of momenta of gravity and increments of speeds was expounded, and notice how Galileo is clear on the fact that it is indifferent whether these momenta are natural or violent. This reading is further confirmed by another fragment which was dictated by the by then blind Galileo's to Viviani in which he compares the action of gravity in natural motion with the wind which moves a boat.[686]

Further indirect proof is provided by the fact that Galileo was not as reluctant as suggested by Galluzzi to consider the continuous action of the momenta of gravity as the cause of the acceleration in free fall. On introducing the system with the two connected bodies on an inclined plane as a way to measure the moment of percussion, Galileo also considers a special case: what happens if the bodies have the same weight? The body moving along the vertical is in free fall until it snaps the cord. At this point the weights of both bodies cancel out, and the combined system has a speed conferred to it by the moment of percussion of the first body. Given that there now is equilibrium of forces, this speed will be equably conserved. Significantly, Galileo himself explicitly likens this situation to what happens on a perfectly horizontal plane.[687] Even more suggestive, he then adds the following explanation for this situation, linking it with the acceleration which gave the percussion its moment:

> Now it is evident that this degree of speed will not go on increasing when *its cause [*cagione*] of increase* is taken away, this *being the weight [*gravità*] of the descending body itself*; for its weight [*gravità*] no longer acts when its propensity to descend is taken away by the repugnance to rising of its companion of equal weight [*peso*].[688]

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[686] *Opere* VIII, pp. 441-442. Galluzzi also cites this fragment as evidence for the fact that Galileo in the end came close to the kind of view just expounded in this paragraph. He also cites from writings of Torricelli and Baliani where a similar view is expressed. (Galluzzi 1979, pp. 323-326.) The example of the boat again has a long history. In his reply to the reaction of his Aristotelian opponents to his *Discourse* on floating bodies, which he wrote together with Benedetto Castelli, Galileo explains the effect of violent motion where the mover has the opportunity to stay in contact with the moved body by analogy of the wind that keeps on adding extra speed to the sailing ship. As noticed by Michele Camerota and Mario Helbing, Galileo borrows the example of the ship from the writings of the Jesuit Benito Pereira, who used it in a rather similar context in a book that was referred to by Galileo in his *De motu* (Camerota and Helbing 2000, p. 356). We have by now already noticed a few times how Galileo returns over and over again to the same simple analogies, but adds new layers of meaning to them through his evolving conceptual structures and physical insights. This offers a fascinating glimpse of the working of Galileo's imaginative reasoning. (In the earlier occurrences of the ship example, e.g., this is not yet linked with the natural acceleration of falling bodies, but on the contrary with the violent extrusion of a body lighter than the medium in which it is moving!)

[687] This confirms the analysis of Galileo's proto-inertial principle in chapter 6, see especially section 6.1.5.

[688] *Opere* VIII, p. 337. (Transl. from Galilei 1974, p. 297.)

A similar view is also contained in the fragment where Galileo compared the action of gravity with the wind blowing in the sail of a boat: in both cases the motive force acts to add extra speed on a body which is already in motion due to the earlier action of the force – the accelerated character is thus explained as the joint effect of a constant force and the conservation of motion, both linked with a uniform flow of time.

If we take all this together, the following picture emerges: at every instant of time the body's gravity gives rise to a moment of gravity, which in its turn gives the body a degree of speed – which will be independent of the particular strength of this moment. Both these momenta and degrees of speed are conserved during the next instants of time, respectively explaining the percussive effect and the natural acceleration. This also provides an alternative explanation for Galileo's reluctance about claiming a direct causal relationship between the accumulation of momenta of gravity and the degrees of speed, which merely were said to increase in the same way. To claim such a direct causal relationship would indeed be too hasty, since this would not take into account the independent conservation of momenta and speeds – or to put it differently, *this would ignore the crucial role played by time*. Yet, this does not preclude that each individual moment is the cause of each individual degree of speed.

8.2.3 Weight, time, and acceleration

It is clear that the foregoing attribution of these ideas to Galileo is in part a reconstruction on the basis of what may seem rather scant information. The main reason for doing so lies in the intimate link of these ideas with the lessons learned from the thought experiment. Without taking the latter into account as a natural source for the further development of Galileo's dynamical thinking, these latest ideas might indeed appear as a loose set of fragmentary insights.[689]

There are a few places where we can clearly detect the influence of the way Galileo rethought his thought experiment in his attempts to ascertain the moment of percussion. In at least two passages in his dialogue on percussion, there is a direct return to the analysis of weight that he had attempted in his *postils to Rocco*. At one point he describes an attempt to measure percussion involving a balance with at one end a counterweight and at the other end a bucket filled with water, under which was hung another empty bucket. The upper bucket was then pierced with a hole, and the idea was that the percussive effect of the water could then be ascertained through the extra counterweight that had to be added. Yet a complication arises because the water, while it is in the air in between both buckets,

> does not weigh [*non gravita*] at all against either upper or lower bucket. Not against the upper, for the parts of water are not attached together, so they cannot exert force [*far forza*] and draw down on those above, as would some viscous liquid, such as pitch or lime, for example. Nor [does it

[689] As implied by Westfall 1971, p. 39; Wisan 1984, p. 286.

weigh] against the lower [bucket], because the falling water goes with continually accelerated motion, so its upper parts cannot weigh down [*gravitare*] or press against its lower ones. Hence it follows that all the water contained in the jet is as if it were not in the balance.[690]

It is noteworthy that by now Galileo explicitly stresses that it is the relative acceleration that is of importance rather than the speeds, a fact which was not mentioned in the *postils* (where acceleration remained completely out of the picture – although Galileo consciously seems to have left ample room for its introduction by always using "degrees of speed"). In a second passage Galileo repeats the example of the ball and hand moving down with the same speed.[691]

However, the effect of the thought experiment is much more pervasive: it does not just provide a few striking examples, *it offers a new way of thinking about weight itself*. The use of active language is conspicuous throughout the notes on percussion. Galileo continually speaks about a body exerting its gravity ("essercitasse sua gravita"[692]) and about the operation of its gravity ("operando colla gravità"[693]). This is obviously linked with his central goal, i.e. measuring the effect of percussion. But is seems that he had now found a way of moving ahead towards this goal,[694] precisely *because he had realized that he had to conceptualize a body's dead weight ("peso") as an effect as well*. This moreover immediately paved the way for a reintegration of this weight in Galileo's still developing dynamical scheme which he is exploring in these notes on percussion. It was only a small conceptual step from the realization that the measurement of weight is only possible if there is a continually re-acting force to the point where we find Galileo explicitly speaking of a body's "moment of its own absolute weight [*gravità assoluta*] which it eternally exercises when placed on any resistant body".[695]

The thought experiment thus had provided Galileo with the necessary basis to conceive of a body's weight peculiar *non-relation with time*. Every body has gravity, which at every moment of time generates a moment of gravity. Either this moment of gravity is opposed by a resisting force which arises because the body presses which its moment on another body, or a degree of speed is

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[690] *Opere* VIII, pp. 324-325. (Transl. from Galilei 1974, p. 285.)

[691] *Opere* VIII, p. 331.

[692] *Opere* VIII, p. 325.

[693] *Opere* VIII, p. 325.

[694] As already indicated, there is short section in *Le mecaniche* which deals with percussion, but in it Galileo did not reach any interesting results. Torricelli also describes some early experiments of Galileo, which he did in Padua, but which again were unable to lead to any unambiguous conclusions (cf. Moscovici 1967, pp. 433-435). We will see in section 8.3.1 how Galileo in 1639 wrote to Baliani that he had finally been able to reduce the force of percussion to a very easy explanation. The fact that all these developments in Galileo's thinking postdate the *Discorsi* argues for the fact that rethinking the thought experiment was momentous for Galileo, and that he accordingly had not started doing this at the time of De motu. This is an important argument for the more cautious conclusion that I drew in the postscript to chapter 7.

[695] *Opere* VIII, p. 344. (Transl. from Galilei 1974, p. 304.)

generated.[696] If the resisting body remained in place, because it is somehow fixed, all the continuously arising momenta of gravity will in their turn be continuously annihilated. If the body is not opposed at all, the continuously arising momenta will cause a universal uniformly accelerated motion as explained in the previous subsection.[697]

Paolo Galluzzi, in his study of the concept of moment in Galileo, has stressed the polysemic nature of the term.[698] It could refer to an infinitesimal quantity in general, to a body's tendency to motion, and to the more specific concept of mechanical moment. We can see how this provided Galileo with the needed latitude that finally opened up the promise of closure at a new level of abstraction. The multiple meanings of moment can be tied together in a single conceptual framework by paying attention to the physical significance of the parameter time. In *Le mecaniche,* time also played a role in conceptualizing the transformation of moment, but this role remained completely interchangeable with space, due to the fact that both sides of the machines are always operated in equal times. However, by thinking of percussion as also being a mechanical instrument, this constraint must be abandoned. Time suddenly gains in physical significance: it is not merely an explanandum, as with the isochrony of circular motion, but a causal factor in its own right. Galileo is learning to discern mathematical closure in non-constrained systems, thus widening the scope for his new science of nature. What he misses, though, is the right kind of mathematical apparatus that would have allowed him to really move ahead to exploit this closure. The infinitesimal characteristics of the flow of time remained quasi intractable.

8.3 New principles for a new science

8.3.1 Galileo's postulate and the gap in the *Discorsi*

The final presentation of Galileo's mathematical science of motion in the third and fourth days of the *Discorsi* was built upon the definition of uniform acceleration ("equal momenta of swiftness added in equal times") and a postulate that stipulated that "the degrees of speed acquired by the same moveable over different inclinations of planes are equal whenever the heights of those planes are

[696] The latter generation is an action without a reaction. This is another example of the limited nature of Galileo's action-reaction principle (see also *supra*, note appended in section 8.1.3). This is of course connected with Galileo's conceptualization of gravity as internal to a body, whereas Newton's gravitational force will have an unproblematic reaction in the attraction of the earth by the falling body. (It is somewhat imprecise to refer to the moment of gravity as a force internal to the body, since in its pressing and percussion it has an external action on other bodies – however when its effect is the addition of a degree of speed its action remains internal and devoid of reaction.)

