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Science And The Systematicity Of Nature: A Critique Of Nancy Cartwright's Doctrine Of Nature And Natural Science

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SCIENCE AND THE SYSTEMATICITY OF NATURE:

A Critique of Nancy Cartwright's Doctrine of Nature and Natural Science

by

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ABSTRACT

Whether nature is or is not systematic sounds at first like an idle metaphysical question, but considered in relation to (i) the aims of science and (ii) the methods of appraisal of scientific theories, it can be given clear (and quite plainly empirical) content. It is also necessary to ask the question in order to study (iii) the relation of causation, laws of nature, and theoretical structure.

(i) *Aims*. The doctrines (1) that science aims to provide explanations, (2) that science achieves success in this aim, (3) that explanation involves unification, and (4) that the principles on which explanations, properly so-called, are based, must be true, together imply that nature is a system. For a kind of explanation she calls "causal", Nancy Cartwright affirms (1), (2) and (4) but denies (3); for a kind of explanation she calls "theoretical", Cartwright affirms (1), (2) and (3) but denies (4). I show by historical examples (in particular, in the work of Copernicus, Kepler, Newton, Maxwell and Einstein) that Cartwright's distinction between "theoretical" and "causal" explanation is often impossible to make out. I show, largely through discussions of Galileo and Newton, that Cartwright has a misleading view of the role of idealization in physical science, a view apt perhaps for physics before Newton, but not for Newtonian physics. I use my historical case studies to undermine numerous specific sceptical arguments by which Cartwright supports her novel conception of "theoretical" explanation.

(ii) *Methods*. I argue that the Newtonian, "bootstrap" method in terms of which Cartwright reconstructs low-level experiment- and measurement-based inferences to specific causal conclusions has its clearest and most cogent applications in inferences to high-level theoretical conclusions. Newton's method, however, *presupposes* that nature is a system. Nature must be systematic in order to be well suited to study by the bootstrap method. I argue that the method has been notably successful, and that it consequently is appropriate to assimilate the method's substantive presupposition concerning natural systematicity to what has been learned from the experience of the method's successful application, and to say that we have evidence that this presupposition is true, that is, evidence that nature is systematic.

(iii) *Causation, Laws, and Theory*. Against Cartwright I defend a top-down, anti-metaphysical conception of this relation, and an "internal realist" conception of theoretical structure. By highlighting some facets of the mathematics appertaining to fundamental physical laws presumed true, I argue that certain phenomena concerning scientific practice from which Cartwright's metaphysical view of causes gains apparent strength in fact are conformable to my own account.

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Preface

Nancy Cartwright's conception of what empirical science is and how it develops and explains things involves the thesis that nature is unsystematic. In this dissertation I criticize Cartwright's conception of what empirical science is, how it develops, and how it explains things, and I defend the thesis of the systematicity of nature against Cartwright's objections to it. My arguments are grounded in case studies from the history of physics, in empiricism and pragmatism, and in metaphilosophical reflections on the aims and methods of philosophy of science.

Whether nature is or is not systematic sounds at first like an idle metaphysical question. It connects, however, with important issues concerning the aims of science and the methods of appraisal of scientific theories. These connections can give it a clear content, and can make it a worthy subject of discussion by philosophers of science.

Cartwright thinks that we must deny that nature is a system in order to account satisfactorily for the actual practice of science. In this connection with scientific practice, Cartwright's thesis of the unsystematicity of nature is clear and contentful; I argue, however, that what it says in this connection is mistaken. Cartwright also connects her thesis with what on her view are genuinely metaphysical issues concerning the truth or falsity of fundamental physical theories, and concerning the relation of theory, laws, and causes. Because she holds that nature is no system, Cartwright spurns the traditional (regularity-based) empiricist, epistemic account of laws and causes. In its stead she develops an overtly metaphysical conception of causation and a sceptical view of laws. In general, Cartwright shows no compunction to avoid metaphysics. She thinks that there is much that is genuinely metaphysical that a philosopher must accept, as a price worth paying for the sake of adequately reconstructing the practice of science. I shall oppose this view. I shall argue that we can do without, and should do without, all the metaphysics that Cartwright sees as indispensable for philosophy of science. I argue that the thesis of the systematicity of nature is (in its connection with methodology) *empirically* contentful and *empirically* grounded in the history of science.

Cartwright has developed in two books two somewhat different, systematic conceptions of the character of scientific reasoning, conceptions that are partly new, partly old, and support a novel form of scepticism about theoretical thinking in science. My principal disputes with Cartwright concern the systematicity of her view of science, and the sceptical thesis that carries with it her thesis of the unsystematicity of nature. I also contend that Cartwright has given insufficient thought to the question, "what warrants scientific reasoning's taking a given character?", and that her sceptical position concerning theoretical laws in fact leaves her unable to answer this question satisfactorily. For my part, I answer this question as does J. S. Mill: I believe that all facets of scientific methodology are themselves a part of scientific knowledge. What it is to reason aright in empirical science is something that, granting that it has been discovered, science has discovered, empirically. We philosophers have no basis except the development to the present time of science on which to judge what methods work in science. If by some method for empirical science nature seems comprehensible, that fact is incomprehensible; nothing can explain it, it can only be shown by science. In other words, if a method works, that fact is brute. There are no higher standards than experience against which to evaluate method. Empirical science has no foundation in pure reason, and no option save to employ the methods that by its present lights have borne the best and most abundant fruit.

On Mill's conception (and my own), methodological principles, the presuppositions they carry and other highest-level elements of scientific thought may be among the empirically best confirmed elements in the entire corpus of presumed knowledge. But the way in which these elements are made to confront experience and hence confirmed is not essentially different from the way in which lower-level judgments are confirmed on the basis of measurement or experiment. Cartwright thinks all high-level theory is false, and thus she clearly thinks that high-level principles are not at all well confirmed empirically. Although Cartwright describes her methodology as empirical, it is impossible to tell from her writings whether she is an empiricist *about* meth‹.d. She does assert that the touchstone for philosophy of science is the actual practice of science. However, Cartwright does not say whether or not in her view the methodological principles of interest to philosophers that are reflected in this practice are also scientific principles and empirically based. Her picture is very different from Mill's picture of the character of scientific reasoning, for she implies that scientific reasoning is different at the level of theory from what it is at the level of experiment. According to Cartwright science settles upon high-level theoretical principles in a way different from how it settles upon lower-level principles. In the former sphere, the task is that of giving fictive theoretical explanation, and inference, properly so-called, is not in question. All the true inference and methodological action (she contends) concerns inference to low-level "causal" conclusions.

I assume that Mill is correct not only in his empiricism about method, but also in discounting as he does a sceptical worry that may seem to ensue from empiricism about method. Elements of method concern conditions of science's very way of answering its questions empirically. So they seem to be above empirical scrutiny. Does not the view that they must be empirically discovered imply a problem of complete foundational circularity in science, and thus that the whole edifice of science could be wrong, including the principles of method that suggest to us that science has made much progress? Mill dismisses this worry essentially in the manner of pragmatists. The sceptical worry just mentioned is perfectly unpragmatic. Pragmatism enjoins us always to set out from where we are. In investigating knowledge, we must start with the knowledge we already have. *Wir sind*, as Otto Neurath was fond of commenting, *wie Schiffer, die ihr Schiff auf offener See umbauen müssen.* The sceptic fails to see this, and sins against the injunction always to set out from where one is. The sceptic thereby falls into irrelevance --or, in Neurath's picture, into the sea. It is only pragmatic to suppose that science has achieved *some* knowledge in coming to its present set of doctrines. The pragmatic view of science grants that science is actual, so that we are, like Immanuel Kant, to study not whether but how it is possible. In that case there *is* right reasoning in science. But what is its character? Because there is (or so I have assumed in following Mill) no Plato's heaven nor Form of scientific rationality to be found there, we have no way of simply reasoning our way to, or intellectually "recollecting", what it is to reason aright in empirical science. What it is to reason aright in empirical science is something that, granting that it has been discovered, science has discovered, *empirically*. Philosophy of science simply brings to reflective consciousness what is thus empirically based and has passed into the reflexive practice of science. From this pragmatic perspective on method I shall criticize Cartwright, not because her scepticism is based on the problem of foundational circularity (it is not, but rather on a different worry concerning theory which I shall consider in due time), but rather because her scepticism concerning high-level theory seems to preclude our understanding principles of method in this pragmatist and empiricist way, and so makes their warrant mysterious.

To those methodological elements that Cartwright discerns in the practice of science I believe I only add. Yet by adding to Cartwright's account both additional elements and additional scope for the elements she identifies as important, I undermine her case for her novel sceptical view of scientific theories. I argue along the way that philosophy of science cannot legitimately be made as tidy as Cartwright would like to make it. In the methodological toolkit of the philosopher of science I believe there must be a variety of somewhat mismatched tools with partially overlapping capabilities. More importantly, I argue that the methods (involving deductions from phenomena) that Cartwright discerns in measurement- and experiment-based inference to special low-level causal conclusions, and implies are employed strictly in that domain, in fact were learned in the context of high-level theorizing, a context in which they remain of signal clarity and epistemic importance.

In this dissertation I champion, on appropriately historico-empirical grounds, the scientific method of Isaac Newton, within which deductions from phenomena play an important role. Clark Glymour has developed, under the rubric of "bootstrap empiricism", a slightly more general account of the relation between theory and evidence that Newton illustrated for us. According to bootstrap empiricism, rational systematicity of theory arises as an epiphenomenon of the preference for the best bootstrap-confirmed theories. Precisely for this reason, bootstrapping (or, what is nearly the same thing, Newton's method) presupposes the rational systematicity of nature. The thesis that nature is well suited to study by this method carries the substantive metaphysical-seeming implication that nature is systematic. But I argue that this implication is not metaphysical: it is supported by empirical evidence, viz. that science in all its experience has chosen this method as one that works.

Support for bootstrap empiricism among philosophers of science has been strong: philosophers of science have discerned its application very widely in the sciences. Even Cartwright, who denies that nature is systematic, "advocate[s the bootstrap method] ... throughout" a recent book. I shall argue that Cartwright is wrong to ignore the presupposition of this method concerning the rational systematicity of nature. Nevertheless, she has adduced important new evidence for the prevalence in science of Newton's approach. Empirical science, I argue, has no option save to employ Newton's method, not because there is any demand of reason to accept the standards for theory appraisal associated with this method, but rather on the empirical ground that by the present lights of science this method has borne the best and most abundant fruit. The experience from past science that Newton's method works well for science is evidence of the only sort possible that nature is such as to be well studied by that method. Because the method has been notably successful it is appropriate (I shall argue) to assimilate the method's substantive presupposition concerning systematicity to what has been learned from the experience of the method's successful application, and to say that we have evidence that this presupposition is true, that is, evidence that nature is systematic.

Half of this dissertation is historical. I examine some key advances in physics, to show that the methodological character of these advances is very multiform, and such that each of several methodological conceptions illuminates their character as advances. In this way I illustrate that the bootstrap method with its presupposition of the systematicity of nature illuminates what not only by its own lights but also by the lights of numerous alternative methodologies are advances in science. One need not adopt the method's own standards of progress to judge that its application conduces to progress.

My defense of Newton's method (and thus my conception of the warrant of the thesis that nature is such as to be well studied by this method) is clearly much like Mill's empiricist defense of induction. One difference is that I acknowledge the worth of a variety of methodological conceptions (not only deductions from phenomena) insisting that they each illuminate something important about science, a position I call "methodological pluralism". Some basically Kantian elements also colour the empiricist position I defend below: I emphasise a kinship of explanation with theoretical unity, I adopt a Kantian conception of the relation between theoretical system in science, laws, and causes, and I follow Kant in restricting the scope of empirical science from the noumenal to the phenomenal world. Some basically positivist elements also colour the empiricist position I defend below: in particular, I argue that the positivists' "deductivist" image of science is uniquely well suited to the illumination of at least some important historical episodes, and that the positivists were right to eschew any special metaphysics of laws and causes. On all the points just mentioned Nancy Cartwright is opposed to the position I defend. I summarize Cartwright's philosophy of science in my first chapter, and in each of the succeeding chapters critically engage her philosophy in some way or ways.

Cartwright challenges the traditional concentration in philosophy of science on the rationality of theory development. In the first of her two books, *How the Laws of Physics Lie* (Oxford: the Clarendon Press, 1983; hereafter *Laws*), she also forthrightly challenges the positivists' "deductivist" image of scientific theorizing, prediction and explanation. Both *Laws* and her second book, *Nature's Capacities and their Measurement* (Oxford: the Clarendon Press, 1989; hereafter *Capacities*) champion the approach to the study of science called the "new experimentalism". This approach demotes what traditionally were the chief concerns in philosophy of science (unity, rationality, and theory) to suggest that the true content of scientific knowledge is an unsystematic complex of specific causal conclusions inferred on the basis of measurement and experimentation. *Laws* suggests that the systematicity that theories bring to this complex is an illusion of *construction*, involving important informal aspects of scientific thinking neglected by the positivists. *Capacities* drops the emphasis on informal aspects of scientific thinking, adopts the unapologetically deductivist bootstrap conception of scientific method, and suggests that the systematicity that theories bring to the complex of specific causal conclusions inferred on the basis of measurement and experimentation is an illusion of "*abstraction*". In *Laws* and *Capacities* Cartwright has different, though *singular* conceptions of what is involved in scientific reasoning; in no way is she a "methodological pluralist" in my sense of this expression, and I shall argue that this is a defect in her position and an ironic one, given that she evidently wishes to make a system of philosophy of science even while denying that a veritable system can be made of any science. In both her books Cartwright assumes an avowedly metaphysical burden concerning causation. She reverses the Kantian priorities by making causes basic, and regarding laws and theoretical systems as consequences of science's insistence on discerning systematicity even where it is not. In my final chapter (using work by John Earman, Philip Kitcher, Wesley Salmon and Mark Wilson) I argue that this is a mistake.

In making my acknowledgements, I start with my family; for we five all know well the effects of long-term graduate study. We aim soon to make up our great deficit in family recreation -- in particular, we aim to rediscover the *mountains*, and family camping, hiking, and climbing. My first and greatest thanks are to Judith Oakley Catton, for wanting to wait on these things. I also thank Felicity, William and Eleanor for so long postponing really getting to know what they have been missing.

To my supervisor, Professor William Demopoulos, past whose critical pen I sent my prospectus, overview and early chapters an embarrassingly large number of times, I owe great thanks for patience and painstaking help. He would have preferred a more circumscribed project and technique of analysis than those reflected here. I often rebelled against the circumscription necessary to philosophize in the way he prefers, in which the goal is the sharpness of a focussed, central insight. Difficult though I often was, I have nevertheless benefitted from his shrewd criticism and guidance. I thank him warmly for seeing me through to the end of this project.

Over the course of my work, including the period of her sabbatical leave and periods of enormous other commitments, Professor Kathleen Okruhlik has steadily supplied me time, instruction, critical comments, vital good sense and encouragement. Her knowledge of Cartwright's work was, to say the least, a great help to me; I acknowledge her guidance in all that I have got right, and I admit, here, with chagrin, that she remains critical of some aspects of my treatment of Cartwright. Professor Robert DiSalle also gave his time very generously to me, and supplied detailed critical comments, particularly on historical and technical issues. Some of these criticisms prompted me to improve the dissertation. Other worries of his I have not managed here to dissolve.

Professors Robert Butts and William Harper, a number of whose graduate courses I have attended while at Western, supplied or inspired several of the leading ideas of this dissertation, and I gratefully acknowledge their instruction, and generous encouragement. I thank Robert Butts for his varied and very generous attentions to my professional training and advancement. It has been my particular good fortune to have had an office next to William Harper's, and thus nearly every day to have benefitted from his thoughts on relevant subjects, and from his constant encouragement and good cheer. His work on Newton was a very direct and very evident help to this dissertation, and his reading and criticizing my Chapter 3 helped me to make several improvements.

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Chapter 1

Introduction

Are there truly the deep-lying structures in the world that the fundamental laws of physical theory, if they are true, disclose to us? Cartwright, on the basis of novel views of the function of laws in scientific thinking and the character of abstraction and idealization in science, argues not. This dissertation argues that there are these structures, and that the evidence for this falls outside any one systematic philosophy of present-day science. For the evidence is partly historical: it concerns the discovery of what methods work best for the investigation of nature. The historical evidence argues against the lessons Cartwright draws from her concepts of idealization and theoretical abstraction, and suggests that neither past nor present-day science has any one systematic character for the philosopher to unveil.

Cartwright believes that only the nitty-gritty knowledge science provides of the concrete and the particular has truth. Against this view, I shall argue that the surest, most tested, truest knowledge in science is abstract and general, and includes both high-level theoretical laws and principles of method. Against Cartwright's view that theoretical explanation is a very different thing from causal explanation, I show by historical examples that her distinction between two kinds of explanation is not robust. I also argue that we do not need

Cartwright's special metaphysics of causation. In these arguments I show how one may still discover key insights in various positivist conceptions from which Cartwright intends her views to afford a systematic escape.

My first project here is to detail Cartwright's doctrines, both in her 1983 work *Laws*, and in her new and different 1989 work, *Capacities*. Then I shall preview the historical projects and arguments of my later chapters.

1.1. Cartwright's *How the Laws of Physics Lie*. In *Laws* Cartwright attempts to make a unified system of much post-positivist philosophy of science by introducing a novel, sceptical view of the systematicity of scientific theories. She is critical in *Laws* of what may be called the "deductivism" of the positivists, and specifically of the positivists' prejudice that natural phenomena in principle are comprehensible within a fundamentally simple, logically articulated formal structure. Cartwright contends that there are indispensable informal aspects of scientific thinking (involving models and analogies) without which it would be impossible to create the theoretical systematicity that passes for a theoretical explanation of the phenomena. Because these informal aspects are indispensable in order for the phenomena to be saved (she contends), theoretical laws certainly cannot deductively comprehend the phenomena as the positivists supposed. On the contrary, it is only via a messy and inconsistent assortment of intermediary informal elements that any theoretical law manages to embrace a complex of phenomena.

Cartwright's post-positivist position has both old elements and new. As has become standard in post-positivist philosophy of science, Cartwright emphasises that science is a human activity, and she considers that a philosophy of science should reflect how science is actually learned and practised. Cartwright intends to study not imaginary, ideal science, but real science; and she thinks that her attention in *Laws* to informal aspects of scientific thinking is an important mark of this concern. Such attention is not new; informal aspects of scientific thinking were emphasised three decades ago by Hanson, Polanyi, and Kuhn; during the sixties, Hesse and followers studied the role of models and analogies in science; and the current topicality of informal thinking in science is due to widespread interest (spearheaded by Suppes, Suppe and van Fraassen) in a "semantic view of theories". The thinking of other, "realist", post-positivist philosophers of science also finds expression in Cartwright. In an argument very much like that given years ago (for example by Scheffler and Putnam) for the idea of referential constancy across major shifts in scientific theory, Cartwright balances her scepticism or anti-realism about theoretical laws with a realism about theoretical entities. "I believe in theoretical entities. But not in theoretical laws," writes Cartwright (*Laws*, p. 99). In defending her novel "entity-realism", Cartwright harkens back to an idea emphasised by Bacon and Galileo of which other post-positivistic philosophies have taken little note, that measurability or experimental manipulability is the surest mark of the real. Cartwright accepts this idea and connects it to her belief that there are cogent low-level measurement- and experiment-based inferences to specific conclusions concerning the causal relationships among natural phenomena. She believes that these inferences establish as well as anyone could want not only the reality of the causes inferred and of the theoretical entities that are components of these causes, but also certain *ceteris paribus* "phenomenological" facts that become the object of theoretical explanation. Thus Cartwright's scepticism about theoretical laws is not based in any way on the old, much-discussed problem posed against the postivists, concerning the lack of a theory-neutral observation basis for scientific theory. On the contrary, in Cartwright's view science routinely establishes "phenomenological laws" about which sceptical doubts are out of place. "Phenomenological laws" are, in Cartwright's view, the principal phenomena for scientific theories to explain. Such phenomena are scarcely theory-neutral. But (Cartwright contends) inference to the truth of these phenomena is something much weaker than inference to the truth of the theory involved in their description (because "phenomenological" laws, unlike "fundamental" theoretical laws, are accompanied by generous *ceteris paribus* clauses). Moreover (what for Cartwright is the key point) it is foreign to the practice of science to doubt the (*ceteris paribus*) truth of these phenomena; these are the empirical *facts*, Cartwright contends, from which scientists actually commence the task of constructing theoretical explanations.

Cartwright uses a notion of "phenomenological" that contrasts with "fundamental" and (borrowed as it is from physicists rather than philosophers -- see *Laws*, pp. 1 ff.) is new to philosophy of science. For Cartwright the laws that are *phenomenological* (1) correctly describe actual situations, but (2) are able to do so only *ceteris paribus*, whereas "laws" that are *fundamental* (1) correctly describe only objects in highly idealized models, but (2) are able to do so without *ceteris paribus* qualification. Cartwright contends that

> it usually does not make sense to talk of the fundamental laws of nature playing out their consequences in reality. The kind of antecedent situations that fall under the fundamental laws are generally the fictional situations of a model, prepared for the needs of theory, and not the blousy situations of reality. (*Laws*, p. 160).

Phenomenological laws are (Cartwright contends) in no way "covered" by the fundamental theoretical laws that are used to explain them. For this reason they form (Cartwright contends) a separate component -- on Cartwright's view, the major component -- of the content of scientific theory.

Another respect in which Cartwright's scepticism concerning theoretical laws is new is that it does not rest on the traditional problem of observational underdetermination of theory, a problem on the basis of which some positivists have argued for a conventionalist view of theories. Conventionalists contend that the phenonomena in any domain may be saved equally well by a variety of theories. Cartwright argues, on the contrary, that there are *no* good ways to save the phenomena. Not, at least, in the "deductivist" terms that the positivists presupposed, that the theory should comprehend the phenomena within a fundamentally simple, logically articulated formal structure. In *Laws* Cartwright contends that without the informal aspects of scientific thinking on which she lays stress it would be impossible to comprehend the phenomena within a reasonably simple theoretical system. On Cartwright's view, a theory involves, first, a small class of kinds of models, second, certain modes of description, formal and informal, that are used in characterizing the behaviour of objects in these models, and third, a store of informal "causal stories" pertaining to known "phenomenological laws" -- causal stories which may be represented formally in the idealized context of the theory's models, so that, in that context, approximations to all the known "phenomenological laws" are (if the theory is successful) formally deducible from "theoretical laws". Cartwright insists that without idealization no systematic comprehending of phenomena within a theory would be possible: the laws can never do it directly. (Because of its emphasis on idealization she calls her account of theoretical explanation a "simulacrum" account.) It is partly on her thesis that idealization is indispensable in theoretical explanation, and partly on an independent argument concerning component causes (which I shall discuss later), that Cartwright bases her conviction that all theoretical laws are false.

Cartwright thus aims to replace an old and particularly resilient image of scientific achievement with a new one that emphasises piecemeal causal knowledge ahead of theoretical laws. On the old image theoretical laws are the core content and prize accomplishment of science. On Cartwright's conception, "the real content of our theories in physics is in the detailed causal knowledge they provide of concrete processes in real materials" (*Laws*, p. 128). Theoretical laws are no distillation of this content; they seemed so only on the false positivist image that they are axioms within a deductive system that comprehends the phenomena. Rather (Cartwright contends) theoretical laws bear only obliquely on the causal content of physical theory, conditioning the models within which causal details are represented, and conditioning the allowable "ways of talking" about these details. Cartwright argues that theoretical laws can perform this systematizing role only because they are false.

> Really powerful explanatory laws of the sort found in theoretical physics do not state the truth. ...We have detailed expertise for testing the claim of physics about what happens in concrete situations. When we look to the real implications of our fundamental laws, they do not meet these ordinary standards. ...We explain by *ceteris paribus* laws, by composition of causes, and by approximations that improve on what the fundamental laws dictate. In all of these cases, the fundamental laws patently do not get the facts right. (*Laws*, p. 3.)

It is true that the positivists' deductivist image of scientific advance (possession of incrementally more and deeper laws of nature) is grossly over-simple as a portrayal of the development of scientific thinking. To know a body of natural science, the deductivist image suggests, is just to know some laws, laws of varying degrees of generality. The rest is logical knowledge: one reasons deductively from the laws to conclusions of the form that if such-and-such conditions obtain then such-and-such other conditions must also obtain. This, supposedly, is the character of scientific prediction and explanation. Cartwright disagrees with this view both by opposing the positivists' deductivist conception of theoretical explanation, and by opposing the positivists' assimilation of causal explanation to theoretical explanation. Against the positivists' deductive-nomological (D-N) model of scientific explanation Cartwright proposes that "[t]o explain a phenomenon is to find a model that fits it into the basic framework of the theory and that thus allows us to derive analogues for the messy and complicated phenomenological laws which are true of it" (*Laws*, p. 152). That is, theoretical explanations concern themselves with mere "simulacra" of co[illegible]e situations. The true explananda for theoretical explanations are the phenomenological laws. The *ceteris paribus* phenomenological laws are themselves the basis for a different kind of explanation which Cartwright calls causal explanation. In causal explanation a phenomenon is explained when a different factor is picked out as its cause.

In *Laws* Cartwright is a "constructivist" philosopher of science (so far as the laws, but not the entities, of scientific theories are concerned), insisting that theoretical development proceeds by invention rather than discovery. The constructivism of her position in *Laws* gives this position its various post-positivist strengths. Judged as a corrective to the reductivist empiricist persuasions of the positivists, constructivism undoubtedly has some truth. According to the positivists, theoretical development is all discovery rather than invention; the increase in the content of theory as theory develops simply is increase in its "empirical content", and this is discovery; given reductivist empiricism, nothing more "of significance" can be involved in theoretical advance. In *Laws*, Cartwright uses constructivism not only to liberate theoretical meaning or significance from its positivist equation with "empirical content", but also in support of her general scepticism concerning theoretical laws and theoretical explanations in science. *Laws* maintains that theoretical systematicity *in general* is invention or artifice; it is achieved through imposition of false patterns on an in fact unsystematic world. *Laws* maintains that theoretical explanations have no connection with truth, and the laws that are the endpoints of quests for theoretical explanation are one and all false descriptions of reality. These sceptical themes *Laws* ties directly to constructivism, which implies empirical underdetermination of theory.

It is because of the constructivism of *Laws* (and consequent emphasis on informal elements of scientific thinking) that in that work Cartwright can lay claim to having a much greater concern than the positivists for the actual character of scientific thinking. The positivists, as is well known, deliberately concerned themselves not with the actual character of scientific thinking, but rather with the character of "rationally reconstructed" science. They were not unaware that it is for all practical purposes impossible to complete such "rational reconstructions" except (as they thought sufficed for their illustrative purposes) of extraordinarily narrowly defined portions of scientific thought. Their ideas about deductive systematization and unity were not supposed to characterize actual scientific thinking so much as they were supposed to define a *regulative ideal* in the pursuit of science. Within even the most elementary physical systems, difficulties that for all practical purposes are insurmountable may prevent the comprehending of phenomena within a deductive system. For even in relatively simple physical systems the fundamental equations of, say, elementary particle physics, prove insoluble. The class of systems in which *analytical* solutions to the fundamental equations are possible is of measure zero within the class of all possible systems. And *numerical* solutions are typically impossibly computationally complex. So there is no deductive path using the fundamental equations from descriptions of the paramaters characterizing the system at one time to descriptions of the parameters characterizing the system at a later time. It is no surprise then that the physicist has typically to abandon brute analysis from fundamental theoretical laws, and, in order to comprehend known phenomena more or less well within the theory, turn instead to simplified models, perhaps determining by essentially *ad hoc* phenomenological investigations some parameters pertaining to the model that in principle could, but in practice could never, be deduced from fundamental theory. That actual theoretical physics should therefore often comprise -- to quote a reviewer[1] of Cartwright's *Laws* -- "a messy concatenation of inconsistent models of dubious status both vis-à-vis the fundamental theory and the observational data", is completely unsurprising. "Professor Cartwright," the reviewer continues, "has decided to make a virtue out of necessity and claims that that is all there really is to physics, the messy models, and the simple underlying unifying theories are just a foolish myth."[2] For whatever it is worth, Cartwright, with this approach, does succeed in illuminating many aspects of actual thinking in science that positivists exclude from their rational reconstructions. But this is incidental to the larger question (to which we will return later) of whether the positivists' regulative ideal of unity, which Cartwright summarily rejects, is not also in some way a worthy or even constitutive element of science.

In connection with the idea that "phenomena" for high-level theorizing (for Cartwright, phenomenological laws) are themselves the endpoints of empirical inference, *Laws* advances but does not really defend an empiricist thesis that is important for methodology: this is Cartwright's thesis of the *facticity* of what she identifies as *phenomena*, a thesis that implies that the traditional objections to

[1]M. L. G. Redhead, Review of Cartwright's *Laws*, in *Philosophical Quarterly* (1988) *34*: 513-514.

[2]Ibid.

the idea of a theory-neutral observation basis for science are (truisms) without methodological bite. *Laws* leaves the reader puzzled about what the inferences must be like that can thus deliver theory-laden "phenomena" as established facts.

1.2. Cartwright's *Nature's Capacities and their Measurement*. In her new book, Cartwright adopts certain methodological precepts (the "bootstrap" empiricism of Clark Glymour, according to which a theory is tested by the logical *deduction* from the evidence, using high-level background assumptions, of lower-level hypotheses that fall under the theory) by which she thinks the measurement- and experiment-based inferences that scientists make to theory-laden "phenomena" can be satisfactorily illuminated. Cartwright casts into Glymour's model that kind of inference to specific causal conclusions that *Laws* had left mysterious. According to Cartwright, the background assumptions for the bootstrap inferences are not elements of a systematizing theory, but are assumptions more specific to the question of when correlation licenses inference to causation. Such "background assumptions themselves will involve concepts at least as rich as the concept of causation itself. This means that the [bootstrap] deduction [using high-level background assumptions] does not provide a source for reductive analyses of causes" (*Capacities*, p. 22). Thus in *Capacities* Cartwright not only maintains but provides a basis for the views that she adumbrated in *Laws*, that inference to causes is relatively independent of theory, and that causes are *not* reducible to theory-governed empirical regularity. At the same time she presents an impressive case (drawing on case-studies from numerous fields) that Glymour's completely discursive, formally rigorous, unapologetically deductivist model of a form of scientific inference satisfactorily illuminates much scientific thinking. While she thinks Glymour's model satisfactorily illuminates low-level inference to causes, Cartwright also maintains in *Capacities* her earlier scepticism toward high-level theory.

