REPRESENTATIONS OF SPACE IN SEVENTEENTH CENTURY PHYSICS

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The changing understanding of the universe that characterized the birth of modern science included a fundamental shift in the prevailing *representation of space* – the presupposed conceptual structure that allows one to intelligibly describe the spatial properties of physical phenomena. At the beginning of the seventeenth century, the prevailing representation of space was spherical. Natural philosophers first assumed a spatial center, then specified meanings with reference to that center. Directions, for example, were described in relation to the center, and locations were specified by distance from the center. Through a series of attempts to solve problems first raised by the work of Copernicus, this Aristotelian, spherical framework was replaced by a rectilinear representation of space. By the end of the seventeenth century, descriptions were understood by reference to linear orientations, as parallel or oblique to a presupposed line, and locations were identified without reference to a privileged central point. This move to rectilinear representations of space enabled Gilbert, Kepler, Galileo, Descartes, and Newton to describe and explain the behavior of the physical world in the novel ways for which these men are justly famous, including their theories of gravitational attraction and inertia. In other words, the shift towards a rectilinear representation of space was essential to the fundamental reconception of the universe that gave rise to both modern physical theory and, at the same time, the linear way of experiencing the world that characterizes modern science.

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PREFACE

“He remarked to me then,” said that mildest of men,
“‘If your Snark be a Snark that is right:
Fetch it home by all means – you may serve it with greens,
And it’s handy for striking a light.’” (Lewis Carroll, *The Hunting of the Snark*)

I would be remiss and ashamed if I failed to acknowledge the support and guidance of the many who contributed to this project. Their collective help carried me through the cycles of frustration and inspiration that produced this dissertation.

First, I owe the deepest gratitude to the faculty and students of the Department of History and Philosophy of Science at the University of Pittsburgh. Much of the ideas presented here were developed in discussions with them, whether in seminars, in the hallways and offices of the Cathedral of Learning, or over beers at the Holiday Inn and elsewhere. I profited immensely from the collegiality, openness, and intelligence of every member of the department. Their ideas, encouragement, and advice was and will always be welcome.

My advisors, Peter Machamer and Ted McGuire, merit the highest praise and appreciation for their firm yet restrained supervision. This dissertation bears the marks of their alternating approval and criticism, which cajoled me through its writing. I could not have wished for a better pair of teachers, scholars, and friends to guide me. Jonathan Scott (of the History Department) and Paolo Palmieri were also especially important resources upon whom I had the pleasure to call.

Amongst my peers, I gained untold benefit from the friendship and conversation of Zvi Biener, and, especially, Greg Frost-Arnold. Greg, along with Karen Frost-Arnold, must be thanked for so many particular acts of insight, friendship, and tolerance I cannot begin to list them here.

I could not have completed this project without the support of my parents, grandparents, and brother. My family’s sympathy with my scholarly aspirations is remarkable and commendable, even if they are ultimately the source of them. They share any credit that may be given to me or this work.

Above all, there is Dana LeVine. Her steadfast love, caring, and sensitivity, as well as her intelligence, diligence, and energy, will always be my model. I am endlessly thankful for her devotion and companionship. She can teach me more in three minutes than all books could teach me in seventy years.

The bulk of this dissertation was written in a carriage house (which itself deserves some credit) in Athens, Georgia, with the support of a Mellon Dissertation Fellowship from the Faculty of Arts and Sciences of the University of Pittsburgh. I am also grateful for the support of the Philosophy Department of the University of North Carolina at Chapel Hill.

In addition to the few I have named, there are countless others who contributed to this dissertation. They have all helped me avoid the boojums that threatened me and my work. For their sake, I hope this is a Snark that is right. With their assistance I have fetched it home by all means. May it be handy for striking some light!

1.0 INTRODUCTION: PHYSICAL UNDERSTANDING AND REPRESENTATIONS OF SPACE

This thesis argues that the seventeenth century witnessed a profound shift in the *representation of space* employed by physicists and philosophers to describe and explain the phenomena of the natural world. The concepts and presuppositions that conditioned one's experience of physical space markedly changed – from a spherical, Aristotelian framework widely accepted at the beginning of the century to the rectilinear, Cartesian framework accepted at its close. Moreover, it will be suggested, this shift both accompanied and enabled the remarkable developments in physical explanations that occurred during the same period. The shift in representations of space came about as an *iterative reciprocation* between the ways philosophers explained, described, and conceived phenomena in space. Developments at one of these levels allowed and necessitated adjustments at the others. The novel representation of space was both cause and effect of the new physics, and *vice versa*.

This project should be considered a work of intellectual history – a study of the intellectual causes and effects of ideas. The subject of this dissertation is philosophical. It examines the spatial epistemology of early modern natural philosophy, and proposes a philosophically justified framework by which to study that epistemology – namely, representations of space. However the argument will develop by ostension, which is to say, historically. It will attempt to demonstrate how the iterative reciprocation resulting in the shift in spatial concepts occurred in the work of several seventeenth-century authors. The aim here is to make the intellectual iteration and epistemological shift apparent in the historical account.[1]

[1] The following is an "account" in the sense that it attempts a chronological reconstruction and explanation of an intellectual change based on the examination of representations of space. It argues by ostension in that it tries to point out – in the account itself – the constitutive and causal role of representations of space in the iterative reciprocation by which physical thought developed. These aims are meant to be mutually reinforcing. The coherence and likelihood of the historical reconstruction will support the plausibility of the philosophical analysis by which it is constructed, and *vice versa*. See section 1.2, below.

1.1 THREE LEVELS OF UNDERSTANDING

1.1.1 Explanations and Descriptions

Before we begin our account, however, some preliminaries are in order. In what follows, the examination of representations of space will be used as a tool for a historical investigation. At first blush, this might seem anachronistic – we are seeking something of our own devising in the work of historical authors. We should, therefore, explain precisely what is meant by a "representation of space" in order to allay this concern. We will try to motivate the sense that representations of space are necessary elements of physical understanding, regardless of period. Thus, it is reasonable to seek them in texts from the past. Additionally, our explanation will allow a more precise statement of the dissertation's thesis. It will also enable us to explain how the present project stands in relation to some of the relevant literature.

This dissertation is motivated by the basic intuition that human physical understanding consists in the ability to explain facts about the physical world. Understanding, it seems at least in part, is the ability to provide satisfactory explanations – we say we understand a phenomenon when we can give an account of it.[2] This fundamental intuition has led philosophers interested in explicating the nature of human understanding to attempt a clear explication of the concept of explanation.

Following the seminal work of Carl Hempel,[3] most philosophers examining explanations agree that they consist of a set of statements. This set includes an *explanandum* and an *explanans*. The explanandum is a statement (or statements) describing the phenomenon to be explained. The explanans is a set of statements which are meant to account for the phenomenon explained by the explanandum. A satisfactory explanation, therefore, is one in which the statements in the explanans and explanandum, and the relationships between them, satisfy certain conditions. However, it is a matter of great debate, in the seventeenth century as much as today, what conditions a satisfactory explanation must satisfy. Philosophers, especially since Hempel, have proposed various criteria, including logical entailment, statistical relevance, causal

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[2] See Michael Friedman, "Explanation and Scientific Understanding," in *Theories of Explanation*, ed. Joseph C. Pitt (Oxford: Oxford University Press, 1988).
[3] Carl G. Hempel, "Studies in the Logic of Explanation," in *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science* (New York: Free Press, 1965).

relevance, and many others. It is not the aim of this project, however, to adjudicate the debate concerning the essence of explanation.[4] Nor will we try to evaluate the claims of modern philosophers with regard to seventeenth century evidence. That is, we will not come out in favor of one model of explanation over any other.

What interests us, instead, is the fact that explanations, even in the most general sense, rely on descriptions, which rely, in turn, on concepts. Thus, understanding *is* the ability to explain, but this ability rests on more than just the construction of explanations. This point is clear from the fact that at least some of the sentences, both in the explanandum and in the explanans, must be descriptions. On the one hand, the explanandum is meant to identify some feature of the physical world that is meant to be explained. Hempel, for example, defined an explanandum as a "sentence describing the phenomenon to be explained."[5] That is, the explanandum is a description of some fact about the world. Hempel's definition is girded by the difficulty imagining how one might set about explaining an explanandum that is not a description. How could one try to provide an explanation if one does not know *what* to explain? There must be some phenomenon that is the target or "topic"[6] of an explanation, and this topic must be specified in the explanandum. The specification of the phenomenon requires a description. (Being a description is a necessary but not sufficient condition for an explanandum. There are descriptions which are not explicable: e.g., "The distance from New York to San Francisco is (roughly) 2900 miles."[7])

On the other hand, it also seems reasonable to assume that at least one of the sentences in the explanans must be a description. If a physical explanation is meant to show how the properties and relations of physical objects account for the feature of the world described by the explanandum, then some specification of those properties and relations must appear in the explanans. In other words, the explanans must include descriptions of the facts about the world that account for the phenomenon in question. For example, the explanans might include descriptions of the initial conditions that occasioned the phenomenon to be explained. Or it

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[4] If, in fact, it is judicable, which I doubt. See Peter Achinstein, "Can There Be a Model of Explanation," in *Explanation*, ed. David-Hillel Ruben (Oxford: Oxford University Press, 1993).
[5] Hempel, "Studies in the Logic of Explanation."
[6] The term "topic" is due to Bas van Fraassen. Wesley C. Salmon, "Four Decades of Scientific Explanation," in *Scientific Explanation*, ed. Philip Kitcher and Wesley C. Salmon (Minneapolis: University of Minnesota Press, 1989), 139.
[7] The example is from Bromberger via Salmon. Ibid., 39.

might include a description of the physical context in which the phenomenon took place (the forces acting and so on).[8]

One could even argue that *all* of the statements in an explanation are descriptions. In a broad sense, even the law-like generalizations or statistical rules that are adduced in order to link initial conditions with the phenomenon to be explained can be considered as descriptions. Like other descriptions, they specify facts about the world. They assert that the world is such and such a way. Or they assert that certain objects invariably have such and such features (including dispositions).[9] On this view, an explanation is simply a marshalling of facts that account for another fact. To give an explanation, then, is to provide a description of each of these facts. Thus, the requirement that explanations include descriptions does not speak against "ontic"[10] conceptions of explanation, where an explanation is not considered as an argument, but as a collection of sentences or propositions reporting objective facts that account for (causally, statistically, or otherwise) for the phenomenon to be explained.[11] For present purposes, however, we do not have to prove this point. It will suffice if just one statement in an explanation is a description.

Note that we are taking "description" in a very loose sense. We use the term to refer to any specification of a (putative) fact about the world. This need not take the canonical form of a description-sentence, "*x* is *p*," where *x* is some definite or indefinite term and *p* is some predicate clause.[12] By defining descriptions in this manner, we can avoid the concern that legitimate explanations can be provided in which no sentential descriptions appear. For instance, Paul Humphreys has suggested that explanations can be legitimately invoked that have noun clauses as explananda. He suggests that explanations can be given for "the increase in volume of a gas maintained at constant pressure; of the high incidence of recidivism among first-time offenders;

[8] For a detailed perspective on descriptions in the explanans, see Donald Davidson, "Causal Relations," *The Journal of Philosophy* 64, no. 21 (1967); Michael E. Levin, "The Extensionality of Causation and Causal-Explanatory Contexts," *Philosophy of Science* 43, no. 2 (1976); James Woodward, "A Theory of Singular Causal Explanation," in *Explanation*, ed. David-Hillel Ruben (Oxford: Oxford University Press, 1993). These authors assume that the explanans describes the salient features of the situation that cause the phenomenon to be explained. They debate, however, whether the description appearing in the explanans must be extensional.
[9] Additionally, one can point out that most philosophers allow that generalizations themselves can be explained. In such cases, they are explananda and, *a fortiori*, descriptions.
[10] See Wesley C. Salmon, *Causality and Explanation* (Oxford: Oxford University Press, 1998), 54.
[11] See Salmon, "Four Decades," esp. 86ff.
[12] See Peter Ludlow, "Descriptions," in *Stanford Encyclopedia of Philosophy*, ed. Ed Zalta (Stanford: Stanford University, 2004).

of the occurrence of paresis in an individual; [etc.]"[13] These possible explananda are not sentences. Hence, they would not satisfy Hempel's definition of an explanandum, which requires them to be sentences. Nevertheless, these terms do specify facts about the world. In Humphreys's words, they specify "usually explicitly, sometimes implicitly... the occurrence or change of a property associated with a system."[14] Hence, they would qualify as descriptions in our sense. Non-sentential statements of fact in explanantia can be handled similarly. Our interest is not the syntactic form of a description, but its semantic content – whether and how it specifies a phenomenal fact.[15]

In this context, we can also set aside the issue of descriptive reference. One might worry that an explanation can include specifications of fictions, rather than facts. Objectively speaking these specifications are not descriptions, since they do not refer to any facts about the world. Suppose, for example, a magician explains the fall of an apple by appealing to malign astral influences. His putative explanation would, presumably, include descriptions of astral influences and their behavior, even though astral influences, as far as we know, do not exist. We are willing to count the magician's explanation as legitimate so long as *he thinks* he is describing something. As we shall discuss below, descriptions are relativized to the cognitive context in which they are generated and interpreted. This relativization includes their status as descriptions. Hence, the magician's appeals are explanatory if he really believes that astral influences exist and that his descriptions really specify facts about them. If, on the other hand, the magician is disingenuously appealing to entities he knows or believes not to exist, then we would count his statements as neither descriptive nor explanatory. Rather, we would claim he was perpetrating a

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[13] Paul W. Humphreys, "Scientific Explanation: The Causes, Some of the Causes, and Nothing But the Causes," in *Scientific Explanation*, ed. Philip Kitcher and Wesley C. Salmon (Minneapolis: University of Minnesota Press, 1989), 298.
[14] Ibid.
[15] Hempel's account of explanation generally glosses over the relationship between phenomenon and description. He assumes that phenomena are always already described. See Michael Scriven, "Explanation, Predictions, and Laws," in *Theories of Explanation*, ed. Joseph C. Pitt (Oxford: Oxford University Press, 1988), 65-66.

trick or fraud on his audience.[16] In either case, we avoid accepting an explanation that is devoid of descriptions.[17]

Philosophers of science usually require that the statements constituting an explanation be *true*, in some relevant sense. In the case of descriptions, they require that they accurately reflect facts about the world, however that accuracy is measured.[18] Our definition of description, however, obviates this condition. The reason for this has to do with the historical aims of our project. Philosophers of science aim to provide the criteria a *good and definitive* explanation must satisfy. The explanations provided by an outdated theory are wrong, so they cannot be good explanations. They do not adequately account for the phenomena. Thus, it seems reasonable to reject them as scientific explanations. Otherwise, one is forced to accept the "awkward consequence" that "originally [an] explanatory account was a correct explanation, but that it ceased to be one later, when unfavorable evidence was discovered."[19] Our project, though, is a historical study of outdated theories. It would be anachronistic to say that because the theories turned out to be wrong, the accounts of phenomena their proponents provide are not explanations. Instead, so long as an account is proposed as an explanation, we will treat it as an explanation. We require only that an author accept, given whatever resources he has at hand in his time, that his assertions adequately account for the phenomenon in question. (The principle of charity will lead us to assume that this is usually the case.) We will say that, whether or not an explanation is correct, it is still an explanation.[20]

Despite our argument that explanations necessarily include descriptions, we do not mean to suggest that explanations are merely descriptions. Explanations are distinct from descriptions

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[16] I am reminded of the comic strip *Calvin and Hobbes*, in which Calvin asks his father to explain why pictures taken before a certain time are in black and white. Calvin's father responds that the pictures are actually color photographs. The world, he says, was actually black and white, and only turned color sometime around the 1950's. Presumably, this is a joke. Yet, if a credulous Calvin were to repeat this account, we would take his statements as an explanation, however poor.

[17] In a more specific context, Salmon presupposes that "the phenomenon we endeavor to explain did occur – that the putative fact is, indeed, a fact." That is, he assumes that the explanandum is a legitimate description of a real phenomenon. He allows, however, that one might "suspend belief" in certain cases. Thus, it would be legitimate to attempt an explanation of a unicorn's horn if one temporarily suspends the belief that unicorns do not exist. In this context, statements about a unicorn's features would be legitimate descriptions, insofar as they specify putative facts about the world. See Salmon, "Four Decades," 6n2.

[18] I will not attempt any analysis of empirical adequacy or truth. The subject is tangential.

[19] Hempel, "Studies in the Logic of Explanation," 248.

[20] For a more extended discussion of this historiographical issue, see Daniel Garber, *Descartes Embodied* (Cambridge: Cambridge University Press, 2001), ch. 1; Quentin Skinner, "A reply to my critics," in *Meaning and Context: Quentin Skinner and His Critics*, ed. James Tully (Cambridge: Polity Press, 1988), 246-47.

insofar as they evince some sort of active principle or entity. In other words, an explanation, either by its structure or content, should relate the phenomenon in question to some underlying productive *cause*. With this caveat, we are again following most post-positivistic philosophers who have cautiously rejected Humean skepticism about the epistemic accessibility of causal relationships. Our intuition is that, in the words of Wesley Salmon, the "commonsense notion of explanation seems to take it for granted that to explain some particular event is to identify its cause and, possibly, point out the causal connection."[21]

By requiring an elucidation of causes, we are also following seventeenth century practice in natural philosophy. In the somewhat tortuous heritage of Aristotelian logic, a prevailing view of the early modern period was that the proper form of a physical explanation followed a *regressus* argument. This entailed two separate demonstrations, each of which established a relationship between the effect to be explained and its proximate cause. One first had to relate the effect to a proximate cause by a *demonstratio quia*. Then the effect was deduced from the cause by a *demonstratio propter quid*. Once these demonstrations were complete, the effect was considered explained. In other words, an effect was not explained until its causes had been identified and it had been shown that the causes produced the effect.

The insistence on *regressus* demonstrations should not be taken as a complete seventeenth century *theory* of explanation. It was only a general condition of explanation. The precise form of *regressus* demonstrations was problematic during the period. What counted as a satisfactory demonstration *quia* and *propter quid* varied from author to author. Moreover, the need for *regressus* arguments at all was questioned at the time (e.g., by Galileo and Newton), so there is no guarantee an author is adhering to this view of explanation. Our point is only that philosophers of the period thought causes should play a role in explanation, so it makes sense for us to think so, as well.[22]

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[21] Salmon, "Four Decades," 46.

[22] See Peter Barker and Bernard R. Goldstein, "Theological Foundations of Kepler's Astronomy," *Osiris* 16 (2001); Peter Dear, "Method and the study of nature," in *The Cambridge History of Seventeenth-Century Philosophy*, ed. Daniel Garber and Michael Ayers (Cambridge: Cambridge University Press, 1998); Nicholas Jardine, "Galileo's Road to Truth and the Demonstrative Regress," *Studies in History and Philosophy of Science* 7, no. 4 (1976); Steven Nadler, "Doctrines of explanation in late scholasticism and in the mechanical philosophy," in *The Cambridge History of Seventeenth-Century Philosophy*, ed. Daniel Garber and Michael Ayers (Cambridge: Cambridge University Press, 1998); William A. Wallace, *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought* (Dordrecht: D. Reidel, 1981); William A. Wallace, "Randall Redivivus: Galileo and the Paduan Aristotelians," *Journal of the History of Ideas* 49, no. 1 (1988).

We should also note that the seventeenth century palette of acceptable "causes" was much broader than today's. One effect of the mechanical tradition has been a severe limitation of what can count as a cause. Though modern philosophers do not agree on what should and should not count as a cause, most would reject many of the causes proposed by early modern philosophers. Chief amongst these are the "natural," teleological causes central to Scholastic accounts of nature, as well as the frequent appeals to God's will. While the status of these as causes was questioned during the early modern period, there is no reason to dismiss them out of hand. Throughout the following, we will not advance any particular explication of causation, modern or otherwise. Instead, we will respect what each author himself considered a legitimate cause or legitimate causal explanation, without trying to unite all views into a theory of causation.

The condition that explanations include some appeal to causes allows us to distinguish explanations from mere descriptions. Suppose that it is possible to deduce, from a series of descriptions of the present behavior of a physical system, any past or future behavior of that system. Unless the deduction appeals to causes, we would not count the deduction as an explanation of the system's behavior. A prediction or retrodiction, in other words, does not an explanation make.[23] A seventeenth century case in point is the distinction between astronomy and physics. In general, most commentators thought astronomy was primarily concerned with providing *descriptions* of the motions of the heavens, which could then be used to predict and retrodict planetary positions. The astronomical systems of Ptolemy and Copernicus alike were seen as calculating methods, not explanations of the motions they described. To *explain* celestial phenomena, even after Copernicus, philosophers relied on Aristotelian physical theories, which appealed to the nature of celestial matter as the *cause* of its motion.[24] Explanations include an appeal to causes. Descriptions can include causes, but need not.[25]

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[23] For a contemporary take on this matter, see Salmon's distinction between explanatory and descriptive knowledge. Salmon, "Four Decades," 126-35.

[24] As we shall see, Kepler broke with this tradition and tried to unify physics and astronomy. See Barker and Goldstein, "Theological Foundations of Kepler's Astronomy."; Nicholas Jardine, *The Birth of History and Philosophy of Science: Kepler's A Defence of Tycho Against Ursus* (Cambridge: Cambridge University Press, 1984); Robert S. Westman, "The Melanchthon Circle, Rheticus, and the Wittenberg Interpretation of the Copernican Theory," *Isis* 66, no. 2 (1975).

[25] We leave it as an open question as to *how* causes can appear in explanations. It is entirely possible that causes appear *in* descriptions. That is, part of the explanans might consist of *descriptions of causes* and the ways they operate. We do not wish to prejudge this issue by claiming that descriptions, as a rule, do not include appeals to causes (though *mere* descriptions, by definition, do not). We only wish to claim that explanations *do*.

A shorthand, but not entirely rigid, way of couching the distinction between description and explanation is in the terms of kinematics and dynamics. Kinematics concerns the measurement and specification of physical behaviors – speeds, distances, positions, etc. As such, it is a descriptive enterprise. Dynamics investigates the principles that cause physical phenomena. It is an explanatory pursuit. Thus, when we come across kinematic terms – motion, speed, distance, quantity, etc. – we can generally assume the author is describing a phenomenon. By contrast, wherever we encounter dynamical terms – force, power, action, cause, etc. – the author is usually engaged in physical explanation. By paying heed to these terms, we can begin to divine an author's meaning.[26]

It should be reiterated that this project is *not* intended to solve the vast philosophical problems concerning explanation, description, and causation in both modern science and that of the early modern period. We are not advocating a particular theory or model of explanation, description, or causation. We are not concerned with judging the merits of any philosophical system in relation to early modern intellectual endeavor. We only claim 1) that explanations contain descriptions and 2) explanations must appeal to causes. We can then set about elucidating different author's approaches to physical phenomena and the relationships between the descriptions and explanations they provide. It does not seem that we are treading heavily on any philosophical toes by adhering to these claims.[27]

1.1.2 Descriptions and Concepts

So far, we have argued that understanding of the physical world consists in the ability to provide explanations of physical phenomena. We have also asserted that explanations consist, at least in part, of descriptions. Now we turn to the relationship between descriptions and concepts.

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[26] For the medieval history of the distinction between kinematics and dynamics, see Marshall Clagett, *The Science of Mechanics in the Middle Ages* (Madison: University of Wisconsin Press, 1959), ch. 4.

[27] David-Hillel Ruben calls our position the "standard view." He writes: "Causal explanation of one chunk of the world... by another, it is often held, is relative to a description... Thus, on the standard view, what is to be explained is never an event *per se*, but a fact or an existential generalization or something with some sort of propositional structure. This view holds that the explanation relation relates relata which are not themselves entirely mind-independent." Causal explanations are relativized to the manner by which they refer to facts about the world. As Ruben notes, and as we shall argue below, this entails that explanations depend on the subjective cognitive frameworks in which they are produced. David-Hillel Ruben, "Introduction," in *Explanation*, ed. David-Hillel Ruben (Oxford: Oxford University Press, 1993), 15.

Just as explanations rely on descriptions, descriptions depend on concepts. We cannot generate or comprehend descriptions unless they are set in the context of a cognitive framework – the set of concepts that give meaning to the terms used in the descriptions. Concepts allow the generation and interpretation of descriptions since they regulate what the descriptions mean.

Consider, as an example, the description "The apple falls down." This statement describes the behavior of a physical object. It specifies an object – the apple – as well as its behavior – a particular form of motion, falling. The statement also specifies the direction of the motion – down. Altogether, then, the statement "The apple falls down" can be comprehended as a meaningful description of the phenomenon. It specifies a certain fact about the world: the behavior of the apple – i.e., it moves in a particular manner in a particular direction.

The meaning of "apple," "falls," and "down" in this description, however, are not inherently and immediately intelligible. The description must be further *interpreted* in order to make its meaning patent. "Apple" must be interpreted to mean a particular object. The term "falls" must be interpreted as a particular kind of accelerated motion. "Down" must be interpreted as a specific direction or path. This interpretation requires concepts.

Now, it is probably fair to say that there is no general consensus amongst philosophers, cognitive scientists, psychologists, or any other group about what a "concept" actually is. There is no well-articulated concept of "concept." This is not a problem we are about to solve. However, it seems fairly well accepted that concepts perform a specific function – namely, to allow the interpretation and generation of descriptive terms as applied to phenomena. Thus, the concept of "apple" allows the interpreter of the description "The apple falls down" to identify the object described. The concept of "falls" allows the interpreter to understand that the apple undergoes an accelerated motion, while the concept of "down" picks out which direction is meant by the description. Similarly, an observer of a falling apple would employ the concepts of "apple," "falls," and "down" in order to generate the description "The apple falls down." Interpreters and generators of descriptions can only operate within the context of some cognitive framework – the set of all concepts one possesses – that includes the concepts of the terms of their descriptions. Put simply, the very possibility of description relies on concepts.

In a very basic sense, a concept operates normatively. It consists of the set of implicit and explicit rules by which the propriety of a descriptive term is judged. Let us focus, for the time being, on the term "down." As noted, this specifies a direction, but *which* direction is not clear from the description alone. The actual direction is fixed by the concept of "down," which provides criteria for determining which direction (or directions) can be properly described as "down." In other words, the concept provides rules by which one can judge whether "down" is a correct description of a direction. Using the criteria given by concepts, the generator of a description can decide which terms adequately describe a phenomenon, and an interpreter can "decode" the resulting description.

There are many different kinds of rules which can be used to constitute concepts. In the case of directional terms, concepts usually appeal to presupposed reference points or privileged orientations which fix the particular direction intended by a description. For example, the concept of down might refer to a privileged location – the center of the earth, say – by which the propriety of the description "down" can be judged. In this case, the concept might include the rule:

> The direction in question can be called "down" if and only if the direction is toward the center of the earth.

On this concept, "down" specifies the direction directly toward the stipulated location. If the actual direction fits this criterion – i.e., it is directed toward the center of the earth – then the description "down" is judged an appropriate description. Conversely, an interpreter interpreting the description "The apple falls down" would understand that the apple moves toward the center of the earth.

The rules comprising a concept can vary. Rather than the direction toward a privileged location, one's concept of "down" might refer to a presupposed direction by which the description of direction can be oriented. "Down," for instance, may be understood as being along one's head-to-toe axis. In this case, a rule in the concept might be:

> The direction in question can be called "down" if and only if it is parallel to and in the same sense as the axis between one's head and toes.

If an apple falls in the presupposed direction, it can be described as moving "down." The head-to-toe axis orients the meaning of "down" to a specific, antecedently understood direction. Something like this concept is applied when we describe certain phenomena in a bed. For example, we turn blankets "down," along the head-to-toe axis of someone lying in the bed. This description is intelligible because we understand it in relation to the head-to-toe axis.[28]

In most cases, these variations amongst concepts do not raise significant difficulties for the generation and interpretation of descriptions. In order for speakers of a language to communicate, they must share roughly similar concepts. Thus, two observers operating with the two concepts of "down" just mentioned would nevertheless agree that an apple falls "down." Communication, and the possibility of description, breaks down only when interlocutors possess different or radically divergent concepts. Thus, if one were to say, on observing the apple, that "The apple falls umpwise," one might think it was a description, but an interpreter of the description lacking a concept of "umpwise" would be unable to understand its meaning. Likewise if the interpreter's concept of "down" meant toward the center of the sun, in which case he would deny that the description "The apple falls down" is an acceptable description of the phenomenon.[29] Usually, such failures of description are rare and limited to extreme cases (at least amongst contemporaries), and speakers of a language can simply assume that their audience shares their concepts. Thus, generators of descriptions usually describe phenomena without explicating the concepts they employ. (As we shall note below, though, the rarity of explicit conceptual explication raises historiographical difficulties for this project.)

Of course, the examples given are overly simplistic. The concept of "down" cannot be given in a one sentence rule. Another, equally legitimate concept would be a concatenation of the rules above. For example, one's concept of "down" might include:

> 1. For most cases, the direction in question can be called "down" if and only if the direction is toward the center of the earth.
> 2. If describing a phenomenon in a bed, the direction in question can be called "down" if and only if it is parallel to and in the same sense as the axis between one's head and toes when one is lying in the bed.

Further qualifications and cases might also be present. For example, the above plus:

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[28] George Lakoff, *Women, Fire, and Dangerous Things: What Categories Reveal About the Mind* (Chicago: University of Chicago Press, 1987).

[29] The point here is similar to Philip Kitcher's discussion of "reference potentials." According to Kitcher, interlocutors must at least possess overlapping "reference potentials" for the terms they use in order to communicate. If the reference potentials do not overlap (i.e., share at least one common possible referent), the interlocutors cannot interpret the reference of each other's utterances. Here, of course, the *meaning* constituted by a concept is not limited to *reference*. See Philip Kitcher, "Theories, Theorists and Theoretical Change," *The Philosophical Review* 87, no. 4 (1978).

3. If describing a phenomenon on a bench, or plank, or other suitable horizontal surface, the direction in question can be called "down" if and only if it is parallel to the axis of the horizontal surface in either sense.
4, 5, 6... (Other rules in the concept delineating what qualifies as a "bench," "plank," and "suitable horizontal surface.")

It is readily apparent, then, that the concept of down can be very complicated. At the very least, it is linked to other concepts (e.g., those of "head-to-toe axis," "center of the earth," and even "plank"), and it may not be clear where the concept of "down" ends and another concept begins. Moreover, it is not clear that all the rules in a concept can be verbally expressed. Some rules may refer to exemplars – particular objects – that cannot appear in sentential rules. For instance, one might have in mind one's own dog when judging whether another object can be described as "a dog."[30] The relevant rule, in this case, would entail a comparison to the exemplar in mind. Also, the rules comprising a concept may be simply heuristic and therefore subject to inexpressible exceptions. These complications make it difficult to express the rules that can constitute a concept and thus make it hard to identify just what the concept of any given term actually is.

Despite this complexity, our basic characterization of concepts remains. Concepts consist of implicit and explicit rules by which the ascription of terms is judged. The concept of "down" is whatever legitimates the term "down" as a description of a direction. So long as we are catholic about what counts as a rule, it does not seem that this general, functional notion of concepts is particularly objectionable. In what follows, we will be satisfied to identify particular features of a limited set of concepts. We will leave it to elsewhere to try to provide more general and articulated characterizations of concept function and formation. Meanwhile, the need for concepts to ground the generation and interpretation of descriptions is not limited to directions such as "down." We can generalize our basic argument to all elements of descriptions. It is easy to see how descriptions of location, shape, distance, speed, color, size, etc., require the normative

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[30] This is a particularly simple example. In fact, exemplars may be quite technical and complicated. Galileo's appeal to the behavior of bitumen on a hot iron pan, for example, helped make his description of sunspots intelligible. Similarly, he often used the lever to generate descriptions of phenomena, as in the cases of floating bodies and inclined planes. Machamer calls these exemplars "models of intelligibility." See R. Feldhay, "Producing Sunspots on an Iron Pan," in *Science, Reason, and Rhetoric*, ed. Henry Krips, J.E. McGuire, and Trevor Melia (Pittsburgh: University of Pittsburgh Press, 1995); Peter K. Machamer, "Comment: A New Way of Seeing Galileo's Sunspots (and New Ways to Talk Too)," in *Science, Reason, and Rhetoric*, ed. Henry Krips, J.E. McGuire, and Trevor Melia (Pittsburgh: University of Pittsburgh Press, 1995).

criteria (some more expressible than others) provided by concepts in a cognitive framework to be properly generated and comprehended.

Physical understanding, then, can be divided into three distinct levels. On the surface, understanding is simply the ability to provide explanations of phenomena. As we have seen, though, explanations include descriptions. In order to generate an explanation, one must describe the phenomenon in question. Descriptions, in turn, rely on concepts, which give meaning to descriptions. Without concepts, one cannot generate or interpret descriptions, and one cannot provide explanations. Understanding subsists on all three levels.

1.1.3 Representations of Space

A *representation of space*, finally, is the subset of the concepts in a cognitive framework that concern spatial properties and relations. It includes, among many others, the concepts of "up," "down," "above," "below," "far," "near," and so on. Thus, a representation of space is the set of concepts that underwrites descriptions of directions, locations, sizes, shapes, distances, and any other spatial property or relation. Altogether, a representation of space constitutes one's conception of space. Hence, a representation of space is an important element of the understanding of physical phenomena, since such phenomena occur in physical space. It forms part of the conceptual context in which physical phenomena are described and explained. Our representation of space conditions our understanding of the natural world.[31]

A representation of space, however, denotes more than just a bare set of concepts. Concepts are not held *in vacuo*, one by one. Each is invariably linked to others, forming intellectual complexes and structures.[32] For example, "up" is usually the opposite of "down." If "down" is conceived as directly toward a presupposed location, "up" is usually conceived as directly away from the same location. The concepts of "above" and "below," "top" and "bottom" will also often refer to the same location, such that if "above" is further from the

[31] I do not intend to sketch a concept of space here. To do so would be prejudicial and anachronistic, since the concept of space is the very issue of this dissertation, and the historical authors discussed all have their own view. Moreover, I am only interested in the epistemological aspects of spatial concepts, and an attempt to give a full, metaphysical description of space would be tangential. For the time being, I will simply appeal to the reader's intuitive notion of what space is.
[32] Lakoff calls this feature of cognitive systems "polysemy." Lakoff, *Women, Fire, and Dangerous Things: What Categories Reveal About the Mind*, 12.

location, "below" is nearer, "top" is furthest, and "bottom" is nearest. Thus, the interrelations between the concepts included in this representation of space form a coherent structure, built around a single presupposed location to which each of the concepts refers. A similar structure will result if the concepts are referred to a presupposed axis, as well. A representation of space includes such relationships between concepts. It is a conceptual structure in which each of its constitutive concepts has a place.

It is possible to characterize the geometry of these conceptual structures. If, for example, the concepts in a representation of space generally refer to a presupposed, privileged location, then directions, such as "up" and "down" will converge or diverge toward or away from the presupposed location. That is, the direction an observer employing this representation of space will describe as "down" will converge towards the central point his or her concept of "down" refers to. Each region of space may also be conceived with a determinate privileged orientation – e.g., the direction toward or away from the privileged location. The observer will be able to tell which way is "up" or "down." And different regions of space will be conceptually distinguishable from one another by their distance from the privileged location. The observer, therefore, will be able to describe regions of space as "higher" and "lower." Put another way, a representation of space that presupposes a single privileged location is convergent, anisotropic, and heterogeneous. This is a *spherical* representation of space, since the conceptual structure exhibits the properties of a spherical geometry.

Perhaps the best way to illustrate a spherical representation of space is with an example. Consider Aristotle's description of the "simple motions" in *De Caelo*:

> Now revolution about the center is circular motion, while the upward and downward movements are in a straight line, 'upward' meaning motion away from the center, and 'downward' motion towards it. All simple motion, then, must be motion either away from or towards or about the center.[33]

Aristotle conceives of directions in relation to a presupposed privileged location, namely, the center of the universe, which happens to coincide with the center of the earth. The center of the universe is a primitive feature of Aristotle's representation of space. The location is presumed in order to make descriptions of direction intelligible. "Downward" means toward the center.

[33] *De Caelo* I.2, Aristotle. *The Complete Works of Aristotle*, Jonathan Barnes, ed., (Princeton: Princeton University Press, 1984), 448.

"Upward" means away from it. In other words, Aristotle's geometrical structure of space is *spherical* – it is structured spherically around a single, privileged location.

This spherical representation of space allows the description of three "simple motions" – upward, downward, and about the center. Motion in these directions is simple because a body moving along these paths does not change *direction*. The direction of the motion is always described the same way – upward, downward, or about the center. The motion is thus described *because* the direction is conceived in relation to the center. Hence, the simple motions are either linear (towards or away from the center) or circular (around the center). These motions are simple because direction is conceived in relation to the spherical representation of space. The "simplicity" of the motions depends on how direction is conceived and, thus, how the motion is described.

The explanatory consequences of this spherical geometry in Aristotle's physical theories are well known. Aristotle can distinguish different regions of space, or "places," by their distances to the center. This heterogeneity of space allows him to use locations as (in later terms) *termini a quo* and *termini ad quem* in his physical explanations. He can then argue that bodies possess inherent natures that cause motion towards their proper places. Thus, the heavy elements earth and water naturally move "downwards," toward the center. The light elements air and fire naturally rise "upward," away from the center (or toward the periphery), and the celestial spheres remain in "place," naturally rotating around the center. Aristotle also argues that, since they are simple bodies, the elements should move simply – with simple motions. Hence, the heavy and light elements move linearly toward and away from the center, and the heavens rotate circularly around it. As we have seen, though, Aristotle's description of "simple motions" also relies on his spherical representation of space.

These basic physical explanations have further consequences. For example, the earth's center and the geometrical center of space must coincide. Otherwise, as Aristotle notes, the earth would "fall" towards the center. Also, the earth must remain motionless at the center of the moving heavens, since its parts are all "balanced" upon the center, and there is no cause of additional motion.[34] The spherical representation of space conditions Aristotle's descriptions of locations and directions, and thus of motions. As a result, it grounds the physical explanations he provides.

[34] *De Caelo* II.14, Ibid., 487f.

Consider, by contrast, a representation of space in which the concepts are referred to a presupposed line or axis, rather than a privileged location. In this case, "down" might mean along the line in one direction, "up" along the line in the other. Similarly, "above" would be further along the line in the "up" direction, "below" would be further along the "down" direction. Here, directions would be self-parallel. The direction described as "up" or "down" in one part of space would be parallel to the direction described as "up" or "down" in another. By the same token, an observer would be able to distinguish a privileged direction in each part of space. In other words, the representation of space is self-parallel and anisotropic.

In this case, space would also be conceived as homogeneous. Without a presupposed privileged location by which other locations could be uniquely specified, there is no way to determinately distinguish different parts of space. One region of space might be correctly described as "higher" than another, but it could also be described as "lower" than a third. More generally, a single region of space can be both "further" and "closer" to other regions. There is nothing inherently distinguishing about the way any location is conceived. No feature of the spatial concept allows a unique specification of place.[35]

This kind of spatial concept could be called *Euclidean*, since its structure is similar to that of "Euclidean" geometry. The label, however, is somewhat misleading, since Euclid himself was ecumenical in his approach to geometry. His methods presupposed, on an equal footing, both lines, in the form of the straight edge, and central points, in the form of the compass point. Someone trying to describe phenomena could appeal to Euclid's proofs, whether his own representation of space was spherical, "Euclidean," or otherwise. In what follows, we will call representations of space that presuppose a privileged line (rather than a point) *rectilinear*.

One example of a rectilinear representation of space is the one employed by Epicurus in his "Letter to Herodotus:"

> Moreover in speaking of the infinite we must not use 'up' or 'down' with the implication that they are top or bottom, but with the implication that from wherever we stand it is possible to protract the line above our heads to infinity without danger of this ever seeming so to us, or likewise the line below us (in

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[35] Consequently, this kind of representation of space is fundamentally relative. An observer can describe locations only relative to other locations. To do so, he or she stipulates a location to serve as a reference point, but whose location is not itself specified.

> what is conceived to stretch to infinity simultaneously both upwards and downwards in relation to the same point).[36]

Epicurus conceives of "up" and "down" in relation to a line protracted "above our heads" and "below us" – i.e., the extrapolation of the head-to-toe axis. "Up" is along the line in one direction, "down" along it in the other. "Above" is further "up;" "below" is further "down." The spatial concepts in his representation of space refer to a presupposed line. Moreover, the directions and other concepts remain "parallel" to themselves throughout the space, since they retain the same relation to the line extended above and below us.

Epicurus, as an intellectual heir of the early atomists, held that space was an infinite void. This infinite space, he argued, could have no boundary and no center. As a result, he explicitly rejects any appeal to privileged locations in his conception of space. Directions stretch to infinity. "Up" and "down" are directions *simpliciter*. They are not to be conceived as toward a "top" or "bottom." There is no *terminus a quo* or *terminus ad quem* by which directions or other spatial concepts can be designated:

> Therefore it is possible to take as one motion that which is conceived as upwards to infinity, and as one motion that which is conceived as downwards to infinity, even if that which moves from where we are towards the places above our heads arrives ten thousand times at the feet of those above, or at the heads of those below, in the case of that which moves downwards from where we are. For each of the two mutually opposed motions is none the less, as a whole, conceived as being to infinity.[37]

There is no "place above our heads" such that motion towards that place is "up." When an object moving upward reaches those "places above our heads," it continues to move "upward" (in the infinite void) even though, having arrived at and passed any given place, it moves away from, rather than toward, any point we might use to identify "up." The same is true for "downward" motion. The directions are "conceived as being to infinity," not toward (or away from) a location. The Epicurean rectilinear representation of space, therefore, is self-parallel, anisotropic, and rigorously homogeneous. As Epicurus writes, "For this [i.e. that there should be a top and bottom] is unthinkable."[38,39]

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[36] Epicurus, "Letter to Herodotus," in *The Hellenistic Philosophers*, ed. A. A. Long and D. N. Sedley (New York: Cambridge University Press, 1987), 10.
[37] Ibid.
[38] Ibid.

This conception of space, of course, is important for Epicurean physics, which holds that the atoms have weight which causes them to forever fall "downwards" through the void. This assumes that the physical space inhabited by the atoms is already oriented such that "downwards" is determinable.[40] In other words, the explanation that atoms naturally fall "down" is based on the rectilinear representation of space which gives meaning to the description "downwards."

This discussion is not meant to suggest that representations of space fall neatly into two categories, spherical and rectilinear. There can be countless variations within each kind of spatial concept. In fact, each author we address will have a slightly different conceptual scheme, and it is the point of this thesis to show that aspects of those representations changed over time. Nevertheless, broadly speaking, the representation of space natural philosophers used to frame their descriptions and explanations of phenomena at the beginning of the seventeenth century was spherical. Authors tended to presume privileged locations – e.g., the "center of the universe" – and conceive of spatial properties and relations by referring to those points. By the end of the period we will discuss, however, the prevailing representation of space was rectilinear. Authors presupposed privileged lines in order to describe and explain phenomena.

Hopefully, the foregoing discussion has dispelled any concerns over the legitimacy of representations of space as a historical reality rather than anachronistic figment. We have tried to show that representations are a necessary part of any physical understanding,[41] since they

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[39] As Max Jammer puts it, Epicurus "...conceived space as endowed with an objectively distinguished direction, the vertical. It is in this direction in which the atoms are racing through space in parallel lines. According to Epicurus and Lucretius, space, though homogeneous, is not isotropic." Max Jammer, *Concepts of Space* (Cambridge: Harvard University Press, 1954), 13. See also Maurice Clavelin, "Conceptual and Technical Aspects of the Galilean Geometrization of the Motion of Heavy Bodies," in *Nature Mathematized*, ed. William R. Shea (Dordrecht: D. Reidel, 1983); David J. Furley, "Aristotle and the Atomists on Motion in a Void," in *Motion and Time, Space and Matter*, ed. Peter K. Machamer and Robert G. Turnbull (Ohio State University Press, 1976); David E. Hahm, "Weight and Lightness in Aristotle and His Predecessors," in *Motion and Time, Space and Matter*, ed. Peter K. Machamer and Robert G. Turnbull (Ohio State University Press, 1976); Peter Machamer, "Aristotle on Natural Place and Natural Motion," *Isis* 69, no. 3 (1978).

[40] The rectilinear structure of space also led to trouble for Epicurean philosophers. Since "downwards" is everywhere parallel to itself, atoms simply falling "downwards" would never encounter one another, and the world would never change. As Lucretius wrote, "…everything would be falling downward like raindrops through the depths of the void, and collisions and impacts among the primary bodies would not have arisen, with the result that nature would never have created anything." "Sideways" motions – the "swerve" – had to be introduced to bring about collisions between atoms that could bring about change. Such motions were uncaused, however, which made their philosophical warrant suspect. See Furley, "Aristotle and the Atomists on Motion in a Void," 96-98; Lucretius, "De Rerum Natura," in *The Hellenistic Philosophers*, ed. A. A. Long and D. N. Sedley (New York: Cambridge University Press, 1987), 11.

[41] Regardless of temporal or cultural context.

enable the description of the phenomena to be described and explained *in space*. Though representations of space can be constrained by the context of a phenomenon, they are not trivial. They are an essential part of physical understanding. Therefore, it behooves the historian of scientific understanding to describe representations of space developed in concert with changes in physical explanations of phenomena.

This, then, is the first part of the thesis to be defended in this project: the first half of the seventeenth century witnessed a profound and rapid shift in the representation of space used by natural philosophers to understand the physical world. A convergent, heterogeneous, and anisotropic spatial concept was replaced by a self-parallel, homogeneous, and (unlike Epicurean space) isotropic[42] framework. That is, space was conceived spherically at the beginning of the period and rectilinearly at its end.[43]

1.1.4 Reciprocal Iteration

Beyond establishing, we hope, *that* a shift in the prevailing representation of space actually occurred during the early modern period, we can try to say something about *how* that shift came about. In other words, we can try to establish the intellectual mechanism that produced the new representation of space.

When we began this project, we assumed that the representations of space employed by various authors would be the results of influences from outside the proper domain of natural philosophy. That is, an author's representation of space would "come from" his thoughts about art or mathematics or something else. We also assumed that shifting representations of space would lead to changing physical explanations. However, upon investigation, we found that,

[42] On a lesser note, we will also point out that the shift from spherical to rectilinear representations of space was accompanied by an abstraction of spatial concepts from the physical objects of the world. In other words, by the end of the period we examine, philosophers no longer believed that the geometrical structures they presupposed in order to describe and explain phenomena were actually instantiated by the phenomena themselves, as in the heavenly spheres or the vertical fall of the atoms. Instead, philosophers recognized that their assumptions about the structure of space were, at least in part, subjective presuppositions. This abstraction brought about a shift from fixed orientations taken to be imposed by the physical furniture of space to arbitrary orientations imposed by those trying to describe the physical furniture. In other words, the shift in representations of space included a move from anisotropy to isotropy. However, the discussion of this move should not be taken as a central part of this thesis.
[43] For a more (mathematically) rigorous characterization of various authors' representations of space (and time) see John Earman, *World Enough and Space-Time* (Cambridge: The MIT Press, 1989).

while "external" influences may have played a part in altering representations of space,[44] considerations within natural philosophy had a more direct effect on representations of space during the period. Moreover, the patterns of intellectual development were reciprocal – not only did representations of space affect physical explanations, but explanations affected representations. The shift from spherical to rectilinear representations of space occurred piecewise, as an iterative reciprocation amongst the three levels of understanding (concepts, descriptions, and explanations) – both within the work of individual authors and in the body of natural philosophy as a whole.

To prepare the ground, we should say something at this point to establish the plausibility of this proposed mechanism. That is, we should establish the possibility of an iterative reciprocation between the levels. From what has been said already, it is almost trivial to claim that explanations depend on descriptions. A change in the way a phenomenon is described entails a change in the way it is explained (or even if it can be explained at all) since the explanation includes at least a partial description of the phenomenon in the explanandum, if not also in the explanans. We can further motivate this point by noting that explanandum-descriptions determine what features of a phenomenon are to be explained. As Hempel writes:

> Indeed, requests for an explanation of the aurora borealis, of the tides, of solar eclipses in general or of some individual solar eclipse in particular, or of a given influenza epidemic, and the like have a clear meaning only if it is understood what aspects of the phenomenon in question are to b/e explained; and in that case the explanatory problem can again be expressed in the form 'Why is it the case that *p*?', where the place of '*p*' is occupied by an empirical statement specifying the explanandum.[45]

We do not attempt to explain every aspect of a phenomenon at once. Instead, we explain particular features of a phenomenon, such as the frequency of the tides. Other features of the phenomenon, such as the height of the tide, or even the color of the water, are left to other, associated but distinct, explanations. In order for an explanation to be provided, it must be clear which features of the phenomenon are in question. Precisely which features of a phenomena are to be explained, meanwhile, are specified by the explanandum-statement – a description of the

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[44] See our discussion of Gilbert and geography, for example.
[45] Carl G. Hempel, *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science* (New York: Free Press, 1965), 334. Here, Hempel is using "explanandum" to refer to the phenomenon itself, whereas we have used the term to refer to the statement describing it, which Hempel himself does elsewhere. See Hempel, *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*, 336.

phenomenon which only includes features we seek an explanation for. The explanans can then account for the features included in the explanandum. The explanandum-description separates the phenomenal features of interest from the folderol of the natural world.

Of course, if we change the way a phenomenon is described, we change the explanation that is given to explain it. In particular, if we change what features of the phenomenon are included in the explanandum, we change what is required of the explanans. Suppose an explanadum is "The apple separates from the tree." In this case, the task is to explain why the separation takes place, and we would refer to dehiscence zones, fruit maturation, temperature, season, and so on. The apple's attraction to the earth is a secondary matter, simply assumed, if addressed at all. If the phenomenon were described as "The apple falls down," however, the attraction would be made the focus of our explanation, and we would account for it by referring to weight, gravity, etc. We would not address the physiology of fruit maturation. The explanation changes relative to the description of the phenomenon.[46]

Changing descriptions can similarly affect explanantia. Since an explanans presumably contains a description, it too would change if that description were to change. For example, if initial conditions were described differently, a different explanans, and thus different explanation, would necessarily result. In the above example, our explanation of the apple's separation from the tree might depend on whether we describe the tree as well-irrigated or drought-stricken.

It is equally clear that descriptions, and thus explanations, depend on concepts. By altering concepts, one can change the way a phenomenon is described and explained. Consider the case of the falling apple. If "down" is conceived as "toward the center," the description "The apples fall down" will be taken to mean that the apple falls toward the center. The phenomenon can then be explained by appealing to the apple's inherent tendency to move toward the center. Suppose, however, that "down" is conceived as "along the head-to-toe axis." In this case, the description is taken to mean that the apple falls along the axis. That is, the meaning of the description changes. In this case, the original explanation is not adequate, since it presupposes that the motion of the apple *is* toward the center of the earth. Even if the ultimate cause of the apple's motion – the apple's natural tendency to move toward the center – is the same, the explanation must include some statement to the effect that the direction "toward the surface"

[46] See Woodward, "A Theory of Singular Causal Explanation."

coincides with the direction "toward the center." The changed concept of "down" affects the meaning of the description "down," and thus the way the phenomenon is explained.

It is only slightly more difficult to establish that intellectual influence can work in the reverse direction – that is, explanations can affect descriptions and concepts. In seeking satisfactory explanations, one might come to accept an explanation or theory. However, acceptance of a certain explanation forces adherence to a particular way of describing phenomena – namely that exemplified by the descriptions appearing in the explanandum and explanans. These descriptions, in turn, rely on a particular set of concepts, which must also be accepted. Hence, to accept a theory entails acceptance of its descriptions and concepts. For example, Kepler, as we shall see, was frustrated by his inability to satisfactorily explain the movement of Mars in its orbit. As a result, he adopted parts of Gilbert's magnetic theory of planetary motion. This theory, however, entailed that the behavior of a planet's magnetic axis be described as a non-motion, rather than a rotation. To describe the phenomenon this way, however, required a novel conception of direction, which Kepler also accepted. In order to put Gilbert's explanations to use, Kepler was forced to adopt Gilbert's description of the phenomenon and the concept of direction that grounded it.[47]

In general, a change at any level of understanding both enables and brings about changes at the other levels. This leads to further adjustments at the level of the original change, which might lead to new changes, and so on. Thus, the whole edifice of understanding – our ability to account for phenomena – advances via an iterative reciprocation between the concepts,

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[47] We will encounter circumstances in which a single phenomenon is described in different ways. Of particular interest will be cases in which a phenomenon is described both as a change or motion and as a non-change or non-motion. In either case, authors are called upon to explain the phenomenon as described. The explanations they provide, however, are quite different depending on the description. In general, a "change" requires a more sophisticated account than a "non-change." The author must explain why the change takes place, and why the change occurs in the particular way it does, rather than simply why the situation is as it is and stays constant. In fact, authors, like Kepler, sometimes change their descriptions of phenomena in response to explanatory difficulties, describing a "non-change" rather than trying to provide an account of a "change," or vice versa.

Note, however, that a shift from describing a "motion" to describing a "non-motion" is not a shift of explanation. One must still explain the cause of the motion or non-motion. It is just that the causes in each instance are usually quite different. Copernicus, for example, ascribed some of the observed changes in planetary positions to the earth, rather than the planets themselves. This was not an explanation. Copernicus's claim was simply that the description "moving" should not apply to the planets, but to the earth. Thus, the planets' motions were merely "apparent," due to the earth's "real" motion. Copernicans still needed to appeal to physical causes to explain the "motion" of the earth, just as Ptolemaic astronomers had appealed to (Scholastic) physical causes to explain the motions of the planets as they had described them.

descriptions, and explanations that constitute it. In this project, we are interested in the iterative advancement of understanding insofar as it involves representations of space.

Putting our general point another way, understanding is relativized to a cognitive framework. Understanding can only be obtained in the context of a particular set of concepts, since concepts are needed to generate descriptions, and thus to constitute understanding. Consequently, if one's conceptual framework were to change, the content of one's understanding would change as well. This relativization of understanding to concepts has been extensively noted in the philosophical and historical literature, especially by those trying to explicate scientific "progress."[48] Many authors have worried that the conceptual change that accompanies changes in theoretical explanations renders comparison of explanatory theories impossible. Others have sought ways to compare explanations despite their conceptual relativity.[49] Thus, the basic notion that understanding relies, at least in part, on concepts, is not new.

This project accepts the assumption that conceptual change underlies the development of explanatory theories. However, we will assume that that the conceptual change accompanying theoretical development is gradual and open to historical investigation, rather than cataclysmic and idiosyncratic, as some have argued. It may well be, to use Hanson's example, that Kepler and Tycho possess different concepts regarding astronomical phenomena, and thus would describe or even experience[50] the rising of the sun differently. We will hold, though, that they each came to hold those concepts via a process of rational contemplation open to historical analysis.[51] The intellectual development that occurred during the seventeenth century was self-motivating. Concepts and theories in the light of natural phenomena exerted intellectual

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[48] We have in mind Sellars, Kuhn, Feyerabend, Hanson, and their successors.

[49] See, for example, Philip Kitcher, "Explanatory Unification and the Causal Structure of the World," in *Scientific Explanation*, ed. Philip Kitcher and Wesley C. Salmon (Minneapolis: University of Minnesota Press, 1989).

[50] I am not sure how one would distinguish between descriptions and experience. What does Tycho experience? If we ask him, he will give us a description (e.g., "The sun moving."). If we conjecture about his thought processes, we speculate about how he would describe or "see" the phenomenon (e.g., Tycho "sees" the sun moving). In either case, the descriptions attributed to Tycho *would* be different from those attributed to Kepler. This descriptive difference, it seems, is the basis for saying they have different experiences. In any case, 400 years later, we have no evidence of Kepler's and Tycho's experience besides their descriptions, so, for the purposes of this project, the question is moot.

[51] We reject Kuhn's contention that the final stage of a paradigmatic shift "how an individual invents (or finds he has invented) a new way of giving order to data now all assembled – must here remain inscrutable and may be permanently so." Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 3rd ed. (Chicago: University of Chicago Press, 1996), 90. It must be noted, though, that Kuhn's notion of a paradigm shift includes, but is not identical to, the conceptual shift we are discussing here. A paradigm comprises shared concepts (and therefore leads to shared descriptions of phenomena and the ability to communicate within a field of study), but it also consists of shared practices that go beyond mere conceptual understanding.

pressures which brought about the advances of the period. This dissertation will trace one feature of the conceptual changes underlying the theoretical achievements of the seventeenth century – the shift in representations of space.

This project, however, is not an attempt to comment generally on the problems related to conceptual change in science. The historical account we will elaborate is specific to our discussion and should not be taken as a general model.[52] We will not claim that the reciprocation follows any strict pattern. The adjustments made at each of the levels by the authors we discuss are, for the most part, peculiar and unique the circumstances. By outlining a process of reciprocal iteration, we only mean to suggest that the process of conceptual and theoretical change can be traced through the works we cite. It is open to investigation, rather than buried in the inscrutable workings of genius.

Finally, we also claim that, since the process of intellectual development was reciprocal, the development of physical understanding – that is, at the level of explanation – was both enabled by and a cause of the conceptual shift we aim to illustrate. The shift in representations of space allowed the development of novel physical explanations, and the development of explanations brought about the shift in representations.

1.2 METHOD OF INVESTIGATION

In this project, we will be trying to identify the representations of space held by various authors, as well as the relationships between their representations and their physical theories. We should comment briefly on the methods we will use to carry out this investigation.

1.2.1 Historical Description

As we have noted, representations of space can vary. Individuals interpret and generate descriptions in the context of their own schema, with possibly different privileged geometrical

[52] On the other hand, there is nothing to prevent this project from being used as *evidence* for a general model of theoretical or conceptual change. That is, this historical study can establish desiderata such models might be expected to satisfy. Models can then be tested against the actual theoretical and conceptual changes described here.

objects and conceptual structures. In fact, spatial concepts may even vary from situation to situation for one individual. We tend, for example, to employ a spherical scheme when observing the stars, but a rectilinear one describing objects in a room. Despite this variation, a particular situation usually presents obvious privileged objects and a convenient geometrical structure. It simply makes sense, for example, to use a spherical geometry to represent the vault of the heavens and a rectilinear geometry in a room with flat walls, floors, and ceiling at right angles to one another. The obvious features of a given situation tend to constrain the frameworks individuals use to a limited set that are similar enough to allow meaningful communication between individuals. Hence, though one person may understand "down" in relation to her head-to-toe axis and another in relation to the center of the earth, both conceive the phenomenon described as "falling down" in roughly the same way – as something like the accelerated motion towards the center of the earth. In other words, the frameworks are *congruent* in this context since applying either set of criteria yields descriptions and explanations that are effectively indistinguishable. They become distinguishable only when the representations of space are directly compared or when they are extended to other contexts, such as in a bed, where the descriptions and explanations they generate diverge.

Since the situation usually constrains the frameworks one uses, generators and interpreters of descriptions only rarely need to explicate the representation of space they use to fix their meanings (so long as the situational context of their description is clear). As a result, spatial concepts are often adopted without comment. Indeed, they may not even be consciously acknowledged. This raises a problem for our project. Only rarely will an author state, for example, that he thinks "the cosmos is spherical." In most cases, the framework an author is assuming must be "read back" from the descriptions he provides. Only by examining the ways in which the author describes phenomena and eliciting the underlying presuppositions can we understand his representation of space.

Notice that spherical and rectilinear representations of space can generally be distinguished by the geometrical entities they take as primitives. Aristotle, for example, assumes that the universe has a center. As a result, the concepts in his representation of space refer to that center as a privileged reference point. Epicurus, meanwhile, assumes the universe is oriented along a privileged vertical axis. His representation of space is thus constructed with reference to that axis. The spherical representation of space presumes a point (or a sphere). The rectilinear representation of space presumes a line.

Therefore, one way we can characterize an author's representation of space is by investigating which geometrical entities are taken as primitives in his conceptual scheme. This suggests several specific questions we can ask of a text in order to discover its underlying representation of space. Most importantly, we might ask what the basic geometrical elements of space are. Does the author presuppose spheres or lines? Does he presuppose fixed points or not? We might also ask how an object is located in space. That is, how is an object's location specified? What objects are referenced by this specification? What features of the objects are important? What geometrical presuppositions are necessary for the specification? Again, we may ask how direction is specified at a given point in space and what objects (geometrical or otherwise) are referenced by this specification. The answers to these questions will all point to the basic elements of the conceptual "structure" of space and, in turn, to the author's representation of space.

Though the representations of space are often merely implicit, and discovered only by interrogating a text, we will find that those describing phenomena sometimes find it necessary to explicitly describe the framework they employ in order to make themselves understood. We shall also see instances where the framework in use is not apparent or is inadequate, rendering the resulting descriptions confusing or even unintelligible. In these cases, deciphering the representation of space at work is easier.[53]

1.2.2 Historical Explanation

In addition to describing the changing representations of space employed by early modern natural philosophers, this project offers to *explain* how the representations of space changed: namely, by reciprocal iteration. This mechanism was described above, and an argument was given to make this explanation at least plausible. It should be noted, however, just how this mechanism will be discovered in the texts here under investigation. That is, the criteria by which

[53] Gilbert, for example, explicitly adds a rectilinear orientation to his implicit spherical geometry. Galileo's spherical framework, meanwhile, is inadequate for his rectilinear physics. See chapters 3 and 5, below.

historical explanations will be identified should be explicated and defended. In this way, the underlying structure of the following account can be brought to the fore.

In the first place, it will be simply assumed that the texts studied below, whatever else they might have been, were, at least in part, works of natural philosophy intended to provide an understanding of natural phenomena. Thus, it is supposed from the outset that the authors tried to provide a satisfactory understanding of facts about the natural world. This assumption should not be very worrisome. In a fairly obvious way, the texts examined below are all *about* the natural world. They discuss physical phenomena, and they at least include assertions about physical causes and principles, however defined. The texts may have been intended to fulfill other tasks, even primarily so, but they do contain some natural philosophy.

This assumption is also very weak. The assumption that the authors *intended* to explain natural phenomena does not say anything about how they went about doing so. In particular, it does not dictate anything about what counted as a satisfactory explanation. The standards used to judge whether a physical explanation was satisfactory and adequate varied according to the intellectual resources and constraints pertaining to each author. Each author held his explanations to different standards of judgment, and these standards were grounded by idiosyncratic considerations. The assumption of a natural philosophical intent does not imply any transcendent (and possibly anachronistic) standard of explanatory adequacy or success. Nor does the assumption imply any of the authors actually achieved explanatory success, even by their own standards. The assumption made here is only about the kind of problems authors were trying to solve. It is not about the solutions they gave.

We can also assume that the authors intended their texts as attempts to solve natural philosophical problems – inability to satisfactorily explain a phenomenon or phenomena given the theoretical and conceptual resources at their disposal. To think otherwise is tantamount to holding that the texts discussed below were (and are) pointless, in which case it becomes impossible to understand why they were written at all. Since, as is assumed, the authors were trying to provide satisfactory understanding, i.e., the ability to generate satisfactory explanations, the failure to do so motivated authors to adjust their explanatory resources (meaning concepts, descriptions, and explanations) in order to bring their understanding into satisfactory alignment with the phenomena. In other words, these problems exerted the intellectual pressure referred to above. Of course, explanatory problems can occur at and generate adjustments at any level of understanding, and each adjustment can lead to new problems, thereby bringing about a reciprocal iteration amongst the levels of understanding. Thus, the intellectual shifts this project describes were, in a historical sense, *caused* by the intellectual pressures exerted by failures of understanding.