[697] As is often the case with Galileo, he is not entirely consistent in his terminology, but one can see a fairly general attempt to use "peso" exclusively for what we would call static weight, and "gravita" for the underlying dynamical cause.

[698] Galluzzi 1979.

equal"[699] (cf. figure 8.1). This was one of the first results that Galileo had reached on the basis of his treatment of the inclined plane in *Le mecaniche.*[700] As has been shown by Winifred Wisan and Paolo Galluzzi, in the period before 1610 Galileo attempted to establish a dynamical explanation for the accelerated character of fall that was based upon his concept of mechanical moment.[701] The basic idea was that the change in moment (as defined in *Le mecaniche*) on differently inclined planes could somehow cause the different accelerations that bodies have on these planes. As explained by Wisan: "Thus, Galileo must be thinking here in terms of an increasing velocity which is, at each instant, proportional to an increasing momentum, while the latter is, in turn, generated by and in some sense proportional to, the static momentum, or the effective weight of the body."[702] This scheme makes acceleration dependent on a body's absolute weight, which explains its abandonment sometime after 1610. As a result of this change in mind Galileo no longer had the means to prove that the speeds of bodies falling along differently inclined planes of the same height would be equal. However, this fact was essential to the general structure of the mathematical science of motion that he had built up. There seemed to be no other option but to present it as a postulate, "un solo pricipio domanda e suppone vero."[703]

We have already seen how the ideal of a mixed science required that its basic principles would be evident and as a result could be conceded by all.[704] The presence of this *postulated* principle accordingly presents an important gap in the formal structure of Galileo's science of motion. He tries to compensate for this by introducing a clever experiment with a pendulum. The experiment establishes that a bob swinging on a pendulum will always have acquired an amount of momento in its downward swing that suffices to bring it back to its original height. This will also be true if we shorten the length of the cord at the moment it has reached its lowest point, as a result of which its upward path will be steeper (this is achieved by placing a nail perpendicularly under the cord's point of suspension; cf. figure 8.2). But if the bob would have started by swinging down along this steeper path, it would also have gone up to the same height (since the height of the swings is always the same for the same pendulum). The momenta acquired along different paths are thus the same as long as these paths have the same height. But, as Galileo notices, this experiment supposes circular paths, whereas the postulate is about inclined planes. His demonstration thus falls "little short of equality with necessary demonstration"[705] – he cannot render his principle evident and understood by all.

[699] *Opere* VIII, p. 205. (Transl. from Galilei 1974, p. 162.)

[700] Cf. Wisan 1974, p. 162.

[701] Wisan 1974, pp. 222-229; Galluzzi 1979, chapter 3 of the second part.

[702] Wisan 1974, p. 223.

[703] *Opere* VIII, p. 205.

[704] Cf. chapter 4, section 4.2.2; chapter 7, section 7.6.1. I already quoted Galileo's statement in the first day of the *Discorsi* that it is "the most admirable and estimable condition of the demonstrative sciences that they arise and flow from well-known principles, understood and conceded by all." *Opere* VIII, p. 131. (Transl. from Galilei 1974, p. 90.)

[705] *Opere* VIII, p. 205. (Transl. from Galilei 1974, p. 162.)

This gap implies that Galileo is confronted with the problem that it is not immediately clear whether his mathematical demonstrations are actually about the behaviour of physical bodies. It signals the lack of an evident principle that would allow him to connect physical events with mathematical explanations, as he had earlier done in *De motu* with the help of the balance. But as he was convinced by now, the balance could no longer play this role. In a letter Baliani, written in 1639, Galileo admits that the postulate constitutes the weak spot of his new science of motion. However, the seventy-five years old man (who had become completely blind by now) continues:

> Know, then, that after my having lost my sight, and consequently my faculty of going more deeply into propositions and demonstrations more profound than those last discovered and written by me, I [instead] spent the nocturnal hours ruminating on the first and simplest propositions, recording these in and arranging them in better form and evidence. Among these it occurred to me to demonstrate the said postulate in the manner you will in time see, if I shall have sufficient strength to improve and amplify what was written and published by me up to now about motion by adding some little speculations, and in particular those relating to the force of percussion, in the investigation of which I have consumed hundreds and thousands of hours, and have finally reduced this to very easy explanation, so that people can understand it in less than half an hour of time.[706]

Exactly one month later, Galileo proposes to send the completed demonstration to Baliani.[707] In a letter to Benedetto Castelli, written at the end of the same year, Galileo announces again that he has a demonstration for the postulate and tells that he intends to include it in further editions of his *Discorsi*.[708] The demonstration was for the first time published in the posthumous second edition from 1655.

8.3.2 The inclined plane revisited

It is clear from the letter to Baliani that Galileo was simultaneously thinking about a possible demonstration for his postulate and the problem of percussion. We have seen that in the latter context the relation between absolute weight and gravity as a dynamic cause was in the process of being significantly restructured. It is accordingly no surprise that the earlier mechanical proof of the postulate could find a new appeal in Galileo's mind.

The actual proof of the postulate is preceded by a lemma in which Galileo proves his inclined plane theorem, already established in *De motu* and *Le mecaniche*.[709] There are some clear marks that link this new version of the proof with the period after the *Discorsi*, such as the claim that along the

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[706] *Opere* XVIII, p. 78. (Transl. from Drake 1978, pp. 400-401.)

[707] *Opere* XVIII, p. 95.

[708] *Opere* XVIII, pp. 125-126.

[709] Cf. chapter 6, section 6.1.1 and 6.1.2.

horizontal a heavy body's impetus for descending is completely "extinguished". Another conspicuous difference with the earlier proof is the absence of the detour via the balance which was suspended above the inclined plane. Galileo instead immediately exploits what in *Le mecaniche* was only presented as a confirmation of the validity of the proof, not as an independent proof: the relationship between the *vertical* spaces which two connected bodies traverse when the first moves on the inclined plane and the second along the vertical side of the right-handed triangle formed by the inclined plane and a horizontal plane (cf. figure 8.3). He argues that in order for there to be equilibrium the body along the vertical side must be lighter than the other body in the same proportion as the vertical height is shorter than the length of the inclined plane, as this is always the inverse ratio of their respective (vertically measured) descent and ascent. Galileo himself explicates that this condition of equilibrium is "exactly as is demonstrated in all cases of mechanical movements" – i.e. that "when equilibrium (that is rest) is to prevail between two moveables, their speeds or their propensions to motion [*le loro propensioni al moto*] – that is, the spaces they would pass [*si passerebero*] in the same time – must be inverse to their weights [*gravita*]".[710] Galileo stresses the virtual aspect of the motions with much care. Notice moreover that strictly speaking both bodies move with the same speeds as they of necessity will have travelled over an equal path in an equal time. One must of course only consider the vertical space they would pass, since the moveable "exclusively exercises its resistance"[711] through that direction. It is not so much different physical speeds (which are equal) that change the moment of a body, but rather the relative direction with respect to the perpendicular of their actual path of motion. Galileo further comments that as a result of this, "the lesser weight [*peso*] …, which exercises its total moment in the vertical …, will be the precise measure of the partial moment that the greater weight [*peso*] exercises along the inclined plane"[712].

At first sight it might seem that Galileo is simply reversing to the old idea that the absolute weight of a body is somehow the dynamical cause of its natural motion, as he again is using (static) weight as a measure for a body's moment of descent.[713] But if we look back at his earlier treatises from the vantage point which had now been reached in his thinking on percussion, it is clear that these contained some ambiguities. (And this is exactly what Galileo claims to have been doing. He could not think out new abstruse geometrical derivations without actually seeing the diagrams. But his agility of mind seems to have been undiminished in all other respects.) To put it a little more precisely: because of the conceptual choices that had now unambiguously been made by Galileo, it becomes possible to discern different options that were compatible with the earlier treatments. *Weight can be a measure for*

[710] *Opere* VIII, p. 217. (Transl. from Galilei 1974, pp. 173.)

[711] *Ibid.*

[712] *Ibid.*

[713] Although most scholars express this doubt without really endorsing it, Dijksterhuis 1924, pp. 261-264, does not hesitate to claim that in this passage Galileo is still reasoning completely within an Aristotelian framework.

moment of descent without necessarily being its cause. One can also try to understand both as effects of a common cause: gravity.

This insight also opened up the leeway to understand why bodies of different weight would still undergo the same acceleration in natural motion. Since weight is only measurable for bodies at rest, it can at most be a measure for the tendency – the propension – to motion, but not for the actual motive effect of gravity. And if this motive effect is to arise, the body with all its bulk must first be put in motion. Weight and acceleration are simply distinct effects, related in a different way to their common cause. Galileo's raised awareness of the need to stress the virtual nature of the motions thus makes perfect sense.

Yet despite this severing of the link between weight and motion, Galileo in the demonstration of his postulate still measures the moment of gravity of a body on an inclined plane through the weight necessary to equilibrate the body. This need not have bothered him much, however. He is not so much interested in the differences (or similarities) between bodies of different weight along the same path, but rather in the differences between the same body when falling vertically and along an inclined plane of the same height. After all, the postulate states that "the degrees of speed acquired by *the same moveable* over different inclinations of planes are equal whenever the heights of those planes are equal."[714]

Since a body obviously does not change its bulk, the fact that it will exert a precisely measured smaller moment of moment along the inclined plane results in a different rate of acceleration: "Whatever the impetuses at the beginning [*nella prima mossa*], that proportionality will hold for the degrees of speeds gained during the same time, since both [impetuses and speeds] increase in the same ratio during the same time."[715] The demonstration of Galileo's postulate then exploits this measure for the reduction of the acceleration to show that the body will acquire the same speeds when falling vertically and obliquely along the same height. The specifics of this demonstration, which also depends on the times-squared relation, need not bother us here.