Concerning high-level theory, it at least *seems* that Cartwright's sceptical position implies, even if it does not rest upon, the thesis of the empirical underdetermination of theory. How could theory that is presumed false be uniquely determined by the evidence? However, if Cartwright's sceptical position implies empirical underdetermination of theory, it is challenged by the various extant challenges to the underdetermination thesis. Ironically, Glymour's development of his bootstrap model of scientific inference was intended as just such a challenge to underdetermination. Underdetermination is a problem besetting the hypothetico-deductive method; Glymour argues that hypothetico-deductivism is not the method followed by the sciences.[3] Rather, his own

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[3]Glymour thinks that some inference patterns in the social sciences (particularly those leading to specific conclusions about causal linkages between quantitative variables) are irredeemably hypothetico-deductive, so he does not challenge underdetermination in respect of some theoretical thinking in the social sciences. Ironically, Cartwright thinks that the relevant social-science inference patterns *are* bootstrap, not hypothetico-deductive, in form. So in the context of social science it is she alone who challenges the underdetermination thesis, albeit challenging this thesis in respect only of low-level "causal", not high-level theoretical, economic or sociological conclusions. In this disagreement with Glymour, Cartwright

bootstrap method, a determinedly non-hypothetico-deductive method, Glymour reckons can be discerned very widely in the sciences. His theory of empirical confirmation has a novel consequence: by its lights, two scientific theories that are "empirically equivalent" in the hypothetico-deductivist sense of having identical observational consequences, can nevertheless be *unequally* confirmed by *one and the same* body of empirical evidence. By being more "bootstrap" testable, one theory may engage more richly than the other theory the shared empirical evidence, and so carry away with it greater empirical confirmation. Glymour appeals to this idea to argue that cases of genuine empirical under-determination of theory are rare. The examples with which Glymour illustrates his ideas all concern high-level theory, and thus suggest that the pattern of inference that Cartwright recognizes in low-level empirical inference to causes applies also at the higher level of inference to general principles of theory.

Cartwright, who contends that by bootstrap methods scientists successfully infer truths about the causes of natural and experimental phenomena, must think either that bootstrap methods lead to true conclusions only in their application

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suggests that social scientists implicitly commit themselves to various high-level theoretical principles by reference to which one can understand their lower-level inferences as bootstrap inferences, or deductions from phenomena. In my view, the high-level theoretical principles in question are too strong to be plausible. I doubt whether social scientists really would assent to these principles, or thus admit them as "implicit in the practice" of their science. Even if sociologists or econometricians really do implicitly affirm these principles, I doubt whether in so doing they are practising good science.

within these low-level inferences, or that (very much contrary to Glymour's own view) they are not applied by scientists at the higher level of inference to general principles of theory. I believe that something like the latter thesis is implied by *Laws* (in which, however, Cartwright does not mention the bootstrap method); whereas something like the former thesis better characterizes Cartwright's position in *Capacities*. In *Laws*, we have seen, Cartwright is constructivist. I think *Capacities* is best understood as abandoning the constructivism of *Laws*, even as it maintains the sceptical position of the earlier work according to which all theoretical laws are false. In *Capacities* Cartwright affirms empiricism -- not empiricism as the positivists understood it, but empiricism nonetheless completely opposed to the spirit of constructivism. *Capacities* explicitly denies the thesis of the empirical underdetermination of theory, and so denies a consequence of the constructivism of *Laws*. *Laws* implies that theoretical laws are artifacts. They are true only of objects in highly idealized models, and idealization is artifice. The theoretical systems of which laws are core elements *Laws* implies are consequently constructed or invented. No unique such system could possibly be forced on us by the empirical facts. But in *Capacities* Cartwright explicitly dispenses with the underdetermination thesis that is the endpoint of this reasoning, and so suggests that in some way our false theories are not freely constructed, but are constrained by the evidence to have precisely the form they do have. *Capacities* implies that while our choice of laws is uniquely determined by the evidence, laws are nonetheless false. They are false because they claim a generality that no true law has; but their being false does not imply that they are artifacts. In fact, read as ascriptions of causal "capacities" (I shall say more about this conception of Cartwright's in a moment) and not as statements of a regularity in nature, theoretical laws *are* true. *Capacities* implies that the methods by which causal capacities are empirically discovered have picked out extant high-level theoretical laws in a basically unique, non-accidental, non-artifactual way (modulo methodological mistakes by flesh-and-blood scientists). These methods employ various high-level assumptions that perhaps are false, but (modulo the question about the truth of these assumptions) they do not leave theory underdetermined by empirical evidence. *Capacities*, in contrast to *Laws*, has little to say about models or simulacra, or thus about informal elements or scientific thinking. *Capacities* subsumes the practice of idealization under the supposedly different practice of abstraction, fitted to the new anti-underdetermination view. Although *Capacities* thus differs from *Laws*, it preserves in its own way the scepticism concerning theoretical laws and theoretical explanation of the earlier book.

Cartwright's "capacities" are general *sui generis* causal truths -- general causal truths not reducible to associations -- whose modal character, she says, outstrips that of a causal law. Not only will an ascription of a causal capacity be universal in space and through time, support counterfactuals, license inferences, and so forth, in the manner of a causal law; it also will constrain the choice of systematizing theoretical laws, and in this way constrain what other capacities may be inferred to exist; it makes more definite both by inclusion and exclusion the range of "causal stories" admissible for causally accounting for the facts. A simple example that Cartwright considers is the capacity or tendency that things have to accelerate toward distant masses. The law of gravity describes this capacity, and the law is true when read as an ascription of it. But the capacity in question is often not exercised by a body (gravity is swamped by other forces); so, read as a statement about a regularity in nature, the law of gravity is false. A capacity is, in short, itself rather like a theoretical law, and theoretical laws may be read as a general ascription of a capacity. But whereas a theoretical law, read as a statement about a regularity in nature, is (in Cartwright's view) *false*, an ascription of a capacity holds true in the face of the same evidence. To see why Cartwright says this we have to examine some of the most metaphysical and consequently least clear elements of her position in *Capacities*.

In *Capacities* Cartwright maintains from *Laws* the image that

> nature is complex through and through: even at the level of fundamental theory, simplicity is gained only at the cost of misrepresentation. It is all the more so at the ... concrete level at which ... causal structures ... obtain. Matters are always likely to be more complicated than one thinks, rather than less. ...Simplicity is an artefact of too narrow a focus. (*Capacities*, pp. 72-3.)

On the strength of this image she rejects the postivists' characteristic credence in the unity of nature -- that is, their credence in the idea that a deductivist systematization of nature is possible. The positivist view was that the unity of all scientific knowledge can be achieved by the reduction of laws (including low-level causal laws) in the various sciences to the laws of physics. The thesis that such a deductivist unity of science is possible implies a particularly clear and strong thesis of the underlying unity of nature. Cartwright rejects both the thesis of the unity of science and the thesis of the unity of nature. In *Laws* she writes that

> science ... is divided into tiny, separate subjects that irk the interdisciplinist. Our knowledge of nature, nature as we best see it, is highly compartmentalized. Why think nature itself is unified? (*Laws*, p. 13.)

In *Capacities* this view of the underlying disunity of nature becomes the view that capacities, not laws, are fundamental.

> [W]e must admit capacities, and ... do away with laws. Capacities will do more for us at a smaller metaphysical price.
> (*Capacities*, p. 8.)

> It is not ... laws that are fundamental, but rather ... capacities. Nature selects the capacities that different factors shall have and sets bounds on how they can interplay. Whatever associations occur in nature arise as a consequence of the actions of these more fundamental capacities. In a sense, there are no laws of association at all. They are epiphenomena.
> (*Capacities*, p. 181.)

Cartwright reiterates her old complaint that just as philosophers of science have traditionally exaggerated the credentials of unifying theory, they have also paid too little attention to experiment and r :asurement and to piecemeal causal judgments. But Cartwright no longer implies in *Capacities*, as she did in *Laws*, that inference to systematizing elements of high-level theory is *methodologically* very different from inference to low-level causal conclusions. She has dropped her constructivism, her emphasis on the informal thinking surrounding models and analogies. The difference between the high-level and low-level inferences now is supposed to concern the "abstractness" of the reasoning on which the inferences are based. Theoretical structure is still supposed to be artifically imposed upon, rather than discovered in, the evidence, but the artificiality does not consist, as constructivists would insist, in the free imposition of empirically unconstrained, informal, "constructed" elements, but rather in leaving elements out (including, Cartwright says, the matter itself), and thereby dropping all complications attention to which would imply the need for *ceteris paribus* clauses. In inference to systematizing elements of high-level theory, what is inferred, *qua* statement of a certain regularity in nature, is still supposed to be false, even though the form of the inference is like what leads at a lower level to piecemeal causal judgments that are true. Regarded in light of the lower-level inferences, the higher-level inference *may* be conceived as leading to truth -- not general empirical truths, or truths about regularities, but true ascriptions of causal capacities. Since (on Cartwright's view) any such capacity resides among myriad others neither more nor less fundamental than it is, yet equally determinative of the run of empirical phenomena, an inferred capacity-ascription never supports more than a *ceteris paribus* empirical generalization or statement of a natural regularity. Cartwright's complaint against theoretical laws, as "fundamental", is that here the *ceteris paribus* clause is dropped, without which (Cartwright contends) theoretical laws cannot have any success as empirical generalizations or statements of a natural regularity.

1.3. Component Causes. In *Laws* Cartwright gives a swift argument for the falsity of a force law such as the law of universal gravitation. It is that the law calculates what in most cases will be only a component of the force acting on a given body. The law "must ignore the action of laws from other theories" (*Laws*, p. 12) for us to apply it as we do. Consequently the law itself "patently does not get the facts right" (*Laws*, p. 3). An obvious way of objecting to this is to say that the law gets right precisely those facts that concern the component force due to gravity. Other theoretical force laws get the facts right, too -- the facts concerning the component force due to electromagnetism in the case of the law of electromagnetism, for example. In that case the law of gravitation may be true even though "the action of laws from other theories" may have to be considered in order that the resultant motion of a body be correctly described.

In Chapter 3 I shall discuss and criticize Cartwright's response to this criticism. In part, her reply rests on what I shall term the "dilemma of the double effect"; there cannot be both the component force and the resultant force, Cartwright insists, lest the effect be twice what it should be. Cartwright holds that the resultant force is real and consequently denies the reality of component forces. The alternative of accepting the reality of component forces over the reality of the resultant Cartwright calls the "components scheme". Cartwright's objections to the "components scheme" are convoluted; I shall give fuller critical discussion of them in Chapter 3. It is a problem for Cartwright (though she scarcely admits this) that, in the course of her discussion, she specially excepts laws in pure theoretical mechanics from her general scepticism concerning theoretical laws. She says (*Laws*, p. 63; *Capacities*, p. 163) that mechanics provides from its own principles a general law of interaction (the parallelogram law for composition of forces) and for this reason -- unlike any other theory -- need not depend for its defensibility on *ceteris paribus* clauses. Even if other things are not equal, and extra force terms are relevant, mechanical laws still apply -- to the then resultant forces. This is all the concession that I need in order to make three objections to Cartwright's scepticism concerning fundamental force laws such as the law of gravitation. First, there is (quite contrary to Cartwright's implication) a scheme that is realist about both the components and the resultant and yet escapes the dilemma of the double effect. Second, on the sole basis of laws of pure theoretical mechanics, Newton was able to mount a particularly cogent bootstrap argument for the truth of his law of universal gravitation. I develop these two points in Chapter 3. Third, Einstein found that, because of the universality of the gravitational force, by an emendation of physical geometry it could be assimilated without remainder into the laws of mechanics. I develop this last point in Chapter 5.

1.4. Structure versus Ontology. Cartwright characterizes commitment to capacities as ontic commitment. "Capacities are as much in the world as anything else," she writes (*Capacities*, p. 198). "The point" of her new book, Cartwright says, "is to argue that we must admit capacities, and my hope is that once we have them we can do away with laws. Capacities will do more for us at a smaller metaphysical price" (*Capacities*, p. 8). Capacities will do more for us, Cartwright thinks, in accounting for scientific practice -- and that seems to be for Cartwright the highest desideratum in philosophical thinking about science. I question Cartwright's method of argument, here. The presuppositions with which I disagree are (1) that the admission of laws involves ontic commitments and thus carries a "metaphysical price", and (2) that the admission of causes also involves ontic commitments, rather than commitment to the obtaining of some law. Cartwright's scaling of philosophical desiderata I think untenably implies (3) that our assuming an obscure metaphysical commitment to capacities so as to rationalize scientific practice is preferable to any alternative that does not perfectly rationalize this practice. For Cartwright it would, for example, be better to assume the metaphysical commitments, than criticize scientific practice on some points, or rationally reconstruct scientific practice in some less-than-faithful way, so as to avoid or explain away scientists' seeming metaphysical commitments to capacities. The first two points I shall address in Chapter 6 by arguing that there is life yet in the traditional empiricist conception of laws and causes. The other chapters of the dissertation supply materials in support of the traditional conception of system, laws and causes, by laying out the empirical case for taking nature to be systematic, and defending on this basis a system-seeking empiricist methodology (centrally involving the bootstrap methods that Cartwright herself endorses). The third point -- about Cartwright's scaling of philosophical desiderata -- is easily answered if, despite a change of philosophical priorities, the phenomena concerning scientific practice from which Cartwright's position draws apparent strength are otherwise explained, something I also attempt in Chapter 6.

Perhaps because Quine made out his strictures on ontic commitment in a clear and convincing way, philosophers in recent years have tended to emphasize ontic commitment as the principal burden of theory acceptance. I see in Cartwright's entity realism / theoretical anti-realism a striking sceptical extension of the tendency in recent philosophy of science to emphasize the ontic at the expense of the structural. It is clear that, given her view of capacities, the language of ontic commitment is needed; she cannot present her commitment to capacities in structural terms, for capacities are only sometimes manifested in behaviours, so the relevant structures will sometimes be lacking even when by

Cartwright's lights the capacity is there. By my defense of the traditional, anti-metaphysical, empiricist conception of laws and causes, I mean to open the way to recasting the relevant commitments as structural commitments. Cartwright's ontic view that is sceptical about theory seems to me out of place precisely because I believe there are theories whose burden is wholly structural, and whose "abstractness" cannot therefore be understood along the sceptical lines Cartwright advocates. It is theories of this sort that I shall principally discuss in the following chapters. I shall discuss the methods by which they were discovered and the nature and strength of their evidential warrant. I shall argue that these theories capture important aspects of the world's form or nature; that their successes argue for the real systematicity of nature; and that for this reason, and for this reason only, the methods underlying them (including the bootstrap method that Cartwright adopts as her own) may be deemed well-adapted to the investigation of our world.

1.5. Bootstrapping, Unity, and Theory. The philosophies of theoretical science of Newton, Maxwell and Einstein are important points of reference in my thinking, and in their light, particularly as that light plays upon the theories of Newton, Maxwell and Einstein, I argue that the leading sceptical contentions in Cartwright's *Laws* simply do not ring true. Some of the changes in Cartwright's position in *Capacities* (and in particular, her own adoption of bootstrap empiricism) strengthen my case against her views on theoretical laws and theoretical explanations, and I explore this, making, however, only a selective study of the new book in this dissertation. I believe that in both books Cartwright illicitly foists upon realists about theoretical laws a naive commitment to inference to the best (deductive-nomological) explanation. She suggests that to argue at all for the truth of a physical law is to appeal (in a way which she argues is surely faulty) simply to the broad successes of the law in deductive-nomological "theoretical" explanation and hypothetico-deductive prediction. In effect, Cartwright commits her realist adversaries to hypothetico-deductivism. In particular, Einstein was a hypothetico-deductivist according to Cartwright (*Capacities*, p. 6). In fact, Einstein's practice was not hypothetico-deductive and the power of his and Newton's arguments in support of various high-level theoretical principles or laws can by no means be captured by hypothetico-deductivism. Exploring Newton's and Einstein's actual methods for establishing theoretical claims is the work of roughly a third of the dissertation. Equally importantly, I discuss Maxwell's philosophy of science, to show that his instrumentalist, model-building side (much emphasized by recent "constructivist" commentators, and by Cartwright) was in fact part of a general doctrine that, like Newton's and Einstein's, was realist about underlying abstract structure, and hence about fundamental laws.

In Chapter 2 I discuss the historical development from antiquity to the scientific revolution of the idea of nature as a system. From this general discussion, I derive the following points. (1) In astronomy, the investigation of causes was long held separate from attempts to improve formal, mathematical description of phenomena, but Copernicus changed this, and Kepler completed the transformation, not only showing that consideration of causes is heuristically important for developing a mathematical astronomy that is predictively the best, but also, in the end, fully translating a causal question (why the planets move in the way they do) into a question concerning the formal simplicity, or systematicity, or harmony of the set of ideas that says: the planets move in such-and-such a way and not otherwise. This "formal" notion of cause links causes to laws and laws to the systematicity of knowledge. (2) Even in Copernicus's work (in arguments much improved upon by Kepler) a premium is placed on *empirical overdetermination* of key theoretical parameters, and parameters that can be thus measured are supposed to belong to the real causes of the phenomena. This was an early example of bootstrap inference. (3) Kepler and Galileo both recognized that efforts to systematize the phenomena may condition what are counted *as* the phenomena. Galileo argued forcefully that, to accommodate changes in highest-level theory, the very understanding of "observational success" of scientific theories needed to change. Through these arguments Galileo closed the hermeneutical circle in the interpretation of nature, completely remaking, for theoretical ends, the evidence from which a theory of physics would make its start. His efforts however were not "irrationalist propaganda" (as Feyerabend has argued) but rather, for reasons that I discuss that underline the importance to science of the pursuit of systematicity, amounted to great science. (4) Galileo's conception of the relation of theory to evidence placed great weight on idealization, and left the thesis of the systematicity of nature in legitimate doubt.

1.6. Idealization and Abstraction. Cartwright sees grist for her mill in Galileo. In inferring mechanical principles Galileo dealt in idealizations. In *Capacities*, Cartwright explicitly links her sceptical theses concerning the character of abstract theoretical laws to Galilean idealization. She says that

> the method of Galilean idealization, which is at the heart of all modern physics, is a method that presupposes tendencies or capacities in nature (*Capacities*, p. 188).

The method for arriving at ascriptions of capacity Cartwright calls "abstraction" which she differentiates from idealization, insisting however that

> idealization would be useless if abstraction were not already possible (ibid.).

My discussion of Galileo in Chapter 2 does show the conformability of some of his views to Cartwright's "simulacrum" account of explanation. Chapter 3, however, concerns Newton's advance beyond Galilean idealization; I argue that Newton established a precedent and a set of standards for physics that Cartwright's emphasis on Galilean idealization altogether misses. Newton's own examples suggest a variety of ways in which theory may engage empirical evidence much more richly than through mere breadth of successful predictions, ways that bear very significantly on the question of the status of theoretical laws, and that surpass anything that can be found in examples from Galileo's work.

In the case of precisely those elements of theory that *Laws* insists are most invented or artifactual, I oppose the constructivist image of science. Those elements -- the highest-level theoretical laws -- I believe are our purest discoveries. I shall briefly describe below (in returning to the earlier discussion of system, laws, and causes) how I think this view is defensible even in the face of Cartwright's many telling points about scientific practice. I admit that much rings true from the constructivist image of scientific practice that Cartwright defends in *Laws*. The simulacrum account of explanation that Cartwright develops in constructivist spirit I think connects well with much scientific practice. There is a clear illustration of this in the work of other scientists that Newton, Maxwell and Einstein had to survey before they brought new unity to their fields. The disparate parts of physics that were not unified prior to their work, were unifiable, and because of this must have been held apart by something otiose, something artificially constructed, in their respective conceptual bases. The task, in part, of Newton, Maxwell, and Einstein, was to identify this otiose content, and then having done so to show how the disparate parts of physics could be comprehended together in a logically unitary way. There is work for both empiricism and rationalism in this; for rationalism, the quest for unity; for empiricism, the effort to eliminate the otiose content. Of course logical positivism was inspired precisely by the known grand successes in this work. In reference to the accomplishments of Newton, Maxwell or Einstein, I hope to show (against Cartwright, whose position is systematically post-positivist) that logical positivism is no gross mistake. On the contrary, truly unifying theories express little more than the structure of known experience, and that, indeed, is, as we shall see, very much how Newton, Maxwell and Einstein saw the content of their own theories.

1.7. Aims and Standards for Philosophy of Science. Because Cartwright tacitly abandons in *Capacities* the constructivism, and emphasis on informal thinking in science, of *Laws*, she is in a poor position to claim that it is actual rather than idealized thinking in science with which she concerns herself. She becomes like the positivists, at least in her willingness to idealize (for example, in paying to informal elements of scientific thinking much less than the very full attention encouraged by the perspective of *Laws*). She casts all scientific thinking into the formal mode that (the self-styled positivist) Clark Glymour has characterized and championed. In doing so she can no longer engage the elements of "actual scientific practice" that her earlier constructivism allowed her to engage. I believe that this is symptomatic of the unreasonableness of the very idea of making "actual" rather than "idealized" thinking the object of a philosophy of science. In fact the philosopher of science may idealize more or idealize less, or idealize in one way rather than another. But the goal of describing science, a human activity, "as it actually is", I think is silly pretension. Cartwright, who believes (as I do not) that the aim for *science* of theorizing how the *world* actually is is silly pretension, might have been expected to steer clear of such pretension. But she does not.

Cartwright's message about philosophy of science does however *partly* parallel her contention that in physics theoretical systematicity is achieved through imposition of false patterns on unsystematic details. In the past, philosophy of science has attempted to reach systematic understanding of science by concentrating on theory growth and change. Today a school dubbed "the new experimentalism" challenges this orientation, complaining that philosophers' traditional orientation to scientific *theory* has left philosophy of science quite desperately blind to the bulk of scientific work, namely, *experimentation*. Cartwright endorses this complaint. The implication (which neatly parallels Cartwright's sceptical view of high-level theory in physics) is that the patterns allegedly discerned in science through the study of theory growth and change in fact make very poor contact with scientific practice and with the diversity of actual facts. One might expect Cartwright to say that the whole project of generalizing about scientific practice is ill-conceived. But Cartwright (as I have just observed) herself returns to the project of gaining a general philosophical view of science, albeit a different general view, one that she sees as consonant with the "new experimentalism". (Some of her "new experimentalist" colleagues, such as Andrew Pickering[4], Ian Hacking[5], and Peter Galison[6], do not follow her in this, and prefer a more or less piecemeal, unsystematized, and also amethodological approach to the study of science.) Cartwright seems committed to the *methodological* unity of science, though of course she very much lowers the expectations to be placed upon methodology with respect to the warranting of *theory*.

My expectations for understanding science in a general way are, ironically enough, lower, and so more in the spirit of Cartwright's general view, than Cartwright's. I expect the philosopher's toolkit for illuminating science to contain a variety of mismatched tools with partly overlapping capabilities. I illustrate this, in particular, in a pluralistic discussion in Chapter 3 of philosophical theories of empirical confirmation, and in a discussion in Chapter 5 defending pluralism with respect to the question what is a scientific theory. But the point is also illustrated by the debate concerning theory versus experiment as the focus for the study of science. This debate is simply out of place, I believe:

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[4]See for example his *Constructing Quarks*, Chicago: University of Chicago Press, 1984.

[5]See for example his *Representing and Intervening*, Cambridge: Cambridge University Press, 1983; and his "The Disunity of Science", unpublished.

[6]See his *How Experiments End*, Chicago: University of Chicago Press, 1987.

neither the emphasis on theory nor that on experiment is in itself adequate, and each approach brings some things to light about science that it would not do to miss. The same, or at least an overlapping, point, can be put this way: one can idealize about scientific thinking the way the positivists did and recover much that is illuminating about science, including some that will be lost when one idealizes in a lesser degree or in a different way, say with a view to illuminating informal elements of scientific thought, or with a view to taking proper notice of low-level measurement- and experiment-based inferences.

Cartwright implies that there is no single nature of nature, that is, that there is no one coherent underlying order in the physical world for our physical theories to grasp (a claim I reject). In a similar way, Richard Rorty[7], Ian Hacking[8], Nancy Nersessian[9] and others have argued that there is no single nature of *science* that a theory of science can grasp. Science is not a "natural kind", and so we must expect our knowledge of it to remain forever piecemeal and ununified. I think that there is much truth in this latter claim, and in my

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[7]*Philosophy and the Mirror of Nature*, Princeton: Princeton University Press, 1979.

[8]*Representing and Intervening*, op. cit.

[9]*Faraday to Einstein: Constructing Meaning in Scientific Theories*, Dordrecht: Nijhoff, 1984.

conclusion shall bring together the evidence I think comes from my own historical case studies in its support. There I shall also link this thesis that science has many natures to my view that the method is empirically learned.

1.8. Conceptions of Empiricism. To show that Cartwright's sceptical philosophy is inadequate in its conception of theoretical science, I have chosen examples from the classical physics of forces and fields, as well as from classical and modern spacetime physics; this part of my argument (which comprises Chapters 3, 4, and 5) involves my surveying theories of empirical confirmation and leads (in Chapter 6) to my championing an empiricist account of laws of nature. In the course of this I show how there are various worthwhile elements in *empiricism* as I understand it for which Cartwright can provide no home in her philosophy of science. I use work[10] of Mark Wilson's to argue that causal facts can be as diverse as Cartwright says they are in a world in which simple, fundamental laws are true. Even in the face of some phenomena concerning scientific practice from which Cartwright's position derives apparent strength, it remains perfectly reasonable to adhere to the traditional, empiricist, regularity

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[10]See Mark Wilson, "Honorable Intensions", unpublished; and also: "The Double Standard in Ontology", *Philosophical Studies* (1981) *39*: 409-427; "The Observational Uniqueness of Some Theories", in *The Journal of Philosophy* (1980) *77*: 208-233; "What is this Thing Called 'Pain'? -- The Philosophy of Science behind the Contemporary Debate", *Pacific Philosophical Quarterly* (1985) *66*: 227-267; Review of D. M. Armstrong's *What is a Law of Nature*, *The Philosophical Review* (1987) *96*: 435-441; Review of J. Earman's *A Primer on Determinism*, *Philosophy of Science* (1989) *56*: 502-532.

conception of laws and causes. The two halves of this contention (respectively concerning laws and causes) have already been argued, very satisfactorily I believe, respectively by John Earman[11] and Philip Kitcher[12], and my Chapter 6 largely reviews this defence adding the considerations brought to light by Wilson.

1.9. Phenomena. One empiricist thesis of Cartwright's that I believe my historical studies below amply support is that "phenomena" are the end-points of empirical inference, but because the relevant inference pattern (bootstrapping) is an exacting one, are still reasonably solid starting-points for inference to higher-level principles. I discuss how the "phenomena" from which Newton, Maxwell and Einstein made their important deductions were not *data*, but were already reasonably high-level *colligations* of data in Whewell's sense (a sense which we will thoroughly examine in Chapter 3, refering to work of Malcolm Forster[13]). Cartwright may well be correct, and I take her case studies as helping to show, that bootstrap methods play a role in establishing such colligations. Cartwright

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[11] *A Primer on Determinism*, Dordrecht: D. Reidel, 1985.

[12] "Explanatory Unification and the Causal Structure of the World", a monograph-length study in the volume jointly authored with Wesley Salmon entitled *Scientific Explanation, Minnesota Studies in the Philosophy of Science*, vol.13, Minneapolis: University of Minnesota Press, 1989. Pp. 410-505. See also his "Two Approaches to Explanation", *Journal of Philosophy* (1985) *82*: 632-639.

[13] "Unification, Explanation, and the Composition of Causes in Newtonian Mechanics", *Studies in History and Philosophy of Science 19* (1988): 55-101.

is certainly correct to view such phenomena as (as it were) the observation-basis for higher-level theorizing. The distinction between phenomena and data is an important one, and what one can say about it is much richer than what one can say in support of the old cliché that all observation is theory-laden. (James Bogen and James Woodward give detailed support for this point in an important recent paper.[14]) Cartwright (particularly in *Capacities*; see, for example, pp. 35, 168-169) has shown that the "data" from which causal phenomena are typically inferred themselves are phenomena (probabilities) bootstrap-inferred from still more primitive data (frequencies).

1.10. Method, History, and the Systematicity of Nature. If Cartwright helps us discern bootstrap methods in lowest-level ("most observational") thinking in science, I attempt with my case studies to show the importance of these methods in highest-level (most highly theoretical) thinking in science. Bootstrap methods, involving the ingenious use of phenomena to "measure" key causal or theoretical parameters (always in light of background theoretical assumptions) I believe were *first* employed, and employed very compellingly, in inferences to high-level theoretical principles. There were (as I have mentioned we will examine in Chapter 2) some anticipations of these methods in Copernicus and Kepler, but Newton made the first commanding use of them under the rubric of "deductions

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[14] "Saving the Phenomena", *The Philosophical Review 97*: 303-352. 1988.

from phenomena".[15] One is not in a position to appreciate the *nature* of the systematicity of today's accepted theoretical knowledge unless one studies the successful use Newton, Maxwell and Einstein made of these methods. One is not in a position to appreciate the credentials of these methods themselves unless one studies how the work of Newton, Maxwell and Einstein established empirical credentials for them.

The choice of a method itself naturally conditions in some way the character of the theories that will be inferred or selected when the method is used. Glymour argues that the theory that most richly engages the empirical evidence in a "bootstrapping" way will typically also be judged simpler, better explaining, and better unifying of phenomena. It is, Glymour argues, only as an epiphenomenon of science's quest for the *empirically* best confirmed, that is, for the best "bootstrap-confirmed", theory, that nature has come to seem to us intelligible, unified, systematic. In effect, Glymour builds into his theory of empirical confirmation the view that nature is intelligible, unified, systematic. For he assumes -- as is by no means a necessary truth -- that nature is such that it is best studied by the bootstrap method, from which, along with the point

[15]In fact Newton's deductions from phenomena establish somewhat more than Glymour demands from bootstrapping, and, as we shall see in Chapter 3, they are not the only aspect of Newton's method that makes for richness in the engagement of the evidence by the theory. What Cartwright calls "bootstrap inference" really involves all the elements of a Newtonian deduction from phenomena (see *Capacities*, p. 147).

about how bootstrapping conduces to theoretical systematicity, the systematicity of nature follows. Of course Glymour thinks he has an empirical basis for his assumption that nature is suitable for study by the bootstrap method. For he thinks that this is the method that science has, in the fullness of its experience, adopted. Cartwright ironically provides rich, new additional evidence that this is so. It is my principal task to show that she needs (given her adoption of the method in effect of Newton, Maxwell and Einstein) to accept the further consequence explicitly assumed by Glymour -- that nature is evidently systematic after all, precisely in the sense that it is suitable for study by the bootstrap method. Newton, Maxwell and Einstein were all consciously empiricist about method, recognizing that method itself embodies presuppositions that are substantive and that require support from experience. Method must be warranted through the experience of its delivering theories that are impressive, preferably not only on the chosen but also on several alternative measures of empirical strength. If it is a fact, it is a *brute* fact that a method for science may arise that is warrantable in this way. (Einstein recognized this when he said that the most incomprehensible thing about nature is that it is comprehensible.)

In Chapter 2 I shall outline the emergence of the idea of nature's systematicity and discuss the slow and uncompelling improvement of the warrant for it up to and including the work of Galileo. In Chapter 3 I argue that Newton radically readjusted the standards of appraisal in physics, and launched a new method for the investigation of nature. The method was to be judged according to the success of the physical theory to which it led. The method assumed the truth of three laws (or "axioms") of motion. It based on these laws detailed deductions from phenomena of the forces at work in a system. We study Newton's most successful investigation of this form, his investigation of the forces at work in the solar system, and his inference to the law of universal gravitation. In Chapter 4 we study how Maxwell employed Newton's method, making advances that for the first time rendered empirically problematic the mechanical principles that the method assumes, thus underlining the empirical status of the method. In Chapter 5 we study how Einstein, still employing Newton-styled deductions from phenomena, inferred a new kinematical basis for the principles of mechanics, thus amending Newton's mechanical assumptions in a way that sustained their former successes. Along the way I illustrate how (1) positivist ideas illuminate the historical development of high-level theory in physics, and indeed seem uniquely well suited to illuminate some aspects of this development; (2) many post-positivist ideas help further to illuminate the same development, illustrating my pluralist thesis about the philosophy of science; and (3) *pace* Cartwright, there are grounds (from the multiply characterized, historically sustained successes of Newton's method) for thinking *true* the idea that nature is *systematic*, that is, is such as to be suitable for study by the system-inducing bootstrap method.