For the purposes of this project, then, explaining an intellectual change at any level of understanding entails identifying the intellectual problem the change was meant to overcome. As noted, though, this identification must proceed against the background of an author's own conditions of explanatory satisfaction and adequacy. The problem can only be offered as a plausible explanation of an intellectual change if the author recognizes the problem *as a problem*. Furthermore, the problem can only explain the change if the change solves the problem. In other words, the change must be shown to alleviate the difficulty that the author has been shown to recognize.

Therefore, a historical explanation, in the context of this dissertation, must meet several criteria. First, some difficulty in generating a satisfactory and/or adequate understanding of some phenomenon or phenomena must be identified. Then, it must be shown *that* the author recognizes this problem and *why* the author does so. This entails elaborating the standards of explanatory adequacy employed by the author and demonstrating why the available explanatory resources fail to meet these standards. Finally, it must be shown that the author offers the change in question as a (possible) solution of the problem. That is, it must be demonstrated that the new explanatory apparatus allows explanations that (seem to) meet the standards of satisfaction and adequacy employed by the author. Obviously, satisfying all of these criteria is sometimes quite difficult. Authors typically do not make their motivations explicit, and, even when they do, their statements are often hard to interpret. Instead, authors present their solutions without describing the intellectual problems they are meant to solve. In particular, authors often leave their idiosyncratic standards of explanatory adequacy unelucidated. Hence, as with representations of space, rendering these standards clear requires thorough interrogation of the authors' texts.

Some readers might worry that this methodology assumes knowledge of the inaccessible. In particular, they might hold that any attempt to discern an author's intentions will inevitably be colored by the historian's own *post hoc* perspective. Consequently, any attempt to understand a text in terms of what an author *intends* to do or, more to the point, what problem an author *intends* to solve will be fruitless, at best, and seriously misguided, at worst. Hence, there might be concern that the explanations offered here are distorted by the historian's own assumptions, rather than actual accounts of the historical phenomena themselves.

While it is not possible to completely avoid this objection, perhaps these fears can be minimized. In the first place, whatever the case may actually have been, the authors examined in this project were offering solutions to *some* problems. Therefore, it is at least reasonable to seek *some* problem by which to explain an author's assertions in his texts. A historian has grounds to attempt a plausible reconstruction of the problem that led to the assertions, including the author's standards of explanatory adequacy that make the problem problematic.

This does not completely solve the historiographical problem. Textual interrogation will always fail to fully determine the explanatory problems and, especially, the explanatory standards that motivated an author. Thus, there will always be room for a historian's own perspective and interpretation to pollute his reconstruction of the historical phenomena. Note, though, that the historian's position is exactly analogous to that of the natural philosopher. Both are attempting to generate satisfactory and adequate explanations in the face of underdetermining phenomena. Both might allow assumptions and prejudices to affect their explanations. Both are always subject to correction. Given these concerns, though, the historian, like the philosopher, should not retreat in despair. Rather, he should go ahead and attempt a reconstruction, attempting to bring his own explanatory framework into line with the text.[54] Nevertheless, he should be aware that his successors might reject or modify his understanding for reasons of their own. The explanations given here are not presented as definitive and beyond reproach. They are intended only as the best reconstruction given the reading and reasoning of one historian. Just as the authors studied below brought about a the *physical* understanding of the world by a process of iterative reciprocation, so might the present project become part of the reciprocal iteration that develops the *historical* understanding of their work.

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[54] I am here advocating a position somewhere between those of Skinner and Gadamer. I agree with Skinner that, though the unadulterated reconstruction of an author's intent is the ideal goal of intellectual history, reconstructions are always affected by the historian's own perspective, "paradigm" (in Skinner's term), or "horizon" (in Gadamer's). I agree with Gadamer that the proper hermeneutic strategy is "play" – a continual re-adjustment of understanding in the face of the text that brings historical understanding into line with authorial intent, both by individual and successive historians. A reciprocal iteration is one way of describing the process of play, though it is here used in relation to the natural world, instead of texts. See Hans-Georg Gadamer, *Truth and Method*, trans. Joel Weinsheimer and Donald G. Marshall, 2nd, revised ed. (New York: Continuum, 1989); Quentin Skinner, "Meaning and Understanding in the History of Ideas," *History and Theory* 8, no. 1 (1969); Skinner, "A reply to my critics."

1.3 DISTINCTION FROM THE LITERATURE: JAMMER AND KOYRÉ

We should pause for a moment to say what this project is *not* about in order to distinguish its territory from that already covered by other authors. This is a historical, not philosophical project. As stressed above, we are not interested in providing general models of explanation, causation, or description. Nor are we suggesting any generalized theory of conceptual change. This discussion should be considered only in the particular intellectual/historical context addressed in this project. We have no pretensions of being a cognitive scientist or philosopher of language or mind. We will leave for elsewhere the details about what concepts consist of, how they are formed, and how they function in the conditioning of experience, whether or not others have actually done so. Furthermore, we will not address whether representations of space should be considered as conventions or something else. Nor are we interested in the *a priori*/*a posteriori* and synthetic/analytic status of representations of space.[55] It will suffice, for our purposes, to point out simply that representations of space, even if they are implicit, *must* be employed to make sense of any description or explanation of a physical phenomenon. This project will study *how* they were once employed.

Also, notice that a representation of space is a conception of space, not a concept of "space." We are interested in how ideas about space affect descriptions and explanations of phenomena. We are not concerned with the existence of space itself. Put another way, this is a project in the history of spatial epistemology, not in the history of spatial ontology. Though clearly related, these lines of inquiry pose rather different questions. We will ask how spatial concepts condition an author's descriptions of locations, directions, and so on. We will not ask if an author believes space to be infinite, eternal, void, a plenum, an attribute of God, etc. Our subject is the function of the concept of space, not what the concept of space *is*.

Of course, questions about the ontology of space spawned significant lines of inquiry in natural philosophy during the period we are studying. Up to a point, however, those developments remained separate from the ones that concern us. Metaphysics only applied external constraints on the explanatory projects faced by natural philosophers. Certain kinds of

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[55] Note, though, that the term "representation of space" is taken from Kant's Transcendental Aesthetic, where he argues that a "representation of space" is synthetic and *a priori*. See Immanuel Kant, *Critique of Pure Reason*, trans. Paul Guyer and Allen W. Wood (New York: Cambridge University Press, 1998), 175.

explanation might be ruled in or out on metaphysical grounds. Within these constraints, natural philosophers still had to go about the task of actually fitting explanations to phenomena. For example, Scholastic philosophers as well as Descartes appealed to the perfection of divine creation. This metaphysical position grounded explanations based on the immutability of nature. Yet, for the Scholastics, the immutability of the heavenly spheres explained their continued circular motion, while Descartes maintained that the immutability of God's action implied the conservation of linear motion. In these cases, essentially the same metaphysical position lay behind rather different physical accounts. This is not to say that metaphysics did not influence the physical project – we will have occasion to comment on metaphysical considerations, *especially* in relation to Descartes – but only that discussion of the two projects can be teased apart. The ontic significance of space in early modern thought can be treated separately from the epistemic advances we trace here.[56]

This attitude sets the present project apart from the foremost historical studies of thinking about space during the early modern period: Max Jammer's *Concepts of Space* and Alexandre Koyré's *From the Closed World to the Infinite Universe*.[57] Both authors recognize the fundamental importance of spatial concepts in the understanding of the natural world. They also both acknowledge the distinction between the epistemological and the ontological aspects of the "concept of space." At least for the early modern period, however, they focus on ontological thinking about space, whereas this dissertation addresses the epistemic significance of spatial concepts. Jammer and Koyré leave out the internal physical problems that actually drove the changes we are interested in.

Jammer, for his part, writes that "we may safely regard the concept of space as an elementary and primary notion,"[58] and expresses surprise that "no historical research on the concept of space has been published so far that deals with the history of the subject from the

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[56] Our ability to elaborate a coherent narrative in the chapters that follow is the best argument for this. That said, we will argue in chapter 6 that the epistemic and ontic aspects of the spatial concept became significantly entangled in the work of Descartes.

[57] Other basic treatments include Edwin A. Burtt, *The Metaphysical Foundations of Modern Physical Science*, Rev. ed. (Garden City: Doubleday, 1954); Herbert Butterfield, *The Origins of Modern Science, 1300-1800* (New York: Free Press, 1965); E. J. Dijksterhuis, *The Mechanization of the World Picture*, trans. C. Dikshoorn (Princeton: Princeton University Press, 1961); Arthur Koestler, *The Sleepwalkers* (New York: Macmillan Co., 1968); Stephen Toulmin and June Goodfield, *The Fabric of the Heavens: The Development of Astronomy and Dynamics* (Chicago: The University of Chicago Press, 1961).

[58] Jammer, *Concepts of Space*, 6.

standpoint of physics."[59] To fill the lacuna, Jammer addresses a vast period, from antiquity all the way to the late twentieth century. At least temporally, much of Jammer's work falls outside the scope of this project.

As for the epistemological and ontological aspects of space, Jammer attempts to cover them both:

> It is the purpose of this monograph to show the development of the concept of space in the light of the history of physics. On the one hand the most important space conceptions in the history of scientific thought will be explained and their influence on the respective theories of mechanics and physics will be investigated; and on the other, it will be shown how experimental and observational research – together with theological speculations – affected the formulation of the corresponding metaphysical foundations of natural science as far as space is concerned.[60]

Jammer aims to investigate how the concept of space conditions understanding of the natural world. That is, how different concepts of space affect "theories of mechanics and physics." At the same time, he discusses how the ontology of space – the "metaphysical foundation of natural science as far as space is concerned" – developed over the course of history. In other words, Jammer tries to provide a history of the concept of space as a whole, including both its epistemic and ontic aspects. Even so, he acknowledges that his treatise follows "two more or less independent intellectual developments reaching back to antiquity and coming together in Newton's theory of absolute space."[61]

To be fair, Jammer's treatment of the ancient and medieval periods focuses on the epistemic import of concepts of space. Hence, it is thematically very similar to this project. In fact, Jammer's work constitutes an appropriate prelude to our own. Unfortunately, Jammer interrupts his epistemological study when he addresses the early modern period. At that point, he turns his attention to the ontological and theological aspect of concepts of space. In particular, he is concerned with the influence of platonic and neoplatonic metaphysics on the ontic conception of space. Thus, he addresses figures such as Bernadino Telesio, Franciscus

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[59] Ibid., 1-2.
[60] Ibid., 2.
[61] Ibid., 3. As noted above, we will argue that the entanglement comes in Descartes' relative space, rather than Newton's theory.

Patritius, Giordano Bruno, Thomas Campanella, and Henry More before arriving at Newton, Huygens, Leibniz, and Pierre Gassendi.

On the other hand, Jammer omits those generally thought to be responsible for advancement of *scientific* theories during the seventeenth century – such as Gilbert, Kepler, Galileo, and Descartes (the omission of the last being especially questionable). He does not investigate how concepts of space affected physical explanations during the early modern period, despite the clear implication of his study of earlier periods that they play some role. Jammer only resumes the study of the epistemic features of concepts of space when he examines the period after Newton, where his work is again similar to our own.

Koyré's study is closer to our own in terms of period studied. His narrative begins, aside from a prelude about Nicholas of Cusa, with Copernicus and ends with Newton and Leibniz. Thematically, however, Koyré takes a very different tack. Like Jammer, Koyré separates the intellectual developments of the early modern period into two related but distinct strands. He claims that the "changes [in the early modern world-view] brought forth by the revolution of the seventeenth century" are "reducible to two fundamental and closely connected actions that I characterised [in *Galilean Studies*] as the destruction of the cosmos and the geometrization of space."[62] The "destruction of the cosmos," he goes on to explain, refers to

> ...the substitution for the conception of the world as a finite and well-ordered whole, in which the spatial structure embodied a hierarchy of perfection and value, that of an indefinite or even infinite universe no longer united by natural subordination, but unified only by the identity of its ultimate and basic components and laws.[63]

The "destruction of the cosmos," then, is a characterization of a shift in the ontological considerations of the world that occurred during the seventeenth century. This shift revolved around the finitude of the heavens, the ethical ordering of the universe, and (because He is the ultimate arbiter of such ordering) God's role in the world. The "geometrization of space," meanwhile, is "the substitution for the concrete space of pregalilean physics of abstract space with Euclidean geometry."[64,65] (By "concrete space," Koyré refers to the spherical geometry

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[62] Alexandre Koyré, *From the Closed World to the Infinite Universe* (Baltimore: Johns Hopkins University Press, 1957), viii.
[63] Ibid.
[64] Alexandre Koyré, *Études Galiléennes* (Paris: Hermann, 1966), 15. In *Closed World*, "geometrization of space" is defined as "the replacement of the Aristotelian conception of space – a differentiated set of innerwordly places – by

instantiated by the celestial and elemental spheres postulated in Scholastic physics.) In other words, the "geometrization of space" is Koyré's term for the very epistemic shift we aim to describe in this project. It comprises the move from spherical to Euclidean representations of space. Koyré recognizes the fundamental role played by this shift in the early modern development of physical understanding.

However, Koyré supposes the epistemic shift is a derivative of the ontological one. That is, the "destruction of the cosmos" leads to the "geometrization of space:"

> This [destruction of the cosmos], in turn, implies the discarding by scientific thought of all considerations based upon value-concepts, such as perfection, harmony, meaning and aim, and finally the utter devalorization of being, the divorce of the world of value and the world of facts.[66]

In Koyré's view, the eschewal of value judgments about the physical world forced philosophers to abandon "all considerations based upon value-concepts." In particular, they had to discard any reference to privileged places. In a "devalorized" cosmos, no location is inherently different from any other. Hence, philosophers could no longer employ the heterogeneous, spherical representation of space to describe and explain phenomena. Such a representation presupposes a special location – the center – but this assumption was no longer legitimate. In its place, says Koyré, philosophers were forced to adopt the homogeneous, isotropic Euclidean representation of space. The ontological move caused the epistemic one. The "destruction" brought about the "geometrization."

From the Closed World to the Infinite Universe, therefore, is a book about the seventeenth century shift in the ontic conception of space. Koyré describes how shifting metaphysical conceptions of space led to an acceptance of an infinite, homogeneous void. He does not discuss in any detail how the changing epistemic conception of space affected the physical understanding of the period. Indeed, he barely discusses physical theories at all, focusing instead, like Jammer, on the metaphysical and theological positions of philosophers like

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that of Euclidean geometry – an essentially infinite and homogeneous extension – from now [i.e., the seventeenth century] on considered as identical with the real space of the world." Koyré, *From the Closed World to the Infinite Universe*, viii.

[65] The term "geometrization of space" is perhaps misleading. It implies that geometrical conceptions were not applied to space – i.e., space was not "geometrized" – before the early modern period, a suggestion that is clearly not true. The celestial spheres were geometrical entities as much as they were (hypothetical) physical ones. Not to mention the geometrical reasoning of astronomers like Ptolemy.

[66] Koyré, *From the Closed World to the Infinite Universe*, 2.

Nicholas of Cusa, Thomas Digges, Giordano Bruno, Henry More, and Joseph Raphson. Even those, like Gilbert, Descartes, Newton, who made significant contributions to physics capture his attention only insofar as they advanced metaphysical theories of space.

In the end, Koyré may be right. The ontological development of concepts of space may have caused the epistemic development of representations of space. Certainly, the two issues became significantly entangled at some point during the period (though we are inclined to place this point relatively late, after the development of rectilinear representation of space[67]). Even if this is so, however, there is no reason *not* to study the epistemological shift as it occurred. Just because the "geometrization" of space is assumed to be an effect rather than a cause does not mean it is useless to undertake a study of that effect. Having done so, moreover, one finds that Koyré's assumption that the ontic is prior to the epistemic is, at best, in need of revision. As we hope to show, philosophers, at least before Descartes, altered their representations of space not in response to metaphysical commitments regarding space, but because of epistemological difficulties they encountered while trying to understand nature. It is too much of a simplification to dismiss, as Koyré does, the contributions of Kepler and Galileo toward a rectilinear conception of the world simply because they fail to move toward a belief in the infinite void.[68] At least in these cases, the epistemic shift was orthogonal to the ontic.

This project, therefore, covers significantly different territory than those by Jammer and Koyré. It concerns the development of spatial epistemology during the first part of the seventeenth century in relation to the advance of physical understanding. It avoids discussing the development of spatial ontology, however, which is the primary interest of Koyré and, at least during the same period, Jammer. It follows the line of intellectual development those authors acknowledge but neglect.

[67] See chapter 6.

[68] As Koyré notes, Kepler believed that space was finite and Galileo had nothing to say one way or the other. As a result, for Koyré's historical thesis, Kepler is a recalcitrant anomaly and Galileo is irrelevant. The present project will treat them quite differently. See Ibid., chs. 3,4, esp. p. 95.

1.4 PLAN OF CHAPTERS

We begin our account in the following chapter with a discussion of Copernicus, who recognized a deep conflict between Scholastic explanatory principles and the mathematical descriptions of planetary phenomena in Ptolemaic astronomy. Copernicus attempted to rectify the situation by replacing the Ptolemaic descriptions with his own, which, he thought, could be reconciled with Scholastic explanations. Though influenced by neoplatonic theories, Copernicus believed he was adhering in many ways to the Aristotelian physics that prevailed amongst his learned peers. Nevertheless, Copernicus's new astronomical hypotheses raised difficulties at both the explanatory and conceptual levels, with which subsequent authors were forced to deal. Yet, most of Copernicus's successors thought that he did not provide an adequate explanation of the phenomena he described. They sought ways to fill this theoretical hole in his theory. Moreover, Copernicus described astronomical phenomena by presupposing "many centers." That is, he employed at least two different representations of space, each constructed around its own center. Though each of these frameworks was spherical, they could not be brought together into a single conceptual structure applicable to the whole. Copernicus's successors were forced to adjust their concepts of space to render his descriptions commensurable with themselves and with the explanations they provided. As we shall see, these explanatory and conceptual difficulties are especially well-illustrated by the "Third Motion" of the earth, which was a problem of significant interest to those following Copernicus.

The third and fourth chapters examine the rectification of celestial and (ultimately) gravitational phenomena, beginning with the work of William Gilbert. We provide a detailed exposition of Gilbert's *De Magnete*, where he employs what we call a *geographical* representation of space appropriate to his subject matter. Our main interest, though, is Gilbert's treatment of the "Third Motion." Faced with an inability to satisfactorily explain this phenomenon as described by Copernicus, Gilbert described the same phenomenon as a stasis. To do so, though, Gilbert added a presupposed rectilinear direction to his conception of space. This conceptual move simplified Gilbert's explanatory task, and it constituted a move toward a rectilinear representation of cosmic space.

Johannes Kepler, as we discuss in the fourth chapter, sought a reconciliation of astronomical explanations and descriptions. This project, as Kepler described in a 1605 letter to

David Fabricius, was frustrated by a problem similar to that posed by Copernicus's "Third Motion." At a crucial juncture, Kepler was inspired by Gilbert's solution to that difficulty. The solution, though, entailed the use of a rectilinear representation of space to describe and explain an important aspect of the planetary motions.

Turning to the effect of Copernicanism on inertial phenomena, we examine the work of Galileo in the fifth chapter. Galileo sought to explain phenomena on a moving earth. To do so, he appealed to both "natural" and "impressed" theories of motion, which he brought together under the single rubric of "inertia." As evidenced by Galileo's fleeting adumbrations of the deflection of projectiles (the Coriolis effect), however, he could not reconcile the spherical representation of space underlying "natural" explanations of motion with the linear geometry grounding appeals to "impressed" motion. Despite intimations of linearity, Galileo adhered to a spherical representation of space as the more fundamental way to conceive of the world.

In the sixth chapter, we show that René Descartes, perhaps influenced by his early work in optics and geometry proper, adopted a truly rectilinear representation of space in order to describe and explain inertial phenomena. He uses this spatial concept to describe and explain small scale phenomena – namely, collisions between individual bodies, which he takes to be the sole physical interactions in the natural world. We shall also see how his rectilinear representation of space becomes associated with metaphysical considerations about the basis of physical laws and our knowledge of them. Descartes, however, does not know how to extend his rectilinear treatment of individual collisions to the behavior of ensembles of bodies. As a result, he continues to use a spherical framework when dealing with large-scale phenomena including many objects. Nevertheless, we find in Descartes' work, for the first time, a space that is self-parallel, homogeneous, and isotropic.

Finally, in the seventh chapter we will summarize the progress from spherical spatial concepts to rectilinear representations of space described up to that point. We will then suggest further lines of inquiry not covered by this dissertation. In particular, we will try to hint how the representations of space used by Kepler, for celestial/gravitational phenomena, and Descartes, for terrestrial/inertial phenomena, were further adapted by Isaac Newton and Christiaan Huygens. We leave a more detailed treatment of those topics, however, to elsewhere.

2.0 *PLURIBUS ERGO EXISTENTIBUS CENTRIS*: COPERNICUS, ASTRONOMICAL DESCRIPTIONS, AND THE "THIRD MOTION"

By the beginning of the Renaissance, Scholastic philosophers had constructed a broad system of physical understanding founded on Aristotelian appeals to "natures." This adherence to Aristotelian explanatory principles was accompanied (as described in the previous chapter) by a commitment to the spherical representation of space upon which they were based. The resulting Scholastic understanding of nature constituted a remarkably powerful and coherent edifice. Every physical body had its proper place in a single, universal order, determined by the place's distance to the center. The behavior of all bodies could then be explained with respect to these "natural places." Heavy terrestrial bodies, for example, tend in straight lines toward their natural places near the center. Light bodies tend toward their places further from the center. Celestial elements, meanwhile, rotate circularly about the center, maintaining the place they already inhabit. By supposing a natural order centered on a single point, the Scholastic physics could account for phenomena throughout the physical world, in both the superlunary and sublunary realms.

In the superlunary realm, the prevailing descriptions of phenomena were those due to the Ptolemaic astronomers, who could provide a reasonably accurate account of each planet's motion. Moreover, the Ptolemaic geocentric hypotheses[1] accorded well with the conceptual framework underlying the Scholastic physics, which also placed earth at the center. Nicolaus Copernicus, however, noticed a subtle discrepancy between the Ptolemaic descriptions of planetary motions and the Scholastic/Aristotelian principles meant to explain them. Specifically, Copernicus thought that Ptolemy's equant point, a mathematical device used to describe the

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[1] We are speaking broadly here. The Ptolemaic astronomy is only roughly geocentric, since the centers of its planetary models do not exactly correspond with the center of the earth. Nevertheless, the system is geocentric in the sense that all the planets orbit around the earth. See J. L. E. Dreyer, *A History of Astronomy from Thales to Kepler* (New York: Dover, 1953), ch. 9; O. Neugebauer, *The Exact Sciences in Antiquity* (Providence: Brown University Press, 1957), 191-207.

unequal motion of the planetary orbits, violated the Aristotelian principle that celestial bodies rotate uniformly. As a result, Copernicus sought to alter the Ptolemaic descriptions of phenomena in order to do away with the equant and bring them into line with the explanatory principles proposed by Aristotle.

To do so, however, Copernicus was forced to reject the Ptolemaic descriptions of planetary phenomena and the unitary geometrical system upon which the descriptions were based. Though he remained committed to a spherical representation of space, Copernicus bifurcated the universe into two (if not more) distinct realms, each with its own geometrical center: the celestial realm centered by the sun, and the terrestrial realm centered by the earth. This disjointed geometry, in turn, raised problems for the Scholastic physics Copernicus sought to retain, since it relied on the assumption of a single center. In order to preserve the universal ordering of bodies and the explanations based on it in light of Copernicus's geokinetic hypothesis, philosophers subsequently had to decide which was the true center of the universe – the earth, the sun, or some other point – or if the universe had a center at all.

This general difficulty was exemplified in particular by the trouble surrounding Copernicus's description of the "third motion" of the earth. This "motion" is in fact an artifact of Copernicus's representation of space. Given the spherical framework with which he was working, Copernicus described the behavior of the earth's axis as a motion. However, Copernicus could not explain the motion he had described. His successors were forced to explain the motion – a difficult task, since it did not admit an obvious cause.

Copernicus himself did not intend to cast doubt on the system of Scholastic explanations of physical phenomena. On the contrary, he was trying to strengthen the physical basis of astronomy by appealing to the Scholastic system. Nevertheless, his work raised unforeseen and significant problems. While he solved neither the general problem of centers, nor explained the third motion, Copernicus posed questions subsequent authors were forced to answer. Eventually, the answers to these questions brought about the remarkable shift from spherical to rectilinear representations of space and the attendant demise of Scholastic physics.

2.1 SPHERICAL UNIVERSE

In the *De Revolutionibus*, it is clear that Copernicus envisions a spherical universe. The book's first chapter is entitled "That the universe is spherical," and it presents several arguments for the assertion:

> First we must remark that the universe is globe-shaped, either because it is the most perfect shape of all, needing no joint, an integral whole; or because that is the most capacious of shapes, which is most fitting because it is to contain and preserve all things; or because the most finished parts of the universe, I mean the Sun, Moon and stars, are observed to have that shape, or because everything tends to take on this shape, which is evident in drops of water and other liquid bodies, when they take on their natural shape. There should therefore be no doubt that this shape is assigned to the heavenly bodies.[2]

None of these arguments presents any compelling empirical evidence for the sphericity of the universe. Instead, they rely on *a priori* assumptions such as the perfection of the sphere and the universe, or the "natural" shape of bodies. On the other hand, Copernicus had no pressing need to support his contention. None of his contemporaries would have seriously challenged him. The spherical shape of the heavens was a long-accepted truth. Indeed, Copernicus is simply repeating arguments similar to those found in classical sources, including Aristotle, Plato, and Ptolemy.[3] Copernicus, then, is just *assuming* that the universe is spherical.

This assumption indicates that Copernicus, like Aristotle (and Ptolemy), employs a spherical representation of space. In spherical representations of space, a center is presupposed. Locations and directions are then specified by reference to that center. Locations are given by distances from the center. Directions are specified by reference to a radius to the center. In effect, this means direction at a given point is specified by an angular deflection from a radius from that point to the center. For example, the direction toward the center is 0° deflected, that away from the center is 180° deflected, and the circular direction around the center is 90° deflected.

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[2] Nicolaus Copernicus, *On the Revolutions of the Heavenly Spheres*, trans. A. M. Duncan (London: David & Charles, 1976), 36.

[3] Compare Copernicus's statement that the sphere is "the most capacious of shapes", for example, with Ptolemy's argument in the *Almagest*: "since of different shapes having an equal boundary those with more angles are greater [in area or volume], the circle is greater than [all other] surfaces, and the sphere greater than [all other] solids; [likewise] the heavens are greater than all bodies." See Claudius Ptolemy, *The Almagest*, trans. G. J. Toomer (New York: Springer-Verlag, 1984), 40. (Brackets are translator's interpolations.)

We can see this geometrical conception of space throughout the *De Revolutionibus*, where motions and locations are always specified in relation to a presupposed center. For example, in Copernicus's "order of the heavenly spheres," the sun is stipulated (for a variety of reasons) as the center. The "first and highest" of the spheres, that is, the sphere furthest from the center, is that of the fixed stars. The next sphere, closer to the center, is the sphere of Saturn, "the highest of the wandering stars." The other planets are similarly described, each "below" the last. In each case, the location or "height" of a sphere is described by reference to the center – the sun.[4]

The spherical representation of space also allows Copernicus, like Aristotle, to specify three "simple" directions. The direction directly toward the center has the same description at all points of space – in effect, 0° deflected from a radius to the center. The same is true for the radial direction away from the center (everywhere 180° deflected), and the circular direction around the center (everywhere 90° deflected). These simple directions lead, as in Aristotle,[5] to descriptions of "simple" motions:

> Further, of simple motions, one kind is up, another down. Wherefore every simple motion is either towards the middle, which is down; or away from the middle, which is up; or about the middle and is itself circular... That is Aristotle's theory.[6]

A motion toward, away from, or circularly around the center always maintains the same angular deflection. Hence the description applicable to the direction of these motions is always the same. That is, motion in these directions is "simple" – the *direction* does not change.

Copernicus uses these "simple" motions, which result from his spherical representation of space, to describe phenomena. Consider, for example, Copernicus's description of falling bodies. He believes that the earth rotates daily about its axis. Falling bodies, however, also move towards the center of the earth. As a result, he argues, falling terrestrial bodies have a "compound motion":

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[4] See Copernicus, *De Rev*, 46-51.
[5] *De Caelo* I.2, Aristotle. *The Complete Works of Aristotle*, 448. See section 1.1.3, above.
[6] Copernicus, *De Rev*, 43.

> We have indeed to admit that the motion of falling… bodies is a dual motion in comparison with the universe, and is no less a compound of straight and circular motion.[7]

In other words, the real motion of the falling body, which is some unspecified trajectory both around and toward the earth's center, can be described as a dual motion consisting of two simple motions – the circular motion around the earth's center and a linear motion towards the center. Since the actual motion of the body is complicated, i.e., not "simple," it is described as a composition of two simple motions, each following the radial/circular geometry of the presupposed spherical representation of space.[8]

It should be mentioned that Copernicus's statement in this context that the sphere of the fixed stars is "the location of the universe, to which the motion and position of all the remaining stars is referred"[9] is misleading. What Copernicus means is that the location of a planet is specified by referring to a fixed star or constellation, as in "Jupiter is in Libra." This does not imply, however, that Jupiter is really amongst the stars of Libra. Instead, the description implies that Jupiter is along a radius connecting the assumed center, whatever it may be, with the constellation Libra. Similarly, a description of the planet's speed, e.g., "10° per month," is a specification of the angular velocity at which the radius from the center, passing through the planet, and extending to the fixed stars, rotates around the center. Thus, the center is always presumed in the representation of space underlying Copernicus's specifications of motions and positions. The sphere of the fixed stars is only "the location of the universe" in that it is the fixed background by which these specifications are made. Both the center *and* the fixed sphere must be presumed in order to understand descriptions of motion and position.

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[7] Ibid., 45.
[8] Copernicus is careful to point out that this composition is not real, but merely "intellectual." It is the *description* of the motion that can be decomposed, not the real motion itself. He compares this kind of distinction to that "between a line, a point and a surface," even though "one cannot exist without the other, and none of them without a body." That is, we can describe lines, points, and surfaces, but these only really exist intermingled in real bodies. Similarly, we can describe the motion of the falling body as a composition of linear and circular, but the two motions only really exist intermingled in the real motion. See Ibid.
[9] Ibid., 49.

2.2 SCHOLASTIC PHYSICS

Copernicus aims to use Aristotle's physics to explain celestial phenomena.[10] Thus, in addition to an Aristotelian representation of space, Copernicus also adheres to Aristotelian physical principles. On this view, all motions are to be explained by what is "natural" for a mobile. Bodies with simple natures, for example, will have simple motions – motions that, as we have seen, do not change direction (as described on the basis of a spherical representation of space). Such bodies will move either linearly towards or away from a center, or circularly around a center. These motions are explained by appealing to the bodies' simple natures. They move simply because they are *naturally* simple.

Copernicus argues, for example, that the celestial spheres, which he believes carry the planets about, rotate because circular motion is the natural motion of a sphere:

> The next point is that the motion of the heavenly bodies is circular. For the movement of a sphere is a revolution in a circle, expressing its shape by the very action, in the simplest of figures, where neither beginning nor end is to be found, nor can be the one be distinguished from the other, as it moves always in the same place.[11]

The celestial spheres are simple bodies, expressed in the "simplest of figures." Their natural motion, therefore, must be one of the simple motions: toward, away from, or around the center. The heavenly spheres, however, are already and forever in their natural places. Their natural motion, therefore, is motion in place – that is, rotation about the center. Thus, the motions of the planets are understood to be caused (i.e., explained) by the natural rotation of the spheres carrying them about.

Of course, the motion of the planets is not simply circular and uniform. They speed up and slow down. They oscillate between north and south. The superior planets (Mars, Jupiter, and Saturn) usually move west to east, but sometimes stop, move east to west, stop again, and resume their usual motion across the field of fixed stars (a phenomenon called retrograde motion). Still, all the planets return, time after time, to the same parts of the sky in regular and

[10] Edgar Zilsel has detailed Copernicus's adherence to the "teleological conception of nature" that is the hallmark of Aristotelian explanation, especially with regard to celestial motions. See Edgar Zilsel, "Copernicus and Mechanics," *Journal of the History of Ideas* 1, no. 1 (1940).

[11] Copernicus, *De Rev*, 38-9.

predictable patterns. Despite the various, complicated movements of the heavens, this regularity can only be accounted for by circular motion:

> Nevertheless it must be admitted that their motions are circular, or compounded of a number of circles, because they pass through irregularities of this kind in accordance with a definite law and with fixed returns to their original positions, which could not happen if they were not circular. For only a circle can repeat a previous state of affairs…[12]

The irregularities of planetary movements may be due to the motion of several interconnected spheres, each contributing to the motion of the planets, but each motion is, in itself, circular. Only circular motion can explain the cyclical movements of the heavens as they return, over and over, to their former positions.

These circular motions, meanwhile, must all be uniform around their centers. That is, the angular velocity of each motion, measured from the center of the motion, must be constant. Other kinds of motion, Copernicus insists, simply cannot be explained. There is no possible way to account for non-uniform circular motion:

> …it is impossible for a heavenly body which is simple to move irregularly in a single sphere. That would have to be due either to changes in the moving power, whether derived from elsewhere or from its intrinsic nature, or on account of unevenness in the revolving body. Both these possibilities are unacceptable to the reason, and it is inappropriate to attribute such a thing to bodies which are established in an ideal state.[13]

The celestial spheres are ideal and simple bodies, without part or joint. They do not have, therefore, any internal "unevenness" that could explain irregular motion. Nor can irregular motion be attributed to the source of a sphere's motion, since the spheres move only according to their internal, simple, and therefore (again) uniform, nature. As Copernicus puts it:

> It must therefore be agreed that though their motions appear to us irregular they are regular…[14]

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[12] Ibid., 39.
[13] Ibid.
[14] Ibid.

Celestial motions are only explicable if they are thought to be uniform. In other words, the complicated, irregular motions of the planets can only be properly *explained* if they are *described* as compositions of uniform, circular motions.

2.3 PTOLEMY'S DESCRIPTIVE AIMS

The requirement of uniform motion was the crux of Copernicus's rejection of Ptolemaic astronomical descriptions. To account for the various irregularities of the planetary motions, Ptolemy had employed three geometric models of planetary orbits: the eccentric, the epicycle, and the equant. The first consisted of a circle rotating uniformly around a geometrical center which is not the bodily center of the orbit. In the second model, the planet orbits uniformly on a small circle, the epicycle, the center of which rotates uniformly around the center of the orbit on a circle called the deferent. In the last model, a circle rotates with a constant angular velocity measured, not at the center of the circle, but at another point, called the equant. Each of these models could account for a part of the planet's irregularity and, combined, they accounted for the three apparent motion for each planet. Thus, for example, Ptolemy's models for the superior planets consist of an epicycle moving on an eccentric deferent. Meanwhile, the speed of the center of the epicycle around the deferent is uniform at an equant point that is neither the earth, the bodily "center" of the orbit, nor the center of the deferent.[15]

In the *Almagest*, Ptolemy is explicit that his ultimate aim is not a theoretical explanation of planetary motion, but a description adequate for prediction:

> Rather, if we are at any point compelled by the nature of our subject to use a procedure not in strict accordance with theory… or [if we are compelled] to make some basic assumptions which we arrived at not from readily apparent principle, but from a long period of trial and application, or to assume a type of motion or inclination of the circles which is not the same and unchanged for all planets; we may [be allowed to] accede [to this compulsion], since we know that this kind of inexact procedure will not affect the end desired, provided that it is not going to result in any noticeable error; and we know too that assumptions made without proof, provided only that they are found to be in agreement with the phenomena, could not have been found without some careful methodological procedure, even

[15] See Ptolemy, *The Almagest*, 480ff.

> it is difficult to explain how one came to conceive them (for, in general, the cause of first principles is, by nature, either non-existent or hard to describe)…[16]

Thus, for Ptolemy, the "end desired" is the absence of "noticeable error." Mathematical expedients found by "trial and application," without basis in theory or "readily apparent principle," are acceptable so long as the results "are found to be in agreement with the phenomena." In other words, the ultimate goal is descriptive accuracy – accurate description of planetary positions, so that the planet is, quite simply, where you say it is at any given moment. Explanation by appeal to first principles is merely incidental.[17]

Without general astronomical principles by which similar models can be constructed for all the planets, Ptolemy is willing to countenance a wide variety of models to describe the wide variety of planetary motions:

> …we know, finally, that some variety in the type of hypotheses associated with the circles [of the planets] cannot plausibly be considered strange or contrary to reason (especially since the phenomena exhibited by the actual planets are not alike [for all])…

Ptolemy admits models that do not apply to all of the planets equally. The models cannot be said, therefore, to obey unifying principles. This is not surprising, though, given that the planets exhibit a wide variety of phenomena.

Nevertheless, Ptolemy clings to the more general Aristotelian physical principle of celestial motion – uniform rotation.:

> …for, when uniform circular motion is preserved for all without exception, the individual phenomena are demonstrated in accordance with a principle which is more basic and more generally applicable than that of similarity of the hypotheses [for all planets].[18]

Even though there may be no general astronomical principles by which similar hypotheses may be constructed for all the planets, all models preserve the principle of uniform circular motion.

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[16] Ibid., 422-23.

[17] In a separate text, the *Planetary Hypotheses*, Ptolemy did attempt a fuller elaboration of the heavenly spheres causing the planetary movements. However, the few extant portions of this work only came to the attention of Western scholars in the early twentieth century. See Bernard R. Goldstein, "The Arabic Version of Ptolemy's Planetary Hypotheses," *Transactions of the American Philosophical Society* 57, no. 4 (1967); Alexander Jones, *Provisional Translation of Ptolemy's Planetary Hypotheses, Book 1 Part 1* (2004 [cited 2005]); available from http://www.chass.utoronto.ca/~ajones/ptolgeog/PlanHyp1.pdf.

[18] Ptolemy, *The Almagest*, 422-23.

Each element of each model – eccentrics, epicycles, and equants – can be said to rotate uniformly.

2.4 COPERNICUS'S REDESCRIPTION

Here, Copernicus objects. He has no trouble with eccentrics and epicycles, but equants, in his view, are a mathematical fudge by which one calls uniform that which is not uniform at all. Speaking of Ptolemy's model for lunar motion, which includes an epicycle moving about an eccentric deferent with a motion uniform only at an equant point, Copernicus writes:

> …[W]hat shall we reply to the axiom that 'the motion of the heavenly bodies is regular, except when it seems to be irregular as far as appearance is concerned', if the apparently regular motion of an epicycle is in actual fact irregular, and exactly the opposite to the established principle and assumption happens? But if you say that it moves regularly around the centre of the Earth, and that takes care of the need for regularity satisfactorily, then what kind of regularity will it be which is on a circle not its own, although its motion is not on that circle but on its own eccentric?[19]

Ptolemy's "regular" motion, Copernicus argues, is not regular at all. For Ptolemy has an epicycle rotating uniformly only with respect to an equant, which is neither the centre of the Earth, nor the center of its orbit (the deferent). Thus, the epicycle may sweep equal arcs of a circle centered on the equant in equal times, but this is not a truly regular motion, since the epicycle itself will travel unequal arcs of the deferent in equal times. Ptolemy might be satisfied that the motion is regular about *some* point, but this is a purely mathematical conceit, not uniform motion. Indeed, Copernicus claims, Ptolemy has violated Aristotle's fundamental "axiom" of planetary motion, circular uniform motion.

Ptolemy saw explanation on the basis of physical principles as incidental. He was willing to countenance mathematical expedients in order to give adequate descriptions of appearances. Copernicus is not so flexible. He aims to place astronomy on the firm basis of explanatory principles:

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[19] Copernicus, *De Rev*, 188.

> They [Ptolemaic astronomers] therefore concede in this instance that the regularity of a circular motion can refer to centre which is alien and not its own… But this has already been sufficiently refuted in relation to the Moon. This and similar points occasioned us to think about the mobility of the Earth and other means of preserving regularity and the first principles of our science…[20]

The equant model of planetary motion does not admit explanation on the basis of uniform motion. Hence, Copernicus aims to eliminate its use from astronomy, thereby "preserving regularity and the first principles." Copernicus is trying, unlike Ptolemy, to reconcile astronomical descriptions with explanations of planetary motions. He is no radical, however. The explanations he seeks are Aristotelian – uniform, circular celestial motions resulting from the simple and perfect nature of the heavenly spheres. Thus, Copernicus is not trying to generate new explanations for phenomena. Rather, he is trying to create new *descriptions* for phenomena that are compatible with and susceptible to explanation by classical physics – the "first principles of our science."

In Ptolemaic astronomy,[21] planetary longitudinal positions are calculated on the basis of the planet's mean motion and its total irregularity or "anomaly."[22] Given a date and time, one simply combines the mean motion and anomaly of a planet (obtained from a table or by calculation) to generate the position of the planet. Ptolemy, meanwhile, separates the total anomaly into two parts. First is the synodic anomaly, which, in modern terms, accounts for the fact that the sun, not, as Ptolemy assumed, the earth, is the center of the solar system. Since this anomaly depends on the location of the sun,[23] it is the same for all the planets. Second is the ecliptic anomaly, which accounts, again in modern terms, for the fact that planetary orbits are really elliptical and irregular, rather than circular and uniform. For superior planets, Ptolemy uses an epicycle model to account for the synodic anomaly. The ecliptic anomaly, meanwhile, consists of two corrections – one generated by an eccentric, the other by an equant. For Venus, the ecliptic anomaly is accounted for by an epicycle, while the synodic anomaly consists of

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[20] Ibid., 238.

[21] In the following discussion, I am ignoring Ptolemy's discussion of the moon and Mercury. The moon, of course, really does orbit the earth, so its anomalies arise from slightly different reasons than the other planets. Mercury's orbit is decidedly more complicated than the other planets'. It is a glaring exception in both Ptolemy's and Copernicus's systems, since both resort to oscillations not found in any other planetary model in order to account for Mercury's motion.

[22] The mean motion of the planet is the motion it would have if it moved with constant angular velocity around the orbital center (i.e., the Earth). The rate is equal to 360° over the orbital period. The anomaly is the angular deflection from the mean motion due to the non-uniformity of the planet's actual motion.

[23] Actually, in Ptolemy's system, it depends on the mean motion of the sun.

corrections generated by an eccentric and an equant model. Thus, each of the planets' anomalies consists of three angular corrections to the mean motion, generated, respectively, by an epicycle, an eccentric, and an equant.[24]

To remove equants from Ptolemaic astronomy, Copernicus must remove one of the corrections for each planet, leaving the other two to be generated by an epicycle and an eccentric. He assumes that the earth is in motion around the sun. Thus, the synodic anomaly for each planet is not generated by an irregularity in the planet's motion, but is a result of the earth's motion. Thus, it is merely an "apparent" irregularity, and the "axiom that 'the motion of the heavenly bodies is regular, except when it seems to be irregular as far as appearance is concerned'" is preserved. In other words, by putting the earth in motion, Copernicus redescribes the observed phenomena. The irregularities in planetary motion associated with solar position are now described as "appearances," rather than real motions. Thus, the synodic part of the total anomaly can be eliminated from the planetary models, leaving the ecliptic anomaly.

Copernicus models the planetary orbits using an epicycle and an eccentric deferent around the sun, each rotating uniformly around its own center.[25] As a result, Copernicus can describe the orbit of each planet without appeal to equants. He is left with uniform circular motions, albeit around several centers, which permit explanation by Aristotelian principles of celestial motion.[26]

The point to be made here is that Copernicus's achievement is only a redescription of phenomena. He rejected the Ptolemaic theory because its descriptions of the phenomena did not admit explanation on the basis of Aristotelian physics. In place of Ptolemaic astronomy, Copernicus introduces a system of descriptions in which the phenomena – i.e., planetary positions – are described on the basis of the earth's motion, as well as that of the planets themselves. This makes the description of the phenomena at least compatible with existing physical theories.

Copernicus, however, does not offer any new explanations of phenomena. He does not seriously try to expand or revise Aristotelian explanations of planetary motions. In fact, Copernicus makes notoriously little attempt to explain *anything* regarding his models. He almost

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[24] See Figure 1.
[25] Again, excluding Mercury.
[26] See Figure 2. For a general summary of Copernican astronomy, see Dreyer, *A History of Astronomy from Thales to Kepler*, ch. 8.

never specifies the causes of the motions he describes. He simply assumes that the "first principles" of astronomy – Aristotelian physics – can and will be applied to his system.[27] Copernicus's advance comes at the level of description, not explanation or concepts. Indeed, it was Copernicus's intention to adhere to Aristotelian physical principles that brought him to the novel description of the earth as a planet.

2.5 DIFFICULTIES RAISED

Copernicus's redescription of the solar system does raise significant problems, however, both at the level of concepts and at the level of explanations. First, Copernicus is forced to bifurcate his representation of space. Aristotle employed a single spherical representation of space, centered on a point that coincides with the center of the earth, for the descriptions of the entire universe. For Aristotle, therefore, descriptions in the heavens and on the earth refer to the same point. "Down" and "lower" have the same significance in all parts of the universe. Thus, Aristotle presents, at least in this sense, a unified system. Copernicus, on the other hand, considers two centers. Descriptions of terrestrial phenomena refer to the center of the earth, while descriptions of celestial phenomena refer to the sun.[28] Heavy bodies fall "down" toward the earth, but Mars is "below" Jupiter, closer to the sun. Copernicus employs two different representations of space: one for the description of terrestrial phenomena, another for celestial.

Copernicus's use of multiple representations of space is perfectly acceptable for generating descriptions. Indeed, he is always clear which he is using in a given context, and the reader of *De Revolutionibus* is never confused as to the significance of a description. It is, however, conceptually unsatisfying. For one thing, descriptions of different situations will be conceptually incommensurate. The representation of space that grounds descriptions of one situation will be different from that which grounds another. For example, a heavy body can be described as falling "downward" in a terrestrial context, but this description loses its meaning in

[27] Copernicus's disciple, Georg Joachim Rheticus, reported that he was "fully convinced that for him [Copernicus] there is nothing better or more important than to walk in the footsteps of Ptolemy and to follow, as Ptolemy himself did, the Ancients and those who came before him." Alexandre Koyré, *The Astronomical Revolution: Copernicus, Kepler, Borelli*, trans. R. E. W. Maddison (Ithaca, N.Y.: Cornell University Press, 1973), 30.
[28] To be precise, the center of Copenicus's system is the center of the earth's orbit – a point near the sun.

the solar-centered celestial context, where "downward" means something different. One consequence of this is that the physical explanations generated under differing representations of space will be fundamentally disjoint, since the descriptions they explain are incommensurate. The explanation given for the "downward" motion of a body on the earth has no significance or relevance to the "downward" motion of a body in the heavens. The terrestrial system centered on the earth will be explanatorily distinct from the celestial system centered on the sun. This fundamentally undermines the singular, unified ordering of all things in the Scholastic system and becomes a considerable obstacle for those seeking "universal" physical explanations.[29] The "Newtonian Synthesis" will eventually repair this rift, but only by referring to a geometrical representation of space that does not presuppose centers and is therefore capable of reconciling Copernicus's celestial and terrestrial representations.[30]

A second, more specific problem also arises in the context of Copernicus's description of a moving earth. Though it is not the true subject of his inquiry, Copernicus also adheres to Aristotelian physics when dealing with terrestrial phenomena. According to Aristotle, terrestrial bodies seek out their natural places as determined by the substantial elements that constitute them. Earthy bodies seek the "place" or sphere of earth around the terrestrial center, watery bodies seek the sphere of water around the earth, fiery bodies seek the periphery of the terrestrial realm (below the lunar sphere), and airy bodies seek the sphere of air between the water and the fire. Hence, on the earth's surface (at the junction of earth, air, and water), earth and water are heavy and move towards the center, while air and fire are light and move away from the center.

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[29] I do not wish to imply that Copernicus himself sought such explanations. His primary concern, like Ptolemy's, was a correct *description* of the planetary phenomena. His aim was not a consistent explanation of the entire universe. Thus, he was content with distinct celestial and terrestrial realms. The difficulty of reconciling this disjoint picture of the cosmos was addressed mainly by his successors.

[30] Putting the problem differently, a spherical space is heterogeneous and anisotropic. Different parts of space cannot be superimposed on one another by translation or rotation (about points other than the center). A part of space near the center is qualitatively different from a part further away, and all parts possess a privileged direction toward the center that precluded rotational superposition.

If a single spherical structure is used to represent space, different parts of space are each homogeneous with themselves – each portion of space can be superimposed on itself. Thus, the properties and relations pertaining to that part of space will be uniquely determined. If more than one spherical structure is used to represent the space, however, it loses this property. A given portion of space represented in one frame is not homogeneous with the same portion as represented in another frame, even though, objectively, they are the same space. The two representations will also be anisotropic in different ways, because the privileged direction will be directed toward different centers. As a result, the spatial properties and relation found in any part of space will be described differently according to the different spatial concepts used to represent it. This is troublesome, though not inconsistent, since the properties and relations are objectively unitary, and one usually desires them to be represented as such. Such descriptions violated nature's "consonance with itself" – a fundamental desideratum of physical systems in any age.

Copernicus employs these Aristotelian explanations in his own account of terrestrial phenomena:

> Straight line motion is imparted to objects which wander or are pushed from their own natural place, or in any way overstep this volume. But nothing is more repugnant to the whole pattern and form of the universe than for something to be out of its own place. Hence straight line motion does not occur except in objects not in their proper state, when they are separate from the whole of which they are part, and detract from its unity.[31]

For Copernicus, as for Aristotle, the universe has a spatial order in which all bodies have a proper place. When bodies are removed from these places, they tend to return to them along straight lines.

Furthermore, Copernicus does not question Aristotle's arrangement of natural places. In the case of fire, for example, Copernicus himself notes that "[fire's] expansive motion is away from the middle towards the circumference; and similarly if something from Earthy parts has caught fire, it is carried upwards away from the middle."[32] When discussing the arrangement of earth and water, meanwhile, he writes:

> Hence the Ocean which surrounds the Earth pours out its seas far and wide and fills the deeper hollows… the water should not wholly swallow up the land, as both of them by their weight strive towards the same centre…[33]

Copernicus follows the Aristotelian arrangement of water surrounding the earth, but also notes that both seek out a presupposed center. They "strive" because of their "weight," their natural tendency to seek their proper place. In sum, Copernicus adopts the Aristotelian view that terrestrial bodies seek their natural places – heavy bodies, like water and earth, nearer the center, lighter ones, like fire, further from the "middle" – along straight lines.

However, the Copernican bifurcated representation of space undermines the Aristotelian explanations of terrestrial phenomena in at least two ways. Aristotelian explanations assume a representation of space centered on a single, unmoving point. Yet, in the first place, Copernicus holds that there are at least two centers in the universe. The question immediately arises as to why some physical bodies respect one center while other bodies respect another. The motion of

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[31] Copernicus, *De Rev*, 45.
[32] Ibid.
[33] Ibid., 37.

heavy bodies cannot be simply ascribed to a "tendency to seek the center" when "the center" might not be the center of the earth.[34] In the second place, Copernicus suggests that the centers are moving with respect to one another. The "natural" motion of simple bodies cannot be simply rectilinear when they also partake of the circular motions of the earth. How, then, does one account for the "natural motion" of objects on a moving earth? How is one supposed to explain motions when there is more than one center?

As we glimpsed above, Copernicus attempts to solve these problems. He argues that terrestrial motion is really a "dual motion:"

> We have indeed to admit that the motion of falling and rising bodies is a dual motion in comparison with the universe, and is no less a compound of straight and circular motion… Since therefore circular motion belongs to wholes, and straight line motion to parts, we can say that circular motion accompanies straight just as an animal can at the same time be in the class of sick things.[35]

Thus, falling bodies have two motions. First, since they are out of place, they seek their proper place near the terrestrial center. But since they are part of the terrestrial globe, they take part in the rotational motion of the whole, which is caused by the globe's nature and place in the heavens.

This solution is far from satisfactory. First of all, it ascribes a complex motion to simple bodies, contrary to Aristotle's principles. It also raises a host of other questions, none of which can be answered by Aristotelian theories. For instance, why are the effects of the earth's (rapid) motion unobservable?[36] Why do falling bodies seek out a moving point (the center of the terrestrial globe), and why that point in particular? Why is the sun in the center of the celestial motions, and not another body? And if the sun is the center of celestial motion, why does the moon rotate around the earth, not the sun, like the rest of the planets? Generally speaking, to make use of Aristotelian explanations, one has to stipulate a (unique) center. Where, then, is Copernicus's center?

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[34] Copernicus himself stumbles on this block. Though he appeals to Aristotelian explanations of heaviness while discussing the shape of the earth, he abandons this position once he asserts the earth's motion around the sun: "I myself consider that gravity is merely a certain natural inclination with which parts are imbued by the architect of all things for gathering themselves together into unity and completeness by assembling into the form of a globe." (Ibid., 46.) Whereas Copernicus had said that the earth was spherical because its parts all sought a geometrical center, he now proposes that bodies gravitate because of a "desire" to be part of a sphere.
[35] Ibid., 45.
[36] Ptolemy raised this question in response to early heliocentrists. See Ptolemy, *The Almagest*, 44-45.

Copernicus haltingly provides weak and *ad hoc* answers to some of these questions,[37] but in the end, he throws up his hands:

> Therefore there is more than one centre, and it is not too daring to doubt in the case of the centre of the universe also whether it is in fact the centre of terrestrial gravity or a different one.[38]

Copernicus's multiple representations of space hopelessly complicate physical explanations. Aristotelian explanations rely on the stipulation of a single center. Copernicus stipulates at least two, each moving in relation to the other. As a result, the Aristotelian explanations of terrestrial phenomena are not sufficient to fully account for terrestrial phenomena. It is a problem he cannot solve. It is left to later authors to formulate a terrestrial physics compatible with the motion of the earth.

2.6 THE "THIRD MOTION"

Finally, a very specific problem arises in the context of Copernicus's dual representations of space. Copernicus ascribes three motions to the earth. The first is the daily rotation of the earthly globe around its axis, which accounts for the apparent daily motion of the stars, sun, moon, and planets across the sky. The second motion is the annual revolution of the earth about the sun. As noted above, this motion accounts for some of the apparent irregularities of the planetary motions. These motions, moreover, are explained on the basis of the earth's nature. Since the earth is a globe, rotation about its axis is "naturally fitted for it."[39] Also, since the

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[37] For example, Copernicus appeals to neoplatonic authority to support his placement of the sun at the center: "For who in this most beautiful of temples would put this lamp in any other or better place than the one from which it can illuminate everything at the same time. Aptly indeed is he named by some the lantern of the universe, by others the mind, by others the ruler. Trismegistus called him the visible God, Sophocles' Electra, the watcher of all things. Thus indeed the Sun as if seated on a royal throne governs his household of Stars as they circle round him." (Copernicus, *De Rev*, 50.) These neoplatonic arguments are out of place in an otherwise Aristotelian and astronomical text. They make no reference to the sort of empirical evidence and natural explanations that are offered elsewhere throughout the work. Koyré, for his part, calls Copernicus's arguments in this context "superficial, and even just words [*superficielle, et même verbale*]." Koyré, *Études Galiléennes*, 168.

[38] Copernicus, *De Rev*, 46.

[39] Ibid., 44.

earth "can be regarded as one of the wandering stars" it is appropriate that it, as is natural for a planet, orbits the sun.

The third motion, however, is more complicated. Copernicus knew that, as it orbits the sun, the earth's axis remains pointed towards the same region of the fixed stars (near Polaris, the North Star). As a result, near the summer solstice, the axis is tilted towards the sun, but near the winter solstice, it is tilted away from the sun.[40] This implies that the plane of the earth's axis *rotates* with respect to the earth-sun radius. The angle between the plane of the axis and the radius increases continually through the year. Since, in a spherical representation of space centered on the sun, this angle specifies direction, the *direction* of the earth's axis changes – it *moves.*[41] Copernicus labeled this the "third motion of the earth."

This is a strange motion, however. For one thing, though completely independent from the second motion, it follows it almost exactly, but in the opposite direction. Both the revolution of the earth around the sun and the rotation of its axis occur once every year, the first west to east, the other east to west. Thus:

> These motions being almost equal to each other and in opposite directions, the result is that the axis of the Earth, and… the equator, face almost the same part of the universe, just as if they remained motionless…[42]

In other words, the third motion creates the appearance of motionlessness. The net effect is that the axis remains pointed at the same point in the sky. Moreover, Aristotelian principles offer no explanation for anything like the third motion. The motion is not a simple rotation around an axis. It is a rotation of the axis of the first motion about another axis, like the wobble of a spinning top. Copernicus does not even attempt an explanation, as he does for the first two motions. He leaves it as completely unexplained.[43]

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[40] See Figure 3.
[41] See Figure 4.
[42] Copernicus, *De Rev*, 52.
[43] In the *Commentariolus*, Copernicus comments on the third motion that "it has seemed to most persons [i.e., Ptolemaic astronomers] that the firmament has several motions in conformity with a law not yet sufficiently understood. But it would be less surprising if all these changes should occur on account of the motion of the earth. I am not concerned to state what the path of the pole is." That is, Ptolemaic astronomers, who believed the axis of the ecliptic to move, had no law or principle by which to explain the motion. Copernicus, who ascribes the motion to the earth's axis, likewise has no explanation, and is not even sure what the actual path of the motion really is. See Edward Rosen, "The Commentariolus of Copernicus," *Osiris* 3 (1937); Noel M. Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory: A Translation of the Commentariolus with Commentary," *Proceedings of the American Philosophical Society* 117, no. 6 (1973).

In fact, the third motion is simply an artifact of Copernicus's representation of space. In a spherical representation of space centered on the sun, where direction is specified in relation to a radius to the sun, the behavior of the earth's axis is described as a *motion*. If one were to employ a spherical representation of space centered on the earth, or a rectilinear representation of space, one would say, as we do today, that the earth's axis does *not* move,[44] which is why it remains pointed towards a fixed point in the sky.

Copernicus was unable to give an explanation for the motion he described. It fell on his successors to explain it. This was no easy task, since the "motion" is, in fact, no motion at all. Indeed, the problem of the "third motion" eventually led to a questioning of the spherical geometry by which it was described. A new representation of space eventually allowed authors to describe the behavior of the earth's axis as a "staying" rather than a motion, and this greatly simplified the task of explaining it, as we shall see.

2.7 CONCLUSION

Copernicus is the natural starting point for our history of seventeenth century representations of space. Copernicus's heliocentrism called into question the Aristotelian spherical conception of space that had dominated physical thought since the classical age. What was once a single framework centered on a single point was now a bifurcated system centered on two, if not many, centers. The Copernican hypothesis also required a new physical system to explain the various features of celestial and terrestrial phenomena as they were now described. Eventually, the solution of these problems required the development of a new representation of space, catalyzed, in particular, by difficulties surrounding Copernicus's "third motion." We can now turn to the development of this new, rectilinear representation of space.

It should be mentioned that, besides the difficulties of explaining terrestrial and celestial phenomena arising within the *De Revolutionibus*, an explanatory challenge was raised by the *reception* of Copernicus's work. As mentioned above, Copernicus does not seriously try to expand upon Aristotelian explanations of physical phenomena. He simply assumes that the

[44] Ignoring a *very* slow precession.

existing Scholastic physics will be applied to his system. As a result, he says very little about the causes of the planetary motions he describes in the *De Revolutionibus*. This seeming silence allowed many of his readers to assert that Copernicus was following Ptolemy's *Almagest* by limiting the discussion to descriptions and avoiding any treatment of causes. On this view, the *De Revolutionibus*, and astronomy in general, was purely mathematical work, useful for predicting planetary positions, but devoid of comment about the causal structure of the heavens. By subscribing to this operationalist position, proponents of the so-called "Wittenberg interpretation" could sidestep the trouble of reconciling Copernicus's geokinetic theory with the apparently geostatic descriptions found in Scripture.[45] Even readers, such as Kepler, Gilbert, and Tycho, who did not accept the operationalist view of astronomy came to believe that Copernicus had left his task incomplete. They thought he had only worked *a posteriori* – from the phenomena – and it remained to give an account *a priori* – from the causes. Of course, Copernicus's intention was to bring together astronomical descriptions and physical causes, not to divorce them. He thought he could appeal to the Scholastic explanations already at hand. Under influence of the Wittenberg interpreters, his readers missed the subtle appeal to Aristotelian physics that motivated Copernicus's project from the start.

This was, in the end, a fertile oversight. Copernicus's failure to explicitly address causes led subsequent authors to take up what they (though not Copernicus) thought unfinished business and attempt causal explanations of the planetary motions Copernicus had described. Kepler, for example, had "no hesitation in asserting that everything that Copernicus has demonstrated *a posteriori* and on the basis of observations interpreted geometrically, may be demonstrated *a priori* without any subtlety of logic."[46] Kepler simply failed to recognize the subtle *a priori* nature of the argument in the *De Revolutionibus*, and was led to provide causal explanations of his own. Copernicus had tried to reconcile his novel descriptions with existing explanations. For his followers, like Gilbert and Kepler, the task now became to provide novel explanations to reconcile with the existing Copernican descriptions. A reciprocal iteration took place – a shift in descriptions to save explanations led to a shift in explanations to save descriptions.

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[45] To make matters worse, this was the position suggested by the anonymous preface appended to the *De Revolutionibus* that, before its true author, Andreas Osiander, was revealed by Kepler in the *Astronomia Nova*, made it seem as if Copernicus himself advocated this interpretation. See Westman, "The Melanchthon Circle, Rheticus, and the Wittenberg Interpretation of the Copernican Theory."
[46] Koyré, *The Astronomical Revolution: Copernicus, Kepler, Borelli*, 133.

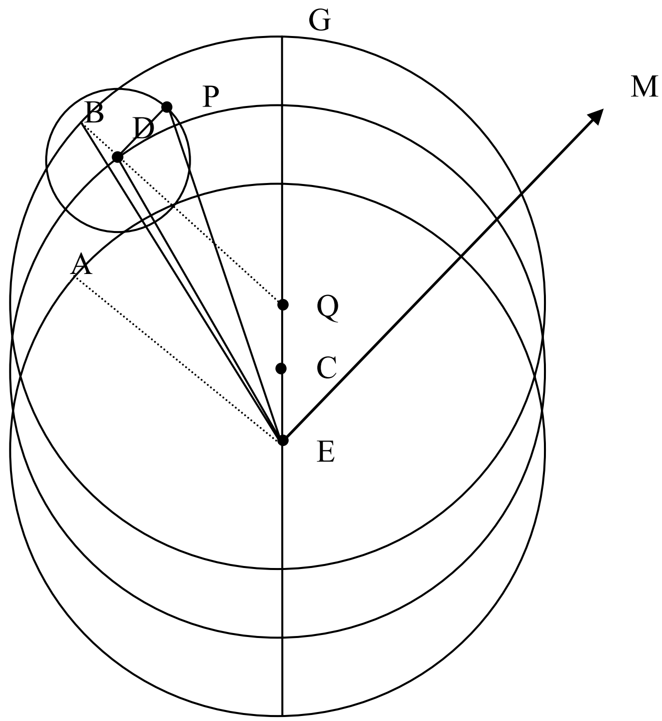

Figure 1. *Ptolemy's Model for a Superior Planet.* In Ptolemy's system, the planet, P, moves on an epicycle, the center of which, D, moves on an eccentric deferent with center C. D's angular velocity is uniform about an equant point, Q. Assume that one eighth of the planet's orbital period has passed since its last apogee, towards G. If the planet were to move at constant velocity on a circular orbit centered on the earth, it would arrive at A (angle GEA = 45°). However, angular velocity is constant only around Q, so the planet is actually somewhere on the segment QB (where EA is parallel to QB). In fact, D lies on the circle with center C. The planet, at P, lies on the epicycle on the segment DP, where DP is parallel to EM, the vector connecting the earth and the mean sun, M. Thus, angle AED is the ecliptic anomaly consisting of two corrections: the angle AEB generated by the uniform motion around the equant, and the small angle BED generated by the eccentric orbit of D. Angle DEP is the synodic anomaly related to the motion of the mean sun, M. Angle GEA is the mean motion of the planet, which is corrected by the total anomaly, AEP. Ptolemy's model for Venus, an inferior planet, is similar, except that the angle AED constitutes the synodic anomaly, and the angle DEP is the ecliptic anomaly.