Most scholars discussing this proof have assumed, following Thomas Settle, that Galileo here simply dodges the issue of the independence of acceleration from weight by restricting the discussion to one body.[716] Now it is true that Galileo does not enter into the issue at all, probably because he did

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[714] *Opere* VIII, p. 205. (Transl. from Galilei 1974, p. 162; my emphases.)

[715] *Opere* VIII, p. 218. (Transl. from Galilei 1974, p. 174.)

[716] Settle 1966, p. 205: "Galileo argued from a constrained mechanical system to an open dynamical system, and it is not at all clear that he did so legitimately. If his analysis were valid, it would seem permissible to argue that bodies weighing differently, and therefore having different total moments, ought to fall (in the same time) distances proportional to their weight. … Now it is not clear whether at this point Galileo was consciously aware of these objections. But he had effectively taken care of them. In all the general discussions, and in all the theorems were problems of this sort could arise, Galileo always wrote of comparing the motions of one and the same ball, or two identical balls, moving on their respective planes." Cf. also Wisan 1974, p. 226; 1984, p. 286; Galluzzi 1979, p. 312-313, n. 5; de Gandt 1995, pp. 105-107. Dijksterhuis' resolutely negative evaluation was already cited in a footnote *supra*.

not want to complicate things further, or maybe because he was not entirely sure yet how to expound his new ideas. Still he might have felt pretty confident that he was in a position to handle it adequately. The theorem is obviously valid for *all* bodies since these always have the same degrees of speed added along the vertical. Moreover, this general validity crucially depends on the fact that the body *considered in the proof* remains the same.[717] The latter fact can also be interpreted as a sign of Galileo's sharp insight in the situation, rather than as an attempt to circumvent an insoluble problem. It is exactly because he had reintegrated weight in his new conceptual scheme that he could continue to exploit its properties – such as its apparent diminishment along an inclined plane.

8.3.3 Statics and dynamics

A mathematical science of nature should have some principles which constrain the mathematical relations in a physically sensible way. The geometrical framework that Galileo developed around the definition of uniform acceleration was constrained through his postulate, which however didn't seem to express anything basic about physical bodies. This is precisely what the dynamical proof on the basis of the inclined plane theorem had to offer. After all, it is based on the basic property that characterizes all simple machines, and which expresses an inviolable principle of nature. It can thus be claimed that the new demonstrations shows that the postulate expresses a property that all bodies have simply in virtue of the things they are.

Stillman Drake claimed that "what one thinks of his dynamic foundation for the science of kinematics will depend on individual taste."[718] This misrepresents the extent to which Galileo could have thought of his own endeavour as "kinematics." His science was about the motion of natural bodies, and all bodies have gravity. The latter fact should accordingly constrain what could be the physically true proportions characterizing these bodies' motions. This is already true about Galileo's earliest attempts in *De motu* and he would never let go of this ideal. After that he discovered that he could not use the balance to introduce the bodies' basic properties into his science of motion, he was left without a means to directly justify his basic principle that was grounded in his inclined plane theorem. But almost all his proportions were actually derived on its basis. There should accordingly also be something right about what he saw on the balance and the inclined plane.

The basic argument that I am making here is that Galileo had the right conceptual tools to argue from a constrained mechanical system to an open dynamical system (although not the mathematical apparatus that would allow him to really move ahead with it). When the constraint is cut, the body will use its moment of gravity, as was measured by a counterweight, to accelerate; and all bodies will do this according to the ratio of their effective weight to their absolute weight, which implies that the role of absolute weight cancels out.

[717] As we would now see it, this guarantees that the mass is equal and that forces thus can be used as direct measures for the accelerations.

[718] Drake 1978, p. 394.

The discovery that both weight and acceleration can be taken as two distinct effects of the underlying cause of gravity is then a momentous insight. By consciously separating the behaviour of heavy bodies constrained to remain at rest and bodies in free motion, Galileo effectively separates what we would call the domains of statics and dynamics. His treatment of these domains moreover shows some structural similarities with our classical understanding of them. Yet we should not loose sight of the essential differences between Galileo's understanding and a modern one. He might have separated what we can recognize as statics and dynamics, but he had "dynamicized" *all* motion. Even "inertial motion" is essentially an effect of a special kind of dynamical situations.[719] Paradoxically, Galileo who is often hailed as the father of modern kinematics, couldn't conceive of kinematics strictly speaking. Motion remained unthinkable for him in the absence of all forces.

On the other side of the historiographical spectrum, one could also recognize some traces of the medieval impetus theories in Galileo's independent conservation of the accumulated momenta responsible for the force of percussion.[720] Yet more important than what remains of the older views, is what has changed in the meantime. In his *De motu* explanation of the accidental acceleration of bodies in free fall, Galileo had already explicitly conceptualized the force which is impressed on a body by someone or something preventing its motion as an artificial lightness. We have also seen how in his notes on percussion Galileo still conceptualized artificially impressed momenta as commensurable to the internal and natural momenta of gravity. But by now the concept of moment has replaced the concept of heaviness/lightness. This has enabled Galileo to see static weight as an effect of something more fundamental. "Statics" is no longer the basis of all his thinking; it is only the special situation in which the natural momenta are opposed by a resisting force. Natural motion can be understood "dynamically" within its own right, with time appropriately being the determining factor that sets apart dynamics from statics. That it can be understood within its own right testifies to the fact that Galileo has by now found a way of offering new incontestable experiences which can anchor his explanatory scheme. Once the thought experiment has taught us to look at the world in the right way, the things themselves indeed show us that we should distinguish between bodies constrained to remain at rest and bodies in free motion.

It is important to see that Galileo not merely separates statics and dynamics. He integrates them within a broader conceptual frame built around the notion of moment of gravity. This makes it possible to reintroduce some of the principles based on what can now be thought of as statical considerations into his science of motion. This is what we see in his demonstration of the postulate. Moment of gravity is measured both by weight and by acceleration, but not simultaneously. This is why a body's gravity indeed constrains the mathematical proportions characterizing its motion in a way that is related to its weight. If Newton would have ever read the second version of the *Discorsi*

[719] Cf. chapter 6, section 6.1, especially 6.1.5.

[720] Cf. *supra* section 8.2.2.

(which he probably didn't), he could have recognized something profoundly right about Galileo's demonstration of his postulate. As we have seen, Newton went on to show how a suitable definition of the concepts of force and mass allowed one to reintroduce a general mathematical closure for non constrained systems; a closure that could be achieved exactly because of the interplay between a body's statical properties and its acceleration when impelled by an impressed force.

FIGURES TO CHAPTER 8

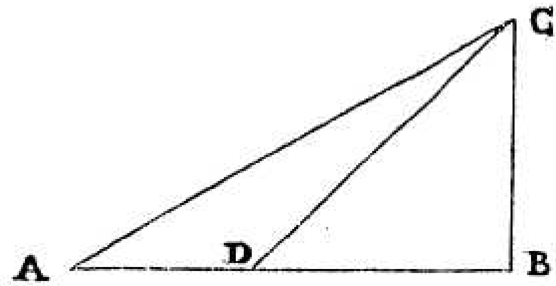

FIGURE 8.1

Galileo's postulate: bodies falling along *CB*, *CD*, and *CA* will have the same speed when arriving at the lowest point. (*Opere* VIII, p. 205.)

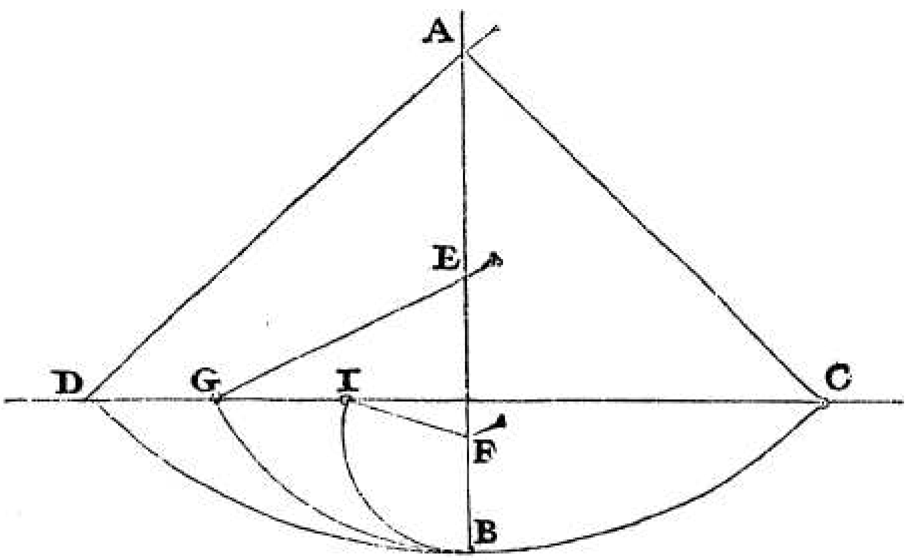

FIGURE 8.2

A bob is hung from a cord attached in the point *A* and is made to swing form *C* to *D*. If we fix a nail in the points *E* or *F*, it is seen that the bob will rise to the points *G* or *I*, which are situated at the same height as C and D. This implies that the moment acquired upon descending from arc *DB* is equal to the moment acquired along arcs GB and *IB*. (*Opere* VIII, p. 206.)