Chapter 2

The Idea of Nature: Thales to Galileo

Cartwright in effect seeks to overturn a conviction that originated with the PreSocratics, was instrumental within the Scientific Revolution, stood at the heart of the thinking of Newton and Einstein, and, according to Kant, is fundamental to positively all scientific inquiry. This is the conviction, begun, perhaps, by Thales, that the world has fundamentally one φύσις or form or nature, and that a *physics* of all nature is therefore possible. If Cartwright is correct, we should reject this conviction. Cartwright thinks, as did Common Sense in ancient Greece, that there is a great plurality, and no neat system, of forms -- or, in her scheme, causal capacities.

For Greeks of antiquity, 'φύσις' had the connotation of the English phrases "determinative principle", "outward form or appearance", and "form resulting from growth"; it also enjoyed a close etymological connection with the notions of the living activity of breathing, the principle of life in plants, and the activity of growing or bringing forth. 'φύσις', often translated into English as "nature", is also often translated as "form", for it strongly connoted to the Greeks simply the visible aspect of a thing, that which makes a thing visibly a unity and visibly disunites it from the background of other things. It was automatic, for most Greeks, to think that the world contains myriad ultimately unrelated natures. In the commonsense picture of the world in that age, "natures" were proliferated; they were as diverse as are things. While Cartwright has no place for the hylozoism and teleology in this conception, she would nevertheless have us return to the anti-rationalistic *pluralism* in the commonsense worldview of the Greeks.

The idea of "natural unity" that Cartwright proposes that we set aside was at the centre of Western culture's development of natural science; it also informs those present-day language forms that are used to characterize success in the quest for knowledge. We think about knowledge, as we think about nature, in terms of unity. It is not by coincidence that our word 'nature', which bears the meaning "the essential qualities or properties of a thing ... giving it its fundamental character", also applies, *in the singular*, to everything that there is. Our language invites us to think that there is one single underlying "nature of nature", not many ultimately unrelated natures in the world about us. In antiquity there was no such connection between the notion of *a* nature (φύσις) and the notion of all that there is. Nor was rational unity implied in the success words for knowledge. In investigating the world we will say our aim is to "comprehend", a notion which suggests the metaphor of grasping interrelated parts as one, and trades on a supposition of unity. Or we will attempt to "fathom" or "understand", words that acknowledge the need to go beneath the surface diversity of things to grasp common underlying form. For the Greeks, by comparison, the success words concerning knowledge were never like this. Rather the metaphor of vision ran through all such success words in Greek. The aim was not to "comprehend" or to "understand" the world in either of these senses, but rather, proceeding thing by thing, to perceive how a thing's activity fulfills its nature. Cartwright in no small measure agrees with the Greeks; she implies, at any rate, that there are many natures in nature rather than one, and that "comprehension" and "understanding" have been much overbilled: according to Cartwright, true knowledge of reality begins and ends with diverse and ununified cognizance of empirically inferred but ununderstood causal capacities in the diverse types of individual things.

It is true that the idea of natural unity was carried forward from its classical inception to its seventeenth-century assimilation into general culture less by *evidence* than by capricious currents of general philosophical thought. Evidence was undeniably thin at the outset, when Thales proposed, against the commonsense pluralism in Greek thought, that *all is water.* Yet it was thus that the idea was (as far as we know) first advanced of a hidden unity in the world. A principal task I have set myself in this dissertation is to examine in its historical connections the idea that Cartwright wants us to reject, in order to appraise whether or not it eventually began to have evidence on its side. This task of course has philosophical as well as historical dimensions. The history of the idea of nature is also the history of the opposition between rationalism and empiricism about knowledge. Because the world appears to be diverse and ununified, early rationalist proponents of the unity of nature were given to mistrust the senses. Early empiricists, on the other hand, defended the adequacy of the senses and thus were given to mistrust the thesis of natural unity. Ultimately this opposition between rationalism and empiricism was surmounted: the scientific work of Copernicus, Kepler, Galileo, and (particularly) Newton combined rationalism and empiricism with mounting success and sophistication. In this chapter we will examine the developments up to the time of Galileo.

2.1. Greek beginnings. Someone once asked Einstein why he thought physics had not arisen first in China or India rather than in Greece, and Einstein replied that the wonder really is how such a pursuit as physics could ever have arisen anywhere. How indeed, in the face of the diversity and irregularity that the world manifests to us, could the idea of a general science of nature ever have first begun?

Astronomy is the most ancient science. Did it lead the way? Astronomers in ancient China and ancient India had documented regular patterns in the motions of celestial bodies, and with some success used these patterns to make astronomical predictions. Evidently such work began early in Greece, too, for we know that Thales gained prominence through successfully predicting an eclipse. Thus celestial regularity was known early by many peoples; in the case of eclipses, it was also known that complex patterns (of recurrence of eclipses) could be built from simple patterns (in the motions of the sun and moon). Yes, astronomy certainly had a role in the advent of physical science. But this role was equivocal, and continued to be so until the time of the Scientific Revolution. Historically, peoples have tended to regard the known regularity of the heavens as marking the heavens off from the corrupt and relatively unpatterned terrestrial domain. The successes of astronomy for this reason never fostered in China or India the pursuit of a physics or general science of nature.

We may speculate that for some reason Thales paused over his discovery that the simple natural motions of the sun and moon could be combined in a more complex natural pattern of solar and lunar eclipses; we may speculate that he became taken up by a new and very general reflection. The idea that simplicity underlies complexity -- does this, Thales wondered, have purchase outside astronomy? Might the complex natures also of individual terrestrial things arise thus compositionally from simpler forms? This so far was just a philosopher's speculation but it nonetheless suggested a programme and the programme had a logical limit. The limit is the thesis of one underlying form or nature, compositionally accounting for all the diverse phenomena of the world.

When, against the commonsense pluralism of Greek thought, Thales proposed that all is water, he advanced the idea of a hidden unity in the world.

Soon Anaximander, and later, Parmenides, pursuing Thales' idea of natural unity, wrote tracts on physics -- tracts not under the title, *On Natures*, but rather under the title, *On Nature*. Like Thales, they sought to specify out of what primitive substance the world is constructed. But unlike Thales they thought this substance might be unfamiliar to us.

Pythagoras, a generation after Anaximander, also saw unity in nature: not unity of primitive substance, but mysterious abstract unity of structure or form. He proposed that in some mysterious way abstract numerical form underlies all things and explains their natures. As is well-known, Pythagoras and the early Pythagoreans discovered a small number of pure and applied mathematical results that lent some plausibility to these otherwise outlandish claims.

Two generations later still, Heraclitus emphasized unity through his doctrine that there is no permanent reality except the reality of change or Becoming. Being appears to us to be enormously diverse, but this diversity is an illusion, for Being itself is an illusion. Parmenides countered the Heraclitean rejection of Being with his own rejection of Becoming. The world, for Parmenides, comprises only unchanging Being; its form is permanent and therefore one. Again the senses are blamed for the appearance of diversity. For Parmenides it is change or Becoming that is a mere illusion of the senses. Neither Heraclitus nor Parmenides had any empirical support for their outlandish world-conceptions; because, in any case, they distrusted the senses, neither did they seek any such support.

Plato, like Parmenides, posited the permanence of form, and he continued the Heraclitean/Parmenidean scepticism about the senses. Like Parmenides, Plato supposed that only what is unchanging is intelligible. In his *Timaeus*, Plato presents matter as unstable, incapable of sustaining intelligibility of form over time. Plato urged that to attain true knowledge one must escape from the intractable irregularity of the ever-changing sensory world into the intelligible unchanging order of Forms. In later life (and this greatly influenced the Neoplatonic tradition) Plato grew more radical in his emphasis on the rational unity of the Forms. He urged that the Forms are not ultimately separate or distinct; rather, in their ultimate unity, they are neither effable nor knowable by us. Not one of Heraclitus, Parmendides, or Plato, gave serious credence to the idea that humans may discover a physics pertaining to the world of experience.

Aristotle opposed this sceptical tendency, and to the extent (an extent which is debatable) that he nevertheless maintained an overriding emphasis on the idea of unity in nature, his was the boldest Greek doctrine to acknowledge the idea of unity. He was, moreover, the only one clearly to suggest that humans may discover a physics pertaining to the world they experience. What throws this interpretation of Aristotle into doubt, however, is that Aristotle inclined in some ways toward the commonsense pluralism that his philosophical predecessors had eschewed. Aristotle urged a return to a commonsense trust in the senses. For Aristotle all that is formal takes its origin from the senses. At the same time Aristotle maintained an emphasis on rational systematicity of knowledge, an emphasis central to his theory of forms. The implication can straightforwardly be read into this that, for Aristotle, an *empirical* physics is possible; such a science discovers, at the highest levels of the hierarchy of forms, the nature of (terrestrial) nature in general. To constitute even a tolerable first step in such a direction Aristotle's own *Physics* was much too dogmatic about conclusions that attached too directly to the commonsense views of the day.

On Aristotle's conception, physics is the general study of what it is for things to carry within themselves a principle of motion or change. For Aristotle, physics is life science at the most general level; given Aristotle's hylozoism, this makes physics also the most general science of terrestrial phenomena. For Aristotle the heavens are no subject for physics; at any rate, they are subject to a completely different physics from that pertaining to the generation and corruption found on earth. Aristotle drew a sharp distinction between physics and mathematics. This reflected Aristotle's leaning toward common sense in his natural philosophy and away from Plato's pessimism about the possibility of an empirical science of nature. In his *Physics*, just as in his *Ethics*, Aristotle aimed to provide to any interested person of good sense useful, general qualitative principles pertaining to matters that are everyday. He wanted these principles to formulate general wisdom about their subject matters. Aristotle's official view of physics therefore ignored the extensive use of mathematical methods and mathematical language that there was already, in his day, in optics, harmonics, mechanics and astronomy, though in fact Aristotle himself sometimes employed mathematical principles in his expositions of these subjects. Aristotle's legacy was a sharp division between a qualitative sphere comprising most problems in natural philosophy and subject to the characteristic Aristotelian ordinary-language analysis, and the spheres of a few special sciences, such as astronomy, whose techniques were mathematical.

2.2. The Renaissance. Aristotle's division between mathematics and physics served to split astronomy into two parts, "mathematical" predictive astronomy and qualitative "physical" astronomy. Renaissance astronomy inherited from the Ptolemaic astronomical tradition of the Middle Ages just such a bifurcation. Predictive astronomy was basically a mathematical *art* and its devices were conceived to have little connection with physical reality or with physics. Astronomers employed mathematical devices (equant points, for example) that neither conformed to any conceivable celestial mechanism, nor were supposed to do so. Their function was solely to help "save the phenomena" in as mathematically economical a way as possible. Only in broad outline were astronomers' cosmological systems supposed to represent cosmological fact. Certainly, the celestial world was supposed to be centered on a stationary earth, for in this way cosmology would dovetail with Aristotelian conceptions of natural place, the absolutness of motion, and the naturalness of rest. This general conformability of cosmology with Aristotelian conceptions was a concern of "physical astronomy", as was the general question of how the celestial motions are possible. Beyond this, the concerns of "mathematical astronomy" were kept almost completely separate from those of "physical astronomy" and so of physics in general.

In the domain of terrestrial physics, Renaissance thinkers mostly read Aristotle as a pluralist and a philosopher of common sense. Scholastic philosophers, for example, concretized physical pluralism in their doctrine of substantial forms. This doctrine was scarcely in the spirit of Aristotle's own philosophy, though it involved ardent insistence on common sense, qualitativity, and the Aristotelian notion of the adequacy of the senses. It altogether dropped Aristotle's own insistence on the rational unity of science and so of the natural world. Scholasticism accorded to the sensory aspect of things an epistemic completeness that forestalled the search for hidden, or occult, characteristics of nature, and in this way concretized the diversity of the world of appearance. The senses were taken adequately to convey to the human understanding the real qualities of things. The essential determinant principle of a thing was its "substantial form", a notion that also connoted simply the visible aspect of a thing. Even the space or place which an object occupies was not for Scholastics hidden within it, but rather was an envelope around it; its place, its form, its various qualities were all supposed to be adequately presented to us by our senses. These views militated strongly against consideration of occult, or hidden, characteristics of nature. They thus also precluded the idea that there is hidden, underlying unity in the world, and so fostered an antipathy to the enterprise of physics. These Scholastic philosophers made not one advance in physics.

Not all Aristotelian commentators were thus kept from undertaking work in physics. Some (beginning with 11th-century Arab scholars such as Averroës, and including 15th-century "impetus theorists" in Paris and Oxford) critically studied Aristotle's *Physics* and addressed its sorest weaknesses, for example free-fall and projectile motion, effecting improvements that were, in part, mathematical. But it remained their chief aim simply to make *qualitative* sense of our everyday experience concerning bodies in motion,[1] and to do so in terms faithful to Aristotle's own. One notable exception was the linking by impetus theorists

[1] The quantitative apparatus that was developed in 15th-century Paris and Oxford for describing (for example) uniformly accelerated motion, was never applied to reality -- in particular, none of the theorems concerning uniformly accelerated motion was ever applied to free-fall. See (among many possible secondary treatments) Marx Wartofsky, "All Fall Down: The Development of the Concept of Motion from Aristotle to Galileo", Appendix "A" in his *Conceptual Foundations of Scientific Thought*, New York: MacMillan, 1968; or Edward Grant, *Physical Science in the Middle Ages*, Cambridge: Cambridge University Press, 1977, especially pp. 58-9.

of their conception of the continuation of motion to the motion of the heavenly spheres. This 15th-century linking of terrestrial and celestial physics was novel and potentially revolution-making, but, failing to take hold, had little long-term impact.

Nature for most renaissance Aristotelians thus was conceived to be split into terrestrial and celestial domains. In the terrestrial domain, the Aristotelians' doctrine of the adequacy of the senses, and their consequent hostility to ideas of occult or hidden connections among things, precluded their assimilating any idea of hidden unity to their worldview or into their conception of the goals of knowledge. In this tradition the overriding emphasis by Aristotle and other Greek philosophers on the idea of unity in nature had been altogether lost.

The resurgence of Neoplatonism beginning in the middle of the 15th-century, replete with mystical Hermetic "natural magical" associations, renewed the idea of unity in nature, and at a singularly opportune time. This was, after all, the full Renaissance, when a new idea could really live and breathe. Any sufficiently suggestive and promising idea could go far indeed, whatever its truth-status might be. Renaissance Neoplatonism with its Hermetic core was a grab-bag of suggestive ideas and the dominance of the Church-based Scholastic philosophy that opposed these ideas was fading fast, partly because of religious upheaval and the consequent general weakening of the Church, partly because of the blossoming ranks of a secular intelligentsia who owed no allegiance to the Church, and partly because, for quirkish reasons, Hermeticism for a while held respect from religious circles.[2] Celestial and terrestrial spheres Hermeticism supposed were in thoroughgoing interaction.[3] In the changed environment there suddenly were more *magicians*, magicians who were increasingly skilled and

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[2]The core texts of the fictive Hermes Trismagistus, texts in fact written in the third century A.D. by a handful of authors, were misdated to a time before Christ, Plato, and even Moses. The elements of Christian, Platonic, and Old Testament doctrine were thus read, even by many within the Catholic Church, such as Ficino, as prophetic, and as establishing the profound sanctity of Hermeticism. See Frances Yates, "Bruno", *Dictionary of Scientific Biography*, and *Giardano Bruno and the Hermetic Tradition*, Chicago: University of Chicago Press, 1964.

[3]Hermeticism brought with it a variant on astrology that Yates has called "astral magic". Whereas the Church had opposed astrology because of its deterministic overtones, thought inconsistent with religiously based ethical notions, in astral magic the deterministic overtones of astrology were gone; the occult influences of the stars were to be called down by the magical practitioner and put to work, on command, in people's lives. The sixteenth-century natural magician

> was a magus or operator who, by reaching back to a secret tradition of knowledge which gave truer insight into the basic forces in the universe than the qualitative physics of Aristotle, could command these forces for human ends. Nature was linked by correspondences, by secret ties of sympathy and antipathy, and by stellar influences. ... Knowledge of these links laid the basis for a 'natural magical' control of nature. The techniques of manipulation were understood mainly in magical terms (incantations, amulets and images, music, numerologies).

[P. M. Rattansi, "The Intellectual Origins of the Royal Society", *Notes and Records of the Royal Society 23*, 1968. P. 132. Quoted in Mary Hesse's "Hermeticism and Historiography: An Apology for the Internal History of Science", in R. H. Stuewer (ed.), *Historical and Philosophical Perspectives in Science, Minnesota Studies in the Philosophy of Science*, vol. v, 1970. Pp. 134-60.]

convincing in their arts. 'Magician' connotes one who deals in mysterious forces, but also one who performs entertaining parlour tricks. A magician even in the latter sense serves to challenge the Scholastic doctrine of the adequacy of the senses. Advances by that day in the mechanical as well as the magical arts, compounded these challenges. People created mechanically animated devices such as clocks and clockwork toys. Such a device's determinative principle (conceived of as a mechanism of some sort) was something explicable but hidden, and was clearly separate from the device's outward form. This was a further powerful challenge to Scholastic doctrine. And it suggested a new, very general mode for the investigation of the operation of things: hypothesize hidden mechanisms. According to Scholastic tradition it was frivolous to speculate about hidden influences on visible phenomena. What is hidden literally is "occult", and serious thinkers had long avoided the occult. Advances in the mechanical arts challenged this high-brow conservativism, just as occultism in general was given new impetus from other sources.[4] The rise of intellectually

[4]Occultism had a traditional form, demonic magic, which (although insuppressible amongst the vulgar) had been kept strongly in check both by Aristotelianism and by Catholicism. Occultism was conceived to be in direct conflict with the Aristotelian notion of a natural order, in terms of which Aristotelians made sense of the world and Catholics made sense of the religiously important notion of miracles. Demonic magic is a form of occultism which clearly threatens the notion of a natural order that can be disturbed only by God. Stigmatized as heretical, occultism was kept underground until the Renaissance. But Hermeticism made magic a much more difficult object for allegations of heresy. Hermeticism replaced demonic magic with *natural* magic. Natural magic denies the equation of "hidden" with "demonic". Hidden influences guide the changes we see in the world, but these hidden influences are not the hands of demons;

respected occultism and the resurgence of Neoplatonism in intellectual circles went hand-in-hand. People began again to distinguish, with Plato, what can be perceived from what is real. This scepticism toward the adequacy of the senses, although less radical for many Renaissance Neoplatonists than it had been for Plato, challenged Scholasticism. Renaissance Neoplatonism also resuscitated Plato's Pythagorean notions of mystical influences and underlying mathematical order in nature. Hermetic Neoplatonism elevated the idea of the occult by incorporating it into the idea of the connectedness and unity of all things.

2.3. Copernicus. In 1543, the year of his death, Nicholas Copernicus officially advanced against the geocentric orthodoxy his heliostatic conception of the world. The Neoplatonic movement had significantly influenced Copernicus. The idea that the earth moves flatly contradicted the Aristotelian doctrine of the adequacy of the senses. In the sixteenth century, motion was not conceived in relative terms: if the earth moved, it moved absolutely, and this was a quality to which human senses were patently inadequate. Without himself demanding deep revision of Aristotelian physics, but simply by invoking occult influences, Copernicus could deflect the charge that motion of the earth is not terrestrially detectable. He said that the earth's influence over all things terrestrial keeps them from falling behind as it moves. Copernicus actually warned his readers to maintain the Aristotelian distinction between celestial and terrestrial "so as not

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they are part of nature herself.

to attribute to celestial bodies what belongs to the earth".[5] It thus was not Copernicus's intention to cause the overthrow of Aristotelian physics. Nevertheless, this was the eventual result of his contribution. The key problem was the absolute Aristotelian distinction between terrestrial and celestial. Within the Copernican system, the earth is *in* the sky. Terrestrial *is* celestial in the precise sense that the earth moves around the sun. How, it was asked, can the physics of the earth and the physics of the sky in that case possibly be distinct?[6]

Copernicus's very basis of dissatisfaction with Ptolemaic astronomy was also Neoplatonistically inspired. Neoplatonism, as mentioned, resuscitated the Pythagorean idea that mathematical form underlies all physical phenomena.

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[5]*De Revolutionibus*, p. 3. Quoted in G. Holton, "Johannes Kepler's Universe: Its Physics and Metaphysics", *American Journal of Physics*, 1956. P. 345. Copernicus's wish not to challenge Aristotelianism earns him the title "The Timid Canon" in A. Koestler's *The Sleepwalkers*, London: Penguin Books, 1959. Copernicus gave the earth a special status in his completed astronomical system, and doing so caused problems for him. In the completed Copernican system, the earth is the only planet without an epicycle. To avoid giving the earth an epicycle Copernicus had to make central to the planets' orbits not the sun itself, but rather a point moving on a circle upon a circle around the sun. Because of the arbitrariness of many of Copernicus's devices, and because of their number, T. S. Kuhn, in his *The Copernican Revolution*, Cambridge, Mass.: Harvard University Press, 1957, argues that (despite what I say below) Copernicanism represented no advance on Ptolemaic astronomy with respect to "simplicity" or "harmony". But Kuhn's charge is much overblown.

[6]For a standard treatment of the importance of Copernicanism in the breakdown of Aristotelianism, see I. Bernard Cohen's classic intoductory work, *The Birth of a New Physics*, London: Heinemann, 1960; or see R. S. Westfall, *The Construction of Modern Science: Mechanisms and Mechanics*, Cambridge: Cambridge University Press, 1971, chapter 1.

Copernicus was dissatisfied with the artificiality of geometrical devices used in Ptolemaic astronomy, and wanted to show that mathematical form properly describes celestial nature, and is not just a part of the cloak of astronomers' descriptions. In the prefatory letter to the Pope in *De Revolutionibus Orbium Coelestium*, Copernicus wrote:[7]

> Supposing these motions which I attribute to the earth later on in this book, I found at length by much and long observation, that if the motions of the other planets were added to the rotation of the earth and calculated as for the revolution of that planet, not only the phenomena of the others followed from this, but also it so bound together both the order and magnitude of all the planets and the spheres and the heaven itself, that in no single part could one thing be altered without confusion among all the other parts and in all the universe. Hence for this reason in the course of this work I have followed this system.

Many regularities that for Ptolemy were simply brute-factual reduced in Copernicus's system to mathematical necessities.[8] For example, many regularities that, in the Ptolemaic system, for no apparent reason tied the motions of planets about the earth to the motion of the sun about the earth, for Copernicus simply reflected geometrical exigencies of his system. It is also noteworthy that the key unobservable parameters of the Copernican system are uniquely determinable

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[7]Quoted in E. A. Burtt, *The Metaphysical Foundations of Modern Science*, London: G. Bell and Sons, 1957. Pp. 63-64.

[8] Clark Glymour's *Theory and Evidence*, Princeton: Princeton University Press, 1980, includes a fine discussion of many such reductions -- see pp. 178-203. See also his "Explanation and Realism", in P. M. Churchland and C. A. Hooker (eds.), *Images of Science*, Chicago: University of Chicago Press, 1985, pp. 101, 110-111.

from observable parameters, whereas many key unobservable parameters in the Ptolemaic theory are not uniquely determinable from observable parameters.[9]

2.4. Kepler. Kepler championed the Copernican system, and also the methodology for theoretical science that he found in Copernicus's writings. Kepler discusses how Copernicus in effect bridged the gap separating mathematical and physical astronomy, and made possible arguments for the truth of one cosmological system whose rival was equally in agreement with astronomical observations. In his *Mysterium Cosmographicum*, for example, Kepler praises Copernicus for discovering how to[10]

> comprehend the causes of the numbers, extents, and durations of retrogradations ... [so as to explain] their agreeing so well with the position and mean motion of the sun. Since in Copernicus' work a most beautiful regularity is revealed in all these things, the cause must likewise be contained therein.

This concern with causation rather than mere mathematical representation was a concern of Kepler's from early days. Ursus had argued that there is no empirical way to distinguish Copernican from Ptolemaic and Tychonic systems. In Kepler's *Apologia pro Tychone contra Ursum* of 1602, an early work that

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[9]Ibid. In Chapter 3 and subsequently we will look further at this kind of virtue in a theory.

[10]Quoted by N. Jardine, *The Birth of History and Philosophy of Science: Kepler's A defence of Tycho against Ursus with essays on its provenance and significance*, Cambridge: Cambridge University Press, 1984. P. 216.

Tycho Brahe had assigned him to compose, Kepler argued in effect that there is no such problem of empirical underdetermination if one looks at the conformability of a cosmology to *physics*. In 1609 Kepler amplified the point in the Preface and Introduction to his *Astronomia Nova*.

Kepler had a mind open to all the new thinking about physics. He took the occult very seriously. All the various facets of Neoplatonism are reflected in his thinking. Because of this, much in Kepler's thinking seems from a modern vantage point to be frivolous mysticism. It seems impossible to hold that Kepler knew well what science ought to be. Yet Kepler's conception of scientific method was exceptionally astute. In his methodological views, Kepler was no transitional figure but successfully grasped the philosophical principles which Newtonian science itself would put in place. Of course in his scientific achievements Kepler was indeed a transitional figure. But it was precisely through his often syncretic attempts to give greater coherence and systematicity to diverse aspects of the thinking of his day that Kepler developed new and revolution-making conceptions of the order of physical nature.

In Kepler's day the Aristotelian framework for physical knowledge was under grave internal tension. But satisfactory reintegration of thinking around new highest-level elements of thought was at least a century away. Despite the problems then developing for Aristotelianism, Kepler's age was not exactly a hiatus in theoretical thinking. Most thinkers were still avowedly Aristotelian, and were without the depth of insight to detect new challenges to old, seemingly well-established fundamentals. In the dynamics of intellectual change critical scrutiny of highest-level elements is not commonplace. With hindsight it is clear that intellectual boldness of just this sort was needed in Kepler's age. But in that age itself a thinker had to have uncommon concern for the overall coherence and systematicity of thought, in order to become critical of such elements. Kepler was such a systematic, philosophical thinker. In order to appreciate why Kepler's contribution counts as *scientific* we must pause to examine, in a very preliminary way, how a concern for systematicity can be crucial for *scientific* advance. To do this I sketch (also for future use) a rough conception of the dynamics of intellectual change.

Imagine ideas about the world to be spread out along a continuum, with those whose truth is judged most readily in the light of experience at one end, and those so highly constitutive of how experience is interpreted that their truth cannot be judged at all readily against experience, at the other. At the one end we have statements about observations, and at the other, the contents of prevailing metaphysics. In between are assorted auxiliary hypotheses about, say, techniques of observation or characteristics of instruments, and other hypotheses and theories, of increasing generality and importance to the prevailing scheme of ideas. The truth-value of any idea along this continuum taken alone is radically underdetermined by the available empirical data. Even observation reports are fallible in innumerable ways, not least in that they themselves rest in part on fallible assumptions, sometimes theoretical assumptions, about what is observed. Nevertheless we can conceive ideas at the observational end of our continuum to be relatively straightforwardly directly conditioned by experience. To achieve conditioning by experience of ideas much further along the continuum, ever more ingenuity is required, for example to make appropriate bootstrap connections with the evidence. Ideas well along the continuum toward the metaphysical end may not be brought into direct relation to evidence. Contrary to the naive empiricist picture of science, according to which all ideas in science are testable in a direct way by experience, typically an idea from empirical science is changed not because of problems with its observational basis, but because of problems, or tensions, that have developed somewhere between the given idea and the observational end of the continuum. Even ideas at the middle of the continuum may initially be thus only indirectly tested, though here it is often straightforward, once the problem is felt, to devise bootstrap tests for inferring to new principles. It is still more true of the ideas at the far metaphysical end of the continuum that they are not challenged by empirical factors so much as by problems, or tensions, that have developed somewhere between them and the observational end of the continuum.

Now the quest for consistency and systematicity in ideas at some remove from the observational end of the continuum helps drive intellectual change. I have suggested that the naive empiricist does not correctly picture this. Nevertheless I shall preserve the essence of empiricism in my own picture, by insisting that ultimately it is accounting for observations that creates the problems that then may propagate further and further along our continuum and become the object of the concern for overall consistency and systematicity. This empiricist premise helps to explain the characteristic sequence of periods of theoretical stability in science interspersed with sharp breaks. For it inevitably takes some while for the problems that develop from the observational end inward to overwhelm the central theoretical ideas of a given age near the far metaphysical end of the continuum. The central theoretical ideas in any intellectual epoch are highly constitutive of thinking during that epoch, and are understandably not easily or quickly changed.

Kepler was the sort of figure to carry intellectual change to the far metaphysical end of this continuum. It was not incidental, but vital, to his having this character, that he was interested in all the currents of thought in his day, and in the question how the systematicity of these ideas could be improved. We shall see that the sophisticated empiricist picture of science just sketched was precisely Kepler's own picture of science. Thus a systematizing thinker is just what Kepler conceived himself to be.

In Figure 1, overleaf, I give a simplistic example of the continuum that I have just discussed. When Kepler discarded the hypothesis of the crystalline

METAPHYSICS

Part of the essential difference between *earth* and the rest of the *cosmos* is that the former is imperfect and the latter perfect; the perfection of the heavens is exemplified in the immutable crystalline spheres upon which the planets ride.

Nothing courses on the heavens except the planets themselves -- no orbs, no epicycles.

~~Every planet rides on a crystalline sphere.~~

Whatever a comet is, it can't pass through crystalline spheres.

That comet passed through the celestial spheres!

Within one thousand radii of the lunar orbit in any direction are other planetary orbits.

l is the least diurnal parallax consistent with the comet's being inside the lunar sphere.

That comet was a thousand-fold closer at its biggest and brightest than when first observed.

Diurnal parallax for that comet was never greater than *l* but when the comet was almost its biggest and brightest was greater than *l*/10.

That comet was a thousand-fold brighter and bigger at its brightest and biggest than when first observed.

One and the same comet was observed over 56 days, getting gradually brighter and bigger and angularly closer to the sun, then gradually dimmer and smaller and angularly farther from the sun.

That comet and the comets observed over the past 56 days are one and the same.

OBSERVATION

That's a comet.

Resolution, through a change in highest-level thought, of theoretical tensions developed out of Tycho's observations of comets. (Schematic.)

Figure 1

spheres for the view that "nothing courses on the heavens except the planetary bodies themselves -- no orbs, no epicycles",[11] he made a modest but important change in metaphysics. This could not have been required directly by observation, but rather was a rational response to tensions in middle-level theoretical ideas. This is a simple example of change driven *directly* not by observation but by a quest for consistency in ideas at some remove from the observational end of our continuum. In fact this quest brought Kepler into opposition with some of the deeper parts of the prevailing metaphysics of his day. Kepler's greatness depended upon his carrying his quest for better knowledge well beyond matters directly susceptible to empirical test.

Kepler recognized that ideas vary in the directness with which their truth may be assessed in the light of experience. He wrote that

> in all acquisition of knowledge it happens that, starting out from those things which impinge on the senses, we are carried by the operation of the mind to higher things which cannot be grasped by any sharpness of the sense.[12]

Kepler has in mind here the relation of astronomical hypotheses to experience. He sees that astronomical systems are at some remove from observation, and may require more than the facts of observational astronomy to decide between

[11]Letter to Fabricius (August 1, 1607). Quoted in G. Holton, "Kepler's Universe", op. cit. P. 345.