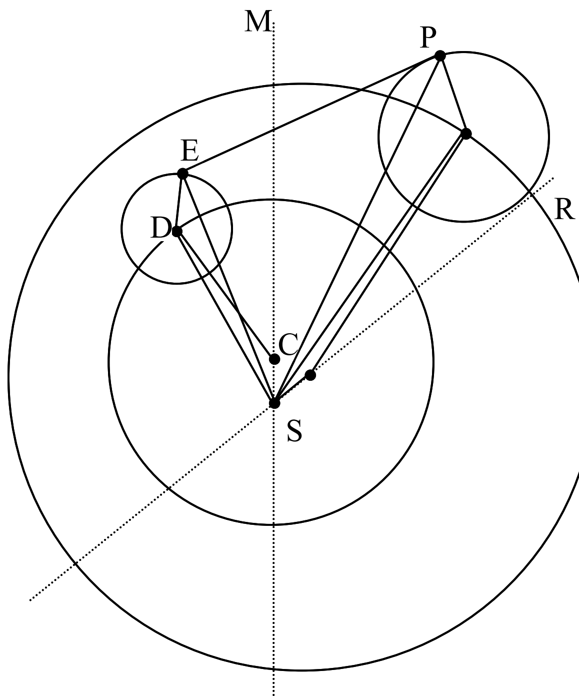

Figure 2. *Copernicus's Model for a Superior Planet*. In Copernicus's system, both the planet, P, and the earth, E, move on epicycles whose centers, A and D, move on deferents eccentric (at C and Q) with the sun, S. Thus, the planet's position in relation to the sun, angle SEP, is given by calculating six quantities, three each for the earth and planet: the mean motions; the eccentric anomalies, MCD and RQA, and the epicyclic anomalies, ESD and PSA.

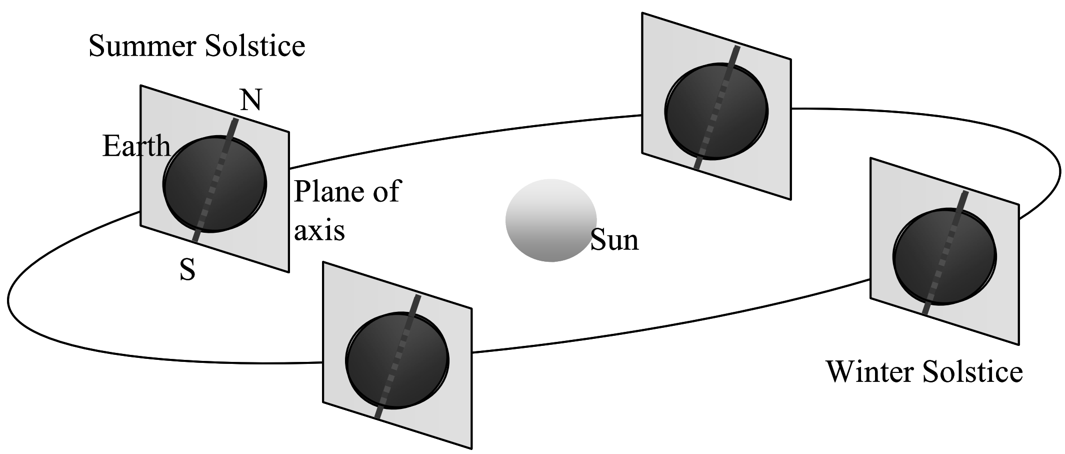

Figure 3. *The Earth's Axis*. As the earth orbits the sun, its rotational axis NS (for North-South) remains parallel to itself. Near the summer solstice, the north end of the axis (the north pole) is tilted toward the sun. Near the winter solstice, it is pointed away.

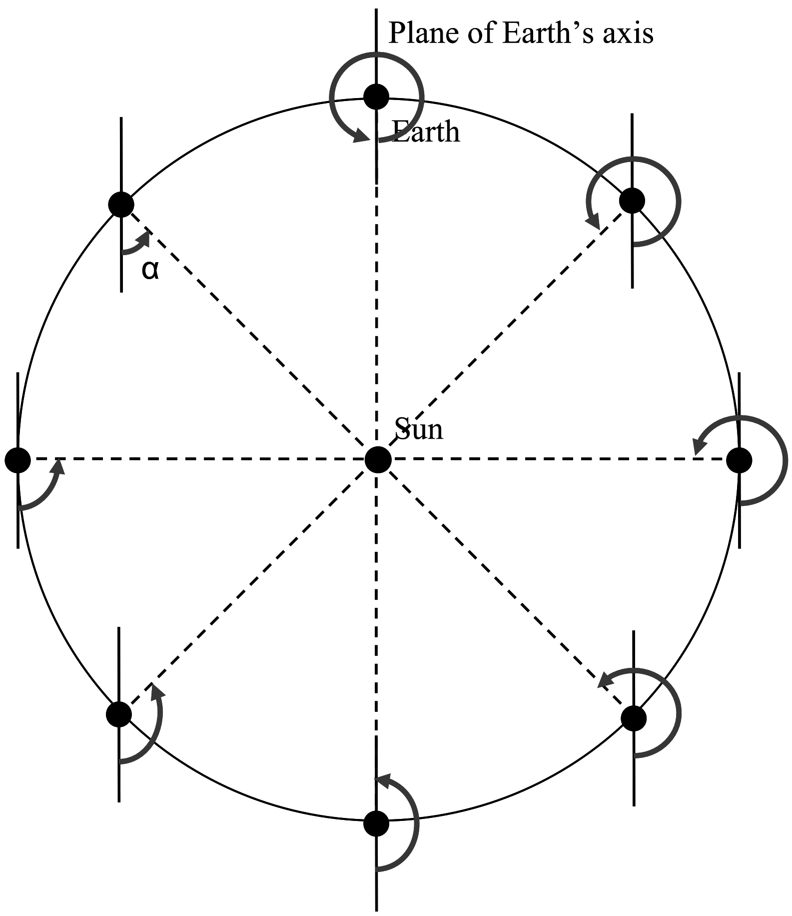

Figure 4. *The Third Motion*. In a spherical representation of space centered on or near the sun, direction is specified by angle α, the deflection from the radius to the sun. As the earth orbits the sun, the plane of its axis remains parallel to itself. As a result, its deflection from the earth-sun radius continually increases. Since this deflection correlates to the description of direction, the phenomenon is described as a changing direction – in other words, a motion.

3.0 GILBERT'S "VERTICITY" AND THE "LAW OF THE WHOLE"

In chapter 1, we spoke of an iterative reciprocation between concept, description, and explanation that led to a new representation of space during the seventeenth century. Copernicus is an illustration of this process. His adherence to Aristotelian explanations of celestial motion led him to novel descriptions of phenomena. These new descriptions, in turn, led to problems at both the conceptual and explanatory levels. One of the first authors to grapple with these new difficulties was the Englishman William Gilbert of Colchester, royal physician to Queen Elizabeth I and King James I.

As we shall see, Gilbert constitutes another iteration of the process we are describing. Gilbert aims to fill some of the explanatory gaps opened by Copernicus. To do so, he focuses on a particular set of phenomena, namely, those associated with magnetism. These phenomena, however, are described on the basis of a particular representation of space appropriate to the magnetic subject matter, which Gilbert then applies to the description of the earth itself. This altered description of the earth, in turn, helps Gilbert give the explanations he seeks (though they may remain unconvincing). In other words, Gilbert carries the intellectual process we have been describing full circle. A new conceptual framework leads to new descriptions. The new descriptions allow new explanations.

3.1 GILBERT'S RESPONSE TO COPERNICUS

As we saw in the last chapter, Copernicus left an important explanatory gap in his theory of the solar system. He showed that one can account for the apparent phenomena by describing a moving earth, but he did not offer any new causal explanations of the earth's motion. He did not say why or how the earth moves. Any follower of Copernicus would have to fill this explanatory lacuna in order to answer his opponents, who, on the basis of Aristotelian physics, saw the motion of the earth as a physical impossibility. Gilbert is one such Copernican. He accepts that the earth is in motion, and he sets out to explain this movement.[1]

Unlike Copernicus, however, Gilbert is not an astronomer. Save for a brief excursion at the very end of *De Magnete* (1600), his interest is restricted exclusively to terrestrial phenomena.[2] As a renowned physician with an appointment at court, Gilbert probably had little leisure to make regular celestial observations. Nor does he demonstrate the mathematical acumen necessary for any serious foray into mathematical astronomy, even by seventeenth century standards.[3] Indeed, *De Magnete* is striking if only for its near complete lack of quantitative descriptions. Even Gilbert's discussion of Copernicus's theory of equinoctial precession is simply a qualitative summary of the corresponding passages of *De Revolutionibus*.[4]

As a result of this curtailed purview, Gilbert does not seek to explain *all* Copernicus's earthly motions. He aims to explain the motions Copernicus ascribes to the earth in and of itself – i.e., the first and third motions.[5] That is to say, Gilbert seeks to explain the earth's daily rotation and why the axis of this rotation remains pointed towards one region of the fixed stars. Gilbert does not, on the other hand, have anything to say about Copernicus's second motion, the annual orbit of the earth around the sun. Any investigation of this motion would require a discussion of the earth's position relative to other heavenly bodies, observations of which Gilbert

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[1] I agree with John Henry's contention, *contra* Zilsel, that "the whole point of *De Magnete* was to offer a solution to a crucial problem for Copernican theory… Copernicus effectively left unanswered the question as to how the earth might be able to move and keep on moving." See John Henry, "Animism and Empiricism: Copernican Physics and the Origin of William Gilbert's Experimental Method," *Journal of the History of Ideas* 62, no. 1 (2001): 106; Edgar Zilsel, "The Origins of William Gilbert's Scientific Method," *Journal of the History of Ideas* 2, no. 1 (1941).

[2] Gilbert's other, posthumously published work, *De Mundo Nostro Sublunari Philosophia Nova* (1651), is likewise generally restricted to terrestrial phenomena. There is some discussion of the earth's movement and the substance of the heavens, which we will touch on below, but the book is primarily concerned with sublunar phenomena, such as elemental substance, heat and cold, weather, and tides. The manuscript, compiled by Gilbert's half-brother and placed in the library of Henry, Prince of Wales, around 1607/8, was read by Francis Bacon and Thomas Harriot. The latter told Kepler about the work in 1608. Though he requested a copy, there is no evidence Kepler ever saw it. Otherwise, the book, published long after Gilbert's death, had little influence on Gilbert's successors. We will not devote much attention to it here. See William Gilbert, *De Mundo Nostro Sublunari Philosophia Nova* (Amstelodami: Ludovicum Elzevirium, 1651). Sister Suzanne Kelly, *The De Mundo of William Gilbert* (Amsterdam: Menno Hertzberger & Co., 1965).

[3] Gilbert, apparently, had some facility in mathematics. He was a mathematics examiner at Cambridge from 1565. Whatever ability Gilbert possessed, however, does not evidence itself in *De Magnete*. Rufus Suter, "A Biographical Sketch of Dr. William Gilbert of Colchester," *Osiris* 10 (1952): 271.

[4] See Zilsel, "The Origins of William Gilbert's Scientific Method," 3.

[5] As well as the precession of the equinoxes, which he also attributes to a motion of the earth.

is not prepared to handle. In fact, nowhere in *De Magnete* does Gilbert explicitly affirm that the earth moves through the cosmos.[6]

This is not to say, though, that Gilbert does not believe that the earth moves around the sun. His project, after all, owes its existence to Copernicus, and he follows Copernicus at many points, including questioning the earth's centrality:

> But the stars or the planetary globes do not move in a circle round the centre of the earth; nor is the earth the centre – if it be in the centre – but a body around the centre.[7]

Gilbert accepts, without comment, Copernicus's conclusion that the earth is not the center of planetary orbits. As a result, like Copernicus, he wonders whether the earth really is the center of the universe. It might be that the earth is not at the center, but revolving around another point. And even if the earth is at the center, its centrality is accidental. The center is a geometric point, unrelated to the body of the earth itself, which just happens to be "around the center." But this is as much as Gilbert will say on the matter.

Far more telling, though, is the very fact that Gilbert sees the need to explain the third motion at all. Recall that Copernicus introduces the third motion to account for the apparent stability of the earth's axis, *given* that the earth is orbiting the sun. If one assumes, conversely, that the earth does not orbit, and remains in place, presumably one would also assume that its axis would remain in place, obviating any need to explain the appearance of stability. Hence, the very fact that Gilbert sees it necessary to explain the fixity of the earth's axis implies that he accepts Copernicus's second motion. One must assume that Gilbert believes the earth circles the sun.[8]

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[6] He is agnostic about the earth's motion in *De Mundo*, as well. See Gilbert, *De Mundo*, 196ff; Kelly, *The De Mundo of William Gilbert*, 66ff.

[7] "…nec terra si fuerit in centro, centrum est, sed corpus circa centrum." William Gilbert, *De Magnete*, trans. P. Fleury Mottelay (New York: Dover Publications, 1958), 337-38. (Hereafter "*On the Magnet*" to distinguish it from the Latin text.) William Gilbert, *De Magnete, Magneticisque Corporibus, et de Magno Magnete Tellure, Physiologia Nova* (London: Peter Short, 1600), 227.

[8] This is consistent with Gilbert's neoplatonic description of the sun as the source of vegetation and nourishment, which the earth "seeks and seeks again," implying (though he does not say so) that the sun inhabits the center as the source of vital energy in the universe. Compare this comment also to Copernicus's own neoplatonic description of the sun as the "lantern of the universe," etc. Copernicus, *De Rev*, 50ff; Gilbert, *On the Magnet*, 333ff. For further support of Gilbert's heliocentrism, see Gad Freudenthal, "Theory of Matter and Cosmology in William Gilbert's *De magnete*," *Isis* 74, no. 1 (1983): 33ff.

3.2 THE *DE MAGNETE*

3.2.1 Book I

Gilbert begins *De Magnete* with an extensive investigation of the properties of magnets in general and spherical lodestones in particular. Book I includes encyclopedic details about prior descriptions of magnetism; the different kinds and names of iron ore, and where they are found; how magnetism is found in iron ore, smelted, and wrought iron; and the medicinal uses of magnets. Upon noticing the collocation of lodestones and iron ore in mines, the magnetism of both, and the similarity of their chemical traits, Gilbert concludes "that loadstone and iron ore are the same" and their "form, appearance, and essence are one."[9]

> …thus loadstone is by origin and nature ferruginous, and iron magnetic, and the two are one in species… and the better sort of iron ore is weak loadstone, just as the best loadstone is the most excellent iron ore in which we will show that grand and noble primary properties inhere. It is only in weaker loadstone, or iron ore, that these properties are obscure, or faint, or scarcely perceptible to the senses.[10]

Lodestone and iron ore are essentially the same substance. Lodestone is just a superior form of ore, in which the special magnetic powers inhere particularly strongly.

The conclusion of Book I is presented in its seventeenth chapter, where Gilbert argues that the earth itself consists of magnetic material akin to the lodestone and iron ore. Though the surface of the earth is "defaced by all sorts of waste matter and by no end of transformations,"[11] it exhibits an immutable magnetic power that influences, not only lodestones, but

> …[all] sorts of fissile stone of different colors; also clays, gravel, and several sorts of rock; and, in short, all of the harder earths found everywhere, provided only they be not fouled by oozy and dank defilements like mud, mire, heaps of putrid matter, or by the decaying remains of a mixture of organic matters…[12]

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[9] Gilbert, *On the Magnet*, 59-60.
[10] Ibid., 63.
[11] Ibid., 67.
[12] Ibid., 70.

Once the "dank defilements" of organic matter are removed, all earthy substances demonstrate their inherent magnetic nature. Ignoring the transformations of the imperfect organic world, then, earth's "inmost nature" and "marrow"[13] is magnetic stuff:

> Such, then, we consider the earth to be in its interior parts; it possesses a magnetic homogenic nature. On this more perfect material (foundation) the whole world of things terrestrial [is based], which, when we search diligently, manifests itself to us everywhere, in all magnetic metals and iron ores and marls, and multitudinous earths and stones…[14]

According to Gilbert, the fact that the earth consists of magnetic matter means the earth is *essentially* magnetic. For the earth is a celestial body, endowed with a peculiar and special nature.

> But the true earth-matter we hold to be a solid body homogenous with the globe, firmly coherent, endowed with a primordial and (as in the other globes of the universe) an energic form.[15]

All heavenly bodies, including the earth, have a primordial form, presumably endowed by the Creator. The earth's substance, then, must partake of the earth's special form. Gilbert has shown, though, that earth-matter, whatever it really is, is magnetic. Hence, this "primordial" and "energic" form is, or at least comprises, magnetism. That is, magnetism is the form of the earth and earth-matter.[16]

A lodestone, therefore, shares the essential magnetic form of the earth itself, as do all other magnetic substances, in varying degrees:

> A strong loadstone shows itself to be of the inmost earth, and in innumerable experiments proves its claim to the honor of possessing the primal form of things terrestrial… So a weak loadstone, and all iron ore, all marls and argillaceous and other earths (some more, some less, according to the difference of their humors and the varying degrees in which they have been spoilt by decay), retain,

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[13] Ibid., 68.
[14] Ibid., 69.
[15] "Sed terram veram volumus esse substantiam solidam, telluri homogeneam, firmiter cohaerentem, primaria, & (ut in globis aliis mundi) valida forma praeditam" Ibid., 68; Gilbert, *De Magnete*, 42.
[16] Gilbert also favorably quotes Guillermo Cardano's proposition that "the loadstone is true earth." Gilbert, *On the Magnet*, 69-70. See also Freudenthal, "Theory of Matter and Cosmology in William Gilbert's *De magnete*," 24-26.

> deformed, in a state of degeneration from the primordial form, magnetic properties, powers, that are conspicuous and in the true sense telluric.[17]

There is a real, formal, affinity between a lodestone, or any other magnetic material, and the earth. They share a single form and nature. Thus, whatever is natural for a lodestone – whatever is a result of its form – will also be natural for the earth itself. In particular, Gilbert will want to assert, the earth's motions are natural, caused by its inherent magnetic nature:

> Thus every separate fragment of the earth exhibits in indubitable experiments the whole impetus of magnetic matter; in its various movements it follows the terrestrial globe and the common principle of motion.[18]

Experiments performed on "separate fragments of the earth" – i.e., lodestones – will exhibit the true nature of magnetic matter, including its principles of motion, "common" to lodestone and earth. Hence, whatever motions are found in the lodestone can be ascribed to the earth, as well.

Thus, in Book I of *De Magnete*, Gilbert has laid the groundwork for an explanation of the earth's motion on the basis of an investigation into the nature of the lodestone. Whatever magnetic properties exist in the lodestone will exist in the earth simply because they share a common magnetic nature.[19]

3.2.2 Book II

Book II begins Gilbert's investigation of the magnetic characteristics of lodestones and their motions. He focuses his attention on spherical lodestones, which he labels *terrellae*, because the spherical shape is also that of the earth. Thus, a terrella will have

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[17] Gilbert, *On the Magnet*, 70.

[18] Ibid., 71.

[19] It is interesting to note that, in this context, Gilbert has adopted the Scholastic structure of explanation based on substantial form. As in Scholastic matter theory, the form is responsible for the properties of the body. In this case, however, the form is magnetism – an active principle akin to neoplatonic affinities and spirits. Gilbert accepts the notion that a form inheres in earthy matter and gives it its various attributes, but he explicitly rejects the Aristotelian view of earth as an ideal, inert, and utterly passive element. The postulated ideal earth, "Aristotle's 'simple element,' and that most vain terrestrial phantasm of the Peripatetics," is merely a product of the imagination and "never appeared to any one even in dreams." Magnets, observable and active, are the true terrestrial substance. Gilbert accepts the outline of the Aristotelian theory of matter, but rejects the specific description of the earthy element. Earth is an Aristotelian element, but it is essentially magnetic, not, in Gilbert's words, "formless, inert, cold, [and] dry." Ibid., 69.

> …got from art the orbicular form that nature in the beginning gave to the earth, the common mother; and it is a natural little body endowed with a multitude of properties whereby many abstruse and unheeded truths of philosophy, hid in deplorable darkness, may be more readily brought to the knowledge of mankind.[20]

A spherical lodestone is most like the earth, so the properties associated with a terrella will be most like the earth's as well. Because the forms have such close similarity, investigation of the properties of the terrella can reveal "truths of philosophy" regarding the earth.

The phenomena associated with a terrella immediately suggest a particular representation of space to be used to describe them, the spherical system of meridians and poles used by geographers and astronomers.[21] Even the most rudimentary examination of a terrella – indeed, of any magnet – reveals diametrically opposed poles where the strength of the magnet's influence is concentrated. A simple procedure locates them on the surface of the sphere:

> To find, then, poles answering to the earth's poles, take in your had the round stone, and lay on it a needle or a piece of iron wire; the ends of the wire move round their middle point, and suddenly come to a standstill. Now, with ochre or with chalk, mark where the wire lies still and sticks. Then move the middle or centre of the wire to another spot, and so to a third and a fourth, always marking the stone along the length of the wire where it stands still: the lines so marked will exhibit meridian circles, or circles like meridians on the stone or terrella; and manifestly they will all come together a the poles of the stone.[22]

This method of finding the poles reveals "lines of force" marked out on the terrella. On a spherical magnet, these will, as Gilbert describes, form great circles, converging at two opposing poles, "like meridians."

Notice, however, that a meridian is not identifiable in a spherical representation of space constructed around an assumed center. Recall, a representation of space legitimates spatial descriptions. Suppose we are presented with the claim, "the line is like a meridian." If all we presuppose is a sphere around a center, all we can determine is whether the line is a great circle of the sphere. But this is not enough to test the claim. We must also determine whether the great

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[20] Ibid., 24.

[21] Gilbert may have been introduced to the rudiments of cartography by his friend, the mathematician Edward Wright, who worked out the modern theory of Mercator projections in *Certaine Errors of Navigation* (1599). See Suter, "A Biographical Sketch of Dr. William Gilbert of Colchester," 376. Wright, incidentally, is the author of the dedicatory address included in *De Magnete*.

[22] Gilbert, *On the Magnet*, 24.

circle connects the poles. Thus, in addition to the sphere (and its center), we must also stipulate poles as privileged locations on the sphere to which we can refer our description. We need to include poles in our representation of space to legitimate descriptions including meridians. In other words, descriptions involving meridians can only be understood in the context of a spherical representation of space that includes poles. The same is true, for example, of descriptions including equators, parallels, and the poles themselves. All such descriptions require a presupposed spherical representation of space with poles.

When Gilbert says that the lines of force on a terrella are "like meridians," he is implicitly assuming that the reader knows what a meridian is and where it might lie on the sphere of the terrella. This requires the assumption that the reader is supposing a spherical representation of space with poles. The phenomena exhibited by the lodestone, then, are described by reference to the sphere and its poles. The descriptions can be understood because the reader shares Gilbert's conceptual framework that makes the descriptions sensible. That is, both reader and Gilbert assume a spherical representation of space that includes stipulated poles. We will call this the *geographical representation of space*.

Gilbert clearly endorses the geographical representation of space as the proper framework for his description for magnetic phenomena.

> Astronomers, in order to account for and observe the movements of the planets and the revolution of the heavens, as also more accurately to describe the heavenly order of the fixed stars, have drawn in the heavens certain circles and bounds, which geographers also imitate so as to map out the diversified superficies of the globe and to delineate the fairness of the several regions. In a different sense we accept those bounds and circles, for we have discovered many such, both in the terrella and in the earth; but these are determined by nature itself, and are not merely imaginary lines.[23]

Gilbert, like a geographer, will use presupposed "circles and bounds" – meridians, poles, equators, etc. – to describe the behaviors of his terrellae. The lines of magnetic force, for example, pass along meridians, and so on.

The presupposition of a geographical representation of space allows the specification of direction and location on the surface of the sphere. As Gilbert writes:

[23] Ibid., 125.

> And neither in earth nor in terrella do the poles exist merely for the sake of rotation; they are furthermore reference points of direction and of position – on the one hand towards one's destination on the earth, and on the other hand as regards the angular distance between them.[24]

Position, for example, is measured as angular distance from the pole, measured at the center – i.e., latitude. Direction is also referred to the pole as a "destination." One can be directed toward or away from the pole, or one can be directed around it, neither toward nor away. (Gilbert has in mind the compass rose. The north/south/east/west specifications of geographers are understood as toward, away from, and around the poles.)

Gilbert's geographic framework is apparent throughout his investigation of the terrella. Consider, for example, his description of "variation," one of the five "magnetic movements" caused by a terrella's magnetic nature. Variation is defined as "deflection from the meridian."[25] In other words, the meridian is considered the null direction. Variation is a change of this direction, measured with respect to the meridian. If something, a compass needle, say, is pointed along the meridian, it has not undergone variation. If the compass needle is deflected from the meridian, it has moved. Thus, the meridian, and the geographic representation of space, is essential to Gilbert's description of variation. The description assumes a meridian.

A similar point can be made regarding another of Gilbert's magnetic movements: declination, "a descent of the magnetic pole beneath the horizon."[26] On a terrella, and on the earth, a magnetic needle only lies parallel to the sphere's surface at the (magnetic) equator. Closer to the poles, it will incline, or dip, towards the pole. At the pole, the needle will stand on end, perpendicular to the surface of the magnet. Notice, however, that Gilbert describes this motion as a "descent" "beneath the horizon," where the horizon is defined as the plane tangential to the sphere.[27] The description presupposes a sphere, so that the direction tangential to the sphere is a null direction. Needles that lie tangential to the sphere exhibit no "descent" and, hence, no declination. The declination increases, however, as the needle moves "below" the tangent to the sphere, reaching maximum (the perpendicular) at the pole. Gilbert's description of declination, as of variation, demonstrates his use of a geographical representation of space.

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[24] "Neq; etiam hi in tellure aut terrella vertendi tantum gratia existunt; sed etiam termini sunt dirigendi, & consistendi, tum versus destinatas mundi regiones; tum etiam inter se iustis conversionibus." I have altered Mottelay's translation slightly. Ibid., 129; Gilbert, *De Magnete*, 81.
[25] Gilbert, *On the Magnet*, 73.
[26] Ibid.
[27] Ibid., 128.

3.2.3 The Instantiation of the Geographical Representation of Space

Gilbert's representation of space is actually an important component of his overall argument concerning the earth's motion. Gilbert asserts that the geographic representation of space is more than just a conceptual framework used to generate descriptions. While astronomers and geographers use the geographical representation, with its meridians and poles, "to account for and observe the movements of the planets" or "delineate the fairness of the several regions" of the world, it is just a conceptual device. The meridians, parallels and poles are merely "imaginary lines."[28] In the context of magnetism, however, these geometrical structures are physical features of the terrella,

> …for we have discovered many such, both in the terrella and in the earth; but these are determined by nature itself, and are not merely imaginary lines. Geographers make a division of the earth chiefly by defining the equator and the poles; and [in the terrella] these bounds are set and defined by nature. Meridians, too, indicate tracks from pole to pole, passing through fixed points in the equator; along such lines the magnetic force proceeds and gives direction.[29]

In geography, the geographical representation of space is imposed by the geographer, who "defines" its elements. In magnetism, the geographical framework is fixed by nature, which "sets" and "defines" its structure. Thus, these "lines," the meridians, the equator, the polar axis, etc., are determined by the nature of the terrella and can be discovered in it.

Gilbert's argument, then, amounts to a claim that the terrella, by virtue of its magnetic nature, *instantiates* the geographic representation of space. The geometric features of the framework are physical features of the spherical magnet, with real and observable effects in phenomena. A meridian, for example, is a geometric structure. It "indicates tracks from pole to pole." On the terrella, though, the meridians are instantiated by the magnetic force, which "proceeds" along them. Poles, meanwhile, are geometric points, which are also the foci of magnetic attraction, where the attractive power is strongest. Similarly, the equator is the line which separates the terrella into two halves, each "imbued with equal energy," and where attraction is weakest.[30] The parallels are loci of constant magnetic declination.[31] These "metes

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[28] Ibid., 125.
[29] Ibid.
[30] Ibid., 126.

and bounds" are real features of the magnetic body. The representation used to describe magnetic phenomena is not just "imaginary," but in the very nature of the phenomena described. It is not imposed on the phenomena, but "discovered" in them.

Gilbert's claim that the geographic representation of space is "discovered" in the terrella is, in one sense, circular and misleading. As Gilbert himself acknowledges, the geographic framework is a presupposed geometry employed to make descriptions possible. This framework is purely intellectual and subjective, however. Its "metes and bounds" are "merely imaginary" in the mind of the descriptor. One can always assume a different geometric structure.[32] The resulting descriptions might not be as useful or specific as one desires, but descriptions can be generated all the same.

In the case of the terrella, Gilbert finds that a particular geometric framework – the geographic system – generates the most satisfactory descriptions of the phenomena. This is because the phenomena exhibit certain features, such as concentrations of magnetic action, that are easily identified with features of the geometric structure, like stipulated poles. Thus, the phenomena are best or most satisfactorily described – for Gilbert, at least – by appealing to the geographic representation of space. This is not a necessary conclusion, though. The foci of magnetic attraction are not geometric points, and nothing entails that they should be described as such. Others might find different representations that are, in their estimation, more satisfactory (though it is admittedly hard to imagine what the other representations might be).

Gilbert concludes, meanwhile, that the close correlation of geometry and phenomena entails that the geographic framework is not subjective and "imaginary," but objective and "discovered" in the phenomena. The terrella's "metes and bounds" are generated by its inherent magnetic nature, not by the observer trying to describe it. In this way, Gilbert slides over the distinction between intellectual framework and objective phenomena. He makes the concepts used to generate the description part of the description.

This subtle slide results in a somewhat circular argument. Gilbert begins by assuming a certain geometry. He assumes a sphere. He assumes poles. When observing the terrella, he finds certain features that answer to these presupposed structures. Then he describes the

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[31] Ibid., 127.
[32] Kepler, for example, will employ a spherical representation of space to describe the magnetic action of the sun. One consequence is Kepler's focus on the spherical propagation of magnetic action from the magnetic center. See below.

terrella's features *as* the assumed structures. The conclusion that the terrella has real, objective poles relies, in part, on the subjective assumption that the terrella has poles.

Of course, Gilbert's choice of geometric framework does not come in a vacuum. As it turns out, the geographic representation of space *is* very appropriate to the terrella, and allows specific and detailed descriptions of its properties. Indeed, the terrella immediately suggests the geographic structure, as we have seen, and it is difficult to imagine another conceptual framework that could be substituted. Thus, Gilbert's conclusion that the geographic structure is an objective feature of the terrella can be weakened to the sounder, almost equivalent claim that the physical features of the terrella can *best be described* on the basis of the geographic representation of space. Thus, the foci of the magnetic attraction are not poles, *per se*, but best described as poles. Lines of magnetic force, meanwhile, are not meridians, but best described as meridians. By contrast, a spherical representation of space, for example, does not provide as easy a way to describe the terrella's magnetic action. Since such a representation provides for the assumption of neither poles nor meridians, there would be no simple way to describe the foci or lines of magnetic force. In the end, as we shall see, this weakening will make little difference to Gilbert's argument, and he may perhaps be excused for eliding the distinction in the first place.

3.2.4 Books III-IV: Magnetic Motions

Book I of *De Magnete* established the common magnetic nature of the earth and terrella, which Gilbert promised to exhibit in "indubitable experiments." Book II begins the presentation of these "experiments." First, it introduces the five "magnetic motions" caused by the magnetic nature of the earth and terrella: coition, direction, variation, declination, and revolution. The latter part of Book II examines coition, "an impulsion to magnetic union" "commonly called attraction."[33] Coition, Gilbert reasons, is a result of the joint nature of both attractor and attracted, rather than, as in the case of electric attraction, the action of the attractor alone.[34] He

[33] Gilbert, *On the Magnet*, 73.
[34] "Thus the magnetic coition is the act of the loadstone and of the iron, not of one of them alone: it is… *conactus* (mutual action) rather than sympathy." Ibid., 110. Gilbert argues that coition is caused by the natural desire for bodies with the same or similar natures to cohere. This is reminiscent of Copernicus's awkward explanation of gravitation. See Copernicus, *De Rev*, 46. and p. 53 above.

also presents numerous experiments demonstrating how magnetic power can be increased (by armoring the stone) or decreased (by corrosion or heating). The essential conclusion of this discussion, however, is that coition follows the inherent spherical geometry of both earth and terrella. Coition respects magnetic poles, where it is strongest, and an equator, where it is weakest.[35] Also, in the terrella and the earth, the strength of magnetic action decreases in proportion to distance from the center of the sphere. This implies the "shape" of magnetic action is spherical. The terrella produces a magnetic "orb of virtue," whose center, the "center of force," coincides with the center of the magnet.[36] Coition reveals the inherent spherical shape of magnetic action.

Book III investigates the phenomenon Gilbert calls "direction," the movement of a magnet or needle to align its poles with those of the earth and the terrella. Here, Gilbert presents experiments demonstrating that magnetized needles will align with the meridians of a terrella. He also shows that magnetized needles (and even wrought iron, suitably worked) will align with the north and south poles of the earth, and how this movement can be used profitably for timekeeping, surveying, navigation, and so forth. Most important for Gilbert, though, is the fact that direction is manifested on both earth and terrella. This similarity constitutes empirical evidence for the affinity between earth and lodestone that is essential to Gilbert's argument.

For reasons we will come to, Gilbert assumed that the earth's magnetic axis coincides with its rotational axis. Of course, as we now know, this is not the case. As a result, compass needles will not perfectly align themselves with the rotational pole, along what Gilbert call the "true meridian," but along the magnetic meridian towards the magnetic pole:

> Yet very oft it happens, afloat and ashore, that a magnetic needle does not look toward the true pole, but is drawn to a point in the horizon nigh to the meridian, and that there is a deflection not only of the needle and magnetized iron in general and of the mariner's compass, but also of a terrella…; for they often look with their poles toward points different from the meridian.[37]

As we have seen, Gilbert calls this deflection from the rotational meridian "variation."

Variation occurs on the earth, but not on the terrella. This dissimilarity calls Gilbert's analogy between earth and terrella into question. Book III of *De Magnete*, therefore, is dedicated

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[35] Gilbert, *On the Magnet*, 115ff.
[36] Ibid., 150ff.
[37] Ibid., 230.

to an argument that variation is merely a "perverted motion."[38] It is caused, Gilbert asserts, not by the true magnetic form of the earth, but the irregularity of its surface – the inequality among the earth's elevations."[39] Landmasses, projecting from the globe of the earth, form irregular concentrations of earthy, magnetic matter, disturbing the "direction" caused by the earth's true nature, resulting in variation. If similar protuberances are constructed on a lodestone, similar disturbances of "direction" are observed. Variation, then is a "perversion" of the true action of the earth, caused by the transient world of "transformations" on its surface. If the earth were a smooth sphere, compass needles would all align with its true, immutable, magnetic – and rotational – poles.

Book V of *De Magnete* comes to "declination," the "descent of the magnetic pole beneath the horizon." Gilbert describes how, on a terrella, magnetized needles will "dip" towards the poles. Gilbert then describes experiments that demonstrate the same phenomenon on the earth. The demonstration of magnetic declination is supposed to clinch Gilbert's argument that the earth and the terrella are of one kind.

> We come at last to that fine experiment, that wonderful movement of magnetic bodies as they dip beneath the horizon in virtue of their natural verticity; after we have mastered this, the wondrous combination, harmony, and concordant interaction of the earth and the loadstone (or magnetized iron), being made manifest by our theory, stand revealed.[40]

Declination conclusively reveals the similarity between earth and lodestone. Magnetized needles on the earth decline according to latitude, just as they do on the terrella. Of course, Gilbert's "theory" is that this similarity is due to a shared magnetic nature. Thus, the harmony and concordance witnessed in the phenomenon of declination is a testament to the formal likeness of all magnets, including the earth and terrella.

The earth and terrella share a magnetic nature. Direction is seen in both. Variation, peculiar to the earth, is a spurious motion that may be dismissed. Most wonderful of all, declination is observed exactly on the earth as it is on the terrella. Hence,

> All the experiments that are made on the terrella, to show how magnetic bodies conform themselves to it, may – at least the principal and most striking of them –

[38] Ibid., 73.
[39] Ibid., 235.
[40] Ibid., 275.

> be shown on the body of the earth; to the earth, too, all magnetized bodies are associate.[41]

All the observed phenomena, so clearly expounded in the first five books of *De Magnete*, indicate that the earth and the terrella are of the same nature. All that is natural for the terrella is natural for the earth.

Notice that this conclusion comes mainly at the level of description, but the conclusion itself has explanatory import. The earth may *be* a magnet, but this means only that it can be described in the same ways any other magnet is described. The features and properties of magnets are the features and properties of the earth. Yet, since the earth can be described this way, one can go on to say that the earth moves *because* it is a magnet – it has a magnetic nature. This nature, moreover, causes the certain motions common to all magnets. This much is an explanation. Gilbert has not shown, though, why the earth is a magnet – why it is endowed with its peculiar nature – or even why magnets exhibit the behaviors they do. This is left to the inscrutable wisdom of the Creator. Gilbert's explanation only reaches so far. The real move is to *describe* the earth and terrellae as members of a single class – magnets – over which ascriptions of features, including motions, can be generalized.[42]

3.2.5 Book VI: The Earth's Motions

Finally, in Book VI, Gilbert is ready to put the pieces of his argument together. Recall Gilbert's assertion that the terrella instantiates the geographical geometry. Meridians, poles, and equator are to be taken as real features of the terrella, determined by its nature. Since the earth is a terrella, and shares this nature, it also instantiates the geographical representation of space:

> And first, on the terrella the equinoctial circle, the meridians, parallels, the axis, the poles, are natural limits: similarly on the earth these exist as natural and not merely mathematical limits.[43]

[41] Ibid.
[42] For one account the explanatory shortcomings of Gilbert's theory, see Mary B. Hesse, "Gilbert and the Historians (II)," *The British Journal for the Philosophy of Science* 11, no. 42 (1960).
[43] Gilbert, *On the Magnet*, 313.

Just as poles, meridians, and equator are real features of a terrella, they are real features of the earth. They are not merely "imaginary" or "mathematical" structures used to describe the earth, but elements of the inherent magnetic nature of the terrestrial globe.

That the earth has real poles is of critical importance for Gilbert's argument in favor of the earth's motion. It is clear that the apparent movement of the sun, stars, and planets daily across the sky is a result of either a rotation of the entire heavens, or a rotation of the earth, "for in no third mode can the apparent revolutions be accounted for."[44] Now:

> Bodies that by nature move with a motion circular, equable, and constant, have in their different parts various metes and bounds.[45]

That is, bodies that move with a uniform circular motion have certain identifiable features. In particular, they exhibit poles, points where the circular motion is minimal, and an equator, where the motion is maximal. From these, other "metes and bounds" can be derived, such as meridians and parallels. The debate between Gilbert and his geo-stationary opponents is whether the heavens or the earth rotates. Gilbert reduces this to a question of geometry. The body that exhibits poles, meridians, equators, and so forth, must be the body in motion. If the earth really has these features, it is moving. Otherwise they belong to the rotating heavens.

Of course, and this is Gilbert's point, the earth has these characteristics. Gilbert's magnetic experiments are meant to prove that they are real features of the earth:

> Now the earth is not a chaos nor a chance medley mass, but through its astral property has limits agreeable to the circular motion, to wit, poles that are not merely mathematical expressions, an equator that is not a mere fiction, meridians, too, and parallels' and all these we find in the earth, permanent, fixed, and natural; they are demonstrated with many experiments in the magnetic philosophy.[46]

Magnetism has provided *independent* evidence for the existence of the geometric features of motion in the earth. The earth has real poles, real meridians, and a real equator, each determined

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[44] Ibid., 328.
[45] Ibid.
[46] Ibid.

by its nature and demonstrable by its magnetic action, without reference to its motion. Thus, the earth is "fitted" – has the necessary geometric features – for diurnal rotation.[47]

The geometric features ascribed to the heavens, meanwhile, have no real existence:

> But no revolutions of bodies, no movements of planets, show any sensible, natural poles in the firmament or in any *primum mobile*; neither does any argument prove their existence; they are the product of imagination.[48]

The firmament of fixed stars is homogeneous and isomorphic. It gives no observable evidence whatsoever for the existence of inherent, natural poles. The celestial poles are "the product of imagination," ascribed to the heavens as a result of their apparent motion, not because of their nature. The earth, on the other hand, possesses its particular motion as a result of it poles, "for nature has set in the earth definite poles and has established definite and not confused revolutions" around those pole.[49]

The earth is a terrella. Like all terrellae, therefore, it instantiates the geographical representation of space. The poles are real features of the earth. The earth, not the heavens, must be the moving body.[50]

We pointed out above that Gilbert's ascription of real poles to the terrella is somewhat misleading. A magnetic exhibits foci of magnetic attraction. Gilbert *describes* these foci as "poles." Thus the "poles" are, at bottom, descriptions of phenomena, not phenomena themselves. However, Gilbert's argument goes through even if one assumes that the terrella does not possess physical poles, but only features best described as poles. As Gilbert himself points out in his presentation of his opponents' claims, it is not that a rotating body has poles – these are merely geometric points used to describe the features of the rotating body. A rotating body has real features – parts where its motion is minimal – that are best described as poles. Thus, if the earth is rotating, it should exhibit features best described as poles. Gilbert can show, on the basis of magnetism, that this is the case. From this point, the argument follows. The earth's magnetic properties imply that it should be considered the body in motion.

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[47] In effect, Gilbert has used magnetism to expand Copernicus's proposition that the earth is "naturally fitted" for circular motion because it is spherically shaped and circular motion is the natural motion for spheres. See Copernicus, *De Rev*, 38ff.
[48] Gilbert, *On the Magnet*, 328.
[49] Ibid., 338.
[50] For a fuller discussion of the metaphorical nature of Gilbert's argument see Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: University of Chicago Press, 1995), ch. 6.

The distinction between the phenomena and description does highlight, in a way not apparent in Gilbert's text, the accidental coincidence of the magnetic and rotational poles. For Gilbert's argument to go through, of course, he must assume the earth's magnetic axis coincides with its rotational axis. Yet, that the earth has a magnetic feature best described as a pole does not entail that the description "pole" also applies to a feature of its motion (or non-motion). By saying that the earth has an objective pole – without specifying whether it is magnetic or rotational – Gilbert can skirt the distinction without arguing that the two necessarily coincide. All the same, observed "variation" raised this problem empirically, since it indicates that magnetized needles on the earth respect a magnetic pole different from the earth's "true" rotational one. For this reason, Gilbert goes to great lengths in Book IV to dismiss variation as a spurious, "perverted" motion.

3.2.6 A Blind Alley

At this point, Gilbert's argument from magnetism runs into a blind alley. He wants to explain the diurnal revolution of the earth – Copernicus's first motion – on the basis of magnetic movements. If it can be shown that terrellae naturally revolve around their axes, Gilbert can argue that the earth's diurnal rotation is caused by its magnetic nature. The earth's magnetic nature would explain Copernicus's motion. Gilbert cannot, however, show that "revolution" – his term for rotation about the axis – is actually exhibited by spherical lodestones.

Gilbert has observed the rotation of terrellae, but this phenomenon does not support his conclusion that terrellae, and thus the earth, undergo revolution:

> That the earth is fitted for circular movement is proved by its parts, which, when separated from the whole, do not simply travel in a right line… but rotate also. A loadstone placed in a wooden vessel is put in water so that it may float freely, rotate, and move about. If the [north-seeking] pole *B* of the loadstone be made to point, unnaturally, toward the south *F*, the terrella revolves round its centre in a circular motion on the plane of the horizon toward the north *E*, where it comes to a rest…[51]

[51] Gilbert, *On the Magnet*, 331.

Here, a terrella indeed moves circularly around its center, but not around its axis, and not continually, as Gilbert would have the earth move. The motion described realigns the terrella's poles with the earth's poles, i.e., along the meridian. It is the terrella's axis itself that rotates around an axis *perpendicular* to the magnetic axis. Once the terrella has reoriented itself in its "natural" configuration, the motion ceases. In other words, the experiment supports Gilbert's ascription of "direction" to the terrella and, hence, the earth:

> The whole earth would act in the same way, were the north pole turned aside from its true direction; for that pole would go back, in the circular motion of the whole, toward Cynosura [the constellation toward which the rotational pole points].[52]

The terrella (and the earth) will naturally rotate about its center to "direct" its magnetic axes in the "true direction," but this gives no evidence, in the terrella or the earth, for *continuous* "revolution" around the *magnetic* axis.

Indeed, Gilbert is forced to reject a report that spherical lodestones rotate. He simply cannot repeat the experiment:

> I omit what Petrus Peregrinus so stoutly affirms, that a terrella poised on its poles in the meridian moves circularly with a complete revolution in twenty-four hours. We have never chanced to see this: nay, we doubt if there is such movement…[53]

Such a motion, if observed, would be evidence that the earth naturally revolved around its axis. If it could be shown that terrellae revolve around their axes, then one could conclude that the earth, a giant terrella, exhibits the same motion. Gilbert, however, has "never chanced" to observe Peregrinus's rotation, even though, presumably, he has tried to replicate the result. The motion of the terrella indicates only that it moves with a natural circular motion about its center, not that it revolves around its axis. This does imply, Gilbert argues, that the earth *can* move circularly.

> The natural movements of the whole and of the parts are alike: hence, since the parts move in a circle, the whole, too, hath the power of circular motion… And this circular movement of the loadstone… shows that the whole earth is fitted, and by its own forces adapted for a diurnal circular motion.[54]

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[52] Ibid.
[53] Ibid., 332.
[54] Ibid., 331-32.

Since the motion of the whole earth will be like its parts, the terrellae, the earth must, like the terrellae, be capable of circular motion. This is a hollow conclusion, however. The behavior of the terrella shows that the earth is "fitted" for circular motion, but it is the *wrong kind* of circular motion. It is not a continuous, diurnal revolution around the magnetic axis, but a short-lived rotation of the axis itself.

In the end, Gilbert's magnetic philosophy successfully describes the earth's magnetic nature. It can show that the earth has a magnetic axis and that it is "fitted" for circular motion. Yet it fails to support the desired conclusion: the earth's magnetic nature *causes* a daily rotation. Gilbert can set the earth upon its pole, but he cannot make it spin.

To complete his explanation of Copernicus's first motion, Gilbert appeals to causes beyond those demonstrated by his observation of magnets. He employs neoplatonic affinities, astral natures, and terrestrial animism to explain the motion of the earth. The earth rotates because of its "magnetic mind:"

> And were not the earth to revolve with diurnal rotation, the sun would ever hang with its constant light over a given part, and, by long tarrying there, would scorch the earth, reduce it to powder, and dissipate its substance, and the uppermost surface of earth would receive grievous hurt: nothing of good would spring form earth, there would be no vegetation; it could not give life to the animate creation, and man would perish. In other parts all would be horror, and all things frozen stiff with intense cold… And as the earth herself cannot endure so pitiable and so horrid a state of things on either side, with her astral magnetic mind she moves in a circle, to the end there may be, by unceasing change of light, a perpetual vicissitude, heat and cold, rise and decline, day and night, morn and even, noonday and deep night. So the earth seeks and seeks the sun again, turns from him, follows him, by her wondrous magnetical energy.[55]

The earth is animate, Gilbert argues, and seeks out the best preservation of itself. This entails a continuous diurnal rotation, forever towards the "benefit" of the heavenly bodies. For, if the earth did not rotate, it "would receive grievous hurt" from the sun on one side, and "all would be horror" on the other. Altogether, then, the animate earth moves itself in harmony with the heavens to receive their most propitious influences.

[55] Ibid., 333-34.

> The motion of the whole earth, therefore, is primary, astral, circular about its poles…so that the globe by a definite rotation might move to the good, sun and stars inciting.[56]

Gilbert, finally, can explain the earth's diurnal rotation. The earth has a natural axis because it is a terrella – a spherical magnet. Poles are set out by its magnetic nature. It spins around this axis, however, because it is animate, and seeks out the harmony and benefit provided by the other heavenly bodies. In the end, however, this explanation is no more supported by evidence than the Aristotelian appeals to quintessence and elemental natures that Gilbert rejects.[57]

3.3 GILBERT'S TREATMENT OF THE "THIRD MOTION": VERTICITY AND THE LAW OF THE WHOLE

We now can turn our attention, finally, to Gilbert's proposed explanation of Copernicus's "third motion," the supposed rotation of the earth's axis. Recall that Copernicus attributed a motion to the earth to account for the fact that the earth's rotational axis remains pointed at roughly the same part of the heavens throughout the year. We have argued that Copernicus is led to describe the phenomenon as a motion because of his spherical representation of space. Gilbert's geographical representation of space leads him to a quite different description. He will say that

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[56] Ibid., 334-35.

[57] Note that this position places Gilbert in between several intellectual positions of his day. His argument relies on a Peripatetic notion of substantial form, but the form is active, like an alchemical principle. Gilbert's analytic approach to the substance of the magnet also evinces the thinking of the chemists. In addition, Gilbert appeals to astral intellects and influences, hearkening to neoplatonism and natural magic. All told, it is difficult to classify Gilbert with any of these groups, let alone as a "traditional" or "modern" thinker. He is a transitional figure, responding to the intellectual climate of his time, to whom labels do not easily apply. Gilbert intrigued contemporaries of all persuasions, but satisfied none. This has not stopped historians from trying to categorize Gilbert. Heilbron, for example, calls him a "moderate peripatetic" and a "plagiarist" prone to "Renaissance bombast." Henry claims him for the magicians. Freudenthal, meanwhile, puts him in the modern camp, but is more careful to note Gilbert's awkward position between Aristotelianism and neoplatonism. Zilsel names him the first experimental philosopher, who gained his insights from nascent capitalists more than any scholarly tradition. Burtt, *The Metaphysical Foundations of Modern Physical Science*, 162-67; Freudenthal, "Theory of Matter and Cosmology in William Gilbert's *De magnete*."; J. L. Heilbron, *Electricity in the 17th and 18th Centuries* (Berkeley: University of California Press, 1979), 169; Henry, "Animism and Empiricism: Copernican Physics and the Origin of William Gilbert's Experimental Method."; Zilsel, "The Origins of William Gilbert's Scientific Method."

the earth's axis does *not* move. In so doing, however, he appeals to an important and novel feature of the representation of celestial space.

Gilbert has drawn an analogy between the earth and the terrella. Both have an essential magnetic nature, and both exhibit certain natural motions, such as (perhaps) revolution, declination, coition, and direction. Now, as we have seen, direction is the ability of a spherical magnet to align other magnets and magnetic materials to its meridians. Thus, magnetized needles will point to the poles, both on the terrella and the earth. This is not all, however.

> Like the earth, the loadstone has the power of direction and of standing still at north and south; it has also a circular motion to the earth's position, whereby it adjusts itself to the earth's law [*quo se ad illius normam componit*].[58]

In addition to directing needles, a terrella will conform itself to the surrounding magnetic field – "the earth's law." It will orient itself and remain in the north-south orientation dictated by the earth, such that the axes of terrella and earth lie in the same plane. Thus, a lodestone not only has the power to direct needles, but the power to move itself according to an external magnetic action. Gilbert labels this power of the lodestone "verticity."

Of course, Gilbert's argument all along has been that whatever movements are natural for a terrella are natural for the earth as well. Hence, the earth itself has verticity:

> For like as a loadstone… does by its native verticity, according to the magnetic laws, conform its poles to the poles of the common mother, -- so, were the earth to vary from her natural direction and from her position in the universe, or were her poles to be pulled toward the rising or the setting sun, or other points whatsoever in the visible firmament (were that possible), they would recur again by a magnetic movement to north and south, and halt at the same points where now they stand.[59]

Just as a terrella has the ability to conform itself to the "law of the earth," the earth has the ability to conform itself to a cosmic magnetic field or "law of the whole."[60]

But what is this "law of the whole" to which the earth adheres? A lodestone instantiates the geographic representation of space. Included in this conceptual framework are poles, which are instantiated as the foci of the magnetic force of the magnet. The poles, however, are not

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[58] Gilbert, *On the Magnet*, 67. Gilbert, *De Magnete*, 42.
[59] Gilbert, *On the Magnet*, 180.
[60] "…the loadstone possesses the actions peculiar to the globe, of attraction, polarity, revolution, of taking position in the universe according to the law of the whole [*totius normam*]…" Ibid., 66. See also Gilbert, *De Magnete*, 41.

symmetric. They can be distinguished from one another by their activity. One seeks the Earth's north pole, the other the south pole. Hence, the poles give the magnet itself an inherent direction or orientation – a way in which it is "pointed." This "pointing" is indicated along the line connecting the two poles – i.e., the axis, from "south" to "north." So, when the lodestone conforms its poles to the poles of the earth, it is bringing its own orientation into line with the "earth's law". It "points" itself along the meridian – north pole towards north pole, south towards south. The earth, like any other lodestone, also has this inherent linear direction indicated by its poles and the ability to align this internal direction to an external magnetic virtue. The lodestone aligns itself with the earth's orientation. The earth aligns itself with the orientation of the cosmos. The "law of the whole" is an extrapolation of the earth's linear direction to the universe itself.

This extrapolation is interesting, however. Unlike Aristotle and Copernicus, Gilbert does not assume that the universe has a recognizable center.[61] Directions in the cosmos are not referred to a central point. Notice that even if the earth were to "vary from her position," the poles "would recur again" and "halt at the same points where now they stand." At any arbitrary place in the universe, Gilbert is arguing, the orientation of the "law of the whole" is the same, towards the same points of the heavens. Here, the "same points" are not points in the field of the fixed stars, but points in the line of the earth's axis as it extends through the universe. Hence, the return to the "same points in the heavens" means that the orientation which the poles of the earth would assume (if it moved) is everywhere parallel. If the earth were to revolve around the sun, for example, the "lines of the axis of the earth [would be] parallel at equinoxes and solstices."[62] The orientation, therefore, must be rectilinear and self-parallel. Gilbert is referring the orientation to a presupposed line – the line connecting the earth's rotational poles. This is the null direction to which direction at all other positions is referred. Direction is not specified in relation to a central point. In short, the "law of the whole" is a rectilinear, self-parallel orientation of space itself, instantiated by a magnetic virtue.

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[61] Gilbert also eschews an assumed center in his account of terrestrial gravity. He vehemently rejects the Aristotelian view that heavy bodies seek a geometric point or place. Instead, he argues that bodies fall because they seek unity with like matter. (A view similar to that of Copernicus.) However, Gilbert is inconsistent regarding the salient features of matter that result in this desire for unity. Sometimes, he says that electrical activity, which has to do with moisture, brings bodies together. Elsewhere, he says it is magnetic mutual attraction that draws and keeps bodies together. In any case, centers, as geometric points, do not play any role. See Gilbert, *On the Magnet*, 97, 142; Gilbert, *De Mundo*, 116.

[62] "Axis telluris lineae in aequinoctiis & solstitiis sunt parallelae…" Gilbert, *De Mundo*, 166.

The direction of the earth's axis is described, moreover, on the basis of this orientation. Since the earth's axis is always directed along this orientation, and would remain so even if the earth moved, the direction of the earth's axis, so conceived, does not change. That is, the axis is described as stable and fixed. It does not move. Compare this to Copernicus's description on the basis of a radius to the center of his representation of space. As the earth moves, the direction of its axis, described in relation to the radius, changes. For Copernicus, then, the behavior of the axis is described as a motion. Gilbert's assumption of a cosmic orientation allows him to describe the phenomenon as a staying – a non-motion.

This yields Gilbert's explanation of the "third motion." The axis of the earth remains pointed towards a fixed point in the heavens not because it moves, but because it stays. It is held in place by the earth's verticity and the magnetic "law of the whole" to which it conforms:

> But why the terrestrial globe should seem constantly to turn one of its poles toward those points and toward Cynosura [constellation of the Lesser Bear], or why her poles should vary from the poles of the ecliptic by 23 deg. 29 min., with some variation not yet sufficiently studied by astronomers, -- that depends on the magnetic energy.[63]

The stability of the earth's poles, even if the earth moves "from her position in the universe" is explained by its conformity to a magnetic "law of the whole." The third motion is not a motion at all, but a staying:

> This third motion introduced by Copernicus is not a motion at all. The direction of the earth is stable, and if it were to go in a great circle, then it would constantly regard one part of the heavens.[64]

Even were the earth to revolve around the sun, its axis would stay in the same, self-parallel direction, specified by reference to the assumed orientation of the cosmos.

Gilbert's redescription of the behavior of the earth's axis greatly simplifies the task of explaining it. To explain Copernicus's "third motion," one would have to appeal to an *active* cause – some power that brings about a change; in this case, the changing direction of the earth's axis. The description of this cause, moreover, would have to account for various features of the change. Thus, the purported cause would have to explain, for example, the speed and sense of

[63] Gilbert, *On the Magnet*, 180.
[64] "Tertius his motus a Copernico inductus, non est motus omnino, sed telluris est directio stabilis, dum in circulo mango fertur, dum unam partem coeli constanter respicit." Gilbert, *De Mundo*, 165.

the axis's rotation. It would have to bring about a yearly rotation from east to west and not, say, a monthly rotation from west to east. On the other hand, Gilbert's description of the phenomena as a "staying" allows him to explain it by appealing to a *static* cause – a cause that maintains a *status quo*. Gilbert does not need to say how his static "law of the whole" operates. He does not need, for instance, to say how fast the axis would move under its influence. In other words, the cause required to explain the phenomena is simpler when the phenomena is described as a "staying" rather than a motion. Hence, the redescription of the phenomena simplifies the task of explaining it.

Of course, the "law of the whole" cannot be properly described or understood without assuming a rectilinear orientation. Once the orientation has been presupposed, it becomes possible to describe the "law of the whole" as a real magnetic virtue that follows its structure. And it is on the basis of this virtue that Gilbert explains the fixity of the earth's poles. Gilbert, in other words, has endowed the universe with an orientation. He has added a rectilinear orientation to his representation of cosmic space.

3.4 CONCLUSION

William Gilbert attempted to respond to part of the explanatory challenge posed by Copernicus's heliocentric description of the solar system. He tried to account for two of the three motions of the earth Copernicus had described but left unexplained. Gilbert carefully investigated and described the phenomena associated with magnets; spherical magnets, or terrella, in particular. He then drew a detailed analogy between terrellae and the earth itself. He showed how the phenomena of one could be described in the same way as the phenomena of the other. On the basis of this similarity, Gilbert concluded that the earth *is* a magnet and suggested that its motions are *caused* by its magnetic nature. His attempts to show *how* a magnetic nature could cause the rotation of the earth, however, were unsuccessful.

Gilbert's argument is mainly descriptive. The point is to describe the phenomena associated with magnets and the earth in the same way so that the two can be identified under the rubric of magnetism. The ultimate cause of magnetism, however, is ascribed to an unelucidated, Aristotelian form. Gilbert's descriptions, meanwhile, rely on a geographic representation of space quite different from the spherical frameworks presupposed by Aristotle, Ptolemy, and Copernicus. Gilbert's magnetic spheres are described by referring to poles, not centers. The descriptive similarity at the crux of Gilbert's work hinges on his "discovery" of this geometric structure in magnets and the earth. That is, the argument relies on the fact that the geographic representation of space underwrites descriptions applicable to all spherical magnets, the earth included.

The geographic representation of space is also particularly important for Gilbert's treatment of Copernicus's "third motion" of the earth. The representation warrants Gilbert's appeal to the earth's axis and its "verticity." By extrapolating this geometric feature of the earth to the universe itself, Gilbert establishes a fixed, rectilinear orientation of space. On the basis of this orientation, he can then describe the behavior of the earth's axis as a "staying" rather than a motion. He can also appeal to a simple cause – the "law of the whole," which instantiates the orientation – to explain the phenomena. Rather than confront the explanatory problem posed by the "third motion" directly, Gilbert shifted the conceptual basis of description, thereby greatly simplifying the explanatory task.

As we shall see in the next chapter, when Kepler later found himself in a similar explanatory bind, he followed the same path out of the quandary. To make the explanatory task tractable, Kepler shifted the conceptual basis of description by adding a rectilinear orientation to his representation of space.

4.0 KEPLER AND THE DISCOVERY OF COSMIC LINEARITY

4.1 PROLOGUE: *O MALE FACTUM*!

In the *Mysterium Cosmographicum* of 1596, the twenty-five-year-old Johannes Kepler rashly banished lines from use in "the pattern of the universe." The presupposition of lines does not allow the specification of privileged locations by which one could construct a spatially ordered universe.[1] Lines "scarcely admit of order," Kepler wrote. Hence, God Himself could have no use for them in laying out the structure of this "complete, thoroughly ordered, and most splendid universe."[2] Twenty-five years later, Kepler reissued his *Mysterium* with additional notes and revisions. To the passage repudiating lines, he appended a note remarkable for its exclamatory tone:

> [*O male factum*.] O, what a mistake! Are we to reject them [lines] from the universe?... For why should we eliminate lines from the archetype of the universe, seeing that God represented lines in his own work, that is, the motions of the planets?[3]

In the years between editions of the *Mysterium*, Kepler had discovered that God had use for lines, after all. They were necessary to lay out the elliptical orbits of the planets. Thus, lines were essential parts of the universe: they were elements in God's transcendental archetype of the creation.

What is it that caused Kepler, in 1621, to lament the rashness of his youth? How did the motions of the planets demonstrate God's need for lines? The answer lies deep in the details of

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[1] As Epicurus had argued. See chapter 1 above.
[2] Johannes Kepler, *Mysterium Cosmographicum: The Secret of the Universe*, trans. A. M. Duncan (New York: Abaris, 1981), 95-97. Kepler does admit finite straightness as the "distinguishing features" – i.e., the geometrical boundaries – of solid bodies. His argument is that the universe itself is inherently ordered by God and, therefore, laid out spherically about a single center.
[3] Ibid., 102-3.

Kepler's discovery of elliptical orbits. Kepler struggled to find an empirically adequate description and physically plausible explanation of Mars's path through the cosmos. He found, however, that a spherical representation of space was insufficient to describe and explain the motion of the planet. The solution came only when Kepler turned to William Gilbert's magnetism and the rectilinear spatial orientation instantiated by the "law of the whole." Adopting an oriented space, Kepler could finally derive the ellipse – the true path of the planet. In so doing, Kepler had shifted, at least partially, to a rectilinear representation of space, introducing lines into the universe.

4.2 INTRODUCTION: SOURCES AND AIMS

In what follows, we will attempt to reconstruct Kepler's discovery of the elliptical orbit of Mars. Kepler came to the ellipse sometime in 1605 and published his achievement in the *Astronomia Nova* (1609).[4] Our investigation, however, will be based on a lengthy letter written by Kepler to David Fabricius, an East Frisian cleric whom Kepler had encountered when both were assistants of Tycho Brahe in Prague in 1601.[5] This approach is a stone aimed at several birds. It allows us to sidestep a historiographical problem that afflicts all reconstructions of the development of the *Astronomia Nova*. Kepler himself presents the *Astronomia Nova* as a narrative history of his thinking. It is clear, however, that the narrative style is a rhetorical device employed to make an argument, so its historical veracity is questionable.[6] One cannot, therefore, use the *Astronomia*

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[4] Johannes Kepler, *New Astronomy*, trans. William H. Donahue (Cambridge: Cambridge University Press, 1992).