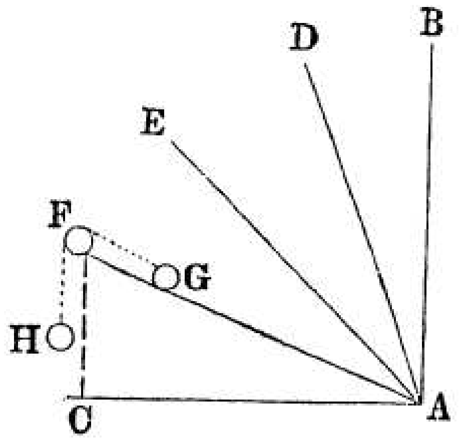

FIGURE 8.3

The proof of the inclined plane. The weight of body *H* will need to have the same ratio to the weight of *G* as the length *FC* has to *FA*, because the vertical distance traversed by *H* will equal *FA* and the one by *G* will equal *FC*. (*Opere* VIII, p. 215.)

9 Discoursing on a discipline

There is not only a liberation in the mathematical project, but also a new experience and formation of freedom itself, i.e., a binding with obligations which are self-imposed.[721]

In this concluding chapter, I return to some of the issues that were introduced in chapter 2. I will try to assess how Galileo is discursively positioning himself in some of his later writings, with a focus on the Dialogo*. We will see which elements he invokes to legitimize his mathematical science of nature. It will turn out that in doing so he refers obliquely to the* Quaestio de certitudine*, but only to deconstruct the discours in which this discussion was inscribed. A central element in his own strategies is the metaphor of the book of nature.*

My discursive analysis should allow us to come up with a more nuanced picture of Galileo's relation to late sixteenth-century Platonic thinking. As already indicated at the beginning of chapter 2, this issue has structured many of the twentieth-century debates on Galileo's science. I won't go into any of these debates, but the consequences of my analysis should be obvious for anyone familiar with them. However, my prime interest lies with the question how Galileo himself is attempting to stabilize the discursive field in which his new sciences are operating.

I feel a hesitation to add more comments on this chapter, which in many ways is the most tentative of the studies presented in this thesis. It feels like the beginning of my own rethinking of the previous chapters rather than as a closure.

[721] Heidegger 1967, p. 97.

9.1 The book of nature

9.1.1 Underwriting authority

That the book of nature is written in geometrical characters is one of the most famous images used by Galileo. It is obviously an important place for anyone interested in assessing how he tried to discursively position his mathematical approach to study nature. It is important to realize that it is a metaphor with a history, though. Not only was the image of the book of nature commonplace in Renaissance thinking, Galileo's appropriation of it also happened in a few consecutive steps.[722]

In a fist stage, Galileo used the image to ridicule Aristotelian philosophers who thought that they were studying nature, whereas they were only studying human books. In a letter written to Kepler in 1610, Galileo talks about his new discoveries with the telescope. He mocks the philosophers who tried to refute his observations by means of logical arguments, "as if they were magical incantations" that could make disappear what is truly in nature.[723] The message is clear: syllogistic logic is an instrument with which we can only clarify the relations that hold between different *texts*. But if we want to clarify the relations that hold in nature, another instrument of investigation is needed (remember that the telescope would have been considered a mathematical instrument). As we have seen in chapter 4, this is a rhetoric strategy that Galileo already uses in *De motu*, where he ridicules the philosophers' concern about interpreting Aristotle correctly rather than studying nature.[724]

A second stage arises when Galileo enters into his dispute with the churchmen over the right to speak on the true constitution of the universe. He tries to safeguard the legitimacy of his own position by presenting it as complementary to that of the theologians. Whereas the latter study the Holy Scripture, he studies *that other book* written by God, the book of nature. As he puts it in his famous letter to the Grand Duchess Catherina, written in 1615:

> I think that in discussions of physical problems [*problemi naturali*] we ought to begin not from the authority of scriptural passages, but from sense-experiences and necessary demonstrations; for the holy Bible and the phenomena of nature proceed alike from the divine Word, the former as the dictate of the Holy Ghost and the latter as the observant executrix of God's commands. It is necessary for the Bible, in order to be accommodated to the understanding of every man, to speak

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[722] For the pedigree of the image, see Garin 1961, Bono 1995. For the development of the image within Galileo's writings, see Biagioli 2003.

[723] "Putat enim hoc hominum genus [the philosophers opposed to him], philosophiam esse librum quendam velut Eneida et Odissea; *vera autem non in mundo aut in natura, sed in confrontatione textuum* (utor illorum verbis), *esse quaerenda*. Cur tecum diu ridere non possum? quos ederes cachinnos, Keplere humanissime, si audires, quae contra me, coram Magno Duce, Pisis a philosopho illius Gymnassii primario prolata fuerunt, *dum argumentis logicalibus, tanquam magicis praecantationibus*, novos planetas e caelo divellere et avocare contenderet?" *Opere* X, p. 423 (my emphases).

[724] Cf. chapter 4, section 4.1.2.

> many things which appear to differ from the absolute truth so far as the bare meaning of the words is concerned. But Nature, on the other hand, is inexorable and immutable; she never transgress the laws imposed on her, or cares a whit whether her abstruse reasons and methods of operations are understandable to men.[725]

Galileo thus for the first time compares his own undertaking as directed to the reading of a book, to position himself as *also* authorized to speak on the true constitution of nature. The important difference between the natural philosopher (Galilean style) and the theologian lies in the nature of their books. But exactly because they deal with different emanations of the same God, they can never be in a true opposition. The theologians must recover the message that God gave to men, and to that end they enter into a hermeneutical exercise, discovering the true meaning that is conveyed through Scripture. The natural philosopher must offer necessary demonstrations based on sense-experiences. The first Book is polysemous, the second unambiguous. But both are legitimate objects of study.[726]

A third stage occurs during the controversy on the comets. In 1623, Galileo returns to the image of the book of nature, but now in a context where he is positioning himself against the Aristotelian philosophers. In the *Assayer* Galileo responded to a treatise by the Jesuit Orazio Grassi, who had published his treatise under the pseudonym of Sarsi.[727] In the course of his arguments Galileo introduces the following famous passage:

> It seems to me that I discern in Sarsi a firm belief that in philosophizing it is essential to support oneself upon the opinion of some celebrated author, as if when our minds are not wedded to the reasoning of some other person they ought to remain completely sterile and barren. Possibly he thinks that philosophy is a book of fiction [*un libro e una fantasia*] by some writer, like the *Iliad* or *Orlando Furiosi* – books in which the least important thing is whether what is written in them is true. Well, Sig. Sarsi, that is not the way matters stand. Philosophy is written in this grand book – I mean the universe – which stands continually open to our gaze, but it cannot be understood unless one first learns to comprehend the language and interpret [*conoscer*] the characters in which it is written. It is written in the language of mathematics, and its characters are triangles, circles, and other geometrical figures, without which it is humanly impossible to understand a single word of it; without these, one wanders about in a dark labyrinth. Sarsi seems to think that our intellects should be enslaved to that of some other man … and that in the contemplation of the celestial motions one should adhere to somebody else.[728]

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[725] *Opere* V, p. 316. (Transl. from Galilei 1957, p. 182.)

[726] Biagioli 2003 offers much more detailed considerations of the ways in which this image is mobilized by Galileo in his struggles on Copernicanism.

[727] Biagioli 1993, chapter 5, contains a very perceptive analysis of the circumstances surrounding this dispute, and its sedimentation in the complex and hybrid text that is *The assayer*.

[728] *Opere* IV, p. 232. (Transl. from Drake and O'Malley 1960, pp. 183-184.)

Galileo is here mocking Grassi's defensive move in which the latter had asked "whom then should be followed?" as a reply to Galileo's (rather unwarranted) attack on his excessive reliance on the authority of Tycho Brahe.[729] But a very similar rhetorical ploy will reappear in the *Dialogue* where the authority in question is the usual suspect, Aristotle.[730]

Whereas Galileo earlier ridiculed the Aristotelians for their reliance on textual strategies, he now claims that he also is reading a book. But his book is of course incomparable to their human-made books. This discursive move allows him to underwrite the notion of truth that already was operative in *De motu*.[731] In that treatise, Galileo stressed that truth has the essential property that once noticed it cannot possibly be denied.[732] The image of philosophy as "written in this grand book – I mean the universe – which stands continually open to our gaze," but "written in the language of mathematics" now brings together the *transparency* of his mathematical method, which he opposed to the Aristotelians, with the *authority* of God's book that he borrowed from the theologians.

9.1.2 Reading the book of nature?

Galileo's image doesn't function without its aporias, though.[733] His geometrical book of nature is not written by human authors, but neither is it written for human readers. It primarily functions to assure the divine guarantee underwriting his notion of truth. The bible is written in a language that is already ours, which must allow us to understand the deeper message that it conveys. The book of nature, on the contrary, doesn't have an intended audience. This implies that Galileo simultaneously needs the transparency (as a divine guarantee) and has to claim that we do not immediately understand its language – we first have to learn to comprehend it. Moreover, as emphasized by Biagioli, in the same letter to the Grand Duchess in which he introduces the book of nature, Galileo also stresses the fallible and progressive character of philosophical knowledge.[734] Again, this seems to sit uncomfortably together with the implied transparency.

Notwithstanding this possible instability of Galileo's own metaphor, its intended meaning is transparent enough: if things in nature are to be seen as signs, they have no other than a literal meaning;

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[729] Drake and O'Malley 1960, pp. 71, 183.

[730] In the *Dialogue* Galileo has Simplicio even repeat Grassi's objection, but with Aristotle substituted for Tycho: "But if Aristotle is to be abandoned, whom shall we have for a guide in philosophy?" *Opere* VII, p. 138. (Transl. from Galilei 2001, p. 130.)

[731] Cf. chapter 4, section 4.3.1.

[732] Cf. again the following expressions: "[truth's] traces shine brightly in various place"; "the force of truth"; "if [the truth] had once been found by someone, immediately and without controversy, being what it is by its nature, it would have allowed itself to be seen and known by all"; "This objection is surely of great importance; but nevertheless it is not so powerful that it can obscure the splendor of the truth." *Opere* I, pp. 274, 284, 294, 335. (Transl. from Galilei 2000, pp. 25, 35, 45, 85.)