[12]In his *A Defence of Tycho Against Ursus*, op. cit. P. 144.

them. Kepler also recognized that there are problems relating to empirical falsification of hypotheses. He writes that it is "normally" in the nature of a false hypothesis to betray itself empirically:

> ... just as in the proverb liars are cautioned to remember what they have said, so ... false hypotheses, which together yield the truth once by chance, do not in the course of a demonstration in which they have been combined with many others retain this habit of yielding the truth, but betray themselves.[13]

But according to Kepler there are two problems with this ready optimism about the falsifiability of false theories. The first, in essence, is the Quine-Duhem thesis: it is always hypotheses *in combination* which yield us predictions, and because of this the falsity of a prediction never logically falsifies any one hypothesis. There is always a choice to be made among the combined hypotheses about where to assign blame for the false prediction. Kepler answers this problem in the spirit of a criminal detective. We will eliminate the least well-tested hypothesis and retain hypotheses formerly employed to yield true predictions; but we will make every such step tentative, and will maintain a readiness, should false predictions continue to flow from the hypotheses that remain, to reverse, re-enlist a discarded hypothesis, and tentatively set out anew with a different hypothesis rejected.[14] On Kepler's view a rational investigator does not guard against capricious choice, but rather recognizes its inevitability, and

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[13]*A Defence of Tycho Against Ursus*, op. cit., p. 140.

[14]*A Defence of Tycho Against Ursus*, op. cit., *passim*.

depends on good sense and memory to correct for it. A second problem for empirical falsification concerns *ad hoc* manoeuvres that may protect any hypothesis from empirical falsification. Someone's hypothesis will be immune from falsification

> [if] you gratuitously allow him who argues to adopt infinitely many other false propositions and never, as he goes backwards and forwards [in his reasoning], to stand his ground.[15]

According to Kepler this problem is not serious, for such a person as would so freely invent new hypotheses to defend old ones seeks no system for thought. We next come to Kepler's arguments for the methodological importance of the pursuit of systematicity in knowledge.

Early in the *Mysterium Cosmographicum* Kepler began a defence of the Copernican hypothesis in the following way:

> I have never been able to agree with those who, relying on the example of an accidental demonstration, which with syllogistic necessity yields something true from false premisses ... used to maintain that it could be that the hypotheses which Copernicus adopted are false, but nevertheless the true phenomena follow from them as if from genuine principles.[16]

Kepler then discussed an objection that may be immediately made against this.

> It can be said with some truth today (and could have been said with some truth in the past) that the ancient tables and hypotheses satisfy the phenomena. Copernicus, nevertheless, rejects them as false. So, by the same token, it could be said to Copernicus that although he accounts

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[15]Quoted in Jardine, *The Birth of History and Philosophy of Science*, op. cit., p. 215.

[16]Ibid.

> excellently for the appearances, nevertheless he is in error in his hypotheses.[17]

There are two parts to Kepler's reply. The first part introduces the view that an astronomical system's adequately "accounting for the facts" involves more than conformity with the data of observational astronomy. It also involves conforming to other, general, knowledge demands, including that the system should make good physical sense. The second part suggests that when incompatible hypotheses yield the same true predictions, this is often because there is in the world some deeper aspect of structure than that to which each hypothesis explicitly reflects, a deeper aspect which in fact is implicit in both hypotheses. This second part is remarkable for its level of abstraction. It achieves considerable clarity concerning the very new idea of relativity of motion. As we shall see, in some ways it suggests the position of structural realism that I shall attribute to Newton, Maxwell and Einstein.[18]

Kepler insisted that knowledge must form one system and hence that astronomy must connect with physics. His insistence on the systematicity of knowledge opened the door from empirical science into a former preserve of speculative metaphysics (the constitution of the heavens and the causes of

[17]Ibid.

[18]For a fuller discussion of it see Jardine's *The Birth of History and Philosophy of Science*, op. cit., pp. 216-221.

celestial motions) and united the domains of "mathematical" and "physical" astronomy that virtually all Kepler's contemporaries thought irreconcilable.[19] Kepler took a decisive step away from Aristotelian physics in asserting a doctrine of *inertia*, not the modern doctrine that equates all uniform motions, but one nonetheless incompatible with Aristotle's doctrine of natural place: Kepler asserted that every body, if undisturbed, will naturally come to rest wherever it may be. However, two bodies near one another disturb one another with a mutual affection, tending to draw every body towards its neighbour. This explains terrestrial gravity; and the oceanic tides are explained by the attractive action of the moon.[20] Kepler insisted that physics should concern itself not with *anima* but with *vis*. His belief that the planets must be acted upon by some (tangential) force to keep them moving in their orbits increased Kepler's concern to have due regard in all his work for the paramount physical importance of the sun. Kepler's discoveries were so often reached only through the influence of physical considerations, that, without his innovative linking of "physical" and

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[19]The view of Girolamo Fracastoro, quoted in Jardine, *The Birth of History and Philosophy of Science*, op. cit., p. 216, is quite typical:

> Those who employ homocentric spheres never manage to arrive at an explanation of the phenomena. Those who use eccentric spheres do, it is true, seem to explain the phenomena more adequately, but their conception of these divine bodies is erroneous, one might almost say impious, for they ascribe positions and shapes to them that are not fit for the heavens.

[20]Galileo condemned this explanation as stupid, since, he said (mistakenly), it should require there to be but one tide per day instead of two.

"mathematical" astronomy, little would have come of all his labours. One historian of science suggests that without this innovation the laws of planetary motion would not have been discovered for generations:

> The conclusions which seem to me to follow ... are firstly, that the great majority of scientists in Kepler's day were committed to a single explanatory schema, either geometrical or physical; and secondly, that neither of these, in isolation, could have led to the discovery of the first two laws. ...So long as the gulf between physical and geometrical schemas remained unbridged, there was no plausible way to arrive at Kepler's laws.[21]

Now of course the corpus of thought within which a great thinker in Kepler's age would seek greater coherence included many elements far removed from modern science. This in itself can be no basis for criticism, but should remind us rather that the struggle out of which our modern scientific viewpoint emerged was arduous and complex. Kepler's many-sided involvement with the intellectual problems of his day was by no means incidental to his greatness as a scientist. Kepler's pioneering use of modern-looking notions of mechanism, mathematical harmony, and unseen forces, connected closely with his critical interest in astrology, alchemy, and numerology; theological tenets acted for Kepler, and for other thinkers for a century to come, as an essential bulwark against potentially devastating criticism of the still emerging, still problematic

[21]J. L. Russell, "Kepler and Scientific Method", *Vistas in Astronomy*, vol. 18, 1975. Pp. 744-745.

new picture of physical reality.[22] Kepler's insights in physics and astronomy went far beyond the general state of knowledge in his day, but this was in part because of -- *not* in spite of -- his attention to problems in astrology, theology, and so on.

Kepler pursued a conception of a unified cosmos within three superimposable idioms:

> the universe as physical machine, the universe as mathematical harmony, and the universe as central theological order.[23]

The idiom of central theological order crucially influenced Kepler's work within the other two idioms. It inspired the inquiry into celestial physics, and without its inspiration Kepler would never have carried through the arduous labours that gave the three laws of planetary motion.

In the excellent studies that exist of Kepler's path to the discovery of his eponymous laws,[24] one learns how the mechanistic idiom helped Kepler at all

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[22]See K. Hutchinson, "Supernaturalism and the Mechanical Philosophy", *History of Science*, vol. xxi, 1983. Pp. 297-333.

[23]G. Holton, "Kepler's Universe", op. cit., p. 351.

[24]See, for example, Curtis Wilson, "How Did Kepler Discover His First Two Laws?", *Scientific American* (1972) *226*: 93-106. For a treatment of the uncertainties and conjectural leaps in Kepler's path to the laws of planetary motion, and for a discussion of well-grounded caution by some contemporaries against accepting them as true, see part 1 (pp. 92-105) of Wilson's, "From Kepler's Laws, So-called, to Universal Gravitation: Empirical Factors", *Archive for History of Exact Sciences 6*: 92-170, 1970.

stages but the last. Along the way to his chief discoveries, Kepler's search for mechanical understanding conditioned his hypotheses in helpful ways. It led Kepler to increase the stature of the sun and to decrease the stature of the earth in his elaboration of Copernican astronomy. (Kepler's was the first truly heliocentric astronomy, and the first properly to conceive the earth as just another planet.) It led Kepler through a host of important advances, among them his jettisoning of epicycles, his positing the concurrence of all the planets' orbital planes in the sun, his deciding to calculate lines of apsides as passing through the "true" rather than the "mean" sun, his gleaning an important clue from the principle of the bisection of the eccentricity, and his hitting upon and adopting the Second Law (law of areas). But at the last, it let Kepler down. In converging on the ellipse hypothesis and thus the First Law, Kepler came to a conclusion about the motions of the planets that made no sense to him mechanically. He could not relate the new elliptical motion to any believable mechanism at work between the planets and the sun. Every step of the inquiry had been guided by Kepler's desire for mechanical insight, but just this eluded Kepler when his kinematic theory reached its finished form. Here we must acknowledge the importance of the second and third idiom within which Kepler conceived unity in the solar system. Because the mechanical idiom for his investigations had failed him, Kepler depended rather on the idioms of harmony and theological order. He did not look further for a mechanism behind the second law, but effectively transformed a causal question (why the planets move in the way they do) into a question concerning the formal simplicity, or systematicity, or harmony of the set of ideas that says: the planets move in such-and-such a way and not otherwise. This "formal" notion of cause links causes to laws and laws to the systematicity of knowledge.

Kepler could not doubt the high merits of his planetary theory. Especially after the discovery of the third law (which mathematically linked all the planets' orbital motions together as a unity), Kepler had truly triumphed in his Neoplatonism. Through inspired speculation he demonstrated beyond any doubt the value of mathematics for discovering hidden patterns in nature's constitution. He completely removed the artificiality of what is mathematical in astronomy, and clearly demonstrated that mathematics somehow reaches deeper into nature than the cloak of our descriptions of it. There, on the grand scale of the planets in their orbits, in the ponderous turnings of nature itself, was mathematical form. What an indictment this was of the Aristotelians' acquiescence in merely qualitative thinking! Kepler's laws elevated anti-Aristotelian occultism by virtue of their mathematical form. Kepler had at last provided some impressive evidence that there is hidden unity or systematicity in nature.

2.5. Galileo. Galileo is usually portrayed to have been a far soberer thinker than Kepler.[25] Galileo's thinking was relatively unaffected by the occultism of

[25]That would also be Galileo's own assessment. Galileo seems to have been thoroughly put off by Kepler's mindset, and he refused to sustain any correspondence with him.

the Renaissance, except that Galileo speculated about physical microphenomena, and of course severely criticized the obscurantism of the Scholastic anti-occultist doctrine of substantial forms. The mystical elements associated with Neoplatonism seem to have repelled Galileo. For example, while Galileo praised Gilbert for the "stupendous concept" of magnetic force and for "the many new and sound observations" Gilbert had adduced in its support, he thought Gilbert's reasonings were often overly speculative.[26] He said they "lack that force which must unquestionably be present in ... necessary and eternal scientific conclusions".[27] Moreover, on Galileo's official view his own speculations about microphenomena lacked the rigour of science proper. Properly, science should be defined by the standards of rigour set by Euclid -- standards discussed as an ideal for knowledge by Aristotle, and for Galileo epitomised in physics by the work of Archimedes.

Galileo, like Kepler, was an inspired Copernican. Unlike Kepler, Galileo was not concerned to adapt the Copernican cosmological system so that it could fit the facts of observational astronomy. Such a task Galileo thought was impossible:

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[26]From Galileo's *Dialogue*; quoted by Ernan McMullin on p. 220 in his "The Conception of Science in Galileo's Work", in R. E. Butts and J. C. Pitt (eds.), *New Perspectives on Galileo*, Dordrecht: D. Reidel, 1978. Pp. 209-258.

[27]Ibid.

> I assure you that the movements, sizes, distances and arrangements of the orbs and stars will never be observed so accurately that they will not need endless corrections, even if the world were filled with Tycho Brahes or men a hundred times as good as he was. We can be certain that there are many movements, alterations, anomalies, and other things in the heavens as yet unknown or unobserved, and perhaps not even observable or explainable in themselves. Who can vouch that the movements of the planets are not incommensurable, and therefore susceptible to -- or rather in need of -- eternal emendation, since we can only deal with them as though they were commensurable?[28]

Galileo thought folly Kepler's search for an accurate *system* for the motion of the planets, and never so much as commented on Kepler's laws of planetary motion.

While Galileo thought hopeless the task of perfecting the Copernican system for the purposes of predictive astronomy, he nevertheless upheld Copernicanism as truth, and he believed that this demanded the complete rethinking of physics. Galileo made two-way this interaction between astronomy and physics: from Copernicanism he adduced grounds for dismissing Aristotelianism and upholding a new physics, and from the new physics he adduced grounds for upholding Copernicanism and dismissing its rivals. The remark quoted above indicates that the standards Galileo would set for the conformability of astronomy to physics were not high. The physics he would develop ruled out geocentric cosmologies, but accounted physically for only an almost absurdly idealized Copernican system: a heliocentric model with all the planets traveling in circles at uniform speeds.

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[28]Letter to Ingoli (1624), quoted in McMullin, "Galileo's Work", op. cit., p. 235.

In his work on motion, Galileo like Kepler made some singular discoveries of underlying mathematical form in nature. Galileo successfully formulated some of the basic laws of mechanics. (Notoriously, Galileo's conception of inertia had a circular aspect; it was preadapted to what Galileo took to be the needs of Copernican astronomy.) Galileo's most notable success was the formulation of a kinematical law of free fall and his ingenious use of it in accounting for some general facts of projectile motion. Galileo researched the "how" and not the "why" of phenomena concerning free-fall, and he did this in an idealizing way. The rigorous mathematical form that Galileo set forth applied only to preselected, or as he put it "well-chosen", free-fall phenomena (concerning, say, the flights of cannonballs but not of feathers) and even here it applied only approximately. Galileo ruled out the bulk of experience as a basis for inferring to physical principles. Most motions are hopelessly complicated by various departures from ideality, departures due to complex, uncontrollable, and often unfathomable "impediments of matter". When an object falls, for example, inevitably some more or less significant

> disturbance arises from the impediment of the medium; by reason of its multiple varieties, this is incapable of being subjected to firm rules, understood, and made into science. ...No firm science can be given of such events of heaviness, speed, and shape, which are variable in infinitely many ways. Hence, to deal with such matters scientifically, it is necessary to abstract from them.[29]

[29]From the *Discourses*, quoted by McMullin, "Galileo's Work", op. cit., p. 232.

Even once we have grasped simple underlying principles, Galileo thinks, "the bounty of nature in producing her effects" so overwhelms our ratiocinative abilities that "there is not a single effect in nature, even the least that exists, such that the most ingenious theorist can arrive at a complete understanding of it".[30]

As Koyré has emphasized, Galileo's quest for mathematical results and consequent "necessity of the demonstration" was often conducted with something like a Platonic disregard for the senses.[31] It is well known that Koyré exaggerates his case for one-sided Galilean rationalism.[32] Yet Galileo himself said that all his work on motion argues merely

> *ex suppositione* ... so that even though the consequences should not correspond to the events of the natural motion of falling heavy bodies, it would little matter to me. ... But in this I have been, as I shall say, lucky: for the

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[30]McMullin, "Galileo's Work", op. cit, pp. 222-223, quoting from *The Assayer* and the *Dialogue*.

[31]See his *Metaphysics and Measurement*, Cambridge: Harvard University Press, 1968, and his *From the Closed World to the Infinite Universe*, Baltimore: The Johns Hopkins Press, 1957.

[32]Stillman Drake is the best-known defender of Galileo's empiricism against Koyré's sweeping claim that Galileo, like Plato, was a rationalist sceptic about the senses. Drake presents a strong case for thinking that Galileo experimented and experimented brilliantly, and that his theorising was strongly conditioned by his doing so. See, for example, Drake's *Galileo* (Past Masters Series), London: Oxford University Press, 1980. Drake's *Galileo Studies: Personality, Tradition, and Revolution*, Ann Arbor: University of Michigan Press, 1970 is a sustained historiographical and historical critical reaction to Koyré's thesis.

> motion of heavy bodies and its events correspond punctually to the events demonstrated by me from the motion I defined.[33]

The good luck is supposed to be that even though actual phenomena can generally be expected to conform quite poorly to the mathematical ideal, some phenomena actually met with in nature (the flights of cannonballs, for example) conform quite well to the mathematically defined ideal motion.

Aristotle's *Physics* concerned the bulk of everyday experience directly. Galileo's physics by no means confronted the bulk of everyday experience directly. Galileo pointedly set aside the demand that physics do justice to everyday experience and used his principles, which he saw as defining ideal motion, to *reinterpret* everyday experience. On Galileo's reinterpretation, the bulk of everyday experience cannot be understood in relation to basic principles. Rather it reflects "impediments" peculiar to the concrete situation at hand, and it is most likely far too complicated to find any explanation in physics. Paul Feyerabend thinks that this move of Galileo's is evidentially unwarranted in principle, since it remakes the evidence itself. Thus Feyerabend labels Galileo's defences of the new physics "irrationalist propaganda".[34] Certainly Galileo has closed the hermeneutic circle in his effort to interpret nature -- he *does* remake

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[33]Letter to Baliani (1639); quoted in McMullin, "Galileo's Work", op. cit., p. 234.

[34]*Against Method*, London: Verso, 1978.

the evidence itself in a way that serves certain theoretical ends. Galileo fundamentally alters the *way* of thinking about ideas that are very closely tied to observation. But Feyerabend's charge of irrationalism is completely mistaken. The move Galileo makes is not an irrational one; rather it signals Galileo's greatness as a scientist. Precisely such reinterpretation of evidence will result when thinking is changed at the metaphysical end of the continuum discussed in section 2.4, thus changing the integrative elements in thought. That Galileo made these changes shows that he was able and willing to move past questions concerning the lower-level theories of the day and their observational successes, to raise questions concerning the mutual coherence of these theories and our understanding of their "observational success". He found the thinking of his day in many ways badly incoherent, and so, to remove the many lower-level tensions, he remade highest-level elements of thought. One highest-level element that he changed was the belief that physics should inform us directly about the bulk of everyday experience.

The sense in which a science of physics is at all possible is for Galileo a particularly interesting one. It is clear that on Galileo's view, physics, through its laws, grasps some fundamental systematicity in nature. Must Galileo not think however that the "impediments" he discusses (as preventing the laws from being exactly realized) largely destroy this systematicity? He says, after all, that in most observed events impediments prevent the laws from being even approximately realized. Nevertheless, Galileo clearly does not think that impediments destroy nature's systematicity. He treats the laws as true, and insists that nature is truly comprehensible in terms of them. Evidently Galileo holds that if, *per impossibile*, a complete account could be given of *everything* contributing to a given effect, matter's impediments would simply disappear as such and the effect would be seen to be in complete conformity with law. Impediments are not really exceptions to law, but are simply hopelessly complicated products of nature's acting in conformity with law. This presumably is part of what he means when he writes of the "bounty of nature in producing her effects".

2.10. Conclusions. The evidence that had accumulated by the middle of the seventeenth century was really quite thin, despite Kepler's and Galileo's successes, for the view that nature is a unity, or thus that a physics of the world is possible. It also remained philosophically obscure how *empirical* evidence could possibly weigh for the general thesis that nature is a unity. How could so general a rationalist conviction as that nature is fundamentally comprehensible, possibly be warranted by experience? It seemed rather to express a chosen goal for than a determinate item of empirical knowledge. One thing was certain, however. By adopting this rationalist goal within empirical science, impressive advances could be made in knowledge. Thus the idea of the unity of nature was prominent in all the general philosophies of the day.

By the middle of the seventeenth century, natural unity was thus the leading idea of the age, despite its tenuous basis in evidence. We shall next see how Newton compounded the successes of this idea, and also developed methods for science that bring into sharper focus the relation of evidence to the unity-of-nature thesis. The following points have emerged from our consideration of the early history of physics. (1) In astronomy, the investigation of causes was long held separate from attempts to improve formal, mathematical description of phenomena, but Copernicus changed this, and Kepler completed the transformation, not only showing that consideration of causes is heuristically important for developing a mathematical astronomy that is predictively the best, but also, in the end, fully translating a causal question (why the planets move in the way they do) into a question concerning the formal simplicity, or systematicity, or harmony of the set of ideas that says: the planets move in such-and-such a way and not otherwise. This "formal" notion of cause links causes to laws and laws to the systematicity of knowledge. (2) Even in Copernicus's work (in arguments much improved upon by Kepler) a premium is placed on *empirical overdetermination* of key theoretical parameters, and parameters that can be thus measured are supposed to belong to the real causes of the phenomena. (3) Kepler and Galileo both recognized that efforts to systematize the phenomena may condition what are counted *as* the phenomena. Galileo argued forcefully that, to accommodate changes in highest-level theory, the very understanding of "observational success" of scientific theories needed to change. Through these arguments Galileo closed the hermeneutical circle in the interpretation of nature, completely remaking, for theoretical ends, the evidence from which a theory of physics would make its start. His efforts however were not "irrationalist propaganda" (as Feyerabend has argued) but rather (according to the "sophisticated empiricist" perspective on science adopted here) amounted to great science, science seeking coherence and systematicity near the metaphysical end of the observational-metaphysical continuum, even as it reworked ideas near the observational end to make this possible. (4) Galileo's conception of the relation of theory to evidence placed great weight on idealization. This is not to say that Galileo saw the world (as does Cartwright) as an ultimately unsystematic place, about which systematic theorizing is possible only with the aid of idealization. Galileo believed in natural systematicity, and in the fundamental truth of his laws. But it does mean that the evidence, from Galileo's own work, for the view that a physics of the world is possible, is painfully partial and unconvincing.

Chapter 3

Newton's Advance beyond Galilean Idealization

Like Kepler and Galileo, Newton sought a match between physics and cosmology that could carry the day for heliocentrism. Before he undertook this work, Newton had learned much about physics and its methods, mostly by negative example,[1] from Descartes; the young Newton's thorough and profound

[1]Descartes made a profoundly negative impression on Newton. Newton reacted with distaste to Descartes' overconfidence about the truth of his sometimes absurdly speculative doctrines. In Newton's view, for example, Descartes' entire vortical cosmology was mere "philosophical romance" (General Scholium to Book II of the *Principia*). Newton's standards for himself in his public presentation of his own thinking were significantly shaped by his reaction to Descartes. Newton determined that he would show, by his example, a better way for natural philosophy to be done than Descartes had shown. Because of this determination, Newton made, as we shall see, great advances in *methodology*. Newton could not have moved farther from Descartes' a priorist conception of method by insisting that not only principles of physics must be learned from experience but also the principles governing how one learns principles of physics from experience must be learned from experience. At the same time, Newton's resolve to account to himself for all the presuppositions of his thinking, to withhold all thinking based on hypotheses or merely speculative presuppositions, and nevertheless to deliver results in natural philosophy, made him attain new and lasting insights on what it is to warrant principles in natural philosophy. Descartes' contributions to Newton's thought by *positive* example were few, but significant. Examples include Descartes' statement of the law of inertia, his discussion of refraction in his *Dioptrique* (a discussion that Newton called "not inelegant" and applied in his own work), and his introducing an understanding of quantity of motion -- in Descartes' terms, volume times speed; in Wallis's terms, weight times velocity; and finally in Newton's *Principia*, mass times velocity -- that could lead Newton (as an energistic conception of quantity of motion could not since Newton's resolution of motion was always into infinitesimal components of time rather than of distance) to his eventual notion of impressed force. (See I. B. Cohen, "Newton's Second Law and the Concept of Force in the *Principia*", in R. Palter (ed.) *The Annus Mirabilis of Sir Isaac Newton*, Cambridge, Mass.: MIT Press, 1970. For a fuller discussion of Newton's

critique[2] of Descartes' *Principles* gave him his first arguments for a career-long battle against the Cartesian theory of vortices and sharpened his grasp far beyond Descartes' of the consequences for the theory of motion of the principle of inertia, a subject of study for Newton that spanned two decades, and in which Newton would slowly transform the concept of force from quantity of motion to change in quantity of motion.[3] Descartes' *Principles* also set Newton to thinking about the presuppositions, effectively concerning space-time structure, that the principle of inertia carried along with it, presuppositions that Descartes had quite completely failed to grasp.[4] Newton had read some of Galileo's work (not, however, *Two New Sciences*) and thoroughly studied Christiaan Huygens' *Horologium Oscillitorium* soon after its publication in 1673. For these reasons Newton had from early days a leg in to the newly developing discipline of rational mechanics, and a great leg up on Kepler and Galileo (who had had to forge

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indebtedness to Descartes in this regard, see John Nicholas, "Newton's Extremal Second Law", *Centaurus 22*: 108-130.)

[2]In his "De Gravitatione et aequipondio fluidorum et solidorum"; original Latin and English translation in A. Rupert Hall and Marie Boas Hall (eds.), *Unpublished Scientific Papers of Isaac Newton*, Cambridge: Cambridge University Press, 1962.

[3]See J. Nicholas, "Newton's Extremal Second Law", op. cit.

[4]See Howard Stein, "Newtonian Space-Time", *Texas Quarterly X*, 1967. Pp. 175-200. Republished as R. Palter (ed.), op. cit., pp. 258-284. See also Robert Palter, "Saving Newton's Text: Documents, Readers, and the Ways of the World", *Studies in History and Philosophy of Science* (1987) *18*: 385-439.

their mechanical conceptions from scratch) in undertaking to match physics with cosmology and cosmology with physics.

Newton's *Principia* has three facets beyond the integral initial project of articulating the new mechanics: first, the successful completion of the Kepler-Galileo programme for solving the problems of physical cosmology and in this way establishing Keplerian heliocentric astronomy over Tychonic geocentrism; second, a host of special contributions to rational mechanics; and third, by far the most powerful and original facet of the three, using the same physical principles that delivered Keplerian heliocentric astronomy as a first approximation to account in detail for the planets' actual deviations from Keplerian motions. That Newton's arguments in the first of these facets use idealized astronomical phenomena to infer to physical principles is a fact that the third facet of Newton's own work serves to underline. But the third facet of *Principia* also begins the dissolution or discharging of those idealizations -- a task Newton himself carried to resounding success on some but not all of the problems that concerned him. Let us compare this with Galileo.[5] Recall that Galileo expected the motions of the planets to be no less beset by "impediments" than motions of, say, projectiles on earth. Galileo freely "abstracted" from the complexities of actual planetary motions. He proposed to account physically for an almost

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[5]George Smith made this comparison in a very stimulating lecture of his that I attended at the University of Western Ontario in 1989.

absurd idealization of these motions: a model with all the planets including the earth traveling in unique (sun-centered) circles at uniform speeds. It is true that by the light of Galileo's physics, geocentric cosmology could not possibly be sustained. Only Copernicanism was even roughly conformable with Galileo's physics. Yet no Copernican model fully conformable with Galileo's physics could possibly recover the facts of observational astronomy. Galileo taught that only such first-approximation conformability of fact with physics was to be expected. For Galileo, idealization is a feature of physics that can never be dissolved or discharged.

Before considering Newton's advance beyond Galilean idealization, I shall discuss some other advances by Newton that also are relevant to the consideration of Cartwright.

3.1. The concept of force. It is well known that Newton initially conceived force to be quantity of motion or what we call momentum; even in the *Principia* Newton applies the term 'force' to finite impulses Δmv. Yet in the *Principia* Newton fully possesses the concept of force according to which force is "impressed" *continuously* on a body and is proportional at any moment to the *instantaneous time rate of change* in the body's momentum or quantity of motion. It is illuminating both historically and philosophically to regard the concept of continuously acting dynamical force as akin to the concept of force from statics.

This is historically illuminating, because John Wallis fashioned a concept of continuously acting dynamical force precisely by extending the force conception from statics to the dynamical case (albeit without satisfactorily breaking free of the concept of impetus with its residual Aristotelianism when he took this step); and Newton was influenced by Wallis's work (as is reflected in the Scholium to the Axioms or Laws of Motion certainly in Newton's reference to Wallis, and possibly also in what he says about machines). This is illuminating philosophically because a condition governing forces in statics is that they always and everywhere sum to zero. We will see that Newton in effect generalizes this condition to the dynamical case, and that consequently Cartwright's "dilemma of the double effect" never legitimately arises.

Newton approached the study of mechanics with the new principle of inertia in hand. This principle placed constant motion on a level with rest. The principle of inertia demanded that constant motion be a state of which rest is a special case. Now statics concerns forces in systems that retain their form over time. Statics thus concerns systems whose parts are all at rest. Because they are at rest, the parts of the system all retain their state; that is why the science is called statics. Given the principle of inertia, however, it becomes necessary to think that a system (for example of many bodies all moving inertially but not all in the same state of motion) can change its form without any of its parts changing their states. On the force-conception in statics, forces must always sum to zero. The principle of inertia dictates that this condition of balanced forces will be satisfied by at least some systems that change their form over time, namely, by systems all of whose parts move inertially. Newton asked, what about the systems some or all of whose parts move non-inertially? Can forces (in the sense taken from statics) sum to zero in such systems also? The forces in a static system may make something snap, and parts of the system may be made to accelerate and fly away. Before the snap, forces sum to zero -- what about immediately after the snap? In effect Newton's answer generalizes the condition that forces sum to zero to cases like this one in which there are non-constant motions. Newton conceived that even in cases of non-constant motions the forces within a system always sum to zero. The accelerated parts react back against the forces that accelerate them just as strongly as they would if they were held rigid against those forces. The condition that forces sum to zero can be satisfied precisely through some or all the system's parts thus sustaining acceleration, but resisting it. The force component in this last kind of case without which the condition that forces sum to zero would be violated -- let us call this (in the spirit of Newton's own thinking) the "inertial force" component -- is proportional to the instantaneous time rate of change in the quantity of motion of the accelerating part.

Cartwright's "resultant force" is equal and opposite to Newton's inertial force component. Cartwright's view in *Laws* is that only what she calls the "resultant force" is real. It is real precisely because it is manifested in its own, separate effect by which it can be measured (*Laws*, pp. 60-61). There are two immediate problems with this view. (We shall take up some different lines of argument against it below.) First, it is questionable whether the acceleration of a body (or therefore the "resultant force" in Cartwright's sense) that is acting on it can ever be truly "manifest". Newton gives grounds for doubting that it can. Corollary VI of the Axioms or Laws of Motion implies that the apparent effects are the same if the whole system relative to which a part is accelerating itself is accelerating en bloc, all its parts "urged in the direction of parallel lines by equal accelerative forces". Yet the true inertial force component is determined by the *absolute* acceleration of the part. Second, Cartwright's implicit criterion of reality from *Laws* (that a "real, occurrent force" (p. 60) must be manifested in a "separate effect" (pp. 60-61)) implies that statics does not deal with forces at all For in statics, there are by definition no dynamical, observable effects by which forces are manifested and by which they may be measured. Bridge-building engineers would have difficulty with the conclusion that statics in fact does not deal with forces. Of course it is the engineers' whole purpose to prevent there ever being in the bridges they build any accelerating parts. Yet the forces that they calculate *can* have dynamical effects, for example, if a dishonest subcontractor uses substandard materials and the bridge fails. The reality of the force components seems perfectly well-founded in what *would* happen *if* the bridge's weight limit were disastrously exceeded, *if* such-and-such a girder were capriciously removed, and so on. These things sometimes do happen and the effects in those cases can be used to measure the normally unobservable forces. It is very difficult to sustain doubts about the reality of component forces when this connection between statics and dynamics is clearly seen.