[5] See appendix. Fabricius and Kepler never actually met, though each worked for Tycho for a short, but separate, period. Over the years, the pair exchanged numerous correspondences. In this essay, though, we are only interested in Kepler's 1605 letter, which served as a "scratch-pad" for the ellipse. James R. Voelkel, *The Composition of Kepler's Astronomia Nova* (Princeton: Princeton University Press, 2001), 170.

[6] The *Astronomia Nova* is constructed as a straightforward narrative of chronologically ordered facts. In effect, Kepler says, "First I did A, then I did B, then I did C, and eventually I found D." The expositions is meant to lead the reader inexorably through Kepler's logic so that he or she also arrives at D (much like a cruder form of Descartes' *Meditations*). However, a perusal of Kepler's notes, correspondence, and other materials indicates that Kepler's story is a lie. At best, the narrative in the *Astronomia Nova* is *suggestive* of Kepler's process of discovery, but it is clear that it is not completely accurate. In actuality, Kepler might have first done B, then did A, puttered off into dead ends X, Y, and Z, figured out the answer had to be D, then found C, and so on. The narrative in the *Astronomia Nova* is false. It does not describe what it purports to describe, namely, Kepler's method of discovery. Yet just what *was* Kepler's method and *how* suggestive his narrative is are questions open to and demanding of historical investigation.

Nova as a historical source without leaving significant questions of interpretation open to dispute. This letter, on the other hand, was begun sometime in early 1605 (probably February, perhaps March), set aside and resumed several times,[7] and finally posted on 11 October 1605. As a result, the letter presents a candid, if incomplete, chronology of Kepler's thinking. We can be fairly certain that the sequence of arguments in the letter was in fact the sequence of Kepler's thoughts.

During this same period, Kepler was also busy composing his "Commentaries on Mars," which were to become the *Astronomia Nova*.[8] It was sometime in the late summer or early fall of 1605, while the letter was still on his desk, that Kepler completed his initial work on the ellipse, a process he describes in detail to Fabricius. Clearly, Kepler subsequently referred to the letter while writing the book. Sections of the letter appear, almost verbatim, in the text, especially in chapters 56-60, though the *Astronomia Nova* reorders the sequence of arguments presented in the letter. Still, the book is useful to fill lacunae in the letter, and *vice versa*. Taken together, then, the letter, supplemented by the *Astronomia Nova* as it was eventually published, provides an accurate and detailed picture of Kepler's thoughts, especially regarding the crucial discovery of the ellipse.

By the same token, this chapter aims to provide a fairly detailed exegesis of some of the content of a letter historians almost universally acknowledge as significant, but to which they rarely devote more than cursory attention. Still, there is a great deal of the letter we must leave aside.

Finally, and most importantly, the letter to Fabricius sets into high relief the importance of William Gilbert's influence on Kepler's thought. This influence appears, of course, in the *Astronomia Nova*, but its significance is shaded amongst the greater breadth of issues addressed in the book. In the letter, Gilbert's ideas lead directly and almost immediately to the ellipse. In the book, the path from one to the other is more convoluted and less clear.

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[7] Unfortunately, I did not have access to manuscript copies of this letter, so I am not entirely sure where the letter breaks off and resumes. On the other hand, Kepler usually indicates a hiatus in the text: "I return to [Mars] after some weeks…," for example. Kepler's manuscript is in the Archive of the Russian Academy of Sciences, St. Petersburg, designated Pulkovo X.

[8] Kepler had completed a great deal of the *Astronomia Nova* before 1605. Comparing the contents of the letter to the finished book shows that he had probably already written most of chapters 1-55. Moreover, by March 1605, Kepler had sent a manuscript of the work to the Emperor and a description of its contents to Michael Maestlin. Kepler, *Astronomia Nova*.

4.3 BACKGROUND: RELIGIOUS EPISTEMOLOGY

To begin tracing the development of Kepler's conception of space, we must first comment on his metaphysical motivations and underlying assumptions, as these have important effects throughout his work. It is important to remark that Kepler was heavily influenced by religious considerations. A Protestant neoplatonism infused every aspect of his inquiries. In particular, he adopted the neoplatonic account of creation in which the created universe is an emanation of God's primordial intellect. This emanation proceeds according to the transcendental archetype or "Idea" of the universe, contained in the divine mind, and coeternal with it. The primordial Idea is thus the plan or form of the cosmos. When substantiated by God's overabundant, emanating being, this form generates the physical world.[9]

While Kepler's neoplatonism may have precluded him from taking Lutheran orders, his native Protestantism was no less important in shaping his beliefs.[10] For Protestantism stressed that all men were created in God's image. Thus, *all* men possessed an image of God's mind and the primordial Idea therein. Therefore, by studying God's words, in scripture, and works, in nature, man could come to some understanding of God's own divine intellect, including the transcendental form of the world. This understanding was open to all. It did not require the intercession of divine inspiration. All could share in some knowledge of God's ways, which were accessible to all mankind.[11]

Of course, according to Christian teaching in general, human understanding was always flawed. It could never match the perfection of the divine mind. Yet, on Kepler's view, one could approximate divine understanding and, by diligent effort, continually improve one's approximation. Thus, human knowledge could asymptotically approach God's, just as straight lines could asymptotically approximate curves.

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[9] It is this "pattern" or "archetype," by the way, from which Kepler banishes infinite lines in the first edition of the *Mysterium*. See Barker and Goldstein, "Theological Foundations of Kepler's Astronomy."; Herbert Butterfield, *The Origins of Modern Science* (London: G. Bell, 1957), 75-79; Max Caspar, *Kepler*, trans. C. Doris Hellman (London: Abelard-Schuman, 1959), 376ff; Johannes Kepler, *The Harmony of the World*, trans. E. J. Aiton, A. M. Duncan, and J. V. Field (Philadelphia: American Philosophical Society, 1997), xiii-xiv; Kepler, *Mysterium*, 23ff. and ch. II.

[10] See Barker and Goldstein, "Theological Foundations of Kepler's Astronomy."; Caspar, *Kepler*, 51-52.

[11] See Barker and Goldstein, "Theological Foundations of Kepler's Astronomy," 99; Caspar, *Kepler*, 375ff; Kepler, *Mysterium*, 55f; Voelkel, *The Composition of Kepler's Astronomia Nova*, 32, 60.

> For in this one respect Nicholas of Cusa and others seem to me divine, that they attached so much importance to the relationship between a straight and a curved line and dared to liken a curve to God, a straight line to his creatures; and those who tried to compare the Creator to his creatures, God to Man, and divine judgments to human judgments did not perform much more valuable a service than those who tried to compare a curve with a straight line, a circle with a square.[12]

The approximation between God's knowledge and Man's can always be closer, but never perfect.[13]

Altogether, these religious and philosophical beliefs entail constraints on the nature of God's knowledge and the universal archetype. Most importantly, the universal Idea must at least resemble something comprehensible by a human intellect, since, in the fullness of time, humankind's understanding can come arbitrarily close to the divine. Thus, God's design is not shrouded in impenetrable mystery, but something nearly comprehensible. In effect, this means that the principles by which the universe operates are at least amenable to rational investigation. Conversely, the universe cannot operate on principles that are impossible to understand. For example, the ratio between two related physical quantities cannot be arbitrary, according to Kepler. It must be "knowable," where "knowability" is determined by the powers of human reasoning.[14]

This epistemic constraint had far reaching implications for Kepler's investigation of nature. For one thing, Kepler rejected the operational "Wittenberg interpretation"[15] of Copernicus that prevailed amongst his peers. On this view, astronomy was only meant to "produce calculations that agree with observations,"[16] that is, accurate predictions of planetary motions. Astronomical models, therefore, were merely "hypotheses" – mathematical conceits that facilitated calculation. The causes of the motions were beyond the scope of science, because the omnipotent God could arrange the heavens in innumerable ways to produce the observed

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[12] Kepler, *Mysterium*, 93.
[13] For discussion of Cusa's influence on Kepler, see E. J. Aiton, "Infinitesimals and the Area Law," in *Internationales Kepler-Symposium, Weil der Stadt 1971*, ed. Fritz Krafft, Karl Meyer, and Bernhard Sticker (Hildesheim: Gerstenberg, 1973); Kepler, *Mysterium*, 24.
[14] In the *Harmonice Mundi*, for example, Kepler argues that irrational proportions are unknowable, even by God. Kepler, *Harmonice Mundi*, 139.
[15] Westman, "The Melanchthon Circle, Rheticus, and the Wittenberg Interpretation of the Copernican Theory."
[16] Osiander's anonymous preface to the *De Revolutionibus*. Copernicus, *De Rev*, 22. Osiander's preface is the *locus classicus* of the "Wittenberg" interpretation. Significantly, it was Kepler who famously revealed its authorship in the *Astronomia Nova*.

phenomena. Simply put, the astronomer *could not know* how the universe was really arranged. Astronomy was merely a descriptive science. Thus, Copernicus's heliocentrism was merely a useful calculating tool, whose physical reality was beyond human knowledge.

Kepler, on the other hand, rejected *a priori* limits on the human understanding of nature. He thought that understanding the physical causes of planetary motions was within the realm of possible human knowledge. The physical reality of hypotheses, therefore, was within the purview of astronomy. As a result, Kepler set out to construct an astronomy that could both describe the heavens and explain them. He called the eventual fruition of this project the *New Astronomy Based Upon Causes or Celestial Physics* – the *Astronomia Nova*. Kepler's astronomy was, if not entirely novel, then at least exceptional precisely because it considered the physical causes of celestial phenomena.[17]

Given this attitude, Kepler had two general desiderata for astronomical hypotheses. First, they had to accurately describe phenomena. That is, he desired a way to calculate planetary positions that agreed with what was actually observed in the sky, in the future, present, or past. This just meant deriving a longitude and latitude of a planet for a given time. Of course, none of Kepler's contemporaries (or even predecessors) would have disputed that this was a central goal of astronomy. However, Kepler was also interested in the physical reality of his hypotheses. Now, these models also predicted planetary distances, which could also be checked against observations, at least indirectly. A planetary model could not be considered an accurate description of phenomena if it did not predict or retrodict proper distances. Thus, Kepler requires that a hypothesis agree with observations in three respects: longitude, latitude, and distance. In the present chapter, we will be concerned primarily with the longitudes (which Kepler calls "eccentric equations") and distances.

Second, Kepler required that astronomical hypotheses have plausible physical explanations. This desideratum must be situated in the context of Kepler's philosophical and religious commitments. Since God's creation is, in principle, comprehensible, so must the causes of planetary motions. This is not to say, however, that Kepler requires absolute proof that a cause he proposes is *the* cause of the observed behavior. God could always do things differently. Still, God's action is amenable to human grasp, so the cause must be something

[17] See Jardine, *The Birth of History and Philosophy of Science: Kepler's A Defence of Tycho Against Ursus*, esp. chs. 6-7.

accessible to reason. Hypotheses that do not admit reasonable explanations cannot be realized. So, when Kepler suggests a physical explanation, he seeks to demonstrate only that the hypothesis is *compatible* with human reason, and thus within the realm of possible truth. Kepler only requires that a hypothesis is physically *plausible* – it can be explained in a humanly comprehensible manner.[18] In general, Kepler's discussion of physical causes is in the hypothetical voice. He leaves aside the question of whether the proposed explanation really accounts for the phenomena.[19]

Still, Kepler sometimes blurs this fine distinction. A case in point is Kepler's talk of "planetary minds."[20] Often it seems that Kepler is really attributing spiritual minds to the planets. He then implies that the minds are responsible for moving the planets about. One might think, then, that the minds are to be taken as the causes of planetary motions. This is not Kepler's real meaning. Instead, Kepler uses minds as stand-ins for physical mechanisms he does not understand. This is coherent if we remember that Kepler is only trying to test the plausibility of his hypotheses. Could an ellipse, Kepler asks, for example, be comprehensibly constructed (other than by mere stipulation)? The easiest way to answer this is to assume that the planet itself is rational. Then, if the planetary mind has a method to "measure" its position and "deduce" its proper movement, then the resulting path can be rationally constructed. If this is the case, then the ellipse is a possible path for the planet. The unknown *real* cause of the motion will *a fortiori* be rationally comprehensible, as well.[21]

This attitude also accounts for Kepler's ubiquitous appeals to magnetism. In a word, actions at a distance are, for the seventeenth century theorist, weird. The obvious relationship between the several heavenly bodies, including the earth, is very difficult to explain given the immense distances separating them. In Kepler's view, however, magnetism is an action at a

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[18] We say rather "plausible," rather than "possible" to emphasize the epistemic constraint on hypotheses. All hypotheses are "possible," but only those that admit reasonable explanation are "plausible." Note that Kepler's criteria for admissible hypothesis is not so different from Copernicus's reason for rejecting the Ptolemaic system. Copernicus argued that Ptolemy's use of an equant did not admit of explanation on the basis of accepted physical principles, and was therefore implausible.

[19] Kepler writes that all physical sciences include "a certain amount of conjecture." Kepler, *Astronomia Nova*, 47.

[20] E.g., in Johannes Kepler, *Epitome of Copernican Astronomy & Harmonies of the World*, trans. Charles Glenn Wallis (Amherst: Prometheus Books, 1995), 52ff; Kepler, *Astronomia Nova*, ch. 57.

[21] Bruce Stephenson, *Kepler's Physical Astronomy* (Princeton: Princeton University Press, 1994), 3. Kepler's method here is akin to Descartes' subsequent method of radical doubt in his *Meditations on First Philosophy*. Just as Kepler assumes planetary minds as a limiting case of knowability, Descartes assumes a deceiving Demon as a limiting case of unknowability. See René Descartes, *Meditations on First Philosophy*, trans. John Cottingham (Cambridge: Cambridge University Press, 1996), 12-15.

distance that lies within the grasp of human understanding. This is merely a stipulation. Kepler does not speculate on the mechanism underlying magnetic action.[22] Yet, if Kepler is able to make the planets seem like they are responding to some cause *similar to* magnetism, Kepler can then claim that they have a plausible physical cause. As a result, Kepler slathers magnetism on the heavens with a thick brush, using it when he requires some sort of action at a distance. Seen in this light, Kepler's claims about magnetic forces are similar to his claims about planetary minds. In both cases, he is using his proposed cause to set a limit on the unknown true cause. Since the former is within the realm of reason, so must be the latter. Kepler, despite his apparent sincerity, should not be taken as claiming that magnetism and minds are real causes. The upshot of his physical explanations is only that the effects he explains are, because they are explicable, possibly real. We will return to these points at the end of our discussion.[23]

4.4 SETTING UP

For the sake of getting to the point, we will pass over Kepler's rejection of earlier planetary hypotheses and the correction of the earth's orbit (i.e., the material presented in chapters 1-39 of the *Astronomia Nova*). These topics have been addressed elsewhere,[24] and there is no need to rehearse them here. By the time Kepler begins his letter to Fabricius, he already knows the orbit of Mars is not circular, but somehow "narrower" than a circle. He also knows how to correct observations in order to calculate accurate planetary distances. The problem at hand, then, is the construction of this "narrower" orbit so that it properly accounts for observations, including distances, and can be plausibly explained by physical causes.

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[22] Magnetism is a mundane phenomenon. Kepler assumes that it is *ipso facto* humanly comprehensible. See Rhonda Martens, *Kepler's Philosophy and the New Astronomy* (Princeton: Princeton University Press, 2000), 81-4.

[23] Consider, for example, Kepler's descriptions of the "*anima motrix*" in the *Mysterium Cosmographicum* and *Astronomia Nova*. Kepler describes this force broadly, as "magnetic." The claim is not that it really is the same force as that between terrestrial magnets, and Kepler never compares the sun's force with terrestrial magnetism. Indeed, the prehensive force acts in ways no magnet would. It suffices for Kepler that it is like magnetism, or "magnetical," and therefore reasonable. See also Kepler's casual willingness to exchange planetary minds for magnetic action in the *Astronomia Nova*. Barker and Goldstein, "Theological Foundations of Kepler's Astronomy," 109; Kepler, *Mysterium*, ch. 22; Kepler, *Astronomia Nova*, chs. 34, 57.

[24] Stephenson, *Kepler's Physical Astronomy;* Voelkel, *The Composition of Kepler's Astronomia Nova;* Curtis Wilson, "Kepler's Derivation of the Elliptical Path," *Isis* 59, no. 1 (1968).

It must be noted that, by early 1605, part of the physical story had already been told. As early as the *Mysterium Cosmographicum* (1595), Kepler proposed an *anima motrix* emanating from the sun which caused the revolutions of the planets. This "extrinsic force" is something similar to both light and magnetism (Kepler's description changes over time), and decreases in some relation to distance. Kepler generally assumes that force is proportional to speed. Thus, Kepler could explain why planets further from the sun have longer periods, as well as why a planet orbits faster near perihelion than at aphelion. By 1605, Kepler had settled on a magnetic action emanating from the sun such that there was a direct proportion between distance from the sun and "delay" in an arc of the orbit. That is, the time a planet took to traverse equal (small) arcs of the orbit was proportional to the distance the (small) arc was from the sun. This "distance law" is presented in chapter 32 of the *Astronomia Nova*.[25]

A planet's general revolution around the sun could be explained by this "magnetic" force. However, planets do not move at constant speed around the sun. Instead, they speed up and slow down as they approach and recede from the sun. Kepler attributed this change in speed to a second power, a *vis insita* inherent in the planet itself, which somehow regulates a planet's distance from the sun.[26] If the planet can move itself to the correct distance, the sun's own "magnetic" power will account for the speed of the revolution. Thus, as Kepler begins his letter to Fabricius, he is concerned with the nature of this planetary *vis insita*. He needs to figure out how the planet gets itself to the right distance in order to be carried along at the proper speed. First, however, Kepler needs to figure out what the right distances are. In other words, Kepler seeks both an accurate description of the orbit and the nature of the intrinsic force that explains it.

As we shall see in what follows, Kepler works both ends towards the middle. Though quite confusing at times, this method is a particularly clear demonstration of the iterative process of reciprocation between description and explanation. When faced with failures of a hypothesis's descriptive accuracy, Kepler considers its physical causes. When faced with physical problems, he considers descriptions. Thus, Kepler's thought plays out as a continual solving and posing of objections and obstacles, such that the essential problem he tries to solve

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[25] Kepler, *Mysterium*, 62-65; Kepler, *Astronomia Nova*, 372-75. By 1605, Kepler only possessed a preliminary version of the Area Law for which he is famous. It was not worked out in full generality until the *Epitome*. See Aiton, "Infinitesimals and the Area Law."; Stephenson, *Kepler's Physical Astronomy*, 161ff.
[26] Kepler, *Astronomia Nova*, 404ff.

gets pushed around like a bump in a rug. One difficulty is reduced to another and then converted to a third. Our exposition will follow the general problematic shifts in the letter to Fabricius, culminating in the ultimate derivation of the ellipse.

4.5 THE PROBLEM OF SHAPE: THE ELLIPSE

At the beginning of the letter, Kepler is working on descriptions. He is trying to construct a geometric model that will generate accurate predictions of planetary positions. In this context, Kepler reconsiders two planetary hypotheses he had constructed previously, in 1602 and 1603. Both are based on a concentric deferent-epicycle system with an equant, as in Figure 5. Thus, the planet (F) moves on an epicycle (RSF) around a point (D) that moves around a deferent (EDG) with speed uniform at an equant point (C). The sun (A), meanwhile, is the center of the deferent, on a line connecting the apsides (E and G) and the equant (C). In this system, Kepler assumes that the epicycle rotates uniformly, completing one revolution in a Martian year. Thus, when the epicycle is centered at D, the planet will have rotated through an angle equal to ECD, the mean anomaly of the planet's motion. The two hypotheses differ as to where in the epicycle this angle is measured from. In other words, Kepler asks, where would the planet be if the epicycle *did not* rotate? In Kepler's terminology, what is the "true apsis" of the epicycle?

If the angle is measured from the line connecting the center of the epicycle, D, and the equant point, C, the path of the planet will be a circle, centered on the equant point, with a radius equal to the deferent's, AE.

> And thus, if the line CDR equals the true apogee[27] of the epicycle, then the path of the planet follows a perfect circle. For DF is led parallel to AC, RDO equals ADC, and ODF equals DAE, and RDF equals DCE, the mean anomaly, because the period [return] of the epicycle and concentric are equal… Now, joining F and

[27] Kepler consistently labels the apsides of the Martian orbit "apogee" and "perigee." This is a confusing mistake, held over from Ptolemaic astronomy, when the Earth was considered the center of the orbit. He should call them "aphelion" and "perihelion," since they designate maximal and minimal distances to the sun, not the earth.

> A makes a line so long, that if from C a perfect eccentric is described with radius AE, it will indeed cross at F.[28]

In this hypothesis, the angle RDF will always equal angle DCE, and line DF will always be parallel to the line of apsides, AC. The result is simply to shift the deferent along the line of apsides a distance equal to DF, which is assumed to be equal to AC. Thus, the path will be a circle, equal in size to the deferent, with radius AE, but centered on the equant point, as in Figure 6.

Alternatively, if the true apsis of the epicycle is the line connecting the center of the epicycle, D, with the sun, A, the radius of the epicycle containing the planet, DF, will "incline" toward the apsidal line EG.

> On the contrary, C remains the equant point of D, line ADO follows the line of true apsides of the epicycle, and O is the true apsis of the epicycle; as it is DCE, the mean anomaly, is constituted equally by ODF, and DF is inclined to AC, which is equally if the said epicycle moves equally in equal time around its center. Now this is the very close hypothesis, which I use in 1603… And which had a tolerable natural cause.[29]

Angle ODF is equal to DCE, so the line DF will be more "inclined" to AC than if DF were parallel to AC. Thus, the path generated by this model will fall inside the path generated by the previous model. In other words, the path of the planet will be "narrower" than a circle. Indeed, Kepler has already discovered that this path forms an ellipse, as in Figure 7.

Both these models, however, fail to describe the phenomena. Specifically, they inaccurately predict and retrodict planetary distances. Their failure is intriguing, however, because the 1602 circular orbit is too wide by exactly the same proportion that the 1603 ellipse is too narrow – 429 parts of 100,000. Thus, the true path of Mars must fall exactly between the circumscribing circle and the inscribed ellipse (see Figure 8):

> Seeing the distances constructed from a perfectly circular eccentric sin in excess... just as much as my ellipse (which varies very little from the oval), which I described numerically to you above, sins in the defect: very rightly I have argued this [following] way. The circle and ellipse are from the same genre of figures, and fail equally in different ways, therefore the truth is in the middle, and the

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[28] Johannes Kepler, *Johannes Kepler Gesammelte Werke*, ed. Walther von Dyck and Max Caspar, vol. XV (München: C.H. Beck, 1937), 248.
[29] Ibid.

> figures between ellipses are nothing but ellipses. And thus, the path of mars is definitely an ellipse, the leftover little moon shape [*lunula*] of half the width of the previous ellipse.[30]

The true width of the orbit (its radius at around 90° anomaly), derived from observations, is precisely halfway between the circle and the ellipse: the former "sins in excess" exactly as much as the latter "sins in defect." Still, geometrically speaking, the only figure that falls exactly between a circle and an ellipse is another ellipse. Therefore, Kepler concludes, the true path of Mars must be elliptical.

The moment Kepler wrote this, sometime in the spring of 1605, is, in one sense, the "discovery of the ellipse."[31] While it is true that Kepler became convinced that the planet of the true orbit could be described as an ellipse,[32] this was not the definitive statement it might seem. Significant problems remained. Most importantly, at this point, Kepler did not know how to construct the proper ellipse. He had no geometric model that would generate the correct path. Nor did Kepler have any plausible physical explanation for an elliptical path besides the faulty one he had already derived and dismissed. Kepler may have known an ellipse was his ultimate goal, but he had no idea how to get there.

4.6 THE PROBLEM OF DISTANCE: THE SECANT MODEL

As Kepler writes to Fabricius, one possible construction of the ellipse is suggested by the failed epicyclic models. There is a third, as yet unexplored, possibility for the "true apsis" of the epicycle, namely, the line connecting the epicyclic center, D, with some point on the apsidal line between the sun and the equant:

> But, deducing from the first excess [of the eccentric] and the second defect [of the ellipse], CA is to be halved or bisected in B, and BDS is to be the true apsis of the epicycle, and thus C hitherto the equant center of D. But now, SDF is equal to

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[30] Ibid., 247-8.
[31] See Caspar, *Kepler*, 134; Voelkel, *The Composition of Kepler's Astronomia Nova*, 189-90.
[32] Kepler had at least considered the possibility of elliptical orbits earlier. As early as July 1603, he wrote to Fabricius, "if only the shape [of the orbit] were a perfect ellipse all the answers could be found in Archimedes' and Appollonius' work." But Kepler did not actually accept that the orbit *was* an ellipse at this point. Kepler, *Werke XV*, 409f; Koestler, *The Sleepwalkers*, 330.

> DCE, the mean anomaly, and DF is less inclined to AC than before. And with this hypothesis, the distance of F from A is now closer to the truth than before, and FAE is closer to the true coequated [anomaly]. Indeed, I say "closer," but not "true," so as to allow the construction of the computation of the physical equation.[33]

If the eccentricity of the equant is "bisected" – that is, by a point B (in Figure 9) precisely halfway between the sun, A, and the equant, C – then the radius of the epicycle including the planet, DF, will be roughly half as inclined to the line of apsides as in the prior ellipse. This, it seems to Kepler, could generate an orbit half as "narrowed" as before, as the true orbit must be. The model, Kepler thought, might describe the phenomena more accurately than before. Considering causes, however, Kepler was not particularly pleased with this model. He did not see how it could be physically explicable:

> Still, this hypothesis (as I may go on in the laying out of my ratiocinations) did not satisfy me, since the point B lacked a natural cause. For point C will have a natural cause, which is to say AC and DF are made equal, which amounts to, if I may say, the distances being [proportional to] the delays in equal arcs of the eccentric. On the other hand, another thing attracted me to the natural cause: this, of course, which I have seen helping the secant of the greatest equation of the epicycle.[34]

The eccentric circle around the equant (point C) has a reasonable physical cause since it results in the planet-sun distances being proportional to the time taken to traverse each arc of the eccentric, which conforms to the area law governing the magnetic force moving the planets. Similarly, the 1603 ellipse has a "tolerable natural cause" in that the rotation of the epicycle was measured from the line to the sun, a real, physical body. In the present model, however, the planet would have to move itself with respect to point B, which is an empty point in space with no physical significance. There does not seem to be any physically plausible way a planet could relate its motion to an empty point. The hypothesis might be "closer," but without a plausible explanation it could not be "true."[35]

Despite his dissatisfaction with the physical causes of his model, Kepler began calculating the distances it predicted at various points in the orbit. He did not get far. The model would, of course, be accurate at aphelion and perihelion, where it predicted the same distances as

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[33] Kepler, *Werke XV*, 249.
[34] Ibid.
[35] See Kepler, *Astronomia Nova*, 127, 410.

the two previous models, but what would it predict around quadrature? The result of this calculation, probably Kepler's first (it is the only one he reports), brought him up short:

> On the other hand, another thing attracted me to the natural cause: this, of course, which I have seen helping the secant of the greatest equation of the epicycle. AF [see Figure 10], that is, (at angle 5 degrees 18 minutes) would be 100429. And thus, FA is longer than DA, by 429 small parts.[36]

Around quadrature, the epicycle would reach its "greatest equation" – the point at which the planet is maximally elongated from the radius of the deferent, and thus where the angular correction of the planetary longitude derived from the epicycle is greatest. This position is easily calculated since it entails the radius of the epicycle including the planet, DF, is perpendicular to the radius of the deferent including the center of the epicycle, DA. If we assume, as Kepler did, the radius of the deferent is 100,000 units, and that of the epicycle is 9264,[37] the angle of elongation at "greatest equation" will be 5° 18' 30", and the distance from the planet to the sun will be 100,429, the secant of the angle (normalized to the radius).[38]

This result, 100,429, struck Kepler because 429 parts was exactly what he needed for the "true hypothesis." The eccentric had to be "narrowed," and the ellipse "widened," by precisely this amount. Thus, if the secant of the angle were substituted for the radius of the deferent, the "right distances" would be produced in the "middle longitudes" – i.e., near quadrature, when the planet had completed about a quarter of its orbit.[39]

Instead of completing his calculations for the epicyclic model, whose physical causes he doubted, Kepler took this fortuitous calculation to suggest an entirely new model of the Martian orbit.

> And because the distance FA follows from the re-utilization of the perfect eccentric, and 429, found above, is exactly the shortening of distances for the true

[36] Kepler, *Werke XV*, 249.
[37] This is equal to half the eccentricity, which is determined (from observations) by the apsidal distances.
[38] Kepler did not happen on this calculation "quite by chance," as he reports in the *Astronomia Nova*. He was checking the predictions of a possible hypothesis. Kepler, *Astronomia Nova*, 543.
[39] The meaning of "middle longitude" is vague, since a "quarter" of the orbit can be measured several different ways – e.g., by mean anomaly, coequated anomaly, eccentric anomaly, etc. – all of which are similar but not equivalent. Here, Kepler is working with the "width" of the orbit – the point of maximum elongation from the apsidal line. This will be equivalent to the distance at 90° eccentric anomaly, but he has not established this yet.

> hypothesis. Therefore, if we substitute FA for DA, we have the right distances in the middle longitudes.[40]

He resurrects an eccentric orbit,[41] now centered on B, the point halfway between sun and equant, and substitutes the secant of the optical equation (the angle formed at the planet by the rays connecting it to the eccentric center and to the sun) for the radius. This substitution gives the "right distances in the middle longitudes" as well as at apsides, where the optical equation is null.

In effect, however, this substitution treats the planet as if it were always along the line connecting the sun to the center of the epicycle, rather than somewhere on the epicycle's circumference.[42] For example, when the planet is 100,429 units from the sun, it will be at "middle longitudes" only if it is at the center of the epicycle, not on its circumference. As a result, Kepler is rejecting the epicyclic model in favor of a "libration:"

> At once, I seized on this for the natural hypothesis: the planet does not rotate in the circumference of the epicycle GFI, but librates in the diameter HDK. And now I constructed the distances and the whole table of equations from this.[43]

If the planet were to "librate" – reciprocate sinusoidally – along the diameter of the epicycle, it would be constrained to the line connecting the center of the epicycle with the sun, as required by the substitution of the secant. Optimistic about this possibility, Kepler leaves off his letter intending to test the positions predicted by his new theory against observations.

Some time later, however, Kepler returns to the letter despondent:

> But yet, I am a wretch. [*At miser.*] This Easter holiday at last I tested the thing, which I had been considering. I was able to remember now the earlier demonstration in my commentaries, that this kind of path of the planet does not compose an ellipse, which my above argument stated, but in the octants expands in cheeks from the ellipse towards the perfect circle.[44]

Kepler has compared his theory to observed positions. The libration model, so promising at middle longitudes, simply fails to work in the rest of the orbit. Yet, besides a diagram and the somewhat cryptic remark that "an earlier demonstration" had shown that the path was not an

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[40] Kepler, *Werke XV*, 249.
[41] Kepler had previously considered and rejected an eccentric orbit.
[42] Because AF is a line from the sun to the circumference, while AD is to the center. Substituting one for the other places the planet F on the line AD.
[43] Kepler, *Werke XV*, 249.
[44] Ibid.

ellipse, but a "puff-cheeked" orbit, Kepler does not elaborate the reasons for this failure. To follow the train of his thoughts during the letter's hiatus, we must turn to the other text Kepler was composing at the time, the *Astronomia Nova.*

We find the libration model presented in chapter 56. Here, Kepler first points out that the secant hypothesis is equivalent to one he has already calculated, albeit in a different context, in chapters 39-40. There, Kepler had used the secant of the optical equation to estimate the area swept out by the planet on a circular orbit.[45] Now, he uses the secant as measure of the true distance. Kepler reintroduces the diagrams (see Figure 11) from the earlier chapters, and summarizes the new model:

> …quite by chance I hit upon the secant of the angle 5° 18', which is the measure of the greatest optical equation. And when I saw this was 100,429, it was as if I were awakened from sleep to see a new light, and I began to reason thus. At the middle longitudes the lunule or shortening of the distances is greatest, and has the same magnitude as the excess of the secant of the greatest optical equation 100,429 over the radius 100,000. Therefore, if the radius is substituted for the secant at the middle longitude, this accomplishes what the observations suggest. And, in the diagram in chapter 40, I have concluded generally if you use HR instead of HA, VR instead of VA, and substitute EB for EA, and so on for all of them, the effect on all the eccentric positions will be the same as what was done here at the middle longitudes… And so the reader should peruse chapter 39 again. He will find that what the observations testify here was already urged there, from natural causes, namely that it appears reasonable that the planet perform some sort of reciprocation, as if moving on the diameter of the epicycle that is always directed toward the sun.[46]

Starting with an eccentric around B, the point halfway between the sun and equant, substitute the length of the secant of the optical equation (e.g., HR at angle AHR) for the radius to the sun (e.g., HA). Consequently, the planet will be found at some point X along HA, such that the distance XA is equal to HR. Just as the correct distance is given at middle longitudes, the correct distances will be given at "all the eccentric positions." As Kepler notes, this model suggests that the planet is "librating" along the radius from the planet to the sun. That is, in Figure 11, the planet moves along the moving radius HA, EA, VA, etc., such that at equated anomaly CAH, the planet has "descended" from the circumscribing eccentric CED along AH to point X. The

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[45] The secant is the height of the "Archimedean triangles" he employs to measure the area. See Kepler, *Astronomia Nova*, 422-3.
[46] Ibid., 544.

planet's approach toward the sun is maximal at equated anomaly EAB, after which the planet recedes, coinciding with the eccentric once more at perihelion, D.

Kepler's reference to the epicycle in the above passage elides the fact that this eccentric, secant model can also be reproduced by an equivalent epicyclic model. Assume a deferent of a radius equal to that of the eccentric, centered on the sun, and an epicycle of radius equal to the eccentricity, as in Figure 12. The secant model is exactly reproduced if the epicycle is assumed to rotate through an angle equal to the eccentric anomaly, measured from the radius to the sun (angle ODF), and the planet's position is given by a projection onto the same radius (i.e., X), instead of a point on the circumference of the epicycle. Thus, in this model, the planet also appears "to perform some sort of reciprocation, as if moving on the diameter of the epicycle that is always directed toward the sun." That is, the planet seems to librate along the diameter ODP, which is always along OA, the line to the sun.[47]

At the end of chapter 56, Kepler tests his secant/versed sine model against observations at known equated anomalies. He finds that the calculated distances agree with those derived from observations.

> For I previously used this same method of reciprocation to find out the distances of Mars from the sun which I presented in order to compute Mars's apparent positions [in chapters 39-40]. And since they are in agreement with the observations, they are therefore correct.

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[47] The planetary distances predicted by these two models are equivalent. In the epicyclic model (Figure 12), the distance is given as the sum of the deferent radius, AD, and the epicycle radius, DO, minus the descent, OX. That is:

$$\text{Distance} = AD + DO - OX$$

Now, AD is by assumption R, the radius of deferent and eccentric (assumed to be 100,000). DO is e, the eccentricity of the planetary orbit. By construction, OX is the versed sine of the eccentric anomaly ε. Thus:

$$\text{Distance} = R + e - e \text{ versine } \varepsilon$$

Versed sine is the same as 1 – cosine. Substituting, we have

$$\text{Distance} = R + e - e\,(1 - \cos \varepsilon)$$

Simplifying yields

$$\text{Distance} = R + e \cos \varepsilon$$

Now, in the eccentric-secant model (Figure 11), notice that the distance is the sum of the eccentric radius, R, and the small addition BR (for eccentric anomaly ε). Consider the small right triangle ABR. BR is equal to the cosine of angle ABR, which is equal to the eccentric anomaly, ε. Hence,

$$\text{Distance} = R + e \cos \varepsilon$$

as in the epicyclic model. Kepler himself prefers the epicylcic formula for distance, $R + e - e$ versine ε. His geometry does not include complementary functions, including cosines. Thus, the versed sine of the eccentric anomaly is a more direct calculation than the secant of the optical equation, which must be first derived from the eccentric anomaly.

> As you see, therefore, the distances measured on the diameter, found *a priori* in ch. 39, are confirmed by closely spaced and very reliable observations throughout the entire perimeter of the eccentric.[48]

Kepler had calculated the distances from the secants in chapter 39. Now he finds they are, indeed, accurate. This confirms that his calculation of distances using the secant or versed sine gives the true path of Mars. In other words, Kepler has now satisfied one part of his descriptive project. He knows how to derive satisfactory distances for a given anomaly. He has solved the problem of describing distance.

Several problems remain, however. Kepler's task is to give the actual place of the planet – its equated anomaly (longitude) and its distance – for a given time, expressed as a mean anomaly. So far, Kepler has computed a distance from a given equated anomaly, but he has not computed an unknown anomaly from a given time. He still needs to overcome this obstacle to derive the path of the planet.

4.7 THE PROBLEM OF LONGITUDE: THE ELLIPSE

Chapter 58 of the *Astronomia Nova* describes Kepler's first, ultimately faulty attempt to deduce longitude from time:

> Therefore, by what has previously been demonstrated in ch. 56, AE [in Figure 13] will indubitably be the correct distance at this eccentric anomaly. The question remains how much time was taken to arrive at it. Now the versed sine of its arc, GC, which, after multiplication, becomes LE, when subtracted from GA, yielded the correct distance AE. These indications persuaded me that the other end of AE should be sought… at the point I of the line DB, such that if I were to draw the arc EIF about center A with radius AE, it would intersect DB at I. Thus, according to this persuasion, AI would be the correct distance, both in position and length, and IAG would be the true equated anomaly.[49]

The comparison with observations completed in chapter 56 has convinced Kepler that the versed sine of the eccentric anomaly yields the correct distance. It seems natural to assume, then, that the eccentric anomaly would also give the longitudinal position of the planet. That is, the planet

[48] Kepler, *Astronomia Nova*, 546.
[49] Ibid., 574.

should be found at I, somewhere along the radius DB, where GBD is the eccentric anomaly. Leaving aside the thorny question of deriving the eccentric anomaly from the mean anomaly, both the distance AI and the equated anomaly IAG can then be derived from the eccentric anomaly by solving triangle AIB, where AI is the calculated distance, AB the eccentricity, and angle ABI the supplement of the eccentric anomaly. Thus, both distance and longitude can be calculated from the eccentric anomaly.

Much to Kepler's dismay, though, this solution fails. A quick glance at the distance calculations (already completed in chapter 40) shows that the planetary distances and longitudes are correct at apsides and quadrature. However, in between apsides and quadrature, at the "octants," the planet falls outside the ellipse of the proper width.[50] Thus, the path "expands in cheeks from the ellipse toward the perfect [eccentric] circle."

This, finally, brings us back to the despondent Kepler resuming his letter to Fabricius following the Easter holiday. The promising libration hypothesis, clearly correct at apsides and quadrature, has failed utterly. Kepler has realized that it is equivalent to "an earlier demonstration" – the one in chapters 39-40 of the *Astronomia Nova* – and that it produces a puff-cheeked orbit.

> The argument, therefore, has been all wrong: Libration in the diameter of the epicycle equals the ellipse in the middle longitudes and in apsides, therefore equals it wherever. False! And thus this [hypothesis], as before in the old false hypothesis, performs neither the duty of distances nor of eccentric equations. O fruitful society of both things [i.e., the earlier models], which never does not direct me into total perplexity.[51]

Despite Kepler's perplexity, however, the episode has not been a total loss. Though the orbit fails, Kepler has convinced himself that the versed sine of the eccentric anomaly (or the secant of the optical equation) yields accurate distances. The problem is that, if the planet moves in an ellipse, the eccentric anomaly is not measured to the planet itself. The issue, then, becomes one of correlating distance and longitude. That is, given an eccentric anomaly, which gives the distance, what is the equated anomaly – the longitude?

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[50] In the upper part of the orbit (around aphelion). In the letter to Fabricius, for reasons that are unclear, Kepler indicates that this is also true in the lower part of the orbit (around perihelion). In fact, near perihelion, the path falls *within* the ellipse, making the path "fat-headed" – too wide in the upper part and too narrow below – rather than "puff-cheeked." Kepler corrects this error in the *Astronomia Nova*, but retains the same name for the mistaken orbit.

[51] Kepler, *Werke XV*, 249.

At this point, Kepler falls back on what he knows for sure – the elliptical shape of the orbit:

> Therefore, I now have this, Fabricius: the path of the planet is truly an ellipse, which Dürer has similarly called an oval, or certainly insensibly different from some ellipse… And thus the whole hypothesis I will delineate to you.[52]

The orbit must be an ellipse, and therefore must have the properties of an ellipse. This requirement alone should give enough constraints to construct an orbit. Forgetting physical considerations for the time being, Kepler sets out to derive the orbit geometrically.

He assumes a construction like that in Figure 14. DEG is an ellipse inscribed in a circle DFG. Now, a property of the ellipse is that the proportion of FC to EC is the same as HB to IB, for all points F on the circle. Kepler also knows that if the radius of the circle DFG is 100,000, HI is 430 and BA is 9246. If the eccentric anomaly, ε, is given, again setting aside how this is derived from the mean anomaly, Kepler seeks to find the equated anomaly, γ, and the distance EA. He gives the derivation to Fabricius thus:

> Have the eccentric anomaly as you multiply the sine of 45 deg., 70711, in 430 parts, giving 303, which you take from the sine 70711, leaving 70408. You take therefore the sine of the complement of the eccentric anomaly, to it you add the eccentricity 9264 in the upper semicircle of the eccentric, that is from 270 to 90. You subtract in the lower, from 95 1/3 to 264 2/3. Or from the eccentricity you steal the sine of the complement if it is less. Then let it be that to that shortened sine, this sum or remainder, thus the whole sine to the tangent, which gives the angle of the coequated anomaly. This will be either the coequated anomaly itself, or the excess of the coequated over a semicircle, or otherwise the complement of these to the semicircle, for the thing born. Of this angle, you cut off a secant, and let it be that as the whole sine is to the sum or remainder, thus this secant is to the genuine distance from Mars to the sun.[53]

This passage is, to say the least, dense. We will take the opportunity to elaborate it in modern notation.

Kepler is deriving the orbit by solving triangle AEC. He assumes that the eccentric anomaly is 45°. By the properties of an ellipse, EC is to FC as IB is to HB. I.e.:

$$\frac{EC}{FC} = \frac{IB}{HB}$$

[52] Ibid., 249-50.
[53] Ibid., 250.

Now, FC is the sine of the eccentric anomaly, multiplied by the radius. HB is equal to the radius, and IB is the radius minus 430. Thus:

$$\frac{EC}{100{,}000\sin\varepsilon} = \frac{100{,}000 - 430}{100{,}000}$$

So,

$$EC = 100{,}000\sin\varepsilon - 430\sin\varepsilon$$

For ε equal to 45°, this is:

$$EC = 70711 - 303 = 70408$$

That is, EC is calculated as you "multiply the sine of 45°, 70711, in 430 parts, giving 303, which you take from the sine 70711, leaving 70408."

BC, meanwhile, is simply the radius multiplied by the cosine of the eccentric anomaly. In Kepler's terminology, this cosine is the "sine of the complement of the eccentric anomaly." Hence, AC, the second side of the triangle, is BC added to the eccentricity, AB, when the eccentric anomaly is less than 90°, "in the upper semicircle of the eccentric." When the anomaly is greater than 90°, side AC is the difference of the cosine and the eccentricity. Now, side EC, "that shortened sine," will be to side AC, "this sum or remainder," as the "whole sine," 100,000, is "to the tangent of the equated anomaly," γ. That is:

$$\frac{\tan\gamma}{100{,}000} = \frac{EC}{AC} = \frac{70408}{9246 + 100{,}000\cos\varepsilon}$$

Solving this equation yields γ, the equated anomaly.[54]

The secant of the equated anomaly gives the remaining side, AE, the true distance to the planet. As Kepler puts it, "you cut off a secant, and it may be that as the whole sine," 100,000, "is to the sum or remainder," side AC, "thus this secant is to the genuine distance from Mars to the sun," side AE. I.e.,

$$\frac{\sec\gamma}{AE} = \frac{100{,}000}{AC} = \frac{100{,}000}{9246 + 100{,}000\cos\varepsilon}$$

[54] Kepler does not calculate tangents (or sines) of greater than 90°. Thus he adds that the solution will either be the "equated anomaly itself" (for 0°<ε<90°), the "excess of the equated anomaly over a semicircle" (for 180°<ε<270°), or the complement of these to the semicircle (i.e., the supplement of the equated anomaly, for 90°<ε<180°, and the supplement of the excess, for 270°<ε<360°). In modern terms, the solution given is general.

The geometric validity of this equation is easily shown. Solving for AE gives the distance in terms of the eccentric anomaly and the equated anomaly. The latter, however, was just derived from the eccentric anomaly, so the distance is now also a function of the eccentric anomaly.[55]

It remains for Kepler to deduce the eccentric anomaly from a given mean anomaly. His area law dictates that the area swept out by the radius from the sun to a point on the eccentric (not the planet, as in the modern formulation) will be equal in equal times. Thus, in Figure 14, *area* DFA is the mean anomaly, not an angle, though the area is expressed as a proportionate angle.[56] This requires a general re-definition of astronomical terms:

> And so although DEG is shorter than DFG, nevertheless if DEG is allowed to be called 180 deg., then part DE is allowed to be called that which DF really has. Therefore, the eccentric anomaly here is DE. Thus not arc DBE, which had deceived me from the time of Christmas to this Easter time. FC is greater than EC as area DFA is to area DEA. Therefore... if area DEG retains the same name as area DFG, parts DEA and DFA will also have the same name, similarly DEB and DFB, as well as AEB and AFB, the area measuring the part of the physical equation. Therefore, if the circle is given, then DF, or DBF, will be the eccentric anomaly, and area DFA will be the mean anomaly. But now, in the ellipse, not DBE, but DE is the eccentric anomaly, and area DEA is the mean anomaly, and angle DAE is the coequated anomaly, and AE the true distance.[57]

Kepler uses angular measures on the circumscribing circle to designate points on the ellipse. Thus, arc DE is "named" by arc DF. The eccentric anomaly, really arc DE, is specified by angle DBF, where F is a point on the circle. Similarly, area DEA is the mean anomaly, but this is proportional to area DFA, and the two will "have the same name." Finally, the equated anomaly is, as before, the planetary longitude itself, angle DAE. The new terminology solves the problem which had "troubled" Kepler from the end of the previous year, when he had begun considering his two old models, to the construction of the *via buccosa* during the Easter holiday. The eccentric anomaly now measures the angle to D, not the planet itself. Thus, the planetary

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[55] Note that this is not the construction of the ellipse Kepler gives later in the letter or in the *Astronomia Nova*. Not satisfied with the physical basis of his secant/versed sine distance model, Kepler derives the planet-sun distance from the properties of the ellipse (i.e., geometrically). When he has secured the causes of his libration model, he calculates the distance directly (i.e., from the formula). See Kepler, *Werke XV*, 259; Kepler, *Astronomia Nova*, 592ff.

[56] For example, in one quarter of the periodic time, area DFA will contain one quarter of the area of the whole eccentric circle. Expressed as an angle, the mean anomaly is 90° – one quarter of 360°.

[57] Kepler, *Werke XV*, 250-1.

distance *can* be calculated from the eccentric anomaly, even if the planet itself is not along the radius to the eccentric center.[58]

Given this adjusted terminology, to find the eccentric anomaly from a given mean anomaly, Kepler has to find the area, DFA, of the circle that is to the whole circle as the mean anomaly is to the periodic time (when expressed as a time from apsides) or 360° (when expressed as an angle). Kepler, though, has no way to derive this area directly. He can only tabulate it, as he explains to Fabricius:

> From tables thus: the maximum equation from equations of triangular areas, which is 5 deg. 18 min. 30 sec., resolve this into seconds and divide this sum for all steps of the eccentric anomaly, then reduce back into the steps; and put [them] to your steps of the anomaly such that at 90 deg., the eccentric anomaly will be 5 deg. 18 min. 30 sec. Therefore, at 95 deg. 18 min. 30 sec. of mean anomaly, 90 deg. of eccentric anomaly is selected. Indirectly, the same eccentric anomaly is thus selected. While before semicircularity it is always less than mean anomaly, afterwards more, by conjecture you preconceive how much smaller, such that if a mean anomaly of 48 deg. 46 min. 0 sec. were given, I would conclude that the eccentric anomaly would be 45 deg. This sine in sums of seconds 5 deg. 18 min. 30 sec. multiplied and by 100000 divided, should give me 3 deg. 46 min. 0 sec. if I calculated well, at 45 deg, and 3 deg. 46 min. gives the mean anomaly.[59]

Kepler can only work "by conjecture." He selects some arc DF (in Figure 14), which he "preconceives" is the eccentric anomaly. The area of the maximum optical equation, triangle HBA, expressed as a proportion of the whole circle of 360°, is 5° 18' 30". Kepler "resolves" this into seconds (5° 18' 30" = 19,110"), then multiplies by the sine of the selected arc, FC, yielding the area FBA in seconds. He "puts" this area to the area of the sector DF, adding it before semicircularity (from 0° to 180° eccentric anomaly), subtracting it afterwards (from 180° to 360° eccentric anomaly). This yields area DFA, expressed as seconds of an angle. If the angle equals the given mean anomaly, then arc DF is indeed the correct eccentric anomaly. If not, a different arc must be selected and the calculation rechecked.[60] Working this way, however, Kepler can construct a table of mean anomalies calculated from given eccentric arcs. The table can then be used to work from mean to eccentric.

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[58] Voelkel, *The Composition of Kepler's Astronomia Nova*, 196-7.
[59] Kepler, *Werke XV*, 250.
[60] See Kepler, *Astronomia Nova*, 592-3, 600n8. For the given example, Kepler selects an arc of 45° as the eccentric anomaly. The sine of 45° times 19,110" gives 13,513". This is equivalent to 3° 45' 13" (about 3° 46'), which is added to the selected arc to give the mean anomaly (i.e., about 48° 46').

In the end, this indirect method is the best Kepler can do, and he leaves the direct solution as a challenge to posterity:

> But given the mean anomaly, there is no geometrical method of proceeding to the equated, that is, to the eccentric anomaly… that is, unless you were to have constructed tables and to have worked from them subsequently.
>
> This is my opinion. And insofar as it is seen to lack geometrical beauty, I exhort the geometers to solve me this problem:
> Given the area of a part of a semicircle and a point on the diameter, to find the arc and the angle at that point, the sides of which angle, and which arc, encloses the given area. Or, to cut the area of a semicircle in a given ration from any given point on the diameter.
>
> It is enough for me to believe that I could not solve this *a priori*, owing to the heterogeneity of the arc and the sine. Anyone who shows me my error and points the way will be for me the great Apollonius.[61]

As it turns out, this "Keplerian Problem" is demonstrably insoluble. Tables remain the only method available. Still, they do provide a way to find the eccentric anomaly from the mean anomaly, and once the eccentric anomaly is found, the longitude and distance follow.[62]

At this point in his letter, then, Kepler has finally constructed an elliptical orbit by which he can calculate planetary distances and longitudes from a given mean anomaly. Moreover, these calculations agree reasonably well with observations:

> I have computed the eccentric equations in acronychal positions, they take the task to the nail; of the distances I would say almost the same, but the method of examining them is somewhat more lax, which always leaves me about 100 parts in doubt, while the observations are optimal. Indeed, you know the best observations can err by one minute. But one minute vitiates the distance immensely, if the planet is near [the sun] or [opposition with the sun]. This, though, you will have as certain, that we come near the truth.[63]

Kepler is satisfied that his orbit is accurate within observational error. Subsequent observations may adjust the orbital parameters, but Kepler is confident he has "come near the truth." In other words, he has accomplished his descriptive project. The elliptical orbit is an accurate description

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[61] Ibid., 600-1.

[62] In the letter, Kepler writes that this solution "is not geometrical, and how can the concluder be so happy? A true objection, but it suffices from me that a geometric table can be constructed from given eccentric anomalies, which I have had for some time, and from where I have brought this so happy conclusion." Kepler, *Werke XV*, 259.

[63] Ibid., 250.

of the true planetary orbit. It provides predictions and retrodictions of longitudes and distances. Kepler has *described* the true orbit of Mars.

4.8 THE PROBLEM OF EXPLANATION: CONSIDERATION OF CAUSES

Despite the success of this orbit, Kepler remains unsatisfied. Recall that Kepler also seeks a plausible causal explanation of the Martian orbit. Yet to reach this ellipse, Kepler has had to abandon physical causes altogether and work directly from geometry. Indeed, Kepler does not see how this orbit *could* have a physical explanation.

> But there is also that which I desire in this hypothesis: namely that, though [I am] stretching [my mind] all the way to insanity, I cannot fashion the natural cause why mars, which with such great probability should librate in the diameter (indeed, the thing was reducing so beautifully to magnetic virtues for us), should rather want to go in an ellipse or some path close to it. Nevertheless, I think magnetic virtues may not always respect the sine, but something somewhat different.[64]

Libration along a diameter of the epicycle seemed physically plausible, even probable, yet that model failed. Instead, the planet moves in an ellipse, which seemingly cannot be constructed on the basis of libration. Kepler is at a loss, "stretching to insanity," trying to explain his elliptical hypothesis.

As Kepler indicates, libration along a diameter of an epicycle directed toward the sun has a plausible physical explanation. Kepler, as he is wont to do, likens it to the effect of magnetism. He assumes that the planet is magnetically attracted to the sun from aphelion to perihelion and repelled from perihelion to aphelion. This explanation conforms to Kepler's explanation of the action of the sun that causes the planets to revolve in their orbits, which he also attributes to a magnetical force. It seems eminently reasonable that the planets themselves should also act according to some "magnetic virtue." What is more, assuming a libration along the diameter, however it might come about, gives rise to the secant/versed sine models of planetary motion, which *do* generate accurate distances for all points in the orbit. Every indication, then, is that the

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[64] Ibid., 251.

force moving the planet is magnetic. As Kepler writes, the motion of Mars "smacks entirely of magnetic force."[65]

Yet, it appears to Kepler, magnetism cannot account for an elliptical orbit. It is important to understand why. We have seen above how, according to Kepler, the planet's motion is brought about by the composition of the "extrinsic force" emanating from the sun and the planet's own "intrinsic force" [*vis insita*]. At issue here is the speed of the planet's approach to the sun, which is to say the strength of the intrinsic force. At first, Kepler assumed that this force would follow a simple magnetic libration, in keeping with the action of the extrinsic force. Thus, the epicycle "measuring" the libration would be rotated at the same rate as the planet revolves around the sun. This is to say, both the position of the planet and the libration would be given by the eccentric anomaly – as the planet's forward motion increased, so would its approach, in the same proportion. Physically speaking, then, the intrinsic force is simply proportional to the extrinsic force. Geometrically, this can be represented as in Figure 13, where DI is to DB as LE is to LA. This also entails that the planet is always to be found on the radius to the eccentric center (e.g., along DB or PB). Of course, this assumption produces the *via buccosa*, which fails to square with observations. The approach is too slow near apsides and too fast at quadrature.

The ellipse, which observations show to be the proper orbit, dictates that the "measure" of the speed of the approach is not the forward motion of the planet, but its sine. As Kepler will later explain,[66] the small "incursions" from the circumscribing eccentric orbit to the ellipse increase proportionally to the sine of the eccentric anomaly. Moreover, these approaches are directed along the perpendicular, not along any radius. Thus, in Figure 14, when the eccentric anomaly is angle DBF, the perpendicular approach FE is to the greatest approach HI as the perpendicular FC is to the radius BD. That is, the perpendicular FC "measures" the approach FE. This also entails that the planet is always along the perpendicular dropped from the point on the eccentric circle corresponding to the eccentric anomaly (i.e., F). As before, the extrinsic force accounts for the increase of the eccentric anomaly, but Kepler does not understand how the planet's intrinsic force can be related to the perpendicular and direct its approach along it. That is, he cannot fathom how the planet's intrinsic force can be made to "respect the sine."

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[65] Ibid.
[66] See Kepler, *Astronomia Nova*, ch. 60.

To foreshadow our conclusion, we can point out that the problem here has to do with *direction*. Working under the assumption of planetary minds (the limiting case of physical plausibility), Kepler believes that the planet must somehow measure its position in orbit, and then set its distance to the sun, which it can measure using the apparent solar diameter.[67] In the *via buccosa*, the planet can simply measure the extrinsic force and set distance accordingly. Thus, the orbit has a plausible, even "probable," "natural cause." Indeed, both the extrinsic and intrinsic forces can be "reduced to magnetic virtues," and the planetary minds can be eschewed in favor of magnets.

In order to move in an ellipse, however, the planet has to somehow recognize the perpendicular to the apsidal line. This perpendicular, though, is not directed toward any physical body or even a geometric center. In a spherical representation of space, from which lines have been banished, this direction is literally inconceivable. Neither Kepler, nor his supposed planetary minds can recognize the direction as a direction. This means, for Kepler, that the ellipse is irreconcilable with physical causes. If the planet, even conceived as a mind, cannot "respect the sine," no physical cause could, either. Thus, Kepler does not see how his description of the elliptical orbit can be causally explicable. Facing this obstacle, he begins to consider how magnetic forces acting on the planet could be made to "respect the sine."

4.9 THE PROBLEM OF DIRECTION: GILBERT'S LAW OF THE WHOLE AND THE MAGNETIC BALANCE

Kepler's first attempt to devise a magnetic libration that "respects the sine" ends, as usual, in an instructive failure. He imagines that the planet is endowed with some magnetic axis that causes the planet to approach and recede from the sun:

> The eccentricity smacks entirely of magnetic force, as it is in my commentaries: so that if the globe of mars has a magnetic axis, one pole seeking the sun, the other fleeing, and this axis were pointed in the middle longitudes, then as long it is turned [*versatur*] in the descending semicircle, maximally in middle longitudes, it would point the seeking pole toward the sun, and thus always approach the sun,

[67] See Ibid., 561ff.

> but maximally in middle longitudes, not at all at apsides. And then in the ascending semicircle, it flees the sun equally.[68]

Kepler pictures a magnetic planet, with its axis parallel (or at least inclined to) the plane of its eccentric. At apsides, the poles of the planet are "pointed in the middle longitudes," that is, perpendicular to the line of apsides and, thus, the radius to the sun. At apsides, then, the planet is neither attracted nor repelled by the sun, since neither pole is inclined toward the sun. As the planet moves through its "descending orbit" from aphelion to perihelion, however, the axis is "turned" so that the seeking pole is inclined toward the sun, causing an attraction. This attraction is greatest where the axis is most inclined to the sun – in "middle longitudes." In the other, "ascending" half of the orbit, the reverse occurs, and the planet flees the sun, just as it had approached it.

There are two interesting things to note about this description. First is the notion that the axis of the planet *at apsides* "points in middle longitudes." This is a jarring locution. Until this point, Kepler has consistently used "middle longitudes" to refer to a *place* on the planet's orbit. Though somewhat vague, it has always signified the location of the planet when it has completed about a quarter or three quarters of its orbit. In other words, Kepler means the point on the orbit where a radius from the planet to some center (sun, eccentric center, or equant point) is perpendicular to the apsidal line. In this context, though, the planet is not at "middle longitude." It is at *apsides*, along the apsidal line, at null longitude. That is, the planet is not *located* perpendicular to the apsidal line. Instead, the planet's axis is *directed* perpendicular to the apsidal line.

This "pointing in middle longitude" does not depend on any presumed center. The direction of the axis is not toward or away from any point. It is simply perpendicular to a presupposed line. Moreover, the direction is self-parallel – it is perpendicular to the apsidal line at all points in space. Very subtly, Kepler has introduced a self-parallel orientation of space. "Pointing in middle longitude" has become a way to describe a direction rectilinearly, without reference to a center.

The second thing to notice in this passage is that, besides at apsides, Kepler continues to describe direction spherically, referring to radii to a center. He assumes that as the planet moves through its orbit, the axis must "turn" in order to point toward the sun. At "middle longitude" –

[68] Kepler, *Werke XV*, 251.

now understood, once more, as a location – the axis's inclination toward the sun is maximal. This description implies that if the planet did not "turn," i.e. if its axis maintained the same direction, its axis would stay perpendicular to the planet-sun radius, as it is directed at apsides. Thus, the axis would not incline toward the sun. By way of contrast, if the direction of the axis of the planet were described rectilinearly, and was thought to maintain the same orientation perpendicular to the line of apsides, it would not have to "turn" in order to point toward the sun at "middle longitude." Kepler has introduced a rectilinear orientation of space. Yet he has not fully switched to a rectilinear representation of space. His descriptions still depend on geometrical centers.

Kepler introduces the following section of his letter as a stream-of-consciousness brainstorm. "Indeed, allow me, very happy Fabricius," he writes, "while I work to speak with you, to profit from my exercise."[69] What follows is, in fact, a quick succession of slightly different hypotheses, all based on the assumptions described above. Here, Kepler tries to derive the "strength" of the magnetic *vis insita*, which he believes is proportional to the speed of the planet's approach or repulsion from the sun. He is not primarily concerned with the direction of the force, and he assumes that the planet is attracted or repelled along the radius to the sun.

Kepler realizes, from the ellipse model, that the speed of the planet's approach (and receding) is slow near apsides and maximal around middle longitude. This entails, in effect, that the strength of the attraction goes as the sine of some anomaly, since sine is null at 0° and 180° and maximal at 90°. Hence, Kepler seeks some way to make the strength, and thus the speed, go as the sine. However, he also knows that the distance of the libration is somehow related to the versed sine or secant, both functions of the "sine of the complement" or cosine. At this point, though, Kepler cannot reconcile the fact that physical considerations seem to call upon the sine while the observationally correct path calls upon the cosine. Nor is he able to reconcile the strength of the attraction with the actual distance of approach, which he assumes should be proportional, since the sine changes quickly at null longitude, where the cosine changes slowly, and *visa versa* at middle longitudes. After struggling with the problem without reaching a resolution, Kepler moves on to other topics, then sets the letter aside.

[69] Ibid.

When Kepler returns to the letter "after some weeks' interposition," he brings a new conception of the magnet and a new way to model its action. Remarkably, this new model is explicitly linked to a fresh appreciation of William Gilbert's magnetic philosophy:[70]

> Let the same figure of the body of the planet as above be proposed. I have said similarly above, the planet is considered as a globe or as a plane circle; now also I say this, it is considered as a plane circle or as a line. For, from Gilbert the Englishman, it is certain, and also [it is certain] in itself without his authority, magnetic virtue extends in a right [line]. [*Virtutem magneticam porrigi in rectum*.] Whereby the globe is feigned to consist of infinite plane circles, parallel to the eccentric, of which each is the same reason, thus because of this rectilinear virtue, the plane circle consists of infinite right [lines], of which likewise each is the same reason. Therefore the body of the planet can be thus considered, as any right [line], since none of the others impedes, as above I constructed falsely.[71]

From Gilbert, Kepler has learned two things. First, magnetic virtue is fundamentally linear – it "extends in a right [line]." Thus, though magnetic action propagates spherically through space, the action itself always respects the magnetic axis, the line extending through the magnet's poles. In his previous attempt at describing the intrinsic force, Kepler had reduced the volume of the planet to the area of a cross-section. Now, he further reduces that area to a line representing its magnetic axis.[72]

Second, Kepler has learned that magnets retain their orientation in space, even as they move about. Thus, Mars need not "turn" in order to keep its magnetic axis towards the sun, as he had "constructed falsely." Instead, the axis always points in the *same direction*, and can be considered as a single line, so long as "nothing else impedes." Of course, Kepler is now conceiving direction according to a rectilinear orientation of space, where the axis stays "pointing in middle longitude" rather than changes its deflection from a radius to the center.