[733] Cf. Bono 1995, pp. 193-198; Biagioli 2003.

[734] Biagioli 2003, p. 576.

they don't signify something else, whether this would be a divine message or a philosophical system. All explanations will have to involve exclusively horizontal relations between things that are directly noticeable in the world. There is no place for relations of vertical signification, wherein things in the world would refer to a different level of reality.[735]

But this leaves open the question: what is it about this transparency that obstructs an easy reading? What can it mean *to come to learn* to recognize something that is transparent?

9.2 *Dialogo*: Platonic elements

9.2.1 Anamnesis

Halfway the second day of the *Dialogue concerning the two chief world systems,* there is a long argument in which Galileo tries to rebuke one of the strongest objections against the Copernican hypothesis: if the earth would really be whirling around its own axis at high speeds, then "rocks and animals would necessarily be thrown toward the stars, and buildings could not be attached to their foundations with cement so strong that they too would not suffer similar ruin."[736] The Aristotelian Simplicio seems to be particularly taken by this argument, and announces that "it will be a difficult thing to remove it or to unravel it."[737] This gives Salviati the opportunity to start playing his favourite game:

> The unravelling depends upon some data well known and believed by you just as much as me, but because they do not strike you, you do not see the solution. Without teaching them to you then, since you already know them, I shall cause you to resolve the objection by merely recalling them.[738]

This remark then elicits the following question from Simplicio:

> SIMP. I have frequently studied your manner of arguing, which gives me the impression that you lean toward Plato's opinion that *nostrum scire sit quoddam reminisci*. So please remove all questions for me by telling me your idea of this.
> SALV. How I feel about Plato's opinion I can indicate to you by means of words and also by deeds. In my previous arguments I have more than once explained myself with deeds. I shall pursue the same method in the matter at hand, which may then serve as an example, making it easier for you

[735] On the opposition between horizontal and vertical schemes of representation, see e.g. Hallyn 1987, pp. 20-21.

[736] *Opere* VII, p. 214. (Transl. from Galilei 2001, pp. 218-219.)

[737] *Opere* VII, p. 217. (Transl. from Galilei 2001, pp. 221.)

[738] *Ibid.*

> to comprehend my ideas about the acquisition of knowledge if there is time for them some other day, and if Sagredo will not be annoyed by our making such a digression.[739]

Salviatio thus proceeds with his deeds. To unravel the objection of the whirling earth, he proposes to investigate first what happens with a rock that is thrown after having moved along the arc of a circle in a notch of a stick. To this end he asks what would be the motion of the rock the moment it leaves the stick.

> SIM. Let me think a moment here, for I have not formed a picture of it in my mind.
> SALV. Listen to that, Sagredo; here is the *quoddam reminisci* in action, sure enough.[740]

Is this response "openly ironic"?[741] It is possible to read it thus, certainly if one is already convinced of the fact that Galileo's basic attitude towards Plato's philosophy is ironic (as Hatfield is). But I am not too sure that we should read this remark as something else than a sincere comment on Galileo's views on the acquisition of knowledge and the place therein of recollection.[742] To better see the importance of this question, let us take a closer look at Plato's *Meno*.

The dialogue between Socrates and Meno is aimed at (other than Socrates' wooing of the young man, of course) finding a satisfactory definition of virtue. During their discussions Meno hits upon the following paradox: you cannot search for what you don't know already, and you need not search for what you do know already. That you cannot search for what you don't know already is supposed to follow from the fact that otherwise you could not recognize it when you should hit upon it. So how is genuine learning then to occur?[743] Socrates' answer is that "the whole of searching and learning is recollection."[744]

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[739] *Opere* VII, p. 217. (Transl. from Galilei 2001, pp. 221-222.)

[740] *Opere* VII, p. 218. (Transl. from Galilei 2001, p. 222.)

[741] Hatfield 1990, p. 122. Despite my crucial disagreement on this fact, Hatfield's essay contains many valuable insights and arguments.

[742] My analysis of Galileo's appropriation of the doctrine of anamnesis owes a lot to Heidegger's short but penetrating treatment of "the modern mathematical science of nature" in his 1935-1936 lectures (which were mainly devoted to Kant's critique of pure reason). See Heidegger 1967.

[743] Some might be tempted to dismiss this as a pseudo-problem. This depends on how to construe the terms at issue, of course. But there is something profoundly right – or so is the basic idea behind most of the present thesis – about the insight that one at least needs to start with a general framework in which to categorize and understand possible answers before one is able to pick out one as the right answer. One can of course search for a pair of trousers without knowing where it is, but one must at least be able to recognize it as a pair of trousers when one sees it. It is the latter problem that is fundamental: Plato (as Aristotle after him; cf. already chapter 5, section 5.1.1) is engaged in trying to come to grips with the question what makes a thing the thing it is; e.g. a pair of trousers, or virtue. Of course, one could argue that this does not make the paradox less of a pseudo-problem, as the second premise seems to break down on this interpretation – one can be able to recognize something, without having found it already; hence, although you "know" the thing, you still have to search for it. This reading would turn the problem at hand into something profoundly boring instead of profoundly right. I would urge that we need to push

To clarify this view Socrates calls in a slave boy, ignorant of geometry, whom he will make "recall" a geometrical proposition that he ostensibly did not know: that the length of the side of a square double in size of a given square equals the length of the diagonal of this given square. To this end he draws figures in the sand and probes the boy with his incisive questioning. While doing this Socrates makes sure to check regularly with Meno to "pay attention as to which seems to you to be true of him, either that he is recollecting or that he is learning from me."[745] As he announces at another place, just before he starts a series of questions that will lead the boy to recognize his own earlier mistaken answer as faulty: "Well, observe him recollecting in sequence, as one ought to recollect."[746]; or as he might have put it as well: "Listen to that, Meno; here is the *quoddam reminisci* in action, sure enough."

Plato's distinction between teaching and recollecting boils down to the difference between accepting propositions on the authority of a teacher and making the crucial judgements for oneself. As a result, it hence doesn't really matter that the slave-boy's answers are mostly of a yes/no nature: he still decides himself whether he truly believes certain statements to be true or not. In the same vein, the figures drawn in the sand may be essential to awaken the boy's considered opinion on the matter, but in the end they are quite immaterial to the core of Socrates' "teaching". The latter is primarily and essentially aimed at bringing the boy to see for himself a host of logical relations that hold between his opinions, hence bringing him to knowledge.

> True opinions too are a very fine thing as so long as they stay in their place, and produce all sorts of good things; but they are not willing to stay in their place for a long time, but run away out of a man's soul, so they are not worth very much, *until someone ties them down by working out the explanation. This,* my friend Meno, *is recollection…*[747]

In this way we have at once laid bare the core of Galileo's own dialectical strategy in his *Dialogue.* Salviati makes Simplicio first recall "some data well known and believed by you just as much as me," but the most important step happens when through his probing questioning he can make his companion realize unexpected consequences by simply asking him to keep "in mind the propositions which you have told me, collect them all together, and tell me what you gather for

back the questioning until we reach the interesting reading: i.e. to the question how do you *first* come to know the thing – how do you manage to pick it out as "really" being the object of your study. *Either you have done this already, or you will never be able to do it.* Many of Kuhn's struggles in his *Structure of scientific revolutions* can be read as rehearsals of this same piece in a different key.

[744] *Meno* 81d. (Transl. from Plato 1985, p. 67.)

[745] *Meno* 82c. (Transl. from Plato 1985, p. 67.)

[746] *Meno* 82e. (Transl. from Plato 1985, p. 71.)

[747] *Meno* 98a. (Transl. from Plato 1985, p. 115. My emphases.)

them."[748] The prime effect is often to make Simplicio aware of the fact that what Feyerabend called "natural interpretations" are indeed interpretations after all.[749] I will not enter into the many subtleties of Galileo's argumentative strategies in this respect, however – I only want to point out something almost trivial: making Simplicio *recall* what he already knew almost invariably comes down to make him *think* about his own opinions![750]

Disappointingly trivial? That depends. Galileo shows himself a devilishly accurate reader of Plato's dialogue.[751] Consider the following exchange between Simplicio and Sagredo, following an argument concerning the motion of projectiles, in which Simplicio is challenged to express his opinion on the matter:

> SIM. I should say in the first place that I have not observed any such things; second, that I do not believe them; and then, in the third place, if you should assure me of them and show me proofs of them, that you would be a veritable demon.
> SAGR. One like Socrates's, though; not one from hell. But the showing depends on you; I say to you that if one does not know the truth by himself, it is impossible for anyone to make him know it. I can indeed point out things to you, things being neither true nor false; but as for the true – that is, the necessary; that which cannot possibly be otherwise – every man of ordinary intelligence either knows this by himself or it is impossible for him ever to know it. … Therefore I tell you that the causes in the present problem are known to you, but are perhaps not recognized as such.[752]

Similarly, while unravelling the argument concerning the whirling earth, the following dialogue takes place:

> SIMP. … I understand it completely in my own mind, but I do not know how to express it.
> SALV. I also see that you understand the thing itself, but lack the proper terms for expressing it. Now these I can indeed teach you; that is, I can teach you the words, but not the truths…[753]

Galileo's involvement with Meno's paradox is undeniable, and would not have been missed by any moderately schooled contemporary reader. His allusions to the Platonic theory of recollection run much deeper than being a mere rhetorical (and ironic) embellishment. By stressing that Simplicio

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[748] *Opere* VII, p. 219. (Transl. from Galilei 2001, p. 224.)

[749] Feyerabend 1980, pp. 73 ff.

[750] For analyses of the fine-structure of Galileo's arguments in the *Dialogue*, see Finocchiaro 1980, which also contains a negative assessment of Feyerabend's analysis of Galileo's arguments. Yet, he still acknowledges Feyerabend's insight in the status of these natural interpretations.