Moreover, Newton's statical-cum-dynamical conception of force completely dispenses with Cartwright's dilemma of the double effect. It is easy to see that on Newton's conception, there can be component forces *and* a resultant force without this threatening to double the force-effect. For the "resultant" force is always zero; sum the components with the resultant and the result is the same as summing the components -- zero. This resolves a very small ostensible difficulty for Newton that Cartwright raised. In a moment we will start from Newton's conception and press some difficulties upon Cartwright.

First I will remark that Cartwright's discussion of the question of the reality of component forces is strangely convoluted. In *Laws* Cartwright takes a step toward the capacities conception of her second book, by arguing for the distinction that Hume disallowed "betwixt *power* and the *exercise* of it" (p. 61). She argues that individual force laws may correctly identify *powers*, but that, because of the interfering action of other force laws, they each fail to describe correctly the *exercise* of such powers. She implies that realism about the powers goes hand-in-hand with denying the "facticity" view of laws (pp. 61-62). Against this whole way of thinking, one could propose that the power and its being exercised *are* one and the same, and that an individual force law, by correctly identifying a component force, also correctly describes the exercise of this power -- that there is a component effect due to the component force and due to it alone. Cartwright explicitly argues against this picture but makes a key concession that can be used in its support. She admits that the law of interaction in theoretical mechanics, the vector law of composition of forces, is exceptionless. This leads her to concede that laws in pure theoretical mechanics are true; she insists not that all theoretical laws are false, but rather that among all theoretical laws, only laws in pure theoretical mechanics are true (*Laws*, p. 63; *Capacities*, p. 163). Given these concessions, however, Cartwright has no good reason to deny the reality of separate effects arising from the action of different fundamental force laws. Her arguments in support of a distinction between a *power* and its *exercise*, and against the "facticity" conception of laws, are even by her own concession not cogent in respect of force laws. She admits that the "components scheme" "may well be right about ... forces" (p. 66). To illustrate her argument to the conclusion that there are not separate effects corresponding to the action of each of several interacting laws or powers, Cartwright switches examples. At some length (pp. 63-66) she discusses a case, the study of irreversible processes, in which no general law of interaction is available. In this example, it does indeed seem unlikely, just as Cartwright insists, that there are separate effects corresponding to the action of each of several interacting laws or powers. By switching to this example, Cartwright show that an obvious objection to what she says about force laws like the law of gravity *would* be unavailing *had* she been talking instead about irreversible processes. Switching back to the case of forces, she introduces the separate worry that I have called the dilemma of the double effect. (She says not only that in the case of forces there cannot be both the components *and* the resultant, lest the effect be double what it should be, but also that assuming there are just the component forces rather than just the resultant force implies a more complicated causal picture of how the effect comes about, one that interposes an intermediate event of "summing".) These attempts by Cartwright to scotch attention to the idea of component effects, and thus to preempt an obvious objection to her views, are hardly convincing. The first reply is an obfuscation. Cartwright is supposed to be considering the facticity of fundamental laws. But the laws for irreversible processes that lack a general law of interaction are not fundamental laws, as Cartwright herself points out. So the first reply not only changes the example; it strays from the topic. The second reply mistakenly supposes that there ever is a non-zero "resultant force", a conception that, by following Newton, we have already seen our way past.

In *Capacities* (see pp. 141-182) Cartwright treats fundamental force laws as *true* ascriptions of capacities or "tendencies". But she continues to insist that fundamental force laws fail to get right the facts about "effects". Though they are truths when read as ascriptions of capacities, fundamental force laws, she says, are falsehoods when read as descriptions of regularities. In general,

> fundamental laws are not true, nor nearly true, nor true for the most part. That is because fundamental laws are laws about distinct "atomic" causes and their separate effects; but when causes occur in nature they occur, not separately, but in combination. Moreover, the combinations are irregular and changing, and even a single omission will usually make a big difference. (P. 175)

But does this general view really touch fundamental force laws? It is hard to see why, given Cartwright's concession of the truth of mechanical laws including the vector law of the composition of forces. Why should we not identify the various "tendencies" truly described by various fundamental force laws with certain obvious "component effects", effects not in themselves always wholly manifest, but effects to which, nevertheless, vector analysis will always adequately draw our attention? In this case the distinction between a *power* and its *exercise* will collapse, and we may legitimately be moved by our supposition of the truth of mechanical laws to maintain the "facticity" view of fundamental force laws. This, at any rate, is the view for which I shall argue below. I think my argument implies that, by embracing "tendencies" in *Capacities*, yet maintaining the view of *Laws* that the laws of pure mechanics are true, Cartwright tacitly accepts the "components scheme" in respect of fundamental force laws despite her overt resistance to this scheme in *Laws*.

In the case of the law of universal gravitation, once the truth of laws of pure mechanics is admitted the "facticity" of this law becomes particularly hard to deny. In this chapter we will study some cogent empirical arguments for the truth of the law of universal gravitation, arguments whose principal presupposition is simply the truth of certain laws of pure mechanics. In chapter 5 we will briefly discuss how an emendation of the space-time principles underlying mechanics effectively assimilates the law of universal gravitation to mechanical laws.

Cartwright says in *Laws* that the law of universal gravitation can readily be modified by a *ceteris paribus* clause, as follows:

> *If* there are no forces other than gravitational forces at work, *then* two bodies exert a force between each other which varies inversely as the square of the distance between them, and varies directly as the product of their masses.

This, she says, renders it "true" but "not very useful" (p. 58). Her claim that the generalization just stated is "not very useful" is preposterous. For there exists a wide range of cases of great interest and historical and evidential significance in which other things *are* equal in the relevant sense, that is, in which to all intents and purposes there are no forces other than gravitational forces at work. What Cartwright says on p. 58 illegitimately sweeps aside our solar system. Yet it was from his profound analysis of the motions in the solar system that Newton delivered his arguments for universal gravitation. These arguments depended only upon mechanical assumptions the truth of which Cartwright does not dispute. The conclusion they support is well expressed by Cartwright's second qualified formulation (p. 60) of the law of universal gravitation,

> Two bodies produce a force between each other (the force due to gravity) which varies inversely as the square of the distance between them, and varies directly as the product of their masses.

and *pace* Cartwright, because mechanical principles make perfect sense of how it can do this, this formulation *does* also perfectly well "satisfy the facticity requirement". That, at least, is what I shall argue in this chapter.

I shall also discuss what I see as a general fault in Cartwright's approach. It is evident that the condition imposed by Newton's solution to Cartwright's dilemma of the double effect, that resultant forces everywhere and always sum to zero (just as they do in statics), is unhelpful from the point of view of understanding "what there is" in the world. Rather it places a formal condition relating *m* times *a* to various other conditions. It is not at all helpful to consider this formal condition in the terms Cartwright's own problems with component forces put before us -- terms of "ontic commitment" rather than analysis of relevant theoretical structures. I shall say more later about the importance of conceiving Newton's own commitments as structural, and against Cartwright's tendency to concern herself overly much with theoretical ontology at the expense of concern about theoretical structure.

Earlier, I remarked that Newton accounted very fully to himself for all the presuppositions of his physical concepts. Newton recognized that his force-conception presupposed a connection between instantaneous spaces across time. Only by implicit reference to such a connection could Newton draw his distinction between inertial and non-inertial motion. Only with a thus adequately grounded conception of non-inertial or accelerated motion does Newton's programme for the determination of forces make sense. Newton's particular conception of the space-time connection was richer than he needed for his purposes, and in some respects quite unlike that retained by physics of the present day. Yet contemporary physics retains a space-time connection, and still makes out in terms of it a fundamental distinction between inertial and non-inertial motion. Physics also retains Newton's second law, that is, it retains the fundamental idea that precisely when one takes the product of the mass and the instantaneous time rate of deviation from inertial motion to be a force-component, force always sums to zero. The question I wish to address is whether these principles apply only to objects in highly idealized models, as Cartwright (given her conception of laws) should insist. To suggest that they do requires that we either presume *fictional* the very space-time fabric upon which physics is based, or presume that in real systems force in Newton's extended sense need not always sum to zero. Either option is hard to accept. Cartwright herself seems no fictionalist about the space-time fabric upon which physics is based.

For one thing, her view of some models seems to be that of an idealized "picture" of a real system. Such models are *of* the systems they model precisely because of shared spatiotemporal structure; the spatiotemporal *location* of salient features of the model is what maps them to facets of reality. If the space-time fabric is fictional, the relation our models bear to real systems making them models *of* those systems appears impossible to make out. Yet if the fabric is not fictional, if in real systems it is determinate which parts are accelerated and by how much, where then is the idealization in Newton's laws? Newton's laws would be idealizations only if summed force sometimes does not equal zero. Cartwright has no grounds for suggesting this. In fact she really does not suggest this, but allows, as I noted in the Introduction, that unlike any other theoretical laws, the basic laws of mechanics may well be true. The caution of "may well be" the next section argues is scarcely warranted. Later sections will show how, granting the laws of mechanics, a compelling case can also be made out for the "facticity" of other laws.

3.2. Newton and rational mechanics. The rough mechanical conceptions introduced early in the seventeenth century by mid-century had already shown themselves to be ever more rationally and empirically improvable, through a kind of investigation that set new standards for the linking of mathematics to physical concepts, and for the linking of physical theory to evidence. From a few general principles of mechanics others were found rationally to follow --

either immediately or when taken together with certain general, idealized facts of experience (idealized facts that the new mechanical conceptions themselves helped bring to people's attention). That is, important general laws repeatedly turned up in logical *deductions* from other general statements -- from high-level theoretical principles already admitted, or these together with *phenomena* of a general and idealized sort. Theoretical principles became more and more logically articulated and tightly linked to an (idealized) phenomenological base. Newton was to discover (but was not the first to discover) this wonderful pregnancy of the new mechanics. Huygens in 1667 was able to deduce the general law of conservation of momentum -- and hence, in effect, Newton's third law -- from the accepted high-level theoretical principles of inertia, of Galilean relativity, and of the impossibility of perpetual motion, conjoined with the "phenomena" that (a) bodies falling in a vacuum from rest from a height h acquire a velocity proportional to h and (b) this velocity will suffice to raise the body along a frictionless upward path in a vacuum exactly back again to the same height h.[6] As we shall see, Newton found that such "deductions from

[6]There is a lucid discussion of this deduction in Howard Stein's "On Locke, 'the great Huygenius, & the incomparable Mr. Newton'", in R.I.G. Hughes and Phillip Barker (eds.), *Philosophical Perspectives on Newtonian Science*, Cambridge, Mass.: MIT Press, 1990. Pp. - . Stein mentions that when Newton set down his own definitions and laws of motion, he did not claim originality for them but characterized them as "receiv'd by Mathematicians" such as Galileo, Christopher Wren, John Wallis, and Huygens. In *De Motu*, discussed below, Newton incorporated Huygens' principle into his thinking about the motions of the solar system, and in so doing arrived at the idea of truly universal gravitation and at his third law of motion.

phenomena" are possible not only within rational mechanics but also in physical astronomy, and was the first to use this method of inference in a reflective and explicit way.

In the *Principia* Newton contributes to rational mechanics. Part of this contribution is in Book I (the relevant results having been achieved mostly as spin-off from his work on physical astronomy). The whole of Book II of the *Principia* (in which Newton's wish to confute the Cartesian theory of vortices largely determines the agenda) also is within the tradition of rational mechanics, but Newton's efforts there in many ways overreach what can be measured by evidence (are not deductions from phenomena). However, Book II is significant in a new attitude it evinces toward idealization. Many of the results of Book II are meant to delimit what deviations from ideal inertial motions may be attributed to the influence of an ambient medium. Newton shows that, in light of what the relevant theorems have to say about this and given the Keplerian kinematics of the solar system, the Cartesian vortical cosmology is completely indefensible. Along the way to this, his principal conclusion, Newton makes various direct calculations (so brilliant in conception and execution, that, at least as an inducement to further rational mechanical work by successors such as the Bernoullis and Euler, followed by Lagrange, Fourier, Poisson, Navier, Cauchy, Green, Stokes, Kelvin, Helmholtz, Kirchoff, Maxwell, and Gibbs, they are not less important even than all the rest of the *Principia*[7]) of effects that Galileo would have simply ascribed to the "impediments of matter" and would never have tried to analyze.

3.3. Newton and physical astronomy. The two other principal facets of Newton's *Principia* concern this work in physical astronomy. (1) The first third of Book III together with the first third of Book I comprise the successful carrying out of the project of first characterizing a general physics of the cosmos, and then appealing to this physics to establish heliocentrism. Newton infers to universal gravitation from facts embraced in Kepler's laws, and then on physical grounds which include gravitation argues for the Copernican system over the Tychonic. (2) In the last two-thirds of Book III, drawing upon the last two-thirds of Book I, Newton proceeds to a singularly ambitious and original extension of this project: to use the law of universal gravitation to explain in detail *all deviations* from Keplerian motion, thus showing (a) that *only* gravitation is at work in the solar system, so (b) there is no reason to consider any other mechanism to be at work (such as the vortical mechanism favoured by Cartesians). I shall discuss each of these two facets of Newton's *Principia* under two headings: historical points and methodological points. In section 3.4 I shall examine some contemporary anti-conventionalist theories of empirical confirma-

[7]See Truesdell, "Reactions of Late Baroque Mechanics to Success, Conjecture, Error, and Failure in Newton's *Principia*", in R. Palter (ed.), *Annus Mirabilis*, op. cit, p. 201.

tion that capture aspects of Newton's methodology. In section 3.5 I shall examine an important subtlety in such knowledge as we possess when we establish a law such as the law of universal gravitation. Finally, in section 3.6 I shall consider some implications of this discussion for Cartwright.

(i) *Newton's inferences in physical astronomy: historical points.* From Kepler's third law (and treating planetary motion as uniform and circular) Newton demonstrated that the relationship that obtains between each planet's (average) centripetal acceleration in its orbit and that planet's (average) distance from the sun is inverse-square. Assuming that an inverse-square relationship also holds between distance and accelerations induced gravitationally by the earth, Newton showed that the moon's *weight* towards the earth exactly accounts for its orbiting the earth. On this basis Newton inferred that each planet's weight towards the sun is what accounts for its orbiting the sun. This much (framed, however, in terms of "centrifugal force" rather than in terms of the maturer notion of centripetal acceleration) Newton had worked out twenty years prior to composing *Principia*. Contrary to a popular view that Newton hit upon the idea of universal gravitation in a flash during his youthful *annus mirabilis* (1666), the idea of universal gravitation did not come to him until near the end of these twenty years. In 1675, for example, Newton set out a hypothetical aetherial explanation of gravity according to which gravitation is clearly not universal: according to this aetherial conception, there is gravity toward the earth, toward the moon, toward the sun, and toward all other major bodies, but there is not gravity of each and every smallest part of matter towards each and every other part. There is no firm evidence that Newton entertained the idea of universal gravitation before 1679.[8]

At the close of the 1670s several events led Newton back (from a decade's immersion in alchemical researches) to the problems of physical astronomy. In 1679/80 an exchange of letters with Hooke led Newton to prove to himself the theorem that became Proposition VI of Book I of the *Principia*. This theorem can be used to show (see Proposition X of Book I) that a body moving in an ellipse experiences an inverse-square acceleration towards one focus of the ellipse. The comet of 1680/81 and an ensuing correspondence with John Flamsteed greatly stirred Newton's interest and activity in physical astronomical work. It was during this period that Newton first became aware of Kepler's

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[8]It was not until 1686 that Newton proved a result crucial for the inference from the moon test to truly universal gravitation. This result assuaged Newton's

> doubt whether the proportion inversely as the square of the distance did accurately hold, or but nearly so, in the total force compounded [from inverse-square forces exerted by all the parts of a material sphere such as the earth]; for it might be that the proportion which actually enough took place in greater distances should be wide of the truth near the surface of the planet, where the distances of the parts are unequal, and their situation dissimilar.

(From the commentary on Proposition VIII of Book III of the *Principia*.) Until this doubt was dissolved, the moon test did not even begin to argue for universal gravitation. The well-known results of Section XII of Book I of the *Principia* prove this reasonable-seeming doubt to be misplaced, provided only (as Newton thought it unquestionable to assume) that the inner constitution of the earth is in fairly high degree spherically symmetric.

second law, the law of areas. That is not to say that Newton immediately accepted the areas rule as law. Nor did he accept as established either Kepler's first or third laws. Through Flamsteed, Newton was brought into contact with a well-motivated skepticism among observational astronomers of his day against taking Kepler's laws to be exactly true. In reaction to this uncertainty, Newton sought some basis in physics for Kepler's so-called laws, in order to judge the likely real robustness of those regularities.[9] Newton managed to prove (as would become Propositions I and II of Book I) that a motion sustaining an arbitrary centrally directed acceleration will obey Kepler's law of areas, and a motion obeying Kepler's law of areas about a point *P* will sustain accelerations always directed toward *P*. He proved, in particular, that when such a force is inverse-square, the path is conic and satisfies the law of areas.

In December 1684 Edmund Halley made his famous first visit to Newton, asked Newton whether he knew what shape of planetary orbit is implied by an inverse-square attraction to the sun, and eventually got the *Principia* in reply. Newton's initial presentation to Halley was a document entitled *De Motu* which underwent three revisions. It was in the course of these revisions that Newton for the first time came to think about the solar system in terms of Huygens' law

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[9]See C. A. Wilson, "From Kepler's Laws, So-called, to Universal Gravitation: Empirical Factors", *Archive for History of Exact Sciences* (1970) *6*: 89-170.

of the inertiality of the "centre of gravity" of a closed system of bodies -- an idea that Newton would later reformulate as the third law of motion -- and so decided that the sun must move to accommodate the planets' motions. To think this way, Newton had had to set aside the prevailing view of physical laws, according to which physical laws must apply fundamentally to impact, to how bodies change their states of motion by impulse. Huygens had never intended his law to be read any other way. But Newton did read it another way, and consequently came to see gravitation as an interaction in its own right.

Newton observed, further, that whatever the type of material, heaviness exactly measures resistivity to acceleration. (Newton confirmed this by experiment, and in formulating the very idea of such an experiment for the first time introduced under the term 'mass' a concept distinct from that of 'weight'.) So gravitational interactions within a system of bodies cannot shift its centre of mass. Thinking this way, Newton was led to formulate his third law, of the equality of action and reaction, and also to introduce for the first time officially his idea of universal gravitation.

Universal gravitation immediately implied that Kepler's laws could not be true.

> By reason of [the] deviation of the Sun from the center of gravity the centripetal force does not always tend to that immobile center, and hence the planets neither move exactly in ellipses nor revolve twice in the same orbit. So that there are as many orbits of a planet as it has revolutions, as in the motion of the Moon, and the orbit of any one planet depends on

> the combined motion of all the planets, not to mention the action of all these on each other. But to consider simultaneously all these causes of motion and to define these motions by exact laws allowing of convenient calculation exceeds, unless I am mistaken, the force of any human mind.[10]

Despite the initial pessimism, within a few months Newton himself was immersed in these supposedly impossible calculations. And as we shall discuss in the historical points of subsection (iii), it was on his successes in this work, not on the more nearly heuristic thinking detailed above, that Newton would rest his case for his law of universal gravitation.

(ii) *Newton's inferences in physical astronomy: methodological points.* To call "heuristic" the thinking detailed in subsection (i) is accurate only because the steps in Newton's reasoning were a part of a *way* of thinking in natural philosophy with not-yet established empirical credentials. Newton's successes in his work on deviations (work with which we shall deal in subsection (iii)) served not only to support the law of universal gravitation but also the whole method or way of thinking that Newton had employed to reach it. The cogency as judged from within this way of thinking of all the various steps Newton took in reaching the law of universal gravitation is certainly commanding. If properly understood, its use of idealization does not promote skepticism. Let us examine the forms

[10]From Version III of *De Motu*; quoted in C. A. Wilson, "Kepler's Laws", op. cit., p. 160.

of argument in more detail.[11] Later we shall examine Newton's empiricism about method itself.

Newton's reasoning is replete with *deductions from phenomena*, in which a high-level theoretical principle is logically deduced from certain other high-level theoretical principles plus a body of empirical facts. Thus by geometrical demonstrations Newton repeatedly shows how (holding fixed, as assumptions, the laws of motion) phenomena can *measure* or *determine* the value of some theoretical parameter, thus establishing the form of some high-level theoretical principle. Newton's geometrical demonstrations are contained in Book I; deductions from phenomena come thick and fast right from the beginning of Book III. By the fact, proved as Proposition II of Book I, that the law of areas implies centripetality, Newton deduces from phenomena embraced within Kepler's second law that the forces drawing the planets, the earth's moon, the satellites of Jupiter, and the satellites of Saturn off from rectilinear motion and retaining them in their orbits, are directed respectively to the sun's centre, to the earth's centre, to Jupiter's centre, and to Saturn's centre. By lines drawn from the earth the planets do *not*, by comparison, sweep out equal areas in equal times, and from this Newton infers that the earth is not the physical hub of the

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[11]William Harper has led me through these forms of argument, and what I say in this section derives from that instruction. See his "Reasoning from Phenomena: Newton's Argument for Universal Gravitation and the Practice of Science" (unpublished manuscript).

planets' motions. Newton has established that (given the laws of motion) if orbiting or secondary bodies' periodic times are as the power *n* of their distance from their primary, the centripetal forces that act on them vary inversely as the power $2n - 1$ of the distance. (See Corollary 7 of Proposition 4 in Book I.) Thus from the phenomena embraced within Kepler's harmonic law (the law that periodic times vary as the 3/2 power of the distance, a law known to hold with high accuracy for the planets in relation to the sun, and for the satellites of Jupiter and Saturn in relation to their primaries), Newton can *deduce* that centripetal forces vary inversely as the square of the distance (at least at the various distances of the orbiting secondaries). More powerfully still, Newton established, again given the laws of motion, that rates of orbital precession sensitively *measure* the exponent relating distance to centripetal force. The near quiescence of the perihelion points of the planets in their elliptical orbits about the sun thus allowed Newton to *deduce* that centripetal forces vary inversely as the square of the planets' distances from the sun (at least over the various distances between the perihelia and aphelia of all the planets). In Newton's day quite accurate determinations of the planetary motions had revealed no precession in their orbits (though inaccuracies in Kepler's predictions for the motions of Mercury were beginning to appear). Because the precessional measure of the rate of variation with distance of centripetal force is *highly* sensitive, Newton's deduction of the inverse-square character of the force law from the phenomena effectively embraced in Kepler's first law is still more powerful than the deduction from the phenomena embraced in Kepler's harmonic law. Newton showed, for example, that a variation only slightly faster than inversely as the square, namely a variation of centripetal force inversely as the $2^{4}/_{243}$ power of the distance, implies a highly detectable precession 3° forward per orbit. This is the rate at which the moon precesses in its orbit: Newton takes the near quiescence of the perigee point of the moon to argue that the earth's influence on the moon varies inversely as the square of its distance. The increment of $^{4}/_{243}$ in the power of this variation Newton thinks is a deviation that is to be attributed primarily to the influence of the sun; Newton will endeavour, later, to show by calculation that the sun's perturbing influence accounts for this increment.

The laws of motion that help underwrite these deductions from phenomena Newton calls "axioms" of his system, fully recognizing their role as background assumptions in all his reasoning. Their basis is, in part, in experiments, but remains as well in a much broader aspect empirical. The generality and successes of the investigations that may proceed from them is, for Newton, the proper measure of their warrant. It is remarkable that for Newton not just his mechanical axioms are empirical. Even geometry is based on principles that are "brought from without", "founded in mechanical practice", and presumably always subject to revision if ever that proves to be a condition for advancing to "some truer method" in physics or natural philosophy. For Newton, all items of presumed knowledge are thus tentative and subject to the demand to give ever improved results. Even the most basic assumptions or highly prized forms of argumentation are subject to revision with the advance of knowledge. From the vantage point provided by his own successes, Newton says that "the whole burden of philosophy seems to consist in this -- from the phenomena of motions to investigate the forces of nature, and then from the forces to demonstrate the other phenomena." But this prescription for philosophy (or, as we would say, physics) with its attendant commitments to geometrical principles, definitions, and axioms of motion, is not and can never be something settled. The methods thus prescribed "seem" from their success so far to be "the best way of arguing which the Nature of Things admits of",[12] but Newton is tentative about this. That "the best way of arguing" depends on "the Nature of Things" means that method itself must be empirically learned. Newton has reasons to recommend his method, but is well aware that further discovery may bring about its improvement or replacement. "I hope," Newton writes, that "the principles here laid down will afford light either to this or some truer method of philosophy".

"In this philosophy," Newton writes in his General Scholium, "particular propositions are inferred from the phenomena, and afterwards rendered general by induction." The deductions from phenomena that I discussed above provide Newton with an assortment of particular propositions about the "accelerative

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[12]From Newton's *Opticks*, Query 28.

attractions" of certain bodies for others. The inductive step that renders these propositions general Newton sees as licensed by his third rule of reasoning:[13]

> *The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever.*

Thus since it

> universally appears, by experiments and astronomical observations, that all bodies about the earth gravitate towards the earth, and that in proportion to the quantity of matter which they severally contain; that the moon likewise, according to the quantity of its matter, gravitates towards the earth; that, on the other hand, our seas gravitate toward the moon; and all the planets one towards another; and the comets in like manner towards the sun; we must, in consequence of this rule, universally allow that all bodies whatsoever are endowed with a principle of mutual gravitation.[14]

The propositions discussed above that Newton deduces from phenomena when thus generalized imply that around every body there is an "acceleration field" due to that body's gravitation that is dependent only on the distance of any body influenced by it and diminishes as the square of the distance. Newton appeals to a further geometrical demonstration (the subject of Proposition LXIX, Book I, and its corollaries) to establish, from this fact, the proportionality of the gravitational force to the product of the masses.

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[13]From Book III of the *Principia*, Cajori's revision of the Motte translation, Berkeley: University of California Press, 1934; p. 398.

[14]*Principia*, op. cit., p. 399.

The law that is thus established unifies many phenomena. As is well known, the phenomena of terrestrial free fall and weight become linked to phenomena of the celestial motions. But also some formally very impressive linkages of a more esoteric sort are made under the law of universal gravitation. For example, the phenomena embraced within Kepler's first law and Kepler's third law have independently determined that the form for the law of universal gravitation be inverse-square. Both sorts of phenomena are, accordingly, explained by this law. They are unified by being linked to one and the same fact -- to the exponent -2 in the form of the force law. The exponent -2 in the form of the force law is a theoretical parameter that has been *overdetermined* by the empirical data. In Whewell's terms, there has been a consilience of inductions in the setting of this theoretical parameter at -2.

Another example of overdetermination relates to the harmonic law alone. By the lights of the law of universal gravitation, a secondary's R^3/T^2 measures the mass of the primary. This value is fixed by any one secondary's orbital motion, and the general conformability with the harmonic law amounts to an overdetermination by evidence of the theoretical parameter that represents the mass of the primary.

Malcolm Forster[15] has argued that a thus overdetermined parameter may be regarded as the common cause of the various phenomena that measure it. This use of the word 'cause' may at first sight seem odd, but I think that it is perfectly appropriate. Certainly the theoretical parameter that is thus overdetermined is an "*inus*" condition in Mackie's sense.[16] That gravitation depends on the power -2 of the distance is clearly an *i*nsufficient but *n*onredundant part of an *u*nnecessary but *s*ufficient condition for aphelia to be quiescent, and likewise for the harmonic law to hold. That gravitation depends on the power -2 of the distance is a standing condition rather than an event, of course, but then we often use the word 'cause' in reference to standing conditions rather than events. William Harper has a nice formulation of the lesson that is to be learned from this: Newton pioneered a mathematical style of causal explanation, in which the effect to be explained can be made to measure the critical parameter for the cause which explains it.[17]

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[15]Malcolm Forster, "Unification, Explanation, and the Composition of Causes in Newtonian Mechanics", *Studies in History and Philosophy of Science* (1988) *19*: 55-101.

[16]An inus condition is an *i*nsufficient but *n*on-redundant part of an *u*nnecessary but *s*ufficient condition. See J. L. Mackie, *The Cement of the Universe: A Study of Causation*, Oxford: Clarendon Press, [1974] 1980. P. 62.

[17]Harper, "Reasoning from Phenomena", op. cit., p. 3, pp. 28-29.

The phenomena from which Newton deduces propositions concerning gravitation clearly are idealized. (Newton's own work on deviations, which we shall examine in the next subsection, serves to underline this fact.) In characterizing this idealization both Forster and Harper employ Whewell's notion of a *colligation* (and refer in particular to that stage in colligation that Whewell called "construction of the conception") and both argue that Newton's inferences are not made less cogent because of the need for this step. Harper takes issue, in particular, with Paul Feyerabend's complaint that these idealizations by Newton were preadapted to the propositions that Newton then inferred from them. Harper charges that Feyerabend fails to take into account how tightly constraining the data were on what could count as a phenomenon colligated from them.[18] The high accuracy of Kepler's laws has already been remarked upon. Moreover, these laws were proposed by *Kepler*, who could not have preadapted them to the propositions that Newton later inferred from them. Curtis Wilson has shown how Newton began his investigations into what could be learned in physics from Kepler's laws precisely with the intention of determining on physical principles whether Kepler's laws are at all robust.[19] Newton knew that there are some deviations from Kepler's laws and was interested to discover whether Kepler's or some other formulae for planetary motion are the truer. Thus Newton had not

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[18]Harper, "Reasoning from Phenomena", op. cit., p. 26.

[19]Wilson, "From Kepler's Laws", op. cit.

prejudiced his study towards Kepler's laws, and rather became convinced of their first-approximation correctness only after he had completed the work on their basis to a unified physics of the cosmos. Finally, Forster argues that the idealization of taking Kepler's laws as true is *shown* to be innocuous by the very principle, the law of universal gravitation, that is inferred from these laws. For the law of universal gravitation delivers the Keplerian motions as a first approximation, and does not abandon the Keplerian motions in later approximations, but retains them as the *fundamental component* of the planetary motions.

(iii) *Calculation of Deviations: Historical Points.* After the comet of 1680/81 Newton, with gravitational cosmology much on his mind, became curious as to whether Jupiter and Saturn when they are in closest proximity with one another depart noticeably from Keplerian motions. In 1684 he asked Flamsteed to tell him whether such departures had been observed. It is known that Flamsteed initially said that the discrepancies had not ever been observed, and there is no record that Flamsteed ever directly supplied Newton with the information that there are these discrepancies. However, observational data then extant did already contain the discrepancies, and Newton was eventually to reveal them once sufficient data came into his own hands. Thus Newton's study of deviations from Keplerian motions for clues about universal gravitation began early. It began before he had adduced strong arguments for universal gravitation from the Keplerian motions themselves. The work on deviations would eventually provide the most cogent evidence for universal gravitation.