Here, Kepler is capitalizing on Gilbert's notion of verticity and the "law of the whole." In *De Magnete*, Gilbert showed that the (spherical) magnet possessed an inherent linear axis

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[70] Kepler's first mentions Gilbert in his *Apologia pro Tychone contra Ursum*, which was composed shortly after the appearance of *De Magnete* in 1600. Jardine, *The Birth of History and Philosophy of Science: Kepler's A Defence of Tycho Against Ursus*, 146.
[71] Kepler, *Werke XV*, 253.
[72] This passage is particularly interesting because it includes Kepler's first mention of right lines with regard to planetary motion. Recall that Kepler chides his younger self, in the second edition of the *Mysterium*, for failing to recognize the necessity of lines in the construction of the motion of planets. This comment implies that lines must somehow be necessary to account for the elliptical path of planets. Here it becomes clear that, according to Kepler, lines are necessary to account for the magnetic (or magnetic-like) forces that cause the planets to move in an elliptical orbit.

between its two poles. The magnet could be said, therefore, to "point" or "be directed" along a line. Moreover, the magnetic virtue acted according to this line. "Direction," for example, was the power of a magnet to align needles with this axis. For Gilbert, then, the essential feature of a spherical magnet was its linear axis and poles, instead of its center or even its spherical shape. Kepler takes this to mean that he can represent the body of the planet as a line instead of sphere or circle.

Gilbert also wrote of a magnet's ability to conform its native verticity to an external magnetic influence. In the case of the Earth, the planet conforms its axis to a rectilinear "law of the whole" that pervades the universe. Thus, the direction of the earth's axis, conceived rectilinearly, does not change, even if the planet were to move through space around some center. Gilbert used this conformity to explain Copernicus's third motion – the direction of the earth's axis toward a single part of the sky.[73] Kepler uses Mars's linear verticity to explain why its axis stays along the perpendicular to the apsidal line. In doing so, however, Kepler has adopted a variation of Gilbert's rectilinear representation of space. Just as Gilbert dismisses Copernicus's third motion as a non-motion, Kepler now describes the direction of Mars's magnetic axis as a line which "nothing else impedes," not as the result of a "turning."

Considering the planetary body as a line maintaining a self-parallel orientation allows Kepler to employ a new kind of model for the magnetic action of the sun on Mars.

> Therefore, let AD be the magnetic axis, fleeing in A, seeking in D, representing one of the infinite right [lines] of virtue in the body of Mars. But let B be the middle point of AD, sun in BI, the said approach is the cause such that the flight does nothing, because A and D are in equal operation. Therefore, this is like equilibrium. See my *Optics*, chapter I. Now let the sun be in BGK. And by the center B and the distance BD, circle DG is delineated, and from G, let the section of the circle with the perpendicular line from the sun to DA be led. If therefore GB is the support and AB, BD the arms of a balance, as DC to CA will be the strength of angle DBG to the strength of ABG. And so this flight is as much as DC, and the seeking as much as AC. Take from AC the equal of DC, which is

[73] A phenomenon Kepler considers shortly hereafter. See below. At the juncture in the *Astronomia Nova* corresponding to this part of the letter, Kepler digresses on the subject of Copernicus's "third motion." He notes, in accord with Gilbert, that "Copernicus was deceived here when he thought he needed a special principle to cause the earth to reciprocate annually from north to south and back so as to produce summer and winter, and to bring about the equality of the tropical and sidereal years (to the extent that they are equal) by its efforts at producing equal periods. For all those effects are obtained by having the earth's axis, about which the diurnal motion is made, retain a single, constant direction…" Kepler, *Astronomia Nova*, 551. Once again, we see the crucial importance of Copernicus's "third motion" in the switch from spherical to rectilinear representations of space.

> AS. Therefore SC is the measure of the seeking, and AD the measure of the seeking at no angle. And as AD to SC, thus BD to BC or GH. Therefore the sine of the digression of the planet from apogee or perigee measures the speed of the approach.[74]

Earlier, Kepler had related the "strength" of magnetic action to volumes, areas, and circular angles. Representing the planet as a line, however, suggests the action of a magnetic balance. The magnetic axis, AD in Figure 15, is suspended from the line to the sun GB. The forces on this "balance" will now be measured by parts of a line, rather than the areas and angles employed above, according to the "law of the balance."

Unpacking the dense logic of the above passages is worthwhile, especially Kepler's mention of his *Optics*. The passage Kepler has in mind is found in chapter 1, proposition 20, where Kepler seeks to prove that "Light that has approached the surface of a denser medium obliquely, is refracted towards the perpendicular to the surface."[75] The first part of the proof is a consideration of a balance loaded unequally so that it comes to equilibrium inclined to the horizontal. Here, Kepler relates his "law of the balance," which correlates the inclination of the balance to the difference in the loads on the balance. According to Kepler, each arm of the balance can be found, depending on the loads, anywhere between (in Figure 16) B and F:

> About center A, with radius AC, let the circle AD be described, and in it the perpendicular BAF. It is evident that neither of the weights at C and D can either descend lower than F or be raised higher than B.[76]

The total possible descent of either arm of the balance, then, is from B to F. Accordingly, claims Kepler, the arms will divide this total descent between themselves in a ratio equal to that between their respective loads:

> And since both are of this nature, that they tend to the bottom, and they mutually compete with each other, they divide up the descent BF between themselves in that ratio in which they themselves are. From D and C let the perpendiculars DG, CH be drawn. Now from what has been said, BH, the descent of the weight C, will be to BG, the descent of the weight D, as the weight C is to D.[77]

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[74] Kepler, *Werke XV*, 253-4.
[75] Johannes Kepler, *Optics: Paralipomena to Witelo & Optical Part of Astronomy*, trans. William H. Donahue (Santa Fe: Green Lion Press, 2000), 27.
[76] Ibid., 32.
[77] Ibid.

In other words, the vertical descents of the arms will be in the same proportion as their weights. For example, if the weight on D is twice the weight on C, then the vertical position of D (i.e., G) will be twice as far along BF as the vertical position of C (i.e., H). This implies that BG will be two-thirds of BF, while BH will be one-third of the distance.

Kepler goes on to show that his "law of the balance," applicable to equal-armed balances, is equivalent to the mechanical law of the lever, which dictates that a unequal-armed balance will equilibrate horizontally if the length of its arms are proportional to its loads:

> I say that this is the proportion of the unequal armed balance. For also, because HAC, GAD are equal, and CA, AD are equal, and H, G are right, AH, AG will also be equal, and therefore also the remainders of the equals HB, GF. Therefore, as C is to D, so is FG to GB. From F let a perpendicular be drawn to CD, and let this be FK. Therefore, since CAH, FAK are equal, and CA, AF are equal, and H, K are right, CH, FK will also be equal. Likewise also AH, AK. Consequently, the remainders of the equals AB, AD, AF, that is, HB, GF, and KD, are also equal. Therefore, as C is to D, so is DK to KC. And if the beam CD, thus loaded, be suspended from the support at K, it will be the ratio of the unequal armed balance, and C, D will weigh equally, as is demonstrated in mechanics.[78]

By a series of constructions and comparisons, Kepler shows how a point, K, can be found on the equal-armed balance such that, if the balance were hung from that support instead of its center, the resulting unequal-armed balance would have arms CK and KD such that the ratio of their lengths would be equal to the ratio of the weights C and D. In this case, the resulting balance would "weigh equally" – horizontally – since it satisfies the classical law of the lever: weight C is to weight D as KD is to CK.

Kepler then goes on to apply this statical "protheorem"[79] to a mechanical problem of impact or pressure. He compares light encountering a refracting surface to the percussive action of a "missile" striking a "panel" or to the continuous pressure exerted by a stream encountering an oar.[80] He then tries to determine how the panel or missile will move as a result of the impact. First, he considers a body striking the panel or oar directly, along the perpendicular. In Figure 17:

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[78] Ibid.
[79] Ibid., 33.
[80] In the *Optics,* Kepler defines "violent motion" or "impulse" as an attribute of light. Therefore, he conceives the action of light as something similar to the action of hard bodies colliding. Reflection, for example, is not merely a turning back of a light ray, but a "repercussion [*repercussus*]." See Ibid., 26, 34.

> Let AB be a panel, C the center, ED perpendicular through it; as, if a globe or missile were carried from E into the panel AB, it would drive it forward towards D; or as if AB were oars, of equal length on both sides, and ED were a river. For since ECA, ECB are right, the arms AC, CB are placed in equal balance, and meet the impact of the mobile body with an equal power.[81]

The ends of the panel, or the oars, A and B, will be driven forward by the impact. The distance each end is moved, however, depends on the "power" with which it resists the action of the impact. To measure this power of resistance, Kepler compares the situation to the balance he has already described. He implies that ends of the panel or oars are akin to the arms of the balance, while the force of impact or pressure is similar to the force exerted by the balance's support. This case, therefore, is similar to a level balance, where each arm exerts equal resistance to the support. Carrying the analogy back to the case of impact, Kepler concludes that A and B have equal power of resistance, and thus are moved equally forward.

Next, Kepler considers the case of an oblique impact or pressure:

> Now let the oblique FC strike at C and let it be extended to K. And let the missile or the stream rush in from F to AB. Since the angle ACF is less than the angle FCB, the parts AC, CB will not be impelled with equal force, but the one that resists more will feel the blow more. And the one that faces at an obtuse angle resists more than that facing at an acute angle. Therefore, the exterior part CB will resist more... Therefore, there is a greater impression of violent motion on CB. So when AB is moved in position, B advances more than A.[82]

This situation is akin to an equal-armed balance loaded with unequal weights. In this case, Kepler argues, the ends of the balance resist the force of the support unequally. As a result, the balance will come to equilibrium tilted downward toward the heavier, more resistant, weight. In this position, the support makes an obtuse angle with the heavier weight. (The balance is imagined to hang from a support rather than rest on a fulcrum.) Again arguing by analogy, Kepler concludes that the end of the panel or oar making an obtuse angle with the path of the incident percussant will resist its action more, and therefore be moved farther, than the end or oar making an acute angle. Hence, the panel or oars will be moved from AB to HI by the force of the impact or pressure.

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[81] Ibid., 33.
[82] Ibid., 33-4.

In the *Optics*, Kepler does not attempt to quantify the difference between the motion of A and the motion of B (as he does in the letter to Fabricius). It suffices to show that the motion of A, AH, is less than that of B, BI. From this, Kepler can conclude that the surface of the panel, now at HI, is turned toward the original perpendicular, DE. This, he claims, shows that an incident ray of light will be refracted toward the perpendicular when it encounters the refractive surface, since "this behavior of violent physical motion also flows back to what is analogous to light."[83]

This part of Kepler's proof is not relevant to our discussion except for a tangential comment Kepler makes in passing. Considering specifically the action of a stream on an obliquely positioned oar, Kepler says that, since the end of the oar B resists the impact more than A, it will be moved further. However, he continues, if the oars are "artificially held back in this position AB," the "oar AB will at length be pushed forth to the shore," toward B.[84] In other words, if the oar is kept parallel to itself, "in this position AB," by some "artificial" faculty resisting rotation, then the effect of the stream's pressure will be to move the oar in the direction of B. As noted, this comment is tangential to Kepler's argument, since he is concerned with how the refracting surface is "turned aside" by light, and he makes no attempt to elaborate or quantify this lateral motion. It will become important, however, in the context of the Martian orbit.[85]

We can now turn to the argument Kepler presents in the letter to Fabricius. Though he does not say it explicitly, it is clear from his reference to the *Optics* that Kepler considers the magnetic force of the sun in a way similar to the action of light on a refracting surface. Thus, the impinging solar force can be likened to the force exerted by the support of a balance, while its effect is determined by the "strengths" of resistance in the planet. The more "resisting" end of the planet's magnetic axis will have a greater effect. If the planet is "artificially" kept parallel to itself, "in position," the magnetic axis will not rotate about its center, but *move* closer or further from the sun. The motion, moreover, will be along the direction of the axis, just as the oar is moved along its own length.

As we shall discuss in further detail below, though, Kepler has learned from Gilbert that the earth's magnetic axis *is* kept parallel to itself by its own "native verticity" and a universal

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[83] Ibid., 34.
[84] Ibid.
[85] This is apparently the source of the stream analogy that appears in chapter 57 of the *Astronomia Nova*, though in that case, the oar is assumed to turn, rather than remain parallel.

"law of the whole." Thus, it is plausible to believe that a similar faculty keeps Mars's magnetic axis pointed "in middle longitude." If this is the case, then the planet's intrinsic, magnetic force will cause a motion toward and away from the sun, not along the radius to the sun, but along the direction of its magnetic axis – along the perpendicular to the apsidal line, as required by the geometric construction of the ellipse. As long as some faculty "knows" how to keep the planet's magnetic axis "in position," always pointed in the *same direction* perpendicular to the apsidal line, the *vis insita* will move the planet in the proper direction.

Using the analogy of a magnetic balance also allows a quantification of the planet's motion toward and away from the sun, which Kepler presents in the letter to Fabricius in the passage quoted above. The support of the planetary "balance" is assumed to be BG (in Figure 15), the line to the sun. The strength of the magnetic attraction is assumed to act at D, while the repulsion acts at A. Assuming that the position of the balance is "like equilibrium," Kepler has shown (in the *Optics*) that there is a point C, found by constructing a perpendicular to AD from G, such that DC is to CA as the repulsive action is to the attractive action. That is, the "flight is as much as DC, and the seeking as much as CA." The difference between the seeking and flight, then, will be measured by the difference between these distances, which is SC. Thus, SC is to the net attractive force as AD is to the maximum attractive force possible ("the seeking at no angle"). Halving both these quantities (which preserves the proportions), we find that the net attraction is measured by BC or GH, the sine of angle IBG. Thus, the "sine of the digression of the planet from apogee or perigee"[86] measures the force of attraction or repulsion. Therefore, the same sine also measures "the speed of the approach." Not only is the planet attracted or repulsed in the *right direction*, it is attracted or repulsed by the *right amount*.

This result is interesting for Kepler because he finally has a plausible physical mechanism that measures the strength of approach that varies as the sine of the anomaly. This satisfies the requirements of his observational model, which entails that the rate of the planet's approach to the sun should be slow at apsides and maximal at middle longitudes. Equally important, however, is the issue of direction. The magnetic force acting on the planet is not directed toward a center, but in a constant direction – perpendicular to the apsidal line. Thus, this solves the question raised earlier: how does the planet recognize the perpendicular? The

[86] Earlier, Kepler uses "digression of the planet from apogee" to signify the mean anomaly. Here, he means the eccentric anomaly.

answer is given by the supposition of a rectilinear orientation. The planet's magnetic (or mental) power is then assumed to respect this direction, just as Gilbert's "verticity" respects the "law of the whole" – thereby keeping the planet's magnetic axis pointed "in middle longitudes." That is, the physical cause of the planet's proper distance (the intrinsic force) acts according to a direction conceived on the basis of a presupposed self-parallel orientation, not a center.

4.10 THE PROBLEM OF SINES AND COSINES: SUMS OF FORCE

At this point, however, Kepler stumbles over a block that will require another spark of genius to overcome. He tries to relate the strength of the intrinsic force and, thus, the speed of approach, to the actual lengths of descent at each point on the orbit. At first, he erroneously assumes that the libration itself will also vary as the sine, so that the length of libration is directly proportional to the speed of approach:

> This is geometrically demonstrated and most certain. And thus if our principles are correct, all the libration will follow the law of the sine of the digression from apogee. But because experience and the ellipse by experience very certainly made firm wants the libration to follow the versed sine of the digression from apogee, that is not by GH but by HI, therefore our principles are necessarily to be changed. We have substituted, in effect, for GH the perpendicular HI. And thus, therefore, in principles we ought to accept for AD the perpendicular FI. In other words, one must say that the planet is at apsides not when its magnetic axis is perpendicularly inclined to the line from the sun, but when it is united with it (if it can). While although I am unable to reconcile my first intuitions with the magnetic virtue with the appearances, nevertheless it strikes me in a wonderful way.[87]

The libration, Kepler knows, varies as the versed sine of the anomaly, that is, as HI in Figure 14, not the sine, GH, which is the supposed measure of force. The versed sine, meanwhile, is derived from the sine of the complement of the anomaly, CG. So, the elliptical path seems to require the entire planet to be rotated 90°, so that the magnetic axis would "measure" the complement of the anomaly. In other words, at aphelion, the planet will have its axis directed towards the sun, and it will retain this direction parallel to the line of apsides thereafter. This,

[87] Kepler, *Werke XV*, 254.

however, entails that the force of attraction is maximal at aphelion, contrary to the hypothesis. Kepler is back where he started, unable to reconcile sines and cosines.

Kepler finds the idea that the axis maintains its orientation striking "in a wonderful way." He further explores the notion by considering Gilbert's special case – the earth's magnetic axis. He wonders if the earth behaves as his "first intuitions" suggest, keeping the plane of its axis perpendicular to the line of apsides. To find out, Kepler turns to observations:

> For in my Commentaries, this objection has been left [unanswered]: If the planets produce eccentricities by a magnetic virtue with their axis directed towards the same parts of the universe, the Earth will do the same. But the axis of the Earth is the only one, which is pointed from [Cancer] to [Capricorn]: it falls in this direction in the summer, and winter. Around this the whole remaining body is turned daily. Therefore, the apogee of the earth is fixed in 0 deg. [Aries], 0 deg. [Libra]. But it is seen to be otherwise, and indeed not in [Aries], [Libra] but in [Capricorn] (because the Sun is in [Cancer])… Now, this is testified by experience, the line of apsides meets the line of the axis directly, therefore the apogee of the earth is stable in 0 [Capricorn].[88]

The earth's magnetic axis remains parallel, "directed towards the same parts of the universe," throughout the year. Since the magnetic axis, according to Gilbert and Kepler, is identical to the rotational axis, it must be pointed directly toward or away from the sun at the solstices. Hence, the direction of the earth's axis lies along the line between Cancer and Capricorn. If this direction perpendicular to the line of the earth's apsides, the apsides will be found in Aries and Libra. Observations show, however, that the earth's perihelion is in Capricorn (when the sun is in the direction of Cancer), and the aphelion is in Cancer. Thus, observations indicate it is more likely that the earth's magnetic axis is "united" with the line to the sun at aphelion than perpendicular to it.[89]

These observations lead Kepler to consider physical causes once more.

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[88] Ibid.

[89] In fact, the line of the earth's axis, magnetic or otherwise, has nothing to do with its line of apsides. The two can differ by any angle, and will, in the fullness of time. At this point, Kepler also discusses observational uncertainty about the position of the earth's apsides, noting that different astronomers place them at different points in the ecliptic. Ptolemy had them in Gemini and Sagittarius, while contemporaries placed them at differing points in Cancer and Capricorn. This leads Kepler to suggest that the plane of the earth's orbit "does not remain very perfectly in the same plane." That is, the plane of the earth's orbit might itself rotate around the sun, moving the apsidal line. This is, in fact, the case, but the proposition was very much debatable in the seventeenth century. Kepler clearly worked on this issue between 1605 and 1609. Much of the material published in the *Astronomia Nova* developed subsequent to the letter (excluding the work on latitude) concerns the rotation of the apsidal line. See, especially, Kepler, *Astronomia Nova*, 553-4.

> But before I sing the triumph, I must think about the physical cause, if it is possible that, as the magnetic axis is constructed at apogee, it remains in the direct line from the sun? For what is it, which does similarly, that we may ascribe the cause? The earth, in Aries, is turned about its axis pointing north toward the region of the sun in the center, away in Libra. What therefore is the cause of this recession, the cause of this accession to the sun? Also in [Aries] and in [Libra] days are equaled by nights in the whole world. In [Cancer], [Capricorn] parts of the globe lack light. What, then, this cause of approach? But set aside this present question [and] bring us back to the scheme of the body of Mars.[90]

Suppose a planet keeps its magnetic axis parallel to the line of apsides, i.e., as it is "constructed at apogee… in the direct line from the sun." This, from observations, seems to be how the earth behaves. Thus, the north end of the earth's axis is pointed toward the sun near aphelion, in Cancer, and away near perihelion in Capricorn. Thus, as the earth moves through its orbit from Capricorn to Cancer, through the equinox in Aries, the north end of the earth's axis is "turning" toward the sun. The opposite is true at the other equinox in Libra. What is the cause of this "turning"? That is, what is the physical cause that keeps the planet's axis parallel to itself?

(Notice that Kepler is mixing his representations of space at this point. In the above passage, he describes the fact that the earth's axis remains parallel as a "turning." This description is to be understood in a spherical representation of space, where direction is referenced to the center. On the other hand, he has just written that the axis remains "directed toward the same parts of the universe," implying that it does not turn. Regardless of description, what Kepler seeks is a physical reason the earth's axis stays parallel to itself. His answer will depend on a rectilinear representation of space.)

Some force keeps the earth's magnetic axis parallel to itself. Therefore, a similar force might keep Mars's axis parallel to itself. This returns Kepler to his balance model in his "scheme of the body of Mars." There, he assumed some force acted to keep the arms of the balance in the same direction, perpendicular to the line of apsides, counteracting the tendency of the axis to point toward the sun. The action of this force on the attractive and repulsive ends of the Martian axis obeyed the law of the balance. As a result, the net attraction or repulsion was proportional to the sine of the anomaly, as required by the ellipse.

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[90] Kepler, *Werke XV*, 255.

At this juncture, Kepler reveals his remarkable intuition and mathematical prowess in the crucial realization that the total libration was proportional, not to the strength of the attractive force itself, but to the *sum* of the momentary actions of the force:

> Two words: 1. That the versed sine IH measures the portion of the libration is testified by experience of observation. 2. The right sine GH with the vigorous demonstration given in the *Optics*, measures the force of approach, or of the libration. These two I have thought until now to be contrary, but it seems they are not. For one thing is the measure of the strength of the libration, another thing now performs the measure of the parts of libration. There, IF represents the total libration, IH the part comprising the eccentric anomaly signified by IG. Here, DB represents the maximum strength, GH the strength of the moment in angle GBI. But DB does not signify all the strengths combined, thus GH does not all the strengths for the whole arc of the anomaly GI. But if you collect the sum of the sine of 90 which is 578,943,140, this is the measure of the strength, which indeed is the common effect of half the libration or BI. Thus therefore also if you collect the sum of the sine from all the steps in GI, this will measure the portion of the performance of the libration, which if it produces a line so long, as long as HI, the versed sine, from which experience stands, then we have reconciled experience with the demonstration of the balance.[91]

Kepler had thought that the "portion of libration" measured by the versed sine in the observationally confirmed ellipse was irreconcilable with the "force of approach" measured by the sine in the balance model. Now, he understands that they can be reconciled if the libration is thought of as the sum of the "strengths combined." This is, of course, Kepler's proto-calculus that has been much celebrated by historians.[92] In the letter, Kepler proceeds to execute a calculation showing the proportionality of the sum of the sines (i.e., the area GHI in Figure 14) to the whole quadrant DBI is roughly equal to that of the versed sine HI to the libration at quadrature, BI. In other words, he shows that the libration varies as the sum of the sines. Thus, Kepler is satisfied that the planet's intrinsic force indeed varies according to the sine of the anomaly.[93]

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[91] Ibid., 255-6.

[92] Aiton, "Infinitesimals and the Area Law."; Koyré, *The Astronomical Revolution: Copernicus, Kepler, Borelli*, 272; Paolo Mancosu, *Philosophy of Mathematics & Mathematical Practice in the Seventeenth Century* (New York: Oxford University Press, 1996), 38-39.

[93] I am eliding the very important point that the anomaly in question here is the *eccentric* anomaly. However, Kepler has rejected physical references to empty points, such as the eccentric center. Thus, the anomaly should be the *equated* anomaly, which is measured to the body of the sun. In the *Astronomia Nova*, Kepler dismisses this difference as insignificant. Kepler, *Astronomia Nova*, 558; Stephenson, *Kepler's Physical Astronomy*, 115-16. Later, in the *Epitome of Copernican Astronomy*, Kepler introduces another libration or "wobble" of the magnetic

Finally, then, Kepler has reconciled the requirements of his elliptical orbit with a plausible physical cause:

> Therefore we have adduced the thing in the senses [i.e., observations] into closeness with the best reasons. We conclude, therefore, the body of the planet must be considered as if it were magnetic, which approaches or flees by the law of the balance, and the diameter of virtue points in the middle longitudes.[94]

Mars can be thought of as a magnetic body with an axis that remains "pointed in middle longitudes" – perpendicular to the apsidal line.[95] The two poles of the axis attracted and repelled by the sun. Thus, as Mars moves around its orbit, the planet is first attracted to the sun and descends from aphelion to perihelion. Then the planet is repelled, and it ascends back to aphelion. In both cases, the speed of the planet's approach or repulsion is maximal at middle longitude, when the axis is pointed directly toward the sun.

The strength of the attraction or repulsion at each point in the orbit, meanwhile, is given by the sine of the anomaly. At all points on the orbit, the axis will tend to point toward the sun. However, the axis is kept perpendicular to the apsidal line by some directive faculty, which also acts on the poles of the axis. If the forces counteracting the attraction to the sun keep the axis in equilibrium, they will vary according to the law of the balance. Thus, the net attraction or repulsion caused by the directive faculty is equal to the net attraction or repulsion to the sun that causes the planet to approach or flee the sun.[96] That is, the force of attraction and, thus, the speed of its approach, will vary according to the sine of the anomaly, and Kepler has mathematically shown how a force varying according to the sine can produce a libration varying according to the versed sine.

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axis precisely equal to the optical equation – the difference of the two anomalies. Thus, the axis "measures" the eccentric anomaly even though it is physically affected according to the equated anomaly. See Kepler, *Epitome of Copernican Astronomy & Harmonies of the World*, 99-106; Stephenson, *Kepler's Physical Astronomy*, 146-72.

[94] Kepler, *Werke XV*, 256.

[95] The "truly shattering objection" that the axis of the earth apparently remains parallel to the apsidal line Kepler decides to leave unanswered. Ibid.

[96] Kepler assumes that the more the axis is inclined toward the sun, the more it seeks to direct itself to the sun. Thus, the "retentive force" holding the axis parallel is maximal at middle longitude, even though the tendency to turn in the direction of the sun is, presumably, null (or "evanescent"), since the axis is already pointed toward the sun. See below. Kepler, *Astronomia Nova*, 553.

In the *Astronomia Nova*, Kepler explains this arrangement in greater detail. He envisions a magnetic axis in the body of Mars, which is kept parallel either by a "retentive" force or an "animate faculty":[97]

> As before, let there be certain regions of the planetary body in which there is a magnetic force of direction along a line tending towards the sun. However, contrary to the previous case, let it be an attribute, not of the nature of the body, but of an animate faculty of the sort that governs the body of the planet from within, that as it is swept along by the sun, it keeps that magnetic axis always directed at the same fixed stars… The result will be a battle between the animate faculty and the magnetic faculty, and the animate will win… On the basis of these presuppositions, the planet's mind will be able to intuit and perceive the strength of the angle from the wrestling match between the animate faculty, which is designed to keep the magnetic axis in line, and the magnetic power of directing it towards the sun.[98]

The magnetic axis tends to point toward the sun. It is held in place, however, by the animate faculty, which counteracts the magnetic power. Yet, the magnetic power of direction is increased as the axis is more inclined to the sun, so the animate faculty will have to "struggle" more vigorously to keep the axis in line. Thus, by sensing this "struggle," the planet can "intuit" the angle of anomaly. In other words, the magnetic power of the planet becomes a measuring device by which the planet knows its position in the orbit:

> There was consequently a need for us to equip the mind with an animate faculty, as well as a magnetic one, and to contrive a battle between the two which would remind the mind of its duties, of which it could not have been reminded by the equality of either the times or the spaces traversed. So again we have asked nature to assist the mind.[99]

The natural, magnetic faculty assists the planetary mind to determine its orbital position. Once the mind knows its position, it can (by measuring the apparent solar diameter) fulfilling its "duty" to maintain the proper distance to the sun.

Kepler admits that this magnetic/animate mechanism might seem bizarre to some readers:

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[97] Kepler gives both explanations. See Ibid.
[98] Ibid., 567.
[99] Ibid., 569.

> Moreover, I don not know whether I have given sufficient proof to the philosophical reader of this perceptive cognition of the sun and the fixed stars, which I myself so easily accept, and bestow upon the planet's mind.[100]

Thus, Kepler does not believe he has proven that Mars possesses a mind, or even a magnetic axis as he has described. This, though, is not really his concern. The aim all along has been merely to establish a plausible physical cause, however far-fetched:

> I will be satisfied if this magnetic example demonstrates the general possibility of the proposed mechanism. Concerning its details, however, I have doubts.[101]

If Kepler can establish the "general possibility" of the mechanism, even by appealing to minds, he can cache the mechanism, and thus the elliptical orbit it produces, in the realm of the possible. In this, at least, he has succeeded.

4.11 CONCLUSION: THE STATUS OF LINES IN GILBERT AND KEPLER

Kepler, finally, has satisfied all his desiderata. He has constructed the true, "physical" path of the Martian orbit. It is causally explicable and agrees with observations. As he writes to Fabricius:

> Furthermore, at the same time you see both that that most earnestly desired union is now finally complete… Everything I sought has been accomplished; the causes of each eccentricity are given. You have an astronomy without hypotheses. [*Astronomiam habes sine hypothesibus*.] Of course it seems that up to now it had been an hypothesis when I said that Mars's eccentric is a perfect ellipse. But this was previously concluded from physical causes; it is not therefore a hypothesis in my *Commentaries*. It is indeed in the calculation, but it is also a true supposition of the true path of the planets, giving the distances and the equations.[102]

When Kepler first proposed the ellipse, it was merely a hypothesis – a mathematical conceit. Still, it was a good description of the orbit, and accurate planetary positions and distances could be calculated. Now, however, the physical causes of the motion have been given, so the ellipse

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[100] Ibid., 570.
[101] Ibid., 559.
[102] Kepler, *Werke XV*, 261. Translation from Voelkel, *The Composition of Kepler's Astronomia Nova*, 200-1.

transcends description. It is the “true supposition of the true path,” both descriptive and explicable. By reconciling description and explanation, Kepler has produced “astronomy without hypotheses.”

Kepler could not have effected this “most earnestly desired union,” though, without adopting a rectilinear orientation of space. The accurate description of the Martian orbit required geometric constructions that could not be described in spherical representation of space. In particular, the perpendicular to the apsidal line is a direction that cannot be specified in relation to a presupposed center. As a result, Kepler could not conceive how the planet might “respect the sine.” The solution only came when Kepler realized that magnetic action could be described on the basis of “right lines.” Gilbert had used presupposed lines to describe magnetic forces. These lines, moreover, could pick out a privileged, self-parallel direction, or orientation, “pointing in middle longitude” perpendicular to the apsidal line. Thus, by adopting a rectilinear representation of space, Kepler can assume a direction in space. He can then describe a magnetic or animate faculty on the basis of that direction, and then use this faculty to explain the ellipse. The presupposition of a rectilinear orientation allows the reconciliation of description and explanation. It allows the true discovery of the ellipse.

Kepler’s essential realization that magnets respect lines is taken from Gilbert. Both Kepler and Gilbert rely on a presupposed linear orientation to describe and explain phenomena. There is an important difference, however, between Kepler’s “right lines” and Gilbert’s “law of the whole.” For Gilbert, the “law of the whole” was an objective feature of the universe. It was physically instantiated by what amounted to a magnetic field. Thus, Gilbert’s orientation had a real existence outside the supposition of any (created) mind. For Kepler, on the other hand, the ontic status of “right lines” is less clear. Sometimes, Kepler suggests that a “retentive force” keeps a magnet in the presupposed direction, in a manner quite similar to how Gilbert’s “law of the whole” keeps the earth’s axis in one direction. More generally, however, Kepler writes of an “animate faculty” which “respects” or “recognizes” the orientation. This suggests that the orientation is not physically instantiated, but is a *subjective* feature of the animate faculty’s perception. In this case, “right lines” are more clearly an assumption by which minds come to describe and explain phenomena. They are part of the subjective conceptual framework used to understand the world.

This makes a subtle but important difference in the way rectilinear representations of space are used. For Gilbert, there can be only one rectilinear orientation of space, which is determined by the objective field that instantiates it. For Kepler, the orientation is a mental construct, and minds are free to choose it as is convenient. In other words, the choice of orientation becomes, in some sense, conventional and situation-specific. The choice is dictated by the exigencies of the situation one seeks to describe or explain. Kepler, for example, finds the direction perpendicular to the apsidal line convenient. Thus, this is the direction he assumes when explaining the motion of the planet. However, this direction, in absolute terms, is different for each of the planets. Kepler also uses the direction parallel to the apsidal line at some points in his argument, as he sees fit. With Kepler, then, rectilinear representation of space, originally suggested to Gilbert by the phenomenal features of magnets, is ensconced in the realm of the conceptual and conventional. We shall see how this shift to the subjective is mirrored in the conceptual framework employed by Descartes.

Of course, Kepler's adoption of a rectilinear orientation represents only a small step towards a rectilinear representation of space. In general, Kepler's spatial framework remained spherical. He continued, for example, to privilege a geometric center, embodied by the sun. Indeed, the rectilinear orientation was only assumed in order to describe and explain the *vis insita*. Meanwhile, the *anima motrix* moving the planets around their orbits is always, for Kepler, described on the basis of a spherical framework. It emanates radially from the center and moves planets circularly about it. Thus, even in the late *Epitome*, Kepler composes the radial action of the *vis insita* with the fundamentally circular motion caused by the *anima motrix* in order to derive the motion of the planet.[103] In the end, the final move to a rectilinear representation of space came only when Newton replaced the circular action of the *anima motrix* with the linear action of Cartesian inertia in his descriptions and explanations of the planetary movements.

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[103] As Koyré notes, Caspar's reconstruction of Kepler's argument here is conceptually mistaken along precisely these lines: "Now, the concept of motion underlying Caspar's proof is quite different from that of Kepler; it implies the principle of inertia and the preponderance of the straight line over the circle. Consequently, the elements of motion – lateral and centripetal – which comprise the orbital motion are straight lines; and the elements of this latter motion are (infinitesimal) straight lines whose direction is that of the tangent to that point on the (curved) orbit occupied (for a moment) by the moving body. However, these elements are not at all straight lines in Kepler's view; those connected with libration undoubtedly are; but those connected with lateral motion are (infinitesimal) circular arcs. Even the infinitesimally short tangents play no part in his arguments." Max Caspar and Walther von Dyck, eds., *Johannes Kepler Gesammelte Werke* (München: C.H. Beck, 1937), VII 598; Kepler, *Werke XV*, VII 598; Koyré, *The Astronomical Revolution: Copernicus, Kepler, Borelli*, 321.

Kepler's long and trying struggle with the Martian orbit showed that the presupposition of a rectilinear orientation is necessary to establish and explain the true elliptical orbits of the planets. Thus, God himself could not have constructed this "complete, thoroughly ordered universe" without appeal to "right lines." Kepler was wrong to eschew lines in the *Mysterium*. Enlightened by his own "conquest of Mars," and his encounter with William Gilbert, Kepler candidly reported his error in the *Mysterium*'s second edition.

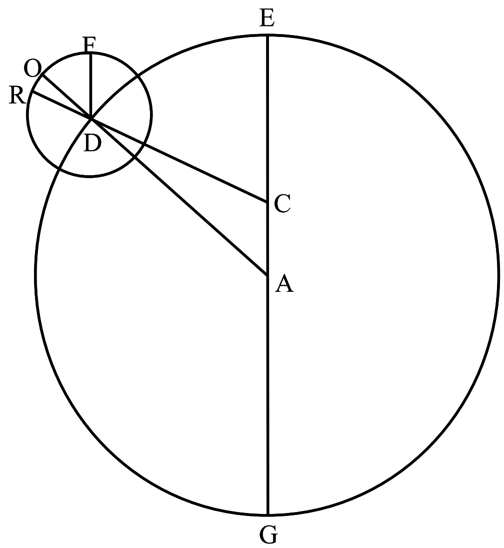

Figure 5. *Basic Epicycle/Deferent Model.*

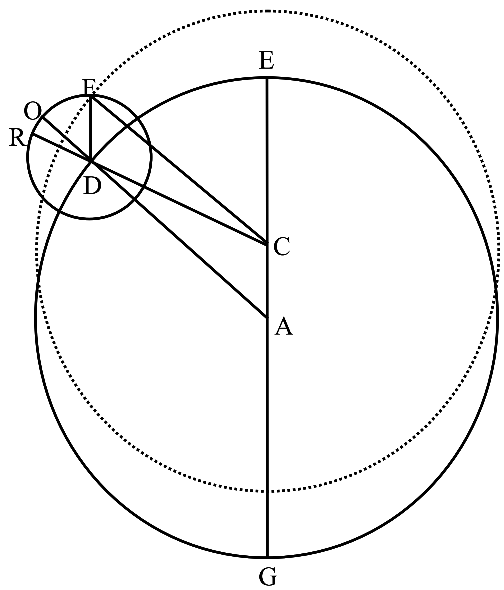

Figure 6. *Circular Epicyclic Orbit.*

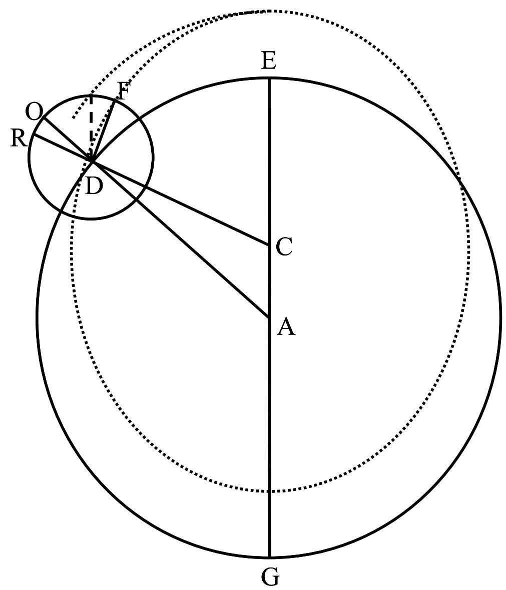

Figure 7. *Elliptical Epicyclic Orbit.*

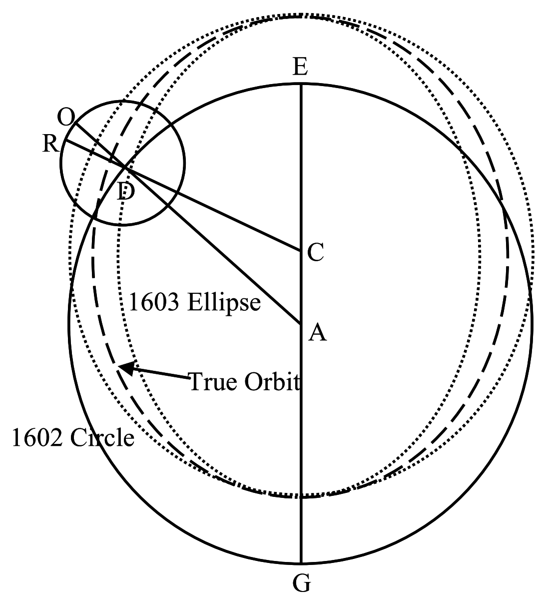

Figure 8. *Kepler's 1602 and 1603 Models.*

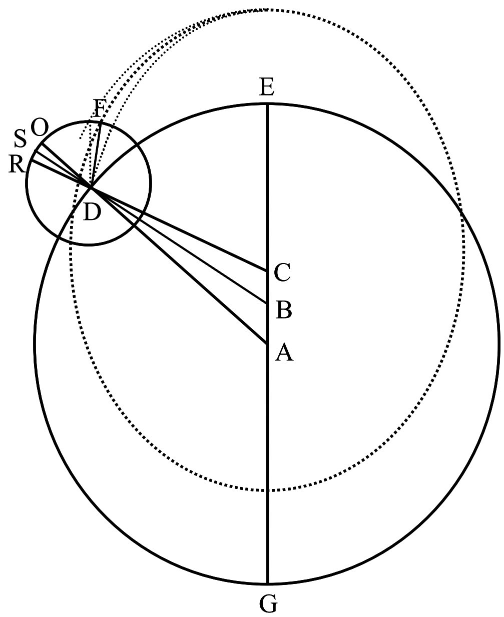

Figure 9. *Bisected Eccentricity*.

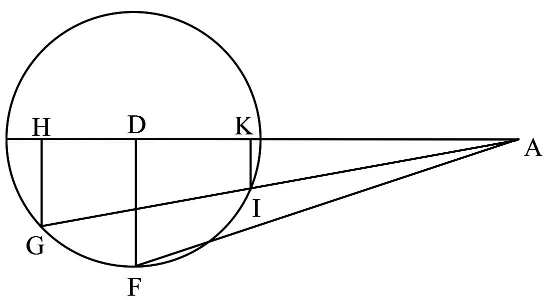

Figure 10. *Elongations on Epicycle*.

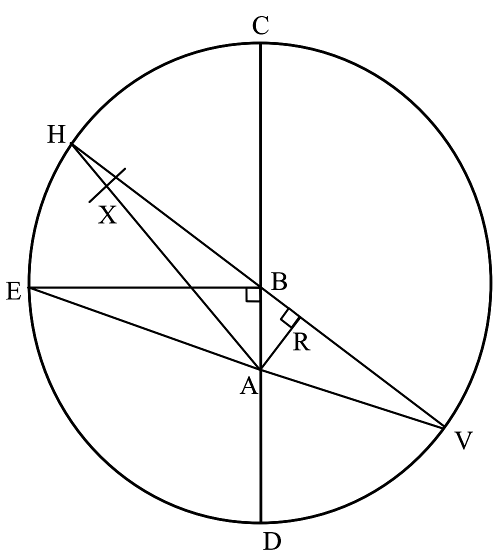

Figure 11. *Secant Model*.

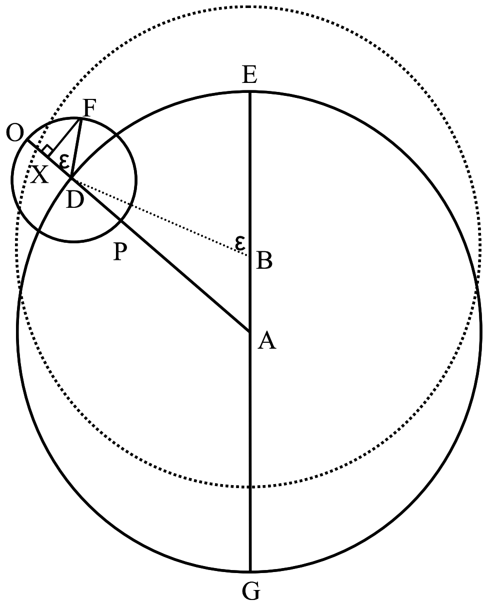

Figure 12. *Libration Model*.

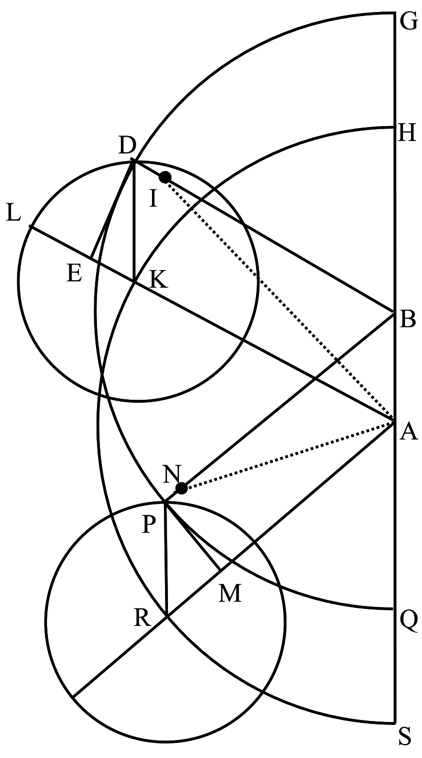

Figure 13. *Descents Along the Radii*.

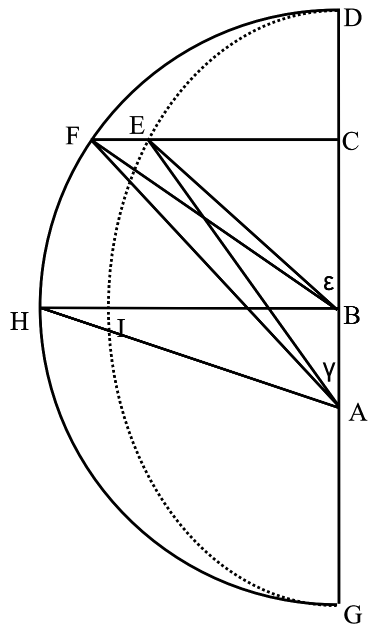

Figure 14. *Descents Along Perpendiculars*.

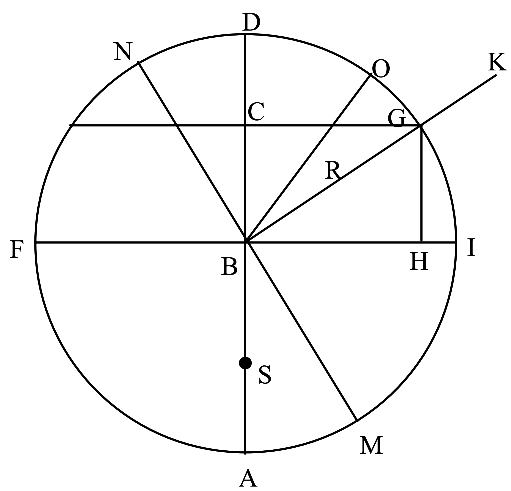

Figure 15. *Measure of Attractive Force in the Planetary Body*.

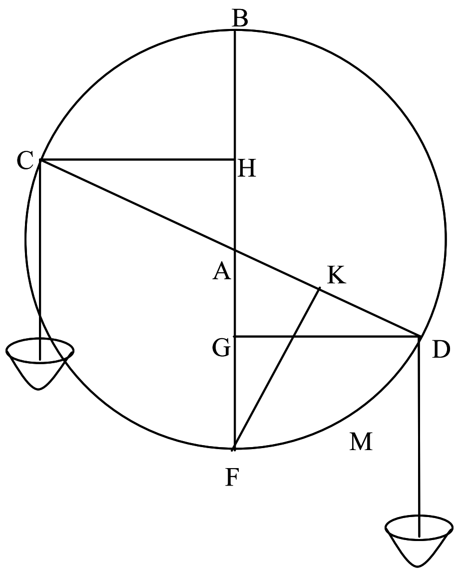

Figure 16. *Derivation of Lever Law*.

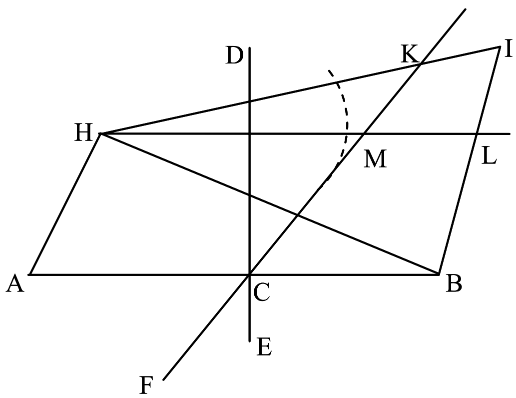

Figure 17. *Optical Percussion*.

5.0 INERTIAL DEFLECTIONS, REPRESENTATIONS OF SPACE, AND GALILEAN INERTIA

This chapter examines two passages in Galileo's *Dialogue Concerning the Two Chief World Systems*, first published in 1632. The passages are fascinating for two reasons. First, they show that Galileo predicted, at least in passing, the inertial deflection of projectiles, a phenomenon not experimentally observed until 1679[1] and not quantified until 1835.[2] This effect is significant, since it depends on the conservation of *linear*, not circular, inertia. Second, these passages expose a deep tension in the way Galileo dealt with spatial relations and properties. Recognizing this spatial ambivalence, in turn, helps clarify Galileo's notion of inertia. Altogether, then, a thorough investigation of these passages informs the vast historical literature surrounding Galilean inertia in particular and the development of early modern science in general.

As we shall argue, Galileo consistently employed an Aristotelian, spherical representation of space in order to describe and explain large-scale phenomena, but used a rectilinear representation of space to handle small-scale phenomena. The tension revealed in the passages arises when these different representations are used to describe the same phenomenon. Yet the intersection of the two representations of space also leads to the prediction of inertial deflections. Ultimately, Galileo himself reconciled these representations of space by appealing to the

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[1] In 1679, Isaac Newton, following the logic sketched below, was the first to straightforwardly predict the eastward drift of falling bodies. Moreover, he designed an experiment to demonstrate this effect that he thought would "argue ye diurnall motion of ye earth." Robert Hooke later carried out the experiment, the success of which he judged a conclusive "Demonstration of the Diurnall motion of the earth as you [Newton] have very happily intimated." See H. W. Turnbull, ed., *The Correspondence of Isaac Newton*, vol. II (1676-1687) (Cambridge: University Press for the Royal Society, 1960), Letter no. 236, p. 302 and Letter no. 240, p. 313.

[2] By Gaspard Gustave de Coriolis. The effects described by Galileo are today subsumed under a class of phenomena known as *Coriolis Effects*. These are "uncaused" deflections brought about by inertial, and therefore linear, motion in a rotating reference frame. The deflections result from the rotation of the frame itself, rather than any physical forces operating within the frame. To avoid the appearance of anachronism, however, we will use the term "inertial deflections." René Dugas, *A History of Mechanics*, trans. J. R. Maddox (New York: Dover Publications, 1988), 374-80; René Dugas, "Sur l'origine du théorème de Coriolis," *Revue Scientifique Revue Rose Illustrée* 79, no. 5-6 (1941).

Archimedean approximation, which explicitly privileged the spherical over the rectilinear. In short, the passages discussed in this paper demonstrate both Galileo's remarkable, forward-looking physical intuitions, as well as his attachment to the spherical framework of his Aristotelian predecessors.

5.1 THE PROBLEM OF FREE FALL

Copernicus's geokinetic hypothesis may have solved problems related to the physics of the celestial spheres. As we have seen, Copernicus himself thought his system was more consistent with Aristotelian principles than the Ptolemaic astronomy he sought to replace. For terrestrial physics, however, the motion of the earth caused a host of problems. It was clear to most late-Renaissance philosophers that the Copernican hypothesis was incompatible with Aristotelian-Scholastic principles of terrestrial motion. The existing physics predicted a slew of effects of a moving earth that were simply not observed.

One of the many effects at issue involved falling bodies. Consider a ball released from the top of a tower. The scholastic account of the ball's motion says the ball, because of its natural heaviness, drops in a straight line toward the center of the universe, a point that happens to coincide with the center of the earth. If one assumes the earth is rotating eastward about its center once a day, however, the tower will move a considerable distance[3] in the second or two the ball takes to fall. Thus, according to scholastic physics, the ball should land far to the *west* of the tower, left behind by the earth's rapid rotation. Of course, this is not the case. The ball actually lands where one would expect it to if the earth did not move – near the foot of the tower. Either the motion of the earth would have to be rejected, or the Scholastic physics would have to be amended to account for the appearance of the earth's stability.

Today, we have come to accept the rotation of the earth, and we have adopted a new set of physical principles to account for terrestrial motion. We understand that the ball falls at the foot of the tower because of *inertia*. Before being dropped, the ball partakes of the general west-to-east motion of the tower, the ground, and the whole earth. After it is released, the ball

[3] Near the equator, more than half a mile in two seconds.

conserves this motion, even as it falls. Hence, the ball moves along with the tower and falls at its base.

However, the ball does not land exactly at the foot of the tower, but slightly to the *east*. This is because of the principle of *linear* inertia. Bodies tend to perpetuate their motion in straight lines. That is, an undisturbed moving body will move at a uniform speed in a straight line. In the case of the ball, the top of the tower is further from the axis of the earth's rotation than points closer to the surface, all of which are rotating at the same angular speed – one revolution per day. Therefore, the top will have a greater linear velocity than the lower parts of the tower and the ground on which it stands. The ball, when released, will retain this greater linear velocity and outpace the tower and the ground beneath, thus falling to the *east* of the point directly below the point from which it was dropped, *ahead* of the earth's rotation.

Notice that a principle of *circular* inertia will not lead to this result. Suppose bodies tend to preserve their motion around a stipulated axis. On a rotating earth, bodies rotate daily around the earth's axis of rotation. If we assume a principle of circular inertia, bodies will preserve this motion. That is, a body left undisturbed will move in a circle parallel to the earth's equator at a constant angular velocity, conserving angular momentum. In the case of the dropped ball, the top of the tower has the same angular velocity as all the other parts of the tower, as well as the ground beneath, since all rotate around the earth's axis once a day. If the ball, once released, conserves this *angular* momentum, it will fall exactly at the foot of the tower, directly below the point from which it was dropped. Circular inertia does not produce an eastward deflection of the falling body.

The eastward deflection of a falling body is a direct consequence of the principle of linear inertia and the rotation of the earth. If an author were to predict its existence, one could reasonably infer that the author at least glimpsed the notion of linear inertia and its implications.[4] In fact, Galileo *does* predict, at least in passing, the eastward deflection of falling bodies.

Towards the end of the second day of the *Dialogue*, Galileo discusses an objection to the rotation of the earth put forward by a pupil of the Jesuit scholastic Christopher Scheiner –

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[4] Alexander Koyré, for example, argues that a correct solution of the problem of free fall, including the concomitant recognition of linear inertia, is the culmination of early modern reasoning. It marks the moment renaissance philosophers were finally able to "free themselves from the conjoint influence of tradition and common sense, to draw – and to accept – the inevitable consequences of their own fundamental concepts." By the way, Koyré explicitly denies that Galileo happened on this solution. Alexandre Koyré, "A Documentary History of the Problem of Fall From Kepler to Newton," *Transactions of the American Philosophical Society* 45, no. 4 (1955): 329.

Joannes Georgius Locher. In his *Mathematical Disquisitions* (1614), Locher had argued that a cannon ball falling from the orbit of the moon would take six days to reach the center of the earth. Galileo places the argument in the mouth of Simplicio:

> SIMP. …it is indeed a most incredible thing (in his [Locher's] view and mine) that during its descent it should keep itself always in our vertical line, continuing to turn with the earth about its center for so many days, describing at the equator a spiral in the plane of the great circle, and at all other latitudes spiral lines about cones, and falling at the poles in a simple line.[5]

A falling ball is not left behind by a spinning earth. Instead, it seems to fall vertically, forever remaining over the same spot on the earth's surface. Now, if the earth were spinning, Locher continues, the ball would have to describe wildly implausible figures in order to remain in the vertical line over the same spot on the earth's surface. This consequence seems absurd. There is no physical reason a body would move in such strange paths. Hence, we have a *reductio ad absurdum*, and the argument serves as a refutation of the motion of the earth.

Galileo, in the mouth of Salviati, replies, based on the square law of falling bodies, that a cannon ball falling from the orbit of the moon would take less than three hours to reach the earth. Thus, it would not have to describe wildly implausible curves as it fell. Simplicio, then, is left with the simpler objection that "it seems to me a remarkable thing in any case that in coming from the moon's orbit, distant by such a huge interval, the ball should have a natural tendency to keep itself always over the same point of the earth which it stood over at its departure, rather than to fall behind in such a very long way."[6]

To this, Salviati replies:

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[5] "...è ben, per suo e mio parere, incredibilissima cosa che ella nel descendere a basso si andasse sempre mantenendo nella nostra linea verticale, continuando di girare con la Terra intorno al suo centro per tanti giorni, descrivendo sotto l'equinoziale una linea spirale nel piano di esso cerchio massimo, e sotto altri paralleli linee spirali intorno a coni, e sotto i poli cadendo per una semplice linea retta." Galileo Galilei, *Dialogue Concerning the Two Chief World Systems*, trans. Stillman Drake (Berkeley: University of California Press, 1967), 219; Galileo Galilei. *Le Opere di Galileo Galilei*, Antonio Favaro, ed., (Florence: Barbera, 1890-1908), VII 245. (Text here and below from Galileo Galilei, *Dialogo sopra i due massimi sistemi del mondo tolemaico e copernicano* (E-text, 2004 [cited 2006]); available from http://www.liberliber.it/biblioteca/g/galilei/dialogo_sopra_i_due_massimi_sistemi_del_mondo_tolemaico__etc/rtf/dialog_r.zip.) See also Koyré, "A Documentary History of the Problem of Fall From Kepler to Newton," 330-2.

[6] "...parmi ad ogni modo che venendo dal concavo della Luna, distante per sí grand'intervallo, mirabil cosa sarebbe che ella avesse instinto da natura di mantenersi sempre sopra 'l medesimo punto della Terra al quale nella sua partita ella soprastava, e non piú tosto restar in dietro per lunghissimo intervallo."Galilei, *Dialogue*, 233; Galilei. *Opere*, VII 259.

> SALV. The effect might be remarkable or it might be not at all remarkable, but natural and ordinary, depending upon what had gone on before. If, in agreement with the supposition made by the author [Locher], the ball had possessed the twenty-four-hour circular motion while it remained in the moon's orbit, together with the earth and everything else contained within that orbit, then that same force which made it go around before it descended would continue to make it do so during its descent too. And far from failing to follow the motion of the earth and necessarily falling behind, it would even go ahead of it, seeing that in its approach toward the earth the rotational motion would have to be made in ever smaller circles, so that if the same speed were conserved in it which it had within the orbit, it ought to run ahead of the whirling of the earth, as I said.[7]

To clarify the sense of this passage, consider a vertical line extended from the center of the earth, through a single point on the earth's surface, to the orbit of the moon. If we assume that the earth is rotating, all segments of the line will move with the same angular velocity as the line rotates around the earth's axis – they all make one rotation in twenty-four hours. However, each segment of the line describes a slightly smaller circle than the segment above, since the former is closer to the axis of rotation. In order for a ball the fall along the vertical line, its rotation must describe "ever smaller circles." By the same token, each segment of the vertical line has a slightly slower tangential velocity than the one above. Thus, if the cannon ball conserves the tangential motion it had at the beginning of its fall, when it was "in the moon's orbit," it will fall ahead of the line, since its tangential motion is greater than that of the lower segments of the line. That is, the ball "ought to run ahead of the whirling of the earth" – to the *east*, ahead of the motion of the ground beneath.

It is amazing how clearly this passage replicates the deduction of the eastward deflection of falling bodies we sketched above. We can very safely infer that Galileo is assuming a principle of linear inertia, at least in the small scale. While Galileo gives no indication that the falling body would fly off to infinity along a straight line if it were suddenly released from the earth's gravity, he does suppose that it conserves its linear, tangential speed from moment to moment as it descends through the "ever smaller circles." The conservation of angular, circular

[7] "L'effetto può esser mirabile, e non mirabile, ma naturale e ordinario, secondo che sono le cose precedenti. Imperrocchè, se la palla (conforme a' supposti che fa l'autore) mentre si tratteneva nel concavo della Luna aveva il moto circolare delle ventiquattr'ore insieme con la Terra e co'l resto del contenuto dentro ad esso concavo, quella medesima virtù che la faceva andare in volta avanti lo scendere, continuerà di farla andar anco nello scendere; e tantum abest che ella non sia per secondare il moto della Terra, ma debba restare indietro, che più tosto dovrebbe prevernirlo, essendochè nell'avvicinarsi alla Terra il moto in giro ha da esser fatto continuamente per cerchi minori: talchè, mantenendosi nella palla quella medesima velocità che ell'aveva nel concavo, dovrebbe anticipare, come ho detto, la vertigine della Terra." Galilei, *Dialogue*, 233; Galilei. *Opere*, VII 259-60.

motion, on the other hand, would cause the ball to fall in the vertical line, not ahead of it. On the basis of this linear principle, Galileo gives both a clear prediction of the eastward deflection and an accurate explanation of its cause, all in one concise statement. At least in this case, a principle of linear inertia is apparent in Galileo's argument.

Notice, however, that the passage itself is equivocal. The first part of the passage appeals to the conservation of *circular* motion. Galileo says that the fall of the ball upon the place beneath is *not* remarkable if the ball "possessed the twenty-four-hour circular motion" and preserves this circular "going around" while it descends. So it's *not* remarkable that "the ball should have a natural tendency to keep itself always over the same point" – which refutes Locher's scholastic objection. In this part of the passage, Galileo assumes that circular motion is conserved and predicts no deflection at all. Only the latter part of the passage appeals to a conservation of linear motion, and only in the latter part that the eastward deflection is mentioned.

5.2 HUNTERS AND CANNONS

Free fall is not the only phenomenon to exhibit a deflection due to the conservation of linear motion and the rotation of the earth. Projectiles will also deflect in relation to the surface of the earth. Consider, for example, a cannon fired directly along the meridian, due south in the northern hemisphere. Just as the top of a tower is further from the earth's axis than its base, smaller latitudes (those closer to the equator) are further from the axis than greater latitudes (those closer to the pole). This is a simple consequence of the earth's orbicular shape. Hence, since the whole earth rotates uniformly, bodies at higher latitudes have a lesser linear velocity than bodies at smaller latitudes. If one fires a cannonball southward in the northern hemisphere (or northward in the southern), the projectile is fired with a lesser eastward momentum than the ground over which it passes in flight. Since the projectile conserves this lesser velocity, it will drift westward relative to the ground. In other words, the cannonball will land west of the meridian from which it was fired and along which it was aimed.[8]

Galileo also predicts this westward deflection in the *Dialogue*. Galileo introduces the case of a cannon firing along the meridian in the voice of Salviati, who recapitulates scholastic arguments against the motion of the earth:

> SALV. …Not only this, but shots to the south or north likewise confirm the stability of the earth; for they would never hit the mark that one had aimed at, but would always slant toward the west because of the travel that would be made toward the east by the target, carried by the earth while the ball was in the air.[9]

According to scholastic principles, the projectile should move in a line toward the pole during its flight. While the ball is airborne, however, the earth's continuing rotation will carry the target a considerable distance eastward. Thus, the shot will miss to the west. An artilleryman's ability to hit a target simply by aiming at it, a scholastic would conclude, is evidence against the rotation of the earth.

Galileo has Salviati return to the issue of marksmanship and aim later in the *Dialogue* by way of a discussion of bird hunting. Salviati reports that hunters are able to hit their targets simply by keeping a moving bird in their sights, as if the bird were motionless:

> SALV. …They work in exactly the same way as if shooting at a stationary bird; that is, they fix their sights on a flying bird and follow it by moving the fowling piece, keeping the sights always on it until firing; and thus they hit it just as they would a motionless one. So the turning motion made by the fowling piece in following the flight of the bird with the sights, though slow, must be communicated to the ball also; and this is combined with the other motion, from the firing.[10]

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[8] The reverse is also true. A cannonball fired northward in the northern hemisphere (or southward in the southern) will drift *east* of the meridian.

[9] "Ma non meno di questi, i tiri altresí verso mezo giorno o verso tramontana confermano la stabilità della Terra: imperocché mai non si correbbe nel segno che altri avesse tolto di mira, ma sempre sarebbero i tiri costieri verso ponente, per lo scorrere che farebbe il bersaglio, portato dalla Terra, verso levante, mentre la palla è per aria." Galilei, *Dialogue*, 127; Galilei. *Opere*, VII 153.

[10] "...operano nello stesso modo per appunto che quando tirano all'uccello fermo, cioè che aggiustano la mira all'uccel volante, e quello co 'l muover l'archibuso vanno seguitando, mantenendogli sempre la mira addosso sin che sparano, e che cosí gli imberciano come gli altri fermi. Bisogna dunque che quel moto, benché lento, che l'archibuso fa nel volgersi, secondando con la mira il volo dell'uccello, si comunichi alla palla ancora e che in essa si congiunga con l'altro del fuoco..." Galilei, *Dialogue*, 178; Galilei. *Opere*, VII 204.