[751] Let me just mention that Plato's *Opera* were also part of Galileo's library.

[752] *Opere* VII, pp. 183-184. (Transl. from Galilei 2001, p. 183.)

[753] *Opere* VII, pp. 218. (Transl. from Galilei 2001, p. 223.) Compare with Socrates who can teach the slave-boy the name of the line that he picks out as being the line sought (*Meno* 85a) – but that it is indeed *this* line that was being sought is the boy's own judgement.

already knows the answers to the questions being discussed although he does not yet realize it, Galileo in the first place propagates a certain view on the nature of teaching.[754] It is best summed up in one other dialogue fragment:

> SIMP. I leave it to you to judge.
> SALV. Rather, I want you to be the judge.[755]

True teaching is at most a pointing out, a guidance; its essence consists in offering to others the possibility to learn themselves, rather than in the offering of true statements.[756] Only when someone comes to recognize the truth as such can he be said to have learned something. Only upon acquiring *the stability that necessarily turns all cognition in re-cognition* – i.e. as having already been always true, as "that which cannot be otherwise" – does knowledge arise from opinion. The moment one comes to know something it ceases to have been possible that one did not yet know it (Meno's paradox). That is why it is necessary that a pupil is "recover[ing] the knowledge *from* himself"[757] *for* himself. If not tied down thus, it would never acquire the needed stability. That Simplicio's cognitive acts in the *Dialogo* "involve something more than mere recollection"[758] only shows how diligent a pupil of Plato Galileo was.

Plato complemented his analysis in the *Meno* with a metaphysical theory aimed at further developing the concrete import of recollection in achieving true knowledge. Galileo remains completely silent on anything to do with Platonic forms. *He reads Plato as if the example with the slave-boy constitutes the complete answer to Meno's paradox*. That is, he puts all weight on the dialectical process and remains silent on the innate and ideal nature of the recollected knowledge. In that sense he is more Socratic than Platonic. But Plato himself still seems rather uncommitted on this issue in the *Meno*. The most important advantage that Socrates claims for his way of dealing with

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[754] That Simplicio already knows the answer by himself is announced almost before any single argument in the *Dialogo;* cf. next to the passages already quoted *Opere* VII, pp. 36, 48, 107, 113, 115 (a very clear allusion to the Meno), 162, 166, 171, 186, 194, 220, 222, 223, 276, 351ff, 359 (I might have missed some further places). This obviously plays an important rhetorical role, as admitted by Galileo himself, when he has Sagredo say: "since proceeding by interrogations seems to me to shed much light upon things, in addition to the pleasure one may get out of pumping one's companion and making things drop from his lips which he never knew that he knew, I shall make use of that artifice." *Opere* VII, p. 276. (Transl. from Galilei 2001, pp. 291-292.) Yet even here he simultaneously states that it "sheds much light upon things."

[755] *Opere* VII, p. 262. (Transl. from Galilei 2001, p. 273.)

[756] Let me add yet another fragment, only to show how much Galileo keeps on emphasizing this message throughout the *Dialogo*: "SIM. I know it, and Aristotle taught it to me. SAGR. Please tell me by what kind of proof. SIM. Proofs from the senses. SAGR. Then has Aristotle made you see what you would not have seen without him? Did he even lend you his eyes? You mean that Aristotle said it to you, made you notice it, reminded you of it; not that he taught it to you." *Opere* VII, p. 184. (Transl. from Galilei 2001, p. 184.) How right is Brian Vickers's remark that "one of the running motifs in the *Dialogo* might be called "The Education of an Aristotelian""! (Vickers 1983, p. 99.)

[757] *Meno* 85d. (Transl. from Plato 1985, p. 79. My emphasis.)

[758] Hatfield 1990, p. 123.

Meno's paradox is that it "makes men active and ready to search" whereas just accepting the dilemma ("that contentious argument") without further ado "would make us lazy", which is only "pleasant to hear for those men who are soft."[759] The insight that all true knowledge is recollection is hence taken as an incentive to inquiry by Plato. One can be in the position to truly know and still have to search. Remember that Plato spoke about the necessity of "recollecting in sequence". It is not question of "merely" recalling – the process in which one comes to anamnesis involves a *regulated* calling to mind. As a result one comes to better understand one's own opinions; one sees how one *should* be thinking about the topic at issue if being faithful to one's own ratio; the contents of one's mind are tied down by the mind itself.

So I propose: recollection is just another word for understanding. It involves an *act* of the mind in which it grasps something *in its invariance* – which it can only do by thinking it as mathematical.

9.2.2 Mathesis

It is no accident that Plato chooses a "mathematical" (i.e. geometrical) example to illustrate the nature of recollection while the dialogue is actually devoted to an analysis of virtue. Let us not forget that the Greek word for "learning" is *mathesis.*[760] Meno's paradox draws attention to the fact that not anything is learnable, but geometry provides a paradigmatic example that it is nevertheless possible to achieve objective knowledge. So what is special about this kind of knowledge?

Galileo had been trying to provide an answer since his earliest writings. We only need to recall the passage from his *De motu*, where he explicitly opposed the *teaching* of "his" mathematicians with that of the philosophers.[761] Given this early concern, it is no wonder that Plato's *Meno* could have made such an impression on him. One important extra element is added now: the dialectical context in which this judging takes place.[762] Salviati not only stresses that Simplicio already knows what he is about to "teach" him, he also adds that these things are "known and believed by you *just as much as me*" – Sagredo states to the same effect that "*any man of ordinary intelligence* either knows this by himself or it is impossible for him ever to know it". The results that will be reached in these investigations are hence objectively binding for anyone – by learning to think for himself, Simplicio is actually thinking for everybody.

[759] *Meno* 81d-e. (Transl. from Plato 1985, p. 67.)

[760] "Mathesis" is also the term used by Plato (cf. the Greek text in Plato 1985).

[761] Cf. chapter 4, section 4.1.2.

[762] "Dialectical" is here obviously intended in a broad sense, not tied to the Aristotelian discussions of the topic understood under that term.

There is no doubt that the use of the dialogue form was part of Galileo's tactic in dealing with the precarious situation in Rome.[763] But while this form might have been exploited (to no great success) to help convey the impression that the subject was discussed in Ciceronian fashion *in utramque partem*, it still is true that the arguments *against* the Copernican hypothesis stand refuted at the end of the dialogue. Galileo had moreover already used the dialogue form in his earliest writings, and continued to return to it throughout his career. But it is only in the *Dialogo* that he knows to exploit its intrinsic interest to its full effect.[764] By leading Simplicio through a Socratic questioning on a host of natural phenomena, Galileo actually helps the reader to internalize the proper discipline of mathematical reasoning.[765]

Let me illustrate this with a delightful example from an early work by Galileo, the *Dialogue of Cecco di Ronchitti*. In this wonderful satiric dialogue, published pseudonymously in 1605, Galileo discusses a book that was aimed at undermining his earlier public lectures on the 1604 nova.[766] The two protagonists are Matteo and Natale, both peasants. On the basis of their common sense they ridicule the conclusions reached by Academic Philosophers. Central in the controversy over the nova stood the position of the new star: under the moon or far up in the "perfect" heavens. Crucial (as always) was the absence of parallax, the book criticized by Galileo had claimed that these measurements were not applicable to the nova. To show how misguided this criticism is, Matteo instructs Natale on how to ascertain distances and lengths in a most certain way.[767] To this end he asks him to judge whether a poplar, standing by the river bank where the two friends are waiting for the evening, is higher than a willow. By walking around Natale quickly discovers that his answer will depend on the position from which is he is looking at them, especially when Matteo makes him climb in still another (and higher) tree. By actually having his protagonist moving around, skinning his knee by climbing walnut trees, Galileo makes him see and feel for himself what changes, *and consequently*

[763] For a short and recent treatment of Galileo's use of the dialogue form, see Spranzi 2004. See also Vickers 1983; Moss 1993.

[764] The relative worth of the dialogue form versus other modes of literary presentation was a topic of discussion in its own right during the Renaissance. It would be very interesting, to pursue this further in connection with the analysis of Galileo's use of it that I am presenting here. Let me just offer the following quote from a 16th century work devoted to this topic: "Je me demande s'il ne faudrait pas … dire que le dialogue est le père de toute doctrine véritable, puisqu'il nous montre le chemin qui, si on l'emprunte, nous permet d'aller plus facilement de ce que nous comprenons par l'opinion à ce que nous comprenons par la vue de l'esprit, et de ce qui est fondé sur la vraisemblance à ce qui est fondé sur la vérité." (Carlo Sigonio, quoted and translated in Spranzi 2004, pp. 38-39.)

[765] This move would certainly have been helped by the general humanist tendency to praise thinking for oneself over accepting propositions on authority.

[766] For Galileo's authorship of the dialogue, and for the circumstances surrounding its publication, see Drake's introduction to his translation of the work in Galilei 1976.

[767] The opening of the dialogue immediately sets the tone: "MAT. What is this fellow that wrote the book? Is he a land-surveyor? NAT. No, he is a Philosopher. MAT. A Philosopher, is he? What has philosophy got to do with measuring?" *Opere* II, p. 315. (Transl. from Galilei 1976, p. 38.)

also what remains invariant, under different conditions of observation. He lets the objective facts become "as plain as a cowshed."[768] Despite the presence of initially misleading sense impressions anybody *has* to agree that the willow is higher than the poplar. This conclusion can only be reached on the basis of these sense impressions controlled through mathematical reasoning.