Of all the bodies in the solar system, the one whose motions conform least well to Kepler's laws is the moon. By Newton's day these deviations had been amply studied and characterized. Once the law of universal gravitation was in his possession, Newton sought to explain by reference to the perturbing influence of the sun these known deviations in the moon's motions. His initial discussion (in Proposition LXVI, Book I, and its initial corollaries) of this, the paradigm three-body problem, makes qualitative sense of all the known inequalities in the orbital motion of the moon: its speeding up in two quadrants and slowing down in the others, the variation of the eccentricity of its orbit, the "wobble" or variation of the inclination of its orbit, and the regression of the nodes of its orbit. It is a wonder that Newton did not think this analysis accomplishment enough; instead he pressed on in his researches in order to achieve an analysis of the same deviations in quantitative terms. In the long haul Newton would succumb to "headaches" and give this work over to others; the triumph of successfully completing it was left as we shall see to a later generation. But some initial results from his quantitative investigations were triumphs in their own right. Newton came to see that the additional contribution of the sun to the component of the total acceleration of the moon that is directed to the earth's centre is as the numbers -2, +1, -2, +1 as the moon proceeds from syzygies to quadratures to syzygies to quadratures. Newton was struck by the fact that the variations due to the sun of accelerations towards the earth thus had a period half that of the moon's orbital motion. The same influences, brought down to the surface of the earth, could then explain why the "solar tide" has half the period of the earth's rotation. Similar considerations would show why this semi-diurnal period holds also of the "lunar" tides. It was notorious that the semi-diurnal period of the tides had not been satisfactorily explained.[20] On account of the semi-diurnal period of the tides Galileo had reacted derisively to Kepler's suggestion that the moon attracts the seas, thus accounting for the tides.[21] It seems that the quick transition from discussion of perturbations of an orbit to discussion of tides in the corollaries of Proposition LXVI, Book I is a record of Newton's discovering out of the blue that he could explain the semi-diurnal period of the tides, and that he should consequently shift his investigations to developing a comprehensive theory of them.[22] On the basis of preliminary calculations Newton found ready qualitative terms from his gravitational

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[20]The unsuccess, in this regard, of Kepler's and Galileo's theories of the tides is discussed in many places, among them E. J. Aiton "Galileo's Theory of the Tides", *Annals of Science* (1954) *10*: 44-57.

[21]Galileo wrote: "There are many who refer the tides to the moon, saying that this has particular dominion over the waters; ... the moon, wandering through the sky, attracts and draws up towards itself a heap of water which goes along following it, so that the high sea is always in that part which lies under the moon. And since when the moon is below the horizon, this rising nevertheless returns, he tells us that he can say nothing to account for this ...".
(Quoted in James Cushing, "The Oceanic Tides", manuscript chapter 16 of a forthcoming book.)

[22]This interpretation was argued in a stimulating lecture that I attended by George Smith.

theory for predicting how the tides should vary with varying sun-moon configurations about the earth in varying relations to the earth's equatorial plane. These predictions matched many known variations and suggested still further patterns to be looked for. (See the long discussion to Proposition XXIV in Book III.) Newton also attempted a detailed quantitative analysis of the ratio of the solar to the lunar effect in the tides. I shall say more about that in a moment.

Newton's analysis of the moon's apsidal motion was unsuccessful. Newton falsely assumed that the component of the solar influence that is perpendicular to the earth-moon radius has no net effect on the apsidal motion. Analyzing only the component of this influence that is parallel to the earth-moon radius, Newton could show that it lessens the average centripetal acceleration of the moon, as is required to account for the forward precession, but only by so much as to account for a 1½° aspidal motion, half the actual effect. The mistake in the analysis was repeated by the next generation of mathematicians, and in particular by Euler, Clairaut, and d'Alembert, who began to reassess first a variety of special assumptions (that the earth and moon can be treated as spheres, for example) that had played a role in Newton's calculation, and then even the law of universal gravitation itself (to which Clairaut suggested adding a further term). Then Clairaut discovered the error that they had all been making. He showed that the component of the solar influence that is perpendicular to the earth-moon radius does indeed have an overall effect, an effect that brings the predicted apsidal motion to the observed 3° per revolution. This discovery had a major psychological effect in persuading remaining skeptics to embrace the law of universal gravitation.[23] For Newton's own generation, Newton's precise recovery of the known 26,000 year period[24] for the precession of the equinoxes had a major psychological effect. In an influential article, Richard Westfall has argued that Newton "manipulated" his calculation of this result, achieving it by several overlapping "applications of the fudge factor".[25] In particular, Westfall alleges that Newton so determined the ratio L:S of the lunar to the solar tidal effect that the 26,000-year result would drop out precisely. It is true that Newton's value for the mass of the earth (in relation to the mass of the sun) is too large by a factor of two, and that there is a factor of two error in his

[23]For support for the historical claims made this paragraph see Craig B. Waff, *Universal Gravitation and the Motion of the Moon's Apogee: the Establishment and Reception of Newton's Inverse-Square Law, 1687-1749*, Ann Arbor: University Microfilms International, 1976.

[24]It is somewhat tendentious for me to call the precessional period "known". Certainly, it was known that the sun had moved on in the zodiac by a certain amount, close to one sign, since the times when the zodiacal signs were first set up; from which fact, on the assumption that the motion is constant and periodical, a precessional period of 26,000 years could be readily calculated. But it is not clear that people commonly were thinking this way, or that before Newton they would have had any good reason to do so. So to call the 26,000 year precessional *period* "known" is in an important respect not accurate. Nevertheless, Newton's results were in perfect agreement with the known advance by one sign of the sun in the zodiac since the zodiacal signs were established two milennia previously.

[25]R. S. Westfall, "Newton and the Fudge Factor", in *Science* (1970) *179*: 751-758.

reported L:S that exactly compensates for this. That Newton was striving to set L:S appropriately for the precessional calculation has to be argued on better grounds than this, however. Westfall endeavours to give such grounds, attacking all the steps Newton took in moving from observed ratios of neap to spring tides to an estimate for L:S. In an appendix to this dissertation (Appendix 1), I consider these charges in detail, and I defend the integrity of Newton's reasonings. It is noteworthy that Newton's general idea of what, physically, should give rise to the precessional motion (principally the sun's and moon's effects on the equatorial bulge of the spinning earth) is unquestionably correct, and with correct mathematical techniques and correct values for the masses of the earth, sun and moon, the correct value for the precessional period does drop out.

(iv) *Calculation of Deviations: Methodological Points.* Newton's theoretical precepts were won by deductions from idealized phenomena. A key methodological function of calculating on the basis of such theoretical precepts the known deviations from the prior idealizations is to dissolve the worry that the theory is only an artifact of the idealization.[26] For example, one needs to account by reference to the perturbing influence of other bodies for the whole of the 3°

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[26]This point was stressed by Ronald Laymon in his "Newton's Demonstration of Universal Gravitation and Philosophical Theories of Confirmation", in John Earman (ed.), *Testing Scientific Theories*, volume X of the *Minnesota Studies in the Philosophy of Science*, Minneapolis: University of Minnesota Press, 1983. The same point also arises in different ways in Forster, "Unification", op. cit. and Harper, "Reasoning from Phenomena", op. cit.

apsidal motion of the moon in order to warrant the argument from the *near* quiescence of the moon's perigee to the *exact* inverse-square character of the earth's gravitation. Another methodological function of calculation of deviations is very much to enrich the connectedness of various elements of theory with one another and with otherwise separate domains of evidence for them. For example, the sun's perturbing influence will be calculated using a value for the ratio of the sun's mass to the earth's mass that is measured from phenomena embraced within the harmonic law. Those phenomena then become relevant to our understanding of the aspidal motion of the moon, and the apsidal motion of the moon becomes relevant to our understanding of those phenomena. This integrating of diverse elements can be thought of as supporting the theoretical precepts in two ways. It helps warrant through *cross-induction* the idealizations that are the basis for deductions from phenomena to the theoretical precepts; in cross-induction support for the "reality" or "informativeness" of low-level generalizations about data is increased through their unification in a higher-level theory.[27] Also, the warrant of the theoretical precepts themselves may be thought to increase with the increased unification of lower-level generalizations on the data.[28] Finally, the case of celestial mechanics illustrates that calculation

[27]For a discussion of cross-induction see Mary Hesse's "Theory as Analogy".

[28]This is the conception of William Whewell; it is emulated most notably by Michael Friedman.

of deviations from first-approximation solutions leads to *detailed new predictions* of subtle, perhaps unobserved though observable secondary phenomena. *The phenomena* for which the theory must account are enriched as scientists' attention is directed to ever finer details in the deviations from first-approximation solutions. The deviations become the *principal* phenomena, and the theory's ability to account for them the chief preoccupation of working scientists.

3.4. Empiricist challenges to underdetermination. Many contemporary philosophers of science who will happily own the label "empiricist" embrace one or more of these aspects of Newton's methods and consequently dismiss the ostensible problems of underdetermination. They are opposed to the view that theory is an artifact of our idealizations of phenomena. They think that properly formulated *empiricist* demands on theory are very much stronger than empiricist philosophers of science have traditionally supposed. Just as Newton opposed hypotheses, emphasized deductions from phenomena, and sought to show how the results of induction could be protected against being undercut by hypotheses, so these contemporary philosophers of science oppose the method of conjecture, hypothetico-deductivism as an account of empirical check on theory, and holism and underdetermination about the relation of theory and evidence.

Clark Glymour[29], for example, thinks that the pattern of inference Newton followed involving *over*determination by evidence of the various elements of a

[29]In *Theory and Evidence*, Princeton: Princeton University Press, 1980.

theory can also be discerned very widely in the sciences. Glymour develops in opposition to hypothetico-deductivism a theory of empirical confirmation that specifically highlights and makes a virtue of what he sees as the relevant pattern -- a pattern that Glymour calls *bootstrap testing*. His theory of empirical confirmation has a novel consequence: by its lights, two scientific theories that are "empirically equivalent" in the hypothetico-deductivist sense of having identical observational consequences, can nevertheless be *unequally* confirmed by *one and the same* body of empirical evidence. By being more "bootstrap" testable, one theory may engage more richly than the other theory the shared empirical evidence, and so carry away with it greater empirical confirmation. Glymour discusses Newton's deductions from phenomena as an example of the bootstrap relationship. In fact Newton's deductions from phenomena establish somewhat more than Glymour demands from bootstrapping,[30] and they are not the only aspect of Newton's method that makes for richness in the engagement of the evidence by the theory.[31]

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[30]Ronald Laymon ("Newton's Demonstration", op. cit., p. 180) points out that "[o]ne important difference between Newton and Glymour on confirmation is Newton's insistence that hypotheses are deduced, and Glymour's insistence that only instances are deduced". See Harper, "Reasoning from Phenomena", op. cit., for a much fuller discussion of this difference.

[31]Ronald Laymon, "Newton's Demonstration", op. cit., was the first to argue that Glymour's account provides only very partial insight into what is methodologically significant in Newton's work. Laymon has emphasized the work on deviations as a further component that is significant in its own right, supporting the idealizations that Newton used in making his deductions from phenomena.

Some methodological rules that seem to do work in science accord evidential weight to extra-empirical factors -- rules such as "prefer the simplest theory" or "prefer the theory that gives best explanations for phenomena" or "prefer the theory that most unifies phenomena". Opponents of the underdetermination thesis often appeal to rules such as these. Glymour, however, insists that *this* way of countering the underdetermination thesis is lame. There can be no intrinsic justification for rules according evidential weight to extra-empirical factors. Glymour asserts, instead, that the theory that most richly engages the empirical evidence in a "bootstrapping" way, will typically also be judged simpler, better explaining, and better unifying of phenomena. It is, Glymour argues, only as an *epiphenomenon* of science's quest for the empirically best confirmed, that is, for the best "bootstrap-confirmed", theory, that the above-mentioned rules come to be followed more or less well in scientific practice. Clearly, as a factual thesis about how scientists work, this is not right; scientists often consciously follow these rules. However, as a normative thesis about what *are* correct reasons for preferring one theory to another, it is a defensible view. The idea of "extra-empirical evidence" is, after all, thoroughly enigmatic. To many the skepticism consequent upon embracing hypothetico-deductivism and holism is preferable to embracing the idea of "extra-empirical evidence". It is therefore significant that Glymour has shown us one way in which we might altogether avoid this enigma, and still account within a theory of scientific method for the widespread preference in science for simpler, better explaining, unified theories. More to the point for my purposes, this way may show (1) how to be an empiricist and still beat the underdetermination problem, and so avoid the view that theory is an *artifice* of convention-building or idealization of phenomena; and (2) that, contrary to Cartwright's view, theoretical laws can be among the very *best* confirmed elements of the corpus of presumed scientific knowledge.

One question that Glymour's critics have put to him repeatedly, concerns the warrant for Glymour's novel theory of empirical confirmation. Why should we decide to mean by "empirically well-confirmed" what Glymour has chosen to mean by this expression? I suggest, with Glymour, that should we decide to mean by "empirically well-confirmed" what Glymour has chosen to mean by this expression, our confidence that this is the right decision must itself derive from experience: from evidence that, as it happens, bootstrap testing is noteworthy in key episodes in the advance of science and in daily episodes of more routine scientific investigations. This, certainly, is Glymour's conclusion: he insists that his theory of empirical confirmation picks up support in the broadt -vay possible from the actual practice of scientists. According to Glymour it is *precisely* the intent of scientists in their daily work to defeat the Duhemian problem of underdetermination, that is, to bring about in the pattern of their theoretical thinking and experimental investigation some way of focusing experience on one element of theory at a time. Moreover, Glymour insists, to the extent that this can be accomplished, the pattern in question almost invariably is "bootstrapping".

This latter contention of Glymour's I think is not altogether right. Roger Rosenkrantz[32] has made it seem possible that, very much contrary to Glymour's own view, "bootstrap" reasoning can itself be resolved as fundamentally Bayesian: whether or not this particular reductionist thesis is correct, perhaps *some* reduction is possible of most or all "bootstrap" reasoning to simpler principles;[33] and whether or not any such reduction of "bootstrap" to other forms of reasoning is possible, perhaps we should expect that in the variety and diversity of actual scientific practice there are important other inference patterns. Newton's "deductions from phenomena" do seem to be one pattern that, contrary to Glymour's explicit treatment of it as bootstrapping, needs to be distinguished from bootstrapping in some ways.[34] Moreover the demand that a theory explain in finer and finer detail the deviations in actual systems from first-approximation solutions to it introduces, as I have suggested, a dimension of "richness" in the

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[32]"Why Glymour is a Bayesian", in J. Earman (ed.), *Testing Scientific Theories*, op. cit.

[33]William Harper is attempting such a reduction using Brian Skyrms's notion of "resiliency" and the non-Bayesian idea that certain belief-elements that constitute the "acceptance context" are (for the nonce) to be accorded the probability *one*.

[34]See note 25.

connection between theory and evidence that Glymour's account does not touch. It is possible that accounts such as Michael Friedman's[35] that, unlike Glymour's, discuss unification as directly rather than epiphenomenally important for empirical confirmation, can make better sense, still in logical terms, of this further dimension. It only seems that Friedman accords evidential weight to extra-empirical considerations concerning theoretical systematicity or unification: in fact, Friedman is faithful to empiricism, and shows in terms well able to illuminate the Newton material above how a better unified theory more richly engages the empirical evidence and thereby carries away increased support from that evidence. Friedman simply doubts the generality or ubiquity of "bootstrapping", and draws our attention to the importance of cross-induction and consilience that may arise independently of the pursuit of "bootstrap" confirmation.

Imre Lakatos[36] has another response, very different from those I have been discussing, to the supposed problem of Duhemian underdetermination. In relation to *logical* or synchronic relations between evidence and theory, Lakatos *accepts* Duhemian holism. When researchers are faced with a falsification of

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[35]See pp. 236-250, *Foundations of Space-Time Theories: Relativistic Physics and the Philosophy of Science*, Princeton: Princeton University Press, 1983.

[36]In "Falsification and the Methodology of Scientific Research Programmes", in I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge*, Cambridge: Cambridge University Press, 1970. Pp. 91-196.

their empirical expectations, Lakatos thinks that their logical freedom is always very great to "direct the arrow of modus tollens" not to one but another element of their corpus of presumed knowledge. In this, Lakatos is in agreement with Duhem and in complete disagreement with Glymour. However, Lakatos moves from this synchronic picture that is Duhemian, to some historical or diachronic considerations about science that argue against Duhemian underdetermination. In the synchronic picture the key element is a theory; in the diachronic picture, it is a research programme, or series of theories. While nothing in *logic* dictates that a recalcitrant experience will be accommodated in one way rather than another, or in other words, that a theory *must* be changed in one particular way, there are *heuristic* constraints within a research programme on how in the face of recalcitrant experience theory is to be changed and thus on how one theory comes to be replaced by the next. A *negative heuristic* exists as the defining condition of any research programme, Lakatos believes: it demands that certain *core* elements of theory are maintained from one theory to the next; it demarcates one research programme from another. The *positive heuristic* is roughly the set of strengths of a research programme for adapting *progressively* whenever it is faced with empirical anomalies. An adaptation is progressive if it does not merely remove an anomaly, but does so in a theoretically "natural" way, in a way that makes novel predictions, and in a way the novel predictions from which are empirically borne out. An adaptation is "ad hoc" or regressive if any of these thre , conditions are not met ("ad hoc_3", "ad hoc_1", or "ad hoc_2", respectively, in terms introduced by Elie Zahar[37]). These considerations form the basis for *comparison* of research programmes. A research programme that is progressing generates anomalies for a research programme against which it is competing. The second research programme may, however, handle these anomalies in a progressive way, and thereby generate anomalies for the first research programme. Over time, the responses of one research programme to anomalies may become very ad hoc: it degenerates, while its competitor continues to progress. The key considerations here are diachronic: how *over time* is a programme responding to experience? According to Lakatos, these diachronic considerations are of paramount importance in science. This, I think, greatly exaggerates their overall importance; but supposing, in any case, tnat diachronic considerations do play some role in science, we again are pointed to a way past Duhemian underdetermination. Duhem conceived the links between evidence and theory to be strictly *logical*. His underdetermination thesis arises out of this conception. Lakatos adds *historical* considerations. These considerations do seem immediately germane to the Newtonian case. The work of calculating deviations was not completed in a day. Even in the specific domain of celestial mechanics Newton set in place not a general answer to all questions that might

[37]In his "Why did Einstein's Programme supersede Lorentz's?", in *British Journal for the Philosophy of Science* (1973) *24*: 87-123, 223-261. See pp. 91-104.

be asked but a programme for working out answers to such questions. The way his theory proved its mettle was in part through its success *over time* in displaying that it could again and again dissolve apparent anomolies and through its successes could continue to make Cartesianism flounder in an ever-expanding sea of ad-hoc vortical explanations of effects discovered and rigorously explained by Newtonianism. With Lakatos, we can say that one mark of Newton's success was the great *heuristic strength* of the programme for physical investigations that he set in place.

I have mentioned Newton's own empiricism about method itself. Newton intended his example to establish a method for physics until some truer way in natural philosophy could be discovered. It is remarkable that the several empiricist conceptions of method that I have discussed in this section were developed by philosophers interested in the general sweep of modern science, but each can lay some claim to illuminating Newton's work, to being a part of his method. In the next two chapters I extend the historical discussion of these methods to Maxwell and Einstein. From this historical work we see that following these methods has conduced in science to broad, detailed empirical success, and to the deduction from phenomena of high-level theoretical laws. That following these methods has conduced in science to broad, detailed empirical success is what warrants us in following these methods, that is, in evaluating scientific theories in their light. But such an empirical argument for method makes little sense given Cartwright's general skepticism about theoretical principles. For in that case methodological principles are theoretical (indeed, they are at the highest level of theory). So by Cartwright's lights we should not see them as at all well confirmed by the evidence. This is a problem for Cartwright because she supplies no alternative account of the warrant for method itself. Moreover, she adopts the bootstrap methods we have discussed here with apparently no thought concerning the question of their warrant.

3.5. Newton's empiricism and the character of physical law. On Newton's conception, no a priori demands may be placed on the character of physical laws. We may not demand, for example, as did almost all Newton's contemporaries, that a really *physical* law must comport with the conception of matter as gearing around action-by-contact. Our only source of knowledge of the physical world is experience; we must even learn through experience the right methods for learning through experience about the physical world. In the face of this empiricism, the physical character of physical laws can be explicated in one way only: by the law's rich engagement of experience. Kepler took a small but significant step in the direction of this strongly empiricist conception of physical law, when at the last his programme for making mechanical sense of the celestial motions failed him. For at that point, as we saw in Chapter 2, Kepler effectively transformed a causal question (why the planets more in the way they do) into a question concerning the formal simplicity, or systematicity, or harmony of the set of empirically-based ideas that says: the planets move in such-and-such a way and not otherwise. Kepler effectively used an argument from simplicity to confer the status of *physical* law upon his empirical generalizations. Newton's criterion (involving as it does the demand for a far richer engagement of empirical evidence by the law) is much more stringent, but is similarly free of a priori constraint.[38]

3.6. A subtlety in the law of universal gravitation. Notwithstanding its extraordinarily rich engagement of evidence, and thus the firm basis for its status as physical law, there is a gulf between the law of universal gravitation and the further conceptions in terms of which we explain to ourselves, as it were, how this law operates. It will help our discussion in the next chapter of Maxwell's method of physical analogy to remark here about this subtlety.

According to Newton's own physical conceptions, the law of universal gravitation concerns an action-at-a-distance force. This directly reflects Newton's

[38]It is well-known that Newton wrote to Bentley of the *inconceivability* that matter should affect matter without mutual contact. Was this, as it certainly appears, a flat contradiction of his avowed empiricism? Howard Stein, in his "On the Notion of Field in Newton, Maxwell, and Beyond", in *Historical and Philosophical Perspectives of Science, Minnesota Studies in the Philosophy of Science*, vol. V, Minneapolis: University of Minnesota Press, 1970, pp. 264-287, defends Newton on this point. Stein first further illustrates Newton's empiricism in his dismissal in *De Gravitatione*, op. cit., of the notion of substance, and then makes use of the relevant discussion in *De Grav* of the nature of bodies to explain both Newton's hesitancy to accept innate gravity, and his using misleading terms to express this hesitancy.

formulation of the fundamental laws of mechanics in terms of forces, and ultimately reflects his conceiving what we call *momentum* to be the fundamental measure of a body's motion. The phenomena that Newton used in his demonstration of the law of universal gravitation do not strictly demand the conception of gravitation as an action-at-a-distance force. Another possible conception is that of a local potential field,[39] and still another, that of a general demand on material systems to satisfy a certain minimum principle. If the fundamental measure of motion in mechanics becomes energy or action rather than momentum, the same phenomena that Newton used in his demonstration of an action-at-a-distance force license deductions to these alternative conceptions of universal gravitation. These alternative formulations for the law of universal gravitation and for the mechanics in which it is embedded were developed to fine sophistication by the generation of mathematicians immediately following Newton.

By adopting in turn the three different formulations of "Newtonian" physics we make sense to ourselves of Newton's laws in quite different ways. We

[39]Newton had a fully developed conception of a field of force. Howard Stein establishes this in his "On the Notion of Field in Newton, Maxwell, and Beyond", op. cit. Stein points out (p. 272) that the notion of field is indispensable in Newton's induction to universal gravitation, for it is on its basis that the relationships in several systems between distances and centripetal accelerations, including the moon's acceleration toward the earth and the acceleration at the earth's surface of freely falling bodies, are united as a single body of evidence (p. 268). However, Newton was hostile to the inchoate energy concept of his day and was very far from possessing the notion of a *potential* field.

should be led by Newton's own formulation of mechanics to insist on the reality of momentum and of forces and on the fundamentality of an efficient-cause form of understanding in physics. We should be led by the energistic formulation of mechanics to insist on the reality of kinetic energy and of a potential field and on the fundamentality of a formal-cause form of understanding in physics. We should be led by the variational formulation of mechanics to insist on the reality of a system's action over time and on the fundamentality of a final-cause form of understanding in physics. On the momentum formulation we see a world animated by the interlocking struggle of *forces* of various sorts including the inertial force *ma* that will always balance otherwise unbalanced forces; and we say that it is because of the forces (be*cause* in the sense of *efficient* "cause") that the momenta of material things changes with time and material systems consequently evolve as they do. On the energy formulation we see a conservative world pervaded by a potential *field* (determined, in the case of gravity, by the distribution of matter) such that the time rate of change of the kinetic energy of any material thing depends in a precise way (a way that is dictated by energy conservation) on the character of the potential field where it is; it is because of this (be*cause* in the sense of *formal* "cause") that material systems evolve as they do. On the action formulation we see a lazy world always minimizing a certain measure of its activity; it is because of this (be*cause* in the sense of *final* cause) that material systems evolve as they do.

In the pure case of Newtonian gravitational mechanics, these different conceptions are, from a formal standpoint, *equivalent*,[40] and whether or not one actually appreciates this fact, the commitment to momentum and its principles amounts also to a commitment to energy and its principles or action and its principles.[41] It is nevertheless difficult to see how, even in this case, the meaning of the three formulations exactly coincides. The differences between the formulations is underscored by the fact that the formulations of Newtonian gravitational mechanics are *in*equivalent from the standpoint of *generalization* of the underlying physical principles.[42] It is well known, for example, that Newtonian gravitational mechanics is a limiting case of the dynamics of general relativity. But the dynamics of general relativity admits no action-at-a-distance formulation. Moreover (and more to the point for our purposes) the three formulations, considered separately, give quite different answers to philosophers' questions concerning what, according to physics, there fundamentally is, or concerning what most-universal forms of understanding are presupposed in physics. A position concerning what there fundamentally is and concerning what most-universal

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[40]The sense in which they are so is subtler than some authors have imagined. See Mark Wilson, "The Double Standard in Ontology", *Philosophical Studies* (1981) *39*: 409-427.

[41]Ibid.

[42]Richard Feynman makes this point in his *The Character of Physical Law*, Cambridge, MA: The MIT Press, 1965. See pp. 53-55.

forms of understanding are presupposed in physics I shall say underwrites some "categorical framework" or other for fathoming the physical world.[43] The lesson we learn from studying the alternative formulations is that the real character of Newtonian physics is more exquisite than that of any one such "framework" that we can fathom. Because there is this exquisiteness or difficulty of categorization within our physical theories, we are faced with a considerable gulf between the physical laws we may actually establish and the physical conceptions in terms of which we reckon to ourselves the significance of these laws.[44]

We should therefore regard an established law such as the law of universal gravitation as lying on a higher plane than the particular physical conceptions that we may choose in order to explain to ourselves what the law means. The law of universal gravitation is thus a *formal* constraint on our physical thinking, imposed by the manifold experience that so richly supports the law. When we choose one or another set of physical conceptions, that is, one or another way of

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[43]The combined Aristotelian and Kantian idea of a categorical framework that I use here is that developed by Stefan Körner in his *Categorical Frameworks*, Oxford: Blackwell, 1970.

[44]Roger Jones has expressed the difficulty as a problem for realism. After discussing the treatment given alternative formulations of Newtonian celestial mechanics in an undergraduate physics programme, Jones writes (in "Realism about What?", forthcoming in *Philosophy of Science*):

> Even if a young physicist is a non-critical realist, he or she will have trouble when asked to articulate the fundamental (theoretical) furniture of the Newtonian universe. He or she doesn't know, in some canonical sense, what to be a realist about.

understanding the laws of motion, the law of universal gravitation then makes for us a definite claim about the thus-conceived physical world. Because this formal constraint is imposed by experience it is flexible within tolerances dictated by the limits and accuracy of the experience that supports it. Emendations are possible that may *generalize* the law to formerly inadequately explored ranges of phenomena. Such emendations may require that some formerly usable basic physical conceptions be cast aside, or even that the laws of motion themselves be generalized (such as the relaxation from the "strong" to the "weak" version of Newton's third law in the theory of electromagnetism). But we nevertheless may look upon the formal constraint as "accurately or very nearly true" notwithstanding the variety of metaphysical conceptions at our disposal for explaining the law to ourselves, until such time as the constraint itself is improved upon by generalization.[45]

3.7. **Against Duhemian Skepticism.** Newton's example suggests that Cartwright is correct in her general view that idealization has an important role in physics. Nevertheless she exaggerates when she writes that "physics ... is prone to be driven more by the needs of mathematics than it is by the phenomena" (*Capacities*, p. 180). In thus emphasising the "needs of mathematics" as a determinant of the form of physical theory, Cartwright resembles Pierre Duhem (more closely

[45]Cf. Newton's fourth Rule of Reasoning in Philosophy, *Principia*, op. cit., p. 400.

than she is willing to admit[46]). Duhem insists that nature is too complex for *actual* phenomena to be of any direct use in physics. True, precise, quantitative descriptions of actual phenomena Duhem calls "practical facts"; he reckons that these are beyond the ken of theoretical physics. Rather, physical theory concerns itself with idealized "theoretical facts", that is, phenomena so idealized that they lie outside the limits of observational error from "practical facts". The virtue of "theoretical facts" is that they can be embraced neatly within the theory's mathematical formalism.[47] Against this view, we have seen that Newton was indeed able to subsume the "practical facts" of observational astronomy under his theory. Moreover, Newton's theory met the "needs of mathematics" and yet was "driven by the phenomena" very directly; it was "driven" by *deductions* from the phenomena. (In these deductions, there is of course a theoretical background; the laws of motion are assumed. It is significant for us that Cartwright implicitly grants us the truth of the laws of motion.) The phenomena

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[46]Cartwright takes care to distinguish her own view, according to which the "abstractness" of high-level theory results from the demands for simplicity of representation of an in fact complex and intricate world, from Duhem's, according to which the "abstractness" of high-level theory results from holism, that is, from the fact that theories are an interconnected web of theoretical laws and rules of correction, and can only be applied to reality as wholes. See *Capacities*, chapter 5, especially pp. 192-194. That idealization is essential to the success of theoretical physics is a part of both views, and here and elsewhere in this dissertation I believe my historical examples cut across both views.

[47]See pp. 132-138 of *The Aim and Structure of Physical Theory*, P. P. Wiener (trans.), Princeton: Princeton University Press, 1954.

from which Newton made his deductions are idealized phenomena, as we have discussed; but we discussed an argument that these phenomena are nevertheless *true*, for they describe real *fundamental components* of the actual motions. Even if we balk, as does Cartwright, at the idea that any *component* motion is "real", Newton's calculation of deviations from the first-approximation or fundamental-component motion still shows the idealizations to be innocuous. For his success in calculating deviations shows that the inference made possible by those idealizations leads to a physical theory that does what Cartwright and Duhem insist physical theories cannot do. The theory can be used to calculate deviations ever more precisely, and ultimately to subsume the complicated "practical facts" under theory after all. It thus takes *unidealized* facts as its basis. In section 3.4 we discussed how these facts in a variety of ways *over*determine the form of the physical theory that explains them. The law of universal gravitation is thus no artifact of idealizing the empirical "facts" for the sake of tidy mathematics. Nor is the law of universal gravitation aloof from empirical check. In particular, it is not hopelessly interconnected with other theory and thus underdetermined by evidence: Newton successfully used his genius in a number of ways to *measure* key parameters within his theory by deductions from phenomena, and to *defeat* underdetermination in this and other ways. Thus neither the Duhemian argument concerning idealization (to "theoretical facts"), nor the Duhemian argument concerning holism and underdetermination, may legitimately be used to advance skepticism concerning Newton's theory.