Galileo, as Salviati, imagines a hunter turning on his heel in order to keep his sights trained on a bird. The hunter matches the angular velocity of his gun (and the bullet within) to the angular velocity of the bird, both measured from the axis of rotation, the hunter himself. When the gun is fired, the bullet gains a motion toward the bird. Nevertheless, according to Salviati, it maintains its original angular velocity, equal to that of the bird. Together, the two motions – one toward the bird, one matching its progress across the range – result in the bullet striking the target.

Salviati argues that this is analogous to a cannon firing along the meridian on a moving earth. He notes that in this case, the cannon is always aimed at the mark directly to the north or south. Thus, just as the marksman hits the bird by maintaining his aim, the cannon also hits the mark:

> SALV. …Upon this depends the proper answer to that other argument, about shooting with the cannon at a southerly or northerly mark… I reply, then, by asking whether it is not true that once the cannon was aimed at a mark and left so, it would continue to point at that same mark whether the earth moved or stood still. It must be answered that the sighting changes in no way; for if the mark is fixed, the cannon is likewise fixed; and if it moves, being carried by the earth, the cannon also moves in the same way. And if the sights are so maintained, the shot always travels true, as is obvious from what has been said previously.[11]

As in the hunter's sport, the gun, shot, and target all share the same angular velocity before firing, in this case caused by the rotation of the earth. This angular velocity is then conserved after the cannon is fired. Thus, as long as the cannon is initially aimed at the target, the shot will land true, even if the target moves west to east as the earth rotates.

It is clear from his analyses that Salviati is applying a principle of circular inertia. A projectile shares the diurnal rotation of everything else on the surface of the earth, including the target. This angular momentum is conserved by the projectile before, during, and after its launch. As a result, it strikes the target, just as if the earth were not rotating at all. Salviati

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[11] "E di qui depende la propria risposta all'altro argomento del tirar con l'artiglieria al berzaglio posto verso mezogiorno o verso settentrione... Rispondo dunque domandando se, aggiustata che si sia l'artiglieria al segno e lasciata star cosí, ella continua a rimirar sempre l'istesso segno, muovasi la Terra o stia ferma. Convien rispondere che la mira non si muta altrimenti, perché, se lo scopo sta fermo, l'artiglieria parimente sta ferma, e se quello, portato dalla Terra, si muove, muovesi con l'istesso tenore l'artiglieria ancora; e mantenendosi la mira, il tiro riesce sempre giusto, come per le cose dette di sopra è manifesto." Galilei, *Dialogue*, 178-79; Galilei. *Opere*, VII 204.

concludes, confuting the scholastic argument he presented earlier, that an artilleryman's ability to hit his target simply by aiming at it is not evidence against the motion of the earth, after all.

At this point, though, another character in the *Dialogue*, Sagredo, interrupts to object to Salviati's interpretation. According to Sagredo, it is linear, not angular, velocity that is conserved:

> SAGR. Just a minute please, Salviati, while I bring up something which occurs to me about these hunters and the flying birds. I believe that their way of operating is as you said, and I likewise think that it results in hitting the birds… But in the marksman's shooting, the motion of the fowling piece with which he is following the bird is very slow in comparison with the bird's flight. It seems to me to follow from this that the small motion conferred upon the shot by the turning of the barrel cannot multiply itself in the air up to the speed of the bird's flight, once the ball has left, in such a way that it always stays aimed at the bird. Rather, it seems to me that the bullet would necessarily be anticipated and left behind.[12]

Before being fired, the bullet has a much smaller linear velocity than the bird, since it is much closer to the axis of rotation, even though both move with the same angular speed. Once in flight, then, the shot conserves this smaller velocity. It "cannot multiply itself in the air up to the speed of the bird's flight" and will miss behind. As the bullet passes along its trajectory, it cannot spontaneously increase the small linear speed it possessed in the barrel to equal the greater linear speed of the bird in flight. If the testimony of the hunters were strictly true, they would always miss.

Sagredo's principle of linear inertia leads him to a very different explanation of the hunters' ability to hit a bird. They may indeed report keeping the moving bird in their sights, but other factors must be involved:

> SAGR. …So I believe that among the reasons that the marksman hits the bird, besides that of his following its flight with the gun barrel, there is that of anticipating it somewhat by keeping the sights ahead. Moreover, I believe the shooting is done not with a single ball but with a large number of pellets which,

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[12] "Fermate un poco in grazia, signor Salviati, sin che io proponga alcun pensiero che mi si è mosso intorno a questi imberciatori d'uccelli volanti: il modo dell'operar de' quali credo che sia qual voi dite, e credo che l'effetto parimente segua del ferir l'uccello... ...ma nel tiro dell'imberciatore il moto dell'archibuso, col quale va seguitando l'uccello, è tardissimo in comparazion del volo di quello; dal che mi par che ne séguiti che quel piccol moto che conferisce il volger della canna alla palla che vi è dentro, non possa, uscita che ella è, multiplicarsi per aria sino alla velocità del volo dell'uccello, in modo che essa palla se gli mantenga sempre indirizzata, anzi par ch'e' debba anticiparla e lasciarsela alla coda." Galilei, *Dialogue*, 179; Galilei. *Opere*, VII 205.

> spreading out in the air, occupy a very large space. And on top of this there is the very great speed with which they go toward the bird upon leaving the gun.[13]

The initial disparity in linear velocities is compensated by various other features of the hunters' sport. Moreover, Sagredo notes, the bullet's motion toward the bird is much faster than either transverse motion. As a result, the bullet cannot fall far behind during its flight, and will miss by only a small distance, easily covered by the spread of the pellets. Nevertheless, the fact remains that a single bullet will miss if the gun is kept aimed at its target.

As for cannons firing along the meridian, Sagredo holds that Salviati's discussion is mostly correct, since the difference in initial, eastward linear velocities between cannonball and target is not as great as that between the bullet and the bird.

> SAGR. …but it does not seem to me that these actions exactly agree with those of shooting a cannon, which must hit just as accurately when gun and target are moving as when both are at rest. The disparity seems to me to be that in shooting the cannon, it and the target are moving with equal speed, both being carried by the motion of the terrestrial globe.[14]

Both the cannonball and the target are carried around by the rapid eastward motion of the earth. They can be considered as moving with equal initial linear velocity, which is conserved by the ball after firing. Hence the ball will keep up with the target and hit it just as if both were at rest. In any case, the initial speed of the ball is not "very slow" in relation to its target.

Strictly speaking, however, the linear velocities of ball and target are only equal if both are located at the same latitude on the surface of the earth. As we have seen, if the latitude differs, the cannonball and target *will* have different speeds, and the shot will miss:

> SAGR. …Although the cannon will sometimes be placed closer to the pole than the target and its motion will consequently be somewhat slower, being made

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[13] "Talché del colpire dell'imberciatore crederei che ne fusser cagioni, oltre al secondar il volo col moto della canna, l'anticiparlo alquanto, con tener la mira innanzi, ed oltr'a ciò il tirar (com'io credo) non con una sola palla, ma con buon numero di palline, le quali, allargandosi per aria, occupano spazio assai grande, ed oltre a questo l'estrema velocità con la quale dall'uscita della canna si conducono all'uccello." Galilei, *Dialogue*, 179; Galilei. *Opere*, VII 205.

[14] "...ma non mi par già che tale operazione sia del tutto conforme a questa de i tiri dell'artiglieria, li quali debbon colpire tanto nel moto del pezzo e dello scopo, quanto nella quiete comune di amendue: e le difformità mi paion queste. Nel tiro dell'artiglieria, essa e lo scopo si muovono con velocità eguale, sendo portati amendue dal moto del globo terrestre..." Galilei, *Dialogue*, 179; Galilei. *Opere*, VII 205.

[14] "...e se ben tal volta l'esser il pezzo piantato piú verso il polo che il berzaglio, ed in conseguenza il suo moto alquanto piú tardo, come fatto in minor cerchio, tal differenza è insensibile, per la poca lontananza dal pezzo al segno." Galilei, *Dialogue*, 179; Galilei. *Opere*, VII 205.

> along a smaller circle, this difference is insensible because of the small distance from the cannon to the mark.[15]

The effect may be insensible, since the disparity in initial velocities is almost negligible, but it is clear that Sagredo acknowledges its presence. The ball will fall west of the mark if the cannon is at higher latitude. Once again, we have witnessed Galileo deducing a physical prediction from a principle of linear, not circular, inertia.

It is not Galileo's usual practice to put important arguments, let alone experimental predictions, in the mouth of Sagredo. Usually these come from Salviati, Galileo's stand-in. Here, however, Salviati endorses Sagredo's reasoning:

> SALV. See how far the flight of Sagredo's wit anticipates and gets ahead of the crawling of mine, which might perhaps have noticed these distinctions, but not without long mental application.[16]

Galileo explicitly validates Sagredo's discussion, including the prediction that a cannon fired to the north or south will miss its target. The prediction of an "insensible" variation is left as a positive affirmation. At the same time, though, Salviati does not renounce his own appeals to circular inertia. He simply moves on to other topics.

Though Sagredo's prediction demonstrates Galileo's acceptance of linear inertia, the statement is again equivocal. It comes in the course of an argument between two characters in the text, both of whom seem to speak with Galileo's voice, but only one of whom accepts a principle of linear inertia and the resulting drift of projectiles. In essence, Galileo disagrees with himself over the linearity or circularity of inertia and, in the end, leaves the issue unresolved. He remains unclear as to whether we are to accept circular inertia, like Salviati, or linear inertia, like Sagredo. He never says which of his characters is right.

Galileo's indecision regarding the nature of inertia leaves the appearance a tension between linearity and circularity in his thought. Galileo explicitly recognizes that the two principles of inertia lead to different conclusions about the outcome of a physical situation, so it seems he should choose which principle, and thus which prediction, is the right one. It seems

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[15] "...e se ben tal volta l'esser il pezzo piantato piú verso il polo che il berzaglio, ed in conseguenza il suo moto alquanto piú tardo, come fatto in minor cerchio, tal differenza è insensibile, per la poca lontananza dal pezzo al segno." Galilei, *Dialogue*, 179; Galilei. *Opere*, VII 205.

[16] "Ed ecco di quanto il volo dell'ingegno del signor Sagredo anticipa e previene la tardità del mio, il quale forse arebbe avvertite queste disparità, ma non senza una lunga applicazion di mente." Galilei, *Dialogue*, 180; Galilei. *Opere*, VII 205.

strange, to modern eyes, that he does not. The remainder of this paper will be an attempt to explain this apparent tension. As we shall argue hereafter, this internal tension points to a deeper, conceptual conflict in Galileo's description and explanation of phenomena.

5.3 RECTILINEAR AND SPHERICAL SPACE

Galileo's inertial bivalence ultimately derives from a dual representation of space: he uses two different spatial frameworks to represent phenomena – one rectilinear, one spherical. Galileo uses both kinds of spatial framework throughout the course of his work. Importantly, they are always distinguished by the size of the space they are meant to represent. For global-scale phenomena, particularly those involving the rotation of the earth, Galileo employs a spherical representation of space. That is, he presupposes a center, then specifies locations, directions, and other spatial properties in relation to that point. For smaller, local-scale spaces, such as a laboratory, Galileo uses a rectilinear representation of space. Location, directions, etc., are specified in relation to a presupposed line or orientation. In general, then, small-scale phenomena are described linearly, while global-scale situations are represented spherically.

5.3.1 Large-Scale Space

Galileo's spherical representation of global- or cosmic-sized space is a consistent feature of his thought throughout his career. In the early *De Motu* tract (c. 1591), written even before Galileo accepted Copernicanism, Galileo describes an Aristotelian, spherical structure of space, built around the presupposed center of the universe:

> If, for example, we suppose that nature, at the time of the construction of the universe, divided all the common matter of the elements into four equal parts, and then assigned to the form of the earth its own matter [i.e., earth], and likewise to the form of air *its* own matter [i.e., air], and that he form of the earth caused its matter to be compressed in a very narrow space, while the form of the air permitted the placing of its matter in a very ample space, would it not be reasonable for nature to assign a larger space to air, and a smaller space to earth?

> But in a sphere the spaces become narrower as we approach the center, and larger as we recede from the center.[17]

In this rather peculiar "just-so" story, Galileo speculates about the creation of the universe. He describes how the elements were placed in a previously empty space according to their form. This primordial space, however, has a spherical structure. Spaces, for instance, are distinguished by their distance from the center. Thus, intuitively speaking, this space is "narrower" nearer the center. A portion of space with unit radial height and unit solid angle will have a greater volume further from the center. The appropriate place for denser elements, then, is in the denser, "narrower" space, which explains their location nearer the center. This cosmogony relies on the spherical structure Galileo assigns to cosmic space in and of itself.[18]

The notion of natural place is carried into the *Dialogue*, where Galileo again discusses the notion of cosmic order. He argues that the world is inherently ordered, with each body in its prescribed place:

> SALV. …I admit that the world is a body endowed with all the dimensions, and therefore most perfect. And I add that as such it is of necessity most orderly, having its parts disposed in the highest and most perfect order among themselves.[19]

The various parts of the universe are each "disposed" in their appropriate places. Place, however, is here specified by a distance from a stipulated center. Consequently, circular motions around the presupposed center preserve the inherent order of the universe:

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[17] "Ut si, exempli gratia, intelligamus, naturam in prima mundi compagine totam elementorum communem materiam in quatuor aequas partes divisisse, deinde ipsius terrae formae suam materiam tribuisse, itidem et formae aëris suam, terrae autem formam materiam suam in angustissimo loco constipasse, aëris autem formam in amplissimo loco materiam suam reposuisse, nonne congruum erat ut natura aëri magnum spatium assignaret, terrae minus? At angustiora sunt loca in sphera quo magis ad centrum accedimus, ampliora vero quo magis ab eodem recedimus." Galileo Galilei, "De Motu," in *On Motion and On Mechanics*, ed. Stillman Drake and I. E. Drabkin (Madison: University of Wisconsin Press, 1960), 15; Galilei. *Opere*, I 253. Kepler similarly argues that the density of bodies near the sun is consonant with the "certain form of narrowness" of "the very places which are near the centre." Kepler, *Epitome of Copernican Astronomy & Harmonies of the World*, 40.

[18] A similar discussion appears in the dialogue form of the *De Motu*. There, Galileo comments that the "argument is not to be considered a conclusive reason for this disposition of the elements, still it has in it some appearance of truth." See Stillman Drake and I. E. Drabkin, *Mechanics in Sixteenth-Century Italy* (Madison: University of Wisconsin Press, 1969), 339; Galilei. *Opere*, I 374-5.

[19] "...ed ammetto che il mondo sia corpo dotato di tutte le dimensioni, e però perfettissimo; ed aggiungo, che come tale ei sia necessariamente ordinatissimo, cioè di parti con sommo e perfettissimo ordine tra di loro disposte." Galilei, *Dialogue*, 19; Galilei. *Opere*, VII 43.

> SALV. This principle being established then, it may be immediately concluded that if all integral bodies in the world are by nature moveable, it is impossible that their motions should be straight, or anything else but circular; and the reason is very plain and obvious. For whatever moves straight changes place and, continuing to move, goes ever farther from its starting point and from every place through which it successively passes. If that were the motion which naturally suited it, then at the beginning it was not in its proper place. So then the parts of the world were not disposed in perfect order. But we are assuming them to be perfectly in order; and in that case, it is impossible that it should be their nature to change place, and consequently to move in a straight line.[20]

Galileo concludes that bodies do not move naturally in straight lines. This would entail they sought a change of place, since straight motion always changes distance to the center, and places in the cosmos are specified by their distance from a center. Straight-line motion would cause a rearrangement of what is assumed to be perfectly arranged. Since circular motions around the center do not affect "place," they do not disrupt the "perfect order" of the cosmos. Hence, if a body's nature allows for motion, the movement can only take place circularly around the center. This argument, however, relies on the presupposition of a center and spherical structure of the space wherein bodies are "placed." In the *Dialogue*, as in the *De Motu*, Galileo subscribes to a spherical conception of cosmic space.

(It should be noted that Galileo's view of cosmic space changed between the *De Motu* and the *Dialogue*. When Galileo wrote the earlier tract, he used the stipulation of a universal center as a basis for the argument that all the terrestrial elements had a natural heaviness – i.e., a natural tendency to move toward the center. This, in turn, accounted for the earth's stability at the center of the universe. Since elemental earth, of which the terrestrial globe was primarily composed, was supposed to possess more innate heaviness than the other elements, it tended to congregate and remain around the center. By the time of the *Dialogue*, however, Galileo had accepted Copernicanism, and rejected the Aristotelian identification of the center of the universe and the terrestrial center. The stipulation of a spatial center, therefore, was open to choice:

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[20] "Stabilito dunque cotal principio, si può immediatamente concludere che, se i corpi integrali del mondo devono esser di lor natura mobili, è impossibile che i movimenti loro siano retti, o altri che circolari: e la ragione è assai facile e manifesta. Imperocché quello che si muove di moto retto, muta luogo; e continuando di muoversi, si va piú e piú sempre allontanando dal termine ond'ei si partí e da tutti i luoghi per i quali successivamente ei va passando; e se tal moto naturalmente se gli conviene, adunque egli da principio non era nel luogo suo naturale, e però non erano le parti del mondo con ordine perfetto disposte: ma noi supponghiamo, quelle esser perfettamente ordinate: adunque, come tali, è impossibile che abbiano da natura di mutar luogo, ed in conseguenza di muoversi di moto retto." Galilei, *Dialogue*, 19. Galilei. *Opere*, VII 43.

> SAGR. …Moreover, it appears that Aristotle implies that only one circular motion exists in the world, and consequently only one center to which the motions of upward and downward exclusively refer. All of which seems to indicate that he was pulling cards out of his sleeve, and trying to accommodate the architecture to the building instead of modeling the building after the precepts of architecture. For if I should say that in the real universe there are thousands of circular motions, and consequently thousands of centers, there would also be thousands of motions upward and downward.[21]

In other words, Galileo accepts Copernicus's view that there are "many centers" in the universe.[22] For the purposes of description and explanation, one may select any one of these centers and construct a spherical frame around it. There could be "thousands" of meanings of "upward" and "downward," depending upon which center is chosen as the central point of reference. The center of the earth is not especially privileged. Even so, Galileo continues to assume, as did Copernicus, that spatial properties are specified in relation to centers. "Upward" and "downward" are still directed away from and toward a center.[23])

The *Dialogue* also contains a remarkable discussion of the motion of the moon. Though tangential to the main line of argument in this essay, the discussion is particularly interesting because it contrasts Galileo's spatial conception with those of Copernicus and Gilbert, which we have discussed previously. It is also a striking demonstration of Galileo's use of a spherical conception of space. Recall Copernicus's "third motion." Copernicus described the behavior of the axis of the earth using a spherical representation of space. He specified the direction of the axis in relation to a radius to the assumed center – in this case, a point at or near the center of the sun. On this specification of direction, the direction of the axis changes as the earth rotates

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[21] "Vedesi in oltre che Aristotile accenna, un solo esser al mondo il moto circolare, ed in conseguenza un solo centro, al quale solo si riferiscano i movimenti retti in su e in giú; tutti indizi che egli ha mira di cambiarci le carte in mano, e di volere accomodar l'architettura alla fabbrica, e non costruire la fabbrica conforme a i precetti dell'architettura: ché se io dirò che nell'università della natura ci posson essere mille movimenti circolari, ed in conseguenza mille centri, vi saranno ancora mille moti in su e in giú." Galilei, *Dialogue*, 16; Galilei. *Opere*, VII 40.
[22] See Copernicus, *De Rev*, 46.
[23] A related confusion about which *center* to place at the center of a spherical representation of space underlies Galileo's oft-debated "blunder" in the "Sunspot Proof" of the earth's motion. See Galilei, *Dialogue*, 351-55, 486-87; Owen Gingrich, "The Galileo Sunspot Controversy: Proof and Persuasion," *Journal for the History of Astronomy* 34(I), no. 114 (2003); Keith Hutchison, "Sunspots, Galileo, and the Orbit of the Earth," *Isis* 81, no. 1 (1990); Koestler, *The Sleepwalkers*, 476-79; A. Mark Smith, "Galileo's Proof for the Earth's Motion from the Movement of Sunspots," *Isis* 76, no. 4 (1985); David Topper, "Colluding With Galileo: On Mueller's Critique of My Analysis of Galileo's Sunspot Argument," *Journal for the History of Astronomy* 34(I), no. 114 (2003); David Topper, "Galileo, Sunspots, and the Motions of the Earth: Redux," *Isis* 90, no. 4 (1999). The moon's motion (or non-motion) also raises similar difficulties. Galileo refers the moon's motion to a center, but he cannot decide whether the appropriate center is the earth or the center of the moon's epicycle. See Galilei, *Dialogue*, 65.

around the sun, even though the axis remains parallel to itself, pointed toward the same part of the sky. The representation of space leads to a description of a change of direction – a motion. Hence, Copernicus concluded, the observed behavior of the axis was a "third motion" of the earth.

As we have noted in, Gilbert employed a linear representation of space to describe the motion of the earth's axis. On his view, directions are specified in relation to a self-parallel linear orientation at all points in space. Thus, the direction of the earth's axis is the same at all points in its orbit. It remains parallel to itself not because it changes direction, but because the direction does not change. According to Gilbert, Copernicus's "third motion" is not a motion at all.

Galileo clearly thought highly of Gilbert's magnetic philosophy. In the *Dialogue*, Galileo counts himself among those who have "attentively read [Gilbert's] book and carried out his experiments." Consequently, Galileo reports – as a fact – Gilbert's observation that Copernicus's "third motion" is "not a real thing, but a mere appearance" that does not require "any cause of motion." He also goes on to relate Gilbert's speculation that there is a magnetic "force inhering in the terrestrial globe and making it point with definite parts of itself toward definite parts of the firmament."[24] It is equally clear, however, that Galileo did not understand the rectilinear representation of space that led Gilbert to the rejection of the "third motion." In fact, when Galileo deals with an analogous situation in the *Dialogue*, he applies a spherical representation of space and, as a result, commits the reverse of Copernicus's own mistake. He describes a motion as a non-motion.

The phenomenon in question is the motion of the moon. Just as the earth revolves around the sun, the moon revolves around the earth. Now, as the moon revolves, one side is always facing the earth, so the side of the moon visible from the earth is always the same. In the First Day of the *Dialogue*, Galileo describes this behavior in a fashion similar to the way Copernicus describes the earth. In particular, he has Sagredo comment:

> SAGR. …Assuming for the sake of the argument that there is someone on the moon who can see the earth, he will see the entire surface of the earth every day, by virtue of the moon's motion with respect to the earth every twenty-four or

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[24] Galilei, *Dialogue*, 398-400.

> twenty-five hours. But we shall never see more than half the moon, since it makes no revolution of its own, as it would have to do for all of it to show itself.[25]

Galileo specifies the direction of the face of the moon in relation to the radius between it and the center of its orbital motion, the earth. Since the same face always faces the earth, the radius always intersects the same half of the moon. In other words, the "direction" the moon faces, so conceived, does not change – i.e., the moon "makes no revolution of its own."

Sagredo's comment assumes that the moon rotates around the earth on a simple circular orbit. As Galileo knows, however, most contemporary astronomers supposed that it orbited on an epicycle.[26] In this case, a rotation *would* be required to keep a single face directed toward the earth, as Salviati notes:

> SALV. Provided that the very opposite is not implied; namely, that its own rotation is the reason that we do not see the other side – for such would have to be the case if the moon should have an epicycle.[27]

Again, the direction of the moon's face is referred to a center of its motion. In this case, though, direction is specified in relation to a radius to the center of the epicycle, not the center of the earth. If the moon were not to rotate about its axis, a radius connecting the moon and the center of the epicycle would always intersect the same face. Hence, the same side of the moon would always be directed toward the center of the epicycle, and its aspect toward the earth would change. In order to keep one half toward the earth, Salviati implies, the moon must rotate at the same rate, and in the opposite sense, as it rotates around its epicycle. This change of "direction" would be "the reason we do not see the other side" as the moon orbits on its epicycle.

Galileo's discussion of the moon's rotation makes it clear, however, that he is employing a spherical representation of space. Whether or not the moon orbits on an epicycle, the direction it faces is specified in relation to a radius to a center – that of the earth for Sagredo's simple orbit, that of the epicycle for Salviati's epicyclic orbit. In the former case, the "direction" of the

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[25] "...dato e non concesso che nella Luna fusse chi di là potesse rimirar la Terra, vedrebbe ogni giorno tutta la superficie terrestre, mediante il moto di essa Luna intorno alla Terra in ventiquattro o venticinque ore; ma noi non veggiamo mai altro che la metà della Luna, poiché ella non si rivolge in se stessa, come bisognerebbe per potercisi tutta mostrare." Ibid., 65; Galilei. *Opere*, VII 90.

[26] In the Ptolemaic system, an epicycle (and a rotating eccentric center) was used to model the moon's orbit. Copernicus used two epicycles. See Copernicus, *De Rev*, Bk IV, esp. 189ff; Ptolemy, *The Almagest*, Bk. V, esp. 333ff.

[27] "Purché questo non accaggia per il contrario, cioè che il rigirarsi ella in se stessa sia cagione che noi non veggiamo mai l'altra metà; ché cosí sarebbe necessario che fusse, quando ella avesse l'epiciclo." Galilei, *Dialogue*, 65; Galilei. *Opere*, VII 90.

moon's face does not change. In the latter, it changes in order to negate the effect of the epicyclic rotation. In a rectilinear representation of space, by contrast, the same part of the moon always faces the earth because the moon rotates – changes the direction of its face with respect to a self-parallel orientation – at the same rate it revolves about the *earth*, not an epicycle.

Like Copernicus, Galileo specifies direction in relation to a radius to a center.[28] Thus, though Galileo notes Gilbert's observation that Copernicus's "third motion" is actually a "staying," he, in Sagredo's case, attributes a "staying" to the moon that is really a motion. In Salviati's case, meanwhile, he attributes the wrong rotation. He fails to notice these errors because his specification of direction relies on a spherical representation of space. He reports Gilbert's conclusion, but he has not adopted Gilbert's rectilinear space.

5.3.2 Small-Scale Space

While his representations of global-scale spaces are consistently spherical, Galileo consistently uses a rectilinear representation of space for small-scale phenomena. Consider the description of a laboratory frame in the First Day of the *Dialogue*:

> SALV. Therefore if you assign any point for the point of origin of your measurements, and from that produce a straight line as the determinant of the first measurement (that is, of the length) it will necessarily follow that the one which is to define the breadth leaves the first at a right angle. That which is to denote the altitude, which is the third dimension, going out from the same point, also forms right angles and not oblique angles with the other two. And thus by three perpendiculars you will have determined the three dimensions AB length, AC breadth, and AD height, by three unique, definite, and shortest lines.[29]

Here, dimensions are specified by reference to presupposed "determinants" – basically, conceptual representations of the dimensions. Each of these "determinants" is both linear and perpendicular to the others. A body is "measured," then, by specifying its magnitude in each of

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[28] See Gingrich, "The Galileo Sunspot Controversy: Proof and Persuasion."

[29] "Adunque se voi stabilirete alcun punto per capo e termine delle misure, e da esso farete partire una retta linea come determinatrice della prima misura, cioè della lunghezza, bisognerà per necessità che quella che dee definir la larghezza si parta ad angolo retto sopra la prima, e che quella che ha da notar l'altezza, che è la terza dimensione, partendo dal medesimo punto formi, pur con le altre due, angoli non obliqui, ma retti: e così dalle tre perpendicolari avrete, come da tre linee une e certe e brevissime, determinate le tre dimensioni, AB lunghezza, AC larghezza, AD altezza." Galilei, *Dialogue*, 13-4; Galilei. *Opere*, VII 37.

these dimensions. That is, the measure of a body is given by the distance it subtends along directions parallel to the three assumed "determinants" of dimension. The dimensions denoted by the "determinants" are parallel to themselves (and perpendicular to one another) throughout the space. In the small scale, a body can be described on the basis of a presupposed linear framework.

The three "determinants" of dimension also allow the specification of directions. For instance, the direction "down" is denoted by the "determinant" of height. Thus, in a spatial framework structured by *one* set of dimensions, the direction "down" will be everywhere parallel to itself and everywhere perpendicular to the directions denoted by the other determinants. In other words, the small-scale space structured by the single set of dimensions is *oriented*. It possesses a self-parallel orientation (i.e., the vertical) by which directions can be specified.[30]

There are similar examples of Galileo's use of a rectilinear representation of space for small-scale phenomena throughout his texts. In the early *De Motu*, a linear framework grounds Galileo's discussion of inclined planes:

> Let there be a line *ab* directed toward the center of the universe and thus perpendicular to a plane parallel to the horizon. And let *bc* lie in that plane parallel to the horizon. Now from point *b* let any number of lines be drawn making acute angles with line *bc*, e.g., lines *bd* and *be*. The problem, then, is why a body moving down descends most quickly on line *ab*; and on line *bd* more quickly than on *be*, but more slowly than on *ba*; and on *be* more slowly than on *bd*.[31]

Galileo assumes that the vertical is directed "toward the center of the universe," i.e., the terrestrial center. The direction "down" is specified throughout the discussion as the direction parallel to the vertical line towards the center. The horizontal, meanwhile, is denoted by a plane everywhere perpendicular to the vertical line. Very similar discussions of planes appear in the First Day of the *Dialogue* and the Third Day of the *Discourse*. The derivation of the parabolic

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[30] In fact, any of the three "determinants" would suffice as an orientation. As we shall see, however, the "determinant" of "down" is most important. The assumption of a particular "down" in the small scale allows the transition to the spherical representation of space employed in the large scale. Significantly, this entails that the orientation of the space is not arbitrary. It is determined by the larger scale representation of space within which the local framework is supposed to fit. This nesting of frameworks will be addressed below.

[31] "Sit itaque linea *ab*, ad centrum mundi tendens, quae ad planum horizonti aequidistans sit perpendicularis; in plano autem horizonti aequidistanti sit linea *bc*; ex puncto autem *b* educantur lineae quoteunque, que cum linea *bc* angulos acutos contineant, sintque lineae *bd*, *be*. Quaeritur igitur cur mobile, descendens, citissime descendat per lineam *ab*; per lineam vero *bed*, citius quam per *be*, tardius tamen quam per *ba*; et per lineam *be*, tardius quam per *bd*." Galilei, "De Motu," 63-4; Galilei. *Opere*, I 296-97.

path of projectiles in the *Discourse* also relies on the assumption that the vertical (accelerated) motion of a thrown body is everywhere parallel to itself and everywhere perpendicular to its horizontal (uniform) motion.[32] Thus, the use of linear representations of space for small-scale phenomena persists throughout Galileo's intellectual career.

There are two features of Galileo's framework for small-scale space that deserve special notice. First, note that Galileo never implies that the "determinants" of dimension can be considered as infinite. The assumed lines that denote dimensions do not stretch to infinity. Also, the dimensions denoted by the assumed "determinants" are only parallel to themselves and perpendicular to one another in one small region of space. If one were to move to another region of space and construct a new set of "determinants," there is no guarantee that the new "height" will be parallel to the old "height," and similarly for "length" and "breadth." In fact, as we shall see, in most cases the new dimensions will not be parallel to the old ones. Altogether, then, the space structured by a single set of presupposed lines is limited to some local region of the cosmos. Galileo's linear representation of space only applies on the small scale. Only local phenomena can be described and explained on the basis of a rectilinear representation of space.

Second, note that the orientation of the representation of space is not completely arbitrary. In particular, the line denoting "height" and, thus, the vertical (directions "up" and "down") is everywhere restricted to a specific direction. The vertical always corresponds to a line perpendicular to the earth's surface, and, by extension, toward the terrestrial center. Similarly, since "length" and "breadth" are perpendicular to "height," they are restricted to the horizontal plane – i.e., the plane perpendicular to the vertical. (Within this plane, though, the orientation of "length" and "breadth" is arbitrary.) In other words, the orientations of the presupposed linear "determinants" that ground descriptions and explanations in Galileo's small-scale space are, in part, determined by his framework for larger spaces, which includes a presupposition of the center toward which the vertical is directed. We shall see how this allows the small-scale representation of space to "fit" within a larger spherical spatial framework.

[32] Galileo Galilei, *Dialogues Concerning Two New Sciences*, trans. Henry Crew and Alfonso de Salvio (New York: Dover Publications, 1954), "Fourth Day," esp. 248ff.

5.4 LINEAR AND CIRCULAR INERTIA

Stillman Drake has argued that the apparent tension between linear and circular inertia in Galileo's work is a result of the medieval distinction between "natural" and "violent" or "impressed" motions. We shall argue below that Drake's explanation is insufficient, but his discussion leads us in the right direction. For Galileo's predecessors and contemporaries, "natural" and "violent" motion constituted two distinct physical regimes, each appealing to different causal explanations. Of course, the particulars of individuals' physical theories varied, but the broad outline, including the separation of "natural" and "violent" phenomena, applied generally.[33] Natural motion, on the one hand, was a result of the inherent nature of the moving body itself. The cause, therefore, was an essential and ever-present feature of the body. Hence, the body would always tend to move, and would do so, unless impeded by some external obstacle. Violent motion, on the other hand, was caused by some external source of motion. The external cause would impress some motive power, or *impetus*, on the object (or, on the Aristotelian view, on the medium surrounding it), setting it in motion.

While Galileo adopted the vocabulary of "natural" and "violent," he modified both theories in such a way that essentially dissolved the distinction. As for natural motion, Galileo acknowledged the medieval view that heavy bodies fall, and light bodies rise, because of their internal heaviness, or lightness.[34] In addition, however, he argued that a terrestrial body was, by its nature, "indifferent" to circular motion around the earth. A heavy body might naturally seek the center of the earth, and resist motion away from the center, but it would neither seek nor resist motion that did not change its altitude. Once set in motion by an external cause, then, the

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[33] The distinction between "natural" and "violent" motion is present, in a way, in the *impetus* theories of Galileo's medieval predecessors. Generally speaking, they held that the original "motor" of a body could be natural (e.g., gravity) or unnatural (a violent propulsion). The original force would then impart an *impetus* that continued the motion. For a discussion of this general point, see Clagett, *The Science of Mechanics in the Middle Ages*, 678-81. For particular examples, see the work of John Buridan (Clagett, *The Science of Mechanics in the Middle Ages*, 534-6, 57-62.), Nicole Oresme (Clagett, *The Science of Mechanics in the Middle Ages*, 570.), and Giovanni Battista Benedetti (Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, 216-8.). For one example of the distinction without reference to *impetus*, see Niccolò Tartaglia (Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, 72, 75-6.)

[34] Galileo asserted, however, that the distinction was relative, not absolute. That is, for Galileo, all terrestrial elements possessed an innate tendency to move downward. This tendency was simply more effective in the "heavy" elements. Thus, the "light" elements, fire and air, were displaced upward by the "heavy" elements water and earth. See Galilei, "De Motu." A similar position had been suggested earlier by Benedetti, though it is unclear how much Galileo knew of it. See Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, 37-41, 196ff.

body would continue to move horizontally, i.e., around the center, until impeded by another external obstacle. Strictly speaking, this was initially an impressed motion, but the motion continued by virtue of the body's natural "indifference." Thus, Galileo included continued horizontal motion in the class of "neutral" or "natural" motions.[35]

Galileo accepted, meanwhile, that "violent" motions were caused by an externally impressed impetus. Early in his career, in the *De Motu* treatise, he argued that the impetus in a body would spontaneously dissipate, like sound in a bell or heat in an iron bar.[36] Later, however, Galileo came to believe that this impetus is "indelibly impressed."[37] That is, it does not dissipate spontaneously, but tends to remain in the body. Thus, freed from external encumbrances, a body set in "violent" motion would retain its impressed impetus and continue to move perpetually. In practice, however, this does not actually happen, since the impetus is quickly removed by external resistance.[38]

Notice that Galileo's view of "natural" and "violent" motions reduces the distinction between them. Both theories of motion entail a basic inertial principle. The continued motion (or rest) of a body is explained by the body's ability to preserve an initial state of motion (or rest). If the motion is "natural," the body is "indifferent" to its original motion, and continues to move. If the motion is "violent," the body retains its original impetus, and continues to move. In either case, the body receives an initial motion and then conserves it. Galileo's use of two distinct vocabularies to describe and explain motions belies what Drake calls the "essential core of the inertial concept"[39] common to all: A body is indifferent to motion and rest, and will continue in either state if undisturbed. Galileo uses this basic notion to explain both "natural" and "violent" motions.

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[35] See Galilei, *Dialogue*, 147ff. Similar theories had been adumbrated earlier by Nicole Oresme and Girolamo Cardano. See Clagett, *The Science of Mechanics in the Middle Ages*, 602-3, 81-2; Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, 28-9; Dugas, *A History of Mechanics*, 63.

[36] Galilei, "De Motu," 79-80. This had been the position of his teacher, Francesco Buonamico. Clagett, *The Science of Mechanics in the Middle Ages*, 666-7; Koyré, *Études Galiléennes*, 28ff. Buonamico was himself following the tradition of Simplicius, Philoponus, Avicenna, Franciscus de Marchia and others who held that impetus was self-corrupting. See Clagett, *The Science of Mechanics in the Middle Ages*, ch. 8.

[37] Galilei, *Dialogue*, 154.

[38] This view of impetus corresponded with the theories of Buridan, Oresme, and others who held that impetus tended to remain in a body, but was diminished by external resistance or contrary forces (such as gravity). See Clagett, *The Science of Mechanics in the Middle Ages*, ch. 8, 666-9.

[39] Stillman Drake. *Essays on Galileo and the History and Philosophy of Science*, N. M. Swerdlow and Trevor Harvey Levere, eds., vol. II (Toronto: University of Toronto Press, 1999), 143.

Galileo's idea of inertia, in its barest form, is *not* explanatory, though. The simple proposition "bodies continue their motion" cannot account for any real phenomenon. It must first be deployed in a context that gives significance to this statement. That is, there must be some way to tell whether a body is continuing its motion; some way to tell what such behavior "looks like." Basically, the continuation of motion means that a body moves with the *same* motion at subsequent times. Two motions are the "same" if they are in the same direction at the same speed. The specification of direction and speed, however, are determined by an assumed representation of space. Hence, the core concept of Galilean inertia must be associated with a particular concept of space before it can be applied to a phenomenon. The spatial framework cashes out the meaning of "continued motion" and maps the inertial principle onto the phenomenon it is meant to explain.

Galileo's two conceptions of space profoundly affect his causal account of terrestrial motions. The core principle takes on different explanatory guises – one linear, one circular – depending on the situation to be explained. Whether inertia is assumed to be linear or circular depends on the spatial framework by which the physical situation is represented.

In a rectilinear representation of space, directions are specified in relation to an orientation – a stipulated line or ray. A particular direction is then conceived as an angular deflection from that orientation. Two motions, are in the same direction, then, if they are equally deflected from the orientation – i.e., linear and parallel to one another. Speeds, meanwhile, are specified as some relation between distance and time. Distance, in a rectilinear space, is measured linearly. Thus, two motions have the same speed if they cover the same linear distance in the same time. Therefore, if one supposes a rectilinear representation of space, the core concept of Galilean inertia entails that a body in motion will continue to move parallel to itself – along a straight line – with uniform linear speed.

In a spherical representation of space, directions are related to the center. They are specified as deflections from a radius to the center. Two motions are the same if they share a deflection from the radius. Circular motion around the center, therefore, retains its direction, since the motion is always perpendicular to the radius. Restricting ourselves to this case, since it is the one that concerns Galileo,[40] the speed of circular motion is related to the *angular* distance

[40] Linear motions directly to and from the center also retain their direction in a spherical frame, but Galileo always considers these to be accelerated or decelerated, and therefore not inertial.

subtended in some amount of time. Hence, in a spherical representation of space, Galilean inertia entails circular motion with uniform angular velocity.

We have argued that Galileo employs a linear representation of small-scale spaces and a spherical representation of large-scale spaces. It follows that linear inertia applies in small-scale situations, while circular inertia pertains to large-scale phenomena. In fact, Galileo applies these inertial principles fairly consistently according to scale. For example, Galileo usually considers projectiles as small-scale phenomena. Hence, he usually ascribes a linear principle of inertia to explain their behavior. Take his description of a rock thrown from a notched stick in the *Dialogue*:

> SALV: Then let us proceed. Simplicio, tell me what motion is made by that little rock, tight in the notch of the stick, when the boy moves it so as to cast it a long way?...
> SIMP: So far as I can see, the motion received on leaving the notch can only be along a straight line. Or rather, it is necessarily along a straight line, so far as the adventitious impetus is concerned. Seeing that it described an arc caused me some little trouble, but since that arc bends always downward, and not in any other direction, I recognized that this inclination comes from the weight of the stone which naturally pulls it down. The impressed impetus, I say, is undoubtedly in a straight line…
> SALV: You have reasoned well, and have shown yourself half a geometer. Keep it in mind, then, that your real concept is revealed in these words; that is, that the projectile acquires an impetus to move along the tangent to the arc described by the motion of the projectile at the point of its separation from the thing projecting it.[41]

The thrown rock is a small-scale phenomenon. It occurs in a limited space. As a result, Galileo assumes a rectilinear representation of space. The rectilinear representation of space, meanwhile, leads to a linear principle of inertia or, in this case, "impetus." The rock, if the effect of its weight is ignored, will continue moving in a straight line. Of course, the derivation of the parabolic trajectory of projectiles in the *Discourse* also relies on a linear representation of space,

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[41] "SALV. Seguitiamo dunque: e dicami il signor Simplicio qual sia il moto che fa quel sassetto stretto nella cocca della canna, mentre il fanciullo la muove per tirarlo lontano... SIMP. Secondo me il moto concepito nell'uscir della cocca non può esser se non per linea retta; anzi pur è egli necessariamente per linea retta, intendendo del puro impeto avventizio. Mi dava un poco di fastidio il vedergli descriver un arco; ma perché tal arco piega sempre all'ingiú, e non verso altra parte, comprendo che quel declinare vien dalla gravità della pietra, che naturalmente la tira al basso. L'impeto impresso dico senz'altro ch'è per linea retta... SALV. Voi benissimo avete discorso, e vi sete dimostrato mezo geometra. Ritenete dunque in memoria che il vostro concetto reale si spiega con queste parole: cioè che il proietto acquista impeto di muoversi per la tangente l'arco descritto dal moto del proiciente nel punto della separazione di esso proietto dal proiciente." Galilei, *Dialogue*, 191-2; Galilei. *Opere*, VII 217-18.

where the "horizontal" is understood as a plane "represented by a straight line"[42] perpendicular to the vertical. This assumption leads to the linear conservation of horizontal motion and, in turn, to the parabola.

On the other hand, Galileo usually considers oceanic voyages as examples of global-scale phenomena. Consider the following from the *Letters on Sunspots*:

> And therefore, all external impediments removed, a heavy body on a spherical surface concentric with the earth will be indifferent to rest and to movements toward any part of the horizon. And it will maintain itself in that state in which it has once been placed; that is, if placed in a state of rest, it will conserve that; and if placed in movement toward the west (for example), it will maintain itself in that movement. Thus a ship, for instance, having once received some impetus through the tranquil sea, would move continually around our globe without ever stopping; and placed at rest it would perpetually remain at rest, if in the first case all extrinsic impediments could be removed, and in the second case no external cause of motion were added.[43]

The motion of the ship on a long voyage is a globally-sized phenomenon. Hence, the representation of space is spherical, so the "horizontal" is a "spherical surface concentric with the [center of] the earth." It follows, then, that motion is conserved circularly, and the ship, once moved, will "move continually around our globe." Similarly, in the *Dialogue*, Galileo concludes that a "ship, when it moves over a calm sea" is "disposed to move incessantly and uniformly [around the earth] from an impulse once received."[44] The global scale of the situation leads to a spherical representation of space and thence to a principle of circular inertia.

Every once in a while, however, Galileo presents a situation that is neither small- nor large-scale. The motion of a projectile on a rotating earth is one such circumstance. As we have seen, projectiles are usually thought of as small-scale phenomena, but the rotation of the earth is obviously of a global-scale. In the absence of a clear determination of scale, it is unclear which

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[42] Galilei, *Discourse*, 250.

[43] "...e però, rimossi tutti gl'impedimenti esterni, un grave nella superficie sferica e concentrica alla Terra sarà indifferente alla quiete ed a i movimenti verso qualunque parte dell'orizonte, ed in quello stato si conserverà nel qual una volta sarà posto; cioè se sarà messo in stato di quiete, quello conserverà, e se sarà posto in movimento, v. g. verso occidente, nell'istesso si manterrà: e così una nave, per essempio, avendo una sol volta ricevuto qualche impet per il mar tranquillo, si moverebbe continuamente intorno al nostro globo senza cessar mai, e postavi con quiete, perpetuamente quieterebbe, se nel primo caso si potessero rimuovere tutti gl'impedimenti estrinseci, e nel secondo qualche causa motrice esterna non gli sopraggiugnesse." Galileo Galilei, *Discoveries and Opinions of Galileo* (New York: Doubleday, 1957), 113-4; Galilei. *Opere*, V 134-35.

[44] "...una nave che vadia movendosi per la bonaccia del mare... e però disposta... a muoversi, con l'impulso concepito una volta, incessabilmente e uniformemente." Galilei, *Dialogue*, 148; Galilei. *Opere*, VII 174.

of the two conceptions of space should be used to represent the situation. As a result, Galileo equivocates. He represents the same phenomenon both ways – linearly and spherically.[45]

It is this equivocation that gives rise to the apparent tension between linear and circular inertia. Consider the passages presented at the beginning of this essay. When Salviati responds to Simplicio's argument about the cannonball dropped from the orbit of the moon, his response comes in two parts. The first part is preoccupied with the "twenty-four-hour circular motion" of the "earth and everything else contained within that [i.e., the moon's] orbit." Salviati regards the situation spherically, ascribes circular inertia (or impetus – he speaks of the "force" which makes the cannonball move), and concludes that the ball will fall upon the "point of the earth which it stood over at its departure." The second part of the response, however, focuses on the cannonball's local-scale "approach toward the earth" through successive small regions of space from moment to moment. Thus, Salviati switches to a linear representation of space and a linear principle of inertia. On this view, the ball conserves its linear speed and "ought to run ahead of the whirling of the earth."

In the argument over hunters and cannons, Salviati is mainly concerned with the rotation of the earth and applies a spherical concept of space. Since the turning of the hunter represents the turning of the earth, he assumes a spherical framework for both the hunters and the cannons. The resulting appeal to circular inertia leads to a prediction that the shots will hit their targets. Sagredo, meanwhile, focuses on the motion of the projectiles as they move from gun to target. These are taken to be local phenomena, and he applies a linear concept of space. Thus, he appeals to linear inertia and predicts the shots will miss. Salviati sees one large motion of earth, gun, shot, and target together. Sagredo sees small movements of the projectiles from gun to target. The kind of motion and the scale of the phenomenon seem to indicate both representations of space, and Galileo uses both. Consequently, he predicts two outcomes of the experiment. The appearance of a tension between linearity and circularity arises from Galileo's dual representations of space.

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[45] The situation is also compatible when Galileo uses a small-scale phenomenon as an analogy for larger-scale phenomenon. The discussion of bird hunting above is one example, where Salviati applies a principle of circular inertia to a small-scale phenomenon. Another example is Drake's favorite hobby-horse, Galileo's discussion of centrifugal motion, where Galileo uses the linear impetus applicable to a whirled object to explain why objects do not fly off a spinning earth. (Galilei, *Dialogue*, 192ff.) Drake calls this the "single instance [sufficient] to show that Galileo recognized the *possibility* of rectilinear inertia." Stillman Drake, *Galileo Studies* (Ann Arbor: University of Michigan Press, 1970), 267.

5.5 CONCEPT OF INERTIA

At this point, we should pause to clarify our position on issues often discussed in the literature. Historians have long debated the modernity of Galilean inertia. This debate has run along three lines. First, whether Galilean inertia is circular or linear. Second, whether Galilean inertia is more like the pre-Galilean, medieval theory of impetus or the post-Galilean, "classical"[46] theory of inertia. Finally, whether the first issue has anything to do with the second – i.e., whether a principle of inertia must be linear in order to be "modern." Clearly, the first facet of the argument is the concern of this essay. The second issue, however, is not germane to our discussion, (obviating any need to discuss the third). Yet, since the two points are closely related, we should explain why the latter can be avoided.

On the impetus theory of motion, the movement of a body is the result of a physical cause, or force, *added* to the body when it is first set in motion. When the body is deprived of this additional impetus, it returns to and remains at rest. On the modern, inertial view, on the other hand, motion and rest are simply states of a body. Once a body is put into a state of motion or rest, it will simply remain in that state. No additional cause or force is needed to preserve the motion. The historical debate ensues because Galileo is stubbornly ambiguous about what he means when he talks about bodies retaining their motions. He seems to speak both ways. Sometimes, he describes bodies as "indifferent" to states of motion and rest. Thus, it seems that motion is a state and lacks a continuing cause. Elsewhere, motion is brought about by a persisting impetus, and Galileo certainly drew upon the impetus theories of his forbears and teachers[47] when formulating his own view. In the *Letters on Sunspots* passage quoted above, for example, Galileo mentions states of motion and impetus in successive sentences. Hence, historians can attribute any number of positions to Galileo on the basis of his texts.[48]

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[46] Here, "classical" (Koyré's term) refers to modern physics prior to the advent of relativity and quantum mechanics.
[47] See above.
[48] Koyré, for example, argues that Galilean inertia was, first, irreducibly circular and, second, akin to medieval theories of impetus. On both counts, he claims, Galileo's "physics of impetus is incompatible with the [modern] principle of inertia." Koyré, *Études Galiléennes*, 64. Drake, for his part, is satisfied that Galileo "could perceive the possibility of continued uniform rectilinear motion," even though most of his statements actually refer to some form of circular inertia. Drake, *Galileo Studies*, 271. Galileo's retention of circularity, however, is merely a "secondary" consideration. For Drake, Galileo's principle of "indifference" to states of motion and rest is paramount, and draws his notion of inertia away from medieval notions of impetus and into line with the modern concept, linear or not. See Drake, *Galileo Studies*, 246-7. Anneliese Maier and Maurice Clavelin advocate a similar position, stressing the continuity between motion and rest in Galileo's later work. See Clavelin, "Conceptual and Technical Aspects of the

We can skirt this debate because it revolves around the ontic status of the continued motion, not the epistemological and physical issues with which we are concerned. We are not interested in deciding whether the motion is caused by a real power, an impetus, superadded to the body, or if the motion is simply an inherent state of the body itself. In either case, the explanatory value of "inertia" is the same. Galilean inertia, whatever its ontic status, entails that a moving body, left undisturbed, will continue its motion. Galileo can appeal to "inertia" to explain why a body keeps moving. He says that the continued motion is *caused* by a body's inertia. That is, inertia explains phenomena, regardless of the metaphysical details of the concept. Galileo's lack of interest in the metaphysics, meanwhile, leaves the ontic question subsequent historians and philosophers have argued how to fill.

Like Galileo, we are concerned with the explanatory role of inertia, not its true being.[49] Notice also that Galileo's metaphysical disinterest helps account for the dissolution of the distinction between "natural" and "violent" motion mentioned above. For Galileo, "natural" motion is merely an internal state of a body otherwise indifferent to motion or rest. No thing has been added to the body to make it move. "Violent" motion, on the other hand, is the result of an "impetus," a quality superadded to the body by an external cause. The body gains a quality it did not possess before it was set in motion. Thus, the difference between these two kinds of motion is the ontic status of its theoretical cause. The phenomenal properties of "natural" and "violent" motion are essentially the same: the body is set in motion and then retains that original motion. It is not possible to tell what kind of motion is taking place just by looking at it. By disregarding ontological differences, Galileo *did* dissolve the distinction between "natural" and "violent" motion into a "core inertial concept."

As Stillman Drake notices, however, Galileo continues to distinguish between "natural" and "violent" motion. Moreover, Galileo generally associates rectilinearity with the "violent" motion of bodies. That is, the continuation of "violently" impressed motions was in a straight

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Galilean Geometrization of the Motion of Heavy Bodies."; Anneliese Maier, *On the Threshold of Exact Science*, trans. Steven D. Sargent (Philadelphia: University of Pennsylvania Press, 1982), esp. 103-23. Wallace Hooper, meanwhile, holds that Galileo possessed a theory of linear conserved motion, but that this was too similar to medieval impetus to be considered modern. See Wallace Hooper, "Inertial problems in Galileo's preinertial framework," in *Cambridge Companion to Galileo*, ed. Peter Machamer (Cambridge: Cambridge University Press, 1998), 171.

[49] This is why, like Galileo, we have used the terms "inertia" and "impetus" interchangeably. For Galileo's indifference to causes, see Galilei, *Discourse*, 166; Galilei. *Opere*, VIII 202; Koyré, *Études Galiléennes;* Wallace, *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought*, 149.

line.[50] Galileo's theory of "natural" or "neutral" motion, meanwhile, usually implies circular motion. Terrestrial bodies are "indifferent" to motions that do not change their altitude, so they continue in circular motion around the earth. As Drake argues:

> ...Galileo is pretty consistent in applying the idea of essential circularity to instances in which the motion is a "natural" one in his sense; that is, a motion induced by an innate tendency of the body to move when it is set free. The idea of essential rectilinearity, on the other hand, he applied most specifically to instances of "violent" motion – cannonballs and projectiles thrown by slings.[51]

Whenever Galileo speaks of "natural" motion, he generally assumes a principle of circular inertia.[52] When he describes "violent" motions, he appeals to linear inertia.

This terminological practice serves to confuse the first two facets of the historical discussion of Galilean inertia. "Natural" motion, akin to the classical notion of inertial indifference, seems attached to decidedly medieval circular motions. "Violent" motions, redolent of medieval impetus, seem associated with modern linearity. Historians, therefore, are free to emphasize and downplay these links.[53] In our view, though, Galileo's consistent association of linearity and circularity with violence and nature does not imply that the two distinctions are the same. Indeed, Galileo's ignorance of the latter, ontological distinction does not imply that he can sidestep the former, explanatory distinction. While the distinction between "natural" and "violent" motions may not be observed in phenomena, the distinction between linear and circular inertia is. The two principles lead to different explanations and different predictions of phenomena, as is clear in the passages discussed above. Thus, the distinction between linear and circular inertia is not grounded merely in the difference between "natural" and "violent" motion.

The essential concept of inertia, common to both "natural" and "violent" modes of explanation, must be deployed in the context of a representation of space, and there is a real,

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[50] Drake, *Galileo Studies*, 270.
[51] Ibid., 274.
[52] We are restricting the discussion to the unaccelerated, horizontal motion of bodies around the earth. Clearly, Galileo also speaks of falling and floating bodies as examples of vertical, accelerated motions that are "natural" but neither circular nor inertial.
[53] Drake, for example, argues that the association of "natural" and "violent" with circular and inertial is merely a rhetorical device. Drake claims that Galileo's haphazard switches between "natural" and "violent" are intended to "lead his readers easily along" from the "less involved" notion of circular inertia to the "more refined principle of linear inertia." Drake, *Galileo Studies*, 270. Koyré, on the other hand, argues that the circularity of "natural" motion reveals the medieval *hantise du sphérique et du circulaire* in Galileo's work. See Koyré, *Études Galiléennes*, 187, 273.

conceptual difference between linear and spherical spatial concepts. Linear inertia results from a linear representation of space, the circular inertia a spherical frame. Thus, one cannot simply discuss linear and circular inertia in terms of “natural” and “violent” motions. The first two facets of historical debate, often confused, must be cleaved off from one another. Galileo’s appeals to linear and circular inertia point to a significant conceptual bivalence in Galileo’s treatment of phenomena: his dual representation of space. The terminological distinction between “natural” and “violent” motion, meanwhile, is merely incidental to the true conceptual difference between circular and linear inertia. We can therefore concern ourselves with the linearity and circularity of Galilean inertia without addressing its ontic status, or, for that matter, its modernity.

Galileo’s consistent association of the terminological distinction with the conceptual one, which Drake was right to point out, only obscures the duality of his representations of space. Yet the very consistency of the association indicates that there is some link between the modes of explanation and the inertial principles that must be accounted for. In fact, the linkage is established via Galileo’s representations of space. For Galileo, and just about everyone else before him, “violent” motions were necessarily temporary and short-lived. The motion imparted to the moving body (or the conducting medium) always dissipated rapidly, either spontaneously or, as Galileo held, because of external resistance.[54] This implies that “violent” phenomena are temporary, fleeting, and – most importantly – small. In other words, “violent” motion, such as that of a projectile, generally takes place in the small-scale. This entails, as we have seen, that “violent” motions are represented linearly, and *this*, in turn, means they are explained by linear inertia. Conversely, for Galileo and his peers, “natural” motions, like those of terrestrial bodies around the center of the earth, were perpetual and global. For Galileo, these motions demanded a spherical representation and, thus, circular inertia. The indirect association of linearity with “violent” and circularity with “natural” is a consequence of their separate, direct associations with linear or spherical representations of space. The indirect association, meanwhile, hides the direct ones. Drake noticed the indirect links, but not the direct ones. The direct links, moreover,

[54] Galileo believed one could separate the conserved motion from the resistances it encountered, but only in the abstract. In reality, resistance always overcomes the impressed motion. In the *Dialogue*, for example, Galileo assumes that an impressed motion is preserved supposing all “external and accidental impediments” are ignored. Galilei, *Dialogue*, 147-8.

reveal the real explanatory difference between linear and circular inertia, which Drake and others likewise missed.

5.6 THE ARCHIMEDEAN APPROXIMATION

While the use of two representations of space generates the appearance of inconsistency when it comes to his explanatory principles, Galileo himself does not think the two spatial concepts are incompatible. In the small-scale, rectilinear space is to be used, just as spherical space is to be used at larger scales. As for middle-sized phenomena, the two representations are meant to "fit" together such that the differences in description, explanation, and prediction arising from the different conceptual frameworks are "insensible" and therefore negligible.

In fact, Galileo gives an explicit approximation by which one can translate the representation of a phenomenon from one spatial framework to the other. The "Archimedean" approximation handles the transition from one spatial frame to the other, as explained in the *De Motu*:

> Now I am not unaware that someone at this point may object that for the purpose of these proofs I am assuming as true the proposition that weights suspended from a balance make right angles with the balance – a proposition that is false, since the weights, directed as they are to the center [of the universe], are convergent. To such objectors I would answer that I cover myself with the protecting wings of the superhuman Archimedes, whose name I never mention without a feeling of awe. For he made this same assumption in his *Quadrature of the Parabola*…[55]

Or in the *Discourse*:

> I ask you not to begrudge our Author that which other eminent men have assumed even if not strictly true. The authority of Archimedes alone will satisfy everybody. In his Mechanics and in his first quadrature of the parabola he takes for granted that the beam of a balance or steelyard is a straight line, every point of

[55] "Hic autem non me praeterit, posse aliquem obiicere, me ad has demonstrationes tanquam verum id supponere quod falsum est: nempe, suspense pondera ex lance, sum lance angulos rectos continere; cum tamen pondera ad centrum tendentia concurrerent. His responderem, me sub suprahumani Archimedis (quem nunquam absque admiratione nomino) alis memet protegere. Ipse enim hoc idem in sua Parabolae quadratura supposuit;" Galilei, "De Motu," 67; Galilei. *Opere*, I 300.

> which is equidistant from the common center of all heavy bodies, and that the cords by which heavy bodies are suspended are parallel to each other.[56]

An authority no less than the "superhuman Archimedes" legitimates Galileo's use of dual spatial concepts. The direction in which heavy bodies tend – i.e., "down" – can be represented by a "convergent" direction in spherical space, or by a "parallel" one in rectilinear space. In either case, the one representation approximates the other. Similarly, the "horizontal plane, which slopes neither up nor down" can be "represented by a straight line as if each point on this line were equally distant from the center" or as a curved surface concentric with the spatial center.[57] As one shifts from one representation to another, one can simply substitute one signification of "down" or "horizontal" for the other. Galileo assumes that any difference in the "proofs," descriptions, and explanations resulting from different spatial concepts will be negligibly small. Thus, both the large-scale spherical representation and the small-scale rectilinear representation of space are legitimate conceptual bases for the description and explanation of phenomena.

Of course, Galileo acknowledges, the Archimedean approximation is only valid under certain conditions:

> Some consider this assumption permissible because, in practice, our instruments and the distances involved are so small in comparison with the enormous distance from the center of the earth that we may consider a minute of arc on a great circle as a straight line, and may regard the perpendiculars let fall from its two extremities as parallel.[58]

The linear representation must "fit" within the larger spherical space. That is, the dimensions of the bit of space to be represented must be small compared to its distance to the center of the spherical frame (in this case, the earth's center) so that the curvature of the space's spherical representation is negligible. Put more simply, rectilinear representations are only applicable to

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[56] "...ma ben, all'incontro, domando che elle non contendano al nostro Autor medesimo quello che altri grandissimi uomini hanno supposto, ancor che falso. E la sola autorità d'Archimede può quietare ogn'uno, il quale nelle sue Mecaniche e nella prima Quadratura della parabola, piglia come pricipio vero, l'ago della bilancia o stadera essere una linea retta in ogni suo punto equalmente distante dal centro commune de i gravi, e le corde alle quali sono appesi i gravi esser tra di loro parallele..." Galilei, *Discourse*, 251; Galilei. *Opere*, VIII 274.

[57] "...noi supponghiamo che il piano orizontale, il quale non sia nè acclive nè declive, sia una linea retta, quasi che una simil linea sia in tutte le sue parti equalmente distante dal centro..." Galilei, *Discourse*, 250; Galilei. *Opere*, VIII 274.

[58] "...la qual licenza viene da alcuni scusata, perchè nelle nostre pratiche gli strumenti nostri e le distanze le quali vengono da noi adoperate, son così piccole in comparazione della nostra gran lontananza dal centro del globo terrestre, che ben possiamo prendere un minuto di un grado del cerchio massimo come se fusse una linea retta, e due perpendicoli che da i suoi estremi pendessero, come se fussero paralleli." Galilei, *Discourse*, 251; Galilei. *Opere*, VIII 274-75.

small-scale phenomena relatively distant from the presupposed center. Also, the vertical direction of the rectilinear representation must roughly coincide with the vertical of the spherical. That is, the linear vertical must be directed to the spherical center at some point in the space, as we have seen. If these conditions are met, Galileo assumes, the approximation will hold and his deductions will be valid in either conceptual frame. For example, he claims only "insensible changes" in the parabolic trajectory would result from switching from a rectilinear to spherical representation of projectile motion.[59]

The Archimedean approximation finally resolves the apparent tension between linearity and circularity we have been discussing. It does so in two respects. First, Galileo is not concerned with unobservable effects. His project in (the first three Days of) the *Dialogue*, for example, is to "show that all experiments practicable upon the earth are insufficient measures for proving its mobility, since they are indifferently adaptable to an earth in motion or at rest."[60] In other words, Galileo wants to demonstrate that no terrestrial observation can prove the motion or stability of the earth. (In the Fourth Day, Galileo attempts to use observations of the tides to this end.) Unobservable effects are neither here nor there – they neither help nor hinder Galileo's case. Galileo is perfectly aware that, in the case of the hunters and cannons, Salviati and Sagredo reach different conclusions about the outcome of the situation because of their differing representations of space. But because the two representations approximate one another,[61] Galileo assumes that the resulting differences will be negligible. Thus, the argument between Sagredo and Salviati, which seems like conceptual dissonance to a modern reader, is actually acceptable in Galileo's eyes, so long as the difference between Sagredo's prediction and Salviati's remains unobservable.[62] In the end, Galileo can dismiss their disagreement by

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[59] "...ben potranno solo insensibilimente alterar quella figura parabolica..." Galilei, *Discourse*, 252; Galilei. *Opere*, VIII 275. Galileo leaves these "insensible changes" unanalyzed. He argues that they would be easily overwhelmed by air resistance, anyway. On the other hand, he does say that his argument would fail were the projectile to fall to the center of the earth, where the Archimedean approximation breaks down. See Wallace, *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought*, 151ff.
[60] "...mostrare tutte l' esperienze fattibili nella Terra essere mezi insufficienti a concluder la sua mobilità, ma indifferentemente potersi adattare così alla Terra mobile, come anco quiescente;" Galilei, *Dialogue*, 6; Galilei. *Opere*, VII 30.
[61] The phenomenon in question – cannon fire on the surface of the earth – occupies a small space far enough from the center of the earth to satisfy, in Galileo's view, the conditions of the Archimedean approximation.
[62] For more on the rhetorical role of experiment in Galileo, see Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution*.

pointing out that the westward deflection caused by the rotation of the earth, as predicted by Sagredo, "is insensible because of the small distance from the cannon to the mark."[63]

In the second place, Galileo's discussions of the Archimedean approximation leave no doubt as to which spatial concept he considers the "real" representation of space and which the mere approximation. Even though they are legitimate presuppositions from which valid arguments can be deduced, the propositions underlying a rectilinear representation of space are "false." Rectilinear concepts do not correspond to the actual state of affairs. "Down" is not self-parallel, but convergent. The "horizontal" really is a spherical surface. While it is permissible to use a rectilinear frame to represent small-scale spaces, this is only an approximation of the spherical reality. Galileo is willing to assume a rectilinear conception of space for the sake of argument. In reality, though, Galileo thinks space is spherical. It follows from this that, though linear inertia can legitimately be used to predict phenomena, it is just the consequence of a small-scale, approximate spatial framework. Galileo does not deny that Sagredo's prediction is valid, yet the conclusion has been drawn from a false premise. Globally, Galilean inertia is circular.