In the *Dialogo*, Sagredo offers another beautiful illustration of this through an event (*un accidente*)…

> …from which (in complete agreement with what we are saying) one may learn how easily anyone may be deceived by simple appearances, or let us say by the impressions of one's senses. This event is the appearance of those who travel along a street by night of being followed by the moon, with steps equal to theirs, when they see it go gliding along the eaves of the roofs. There it looks to them just as would a cat really running along the tiles and putting them behind it; an appearance which, if reason did not intervene, would only too obviously deceive the senses.[769]

Remember Meno's paradox: you need to be in a position where you can recognize what you are presented with.

His own active involvement makes it possible for Natale to discern a stable kernel in the changing appearances of things. The experiences in which Salviati similarly wants Simplicio to anchor his own beliefs *do not function as premises in a syllogistic framework, but as the elements of an analysis of more complex phenomena*. This is what *mathesis* is about: to bring one to the position where one can recognize the underlying "objective" relations. And this involves asking Simplicio to imagine that he were in a boat, looking at the top of the mast moving together with him on the waves; or that he were shooting arrows from a riding carriage; or throwing hoops; etc. But just as Matteo not only had Natale climbing trees, but also kept asking questions about what he could learn by juxtaposing his judgements, so Simplicio is never left a moment of rest. Salviati and Sagredo pose as the land-surveyors of Simplicio's mindscape. They show him how to ascertain relations of implication between different judgements that he makes himself. And they show him that, as a result, there exists another mode of determining the nature of empirical facts. Just as Matteo stressed that parallax measurements remain valid even if the moon were made of polenta, so Salviati and Sagredo keep reminding Simplicio that these determinations can be reached completely independent from any considerations on the essential nature of things in the world.[770]

Galileo also exploits geometrical diagrams to make palpable the consequences of some of these structural relationships that he is teaching Simplicio to notice. But it is important to see that such a diagram only becomes a *model* because it is discursively embedded within the text where it is put to

[768] *Opere* II, 330. (Transl. from Galilei 1976, p. 48.)

[769] *Opere* VII, p. 281. (Transl. from Galilei 2001, pp. 297-298.)

[770] For the polenta: *Opere* II, p. 315.

use. It is only because Salviati is disciplining Simplicio's way of approaching the things that surround him that these diagrams can take on their particular sense.[771] We have already seen something similar at the end of chapter 6, where it was explained how mathematics can be used to represent facts about the world.[772] This depends as much on our way of engaging with the world as it does on the resources of mathematics itself.

To sum up: Galileo is illustrating under which conditions observational facts can be turned into evidence. Salviati is teaching Simplicio the *relevance* of facts already known by him. As a result, relationships that can be noticed to hold between things in the world are imbued with a new kind of significance – they can become ratios. And the dialogue format allows him to display the ways in which we have to actively search for these conditions. Once Simplicio lets his thinking be guided by Salviati's questioning, he is forced to leave behind his beloved peripatetic framework. With a sure hand Galileo leads the investigations towards the properties which enable him to construct a completely different way of approaching these experiences.[773]

In the foregoing chapters we have seen with sufficient detail what this kind of approach consists in. I won't repeat these analyses here. Let me, in closing, only stress that this shows that Galileo's references to the method of anamnesis do not serve as a concealment of the true sources of his revolutionary moves, as Feyerabend maintains;[774] on the contrary, they highlight them. Anamnesis is no passive quasi-mystical process, but it is the result of a search process aimed at uncovering invariancies that anyone can notice.

9.3 Mathesis versus methexis

9.3.1 Stabilizing the metaphor

We left section 9.1 with the question what it could mean to come to learn to recognize something that is already transparent, as Galileo's book of nature supposedly is. We can now see that this seeming aporia is actually a version of Meno's paradox. We first need to be in the right position to

[771] Cf. e.g. the argument on the whirling earth. *After* that Simplicio answered Salviati's questions on the projection of heavy things from a swinging sling, Salviati answers Sagredo's doubts on a particular point as follows: "The objection does you credit, Sagredo, and in order to shed light on it so that we can more clearly comprehend it … let us define it by reducing it to a diagram, which will perhaps also bring it more easily to a solution." *Opere* VII, p. 225. (Transl. from Galilei 2001, p. 231.) The diagram is then constructed on basis of the properties which Simplicio already was made to notice as a result of Salviati's probing questions.

[772] Cf. chapter 6, section 6.3.2.

[773] As stated by Sagredo, referring to Salviati's "teaching": "I feel myself being gently led by the hand; and although I find no obstacles in the road, yet like the blind I do not see where my guide is leading me, nor have I any means of guessing where such a journey must end." *Opere* VII, p. 472. (Transl. from Galilei 2001, p. 518.)

[774] Feyerabend 1980, p. 81.

recognize what we are presented with; a right position that has to be actively constructed. The link with the Meno allows Galileo to bring in the Platonic anamnesis as a discursive element and this allows him to stabilize his own metaphor.[775] We have already noticed how anamnesis is severed from the Platonic theory of forms in Galileo's text. This is why he had Salviati stress that he would illustrate his views on the acquisition of knowledge with deeds rather than by means of words. The recollection lies in the active process itself (it is a collecting that is of such nature that it can be repeated at any moment by anyone), not in a passive remembrance. The transparency of nature must be ascribed to the fact that anyone can always bring himself in the position to recognize what he sees.

The instability that threatened Galileo's metaphor can be undone by abandoning a strictly visual understanding of its message. At the end of the first day of the *Dialogo,* Galileo distinguishes two ways to understand the human understanding: *intensively* and *extensively*. The latter mode is related to the number of propositions understood, the former to the perfection with which a proposition is understood. Salviati claims that taken extensively, humans know very little, but that taken intensively, the human intellect "equals the Divine in objective certainty"[776] when it comes to the mathematical sciences. Simplicio is shocked but Salviati disagrees that this would detract "from the majesty of Divine wisdom."[777] Thereupon follows an important clarification: when it comes to mathematical truths, "our method proceeds with reasoning by steps from one conclusion to another [*procede con discorsi e con passage di conclusione in conclusione*], while His is one of simple intuition."[778] God sees a circle and in one glance understands what he sees; we have to proceed by reasoning from property to property. When confronted with simple figures such as circles and triangles, it is not true for us that "everything is always present."[779] Even when it comes to pure mathematics, we are never in a position to grasp immediately what we are confronted with. But what we come to know, we know as perfect as God knows it.

Again, we see the same double movement which involves both appropriating divine authority, while simultaneously pushing it away as a far removed, but ultimately attainable limit point. In Galileo's science, mathematics has become a universal instrument to solve local problems. His discourse on his own discipline could not but mirror this bivalence.

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[775] The metaphor recurs in the dedication to the *Dialogo*. When defining what it means to be philosopher, Galileo claims that "the great book of nature … is the proper object of philosophy" (*Opere* VII, p. 27; transl. from Galilei 2001, p. 3). We have already seen a few times how these dedications were crucial in the authors' discursive positioning. The metaphor thus occupies a truly central role in Galileo's discursive strategies in the *Dialogo*.

[776] *Opere* VII, p. 129. (Transl. from Galilei 2001, p. 118.)

[777] *Opere* VII, p. 129. (Transl. from Galilei 2001, p. 119.)

[778] *Ibid.*

[779] *Opere* VII, p. 129. (Transl. from Galilei 2001, p. 120.)

9.3.2 Dislocating the *Quaestio*

By invoking anamnesis to illustrate the mathematical method, Galileo would have brought the *Quaestio de certitudine* to the mind of all educated readers. We have seen in chapter 2 how a mathematical study of the empirical world could be granted legitimacy by inscribing it in a Platonically inspired discourse. Philosophers as Catena and Mazzonni argued that the boundary between an ideal realm of mathematical objects and the empirical world could be mediated through Platonic reminiscence. Because God has implanted his geometrical ideas in our minds, we can recognize them in their imperfect material realizations. After all, the world is also to be thought of as a manifestation of the Divine rationality – the material things partake in the ideal forms on the model of the Platonic *methexis*. Blancanus also gestured towards a similar justification but could be less explicit on the role of anamnesis because of his Jesuit background.[780]

To see how Galileo invokes these positions in introducing his own views, it is useful to go back to the earlier quoted passage in which the topic of anamnesis was explicitly introduced, but now adding the further reaction by Sagredo:

> SIMP. I have frequently studied your manner of arguing, which gives me the impression that you lean toward Plato's opinion that *nostrum scire sit quoddam reminisci*. So please remove all questions for me by telling me your idea of this.
> SALV. How I feel about Plato's opinion I can indicate to you by means of words and also by deeds. In my previous arguments I have more than once explained myself with deeds. I shall pursue the same method in the matter at hand, which may then serve as an example, making it easier for you to comprehend my ideas about the acquisition of knowledge if there is time for them some other day, and if Sagredo will not be annoyed by our making such a digression.
> SAGR. Rather, I shall be much obliged. For I remember that when I was studying logic, I never was able to convince myself that Aristotle's method of demonstration, so much preached, was very powerful [*Perché mi ricordo che quando studiavo logica, mai non potetti restar capace di quella tanto predicata dimostrazion potissima di Aristotile*].[781]

This is a revealing mistake in translation. As Galileo was familiar with the issues surrounding the *Quaestio de certitudine*, there can be no doubt about how we should read the last sentence.[782] Here is a

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[780] Cf. chapter 2, section 2.1.2.3, 2.1.2.5, 2.1.2.6.

[781] *Opere* VII, p. 217. (Transl. from Galilei 2001, pp. 221-222.)

[782] In his early notebooks on logic, in all probability composed while he was a young professor in Pisa, he cites a definition of demonstration *potissima*, which is the one by Averroes that was also invoked by Piccolomini. (These notebooks have been translated and published in Wallace 1992a. For the definition see Wallace 1992, p. 102.) His library did hold Barozzi's book on the *Quaestio*… as well as Mazzoni's. (The contents of Galileo's library can be searched at the website of the Firenze Instituto e Museo di Storia della Scienza - http://www.imss.fi.it/indice.html.) He held close contacts with the Jesuit professors

mathematician who also claims to be a philosopher, but who outright dismisses this debate on the status of *potissima* demonstrations as senseless. True, the person speaking is Sagredo, not Salviati who primarily functions as Galileo's mouthpiece – but the effect is only more devastating, as the former is presented as the person of good sense, with whom the reader is invited to identify.