3.8. Implications for Cartwright. Carwright insists that laws truly describe only the behaviour of objects in highly idealized models. The physical system that Newton studied, the solar system, was, however, no idealized model: the solar system is as concrete as you please. In it, the forces at work are to all intents and purposes purely gravitational forces. Newton showed this, as part of his singularly cogent empirical argument for his theory of universal gravitation. The role of idealization in Newton's work was thus not what Cartwright needs it to have been, in order for her to make out her claim that Newton's law is "abstract", essentially dependent on idealization, and false. Newton set new standards for the empirical adequacy of physical theory, standards which advanced beyond those profferred (perhaps only with a view to practice) by Galileo, and endorsed (as a fundamental matter of principle, indeed of metaphysics) by Cartwright. According to the lower standards, the standards that Cartwright endorses but Newton set aside, idealization is inevitable in all high-level physical theorizing and can never be dissolved or discharged. According to Newton, by contrast, though theoretical physics indeed starts from idealized phenomena (it deduces from these a general force law that explains in first approximation the idealized phenomena that have been used to "measure" its key parameters and thus determine its form), afterwards physics wholly discharges its initial dependence on idealization. The general force law is used to calculate exact, quantitative descriptions of the deviations from first-approximation solutions. This reveals how the idealized phenomena are proper component effects of the underlying force law, and thus justifies the use of the idealized phenomena in the initial deduction of the form for the underlying force law. So if Newton is right, Cartwright is wrong -- wrong in her antipathy to component effects, wrong in her skepticism concerning the law of gravitation, and wrong in her conception of the method of high-level theoretical physics. We proceed in the next two chapters to appraise further whether and how far Newton was right.

Chapter 4

How Maxwell, by Applying Newtonian Methods, Severely Challenged Newtonian Physics

If Cartwright is to be believed, James Clerk Maxwell's methods of investigation in physics were very much those that Cartwright outlines in *Laws*. Maxwell constructed models (his famed "mechanical analogies") that interpose a fiction between theory and phenomena, just as models in Cartwright's simulacrum account of theoretical explanation interpose a fiction between theory and phenomena. That these models idealize phenomena serves to simplify the theory that describes them. But the models' disparities with reality in some ways compensate one another, thus conducing to the theory's being roughly adequate empirically. For example

> Maxwell saw statistical mechanics ... [as] a significant misrepresentation of nature, ... [but] nevertheless the best device to couple to the equally misrepresenting atomistic, or non-continuous, mechanics, in order to save the phenomena.[1]

On Cartwright's view, Maxwell judged that theoretical laws are not true to nature since they accurately describe only the operations of the multiply misrepresentational models. Maxwell thought, like Cartwright, that the "needs of mathematics" are more important than the phenomena in conditioning the form

[1]*Capacities*, p. 196.

of theory. "Maxwell ... despaired that any mathematics accessible to the human mind could represent nature even approximately as it is."[2]

In all these conceptions Cartwright is much mistaken. Maxwell was a Newtonian, and believed in deductions from phenomena. Thus he believed that it is possible to forge a direct link between mathematics and phenomena, precisely as Newton had demonstrated. "[T]he aim of exact science is to reduce the problems of nature to the determination of quantities," he writes,[3] and the gist of such work is

> to deduce from the observed phenomena just as much information about the conditions and connections of [a] material system as these phenomena can legitimately furnish. When examples of this method ... have been properly set forth and explained, we shall hear fewer complaints of the looseness of the reasoning of ... science, and the method of inductive philosophy will no longer be derided as mere guess-work.[4]

Maxwell often uses Newton's expression "deductions from phenomena", or near equivalents. Like Newton, he was enormously careful to distinguish elements of theory that have been established in this way from the bulk of other scientific thinking. Like Newton, the last thing Maxwell wished was to be "carried beyond

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[2]*Capacities*, p. 195.

[3]"On Faraday's Lines of Force", in W. D. Niven (ed.), *The Scientific Papers of James Clerk Maxwell*, vol. 1, New York: Dover, 1965. Pp. 156-7.

[4]"On the Dynamical Evidence for the Equations of Motion of a Connected System", in Niven (ed.), *Maxwell's Papers*, op. cit., vol. II. P. 420.

the truth by a favourite hypothesis".[5] Maxwell realized that deductions from phenomena are possible only against the background of certain high-level theoretical principles. Whether such high-level background principles are objectively true was a question that greatly interested Maxwell. In his view this question principally concerns whether certain basic "necessary conditions of human thought" faithfully mirror conditions in reality. I shall come back to this point. For the moment, grant the truth of the high-level principles. In that case Maxwell does not doubt the truth of other laws that may be deduced from phenomena in light of the high-level principles. Maxwell merely acknowledges a gulf between the laws that may thus be established and "physical conceptions". The subtlety in the law of universal gravitation that we examined last chapter illustrates Maxwell's worry. Maxwell recognizes that the knowledge we possess when we establish a law in mathematical physics is too subtle to be grasped without an overlay of additional meaning that is strictly foreign to it, and which can be varied without this making an empirical difference (just as one can move from a least-action to a potential-field to an action-at-a-distance conception of the gravitational force law, and, in a classical setting, recover all the same facts). However unavoidable may be this extra-empirical element in our understanding of a law in mathematical physics, it is also something that may carry us into error. Thus Newton's own instantaneous-action-at-a-distance conception of

[5]"On Faraday's Lines of Force", op. cit. P. 156.

gravitation was erroneous, as the advent of special relativity theory, discussed next chapter, helped to show. In this chapter we will see how Maxwell emphasizes "analogies" and the fictional situations of a model precisely because he recognizes the subtlety of laws. He recognizes that "analogies" can be varied, compatibly with known phenomena, and thus have significance beyond what is underwritten by phenomena. But he saw the laws themselves as involving only such subtle, common structure as is shared by all the "analogies" which serve us by helping us grasp the law. Properly understood, Maxwell's conception of the laws themselves is realist. As with Newton, who recommended his mechanics and his method only tentatively, until some truer way in physics could be found, there is a caveat to Maxwell's realism. Maxwell's caveat concerns the general conformity of mind to nature; we shall examine this later.

Maxwell's contributions to physics were extraordinarily wide-ranging. Within the limits of this chapter I shall consider some historical and methodological points concerning two major components of his work, that (for which he is best known) leading to a new theory of electromagnetism, and that which laid out a kinetic theory of gases that was rigorous and as complete and empirically adequate as was possible within the confines of classical mechanics. In both spheres Maxwell employed Newtonian methods, emphasizing deductions from phenomena. In both spheres the rigour of his work is underlined by the following fact: Newtonian physics itself was severely challenged by the results he obtained.

By Maxwell's day there was much evidence to suggest that electrostatics and magnetostatics are aspects of a unitary deeper structure. Using his "method of physical analogy", Maxwell succeeded in formally delineating that unitary structure. The character of his success was broadly the same as the character of Newton's success: its symptoms were unification, cross-induction, consilience, anticipation of novel facts, heuristic strength. But we shall see that in respect of all these symptoms of success, Maxwell's theory merely approached (but did not match) Newton's. Maxwell's method, unlike Newton's, made free use of "hypotheses" (speculative features of "analogies") in the development of the successful theory. Formulae were imbued with physical meaning far surpassing what experience could license. The whole theoretical edifice was *elaborately* constructed. Maxwell, however, himself subtracted all these artificial constructions from his own estimation of what he had established. When Hertz later made his famous remark that "The Maxwell theory is the system of Maxwell's equations", he was stating what follows from Maxwell's own description of his method. It is true that Maxwell conceived his mechanical analogies as a step toward a physical characterization of the underlying material medium within which electromagnetic processes are supposed to take place, and he held out the hope that in the future physics would improve that characterization both substantively and evidentially. Hertz's remark also implied, *pace* Maxwell -- and, as things turned out, correctly -- that no potential progress lay in that direction. But this corrected not Maxwell's methodological stance, but an incidental substantive guess about the future development of his theory. What, from Maxwell's methodological standpoint, was fundamental in Maxwell's theory of electromagnetism was not a mechanical ether but rather a set of formal relationships whose critical parameters had been measured from phenomena.

In fact, properly understood, these formal relationships were incompatible with Newtonian mechanics and thus with the very idea of a mechanical ether. Maxwell did not recognize this. However, he did clearly recognize the hypothetical character of his general working assumption that the ether is a material, mechanical system. He was able to proceed on the basis of this assumption to the correct formulation of the equations for the electromagnetic field, only because he used a formulation of Lagrangian analytical mechanics that strictly speaking contradicts Newtonian mechanics. That Maxwellian electrodynamics contradicts Newtonian mechanics fostered a variety of attempts at theoretical adjustment and reconciliation. Some workers held to the hypothesis of a mechanical ether, seeking reconciliation through readjustment of Maxwell's equations and a mechanical explanation for their (presumably partial) fit with the facts. Others assumed the truth of Maxwellian electrodynamics, and sought a correction of mechanics through a (presumably partial) reduction of it to electrodynamics. We will briefly discuss this work in Chapter 5, and we will discuss Einstein's eventual success in finding a third way: Einstein reconciled mechanics and electrodynamics by modifying the kinematics of classical mechanics. In the present chapter we merely outline how, by cogent application of Newton's methods, Maxwell inferred laws that then made trouble for the Newtonian mechanical principles from which his investigations made their start.

In his work on kinetic theory Maxwell was also successful (but again less successful than Newton had been in the case of gravitational celestial mechanics) at achieving non-speculative foundations. His method was definitely not hypothetico-deductive, but remained nevertheless in one respect "speculative" at a fundamental level. The speculation was the extremely general one that the phenomena of gases "depend on the configuration and motion of a material system", that is, on conformity of a system of particles to Newtonian analytical mechanics. All conclusions of greater specificity than this concerning the mechanisms underlying the behaviour of gases were to be deduced from phenomena. Maxwell's method led directly to the problems concerning specific heats that would lead to the overthrow of Newtonian physics. Thus one of its chief successes was to focus empirical criticism on the mechanical presuppositions from which it made its start. But that was a development that Maxwell only started. *Pace* Cartwright, Maxwell's conviction in the truth of his dynamical theory of gases was by all evidence *complete*.

4.1. Maxwell on analogy and established fact. By Maxwell's day action-at-a-distance force laws were received knowledge in physics. In his earliest electromagnetics paper, "On Faraday's Lines of Force", Maxwell wrote

> There is no formula in applied mathematics more consistent with nature than the formula of attractions, and no theory better established ... than that of the action of bodies on one another at a distance.[6]

Yet the "*formula* of attractions" was by no means the same as its physical content. Maxwell points out that there is an analogy (discovered by Thomson) between heat flow and attraction such that

> the mathematical laws of the uniform motion of heat in homogeneous media are identical in form with those of attractions varying inversely as the square of the distance. We have only to substitute *source of heat* for *centre of attraction*, *flow of heat* for *accelerating effect of attraction* at any point, and *temperature* for *potential*, and the solution of a problem in attractions is transformed into that of a problem in heat.[7]

Or again, changing the analogy somewhat, for all we know from our established formula for universal gravitation, gravitational attraction could arise from the continual flow of an incompressible fluid:

> Sir William Thomson has shown that if we suppose all space filled with a uniform incompressible fluid, and if we further suppose either that material bodies are always generating and emitting this fluid at a constant rate, the fluid flowing off to infinity, or that material bodies are always absorbing and annihilating the fluid, the deficiency flowing in from infinite space, then, in either of these cases, there would be an attraction between any two bodies inversely as the square of the distance.[8]

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[6]"On Faraday's Lines of Force", op. cit., pp. 156-157.

[7]"On Faraday's Lines of Force", op. cit., p. 157.

[8]From "Attraction", an article by Maxwell in the *Encyclopedia Britannica*;

It is, to be sure, scarcely plausible that gravitation does arise in this way, for

> the generation or absorbtion of fluid requires, not only constant expenditure of work in emitting fluid under pressure, but actual creation and destruction of matter. ... According to such hypotheses we must regard the processes of nature not as illustrations of the great principle of the conservation of energy, but [implausibly] as instances in which, by a nice adjustment of powerful agencies not subject to this principle, an apparent conservation of energy is maintained.[9]

Yet it remains the case that the fluid flow analogy can convey our attention to a *formula* that captures important structure behind the facts. That this formula is somehow physically significant can be argued very powerfully by deductions from (electromagnetic) phenomena.

In "On Faraday's Lines of Force", Maxwell develops a different flow analogy, one adapted not to universal gravitation but to Faraday's theory of electromagnetism. He is concerned not to mount any physical explanation for electromagnetism, but rather to explore mathematical relationships among formulae for which support had already been powerfully given by deductions from phenomena.

> By referring everything to the purely geometrical idea of the motion of an imaginary fluid, ... I ... shew how ... the laws of the attractions and inductive actions of magnets and currents may be clearly conceived, without making any assumptions as to the physical nature of electricity, or adding anything to that which has been already proved by experiment.[10]

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vol. 2 of *The Scientific Papers of James Clerk Maxwell*, op. cit.. P. 489.

[9]"Attraction", op. cit., pp. 490-491.

[10]"On Faraday's Lines of Force", op. cit., p. 161.

An example of a formula for which support had already been powerfully given by deductions from phenomena is Coulomb's law for electrostatic attraction. Henry Cavendish had shown how the electrostatic inverse square law could be deduced from the result of a single experiment.[11] Maxwell endorsed the idea of this deduction, and improved the accuracy of the experimental determination of the exponent from -2 ± 0.02 to -2 ± 0.00005. The experiment is designed to make use of a proof in Newton's *Principia* of the following result (Proposition LXX, Book I): when the force between the parts of a uniform hollow sphere and a particle within the sphere is inverse-square, the hollow sphere exerts no net force on the particle. The proof appeals to the geometrical fact that once a point P within the sphere is specified, a unique surface element S_2 on one side of the hollow sphere is picked out by projecting through P the perimeter of any element S_1 on the opposite side; and in the limit as S_1 (and hence also its projection S_2 through P) becomes very small, the areas or magnitudes of S_1 and S_2 stand in the *direct* ratio of the squares of their distances from P. Thus if the force exerted at P by an element is *inversely* as the square of P's distance from it, the force exerted on P by each surface element of the hollow sphere is exactly balanced by the force exerted on P by a unique opposing element, thus establishing the result. Cavendish pointed out, further, that

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[11]Here I follow the discussion in Jon Dorling's paper "Henry Cavendish's Deduction of the Electrostatic Inverse Square Law from the Result of a Single Experiment", *Studies in History and Philosophy of Science* (1974) *4*: 327-348.

> if the repulsion is inversely as some higher power of the distance than the square, the particle *P* will be impelled towards the centre; and if the repulsion is inversely as some lower power than the square, it will be impelled from the centre.[12]

Armed with this result, Cavendish placed a hollow conducting sphere inside another. He charged the outer sphere, made an electrical contact between the two spheres, removed the contact, and removed the outer sphere. He then determined that the inner sphere had gained no charge (of either sign) in this process, so far as his electrometer could determine. When the inner sphere was charged, electrical contact between the spheres moved charge to the outer sphere, and again, so far as his electrometer could determine, the inner sphere was left without a charge. Maxwell had an improved electrometer, but his experimental procedure, and results, were the same. Given various additional high-level background principles, including special ones, such as that concerning Newton's result for a hollow sphere, and the general one that the electrostatic force law depends only on some power or other of the distance, the deduction from this single experimental result of the inverse-square character of the force-law is logically valid.[13]

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[12]Quoted in Dorling's "Cavendish's Deduction", op. cit., p. 330.

[13]This is argued in detail in Dorling's "Cavendish's Deduction", op. cit.

4.2. The method of physical analogy. Maxwell's method was meant to express the Newtonian attitude *Hypotheses non fingo*, while at the same time accommodating the truth of Whewell's view that it is impossible to contain the "irrepressible speculative powers of the human mind"[14], and mistaken to try. Whewell (a major influence on Maxwell at Cambridge) had written

> To debar science from enquiries [involving hypotheses], on the ground that it is her business to inquire into facts, and not to speculate upon causes, is a curious example of that barren caution which hopes for truth without daring to venture upon the quest for it.[15]

Maxwell summarizes his "middle way" between Newton and Whewell as follows:

> The first process therefore in the study of the science must be one of simplification and reduction of the results of previous investigation to a form in which the mind can grasp them. The results of this simplification may take the form of a purely mathematical formula or of a physical hypothesis. In the first case we entirely lose sight of the phenomena to be explained; and though we may trace out consequences of given laws, we never obtain more extended views of the connexions of the subject. If, on the other hand, we adopt a physical hypothesis, we see the phenomena only through a medium and are liable to that blindness to facts and rashness in assumption which a partial explanation encourages. We must therefore discover some method of investigation, which allows the mind at every step to lay hold of a clear physical conception, without being committed to any theory founded on the physical science from which that conception is borrowed, so that it is neither drawn aside from the subject

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[14]Quoted from Whewell's *Philosophy of the Inductive Science* by John Hendry in his *James Clerk Maxwell and the Theory of the Electromagnetic Field*, Bristol: Adam Hilger, 1986. P. 31.

[15]Ibid.

> in pursuit of analytical subtleties, nor carried beyond the truth by a favourite hypothesis.[16]

It is valuable, Maxwell believed, to develop *competing* "physical analogies", for then one is reminded of the overlay, in each, of dubious hypotheses. So for example, while Weber's theory, which took over results of Ampère, Neumann, and others, "is a really physical theory ... put forth by a philosopher whose experimental researches form an ample foundation for his mathematical investigations", still it is

> a good thing to have two ways of looking at a subject, and to admit that there *are* two ways of looking at it. Besides, I do not think we have any right at present to understand the action of electricity, and I hold that the chief merit of a temporary theory is, that it shall guide experiment, without impeding the progress of the true theory when it appears.[17]

John Hendry suggests that Maxwell throughout his life would have offered only Newton's theory of gravitation as an example of a "true theory".[18] Maxwell meant by "true theory" a "mature theory, in which physical facts will be physically explained".[19] "By a 'physical explanation', ... Maxwell did not mean a theory based on physical hypotheses, but rather one based on a necessary connection

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[16] "On Faraday's Lines of Force", op. cit., pp. 155-156.

[17] "On Faraday's Lines of Force", op. cit., p. 208.

[18] J. Hendry, *James Clerk Maxwell and the Theory of the Electromagnetic Field.* Bristol: Adam Hilger. P. 154.

[19] "On Faraday's Lines of Force", op. cit., p. 159.

between empirical phenomena".[20] It is clear that, for Maxwell, a "mature theory" sets forth rigorously established physical laws, that is, generalizations such as would count as "physical laws" on Newton's empiricist conception of physical laws. Neither the theory of electromagnetism nor the kinetic theory of gases ever reached complete "maturity" in Maxwell's hands, as we shall see; and Maxwell realized this. Nevertheless Maxwell claimed that these theories approach "maturity", for in some of their facets they display the same manifold overdetermination of theoretical parameters by undisputed facts that characterizes Newton's celestial gravitational mechanics.

4.3. Maxwell's development of a new theory of electromagnetism. Not long after his graduation from Cambridge, Maxwell set himself to the study of the new science of electromagnetism, and was immediately impressed by the progress Faraday had made (helped by his image of lines of force) towards a unitary understanding of electrostatic, magnetostatic, and induction phenomena. Maxwell was impressed by the evidence forthcoming from Faraday's theory-inspired investigations, that the

> transmission of electric and magnetic forces is accompanied by phenomena occurring in every part of the intervening medium.
> ... By shewing [for example] that the plane of polarisation of a ray of light passing through a transparent medium in the direction of the magnetic force is made to rotate, Faraday not only demonstrated the action of magnetism on light, but by using light to reveal the state of magnetisation

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[20]Ibid.

> of the medium, he "illuminated," to use his own phrase, "the lines of magnetic force."[21]

The analogy or "temporary theory" that Maxwell put forward in "On Faraday's Lines of Force" for this reason took over Faraday's view of "'magnetic polarity' as a property of a 'magnetic field' or space" and "the general notion of lines of force and *conducting* power."[22] Maxwell implemented a new fluid-flow analogy in order to capture Faraday's conceptions mathematically. Faraday had introduced

> the idea of stress in the medium in a different form from that suggested by Newton; [not as] ... a hydrostatic pressure in every direction, [but] ... a tension along the lines of force, combined with a pressure in all normal directions.[23]

In Maxwell's analogy, an incompressible fluid flows in fine tubes with infinitesimally thin walls and of variable cross-section, so arranged that there are no gaps, so that the fluid fills all space. Thus Maxwell replaced by a continuous measure Faraday's measure relating the strength of electromotive force in a wire with the number of lines cut. Maxwell employed the mathematics adapted to this analogy to deliver established results concerning "the less complicated phenome-

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[21]"Attraction", op. cit., p. 488.

[22]Quoted by Nancy Nersessian from Faraday's correspondence (contained in J. Larmor (ed.), *The Origins of Clerk Maxwell's Electric Ideas*, Cambridge: Cambridge University Press, 1937). See N. Nersessian, *Faraday to Einstein: Constructing Meaning in Scientific Theories*, Dordrecht: Martinus Nijhoff Publishers, 1984. P. 70.

[23]"Attraction", op. cit., p. 488.

na of electricity, magnetism, and galvanism".[24] In the second part of his paper he added another idea due to Faraday and proceeded to produce the known results concerning "the attractions and inductive actions of magnets and currents".[25] Thus Maxwell largely succeeded in his stated aim, of showing how,

> without attempting to establish any physical theory ... , by a strict application of the ideas and methods of Faraday, the connexion of the very different orders of phenomena which he has discovered may be clearly placed before the mathematical mind.

The additional idea from Faraday that Maxwell introduced in the second part of his paper was Faraday's "electro-tonic state", and Maxwell's formal representation of it allowed the derivation of some key results concerning magnetic intensity and induced electromotive force. But the relation of the formal representation to Maxwell's fluid-flow analogy, Maxwell conceded, was something he was none too clear about.

A key advance on the analogy was made by Thomson, who

> proved, by strict dynamical reasoning, that the transmission of magnetic force is [to be] associated [in a fluid-flow analogy] with a rotatory motion .... He shewed, at the same time, how the centrifugal force due to this motion would account for magnetic attraction.[26]

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[24]"On Faraday's Lines of Force", p. 159.

[25]Ibid.

[26]"Attraction", op. cit., p. 488.

Maxwell was inspired by this suggestion to move from a purely kinematical analogy for lines of force to a dynamical one. Whereas in the paper "On Faraday's lines of Force", Maxwell used the purely kinematical terms of fluid-flow velocity to represent the intensity and direction of lines of force, in Maxwell's second paper, "On Physical Lines of Force", Maxwell used further terms, terms that were newly dynamical. His new analogy showed by what sorts of underlying forces in a material medium these lines of force could arise. The move multiplied the devices that Maxwell could put at his own disposal for the purposes of formally representing within his analogy such formerly recalcitrant notions as that of Faraday's "electro-tonic state".[27] Initially, Maxwell was guided in the development of a dynamical analogy by the expectation that it should account in an intuitively direct way for the tension along lines of force and lateral repulsion between them, for the orthogonality of electric and magnetic actions, and for the rotation by magnetic action of the plane of polarized light that is transmitted through a diamagnetic substance.

In "On Physical Lines of Force" Maxwell developed a dynamical analogy that would meet these and other demands, and by monkeying with it and with the mathematics used to describe it he found the field equations that successfully unified all the electromagnetic laws by then established. The equations in fact

[27]For fuller discussion, see Nersessian, *Faraday to Einstein*, op. cit., pp. 74-86, and J. Hendry, *Maxwell*, op. cit., pp. 156-219.

implied still more: they implied that electromagnetic actions are not transmitted instantaneously, but that there is a finite velocity that is calculable from experimentally measurable parameters characterizing the medium. It is well known that when Maxwell experimentally measured the relevant parameters for space free of ponderable matter and calculated this velocity, the velocity proved to be approximately that of light in free space. Suddenly all known and unknown phenomena of light were to be considered within the purview of the new theory of electromagnetism. This development for some while represented, in Lakatos' terms, a singularly progressive problem shift for Maxwell's electromagnetic research programme.

4.4. "A Dynamical Theory of the Electromagnetic Field". In his third paper on electromagnetism[28] and in his *Treatise*[29], Maxwell claimed that he could now dispense with analogies, and using the Lagrangian apparatus of a generalized dynamics could *deduce* his equations from known facts. Maxwell claimed that he could make this deduction using only one additional premise, that the energy of electromagnetic interactions resides in the field. However, as is well known, Maxwell had difficulties justifying with anything like complete demonstrative

[28]"A Dynamical Theory of the Electromagnetic Field", contained in Niven (ed.), *Maxwell's Papers*, op. cit., pp. 526-597.

[29]*A Treatise on Electricity and Magnetism*, vol. II, part IV, ch. V.

rigour his device of a "displacement current", an innovation that was key to the theory's success. Thus Stein writes:

> [T]he "displacement current" ... appears in Maxwell first as a consequence of a very special characteristic of his detailed model. In the refined version, the displacement current is retained, alongside -- not as a consequence of -- the general dynamical assumptions about the medium; and Maxwell's exposition (both in his definitive paper and in the later *Treatise on Electricity and Magnetism*) is very cryptic on the matter: one sees clearly neither what motivates nor what justifies the introduction of the displacement current; and the physical content of the hypothesis is obscure (because it is the one point of detail assumed about a medium that is otherwise left vague).[30]

Maxwell assumed the materiality of the medium through which electromagnetic actions propagate.

> The theory I propose may ... be called a theory of the *Electromagnetic Field*, because it has to do with the space in the neighbourhood of the electric or magnetic bodies, and it may be called a *Dynamical* Theory, because it assumes that in that space there is matter in motion, by which the observed electromagnetic phenomena are produced.[31]

Maxwell was careful to call his assumption of the materiality of the medium an *hypothesis*.[32] That electromagnetic actions physically propagate, so that the energy of electromagnetic interactions is transferred non-instantaneously, Faraday had experimentally indicated and Maxwell himself had roundly demonstrated.

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[30]Stein, "On the Notion of Field", op. cit., p. 281.

[31]"A Dynamical Theory of the Electromagnetic Field", in Niven (ed.), *Maxwell's Papers*, op. cit., p. 279.

[32]For example, at the close of his two-volume *A Treatise on Electricity and Magnetism*, Third Edition, Oxford: The Clarendon Press, 1904.

And this certainly supported the assumption of the materiality of the medium. For given non-instantaneous propagation, if energy is conserved it must take up positions in the space between two electromagnetically interacting bodies (call this Premise 1). But it is familiar to us that *energy resides in material things* (call this Premise 2); the idea that energy may reside in *empty* space Maxwell dismisses as implausible.[33] From these two premises, the conclusion follows. But Maxwell recognizes that these premises are undemonstrated, hypothetical elements of his system. Maxwell stresses that, having made the assumption of the material ether, it is incumbent upon us to reckon clearly and precisely to ourselves the nature and consequences of this assumption -- we must "endeavour to construct a mental representation of all the details of [the material medium's] action".[34]

Why then should we not see Maxwell as employing the hypothetico-deductive method after all? There are a number of things to say in reply to this question. For one thing, Maxwell's hypothesis is of very great generality. It is scarcely to be compared with the highly specific and intricate hypothesis of, say,

[33]The idea of energy in truly matter-free space was foreign to physicists' thinking and experience in Maxwell's day. Maxwell quotes, in agreement, a remark by Torricelli that explains this orthodoxy: Toricelli says that energy "is a quintessence of so subtile a nature that it cannot be contained in any vessel except the inmost substance of material things". Quoted in the *Treatise*, op. cit., vol. II, p. 493.

[34]Ibid.

the seventeenth-century mechanical philosophers. It was characteristic of *those* hypotheses that others, equally capable of saving the phenomena, could easily be invented to take their place. Maxwell's hypothesis of the materiality of the ether is not like that: an alternative was beyond Maxwell's imagination, let alone ready construction. For another thing, given his hypothesis, Maxwell was free to form a dynamical theory of electromagnetism by what he called "[t]he true method of physical reasoning", namely, "to begin with the phenomena and to deduce the forces from them by direct application of the [Hamiltonian and Lagrangian] equations of motion".[35] There are two marks against saying what might otherwise seem a natural conclusion, that this step made the general hypothesis of a material ether into a specific hypothesis concerning a mechanical ether. First, neither Maxwell nor any other proponent of his theory ever really succeeded in accounting by a specific, detailed mechanical hypothesis for all facets of electromagnetism. There was no mechanical ether-model that really could encompass all known optical and electromagnetic phenomena. To quote J. D. Buchwald, the idea that Maxwell developed his theory by building a mechanical model

> mistakes a future hope for the era for a practical method of investigation. ... It is certainly true that most British scientists hoped one day to obtain a structure for the ether. Nevertheless, this was not generally required for immediate goals: the British were able to develop a theory which is profoundly different from the modern one, but which does not rely on an

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[35]From "On the Proof of the Equations of Motion of a Connected System", in Niven (ed.), *Maxwell's Papers*, op. cit., p. 309.

> ether model. Instead, the theory employed Hamilton's and Lagrange's equations in ways we no longer permit.[36]

The dynamical system that Maxwell successfully described simply is *not* a mechanical system in the strict sense of conforming to Newtonian mechanics. A second reason for denying that Maxwell made the general hypothesis of a material ether into a specific hypothesis concerning a mechanical ether is that Maxwell and his contemporaries attempted to develop the unifying power of their theory in a way that would *not* compound the hypothetical details within mechanical models involving the ether. The resistance to conjecture or wanton model-building was actually stronger then than in the present day. Buchwald explains:[37]

> Modern theory seeks unified explanations in an unmodifiable set of field equations coupled through electron motion to intricate microphysical models. Maxwellian theory sought unity through a highly plastic set of field equations coupled to Hamilton's principle.

Maxwell is in fact explicitly disdainful of the hypothetico-deductive method:

> In forming dynamical theories of the physical sciences, it has been a too frequent practice to invent a particular dynamical hypothesis and then by means of the equations of motion to deduce certain results. The agreement of these results with real phenomena has been supposed to furnish a certain amount of evidence in favour of the hypothesis.[38]

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[36]J. D. Buchwald, *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century*, Chicago: University of Chicago Press, 1985. P. 20.

[37]Buchwald, op. cit., p. 23.

[38]From "On the Proof of the Equations of Motion of a Connected System",

Rather, Maxwell proposes that

> The true method of physical reasoning is to begin with the phenomena and to deduce the forces from them by a direct application of the equations of motion. The difficulty of doing so has hitherto been that we arrive, at least during the final states of the investigation, at results which are so indefinite that we have no terms sufficiently general to express them without introducing some notion not strictly deducible from our premisses. ...[T]herefore ... [we] should invent some method of statement by which ideas, precise so far as they go, may be conveyed to the mind, and yet sufficiently general to avoid the introduction of unwarrantable details.[39]

Maxwell *thinks* that he has avoided all introduction of unwarrantable *details* in his articulation of his dynamical theory of the electromagnetic field. (This is false, as we have seen; the device of the displacement current introduces an unwarrantable detail in his theory.) Maxwell believes that he must introduce one general notion which is not deducible from his Lagrangian starting point, that of the materiality of the ether, in order to begin assimilating electromagnetics to the science of dynamics in the first place. (This belief also proved false, and Maxwell's own theory helped familiarize people with the reason why it is false: Maxwellian electrodynamics is simply not representable by the motions of a material, strictly Newtonian-mechanical system.)

With the advance of physical conceptions (in particular, concerning the nature of "charge" and the relation of "dynamics" to "mechanics") it not only

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op. cit., p. 309. The second sentence is evidently ironic.

[39]Ibid.

became easy to correct Maxwell's mistakes, but also became possible to provide an articulation of electrodynamics more consonant (because it needed no special assumptions concerning the nature of the displacement current or the materiality of the ether) than Maxwell's with Maxwell's own preferred dynamical "method of statement" for physical ideas.