5.7 CONCLUSION

For Alexandre Koyré, the distinguishing characteristic of early modern science is the principle of linear inertia. The development of this single notion marked the passing of ancient and medieval physics and the advent of the new scientific order. To accept it meant the rejection of the "essential traits" of medieval science, teleological metaphysics and the evidence of "common sense."[64] Acceptance of linear inertia also required, said Koyré, the "geometrization of space" – the adoption of a rectilinear, "Euclidean" representation of space.[65] It is this last move that Koyré denies Galileo, who Koyré claims always suffered from *l'hantise du sphérique et du*

[63] Galilei, *Dialogue*, 179. In fact, Galileo even *calculates* the effect of the earth's rotation on the range of a cannonball fired along the parallel, i.e., east and west, and finds that the westward shot would fall about one inch shorter than an eastward one. He finds that the effect is insignificant, not because it does not occur – he insists that "each one of these variations [in the ranges] contains one of one inch caused by the motion of the earth [*cagianato dal moto della Terra*]" – but because to make the effect observable, one would have to find "a method of shooting with such precision [*tanto esatta*] at a mark that you never miss by a hairsbreadth." See Galilei, *Dialogue*, 182; Galilei. *Opere*, VII 208.
[64] Koyré, *Études Galiléennes*, 33.
[65] Ibid., 15.

circulaire, the haunting or obsession of the spherical and circular.[66] Thus, though Galileo did reject teleological explanation and "common sense" evidence, he did not formulate a principle of linear inertia. Hence, Koyré famously argues, Galileo was but the last of the medievals, not the first of the moderns.

Koyré's placement of Galileo amongst medieval philosophers is disputed, most notably by Stillman Drake. Drake contends that it is Koyré, not Galileo, who is "haunted" by circularity. He claims that the "essential core of the inertial concept" is "a body's indifference to the states of motion and rest, and its perpetual continuance in either state if undisturbed."[67] Galileo clearly had possession of this notion. Therefore, concludes Drake, Galileo was the first to formulate the principle of inertia, its circular nature notwithstanding. Koyré's insistence on *linear* inertia, meanwhile, is merely "a question of formal criteria, not one of significant fact." Thus, Koyré's claim that Galileo was unable to conceive of inertial movement in straight lines is just an "ill-founded conjecture concerning Galileo's mental processes," not an accurate characterization of the historically significant aspects of his science.[68]

Galileo's own work indicates that things are both better and worse for the theses of Koyré and Drake. In fact, Galileo comes closer to the principle of linear inertia than Koyré seems to allow. Galileo *does* accept and use a rectilinear representation of space for small-scale spaces. There are instances in which he applies a principle of linear inertia to small- and even medium-scale phenomena. His predictions of the inertial deflection of projectiles in the passages presented at the start of this paper are two examples. Nevertheless, Galileo remained deeply troubled by his fundamentally spherical representation of space – more troubled than Drake would like to admit. Indeed, the tension between linear and circular principles of inertia results in the equivocation we have witnessed in the same passages. The resolution of this tension reveals the true extent to which the "*hantise* of the spherical" affects his science.

Consideration of Galileo's representations of space finally dissolves Koyré's historical paradox:

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[66] Ibid., 187, 273.
[67] Drake. *Essays on Galileo and the History and Philosophy of Science*, 143.
[68] Ibid., 144.

> If, as we believe has been shown, Galileo did not formulate the principle of inertia, how is it that his successors and pupils could think they found it in his works?[69]

Our answer is Drake's answer – the premise is wrong. Galileo did accept a rectilinear representation of space and he did formulate the principle of linear inertia. It was present in his work. Nevertheless, Koyré is, in the end, right. Galileo's linear inertia is not a fundamental principle, but an approximation of one. It results from the rectilinear approximation of an ultimately spherical space. Galileo has not overcome the medieval conception of a spherical space. He still suffers from the *l'hantise du sphérique et du circulaire*. It remained for Galileo's "successors and pupils" – especially, as we will see, Descartes – to promote his approximation to the foundation and, at least in Koyré's eyes, become modern.

[69] Koyré, *Études Galiléennes*, 292. Translation is Drake's. See Drake, *Galileo Studies*, 265.

6.0 PROMOTION TO THE FOUNDATIONS: CARTESIAN SPACE

In the previous chapter, we argued that Galileo's representation of space was fundamentally spherical. His fleeting intimations of a linear space were meant as small-scale approximations of a spherical cosmic space. Galileo's linear space merely "fit" into the larger spherical scheme. As a result, Galileo's ultimate explanations of physical phenomena appealed to a principle of circular inertia predicated on his spherical conceptual framework. As we will discuss in the present chapter, the situation for René Descartes is almost precisely reversed. Descartes supposes that cosmic space is spherical, containing countless vortices whirling about centers, but this conception of space is "built up" out of local, rectilinear spaces. Thus, the local does not approximate the global; the global extrapolates from the local. Descartes brings linearity to the fore and makes his world fundamentally linear. His explanations of phenomena ultimately appeal to a linear principle of motion and to the rectilinear representation of space it presupposes.

One concern of this chapter is to explain why Descartes adopts his representation of space. This is not an easy task. Descartes, unlike some of the authors we have discussed, seems to have "gone linear" from the start. Even in his earliest writings, he has already adopted a linear conception of space. There is no apparent shift to linearity (as in Kepler) or a flirtation with it (as in Galileo) to be found in his work. Nor is there much textual evidence that might indicate why Descartes chooses to assume lines rather than centers. He simply does so. We are forced to speculate as to why. In what follows, we shall suggest that Descartes' linearity is a result of his early work on optics and geometry. In optics, the behavior of light recommended a rectilinear representation of phenomena, while Descartes' geometric method required the presupposition of lines. In other words, Descartes' representation of space is suggested by the subjects he studied at the beginning of his philosophical career.

It is easier to trace the development of Descartes' conception of space once he began using a rectilinear framework. As we will show, rectilinearity becomes an increasingly important and fundamental aspect of Descartes' thinking about the natural world as a result of his attempts to systematize and unify his philosophical system. For instance, a rectilinear concept of space becomes the basis for his fundamental physical principles, including linear inertia. This conceptual development is particularly evident in Descartes' doctrine of place and motion. Rather than abandon the rectilinear context, Descartes defends it against a difficulty it raises in relation to the conception of motion he wants to hold.

6.1 DESCARTES' CONCEPTUAL FRAMEWORK

In his *Rules for the Direction of the Mind*, written before 1628,[1] Descartes develops a somewhat intricate account of sensory experience. According to this theory, physical phenomena are intuited by the human mind as a result of two mediations. First, external objects bring about motions in the body's sensory organs and nerves. These motions are collected by the "corporeal imagination" in the brain, which forms a bodily, corporeal representation or image of the sensory environment.[2] Second, this imaginary representation comes under the consideration of the intellect – the mental "power by which we know things."[3] The thoughts and ideas generated in the mind by this process of consideration constitute the experience of the physical phenomena.

Neither the bodily representation nor the mental consideration proceed by direct representation or resemblance. In the second case, in fact, there can be no resemblance between the imaginary representation and the corresponding thoughts and ideas brought about by the intellect.[4] The intellect, Descartes argues, is a "purely spiritual" faculty,[5] and "nothing quite like

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[1] For this date, see René Descartes, *The Philosophical Writings of Descartes*, trans. John Cottingham, Robert Stoothoff, and Dugald Murdoch, 3 vols. (Cambridge: Cambridge University Press, 1985), I 7.
[2] Ibid., I 42.
[3] Ibid., I 42.
[4] In the process of bodily representation, Descartes argues that there is a correspondence, but not a resemblance, between the motions of material bodies and the motions of the corporeal imagination that represent them, just as the motion of the non-writing end of a pen corresponds to the motion of the tip, but does not resemble it. Ibid., I 45, 47. This preliminary, corporeal representation of physical phenomena does not greatly concern us. Hence, in what follows, we will tend to identify the imaginary representation with the body itself. It should be kept in mind, however, that this process of representation *can* have a significant effect on experience as a whole, as when the sensory organs are diseased, for example.
[5] Ibid., I 42.

this [spiritual] power is to be found in corporeal things"[6] such as the corporeal imagination. The intellect, therefore, cannot transfer the bodily image of a phenomenon into the mind by some kind of mimesis or resembling representation. The mind cannot simply "see" the imaginary representation in the body.

Instead, Descartes explains, the intellect intuits sensed physical phenomena by "measuring" their imaginary representations in certain respects, depending on the features of the sensory objects under consideration:

> Thus, when the problem concerns number, we imagine some subject which is measurable in terms of a set of units. The intellect of course may for the moment confine its attention to this set; nevertheless we must see to it that, in doing so, it does not draw a conclusion which implies that the thing numbered has been excluded from our conception.[7]

If, for instance, the mind is concerned with some question of number, it will direct itself to a measurement of the relevant units contained in the imaginary representation. Suppose the mind wishes to count the sides of an observed cube. It will measure the number of units – sides – represented in the corporeal imagination. The intellect, meanwhile, can ignore (i.e., leave unmeasured) other observed features of the cube, such as its color, even though those features are represented in the imagination, since they are part of the sensory environment.[8]

Descartes calls the various aspects of physical phenomena an intellect can measure (via the imaginary representation) the "dimensions":

> By 'dimension' we mean simply a mode or aspect in respect of which some subject is considered to be measurable.[9]

In other words, the intellect intuits sensed phenomena by measuring their dimensions. The resulting mental experience is constituted in terms of the dimensions. An idea of a physical phenomena consists of a conjunction of measured magnitudes of dimensions.

Of course, Descartes continues, the number of ways the intellect can consider an object is "countless":

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[6] Ibid.
[7] Ibid., I 61.
[8] Descartes' point in this passage is that the intellect's ability to focus on one aspect of a body does not imply that the resulting "measurement" has an existence independent of the object measured. Thus, for instance, the mind can concern itself solely with number, but this does not imply that number exists independently of the things numbered.
[9] Descartes, *The Philosophical Writings of Descartes*, I 62.

> Thus length, breadth, and depth are not the only dimensions of a body: weight too is a dimension – the dimension in terms of which objects are weighed. Speed is a dimension – the dimension of motion; and there are countless instances of this sort.[10]

The intellect can consider an object in any number of ways. A red bouncing ball, presented to the intellect via the senses and corporeal imagination, can be considered with respect to its extension, its weight, its speed, and so on. Together or in part, these measurements constitute the intellect's conception of the ball – i.e., the mental experience of the ball.[11]

Note that the dimensions with respect to which the intellect considers objects are features of the mental faculty apart from the corporeal imagination or physical objects themselves. The dimensions are antecedently understood by the intellect and brought to bear on imaginary representations. They are not perceived or discovered in the representation. Descartes *does* allow that some dimensions are "real." That is, they correspond to some "real basis" in the physical bodies themselves.[12] Yet the "real" dimensions are no different in existence or function from dimensions that are "arbitrary inventions of our mind":

> It is clear from this that there can be countless different dimensions within the same subject, that these add absolutely nothing to the things which possess them, and that they are understood in the same way whether they have a real basis in the objects themselves or are arbitrary inventions of our mind. The weight of a body is something real; so too is the speed of a motion, or the division of a century into years and days; but the division of the day into hours and minutes is not. Yet these all function in the same way from the point of view simply of dimension, which is how they ought to be viewed here and in the mathematical disciplines. Whether dimensions have a real basis is something for the physicists to consider.[13]

On the one hand, measuring a century as a number of years and days is "real," since such units correspond to actual features of the earth's annual orbit and daily rotation. On the other hand, counting a day as so many minutes and hours is an "arbitrary" measure, since these units do not correspond with distinct features of the physical phenomena. These units are merely imposed by

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[10] Ibid.
[11] Though, again, Descartes stresses that a lack of experience (that is, "measurement") does not imply a lack of existence. The ball has a weight even if the intellect does not actually concern itself with it. Moreover, "weight" is a "dimension." The weight of the ball is "a mode or aspect... considered to be measurable." The intellect *could* consider the ball's weight even if it does not.
[12] Given the distinction between corporeal and "spiritual" entities, however, it is hard to see how this correspondence might be established.
[13] Descartes, *The Philosophical Writings of Descartes*, I 62-63.

the intellect. The point here, though, is that the physical difference between "real" and "arbitrary" dimensions does not correspond with any distinction in their use by the intellect. "Real" and "arbitrary" dimensions have the same intellectual function and existence. The mind simply measures dimensions, "real" or not. This implies that dimensions are grounded in the mind rather than in physical bodies. Any correspondence of a mental dimension with the distinct features of physical phenomena is accidental.[14]

Also in this vein, Descartes claims that dimensions can be considered in relation to different physical phenomena or objects:

> Indeed, it is by means of one and the same idea that we recognize in different subjects each of these familiar entities, such as extension, shape, motion and the like (which we need not enumerate here). The question whether a crown is made of silver or of gold makes no difference to the way we imagine its shape. This common idea is carried over from one subject to the other solely by means of a simple comparison, which enables us to state that the thing we are seeking is in this or that respect similar to, or identical with, or equal to, some given thing.[15]

The dimension, shape, is "one and the same," when it is measured in otherwise distinguishable objects. A singular, unitary dimension is in use whenever the intellect measures the shape of an object. Again, this implies that shape, and the other dimensions, have a mental existence apart and prior to the objects they are used to measure. The intellect, of its own accord, brings dimensions to the process of intuition and generates the experience of physical phenomena with respect to them.

Notice, then, that Descartes' dimensions play the role of what we have called "concepts." Dimensions, like concepts, are antecedently understood mental entities that make phenomena intelligible. Dimensions determine how physical objects are intuited by the intellect. For instance, the dimension "weight" warrants the intellect's measurement and, thus, the experience of a body's weight. Reciprocally, the dimension dictates what such a measurement *means* – which feature of the body (as represented in the imagination) is picked out by the intellect's intuition of "weight." In the same way, the mental *concept* "weight" allows the intelligible

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[14] At least at this stage of Descartes' argument. Later, Descartes will argue that "real" dimensions such as length, breadth, depth, and motion are epistemically more fundamental than "arbitrary" dimensions because they are essential properties of physical bodies.
[15] Descartes, *The Philosophical Writings of Descartes*, I 57.

description of weight and dictates what such a description means. Dimensions, like concepts, are the forms of intuition by which we make physical phenomena understood.[16]

6.2 DESCARTES' REPRESENTATION OF SPACE

Having analogized Descartes' dimensions with concepts, we can set about describing his representation of space by seeking the set of dimensions he uses to intuit spatial properties and relations. As we have glimpsed above, though, Descartes argues that the intuition of spatial or bodily extension[17] always entails the intuition of three dimensions: length, breadth, and depth:

> By 'extension' we mean whatever has length, breadth and depth... This notion does not, I think, need any further elucidation, for there is nothing more easily perceived by our imagination.[18]

Extension simply *means* extension in length, breadth, and depth.[19] Whenever we seek to intuit a spatial extension, we do so by measuring its extension in three directions. That is, length, breadth, and depth are dimensions by which the intellect comes to experience spatial extension. Thus, whatever else can be said about Descartes' representation of space, it certainly includes the dimensions length, breadth, and depth. There may be other concepts involved, and we are free, of course, to ignore any or all of these dimensions, as when we only consider a body's surface. However, length, breadth, and depth are always at least available to the intellect. Whenever we encounter a physical object or phenomenon, we are always capable of "measuring" extension in length, breadth, and depth. The directions are always part of the conceptual framework we

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[16] Our original elucidation of "concept" supposed that the intellect could intuit phenomena without being able to intelligibly express such experiences. That is, there could be a mental representation of a physical phenomenon prior to its description. In our view, description would require a further mental operation – conceptualization – beyond bare experience. Descartes' view differs only in that he seems to preclude this possibility. For Descartes, conceptualization and mental experience occur as one. Without measurement of dimensions, there is no experience.

[17] Descartes argues that the intuition of a body is inseparable from the intuition of extension: "We do not form two distinct ideas in our imagination, one of extension, the other of body, but just the single idea of extended body." "Extension" and "body" both refer to the same feature, or "idea," in imaginary representations. Hence, the concept of extension and the concept of body is one and the same. Whatever dimensions used to measure the one will be the same as those used to measure the other. Descartes, *The Philosophical Writings of Descartes*, I 60.

[18] Ibid., I 59.

[19] Here, Descartes is taking over a definition of extension originally employed by Euclid (Definition I of Book XI of the *Elements*). See Jammer, *Concepts of Space*, 56, 175.

employ to experience the physical world. The intellect *already* knows how to apply these concepts in order to generate a measurement. The directions are presupposed. They are prior to experience, and the basis of it.

It is clear from Descartes' discussion, moreover, that length, breadth, and depth are conceived linearly, in relation to straight lines. In Descartes' terms, extension, and therefore length, breadth, and depth, are "simple" concepts – they can be directly and immediately known by the intellect:

> That is why, since we are concerned here with things only in so far as they are perceived by the intellect, we term 'simple' only those things which we know so clearly and distinctly that they cannot be divided by the mind into others which are more distinctly known. Shape, extension and motion, etc. are of this sort; all the rest we conceive to be in a sense composed out of these.[20]

Extension – meaning a body's extension in length, breadth, and depth – can be perceived by the intellect "distinctly" and immediately. These are primitive concepts, out of which others can be composed, but themselves impossible to analyze further. Hence, they require no preliminary act of measurement or conceptualization before they are applied. The intellect does not need to "know" anything else about a body before measuring its extension in length, breadth, and depth.

The simplicity of length, breadth, and depth implies that they are rectilinear directions. In the *Rules*, Descartes draws a distinction between "absolute" and "relative" concepts. He then classifies "straight" as an "absolute" concept:

> I call 'absolute' whatever has within it the pure and simple nature in question; that is, whatever is viewed as being independent, a cause, simple, universal, single, equal, similar, straight, and other qualities of that sort.[21]

"Straight" is a "pure and simple" nature. It can be recognized by the intellect directly. Like other "simple" concepts, it does not require a composition of underlying intellectual "measurements" along more fundamental "dimensions" in order to be recognized in an object. By contrast, "oblique" – i.e., curvilinear – is a "relative" concept. It can only be "deduced" by a series of inferences from absolute, simple concepts.[22] That is, the intellect can only come to know curvilinearity in an object by relating the already intuited knowledge of other, more

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[20] Descartes, *The Philosophical Writings of Descartes*, I 44.
[21] Ibid., I 21.
[22] Ibid.

fundamental concepts. (We shall see below why, according to Descartes, curvilinearity requires reference to other concepts.)

For Descartes, therefore, it would be contradictory if length, breadth, and depth were conceived as curvilinear directions. Length, breadth, and depth are "simple." They can be intuited and known by the intellect directly, without reference to other concepts. If they were curvilinear, however, they could only be known by inference from more fundamental intuitions based on more fundamental concepts. They would not be "simple." Hence, the directions must be straight – i.e., rectilinear – since they are immediately known.

Descartes' account also implies that the recognition of length, breadth, and depth does not require the supposition of privileged locations or centers. Again, these are simple concepts. The intellect always already knows how to apply them to bodies. If length, say, were directed to or away from a privileged location, on the other hand, the "measurement" of length would require a prior recognition of the privileged location or center by the intellect. That is, the intellect would *not* "know" how to apply the concept of length. It would have to "find" the center first. The "measurement" could then proceed as a composition of this preliminary intuition with others. In this case, the intellect's intuition of length would not be "simple," direct, and immediate, and similarly for breadth and depth, contrary to Descartes' assertions. Thus, Descartes' representation of space comprises presupposed rectilinear directions – length, breadth, and depth – independent of privileged locations or centers.[23]

Note, finally, that the orientation of Descartes' rectilinear representation of space is a matter of arbitrary choice, rather than something corresponding to features of physical bodies themselves. Descartes stresses that length, breadth, and depth are concepts applied by the intellect to imaginary representations of physical bodies. There is nothing in bodies or their representations that dictates which "mode or aspect" is to be "measured" as its length, etc.:

> We should note incidentally that there is merely a nominal difference between the three dimensions of body – length, breadth and depth; for in any given solid it is quite immaterial which aspect of its extension we take as its length, which as its breadth, etc. Although these three dimensions have a real basis at any rate in every extended thing simply *qua* extended, we are no more concerned with them

[23] Nancy L. Maull, "Cartesian Optics and the Geometrization of Nature," in *Descartes: Philosophy, Mathematics and Physics*, ed. Stephen Gaukroger (Sussex: Harvester Press, 1980), esp. 262-63.

> here than with countless others which are either intellectual fictions or have some other basis in things.[24]

Physical bodies are extended things. It is always possible to "measure" their length, breadth, and depth – simply because they are extended. Nothing, however, necessitates that a particular aspect – i.e., a certain direction in the body – is to be conceived as its length, breadth, or depth. There is no feature of a body that the intellect must consider as a particular spatial dimension. On the contrary, the aspect of the body measured as its length, breadth, or depth is a matter of arbitrary intellectual choice. In other words, the orientation of the rectilinear space is something chosen by the observing and intuiting subject, rather than something discovered in the physical phenomena. We shall see below how the arbitrary orientation distinguishes the Cartesian representation of space from those of his predecessors.

6.3 THE ORIGINS OF CARTESIAN SPACE

If we seek the source of Descartes' rectilinear representation of space, we must examine his work prior to the *Rules*. Descartes' intellectual career did not begin in earnest until he met Isaac Beeckman in Holland over the late fall and winter of 1618-19. The two men apparently discussed several topics, including music, hydrostatics, and falling bodies. The primary subjects of Descartes' researches, however, seem to have been optics and geometry,[25] and examples from these two disciplines appear in the *Rules*. (Specifically, Descartes discusses the optical problem of the anaclastic curve to illustrate Rule Eight and he applies Rules Fourteen through Eighteen to the case of geometrical reasoning.) Hence, it is reasonable to assume that Descartes' representation of space was, at least in large part, inspired by his work in optics and geometry.

It is somewhat difficult to reconstruct the state of Descartes' thinking about geometry and optics in the period before to the *Rules*. His mature treatises, the *Optics* and *Geometry* were not

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[24] Descartes, *The Philosophical Writings of Descartes*, I 63.
[25] Perhaps harmonics should also be included among Descartes' foremost interests during this period. However, unlike in optics and geometry, this early interest does not persist.

published until 1637.[26] However, it is clear that some, if not most, of the results reported in those tracts were worked out much earlier. Indeed, as noted, the *Rules* mentions the anaclastic curve, which is the primary concern of the *Optics*.[27] Also, the latter part of the extant *Rules* (Rules Fourteen to Twenty-one) provide a near complete summary of the analytic methods found in the *Geometry*, including solving for unknowns using equations (Rule Nineteen), algebraic notation (Rule Sixteen), and representing mathematical operations as manipulations of line segments (Rule Eighteen). Moreover, Descartes' *Cogitationes Privatae*,[28] which date from 1619-21, include a description of the mechanical means of constructing conic sections found in the Tenth Discourse of the *Optics* and of the use of a "circinus" to construct curves, as in the *Geometry*. Thus, it is quite safe to assume that, by the time of the *Rules*, Descartes already possessed the rudiments, if not much more, of what would later appear in the *Optics* and *Geometry*.

6.3.1 Optics

The study of optics in general would not have predisposed Descartes toward the adoption of a rectilinear representation of space. In Descartes' time, light was considered from two points of view.[29] On the one hand, it was thought of as a spherical phenomenon. Light spreads out in all directions from a central source, so that the projection of a luminous point forms a sphere around it. Johannes Kepler, for example, says that all luminous things (those that "share in light") "imitate the sun," which occupies the "middle place... and the center" and "pour[s] itself forth

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[26] See René Descartes, *Discourse on Method, Optics, Geometry, and Geometry*, trans. Paul J. Olscamp (Indianapolis: Hackett, 2001); René Descartes, *The Geometry of René Descartes*, trans. David Eugene Smith and Marcia L. Latham (New York: Dover Publications, 1954).

[27] The *Rules* also seems to adumbrate the sine law of refraction reported in the *Optics*, which Descartes knew by 1626, or so. In fact, I would claim that Descartes' success with the anaclastic led him to attempt a systematization of his method in order to apply it to other lines of inquiry, which is the project of the *Rules*. In other words, the work reported in the *Optics* and *Geometry* directly inspired the *Rules*. Whether this is actually the case does not materially affect the present discussion, though. See Descartes, *The Philosophical Writings of Descartes*, I 29; A. I. Sabra, *Theories of Light from Descartes to Newton* (Cambridge: Cambridge University Press, 1981), 103ff.

[28] René Descartes. *Oeuvres de Descartes*, Charles Adam and Paul Tannery, eds., 12 vols. (Paris: 1897-1913), X 213-56.

[29] This dual consideration of light follows a long tradition. See Dijksterhuis, *The Mechanization of the World Picture*, 150; David C. Lindberg, "The Genesis of Kepler's Theory of Light: Light Metaphysics from Plotinus to Kepler," *Osiris* 2 (1986).

equably into the whole orb."[30] On this view, light is described as a series of spheres expanding from the luminous source at the center of the projected "orb." Naturally, this account suggest a spherical representation of space centered on the source of light.

On the other hand, it was universally acknowledged in the seventeenth century that the individual parts of light or "rays" propagate linearly, unless they encounter refractive or reflective surfaces. For instance, after noting the spherical shape of the luminous projection, Kepler goes on to say that "nature of light is to move in straight lines, as long as it is not at all affected by the interposition of surfaces."[31] Light is here thought of as a collection of straight lines propagating from a source. The infinitude of rays spreading from the source in all directions constitutes the orb of light. Considered individually, though, the behavior of the rays indicates a linear framework. Hence, the behavior of light could suggest both spherical and linear representations of space, depending on whether light was considered as single spherical projections or as a multitude of linear ones.[32]

Descartes' work on optics followed the prevailing theories of the medieval authors Alhazen and Witelo, perhaps as handed down by Kepler himself.[33] His focus, however, was not on optics in general, but on problems in catoptrics and dioptrics – the behavior of reflected and refracted light. This brought his attention to bear mainly on individual rays, which, it was assumed, acted along straight lines:

> And in the same way considering that it is not so much the movement as the action of luminous bodies that must be taken for their light, you must judge that the rays of this light are nothing else but the lines along which the action tends. So that there is an infinity of such rays which come from all points of luminous bodies, toward all points of those that they illuminate, in such a manner that you can imagine an infinity of straight lines... Moreover, these rays should always be

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[30] Kepler, *Optics*, 19-20. Descartes called Kepler his "first teacher in optics." Descartes. *Oeuvres de Descartes*, II 86.
[31] Kepler, *Optics*, 34.
[32] As we have seen, Kepler stresses the former view in his astronomical work. He compares the *anima motrix* that emanates from the sun and moves the planets around their orbits to the action of light. Both propagate spherically from the central body, i.e., the sun, and diminish in intensity as distance increases. Moreover, in both cases the purported forces or actions are described in relation to a spherical framework. A center is presupposed – the sun – as is the indistinguishability of locations at equal distances from the center. The sphericity of the *anima motrix* explains its ability to move the planets circularly around the sun.
[33] Neil M. Ribe, "Cartesian Optics and the Mastery of Nature," *Isis* 88, no. 1 (1997): 45; Sabra, *Theories of Light from Descartes to Newton*, 72n13.

> imagined to be exactly straight, when they go through only one transparent body which is uniform throughout...[34]

For Descartes, as for his predecessors, the action of light spreads out spherically from all points of a luminous source.[35] The individual parts of that action, the "rays," however, follow straight lines. Descartes enjoins his reader to identify a ray of light with the line along which it acts. This, in turn, legitimates the depiction of a ray as a line. Descartes can simply stipulate that a line, perhaps drawn on a page, is a ray and that geometric manipulations of lines represent the actual behaviors of rays of lights. On this basis, the remainder of the *Optics* goes on to use geometric arguments to describe optical phenomena.[36]

This fundamental linearity of light suggests representing optical phenomena using a rectilinear conception of space. Consider, for example, Descartes' analysis of reflection:

> Let us consider then that a ball, being impelled from *A* toward *B*, meets at point *B* the surface of the ground *CBE* which, preventing it from going further, causes it to turn away; and let us see in what direction... Moreover, it must be noted that the determination to move toward a certain direction, as well as movement and any other sort of quantity generally, can be divided among all the parts of which we can imagine that it is composed; and we can easily imagine that that determination of the ball to move from *A* to *B* is composed of two others, one of which causes it to descend from the line *AF* toward the line *CE* and the other at the same time makes it go from the left *AC* toward the right *FE*; so that the two, joined together, conduct it to *B* along the straight line *AB*.[37]

Here, the reflection of a ray of light is compared to the motion of a tennis ball rebounding from the ground. The incident ray is represented by the line AB. Its direction or "determination," moreover, is described in relation to this line – i.e., along AB, from A to B. There is no central point by which the direction is specified. Note also that Descartes argues the direction AB can be decomposed into other directions. These directions are specified in relation to presupposed lines (from AF to CE and from AC to FE – that is, along the perpendicular between the two parallels), which are themselves simply "imagined." The directions must also be self-parallel since their direction toward CE and FE results in the straight motion from A to B. If the two

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[34] Descartes, *Discourse*, 70.
[35] Descartes departs from his forbears, however, by saying that light is not a motion *per se*, but a *tendency* to move. This tendency, something like a pressure, spreads out "in all directions" from a central source, and can act on the optical sense when it comes into contact with it.
[36] Shea argues that this "diagrammatic" form of argument is fundamental to all of Cartesian science. William R. Shea, *The Magic of Numbers and Motion* (Canton, MA: Science History Publications, 1991).
[37] Descartes, *Discourse*, 75-6.

directions were not parallel to themselves throughout the space, their composition at each point of the ray would result in deflected, curvilinear motion. Descartes represents the action of a ray of light using a rectilinear representation of space. He assumes linear, self-parallel directions without referring to centers.[38]

Notice also that the representation of space, in this case, has an arbitrary orientation. The decomposition of the direction of the light ray is a matter of subjective choice. The privileged directions in the space are merely "imaginary," with no basis in the phenomenon. In the given example, Descartes chooses the vertical and horizontal directions because they happen to correspond to the tangential and normal components of the (planar) reflective surface. This choice is convenient, since only the normal component is affected by reflection. This decomposition, though, is chosen only for convenience. There is nothing about the physical situation that dictates this choice. The very possibility of decomposing the motion this way, in fact, allows for decompositions in other directions. As Descartes writes, we are entitled to any decomposition "we can imagine." Already in the *Optics*, we see glimpses of the arbitrarily oriented rectilinear space evident in the *Rules*.

6.3.2 Geometry

Descartes' early work in geometry also suggested the priority of lines, if not a rectilinear space, *per se*. As he explained to Beeckman in 1619, Descartes sought "a completely new science by which all questions in general may be solved that can be proposed about any kind of quantity, continuous as well as discrete."[39] That is, Descartes aimed at a geometrical method that could solve any quantitative problem whatsoever. He acknowledged that this task was "incredibly ambitious," but claimed that he had "through the dark confusion... seen some kind of light."[40]

The "light" he had seen, Descartes continued, was the use of his "new compasses," which he described in the *Cogitationes Privatae* and the *Geometry*. These compasses could be used to find any number of mean proportionals between given quantities expressed as line segments or,

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[38] Maull, "Cartesian Optics and the Geometrization of Nature."; Ribe, "Cartesian Optics and the Mastery of Nature."
[39] Henk Bos, *Redefining Geometrical Exactness: Descartes' Transformation of the Early Modern Concept of Construction* (New York: Springer, 2001), 232; Descartes. *Oeuvres de Descartes*, X 156-7.
[40] Bos, *Redefining Geometrical Exactness: Descartes' Transformation of the Early Modern Concept of Construction*, 232; Descartes. *Oeuvres de Descartes*, X 157-8.

alternatively. It was well-known that the discovery of mean proportionals was the key to several mathematical problems, such as doubling the cube. In fact, in 1593 François Viète had shown that all "solid," third-degree problems in geometry[41] could be solved by the construction of two mean proportionals or the trisection of an angle. Descartes expected (not unreasonably, though he had no proof) that problems of higher degrees, which included nearly all existing unresolved mathematical puzzles, could be solved by the construction of additional mean proportionals. If the any third-degree problem could be solved by constructing two mean proportionals, then perhaps all fourth-degree problems could be solved by the construction of three mean proportionals, and so on. The compasses could be used to construct these additional mean proportionals. In other words, Descartes' compasses presented the possibility of solving most, if not all, existing mathematical puzzles. They held out the promise of a "completely new science."[42]

The compasses operated by constructing curves that satisfied the proportionalities sought in the problem under investigation. They did so by ensuring that the position of each linear part of the compass retained the proper relation to the other parts as the compass was manipulated. The necessary curve was then generated by the intersection of two of these linear parts. In other words, the curve was constructed by the motion of a point along a presupposed line. The line itself, meanwhile, moved along a second assumed line with a motion commensurate with that of the point along the first. (The mechanical connections of the compass ensured the commensurability of these motions.) As Descartes put it, the curve was generated by a "single, continuous motion" of the various mechanically interconnected parts of the compass.[43]

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[41] Problems of the form $x^3+ax^2+b^2x+c^3=y^3$, where all the terms represent three-dimensional solids.

[42] See Bos, *Redefining Geometrical Exactness: Descartes' Transformation of the Early Modern Concept of Construction*, 243. It is not clear that Descartes was aware of the particulars of Viète's work in 1619. Gaukroger and Shea point to Descartes' use of Clavian notation in 1619 to argue that he had not read Viète, who used the modern notation Descartes later adopted in his *Géométrie*. This is not conclusive, nor does it imply that Descartes was *unaware* of Viète. In any case, both Gaukroger and Shea agree that Descartes was inspired to treat the "whole of knowledge" by his geometric successes. Stephen Gaukroger, *Descartes: An Intellectual Biography* (Oxford: Clarendon Press, 1995), 105ff; Shea, *The Magic of Numbers and Motion*, 44-45, 48.

[43] The compass could not generate "mechanical" curves, generated by two or more distinct and incommensurate motions. Problems requiring such curves could not be solved using Descartes' method. In his letter to Beeckman, Descartes called such curves "imaginary only" and he excluded them from the scope of the *Geometry*. For examples of mechanical and geometrical curves and the operation of Descartes' compass, see Henk Bos, "On the Representation of Curves in Descartes' *Géométrie*," *Archive for History of Exact Sciences* 24 (1981); Bos, *Redefining Geometrical Exactness: Descartes' Transformation of the Early Modern Concept of Construction*, 231ff; Mancosu, *Philosophy of Mathematics & Mathematical Practice in the Seventeenth Century*, ch. 3.

The point here is not to limn the details of Descartes' early work on geometry. It suffices to note that Descartes' geometry requires the presupposition of straight lines, in the form of his "new compasses," in order to construct curves. The compass is simply postulated prior to any particular problem or situation and then used to construct the necessary curve. In other words, curves are generated on the presupposition of straight lines. Descartes' geometry, like his optics, requires the assumption of straight lines in order to represent, in this case mathematical, problems. By the same token, it is in his geometry that we find the root of Descartes' assertion that curvilinearity is a "relative" notion requiring a relation between "absolute" straight lines. Curves are constructed from linear motions. The precedence of linearity in Descartes' geometrical method eventually filtered into the nascent philosophical method found in the *Rules*.

In summary, Descartes seems to have adopted a rectilinear representation of space by the time he wrote the *Rules* in the mid-1620s. While the origins of this spatial concept are somewhat unclear, it is not altogether surprising given Descartes' earlier work in optics and geometry. A rectilinear space is appropriate to Descartes' treatment of optical phenomena, and his geometrical reasoning reinforced this suggestion by holding lines to be more basic than curves. Meanwhile, it is possible the success of Descartes' derivation of the anaclastic curve on the basis of these investigations, as well as the promise of a "new science" of geometry, led Descartes to attempt an expansion of his methods into other fields of inquiry, which is precisely the "*mathesis universalis*" he proposes in the *Rules*.[44] This expansion, however, promoted the rectilinear spatial framework from a privileged place in optics and geometry to a privileged place in philosophy in general.

At this point, one might object that Descartes' rectilinear representation of space is a cause of his linear representation of optical phenomena and geometrical problems, not a result of it. We do not have a strong response to this objection. There is no way to be sure what the ultimate source of Descartes' linearity actually was. We only wish to argue that a rectilinear framework was implicit in the optical theories and geometrical method to which Descartes subscribed. Therefore, a rectilinear representation of space was at least associated with his early work on optics and geometry and was then developed into his broader philosophical program.

The important thing to note, though, is that Descartes' rectilinear representation of space, however it actually came about, did so in a practical and applied context. In his early work,

[44] Descartes, *The Philosophical Writings of Descartes*, I 19.

Descartes was trying to solve very specific descriptive and explanatory problems in narrow subject matters. His choice to privilege lines over points and spheres proved appropriate and/or convenient to the problems at hand. Indeed, the choice led to remarkable success: the discovery of the anaclastic curve and analytic geometry. The rectilinear representation of optical phenomena and geometric curves allowed fruitful descriptions and explanations. This initial descriptive and explanatory success led Descartes to adhere to a rectilinear framework in his later, broader work.[45]

6.4 PLACE AND MOTION IN THE *RULES*

Despite Descartes' acceptance of a rectilinear concept of space in the *Rules*, he was also willing to countenance privileged locations as part of his spatial framework. In some contexts, Descartes presupposes points in addition to lines in order to describe phenomena. In one passage, Descartes falls into the Aristotelian, spherical representation of the relative placement of the terrestrial elements:

> An example of composition by way of conjecture would be our surmising that above the air there is nothing but a very pure ether, much thinner than air, on the grounds that water, being further from the centre of the globe than earth, is a thinner substance than earth, and air, which rises to greater heights than water, is thinner still.[46]

In this case, locations as well as directions are described in relation to a presupposed center, which corresponds with the center of the terrestrial body. Above and height are understood as "being further from the centre." Locations are distinguished by their distance from the center. Thus, direction and location are conceived "relatively." They rely on the more basic assumption of a privileged center. While this passage only appears in passing as an example of faulty

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[45] For more on this process of expansion, see Massimo Galuzzi, "Il Problema delle Tangenti nella "Géométrie" di Descartes," *Archive for History of Exact Sciences* 22 (1980): 39-40; Gaukroger, *Descartes: An Intellectual Biography*, esp. ch. 4; Timothy Lenoir, "Descartes and the Geometrization of Thought: The Methodological Background of Descartes' Géométrie," *Historia Mathematica* 6 (1979); Maull, "Cartesian Optics and the Geometrization of Nature."; Ribe, "Cartesian Optics and the Mastery of Nature."
[46] Descartes, *The Philosophical Writings of Descartes*, I 47.

reasoning and should not be construed as a clear depiction of Descartes' concept of space, it does show he is capable of adopting privileged locations in order to describe spatial properties.

Descartes' description of "place" also implies, in an interesting way, the assumption of privileged locations. According to Descartes, the "place" of a body can only be specified in relation to a privileged reference frame:

> When, for example, they [philosophers] define place as 'the surface of the surrounding body', they are not really conceiving anything false, but are merely misusing the word 'place', which in its ordinary use denotes the simple and self-evident nature in virtue of which something is said to be here or there. This nature consists entirely in a certain relation between the thing said to be at the place and the parts of extended space.[47]

Here, "place" or location consists in a "relation" between the body[48] to be located and the "parts of extended space" – the frame of reference in which the body will be located. The determination of a frame of reference, though, requires the identification of at least one spatial "part" to be used as the origin of the frame – a "point of reference." The spatial part has to be stipulated by the intellect before it can locate a body in a place. Descartes requires presupposed privileged locations in order to describe "place."

Notice that this definition makes place a relative concept, at least in the *Rules*. In order to intuit a body's place, the intellect must first recognized a privileged "part" of space to fix the reference frame. Then, the body can be "said to be here or there" in virtue of its position relative to the privileged frame. The intellect, therefore, can only intuit a body's place by composing a prior intuition of the spatial frame with an intuition of the body's relation to that frame. Place is not directly and immediately intuited. It is not a simple concept.

This raises a problem with Descartes' view of motion. According to this position, motion should be a simple and directly intuited concept which is known "so clearly and distinctly that [it] cannot be divided by the mind into others which are more distinctly known."[49] Nevertheless, Descartes goes on to define motion as change of place:

> For example, can anyone fail to perceive all the respects in which change occurs when we change our place? And when told that 'place is the surface of the

[47] Ibid., I 53.
[48] The "place" in question here can refer either to the body itself or the space it occupies, since Descartes identifies these two concepts. In this context we will dispense with the distinction.
[49] Descartes, *The Philosophical Writings of Descartes*, I 44.

surrounding body', would anyone conceive of the matter in the same way? For the surface of the 'surrounding body' can change, even though I do not move or change my place; conversely, it may move along with me, so that, although it still surrounds me, I am no longer in the same place.[50]

The body moves if and only if it changes its place. If the body does not change place, it does not move, even if the surrounding bodies change. On the other hand, if the place changes, the body moves, even if the surrounding bodies remain the same.[51] Place, however, is a relative concept. Place is specified in relation to an assumed origin which must be known prior to the intuition of a body's place. The intellect, therefore, can only know that a body is in motion by first recognizing the privileged "parts of extended space" by which its place is known. Contrary to Descartes' contention, motion is not simple or directly intuited. It cannot be ascribed to a body in and of itself. It is only understood relative to a location taken to be fixed in virtue of which the body is "said to be here or there." We return this issue below.

Of course, assuming a privileged origin is not incompatible with the fundamentally rectilinear space Descartes describes in the *Rules*. In the context of the above definition of place, for example, direction still extends rectilinearly in the "extended space," which is still defined as extension in length, breadth, and depth. Nor does the passage imply the privileged origin is anything but an arbitrary feature of subjective representation. It need not correspond to any actual object, as in the description of terrestrial elements above. Descartes is, at this point, willing to assume primitive locations, in addition to lines, as constituent parts of his representation of space.

6.5 THE *WORLD*

In the *Rules*, Descartes noted that the solution of the anaclastic curve required a "knowledge of the action of light," which in turn necessitated knowing "what a natural power in general is."[52] Thus, in Descartes' view, his optical and geometrical work led naturally to an investigation of

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[50] Ibid., I 49.
[51] Here, Descartes rejects the position that place be identified with the surround – the bodies which contain the place in question. This is significant, since he will later come to adopt a very similar view.
[52] Descartes, *The Philosophical Writings of Descartes*, I 29.

the fundamental structure of nature – that is, to physics. Descartes followed this path of inquiry in the *World*, written from about 1629 to 1633 (and emended thereafter). In that work, Descartes constructs on the basis of a few fundamental physical principles or "Laws of Nature," then argues that this constructed world is indistinguishable from the actual one, implying that its basic principles are in fact those of the real world.

A rectilinear representation of space remains fundamental to Descartes' description of the *World*. Descartes proposes, for example, that the matter of his imagined cosmos "uniformly fills the entire length, breadth, and depth of this great space in the midst of which we have brought our mind to rest."[53] As in the *Rules*, space, in and of itself, is extension in three presupposed directions, which are specified in relation to assumed right lines.

The rectilinear representation of space takes on a new significance in the *World*, however. In the *Optics*, Descartes had acknowledged that light tends to act in straight lines. Now, he extends this principle to all bodies:

> I shall add as a third rule that, when a body is moving, even if its motion most often takes place along a curved line and, as we said above, it can never make any movement that is not in some way circular, nevertheless each of its parts individually tends always to continue moving along a straight line.[54]

Even if a body actually moves along a curved path, its inherent tendency is to move along a straight line. Thus, if a moving body is released from all constraints, it will continue its motion along a rectilinear path. What was originally supposed true about light is now posited as true of all bodies. It is a "rule" or "Law of Nature" that bodies have *linear inertia*. All bodies tend to continue moving along a straight line.[55]

In the *Rules*, the rectilinear action of light was explained on the grounds that nature acts simply and straight motion is more simple than curved. Here, the justification is also based on the simplicity of nature and of the straight line:

> This rule rests on the same foundation as the other two, and depends solely on God's conserving everything by a continuous action, and consequently on His

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[53] René Descartes, *The World and Other Writings*, trans. Stephen Gaukroger (Cambridge: Cambridge University Press, 1998), 22.
[54] Ibid., 29.
[55] As some commentators have noted, Beeckman held that a body, once moved, will continue moving; a principle he probably imparted to Descartes. However, Beeckman's principle, like Galileo's "core inertial concept" applied equally to linear and circular motions. Gaukroger, *Descartes: An Intellectual Biography*, 82.

> conserving it not as it may have been some time earlier but precisely as it is at the very instant He conserves it. So, of all motions, only motion in a straight line is entirely simple and has a nature which may be grasped wholly in an instant. For in order to conceive of such motion it is enough to think that a body is in the process of moving in a certain direction, and that this is the case at each determinable instant during the time that it is moving.[56]

Bodies tend to move linearly because linear motion is simple. In the terms of the *Rules*, linear motion is "absolute" – it can be "wholly" and directly grasped in a single instant, without reference to other bodies or other instants. Since nature or, as in this case, God acts in the simplest way possible, He preserves the state of a body's motion that can be understood most simply or "absolutely." This is linear motion, since linearity alone can be grasped in a single instant. Linear motion is simple, so it is the kind of motion conserved by the simple action of God.[57]

Notice, again, that the "simplicity" of rectilinear motion depends on Descartes' rectilinear concept of space. Straight motion is simple because it preserves direction. "It is enough to think," says Descartes, "that a body is in the process of moving in a certain direction" at every instant of its motion. As it moves along a straight path, a body is always moving in a "certain direction" that does not change. In general, though, straight motion only preserves direction in a rectilinear space, where direction is conceived as self-parallel. In other representations of space, motion along a straight line is not necessarily always in the "same direction." By contrast, curved motion, including circular motion, changes direction when direction is conceived in a rectilinear frame. As Descartes puts it, "to conceive of circular motion, or any other possible [curved] motion, it is necessary to consider at least two of its instants, or rather two of its parts, and the relation between them."[58] In other words, curves are not simple in a linear space. To describe them, one must relate the various rectilinear directions in which the body moves during different parts of its motion. (Notice how this conception of curves as "relative" is a direct echo of the precedence given to lines in Descartes' geometry.[59])

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[56] Descartes, *The World*, 29-30.

[57] Recall, the distinction between linear and all other kinds of motion is simply intuited. The distinction between motion and rest, not addressed here, is more problematic.

[58] Descartes, *The World*, 30.

[59] As Emily Grosholz notes, "the straight line segments of the *Geometry* seem to correspond nicely to the inherently uniform, rectilinear motion of the bits of matter which are the simples in the physics." Emily R. Grosholz, "Geometry, Time and Force in the Diagrams of Descartes, Galileo, Torricelli and Newton," *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association* 1988, no. 2 (1988): 244.

Descartes can call linear motion "simple" because he has already presupposed lines in his conception of space. In the *Rules*, Descartes' rectilinear representation of space was used because it conveniently represented optical phenomena or grounded his geometrical method. In the *World*, though, the scope and importance of the rectilinear framework increased markedly. The rectilinear representation of space comes to ground Descartes' fundamental physical principles, including a principle of linear inertia.[60]

6.6 COSMIC VORTICES

The fundamental "Laws of Nature" Descartes proposes in the *World* all concern local phenomena. They govern the behavior of individual, particulate bodies either alone or in interaction with other particulate bodies. Thus, the rectilinear representation of space that underlies Descartes' basic physical principles applies especially in the small scale, where those principles can be directly applied to the individual interacting bodies. In the *World*, though, Descartes does not limit his discussion to small-scale phenomena. He also considers large-scale systems of many interacting bodies. In particular, he tries to explain celestial phenomena, such as the motions of the planets and comets. To do so, Descartes adopts a spherical representation of space.

Descartes supposes that the universe contains an indefinite number of celestial vortices, each whirling about a central star. These vortices are each described in relation to a presumed center, which happens to coincide with the star. Our solar system, for example, contains a vortex around the sun. Thus, positions and directions within our vortex are specified relative to the central solar body:

> From [the properties of a vortex] you will realise immediately that the highest planets must move more slowly than the lowest, that is, those closest to the Sun,

[60] Machamer asks why and how Descartes assumes that the simplicity of straight lines constrains God's action. This remains an open question. The point here is that the assumed simplicity of straight lines arises in a physical context independent of and prior to Descartes' metaphysical consideration of God's action. See Peter K. Machamer, "Causality and Explanation in Descartes' Natural Philosophy," in *Motion and Time, Space and Matter*, ed. Peter K. Machamer and Robert G. Turnbull (Ohio State University Press, 1976). Playing counterfactuals for a moment, had Descartes held, like his Aristotelian predecessors, that circular motion around the center was "simple," God's assumed simplicity would likely have entailed, for Descartes, a principle of circular inertia similar to Galileo's.

> and that all the planets move together more slowly than the comets, which are nevertheless further away.[61]

The locations of the planets are here described by specifying their distance to the center. "High" is interpreted as further from the center. Hence, "higher" planets are described as further from the central point –the sun. Conversely, "lower" planets are described as closer. In general, Descartes describes the behavior of a vortex using a spherical representation of space centered on a stipulated central body.

Descartes also proposes that the earth itself possesses a vortex. Thus, terrestrial phenomena are described using a spherical representation of space centered on the earth:

> Then consider that, since there is no space such as this beyond the circle ABCD [around the earth] that is void and where the parts of the heavens contained within that circle are able to go, unless others which are exactly similar replace them simultaneously, the parts of the Earth cannot move away any further than they do from the centre T either, unless just as many parts of heaven or other terrestrial parts required to fill them come down to replace them. Nor, conversely, can they move closer to the centre unless just as many others rise in their place... Now it is evident that, since much more terrestrial matter is contained within this stone than is contained in an amount of air of equal extent... the stone should not have the force to rise above it; but on the contrary this amount should rather have the force to make the stone fall downwards.[62]

Here, "above" means further from the center. "Downwards" is toward the center. Descartes, that is, employs a spherical representation of space in order to describe the vortex around the earth, just as he does describing the one around the sun. He assumes a central point near (though not quite identical to) the center of the earth, and then specifies directions and locations in relation to it.

This leads to the striking and tell-tale description of the moon's behavior as a stasis, rather than a rotation:

> So it [the moon] must remain as if attached to the surface of a small heaven ABCD and turn continually with it about T. That is what prevents its forming another small heaven around it, which would make it turn again around its own centre.[63]

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[61] Descartes, *The World*, 35.
[62] Ibid., 49.
[63] Ibid., 46.

In other words, on Descartes' view, the moon does not rotate "around its own center." The fact that it does not have its own vortex is meant to explain why not. Of course, as we have discussed before, such a description only makes sense in a spherical representation of space, where the direction of the moon's "face" is referred to a radius to the center. Only in a spherical conception of space centered on the earth does this direction remain constant as the moon orbits the earth. Thus, the moon's behavior can only be described as a "staying" or non-motion in a spherical space. Descartes description is indicative of his spherical framework.

Now, according to Descartes, there is no inconsistency between the rectilinear space used to represent small-scale phenomena and the spherical space used to represent cosmic vortices. Any contradiction between the two is cleared up by his account of vortex creation:

> For, first, because there is no void at all in this new world, it was not possible for all the parts of matter to move in a straight line. Rather, since they were all just about equal and as easily divisible, they all had to form together into various circular motions. And yet, because we suppose that God initially moved them in different ways, we should not imagine that they all came together to turn around a single centre, but around many different ones, which we may imagine to be variously situated with respect to one another.[64]

All bodies tend to move rectilinearly according to the fundamental physical principles Descartes has suggested. Since the universe is a plenum and "there is no void," however, all actual motion is constrained to follow closed paths. Thus, when matter was set in motion at the creation, it instantly formed itself into whirling vortices filling the universe.[65]

This account makes it clear, though, that the small-scale rectilinear descriptions of phenomena take precedence over the spherical descriptions of the cosmic vortices. Fundamentally, the universe is rectilinear, and bodies really do tend to follow the presupposed rectilinear structure of space – the assumed linear directions. Together, the tendencies of individual bodies generate spherical vortices, but the spherical representation of space used to describe them is just a manner of speaking. It is convenient given the spherical shape of a vortex. A rectilinear representation of space must be assumed in order to describe the basic physical causes of all motions. A spherical space is merely convenient to describe the effects of

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[64] Ibid., 32-3.
[65] See Wallace E. Anderson, "Cartesian Motion," in *Motion and Time, Space and Matter*, ed. Peter K. Machamer and Robert G. Turnbull (Ohio State University Press, 1976).

those causes. For Descartes, rectilinear small-scale space "builds up" into cosmic spherical space.[66]

6.7 PLACE AND MOTION IN THE *PRINCIPLES OF PHILOSOPHY*

The conception of motion and place that Descartes presents in the *World* is not significantly developed beyond that of the *Rules*. He still wants motion to be a simple notion that can be directly intuited:

> By contrast, the nature of the motion that I mean to speak of here is so easily known that even geometers, who among all men are the most concerned to conceive the things they study very distinctly, have judged it simpler and more intelligible than the nature of surfaces and lines, as is shown by the fact that they explain 'line' as the motion of a point and 'surface' as the motion of a line.[67]

Motion is supposed to be an obvious concept in need of no further elaboration. One simply knows it when one sees it. Nevertheless, upon further exposition, Descartes defines motion as change of place:

> For my own part, I know of no motion other than that which is easier to conceive of than the lines of geometers, by which bodies pass from one place to another and successively occupy all the spaces in between.[68]

In other words, motion is simply a continuous change of place. Yet place is no better understood in the *World* than it was in the *Rules*. In fact, for local-scale phenomena, Descartes does not discuss how place is defined, and we can assume that the relative conception of place described in the *Rules* still applies. For cosmic phenomena, meanwhile, places are specified in relation to the assumed center, as we have seen. Thus, despite Descartes' intentions, motion remains a relative concept in the *World*. Nevertheless, Descartes could justify leaving the "simple" nature

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[66] Note that Descartes' account of vortex formation is hand-waving at best. He does not link the vortex to his fundamental laws of nature in anything more than a superficial, qualitative way. In particular, his account of the centrifugal force responsible for gravity is questionable. Descartes' successors, Huygens in particular, take up these issues. See E. J. Aiton, *The Vortex Theory of Planetary Motion* (New York: Elsevier, 1972).
[67] Descartes, *The World*, 26.
[68] Ibid., 27.

of motion unelaborated by appealing to the fact that most philosophers and geometers considered it unproblematic.[69]

In the *Meditations on First Philosophy* (1641), though, Descartes began to question the bases of all intuitions, including the "pure and simple natures" he had taken for granted until then. Once he adopted this program of radical doubt, Descartes had to suppose that philosophers and geometers could be deceived. He was forced to reexamine how one can know something moves. In the *Meditations*, Descartes established a standard for sure knowledge: "clear and distinct" ideas could be known without doubt. Their validity is guaranteed by the benevolence of God. Therefore, one could know that something is moving if one could gain a "clear and distinct" idea of its motion. To ensure that knowledge of motion is possible, then, Descartes was forced to establish that one can obtain a "clear and distinct" idea of motion. He tackles this project in the *Principles of Philosophy*, published in 1644.[70]

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[69] This appeal was somewhat disingenuous. Certainly, philosophers considered motion to be a basic notion. Physical laws depended on the motions of the interacting bodies. However, most of Descartes' peers considered motion a conceptual composition of distance and time. One could not measure motion directly – one had to measure time and distance covered, then calculate the motion. In other words, motion was considered basic but *not* simple. Descartes' insistence that motion is simple is associated with his willingness to adopt collisions as the basic model of physical phenomena. In Descartes' laws of collision, speeds or velocities appear as independent variables. This was a marked departure from his predecessors, who used balance models precisely because one of the two independent variables, time, was removed from consideration by the rigid nature of the balance. Thus, two interacting bodies could have different motions, but the motions would have to take place in the same time, so the motions could be measured simply by the distance covered. See Joseph E. Brown, "The Science of Weights," in *Science in the Middle Ages*, ed. David C. Lindberg (Chicago: University of Chicago Press, 1978); Peter K. Machamer, "Galileo's Machines, His Mathematics, and His Experiments," in *The Cambridge Companion to Galileo*, ed. Peter K. Machamer (Cambridge: Cambridge University Press, 1998).

[70] In the *Meditations*, Descartes concludes that we can be sure of our experience of the physical world, so long as the experience consists of clear and distinct ideas. Thus, our clear and distinct knowledge of motion is ultimately warranted by the direct action of God, whose benevolence prevents Him from deceiving us about the truth of clear and distinct ideas. By the time of the *Principles*, though, it is no longer clear exactly how God warrants our knowledge of motion. That is, it is not clear whether we are to take Descartes to mean that motion is a real property of bodies, a position defended by Daniel Garber, or to mean that God directly endows the intellect with a clear and distinct perception of motion that does not depend in any way on the actual properties of extended things, a position defended by Peter Machamer and J. E. McGuire. This debate, however, is tangential to our argument, since it, at this point, it only concerns the absolute nature of the intuition of motion, regardless of the ultimate cause of that intuition. In either case, though, the appeal to God to warrant knowledge requires an simple and direct intuition of motion. Since God, a supremely independent being, Himself associates motion with body (in the body or in the intellect), there must be a clearly, distinctly, and immediately known fact of the matter about a body's state of motion. Motion cannot depend on the intellectual compositions of mortal, dependent creatures. Daniel Garber, *Descartes' Metaphysical Physics* (Chicago: University of Chicago Press, 1992); Peter K. Machamer and J. E. McGuire, "Descartes' Changing Mind," (forthcoming); Peter K. Machamer and J. E. McGuire, *Descartes' Epistemic Stance: Mind, Body and the Causes* (forthcoming).

The *Principles* does not significantly alter the rectilinear representation of spatial extension or the basic physical principles presented in the *World*.[71] It does, however, present a refined doctrine of place and motion in an attempt to solve this epistemological problem regarding motion. Without too much detail, Descartes' ultimate solution is to require motion to be a simple concept – a property of a body that can be known directly, without needing prior intuitions. If motion is a "pure and simple nature" pertaining to a body in and of itself, then it can be perceived clearly and distinctly whenever a body is perceived. That is, a body's state of motion must be directly intuited, without reference to other intuitions. This entails that motion cannot be a relative mode of a body, distinguished by relation to an assumed origin, since this concept of motion requires a prior intuition of the origin. Hence, Descartes must establish motion as a simple concept.

This epistemological requirement runs counter to a fundamental feature of Descartes' rectilinear representation of space. In a rectilinear space, the presupposed primitives are lines, not points. There are no inherently privileged locations by which places can be simply described. Locations are only specifiable in relation to points taken to be privileged by the subjective observer and superadded to the representation of space. To describe a phenomenon using a rectilinear framework, then, one must stipulate an arbitrary origin over and above the geometric structure of the space itself. One does so by choosing a point to call fixed (usually by selecting an object to "mark" the location – the center of the earth, for instance) and in virtue of which a body is "said to be here or there." This stipulation, together with the directions already part of the spatial concept, establishes the frame of reference by which the phenomenon is described. Locations can then be described relative to that frame. Descartes, as we noted, proposed this conception of location in the *Rules*.

By the time of the *Principles*, this relative description of place is no longer acceptable because it leads to a relative notion of motion. If motion is simply change of place, a body moves only if its place relative to the chosen origin changes. A body's state of motion or rest depends on the point by which its place is specified. Choosing a different point might change the

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[71] Emily R. Grosholz, "A Case Study in the Application of Mathematics to Physics: Descartes' Principles of Philosophy, Part II," *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association* 1986, no. 1 (1986).

body's state of motion.[72] Yet motion is supposed to be an mode of the body itself. It is supposed to be a clear and distinct idea. One should not have to consider any other entity to know if a body is moving, including other bodies taken to be motionless.

In the end, Descartes tries to have it both ways. He presents two different definitions of what he labels "external place."[73] Strictly speaking, place is specified in relation to the body itself:

> ...external place can be taken to be the surface which most closely surrounds the thing placed. It must be noticed that by 'surface' we do not understand here any part of the surrounding bodies, but only the boundary between the surrounding and surrounded bodies, which is simply a mode... which is not a part of one body more than of the other...[74]

Place, in this strict sense, is specified in relation to a set of points – actually, the entire surface – that marks the boundary of the body. Thus, place in the strict sense becomes a simple concept. The place of a body is specified by reference to an extensional property of the body – its surface. Place can be intuited simply, through the same intellectual act by which the extension itself comes to be known. Place is just a "mode" of a body itself. No reference to any prior intuition of another entity is required to specify location.

Motion, therefore, also becomes a simple concept. In the strict sense, a body's place is related only to its own boundary, but Descartes notes that the boundary of a body is a mode shared by the bodies contiguous with it. Thus, a body is considered moving if it separates from the surface shared by the surround, i.e., if it is "transferred" from the vicinity of one set of bodies to another.[75] A body moves if it changes place with respect to its own surface. In effect, a body is its own absolute reference frame. The origin of the reference frame can be "marked" by the surface the body shares with its surround. Since there can be no universal frame of reference, it might not be possible to give an absolute location of the body in the universe, but it *is* possible to

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[72] See Edward Slowik, "Descartes, Spacetime, and Relational Motion," *Philosophy of Science* 66, no. 1 (1999): 125-6.

[73] "External place," the location of an object, is distinguished from "internal place," the volume a body occupies. See Edward Grant, "Place and Space in Medieval Physical Thought," in *Motion and Time, Space and Matter*, ed. Peter K. Machamer and Robert G. Turnbull (Ohio State University Press, 1976).

[74] René Descartes, *Principles of Philosophy*, trans. Valentine Rodger Miller and Reese P. Miller (Dordrecht: D. Reidel, 1983), II.15 p.46.

[75] Ibid., II.25 p.51.

distinguish its motion from rest simply by intuiting all of its *own* modes. Motion and rest are "merely diverse modes of the body in which they are found."[76]

Descartes' strict sense of place answers the question of motion's epistemological warrant. We can know a body moves because separation from its surround is a simple intuition. This opens the possibility that we can have a clear and distinct idea of motion, the validity of which can be guaranteed by the Deity.[77] Physically, however, the solution is not quite satisfactory. Descartes is literally playing a shell game. By reducing a body's place to a property of the body itself, without reference to any other body or location, he is in effect saying, "Wherever a body is, *there* it is." While this makes place and motion simple, it does not allow physically adequate specifications of locations. Describing place this way does not tell one *where* to find the body in space or how it is located relative to other bodies. External place in the strict sense is not useful if one wishes to describe physical situations.