Let us try to be as precise as possible about what is happening in this passage. Simplicio notes the similarity between Salviati's recurring remarks on recollection and Plato's doctrine. Salviati refuses to give a straightforward answer, referring to his deeds as encapsulating his ideas about the acquisition of knowledge. Sagredo adds that he is very interested in the matter *because* he was never able to understand what the philosophical discussions were about. As a result, the reader is invited to see Galileo's method as offering an *alternative* answer to questions such as those discussed in the *Quaestio de certitudine*.[783]

Both the Platonically inspired philosophers and Galileo need God to authorize the mathematical study of nature. In both cases, this authority is delegated to humans through the doctrine of anamnesis. But at this point an important shift has taken place. No longer does this recollection serve as mediation between two different ontological realms. The legitimacy of the mathematical study is not tied to a purely intelligible realm towards the empirical things in the world "strive," it lies in this book that is nature itself.

However, this has a profound effect on the issues discussed in the *Quaestio.* These discussions were structured around the relative importance of mathematics' objects and its demonstrations in explaining its supreme certainty. But Galileo now erases these objects from his own discourse.[784] Halfway his unravelling of the argument concerning the extruding effect of the earthly rotation, he inserts another digression, which throws further light on this erasure:

> SAGR. The argument is very subtle, but nonetheless convincing, and it must be admitted that trying to deal with physical matters without geometry is attempting the impossible.
>
> SALV. Simplicio will not say so, though I do not believe he is one of those Peripatetics who discourage their disciples from the study of mathematics as a thing that disturbs the reason and renders it less fit for contemplation.
>
> SIM. I would not do Plato such an injustice, although I should agree with Aristotle that he plunged into geometry too deeply and became too fascinated by it. After all, Salviati, these mathematical subtleties do very well in the abstract, but they do not work out when applied to sensible and physical matters. For instance, mathematicians may prove well enough in theory that *sphaera*

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at the Collegio Romano, who as we have seen also participated in the debate. And the debate was focused around the University of Padua, where he would spend the years between 1592 and 1610.

[783] For a different view on how Galileo is positioning himself with respect to the *Quaestio*, see Feldhay 1998.

[784] In the *Discourse* on floating bodies, Galileo had already emphatically stated: "I say that shapes, as simple shapes, not only do not operate in physical things, but are never even found separate from bodily substances; nor have I ever proposed shapes denuded of sensible matter." (*Opere* IV, p. 90; transl. from Drake 1981, p. 81.)

> *tangit planum in puncto*, a proposition similar to the one at hand; but when it comes to matter, things happen otherwise.[785]

This is again familiar ground from chapter 2, where we saw Pereira invoking exactly the same example. Salviati answers by immediately attacking Simplicio for his ignorance of geometry. True, Simplicio often admits not being well schooled about the topic, but that should have been irrelevant for the question about the applicability of mathematics to the empirical world, which is properly speaking a purely philosophical issue. Salviati offers a short geometrical proof to establish the fact that if a sphere would not touch a plane in a single point, it would no longer be a sphere (which is of course so by definition).

> SIMP. This proves it for abstract spheres, but not material ones.
> SALV. Show me then where the fallacy of my argument lies, so that it is not conclusive for material spheres although it is for immaterial and abstract ones.[786]

In this way, Salviati actually invites Simplicio to reason mathematically about what the latter claimed could not be treated thus. Simplicio doesn't notice the trap and brings in the imperfection of matter. It is immediately retorted by Salviati that this cannot suffice as an answer because it at most can prove that no material things are actually spherical, which is another thing than claiming that material spheres touch a plane in more than one point.

The general point that Galileo is aiming for is clear. *Any* form can in principle be given a mathematical description. Some of these descriptions will be simple, like that of a sphere, others will be hideously complicated. Things that are spherical touch a plane in one point; things that aren't, touch them in more points. But one cannot make geometrical claims about spheres and falsify them by referring to things that aren't spherical.

> The errors, then, lie not in the abstractness or concreteness, not in geometry or physics, but in a calculator who does not know how to make a true accounting.[787]

The *filosofo geometra* is exactly someone who does know how to settle his accounts. He won't point to perturbed proportions and then claim that the pure proportions are false. He'll show how to account for the differences.

The discussion then returns to the issue of material spheres, and goes on for a few pages, driving home the message that there is no principled distinction between simple and irregular

[785] *Opere* VII, p. 229. (Transl. from Galilei 2001, pp. 236-237.)

[786] *Opere* VII, p. 233. (Transl. from Galilei 2001, p. 240.)

[787] *Opere* VII, p. 234. (Transl. from Galilei 2001, p. 241.)

geometrical forms. Whatever the degree of complexity, any material thing simply has the shape it has. But Salviati brusquely cuts off the discussion:

> Please, gentlemen, it seems to me that we have gone off woolgathering. Since our arguments should continue to be about serious and important things, let us waste no more time on frivolous and quite trivial altercations.[788]

Galileo's message is clear. The really interesting problems lie elsewhere. In the course of one long argument, this is the second time that he introduces an unmistakable reference to the *Quaestio* only to set apart his own position. As the reference to making a true account shows, there is a problem about idealization and abstraction. But as is emphasized in the rest of the discussion, this has nothing to do with the possibility of mathematical *description*. The disappearance of mathematical objects as a special ontological category displaces the problem.

9.3.3 Mathesis versus methexis

We have seen how Galileo structured his mathematical sciences of nature on the model that he inherited from the mixed science tradition. This would have taught him from the beginning that the real problem lay not in giving mathematical descriptions of material things, but in giving fruitful descriptions; i.e. the problem was to find out the right axioms to constrain his mathematical framework in a physically sensible way, thus allowing for explanations.

I already indicated in chapter 2 that the two kinds of criticisms that the philosophers traditionally levelled against the mathematical sciences are not unconnected.[789] The problem of idealizations (physical things never exemplify exact mathematical properties) seems to derive its appeal from an abstractive view on mathematical entities (mathematics deals with purely accidental properties, by abstracting away everything that is natural and essential). We can see this connection reflected in Galileo's strategy: having abandoned the latter view, the problem of idealization as traditionally construed has lost all sense for him.

Ernan McMullin has claimed that Galileo's treatment of idealization wavers between two views: on the one hand the force of mathematics would not be diminished by the presence of impediments which merely make it hard to give a proper description; on the other hand Galileo sometimes talks as if "material nature is seen as not exactly following mathematizable norms, whether simple or complex."[790] However, the presence of passages suggesting both these views does not imply that Galileo was wavering in any sense (at least not on this matter). The passages in which he stresses

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[788] *Opere* VII, p. 236. (Transl. from Galilei 2001, p. 244.)

[789] Chapter 2, section 2.1.1.

[790] McMullin 1978, p. 231.

the first view occur when he emphasizes the fact that any material thing can be given a mathematical description. The passages in which he claims that some things fall outside the scope of a mathematical treatment don't deal with description, but with explanation. This seeming divergence only illustrates the two levels on which any mixed science operates.

However, this also implies that whereas the original problem of idealization has lost all sense to Galileo, it resurfaces at the second level: that of finding out how a mathematical explanation can be given of natural events. The dislocation of the *Quaestio* within Galileo's own text reflects a crucial feature of the way in which he invokes the metaphor of the book. As we noticed above, it entails a purely horizontal level of signification.[791] The problem of idealization that confronts Galileo is one that is situated completely within this realm. Because of this, the impediments themselves become possible objects for thought. There is something that separates the ideal proportions from the perturbed proportions, rather than mere privation.[792] One can in principle account for their *presence.*[793]

In chapter 6, we have analyzed how Galileo deals with the problem of idealization. Most importantly, we have seen how it is regulated by what have seen to be nature's discursive function as a normative instance. Not any way of accounting will do. The geometrical philosopher is accountable to nature. But this allows us to uncover one more discursive layer in Galileo's book metaphor. We began our narrative of the different stages of this metaphor ten years too late. As we have seen in chapter 5, nature's inexorable and immutable character is already operative as a crucial discursive element in *Le mecaniche.* Nature is not only indifferent with respect to human opinions, but also with respect to human desires. It is that which constrains what lies within our powers to achieve.

Galileo's primary models like the balance or the pendulum are representative for natural behaviour. That's the guarantee that underwrites his idealizations. It is because of nature's specific regulative function that we can see what happens in these systems as in some essential aspects *alike* to natural events. Galileo's reading of the book of nature is structured around analogical movements that stay *within* the realm of the empirical world. Let us not forget that Galileo's geometry was a geometry of proportions. The circles and triangles that are the characters of the book of nature encode knowledge about a set of invariant relations. Galileo would have understood the human gaze, to which the book of nature stands continually open, as directed to these structural features. "Seeing" the world as mathematical involves approaching it in a certain way. It means actively searching for invariant ratios, noticing analogies between different structures.[794]

In the *Dialogo* Salviati is teaching Simplicio to be *rational*. He is bringing him to notice *ta mathema*, that what is learnable. And the image of the book of nature simultaneously invokes the

[791] Cf. *supra* section 9.1.2.

[792] Compare with what said about the Platonically inspired discourses in chapter 2, sections 2.1.2 and 2.1.3.

[793] As we have seen, at one point the impediments even become objects for thought in a stronger sense, in that they can be exploited to epistemic ends. Cf. chapter 6, section 6.2.4.

[794] Remember that proportion also means analogy for Galileo; cf. chapter 5, section 5.4.2.

divine *logos* as a presence. The continual transference between these discursive elements constitutes the knot of Galileo's legitimization of his mathematical sciences of nature.

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