As a result, Descartes falls back on the "ordinary" and relative sense of place, where location is specified by reference to a given, fixed location:

> Moreover, in order to determine that situation [among other bodies] we must take into account some other bodies which we consider to be motionless: and, depending on which bodies we consider, we can say that the same thing simultaneously changes and does not change its place.[78]

Here, a body's place or "situation" is described by assuming other bodies to be fixed and motionless. The place is then described relative to the stipulated bodies. This is similar to the concept of place Descartes recognized in his earlier texts.[79] While this notion is not simple, it does accord with the fact that the rectilinear concept of space does not provide privileged locations by which absolute places could be specified. Descartes' dual conceptions of place satisfy divergent requirements of his epistemological system: the need for a simple intuition of place, on the one hand, and the lack of means to describe place, on the other.

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[76] Ibid., II.27 p.52.

[77] Descartes also used this strict definition of place and motion also helped allay concerns about the Copernicanism implicit in his cosmology. Since, in Descartes' system, the earth does not move relative to the layer of air and ether immediately surrounding it, he can contend that the earth does not separate from its surround and, therefore, does not move. This consideration, though, runs parallel to and independent of the development discussed here. Ibid., III.29 p.95.

[78] Ibid., II.13 p.45.

[79] In the *Rules*, Descartes allowed the specification of place in relation to "parts of extended space." Here, he requires the specification in relation to other bodies. This difference is minor, especially since extension and body are equivalent.

We should also note that Descartes considers and rejects another possible solution to the problem of place and motion. Descartes' could try to resolve the impasse by establishing a universal frame of reference by which one might absolutely determine the motion or rest of bodies.[80] He could, for example, stipulate that the sun (or some other body) absolutely does not move. As a result, the sun would be a fixed point of reference. If bodies change their place relative to the sun, they absolutely move. The sun (or any other point motionless with respect to the sun) would be the origin of a fixed, universal frame of reference – an absolute space.[81]

Descartes, however, dismisses this solution outright. On his view, it is impossible to find any inherently privileged center by which a universal frame of reference could be established:

> Finally, if we think that no truly motionless points of this kind are found in the universe, as will later be shown to be probable; then, from that, we shall conclude that nothing has an enduring, fixed and determinate place, except insofar as its place is determined in our minds.[82]

There is no way one could determine whether a point in the universe is fixed and, therefore, a possible origin of an absolutely fixed reference frame. Hence, there is no way to know that the location of such a point is "fixed and determinate." We can specify the location of bodies only by stipulating fixed reference points. But this is a determination of our subjective minds, not reality itself. Objectively, all place is relative.[83]

The dual definitions of place are an ingenious attempt to work around the problem of place and motion, but, in the end, the solution is divergent. Either place is relative or it is simple; it cannot truly be both. In the *Principles*, the dual conceptions of place are never adequately reconciled, and Descartes left it to his successors to resolve the tension. Nevertheless, we should emphasize that the problem itself is due to Descartes' firm allegiance to a rectilinear concept of space, which itself represents a fundamental break with the spherical past. Descartes has

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[80] By point of contrast, this will be Isaac Newton's solution to the same fundamental problem.

[81] Note that the passage from the *World* mentioned above (Descartes, *The World*, 22.) seems to suggest that Descartes flirted with this solution. There, he suggests that one's mind can be placed in an extended space prior to the filling of the space with body. This space, prior to body and motion, would qualify as an absolute space. Yet, Descartes does not elaborate this conception of space, either in the *World* or thereafter, and he rejects it in the *Principles*.

[82] Descartes, *Principles of Philosophy*, II.13 p.45.

[83] In a sense, this is the ultimate solution of the Copernican problem which began this project. Copernicus asked, in a world with many centers, how do we choose the right one? Descartes rejects the question. Since there are many centers, he says, there are none.

spawned a new way of seeing the world. It is not without its difficulties,[84] but the difficulties are ones never faced before.

6.8 ARBITRARY SPATIAL ORIENTATION

At this point, we should address in greater detail two of the more important and novel features of the Cartesian representation of space. First, Descartes acknowledges that the geometric entities he assumes in order to represent space are entirely arbitrary, chosen for the sake of convenience. The lines by which "length," "breadth," and "depth" are specified need not correspond with any real, objective features of the physical world. Physical bodies and spaces are extended in three dimensions "simply *qua* extended." They possess extension *simpliciter*, which is undifferentiated with respect to particular directions. As a result, what aspect one "takes" as "length," etc., is "immaterial" and merely "nominally" distinguished from any other direction. No feature of a body or space is inherently its "length." Dimensions, meanwhile, are only aspects by which a body or space "is considered to be measurable."[85] Thus, we are free to assume any direction we wish in order to measure the "length," "breadth," or "depth" of a body. The dimensions are modes of a subjective idea of body or space – of its representation by a subjective observer – not of the body or space in and of itself.

Descartes' position completes a progression we have noted in earlier chapters. Alongside the general trend towards rectilinear representations of space, conceptions of space, once regarded as transcendent, objective features of the world, have become conventional, situation-specific, and subjective assumptions. Gilbert's cosmic orientation, for example, was fixed by the Creator at the time of the creation. Hence, it transcended any particular observer or situation. For Kepler, the orientation was a feature of the representation of a particular physical situation, i.e., a planetary orbit. However, the orientation corresponded to a real feature of that phenomenon, the perpendicular to the line of apsides (or the apsidal line itself). Thus, while the

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[84] Another problem, not mentioned above, is that it is not clear how motion can be identified over time. If motion is separation from the surround, how can one say that the movement of a body is "the same" in successive instants, when the surround has already changed. This is particularly troubling, since Descartes' physical principles rely on God's preservation of "the same" motion in a body over time. See Garber, *Descartes' Metaphysical Physics*, 171.
[85] Descartes, *The Philosophical Writings of Descartes*, I 62.

orientation may have been specific to the phenomenon, it was not arbitrary, and different observers were not free to choose the orientation of their space. Similarly, for Galileo, a rectilinear representation of space always had to "fit" into the spherical representation it was meant to approximate. Thus, the orientation of the vertical always had to coincide with the vertical of the spherical system – determined by the center of the earth or sun. For Descartes, though, the orientation of the rectilinear representation of space is a purely subjective choice, independent of any features of the phenomena. Of course, different observers might choose to privilege the same direction, but that choice would be merely conventional, made on the basis of descriptive convenience, not objective reality.

Secondly, and consequently, Descartes' concept of space is rigorously isomorphic. For Descartes, there can be no inherently privileged direction or point. The choice of one direction to call "down" is as good as any other. Privileged directions or points, *qua* geometric entities, lose any vestige of significance for the phenomena represented. As a result, they also lose their causal efficacy.[86] For Gilbert, the cosmic orientation was instantiated by a universal magnetic field. Similarly, for Kepler, the orientation of the space was "respected" by a planet's internal magnetic virtue. For Galileo, as for many others, heavy bodies possess an innate tendency to move "downward" toward the geometric point coinciding with the center of the earth or universe. In each of these cases, the actions of causes depend on the geometric features of the space used to represent the phenomena. For Descartes, however, physical causes cannot "line up" with the representation of space, since the representation of space does not respect any objective feature of the physical situation. Different observers are free to choose different representations of a single phenomenon, and it would be absurd to think that this changes the physical features of that phenomenon, or the laws of nature that govern it.

These conceptual developments are reflected in Descartes' physical principles. His "rules" of impact, for example, do not distinguish one spatial direction from another:

> It must also be noticed that one movement is in no way contrary to another movement of equal speed; but that, strictly speaking, only a twofold opposition is found here. One is between movement and rest… the other is between the

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[86] See Dudley Shapere, "The Causal Efficacy of Space," *Philosophy of Science* 31, no. 2 (1964)..

> determination of a body to move in a given direction and the encounter, in its path, with a body which is either at rest or moving in a contrary manner…[87]

The directions in which percussent and resistent move are arbitrary. Motion along different directions is "no way contrary." Thus, for example, motion along the surface of the earth cannot be opposed to motion towards the center of the earth. Motion in a given direction is only opposed to rest and motion along the same direction in the opposite sense, or "determination." Directions, in themselves, are all considered equivalently.

This does not change the fundamental fact that a representation of space *does* affect physical theories. As we have seen, Descartes' physical principles, including linear inertia, are justified by assuming the simplicity of linear motion. This assumption is only warranted, however, supposing a rectilinear concept of space. In any other conceptual framework, Descartes would have to explain why straight motion is simple and, thus, why bodies tend to follow them. Changing the spatial concept changes the explanatory requirements. The representation of space used to describe phenomena frames the theory used to explain them.

6.9 CONCLUSION

The Cartesian rectilinear representation of space marks a profound shift to a new way of seeing the world. For the first time, we are presented with a subjective, isomorphic, and linear representation of space. Descartes himself seems to have adopted this fundamental feature of his thought from the earliest years of his career. While we cannot be sure of the reasons why Descartes adopted a rectilinear spatial concept, it does seem related to his early work on optics and geometry. As his philosophical program developed, however, the linear framework formed the essential basis for Descartes' descriptions and explanations of phenomena. Small-scale interactions – collisions – between bodies were represented in a rectilinear space and explained on the basis of rectilinear physical principles, including rectilinear inertia. In the case of large-scale ensembles of bodies, such as the celestial vortices, Descartes did resort to spherical

[87] Descartes, *Principles of Philosophy*, II.44 p.63.

descriptions, but this spherical construction was constructed out of smaller, rectilinear spaces. For Descartes, the rectilinear took precedence over the spherical.

7.0 CONCLUSION AND PROSPECTUS

This project set out to describe a transition in Western Civilization's understanding of the physical world during the first half of the seventeenth century. This transition affected representations of space – the conceptual frameworks by which observers generate descriptions of spatial properties and relations. In particular, the period witnessed a shift from a spherical, Scholastic worldview to a rectilinear, Cartesian framework. This transition from spherical to rectilinear space came about as an iterative reciprocation between three levels of understanding: concepts, descriptions, and explanations. Adjustments at any level led to adjustments at the others, such that by the end of the period we have studied, the conceptual apparatus, descriptions of phenomena, and explanatory theories used to understand the world had markedly changed. Therefore the conceptual transition both accompanied and enabled significant developments of the physical theories used by natural philosophers to explain the phenomena of the universe.

7.1 THE CONCEPTUAL, DESCRIPTIVE, AND EXPLANATORY PROBLEMS

At the end of the medieval era, Aristotelian Scholastic physics, and the Ptolemaic astronomy based on it, dominated Western understanding of the natural world. These theories constituted a remarkably coherent edifice, and could explain most terrestrial phenomena, as well as the motions of the planets. To do so, they appealed to "natures" – the inherent tendencies of bodies. These "natural" explanations, however, were dependent on the assumption of a central point. In the terrestrial realm, for example, bodies naturally moved either towards or away from the universal center, while celestial bodies naturally rotated around it. Thus, Scholastic physics was grounded in a spherical concept of space, which presupposed the central point essential to its explanations. As long as one granted the conceptual framework, one could broadly accept many, if not most, of the Scholastic explanatory accounts.

The first cracks in this edifice came at the level of description. Nicolaus Copernicus noticed that there was an incongruity between the Scholastic physical explanations and the Ptolemaic astronomical descriptions. The problem was subtle. The general outlines of both theories – planets or spheres moving spherically about a center – were all of a piece and consistent. The trouble lay in the details, with Ptolemy's use of an equant to describe the motion of the planets (and some other finer points). Copernicus thought that the equant violated the principles of Scholastic physics since it described a non-uniform rotation, whereas Scholastic physics accounted for uniform motions. As a result, he gave up the equant point in the descriptions of astronomical phenomena to save the Scholastic explanations. Copernicus sacrificed the descriptions in order to preserve the explanation – scrapping Ptolemy to save Aristotle. In place of Ptolemaic descriptions, Copernicus substituted his own, in which he described many phenomena as motions of the earth, rather than motions of the heavenly bodies.

Copernicus thought he could bring astronomy into line with acceptable physical principles by simply redescribing the celestial motions. However, his move was not so straightforward. It undermined both the conceptual and explanatory parts of the scholastic edifice. The conceptual part because Copernicus destroyed the unity of the center. Copernicus was forced to conclude that there were "many centers," so there were many spherical frames, each an independent space with its own descriptive structure. Notions essential to Scholastic explanations, such as "up," "down," "around," and "simple motion," lost their determinate and unique meanings. Instead of one universal conceptual scheme, Copernicus gave rise to at least two – one around the sun, another around the earth – if not more.

The explanatory part of the Scholastic edifice was likewise threatened. In the first place, the notions crucial to explanations lost their fixed meanings, so it was no longer clear what phenomena they accounted for. The natural "downward" tendency of a stone, for instance, could explain the stone's motion if "down" meant toward the center of the earth, but not if it meant toward the center of the sun. Hence, each descriptive frame had to be associated with its own set of explanations. Celestial physics became distinct from terrestrial physics. Secondly, Copernicus's theory included novel descriptions that needed to be assimilated into the explanatory theory. The rotation of the earth and everything on it was a particular problem. The old explanations were no longer sufficient. They could account for the stone's fall, but not why it kept up with the place on the rotating earth from which it was dropped. Another of the "new" descriptions was especially significant for the story told in this project: Copernicus's third motion. Copernicus's descriptions of the earth's motions ascribed a rotation to its axis in relation to the ecliptic. Those who subsequently hoped to accept this "motion" were forced to provide an explanation of it. This proved difficult, and the problem accelerated the shift toward a rectilinear spatial concept. In the end, the explanatory problem of the "third motion" was solved by changing the representation of space such that the "third motion" was no longer a motion.

As a result of Copernicus's geokinetic hypothesis, two difficulties became the litmus tests of physical thought that natural philosophers were forced to address. First, the descriptive problem of picking the "right" center. If the center of the spatial framework underlying the description of phenomena did not coincide with the center of the earth, where was it? Was it the center of the sun? How might one know? Second, the explanatory problem of accounting for the "new" motions of the earth and everything on it. How was it that the earth rotated around its own axis and revolved around the sun and no terrestrial phenomenon could be made to exhibit those motions? If the earth really moved, what were the physical principles governing the behavior of terrestrial bodies? Eventually, solving the difficulties raised by Copernicus's astronomy required both a new conceptual framework and a new physics.

7.2 THE CELESTIAL SOLUTIONS: GILBERT AND KEPLER

William Gilbert attacked the explanatory problems regarding the earth's motions while avoiding the problem of centers. In fact, he punctiliously restricted his discussion to terrestrial motions, so the issue of centers other than the earth's own was not raised. He never even explicitly admitted the earth's motion around the sun. Gilbert's explanatory strategy, meanwhile, was to describe the earth as a spherical magnet. He argued that all of the descriptions applicable to magnets were applicable to the earth as well. Hence, the motions of a magnet caused by its magnetism are also caused in the earth. The earth's magnetism explains its motion.

Gilbert's line of argument, though, imported the conceptual apparatus appropriate to the description of spherical magnets into the description of the earth. This representation of space is not quite the same as the spherical framework used by the Scholastics. It is a geographical representation, where the spheres are not constructed around a presupposed central point, but in relation to stipulated poles. The poles, in turn, can be used to define an axis. When Gilbert applied this representation to the earth, he extrapolated the axis so that it became an orientation of space itself. This "verticity" was then used to redescribe the earth's third motion as non-motion, thereby simplifying the task of explaining it. Gilbert's conception of the universe presupposed a linear orientation in addition to the poles, center, and spherical surface of the earth. His account of terrestrial motions, and the third motion in particular, required new descriptions based on a novel concept of space.

Johannes Kepler took up a project similar to Gilbert's – to provide explanations for the planetary movements described by Copernicus – only Kepler addressed Mars's revolution around the sun, instead of the earth's own rotations. His explanatory aims were tempered, though, by his particular religious outlook. For Kepler, the goal became reconciling descriptions of planetary phenomena with a plausible causal explanation. He adjusted both descriptions and explanations, each in turn, trying to bring them together. The "most earnestly desired union" of explanation and description, however, could not be effected without an adjustment at the conceptual level. He adopted Gilbert's rectilinear, oriented space in order to represent the planetary *vis insita* – the planet's own contribution to its motion. Thus, for Kepler, one aspect of the planetary phenomena could only be described *and* explained assuming a rectilinear conception of space.

Note that the conceptual adjustments made by Gilbert and Kepler in the context of planetary motions are solutions to *explanatory* problems. Gilbert and Kepler were both faced with a motion they could not explain – the changing direction of a planet's magnetic axis of virtue. Their solution was to adopt an altered representation of space in which the "change" is described as a "staying." This new conception does not obviate the need for causes, and both Gilbert and Kepler appeal to physical causes that respect the orientation of their space (the "law of the whole" and the planet's "animate faculty," respectively). However, it does obviate the need for causal *action*. Gilbert and Kepler do not need to explain how the causes act. They do not need to elaborate physical principles that govern how the causes function. The causes merely maintain a stasis – they remain constant.

Nevertheless, the causes in question do not respect any center in the phenomenal system. One need not "know" where some center is in order to give an account of the cause. In other words, the presupposition of a center is not causally necessary (at least in the limited cases we discussed). On the other hand, the causes do require a presupposition of fixed linear orientations. The causes are said to remain constant in relation to these orientations, keeping a rectilinearly conceived direction the "same." Presupposed fixed rectilinear orientations were not provided by a spherical concept of space, though. A new, rectilinear framework had to be adopted. Thus, the explanatory problems addressed by Gilbert and Kepler were solve by altering the conceptual scheme in which phenomena were described and explained.[1]

7.3 THE TERRESTRIAL SOLUTIONS: GALILEO AND DESCARTES

The trend toward linearity also affected seventeenth century understanding of phenomena on the surface of the earth. Once Copernicus raised the possibility of a rotating earth, both supporters and opponents of his theory were faced with problems regarding terrestrial physics. The Copernicans had to explain why bodies apparently behaved exactly as one would expect if the earth did not move, while their opponents had to argue why supposing the earth moved, even in light of Copernican explanations, was physically unreasonable. Many of the cases involved in these disputes involved the continuation of rotational motion by bodies separated from the earth's motion. It was asked, for example, how falling bodies might keep up with the rotation of the earth so as to fall on the spot below their release point, or how a cannonball fired vertically might maintain its rotational motion so as to fall back upon the cannon. Within the Scholastic framework of the period, there were two possible explanations for a body's continued motion around the center of the earth. First, one could argue that the body's continued motion was "natural." On this view, it was simply a constitutive feature of terrestrial bodies to rotate around the earth's axis, whether they were attached to the earth or not. Second, one could say that the earth's motion was "impressed" on the body before its release and that the body retained this "violent" motion once separated from the earth.

[1] In Kepler's case, recall, only the *vis insita* is rectified. The *vis extrinsica*/*vis praehensiva*/*anima motrix* – i.e., the cause that moves the planets around the sun – remained spherically described and explained.

One of Galileo Galilei's remarkable physical achievements was to dissolve the difference between these two explanatory regimes under the single principle of "inertia": a body, once moved, will continue to move. Galileo did not, however, dissolve the distinction between the two representations of space upon which the two physical regimes were based. "Natural" motion was thought to perpetuate rotation around a presupposed point, the center of the earth, and was thus represented spherically on a global scale. "Impressed" motion was supposed to continue along straight lines for short distances, implying a small-scale rectilinear representation of space. As a result, Galileo used a rectilinear representation of small-scale space in situations where "impressed" motion had applied. He employed a spherical frame for larger spaces, where phenomena had been explained by appealing to "natural" motions. Galileo's attempts to explain terrestrial phenomena on a moving earth, therefore, led to a dual representation of space: rectilinear in the small scale, spherical in the large scale. The Copernican hypothesis demanded novel explanations of phenomena. Galileo's new explanations, in turn, hung two spatial concepts in the balance, each applicable in its own way.

Ultimately, however, Galileo held that the spherical concept of space was more fundamental than the linear. A rectilinear framework could be used to describe small-scale phenomena, but this was merely an approximation of the really spherical cosmos. In the case of Galileo, the explanatory pressures of the geokinetic hypothesis failed to produce a conceptual shift.

For Descartes, the balance swung the other way. He, too, had a dual representation of space, rectilinear in the small scale and spherical in the large scale. Yet, in the face of the same explanatory problems raised by a moving earth, Descartes chose to privilege the rectilinear over the spherical, perhaps because of his youthful interest in optics and geometry. For the first time, an author had proposed a fundamentally rectilinear framework to describe and explain terrestrial phenomena. Descartes' physical principles, including his concept of inertia, relied on a linear space independent of centers. In the same way, Descartes eliminated the conceptual problem raised by Copernicus by dismissing the need for centers altogether. Still, even Descartes' conversion to a truly rectilinear space was not whole-hearted. He, like Galileo, continued to represent large spaces spherically, now as a convenient way to represent situations involving many bodies. In particular, he continued to explain celestial and gravitational phenomena on the basis of spherically described vortices.

In the century between 1543 (the date of Copernicus's *De Revolutionibus*) and 1644 (that of Descartes' *Principles*), the explanatory and conceptual difficulties raised by Copernicus's redescription of celestial phenomena had brought about a reconceptualization of space. The Scholastic, spherical worldview had given way to the rectilinear conception of space embodied, for terrestrial phenomena, in the work of Descartes and, for celestial phenomena, in the work of Kepler. This shift, moreover, was both a result and a cause of the notable advances in physical theory of the period as iterative reciprocal adjustments affected the conceptual, descriptive, and explanatory understanding of nature.

Of course, we have not told the whole story. There are many aspects of the transition from spherical to rectilinear we have merely glimpsed, if we addressed them at all. For example, one could say much more about descriptions of magnetism and geographic representations prior to Gilbert.[2] Once could also investigate the impetus theories of motion that led to Galileo's work or the optical and mathematical theories that led to Descartes'. In addition, one might discuss the history of the parallelogram composition of forces, another physical principle that relies on a rectilinear representation of space.[3] Ultimately, the development of rectilinear representations of space is merely one thread in the vastly interconnected intellectual complex that is the Scientific Revolution. Time and space prevent any project from pursuing all the available connections to their ends.

7.4 THE CONTINUATION: HUYGENS, NEWTON, AND BEYOND

Nor is the work of Descartes, or even the rest of the Scientific Revolution, the end of the story. The rectilinear representation of space raised as many difficulties after Descartes as the spherical representation had after Copernicus. Even Descartes himself never fully accepted the relativism inherent in a rectilinear spatial concept, and he continued to represent celestial phenomena

[2] For geography, the work of Mercator and White is obviously important, while Gilbert's own sources, Cabeus and Peregrinus, are significant for magnetism.
[3] The parallelogram composition of forces supposes that two forces continue to act in the same direction – i.e., parallel to themselves – thereby producing a net effect oblique to both. This is similar to the composition of motions in Descartes' optics discussed above. Simon Stevin, who influenced Descartes' early work on statics, might be an important figure in this context. Descartes. *Oeuvres de Descartes*, X 228.

spherically, around stipulated centers. Responses to these new difficulties led to further iterations of reciprocal adjustments at all levels of physical understanding, even into the present day.

Pointing toward future research, we can outline two very different responses to the challenges of Cartesian space in the work of Christiaan Huygens and Isaac Newton. As we have seen, Descartes never quite solved the problem of place and motion. In a rectilinear representation of space, there are no primitive locations, so it is, strictly speaking, impossible to specify fixed, absolute places. Hence, it is also impossible to specify absolute motions, when motion is defined as change of place. This raises problems of description and explanation, since descriptions usually include a specification of where things are, and explanatory principles often rely on such notions as the quantity of motion or speed of a body.[4] In a rectilinear space, though, these are not simple and clearly defined attributes of a body, but quantities only specifiable relative to an arbitrary reference frame. Descartes' solution was to fudge the concept of place. He adopted two conceptions of the same term – one relative, one absolute – and used each as consistency required.

Huygens, in particular, was not satisfied with Descartes' ambivalence. He recognized the fundamental inconsistency in calling place and motion relative and absolute at the same time. Huygens responded by embracing the relativism inherent in rectilinearity. He precludes any possibility of determining a fixed and absolute sense of place. Place and, thus, motion are strictly relative concepts:

> Both the motion of bodies and their equal and unequal speeds must be understood in relation to other bodies considered to be at rest, even if both sets of bodies happen to be involved in some other common motion.[5]

Huygens rejects Descartes' absolute notion of place. Place and motion are determined relative to "bodies considered to be at rest." A body cannot have a fixed and determinate amount of motion or speed. It can only have a motion relative to something else.

Incidentally, Huygens's relativism allowed the "correction" of Descartes' basic physical principles. Descartes' "Laws of Nature," presented in the *World* and the *Principles of*

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[4] This is at least true of the descriptions and physical principles employed by the philosophers we have discussed.
[5] Richard J. Blackwell and Christiaan Huygens, "Christiaan Huygens' The Motion of Colliding Bodies," *Isis* 68, no. 4 (1977): 575. See also similar statements at Christiaan Huygens. *Oeuvres Complètes*, 22 vols. (Le Haye: M. Nijhoff, 1888-1950), XVI 222 and XXI 507-8.

Philosophy constitute a "contest" model of collision. In a collision between any two bodies, the "stronger" body – i.e., the one with greater "force" of motion or resistance – will "win out" over the "weaker" body. While the concept of "force" is not well developed by Descartes, the general structure of the theory is plausibly coherent if there is an absolute fact of the matter about which of the two bodies is "stronger." This cannot be established using the relative sense of place and motion. Descartes' rules of collision are not independent of the frame of reference, and different physical outcomes are predicted depending on which bodies are considered as moving. Descartes' problematic notion of absolute motion, however, seems to license a distinction between "stronger" and "weaker" bodies. It is supposed to allow an objective characterization of the physical situation. If the concept of absolute motion is granted, Descartes' theory of collisions is (possibly – depending on the interpretation of "force") internally coherent.[6] Nevertheless, Descartes' contest model of collision leads to empirically faulty descriptions of the behavior of bodies. For example, Descartes concludes that a smaller body can never bring about motion of a larger body at rest, a principle that is demonstrably false.[7]

For Huygens, there is no absolute concept of motion, so the "contest" model of collision is untenable. There is simply no way to distinguish the amount of motion or rest in a body. Thus, it is impossible to say which of two colliding bodies is "stronger" or "weaker."[8] All that matters, says Huygens, is the motion of the two bodies relative to one another. Any motion common to both, i.e., resulting from the choice of reference frame, has no physical effect, "as if that additional motion were totally absent."[9] Huygens's basic assumption of relative place implies that the outcome of a collision is independent of the frame of reference. In fact, Huygens assumes this independence of reference frames as an axiom in order to prove his rules of collision – transforming from one frame to another to show one physical situation is equivalent to another already addressed.

As it turns out, of course, Huygens's notion of relative place and motion do lead to empirically valid laws of collision. The point here, though, is that Huygens is motivated by his

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[6] See Garber, *Descartes' Metaphysical Physics*, 241.
[7] Descartes, *Principles of Philosophy*, II.49 p.66.
[8] One can perhaps read an even stronger thesis in Huygens. He can be seen as claiming that there is simply no fact of the matter about a body's state of motion. Thus, it is not just that a subjective observer cannot determine a body's state of motion, but that there is no real state of motion to be determined. This is beyond the scope of our discussion, however.
[9] Blackwell and Huygens, "Christiaan Huygens' The Motion of Colliding Bodies," 575.

dissatisfaction with Descartes' attempt to resolve a conflict between his conception of space, which leads to relativism, and his explanatory principles, which required absoluteness. Huygens comes down firmly on the side of relativism, fully accepting the implication of a rectilinear conception of space. This conceptual stance, though, both forced and allowed him to adjust the basic physical principles used to explain phenomena.

Newton's tack, by contrast, is to fall on the side of absoluteness. Like Descartes, he distinguishes between a "vulgar," relative notion of place "which our senses determine by its position [relative] to bodies" taken as fixed,[10] and an absolute, "immovable" place. This absolute place is not, however, specified in relation to the "superficies" of a body, but to a single, universal reference frame, which is absolutely and objectively at rest. The universal reference frame, however, is merely a stipulation. There is no way to know whether any body is truly fixed with respect to absolute space, "because the parts of that immovable space... do by no means come under the observation of our senses."[11] All we can observe, says Newton, is a body's relative place with respect to other bodies. Thus, even though a body possesses a unique, definite place, this place is not specifiable.

An absolute conception of place, though, grounds an absolute or "true" concept of motion. For Newton, bodies have absolute motions. They move with an objective speed with respect to absolute space. It is not a subjective question whether a body is moving or how fast it moves. However, since absolute space is not accessible to subjective knowledge, it is not possible to determine a body's true motion. Nevertheless, Newton can argue that the "true" motion is a real property of the body which has observable "properties, causes and effects."[12] The motion of two bodies relative to one another, for example, is the observable difference of their unobservable true motions, while the endeavor to recede from the axis is a real effect of a "true" rotation. The objective nature of motion, in turn, allows Newton to use motion to define physical causes of phenomena. Apparent changes in the true motions of bodies are evidence of real physical forces. According to Newton, these forces explain the behavior of the universe.

The existence of an absolute reference frame, even if it is precluded from knowledge, eliminates the relativity inherent in a rectilinear space. Motions are objective features of bodies.

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[10] Isaac Newton, *The Principia*, trans. Andrew Motte (Amherst, NY: Prometheus Books, 1995), 13.
[11] Ibid., 18.
[12] Ibid., 15.

The forces that cause and explain motions, therefore, must also be true features of the universe. The addition of a fixed frame to the Cartesian concept of space licenses Newtonian arguments in favor of a force ontology. That is, the conceptual stance warrants the epistemic evidence for Newton's explanatory entities.

We can also compare Newton's and Huygens's responses to the particular problem of gravitation in the wake of Descartes. Earlier philosophers had explained gravitation, both terrestrially and celestially (though these were distinct problems) by appealing to geometric points or directions *qua* points or directions. Aristotle, and the Scholastic philosophers following him, thought it was "natural" for heavy terrestrial bodies to move toward the center of the universe and for the planets to move around it. In fact, it was elemental earth's tendency to move toward the center (*qua* geometric point) that explained the earth's position at the center of the universe. Epicurus, meanwhile, supposed that all bodies fall "down," where "down" was a presupposed, geometric orientation of space.[13]

Once Descartes adopted a subjective, rectilinear, isotropic representation of space, points and lines lost their causal efficacy. It was no longer possible to explain a body's fall by appealing to a geometric center, since the representation of space rendered such a point conceptually unavailable. Likewise, it became necessary to explain why planets remained in their orbit without assuming they innately respected a central point. An explanation based on an object's tendency toward, away from, around, or along some geometric entity became incoherent in a Cartesian space, where all such entities are arbitrarily determined.[14]

Descartes replaced the scholastic theories of "natural" motion towards centers with his vortex theory. On this view, heavy bodies are pushed toward a central point by the pressure of a subtle fluid rotating around it. Descartes held that all bodies tend to move in straight lines. The rotation of the subtle fluid, therefore, resulted in a centrifugal tendency amongst its constituent parts. Heavy bodies released into the surrounding fluid would be pushed inward by the outward pressure. Thus, bodies tended to move toward the center, not *qua* geometric point, but *qua*

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[13] Some philosophers, including Copernicus and Gilbert, suggested that terrestrial gravity was an inherent tendency of terrestrial bodies to unite themselves with other terrestrial bodies. This suggestion was only mildly satisfactory, as we have seen. It failed to explain the actual location of the earth – that is, why terrestrial bodies congregate around one point rather than another – and, furthermore, why the center of the terrestrial conglomeration moved around the sun. Also, the orbital motion of planetary bodies was not addressed by this theory.

[14] Koyré would label Descartes' move an "abstraction" of space. See Koyré, *Études Galiléennes*, 15, 79. In his terms, Descartes removes the *terminus ad quem* from physical explanations. Koyré, *Études Galiléennes*, 92.

physical center of a real rotation of bodies. This fluid pressure was supposed to account for both the fall of terrestrial bodies toward the earth and the revolution of planets around the sun, since both earth and sun were supposed to be at the center of a vortex.

Huygens accepted vortices as the cause of celestial and terrestrial gravitation. However, he thought Descartes had not finished the job of explaining the operation of a vortex. In particular, he felt that Descartes had not satisfactorily shown how the basic physical principles, governing collisions between two particles at a time, gave rise to the centrifugal and centripetal forces in a rotating ensemble of particles:

> We understand the nature of straight movement well enough, and the laws which govern bodies in the exchange of their movements, when they collide. But as much as one considers this sort of movement, and the reflections which arise from [collisions] between the parts of matter, one finds nothing which determines them to tend towards a center.[15]

Huygens accepted the laws governing the motions of colliding bodies, but he did not see how this essentially rectilinear behavior could bring about a centrifugal tendency in the subtle fluid and thus a tendency toward the center in heavy bodies. (In effect, he accused Descartes of replacing the Scholastics' unexplained tendency toward a center with an unexplained tendency away from a center.) As a result, Huygens set out to investigate fluid dynamics in an attempt to explain how the laws of collision could cause centrifugal force, and thus how a vortex could cause gravitation.[16]

In a sense, Huygens's attempt to link the explanation of collisions with the explanation of gravity was also an attempt to unify the spatial framework used to represent collisions with the concept of space applicable to the vortices. Descartes had already linked the explanation of gravitation on earth with the explanation of gravitation in the heavens by ascribing both to vortices. At the same time, he had used the spherical representation of space associated with vortices to describe both the solar system and the earth. Huygens tried to complete the unification by linking the explanation of small-scale phenomena, collisions, with that of global- and cosmic-scale phenomena, the vortices. Since these two sets of explanations were based on

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[15] Christiaan Huygens, "Discours de la Cause de la Pesanteur," in *Oeuvres Complètes vol. XXI* (Le Haye: M. Nijhoff, 1888-1950), 451.
[16] See Huygens's *De Vi Centrifuga* (written c. 1659, published 1703) and *Discours de la Cause de la Pesanteur* (published 1690, though also written earlier)

different representations of space, Huygens's unifying project entailed, in part, demonstrating how the rectilinear framework applied to collisions could "build up" into the spherical spatial concept associated with vortices. The explanatory synthesis required a conceptual one.[17]

Like Huygens, Newton sought to explain gravitation without appealing to geometric centers. To do so, Newton supplemented Descartes' laws of collision with a fundamental physical principle of his own – the law of universal gravitation. He simply argued that all bodies are subject to a force impelling them toward all other bodies that is proportional to the mass of the bodies and inversely proportional to the square of the distance between them. This relationship could be derived from the observed behavior of bodies.[18] Basically, though, this was an end run around the problem. Scholastic physics had appealed to an inherent tendency to approach a center. In the absence of centers, Newton appeals to an inherent tendency to approach other bodies. Thus, all bodies have an inherent tendency toward the gravitational center of a system, not as a geometric center, but as a center of gravitational force. Still, Newton left the ultimate cause of this innate tendency unexplained.[19] To many, Huygens included,[20] it was as unintelligible as the Scholastic "natures."

Newton also unified the small-scale linear space of collisions and cosmic space. Without vortices, however, Newton's cosmic space was the rectilinearly-structured space of Kepler's *vis insita*. In the first proposition of the *Principia*, Newton divides a gravitational orbit into an infinity of instants. In each instant, the orbiting body continues its motion due to its (Cartesian) linear inertia, which takes the place of Kepler's *vis extrinsica*. To this movement is added a motion to the center, caused by the (percussive) action of the gravitational force, which is the equivalent of Kepler's *vis insita*. The path of the body is then derived from a composition of the two motions. Newton's analysis, however, relies on the assumption that the two motions of the body – one inertial, the other gravitational – remain parallel to themselves throughout the instant in question. In other words, the motions are not directed toward any presupposed center, but are directed along presupposed, self-parallel orientations – the parallel actions are the *same* action. Newton, like Kepler, assumes that the intrinsic or gravitational force moving the body toward the center respects linear directions, not centers. This assumption allows Newton to treat the motion

[17] At this point of my research, I do not know how successful Huygens was in this regard.
[18] If one assumed an absolute space, as noted above.
[19] Newton, *Principia*, 442.
[20] Huygens, "Discours de la Cause de la Pesanteur," 445-6.

of the planets using the same rectilinear framework used to represent collisions, at least in each instant of the orbit. This Newtonian "synthesis" of small-scale and cosmic spaces depends, in part, on the fact that the space used to represent inertia and percussive action, taken from Descartes, has the same rectilinear structure as the one he uses to represent the gravitational force, taken from Kepler. Their shared rectilinear framework renders the forces commensurate.[21]

Furthermore, by treating all phenomena using a single spatial framework, Newton could use one set of explanation in all parts of space and at all spatial scales. In other words, the conceptual synthesis allowed Newton to assimilate all phenomena, both celestial and terrestrial, into one explanatory scheme – the Newtonian Synthesis as commonly understood. The shift to a rectilinear representation of space, for both terrestrial and celestial phenomena, was an essential and enabling feature of Newton's remarkable physical system.

7.5 CONCLUSION

The reciprocation between concept, description, and explanation continued long after Descartes, Huygens, and Newton. Philosophers like G. W. Leibniz and Immanuel Kant and physicists like Michael Faraday and J. C. Maxwell contributed to the development of representations of space as part of physical understanding. In the modern era, the links between concept, description, and explanation have become more explicit, especially in the context of general relativity. In a sense, solving Albert Einstein's field equations is determining the spatial concept – the shape of space. The shape of space, meanwhile, explains the inertial and gravitational behavior of bodies. Concept, description, and explanation continue to interact, but now they do so within a single, overarching metatheoretical framework. Philosophers of science, meanwhile, are also concerned

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[21] For the relation between Newtonian to Cartesian space, see J. E. McGuire, "Space, Geometrical Objects and Infinity: Newton and Descartes on Extension," in *Nature Mathematized*, ed. William R. Shea (Dordrecht: D. Reidel, 1983). Note that Newton's views on space developed through his career. His early *De Gravitatione* privileges closed geometric solids, including spheres, as the basis of spatial descriptions. Later, Newton bases his treatment of space on straight lines. See Isaac Newton, "De Gravitatione et Aequipondio Fluidorum," in *Unpublished Scientific Papers of Newton*, ed. A. Rupert Hall and Marie Boas Hall (Cambridge: Cambridge University Press, 1962).

with the concept of space as it is used in today's physical theories. Our work is a natural prelude to theirs.[22]

Representations of space have long been significant in the development of physical understanding. Physicists and philosophers have adjusted explanations in response to conceptual changes, and concepts have been altered in response to explanations, while descriptions have evolved according to both. The result has been the continued advance of the understanding of nature. The present project has only addressed this reciprocation as it occurred in the first half of the seventeenth century, and this only superficially. We hope, though, that this is merely the start of a broader and deeper examination of representations of space throughout the course of mankind's investigation of nature.

[22] This is only meant to be suggestive. I do not pretend to be a philosopher of space or relativity. See Albert Einstein, "Foreword," in *Concepts of Space*, by Max Jammer (Cambridge: Harvard University Press, 1954).

APPENDIX

EXCERPTS OF LETTER FROM JOHANNES KEPLER TO DAVID FABRICIUS, 11 OCTOBER 1605

The following consists of a translation of significant portions of a letter written (in Latin) by Johannes Kepler, at Prague, to David Fabricius, in Osteel. This translation is based on the transcription found in *Johannes Kepler Gesammelte Werke* vol. XV.[1] I have placed the figures in the text as they appear in the transcription. The original manuscript is to be found in the Pulkowo Observatory, Kepler Manuscripts X. For a more complete discussion, see the chapter on Kepler, above.

[*247*[2]] 11. You will have all of what I have professed up until now in my Mars [Commentary]. Seeing the distances constructed from a perfectly circular eccentric sin in excess, both in [the distances] themselves, and in their effect on the prostaphaerises of the annual orb, and in the equations of the eccentric, just as much as my ellipse (which varies very little from the oval), which I described numerically to you above, sins in the defect: very rightly I have argued this [following] way. The circle and ellipse are from the same genre of figures, and fail equally in different ways, therefore the truth is in the middle, and the figures between ellipses are nothing but ellipses. And thus, the path of Mars is definitely an ellipse, the leftover little moon shape [*lunula*] [*248*] of half the width of the previous ellipse. The size of the lunula, though, was 858 of 100000. Therefore, it ought to be 427 parts [wide], which is exactly the shortening of the

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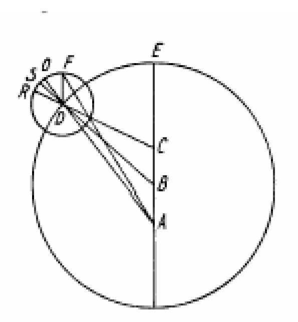
[1] Kepler, *Werke XV*, no. 358, 240-80. Special thanks are due to Paolo Palmieri for his assistance in preparing this translation.
[2] Page numbers correspond to the pagination in the *Gesammelte Werke*.

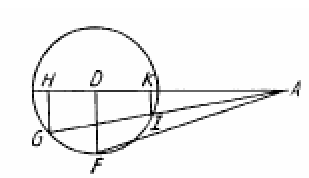
distances in the middle longitudes, constructed from the perfect circle. This I say is the truth itself. And see how meanwhile I was thrown back into mental wanderings and new labor. Indeed, see how miserably I wander onto the truth, following that [maxim], one who never doubts, is never certain about anything. The former ellipse with shortening of 858 had a natural cause, namely that the said center of the epicycle slowly advances, when the planet is turned in the apogee of the epicycle, [and] faster below. But, the epicycle itself advances equally in equal times. This was moderately agreeable to nature. Now, however, if the ellipse with shortening of 429 were to obtain, I am missing the natural cause. For it was absurd that the center of the epicycle advances unequally, and the circumference of the epicycle neither equally, nor unequally around its own center, but with a peculiar inequality, which would be at least half the inequality of the center. Indeed, I speak now with you not out of my Commentaries – that is, on the basis of natural reasons – but from Ptolemy and the ancient astronomy, as you may understand me.



A is the sun, AE the line of apsides, AD 100,000, AC 9264. And C is the point of equal motion of D, the center of the epicycle. And thus, if the line CDR equals the true apogee of the epicycle, then the path of the planet follows a perfect circle. For DF is led parallel to AC, RDO equals ADC, and ODF equals DAE, and RDF equals DCE, the mean anomaly, because the period [return] of the epicycle and concentric are equal, here clearly turning by equal motion, which is in itself unequal. Now, joining F and A makes a line so long, that if from C a perfect eccentric is described with radius AE, it will indeed cross at F. And also, this hypothesis, which I proposed in 1602, is false. On the contrary, C remains the equant point of D, line ADO follows the line of true apsides of the epicycle, and O is the true apsis of the epicycle; as it is DCE, the mean anomaly, is constituted equally by ODF, and DF is inclined to AC, which is equally if the said epicycle moves equally in equal time around its center. Now this is the very close hypothesis, [*Kepler's marginal comment*: NB this in the Commentary.] which I use in 1603. And which you hold in 1604. And which had a tolerable natural cause. But, deducing from the first [*249*] excess and the second defect, CA is to be halved or bisected in B, and BDS is to be the true apsis of the epicycle, and thus C hitherto the equant center of D. But now, SDF is equal to DCE, the mean anomaly, and DF is less inclined to

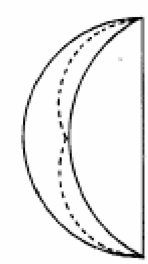
AC than before. And with this hypothesis, the distance of F from A is now closer to the truth than before, and FAE is closer to the true coequated [anomaly]. Indeed, I say "closer," but not "true," so as to allow the construction of the computation of the physical equation.

Still, this hypothesis (as I may go on in the laying out of my ratiocinations) did not satisfy me, since the point B lacked a natural cause. For point C will have a natural cause, which is to say AC and DF are made equal, which amounts to, if I may say, the distances being [proportional to] the delays in equal arcs of the eccentric. On the other hand, another thing attracted me to the natural cause: this, of course, which I have seen helping the secant of the greatest equation of the epicycle.



AF, that is, (at angle 5 degrees 18 minutes) would be 100429. And thus, FA is longer than DA, by 429 small parts. And because the distance FA follows from the re-utilization of the perfect eccentric, and 429, found above, is exactly the shortening of distances for the true hypothesis. Therefore, if we substitute FA for DA, we have the right distances in the middle longitudes.

At once, I seized on this for the natural hypothesis: the planet does not rotate in the circumference of the epicycle GFI, but librates in the diameter HDK. And now I constructed the distances and the whole table of equations from this.



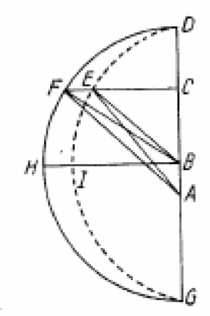
Hic extima linea repraesentat circulum; intima Ellipsin, punctata uerò buccosam

But yet, I am a wretch. This Easter holiday at last I tested the thing, which, if I had been considering [properly], I would have been able to remember that earlier in my commentaries it was demonstrated that this kind of path of the planet does not compose an ellipse, which my above argument stated, but in the octants expands in cheeks from the ellipse towards the perfect circle. The argument, therefore, has been all wrong: Libration in the diameter of the epicycle equals the ellipse in the middle longitudes and in apsides, therefore equals it wherever. False! And thus this [hypothesis], as before in the old false hypothesis, performs neither the duty of distances nor of eccentric equations. O fruitful society of both things, which never does not direct me into total perplexity. Therefore, I now have this, Fabricius: the path of the planet is truly an ellipse, which Durer has similarly called an oval, or certainly insensibly different from some ellipse. I have computed the eccentric equations [*250*] in acronychal positions, they take the task to the nail; of the distances I would say almost the same, but the method of examining them is somewhat more lax, which always leaves me about 100 parts in doubt, while the observations are optimal. Indeed, you know the best observations can err by one minute. But one minute vitiates the distance immensely, if the planet is near [the sun] or [opposition with the sun]. This, though, you will have as certain, that we come near the truth. And thus the whole hypothesis I will delineate to you.

Given the mean anomaly (by noting the location of aphelion, from which you now remove 5 minutes, and noting the mean motion, which remains the same) the eccentric anomaly is sought, directly or by tabulation. From tables thus: the maximum equation from equations of triangular areas, which is 5 deg. 18 min. 30 sec., resolve this into seconds and divide this sum for all steps of the eccentric anomaly, then reduce back into the steps; and put [them] to your steps of the anomaly such that at 90 deg., the eccentric anomaly will be 5 deg. 18 min. 30 sec. Therefore, at 95 deg. 18 min. 30 sec. of mean anomaly, 90 deg. of eccentric anomaly is selected. Indirectly, the same eccentric anomaly is thus selected. While before semicircularity it is always less than mean anomaly, afterwards more, by conjecture you preconceive how much smaller, such that if a mean anomaly of 48 deg. 46 min. 0 sec. were given, I would conclude that the eccentric anomaly would be 45 deg. This sine in sums of seconds 5 deg. 18 min. 30 sec. multiplied and by 100000 divided, should give me 3 deg. 46 min. 0 sec. if I calculated well, at 45 deg, and 3 deg. 46 min. gives the mean anomaly.[3] Have the eccentric anomaly as you multiply the sine of 45 deg., 70711, in 430 parts, giving 303, which you take from the sine 70711, leaving 70408.[4] You take therefore the sine of the complement of the eccentric anomaly, to it you add the eccentricity 9264 in the upper semicircle of the eccentric, that is from 270 to 90. You subtract in the lower, from 95 1/3 to 264 2/3. Or from the eccentricity you steal the sine of the complement if it is less. Then let it be that to that shortened sine, this sum or remainder, thus the whole sine to the tangent, which gives the angle of the coequated anomaly. This will be either the coequated

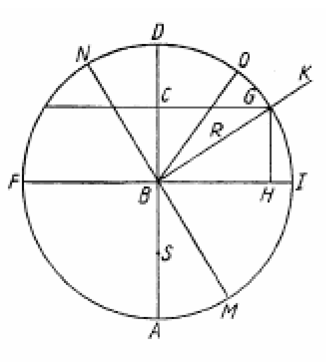
[3] 5 deg. 18 min. 30 sec. x sin 45 = 3 deg. 46 min.
[4] 430 x sin 45 = 303. 430 is the excess over 100000 of the planet sun distance at 90 deg. eccentric anomaly.

anomaly itself, or the excess of the coequated over a semicircle, or otherwise the complement of these to the semicircle, for the thing born. Of this angle, you cut off a secant, and let it be that as the whole sine is to the sum or remainder, thus this secant is to the genuine distance from Mars to the sun.



This is fundamental in the ellipse and circle, that as the diameter of the circle to the shorter diameter of the ellipse, thus is FC to EC along the whole semicircle. And also arc FD is to arc ED. And so although DEG is shorter than DFG, nevertheless if DEG is allowed to be called 180 deg., then part DE [*251*] is allowed to be called that which DF really has. Therefore, the eccentric anomaly here is DE. Thus not arc DBE, which had deceived me from the time of Christmas to this Easter time. FC is greater than EC as area DFA is to area DEA. Therefore, although area DFG is greater than 1800000 (which I prove myself), nevertheless if area DEG retains the same name as area DFG, parts DEA and DFA will also have the same name, similarly DEB and DFB, as well as AEB and AFB, the area measuring the part of the physical equation. Therefore, if the circle is given, then DF, or DBF, will be the eccentric anomaly, and area DFA will be the mean anomaly. But now, in the ellipse, not DBE, but DE is the eccentric anomaly, and area DEA is the mean anomaly, and angle DAE is the coequated anomaly, and AE the true distance.

Finally, you use the proportion of the orbs, which is 100000 to 152500. If, then, you see that this is to be useful at all points, you will be able to use 152400 or 152600. In [Pisces] I think two or three minutes are lacking by this and by the old hypothesis, perhaps because of a false assumption in [Pisces]. For [Mars] will have the greatest latitude at [opposition] in 93 years. But I do not see how I may be able to correct [this], such that the remaining points are inflicted with no detriments. And yet these 3 minutes can [at opposition of sun and Mars] effect an appearance of 10 or 11 minutes. But there is also that which I desire in this hypothesis: namely that, though [I am] stretching [my mind] all the way to insanity, I cannot fashion the natural cause why Mars, which with such great probability should librate in the diameter (indeed, the thing was reducing so beautifully to magnetic virtues for us), should rather want to go in an ellipse or some path close to it. Nevertheless, I think magnetic virtues may not always respect the sine, but something somewhat different. [*Marginal comment*: Such a fool I was not to see how to construct the same distances with libration.]



The eccentricity smacks entirely of magnetic force, as it is in my commentaries: so that if the globe of Mars has a magnetic axis, one pole seeking the sun, the other fleeing, and this axis were pointed in the middle longitudes, then as long it is turned in the descending semicircle, maximally in middle longitudes, it would point the seeking pole toward the sun, and thus always approach the sun, but maximally in middle longitudes, not at all at apsides. And then in the ascending semicircle, it flees the sun equally. And thus perhaps (indeed allow me, very happy Fabricius, while I work to speak with you, to profit from my exercise) the law is something else, by which the magnet flees and follows something, than the sine. Indeed, I posit, DFA is [*252*] the body of Mars, round, and DA the magnetic axis: I have thus far posited that Mars is thus arranged, as having the sun in line BG, which is to say in K; this may be the proportion of its speed in approaching to the speed in approaching when it has the sun in D, which is the proportion of sine IH to sine IB. And in this position IH is discovered exceedingly small, indeed, almost exactly the measure of the part of libration. What if, therefore, thus this is the true proportion of speed, as HG to BD? Or some other way. For if we adhere to this magnetic hypothesis, we will be forced by certain reasons to ask for some other ways. First, if the sun and the magnetic pole DA are in the same plane of the eccentric, we ought not be disturbed by a certain suspicion, as if what is said about the solid body of the planet's globe differs for that of its greatest circle. For I posit, as we have posited: it is understood that the whole globe can be divided in infinite parallel circles, from greatest to smallest on both sides, all of which are equally disposed toward the Sun: and thus the proportion will remain the same [after] multiplying the terms. Therefore, circle FDIA is some circle in the body of the planet parallel to the eccentric, and FDI is the seeking semicircle, FAI the fleeing, the Sun in turning BI makes equilibrium, because of the semicircle, which is observed by the Sun, that is DIA, half DI is seeking, half IA fleeing. Truly the Sun in BK, semicircle NGM is considered, in which NI is the seeking parts, IM the fleeing. Put IO equal to IM, therefore IO will be annihilated by IM. NO of the considered [part] is left as measure of the seeking faculty. Where MI is the complement to

IG, the departing of the planet from apogee, and IO similar [to MI]; therefore arc ON is twice to IG the departure from apogee. Because if the seeking parts equally large always seek equally wherever angle DBK, now the departure from apogee will measure the seeking entirely, and the shortening of the distances from [Mars] to [sun] will be equal in equal times. But the strength of the angle is also to be considered. For the Sun turning in BI, although nothing is operated on AI fleeing, nevertheless DI does not seek, because angle DBI has no strength, this is because parts DI are not directed to the sun in respect of their line of virtue BD. But here I hesitate to project the measure of the angle as cause of the strength. For perhaps the complement of angle DBG, GI, is measure of this? I think not. For when DBI begins to get smaller, then more it is useful to that, to the strength of the seeking, some small part of the shortening, [*253*] than while it is all spent. Therefore maybe IH measures the strength by the sine DBG? But this now is much more repugnant to what I have said. [*Marginal comment*: This is false.]

[…]

13. I return to [Mars] after some weeks' interposition. Let the same figure of the body of the planet as above be proposed. I have said similarly above, the planet is considered as a globe or as a plane circle; now also I say this, it is considered as a plane circle or as a line. For, from Gilbert the Englishman, it is certain, and also [it is certain] in itself without his authority, magnetic virtue extends in a right [line]. Whereby the globe is feigned to consist of infinite plane circles, parallel to the eccentric, of which each is the same reason, thus because of this rectilinear virtue, the plane circle consists of infinite right [lines], of which likewise each is the same reason. Therefore the body of the planet can be thus considered, as any right [line], since none of the others impedes, as above I constructed falsely. Therefore, let AD be the magnetic axis, fleeing in A, seeking in D, representing one of the infinite right [lines] of virtue in the body of Mars. But let B be [*254*] the middle point of AD, sun in BI, the said approach is the cause such that the flight does nothing, because A and D are in equal operation. Therefore, this is like equilibrium. See my *Optics*, chapter I. Now let the sun be in BGK. And by the center B and the distance BD, circle DG is delineated, and from G, let the section of the circle with the perpendicular line from the sun to DA be led. If therefore GB is the support and AB, BD the arms of a balance, as DC to CA will be the strength of angle DBG to the strength of ABG. And

so this flight is as much as DC, and the seeking as much as AC. Take from AC the equal of DC, which is AS. Therefore SC is the measure of the seeking, and AD the measure of the seeking at no angle. And as AD to SC, thus BD to BC or GH. Therefore the sine of the digression of the planet from apogee or perigee measures the speed of the approach.

This is geometrically demonstrated and most certain. And thus if our principles are correct, all the libration will follow the law of the sine of the digression from apogee. But because experience and the ellipse by experience very certainly made firm wants the libration to follow the versed sine of the digression from apogee, that is not by GH but by HI, therefore our principles are necessarily to be changed. We have substituted, in effect, for GH the perpendicular HI. And thus, therefore, in principles we ought to accept for AD the perpendicular FI. In other words, one must say that the planet is at apsides not when its magnetic axis is perpendicularly inclined to the line from the sun, but when it is united with it (if it can). While although I am unable to reconcile my first intuitions with the magnetic virtue with the appearances, nevertheless it strikes me in a wonderful way. For in my Commentaries, this objection has been left [unanswered]: If the planets produce eccentricities by a magnetic virtue with their axis directed towards the same parts of the universe, the Earth will do the same. But the axis of the Earth is the only one, which is pointed from [Cancer] to [Capricorn]: it falls in this direction in the summer, and winter. Around this the whole remaining body is turned daily. Therefore, the apogee of the earth is fixed in 0 deg. [Aries], 0 deg. [Libra]. But it is seen to be otherwise, and indeed not in [Aries], [Libra] but in [Capricorn] (because the Sun is in [Cancer]). What was truly in [Gemini], [Sagittarius]. To this objection I can respond nothing except this, similar things will be similarly demonstrated, [but are] not clearly the same thing. Now, this is testified by experience, the line of apsides meets the line of the axis directly, therefore the apogee of the earth is stable in 0 [Capricorn]. Where one part of the objection is solved. Of the other part I will respond thus: in the maximum equation around 0 [Aries], 0 [Libra] the place of the Sun according to Tycho varies not at all, or the apogee is found at 0 [Capricorn] to 5 1/2 [Capricorn]. In [Capricorn] and [Cancer] the error is some 11 minutes but the error cannot be derived from declination: At a grade of 45 deg. (which is the observed declination to be extracted from the solar theory) some seven minutes of error in the place of the Sun, if the apogee is transposed by 5 ½ grades, but the seven minutes can be admitted, if in the declination of this place, 2 minutes [*255*] of error may be granted in the observing. Because if Tycho says, the declinations vindicate themselves of error, not as much as of 2 minutes, but clearly of a part of one minute: therefore I will be able to deny it, either because the parallax errs in the smallest, or the obliquity of the ecliptic. I will object to him the positions also in middle longitudes. For year 1588, 3 March, the eclipse in [Pisces] shows [it] fixed 7 minutes more forward than in Tycho: and this makes it [agree] with the Landgrave. Or I will perhaps say that the center of the Sun or Earth in annual revolution does not remain very perfectly in the same plane, from the same great circle, as neither [does] the Moon in monthly [revolution]. Of Mars thus I have carefully thought, if its latitude argues an entirely constant inclination. But why if this is the cause, why after five years can I not yet obtain, as operations of an instituted method distances to [Sun] exhibiting agreement to themselves. For meanwhile the place of [Sun] has been assumed to be known most certainly. But why, by the ancients, was the apogee is placed in 5 ½ [Gemini]? They, therefore, are said to have erred by 49 seconds in the place of the sun around apogee. This, when they will have been used in the observation of the imperceptible solstices.

But before I sing the triumph, I must think about the physical cause, if it is possible that, as the magnetic axis is constructed at apogee, it remains in the direct line from the sun? For what is it, which does similarly, that we may ascribe the cause? [*Kepler's marginal comment*: N.B. in the Commentaries.] The earth, in Aries, is turned about its axis pointing north toward the region of the sun in the center, away in Libra. What therefore is the cause of this recession, the cause of this accession to the sun? Also in [Aries] and in [Libra] days are equaled by nights in the whole world. In [Cancer], [Capricorn] parts of the globe lack light. What, then, this cause of approach? But set aside this present question [and] bring us back to the scheme of the body of Mars. Two words: 1. That the versed sine IH measures the portion of the libration is testified by experience of observation. 2. The right sine GH with the vigorous demonstration given in the Optics, measures the force of approach, or of the libration. These two I have thought until now to be contrary, but it seems they are not. For one thing is the measure of the strength of the libration, another thing now performs the measure of the parts of libration. There, IF represents the total libration, IH the part comprising the eccentric anomaly signified by IG. Here, DB represents the maximum strength, GH the strength of the moment in angle GBI. But DB does not signify all the strengths combined, thus GH does not all the strengths for the whole arc of the anomaly GI. But if you collect the sum of the sine of 90 which is 578,943,140, this is the measure of the strength, which indeed is the common effect of half the libration or BI. Thus therefore also if you collect the sum of the sine from all the steps in GI, this will measure the portion of the performance of the libration, which if it produces a line so long, [*256*] as long as HI, the versed sine, from which experience stands, then we have reconciled experience with the demonstration of the balance. Let us see. Firstly, GI is 30 deg. The sum of the first 30 sines is 79,259,831. If 578 etc.[5] gives 100000, then 13,691 follows from 792 etc.[6] But the versed sine of 30 is 13,397, a very small difference. Secondly, IG 60 deg. The versed sine IH will be 50,000, but the sum of 60 sines is 290,801,743, which is a little bit more than half of 578 etc.,[7] which is 289,471,570.

Therefore we have adduced the thing in the senses into closeness with the best reasons. We conclude, therefore, the body of the planet must be considered as if it were magnetic, which approaches or flees by the law of the balance, and the diameter of virtue points in the middle longitudes. That truly shattering objection that the axis of the earth does not remain in the line of apsides yet extends thus for the planets we relinquish. I will add but this non-geometric [point]. In the beginning, when the sines are small, and the pick out small [parts] of the libration, the versed sine is less than half the little sum of librations from the collected sines, as the sum of 90 sines, 5789 etc., gives 100,000, because the sine of 1 deg. – 1745, 30 follows.[8] Against the versed sine of 1 deg. is 15, half. Out of which I learn, which elsewhere I have now tested, the task is not to sum the collected sines and then by the rule of Detrus to operate, the task is only that I may obtain the squares of right sines by some artifice. For the proportion of them is the same, which [is] of these sums. But you ask how I obtain the squares of the right [sines]? This I will teach you with the river derivation from the source of Bürgi. The versed sine of some arc is half the square, of the subtended complement, last five rejected. The arc is 60, sine 86,603. The verse 13,397. Complement 30 deg. Half 15 deg., sine 25,882. The double 51,764 is the chord of arc 30, the square you will find 26,794,[9] the double of course of 13,397 itself.[10]

[5] i.e., 578,943,140.
[6] i.e., 79,259,831.
[7] i.e., 578,943,140.
[8] This comes out to .3, not 30.
[9] The square of 51,764 is 2,679,400,000. "Reject" the last five places, leaving 26,794.

[…]

[*259*] 21. I might have tentatively denied any error in the birth of Cancellarius, but I am not yet able to admit [it]. Because truly you say the year 1595 7 Dec. hour 7 p.m. was a similar position, and because of the failure to you around middle longitudes to elicit the distances, and because the case seems to you to be difficult, I will declare to you a better, and now most correct precept in this example, you will judge it from amongst my earlier forms.

Because we are around middle longitudes, I conclude the area of the triangle of equation to contain 5 deg. 19 min. 10 sec. Then the complement of the eccentric anomaly may be 86 37 10. Let us see if I conclude well. The sine of 86 37 10 is 99,826, the area of the maximal triangle is 5 deg. 19 min. 43 sec. for an eccentricity of 9300. This is 319 first or 19,183 second, which multiplied into the sine 99,826 give 19,150, which is 5 deg. 19 min. 10 sec. clearly as I concluded. But you say this is not geometrical, and who could always be such a happy conjecturer? A true objection, but it suffices for me that a geometrical table can be constructed from given eccentric anomalies, which I have had for some time, and from where I have brought this so happy conjecture. From the same I can immediately say to you the complement of the coequated anomaly would be 80 42 40. And the distance 100,548. But the example is woven of calculations from tables. Therefore because the complement of the eccentric anomaly is 86 37 10, half of the above libration is released, remaining 3 deg. 22 min. 50 sec.

80. 42. 40.
37 24
81 19 4
14
81. 18. 50.

Here I discover 547 is to be added to the radius, and thus I have the just distance. Now the three sides in ADC are given, to use any one for discovering angle A. In principle the order is to inquire DC, which is sine 99,826 diminished by part of 432, answering to the sine. And then use DC and DA in order, and afterwards to inquire AC from AD, DC, but I do not see the need to inquire DC. It suffices for us AC and AD,

Sinus 5878
9300
17|63400
529|02
547

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[10] This is an exceedingly bizarre derivation.

with AC is more simply given. Therefore AB 9300, produces C 8 40 56. Therefore angle A 81 19 4. The eccentric place 7 40 10 [Gemini].

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