4.5. "On the Dynamical Theory of Gases". In the case of his contributions to the kinetic theory of gases, Maxwell's work again is in two distinct phases, the earlier one guided by quite detailed "physical analogies", the later one guided by Lagrangian dynamics. In both phases Maxwell is very aware of the gap between hypotheses and established facts, and in the later phase he purports to avoid hypotheses, establishing elements of theory and measuring key theoretical parameters by Newtonian deductions from phenomena. Here again, Maxwell cannot claim to eliminate quite all hypotheses from his reasoning. But the one hypothesis that he retains -- it "assumes no more than that [gases] are material systems"[40] to which Lagrangian analytical mechanics is applicable, and is thus very like the hypothesis of the materiality of the ether discussed last section -- is singularly general and so is relatively unsusceptible to the complaint that an alternative, incompatible hypothesis could equally well save the phenomena. Armed with this hypothesis (and with Clausius's definition of and mathematical

[40]"On the Dynamical Evidence for the Equations of Motion of a Connected System", in Niven (ed.), *Maxwell's Papers*, op. cit., vol. II. P. 420.

results concerning the *virial* of an attractive or repulsive force between the parts of a material system) Maxwell was able to mount cogent arguments from common observations and well-known experimental results to the conclusion that the smallest parts of a gas are in violent motion, and that this motion gives rise to the pressure from a gas on the walls of its container.

The rigour of the arguments is indicated in part by the way they would later serve to test classical mechanics itself (severely, and ultimately, in fact, fatally). From his very first paper Maxwell points to the inconsistency of kinetic theory with known phenomena of specific heats.[41] Jon Dorling argues that

> regarded merely as a way of devising as severe as possible a test of classical physics, Maxwell's procedure, of attempting to derive as much as possible of kinetic theory from the single assumption that gases are systems obeying the equations of classical analytical mechanics, was a stroke of genius. For it was precisely the combination of increasingly rigorous derivations of the equipartition of energies theorem from assumptions as general as those of Maxwell with the increasingly severe conflicts between the resulting predictions of specific heats (including that of the specific heat of a vacuum) and the results of experimenters (occupying the last quarter of the nineteenth century) that led to the overthrow of classical physics.[42]

In the same paper, Dorling details and critically appraises Maxwell's various

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[41]See "Illustrations of the Dynamical Theory of Gases", in Niven (ed.), *Maxwell's Papers*, op. cit., p. 409.

[42]J. Dorling, "Maxwell's Attempts to Arrive at Non-Speculative Foundations for the Kinetic Theory", in *Studies in History and Philosophy of Science* (1970) *1*: 229-248. See p. 236.

attempts at making the relevant deductions from phenomena; I shall not repeat the details here.

4.6. Maxwell's Newtonian Empiricism. Are the Lagrangian equations to which Maxwell related his investigations both of electromagnetism and of gases not themselves hypothetical assumptions? Maxwell was sensitive to this question. To be sure, the equations merely express Newton's laws of motion. (In the form in which Maxwell employed them, they in fact express a weakened version of Newton's laws.) They are intended to facilitate the science of dynamics in its "primary aim" of deducing the forces at work in a system from the observed motions of the system. Maxwell believes that the Lagrange equations not only facilitate the solution of such dynamical problems, but also "present ... to the mind in the clearest and most general form the fundamental principles of dynamical reasoning". Our question, however, is this: is such reasoning not based on a hypothesis, viz., that the world is fundamentally dynamical in the relevant sense?

Writing nearly two centuries after Newton, Maxwell could contend that

> [t]he fundamental dynamical idea of matter, as capable by its motion of becoming the recipient of momentum and of energy, is so interwoven with our forms of thought that whenever we catch a glimpse of it in any part of nature, we feel that a path is before us leading, sooner or later, to the complete understanding of the subject.[43]

[43]Quoted by J. Hendry, *Maxwell*, op. cit., p. 237.

The Lagrange equations simply give the most helpfully general form to the "fundamental dynamical idea".

> The aim of Lagrange was ... to bring dynamics under the power of the calculus, and therefore ... to express dynamical relations in terms of the corresponding relations of numerical quantities....
> [T]he importance of these [Lagrange's] equations does not depend on their being useful in solving problems in dynamics. A higher function which they must discharge is that of presenting to the mind in the clearest and most general form the fundamental principles of dynamical reasoning.[44]

Maxwell thinks that our knowledge of *relations* or thus of abstract structure may be secure even when knowledge of the things related is not given to us. "[I]n a scientific point of view the *relation* is the most important thing to know," Maxwell writes. This, he thinks, is just the sort of knowledge that application of Lagrange's equations can deliver to us. When one deals thus with relations one is dealing with an analogy, in this case an analogy between certain relations among dynamical variables and certain relations among numerical quantities. If the analogy is "*real*", the abstract relational structure is not something merely subjective; in the world itself there is the same structure; a physically significant aspect of nature *is* analogous to the relational structure we have in mind. Maxwell thinks that geometry furnishes an example of a real analogy between mind and nature:

> When we say that space has three dimensions, we not only express the impossibility of conceiving a fourth dimension, co-ordinate with the three known ones, but assert the objective truth that points may differ in

[44]"On the Proof of the Equations of Motion of a Connected System", op. cit., pp. 308-309.

> position by the independent variation of three variables. Here, therefore, we have a *real* analogy between the constitution of the intellect and that of the external world.[45]

Is there a real analogy, then, between the Lagrangian formalism and the world? Or are our dynamical reasonings just a projection from our minds? This worry about the objectivity of the fundamental patterns of our thought, Maxwell thinks is very general:

> the whole framework of science, up to the very pinnacle of philosophy, seems sometimes a dissected model of nature, and sometimes a natural growth on the inner surface of the mind.[46]

In thus emphasizing the immanence of objective and subjective elements in all human knowledge, Maxwell followed his teacher William Whewell, who had written

> in all human knowledge both Thoughts and Things are concerned .... The combination of the .wo elements, the subjective or ideal, and the objective or observed, is necessary, in order to give us any insight into the laws of nature.[47]

Maxwell discusses this fundamental antithesis in relation to arithmetic ("number implies a previous act of intelligence"[48]), causation ("[causes are] reasons, analo-

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[45]Quoted in J. Hendry, *Maxwell*, op. cit., p. 147.

[46]Quoted by J. Hendry, *Maxwell*, op. cit., p. 146.

[47]William Whewell, *Philosophy of Inductive Science*, 2nd ed., 1847. Vol. 1, p. 17, p. 20. Quoted in J. Hendry, *Maxwell*, op. cit., pp. 28-29.

[48]Quoted in J. Hendry, *Maxwell*, op. cit., p. 147.

gically referred to objects instead of thoughts"[49]), and geometry. In the case of geometry, Maxwell is careful, as we have seen, to state that there is a "*real* analogy" between mind and nature.

Maxwell is in general inclined to the anti-sceptical view that nature has so constituted our minds that the forms of our thinking match the fundamental forms of nature. Sometimes Maxwell writes optimistically and with apparent conviction of "the great doctrine that the only laws of matter are those which our minds must fabricate, and the only laws of mind are fabricated for it by matter".[50] On other occasions, however, he entertains the obvious sceptical worry that nature may have so constituted our minds that we cannot conceive what are in fact the fundamental principles of nature's operation. For example, while Maxwell evidently conceives Lagrangian dynamics to reflect the most general, fundamental form of our physical understanding, he is not willing to say dogmatically that *the world* fundamentally organizes itself around dynamical relations. It is true that Maxwell sought to organize physical *knowledge* around Lagrangian dynamics; but he acknowledges the possibility that this will not work. He supposes that the "book of nature" has been written cover to cover in dynamical characters. Yet he is open to a radical challenge to this metaphor:

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[49]Ibid.

[50]Quoted by J. Hendry, *Maxwell*, op. cit., p. 149.

> Perhaps the 'book', as it has been called, of nature is regularly paged; if so, no doubt the introductory parts will explain those that follow, and the methods taught in the first chapters will be taken for granted and used as illustrations in the more advanced parts of the course; but if it is not a 'book' at all, but a *magazine*, nothing is more foolish than to suppose that one part can throw light on another.[51]

What teaches that nature is book-like rather than magazine-like is the *experience* that physical knowledge has again and again been improved by comprehending formerly disparate elements together in a way organized around the dynamical conceptions that Maxwell held to be fundamental.

> But as physical science advances we see more and more that the laws of nature are *not* mere arbitrary and unconnected decisions of Omnipotence, but that they are essential parts of one universal system.[52]

This empirical basis for Maxwell's method of course is fallible. We may discover ranges of phenomena that are not organizable around the dynamical conceptions that Maxwell held to be fundamental. In that case the mind would need to settle on new fundamental conceptions. Some things that Maxwell says suggest that, in his view, the power of the mind to make such adjustments is quite limited. This, however, is not a rationalist point; the warrant for whatever conceptions are adopted must come from experience.

Thus Maxwell, like Newton, is a thoroughgoing empiricist, an empiricist about even the most basic conceptions and methodological precepts. Maxwell

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[51]Quoted in J. Hendry, *Maxwell*, op. cit., p. 149.

[52]Quoted in J. Hendry, *Maxwell*, op. cit., p. 151.

underscores his empiricism in two other ways. He insists that ultimately all truth is brute. Physical science, he says, makes it "possible for us to have an ever increasing stock of *known* truth concerning things whose nature is absolutely incomprehensible".[53] (This view is unnecessarily harsh on the prospects for human comprehension of nature. The needs of empiricism are satisfied by saying something less, that science's standards of comprehensibility are themselves not given by reason but empirically discovered, and why, given such standards, nature should be comprehensible by their lights is by their lights an incomprehensible, brute fact.) Maxwell also relates the idea of the "physical significance" of the structures called to our attention by a ("mature") theory directly to the connections among phenomena that the theory brings to light.

4.7. Implications for Cartwright. Cartwright presents Maxwell's conception of science as being much like her own. We have seen that this view of Maxwell is much mistaken. Maxwell does not develop theoretical laws by reference to the fictional situations of a model. In some respects he was more shy of model-building than, say, today's electrodynamicists. Maxwell had an uncommon concern for the overall coherence and systematicity of scientific thought. For Cartwright, Maxwell's theoretical efforts, because they systematize, must fudge on the truth. But Maxwell deduced his theoretical results from the phenomena. The successes he achieved in this thus helped establish theoretical *laws* in part

[53] Ibid.

through methods Cartwright insists can only measure *capacities*. Cartwright accepts that the methods in question lead to truth. Thus she is committed by her methodological position to the view that Maxwell's theoretical accomplishments are true, but is also commited by her thesis that nature is not systematized by any laws to the view that Maxwell's theoretical accomplishments are false.

For Maxwell, the method followed by scientists of one generation may need to be rejected by scientists of the next generation. Cartwright does not consider whether methods change or how they are learned. For Maxwell, methodological precepts are warranted empirically, just like everything else. Thus Maxwell concurred with Newton's empiricism about method. He also thought that Newton's method, with its emphasis on deductions from phenomena, or (more specifically) on using motions to measure forces, was still working just fine. In Maxwell's view, the empirical evidence (from the steady advance of Newtonian physics) that recommends this method also argues that nature is systematic. Nature (it seems) is through and through Newtonian-dynamical. In fact the point about systematicity is supported less well by Maxwell's own work than by its aftermath. Maxwell introduced tensions among the received principles of theoretical physics without himself realizing that he had done so. But these tensions were eventually resolved through the empirically progressive developments that I discuss in the next chapter. The work was done by a thinker who, like Maxwell, had an uncommon concern for the overall coherence and systematicity of thought. We shall see that, once again, key inferences in this development were supported by Newtonian deductions from phenomena. Thus the Newtonian method more or less completely survived, even as the underlying physical principles changed their form. Evidence for the systematicity of nature carried forward much increased.

Chapter 5

How Einstein, by Applying Newtonian Methods, Advanced Beyond Newtonian Physics

In Chapter 4 we saw how Maxwell employed Newton's method, making advances that for the first time rendered empirically problematic the mechanical principles that the method assumes, thus underlining the empirical status of the method. A principal task in this chapter is to ask whether Newton's method survived these challenges, and to see in Einstein's work that in fact it did. In particular we will examine how Einstein, still employing Newton-styled deductions from phenomena, inferred a new kinematical basis for the principles of mechanics, thus amending Newton's mechanical assumptions in a way that sustained their former successes.

For Cartwright, Einstein is chief among those physicists who find Cartwright's own "measurement-based empiricism ... too stringent ... to be appealing", who willingly leap beyond "judgements imposed by the phenomena" and let "mathematical considerations ... shape ... theory" (*Capacities*, p. 6). She Cartwright says explicitly that she will "let" Einstein speak for all other physicists of like mind (ibid.). Then she quotes Einstein defending a holist, hypothetico-deductivist conception of the relation of theory to evidence.

> It is really our whole system of guesses which is to be either proved or disproved by experiment. No one of the assumptions can be isolated for

separate testing.[1]

The Einstein whom Cartwright has thus set up is a straw man. Einstein was an outstanding measurement-making empiricist: one may look to his work to find the finest examples of Newton-styled, bootstrap empiricism. The phenomena by which Einstein "measured" or overdetermined elements of theory were, however, often colligations not of facts from any experiment by Einstein, but rather of facts delivered by numerous, diverse, well-known experiments. The elements of theory that he "measured" or deduced from phenomena were typically very high-level. Einstein was no skeptic about high-level theory. The quote puts Einstein in the position of accepting holism and hypothetico-deductivism and consequently the underdetermination thesis. One who has accepted all of that must in consistency be a skeptic about theory. Cartwright presents Einstein as author of a skeptical, because holist, philosophy, "letting" him speak for himself (and "for the rest" who work with mathematics at high levels of theoretical physics) in just two sentences that she draws from a popular book by Einstein and Leopold Infeld on history of physics.

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[1]The quote is from Einstein's popular book, co-authored with Leopold Infeld, *The Evolution of Physics*, New York: Simon and Schuster, 1938; pp. 30-31. The context for it is a discussion of classical mechanics, and its purpose is to condition the reader to expect that despite the systematic successes of that theory it will later be found inadequate. In the next paragraph Einstein and Infeld briefly and equivocally discuss, in effect, the underdetermination thesis, at first appearing to accept it, then appearing to shrug it off. The discussion is neither philosophically deep nor intended to be.

In this chapter I discuss at a length greater than two sentences both Einstein's character as a scientific thinker (I focus on his early period) and his own second-order reflections about scientific thinking. Einstein's example very clearly illustrates the importance of the pursuit of systematicity in knowledge for bringing highest-level elements of physical doctrine under the check of experience. At the same time it shows how the empirical checks even on highest-level elements of physical doctrine can (by brilliant application of Newton's method) be made very cogent. Einstein's own reflections about scientific thinking show that he was an empiricist about method, and that he held the doctrine I have called pluralism about the methods of science. In some of its aspects, Einstein's work is uniquely well-illuminated by the formal, deductivist conceptions of the positivists, and Einstein knew it to be so. But Einstein also knew the inadequacies of positivism. Einstein himself undertook much other work whose character cannot be well-illuminated in its terms. In his work on space-time theory Einstein was thoroughly positivistic, and I will argue that because Cartwright is systematically anti-positivistic she cannot offer a correct picture of space-time theory and its development. Neither Cartwright's simulacrum account of theoretical explanation from *Laws* nor her account of the abstractness of theories from *Capacities* is at all adequate in connection with relativity theory. In arguing that positivism provides the right corrective for Cartwright's problems with space-time theory, I certainly do not signal agreement with positivists who are conventionalists about space-time theory. I discuss why the thesis of space-time conventionalism is unfounded and false, and in this discussion challenge Cartwright further. For conventionalism is defeated by showing that space-time principles are objective principles, that they are, in fact, among the best-confirmed elements in the entire corpus of presumed knowledge. And this conclusion is contrary to Cartwright's sceptical view of elements of high-level theory. I also argue that the commitments of a space-time theory are structural rather than ontic, illustrating a further respect in which Cartwright's ways of thinking about scientific theory, which emphasize ontic commitment, are inadequate in connection with space-time theory.

The fact is that Cartwright has not ventured any philosophy of space-time theory in connection with her general conception of theoretical science. Because accepted space-time principles strongly condition what other principles may be counted as laws, because they determine the form of possible models of physical systems rather than directly describing the behaviour of objects in those models, and because they are thus (in a word) particularly high-level elements of physical doctrine, it may seem that Cartwright could except these principles from the status of laws of nature, and thus see no challenge to her position from the discussion in this chapter. In Chapter 6 I shall argue that such a response would not do. In that chapter I shall discuss and defend an empiricist conception of laws, on which space-time principles are to be considered (because of all

I say about them in this chapter) among the very best examples of laws of nature, and the explanations that can be mounted from them among the clearest examples of theoretical explanation. Thus one ultimate aim of the present chapter, to give an example which (if what I say about it is correct) simply *refutes* Cartwright's thesis of the falsity of all high-level theoretical laws, will actually be fulfilled only after further arguments are introduced in Chapter 6.

5.1. From Maxwell to Einstein. Maxwell's deductions from phenomena established principles of field theory and statistical mechanics that evidently captured aspects of real, deep-lying structures in nature. But it was unclear how these structures could be fleshed out consistently with other established principles (including the strict formulation of Newton's third law of motion) and with other established phenomena (such as those concerning specific heats). It was also unclear how these structures (one continuous, the other discrete) could be satisfactorily united with one another. Maxwell himself pointed to difficulties concerning the interaction of radiation and particulate matter, and to the problem of specific heats for the kinetic theory of gases. Work in the generation following Maxwell only sharpened physicists' sense of the difficulties. Moreover, the view that the ether is a mechanical system just could not be satisfactorily made out.

Maxwell thus took classical physics in many directions as far as it could go, achieving a many-sided appreciation of its strengths, and also many inklings of its limits. A generation later, a still youthful Albert Einstein explored the strengths and weaknesses of classical physics in the same many-sided way. In fact Einstein is the first subsequent physicist of genius to have had interests of equal range with and roughly corresponding in particulars to Maxwell's. How as synthetic a perspective on physics as Maxwell's could have been in Einstein's full command so early in Einstein's life is a subject of wonder, and of unending scholarly interest.[2] For our purposes it is important simply to illustrate, briefly, Einstein's many-sided involvement with classical physics, and then discuss the method by which Einstein made an advance beyond it.

Einstein was captivated by Maxwell's electromagnetic theory from early in his youth, and his interest in no way abated as his formal studies advanced. He later recollected that

> [t]he most fascinating subject at the time that I was a student was Maxwell's theory [of electromagnetism]. What made this theory appear revolutionary was the transition from forces at a distance to fields as fundamental variables. The incorporation of optics into the theory of

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[2]For a fine recent study see Lewis Pyenson's *The Young Einstein: The Advent of Relativity*, London: Adam Hilger, 1985. We learn in this book that, contary to prevalent myths, Einstein's early education contributed very positively to his development as a physicist. Both at the Münich school from which he dropped out at 15, and at the Swiss Cantonschule at which he earned his matriculation, Einstein had science and mathematics teachers who, in today's world, would be shining lights in university-based teaching and research. Moreover, Einstein faced an exacting curriculum, so that before he had finished secondary school Einstein possessed at least the level of formal instruction obtainable from a better present-day tertiary-level programme in mathematics and science. It is true that Einstein scarcely attended his undergraduate classes at the Federal Institute of Technology (ETH). But already by the time of his entry to the ETH Einstein was quite thoroughly prepared for independent study.

> electromagnetism, with its relation of the speed of light to the electric and magnetic absolute system of units as well as the relation of the refraction coëfficient to the dielectric constant, the qualitative relation between the reflection coëfficent and the metallic conductivity of the body -- it was like a revelation. Aside from the transition to field-theory, i.e., the expression of the elementary laws through differential equations, Maxwell needed only one single hypothetical step -- the introduction of the electrical displacement current in the vacuum and in the dielectrica and its magnetic effect, an innovation which was almost prescribed by the formal properties of the differential equations.[3]

Einstein's studies at school principally introduced him to the classical, mechanical physical world-conception, whose

> precise development ... was the achievement of the 19th century. What made the greatest impression upon the student, however, was less the technical construction of mechanics or the solution of complicated problems than the achievements of mechanics in areas which apparently had nothing to do with mechanics: the mechanical theory of light, which conceived of light as the wave-motion of a quasi-rigid elastic ether, and above all the kinetic theory of gases: --the independence of the specific heat of monatomic gases of the atomic weight, the derivation of the equation of state of a gas and its relation to the specific heat, the kinetic theory of the dissociation of gases, and above all the quantitative connection of viscosity, heat-conduction and diffusion of gases, which also furnished the absolute magnitude of the atom. These results supported at the same time mechanics as the foundation of physics and of the atomic hypothesis.... [I]t was also of profound interest that the statistical theory of classical mechanics was able to deduce the basic laws of thermodynamics, something which was in essence already accomplished by Boltzmann.[4]

For his doctoral dissertation, Einstein studied these hidden strengths of mechanics. These statistical researches within kinetic theory continued through to his

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[3]From Einstein's "Autobiographical Notes", in P. A. Schilpp (ed.), *Albert Einstein: Philosopher-Scientist*, New York: Harper Torchbooks, 1959. Pp. 33, 35.

[4]Einstein, "Autobiographical Notes", op. cit., pp. 19, 21.

landmark study of Brownian motion, completed in 1905, and beyond. But even as he explored the subtlest and profoundest strengths of the mechanical viewpoint, Einstein made no firm intellectual commitment to it. From an early age, Einstein could see problems with it. Not least significant was a problem concerning the mechanical ether that he first formulated at age sixteen.

> If I pursue a beam of light with the velocity *c* (velocity of light in a vacuum), I should observe such a beam of light as a spatially oscillatory electromagnetic field at rest. However, there seems to be no such thing, whether on the basis of experience or according to Maxwell's equations.[5]

At age 22 Einstein learned of Planck's discovery, which (ahead of the entire physical community) he immediately interpreted as showing that mechanics and thermodynamics are correct only as limits when a certain non-classical discreteness or finite parcelling of physical actions is regarded as negligible. Einstein already doubted the adequacy of the classical mechanical ether conception of electromagnetic radiation. It was natural, therefore, for him to apply Planck's insight to the question of the nature of electromagnetic radiation. The conclusion that followed -- concerning the quantization of radiation -- was in his own estimation "very revolutionary". His light-quantum hypothesis quickly proved empirically progressive. It was, however, conceptually baffling, not least to Einstein himself.

From the vantage point provided by this work, Einstein could feel intrigued rather than surprised or bothered by the fact that Maxwell's theory of

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[5]Einstein, "Autobiographical Notes", op. cit., p. 53.

continuous fields was resisting interpretation in terms of the detailed workings of a mechanical ether. To interpret Maxwell's equations mechanically

> was zealously but fruitlessly attempted, while the equations were proving themselves fruitful in mounting degree. One got used to operating with these fields as independent substances without finding it necessary to give one's self an account of their mechanical nature; thus mechanics as the basis of physics was being abandoned, almost unnoticeably, because its adaptability to the facts presented itself finally as hopeless.[6]

The problems that beset the hypothesis of the mechanical ether are well described by reference to our continuum picture from Chapter 2. This hypothesis ran into trouble not in the minds of any very substantial group of physicists, but only in the mind of one with a system-seeking, critical and synthetic perspective on physical science as a whole. And it ran into trouble not because of recalcitrant observational results (it is too often suggested that the Michelson-Morley experiment refuted it), but rather because of difficulties with intermediary assumptions about what the ether is like. The observations were all accounted for, but in accounting for them tensions were generated further along the continuum. For example, various null results suggested ether drag, but the transverse nature of light waves suggested a "rigid" ether. Still other results suggested partial ether drag. Many other problems concerning the ether established themselves also, but always at an intermediary point on the continuum. Notwithstanding a plethora of such problems, few physicists questioned the hypothesis of the mechanical ether. Einstein is remarkable for laying stress on

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[6]Einstein, "Autobiographical Notes", op. cit., pp. 25-27.

them. It took someone with an uncommon concern for the overall systematicity of physical thought, someone already concerned that physics was developing away from its traditional mechanical basis, even to consider abandoning the orthodox conception of a mechanical ether. We will examine how, in the process, Einstein drove change in thinking right out to the metaphysical end of the continuum, and yet secured, in Newtonian deductions from phenomena, his advances beyond Newtonian physics.

5.2. Einstein's Quest for a Coherent "World-Picture". Einstein wished to provide a new and adequate explanatory framework (in his terms, a new *world-picture*) for physics.[7] To Einstein, physics appeared to be in "an intermediate state ... without a uniform basis for the entirety".[8] The duality of material-points versus continuous-field fundamental conceptions in physics troubled Einstein.[9] Thus in his 1905 paper introducing the light-quantum hypothesis, Einstein wrote:

> There exists a deep-going, formal distinction between the theoretical representations which physicists have formed for themselves concerning gases and other ponderable bodies on the one hand, and the Maxwellian

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[7]See M. Klein, "No Firm Foundation: Einstein and the Early Quantum Theory", in H. Woolf (ed.), *Some Strangeness in the Proportion*, Reading: Addison-Wesley, 1980, pp. 161-185.

[8]Einstein, "Autobiographical Notes", op. cit., p. 25.

[9]See Klein, "No Firm Foundation", op. cit., and A. Pais, "Einstein on Particles, Fields and the Quantum Theory", in H. Woolf (ed.), op. cit., pp. 197-251.

> theory of electromagnetic processes in so-called empty space on the other hand.[10]

In his profound studies of statistical fluctuations, Einstein explored the strengths of the discrete, material-points conception. This yielded fundamental results on the one hand in the mechanical domain of molecular phenomena, and on the other hand in the domain of radiation phenomena.[11] These results were far-reaching in their significance: Einstein's theory of the Brownian motion provided key parts of the theoretical basis for Jean Perrin's spectacular experimental overdetermination of Avagadro's number, a convergent set of theoretically disparate determinations that were accepted by the scientific community as finally providing compelling evidence for the long-doubted molecular hypothesis (which most chemists and physicists had treated instrumentalistically throughout the nineteenth century);[12] and Einstein's introduction of light quanta was an

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[10]Quoted by G. Holton, "Einstein's Scientific Programme: the Formative Years", in H. Woolf (ed.), *Some Strangeness in the Proportion: A Centennial Symposium to Celebrate the Achievements of Albert Einstein*, Reading, MA: Addison-Wesley, 1980. P. 55.

[11]Abraham Pais has given us a thorough, authoritative account of this work, in his "Einstein on Particles, Fields and the Quantum Theory", op. cit., and in his *Subtle is the Lord: The Science and Life of Albert Einstein*, Oxford: Oxford University Press, 1982.

[12]See M. J. Nye, *Molecular Reality*, New York: Elsevier, 1972; A. Pais, *Subtle is the Lord*, op. cit., chap. 5.; and (for a useful summary) Wesley Salmon, *Scientific Explanation and the Causal Structure of the World*, Princeton: Princeton University Press, 1984, pp. 213-226.

event of singular importance in the early development of the quantum theory.[13] Yet it was the continuous-field fundamental conceptions that were ascendant in Einstein's youth, with Hertz and the great Lorentz working to ground mechanics in electromagnetism rather than the other way about. They thought that they had made good progress toward an adequate theory of the electron as a localized wrinkle in the electromagnetic field; they thought that their theory should be able to explain even the *mass* (or resistivity to acceleration) of the electron in electromagnetic terms, thus suggesting the reducibility of mechanics to electrodynamics. Einstein was vitally interested in this programme, too, and his discovery of the special theory of relativity can be regarded as a theoretical exploration within this programme. In later writings Einstein notes that field theory was the established way of thinking in physics after Maxwell:

> before Maxwell people conceived of physical reality ... as material points, whose changes consist exclusively of motions, which are subject to total differential equations. After Maxwell they conceived physical reality as represented by continuous fields, not mechanically explicable, which are subject to partial differential equations.[14]

The change, however, was by no means completely carried out, and Einstein from early days was aware in manifold ways of the mechanical residuum, and the "deep-going opposition" between it and the field conceptions.

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[13]See Klein, "No Firm Foundation", op. cit., and Pais, "Einstein on Particles, Fields, and Quantum Theory", op. cit.

[14]"Maxwell's Influence on the Evolution of the Idea of Physical Reality", in *Ideas and Opinions*, New York: Bonanza Books, 1954. P. 269.

All of Einstein's work was directed to bringing into closer and more satisfactory relation these two competing conceptions. It is well known that (in the face of his field-theoretic successes with gravitation) the older Einstein became convinced that the continuous-field conceptions were the more basic. He pursued these in the hope that he might discover some basis for recovering the known phenomena of quantum mechanics from within a theory that is not fundamentally statistical. In his early work he moved back and forth between the two conceptions, trying to discern, and extend, what is secure in each -- in his words, trying "to scent out the path which leads to fundamentals",[15] the better to develop a satisfactory new world-picture. We will explore some of the ways in which this search proved fruitful, and in particular, fruitful for the unification of formerly disparate elements of physical theory. It is well to begin, however, by acknowledging that while Einstein's dissatisfaction with the duality of fields and particles was a key inspiration in all of his great theoretical accomplishments, including relativity theory, he was not ultimately successful in resolving it. It is a happy thing that Einstein *was* dissatisfied with this duality, for that is what motivated his greatest work. By contrast, most quantum theorists by the 1920s counselled *simple acceptance* of this duality. Einstein's disdain for this quantum-mechanical attitude was no whim or sign of old-age conservativism, but sprung from considerations from his earliest work, considerations that had led to all his key successes.

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[15]From Einstein's "Autobiographical Notes", op. cit., p. 17.

5.3. Einstein as a Rationalist-Empiricist. We may appraise Einstein's quest for unity and his accomplishments at the level of fundamental theory in light of our continuum picture from Chapter 2. We have asked, are there methods for ensuring that all the ideas in physics are brought ultimately under the control of experience? We have seen that while this is an empiricist question, it has a rationalist answer. The rationalist seeks to comprehend disparate parts of physics together in a logically unitary way. And just in case this goal is adopted can problems in physical thought developing from the observational end inward ultimately condition ideas at the far metaphysical end of the continuum. The quest for consistency is not enough, since consistency can be won at any sufficiently theoretical interval on the continuum by mere complexification. And this would effectively protect from revision, under pressure from experience, ideas further toward the metaphysical end of the continuum.

Einstein helped show that in *physics* experience *can* condition ideas all the way to the far metaphysical end of the continuum. Einstein advanced physics by comprehending together disparate fundamental theories -- mechanics and electrodynamics, space-time theory and the theory of gravity -- in a logically unitary way. This drove change in scientific thinking all the way out to the far metaphysical end of the continuum. This marks an important difference between physics and, say, religion. Without the special contribution of unification-seeking theoreticians like Einstein, there would not be this difference. Einstein said: "to punish me for my contempt of authority, Fate made me an authority myself".[16] There is, I believe, a note of concern in this remark. For Einstein believed that all our science, his own theories included, "as measured against reality, is primitive and childlike".[17] In warning against authority, including his own, Einstein was perhaps concerned that the aforementioned difference between physics and religion is only tenuously secure.

I believe that there is an obvious *epistemic* rationale for desiring that all the ideas in physics ultimately be brought under the control of experience. So I believe that there is an *epistemic* (not merely pragmatic) rationale for the quest for theoretical unification. Further, I believe that in theoretical physics explanation and theoretical unification go hand-in-hand,[18] so that there is an *epistemic* rationale in theoretical physics for the quest for explanation. Einstein also believed that there is an epistemic rationale for the quest for unification and explanation.

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[16]Quoted on p. 124 in A. P. French (ed.), *Einstein: A Centenary Volume*, Cambridge, Mass.: Harvard University Press, 1979.

[17]Quoted in French (ed.), op. cit., p. 66.

[18]See Michael Friedman, "Explanation and Scientific Understanding", *Journal of Philosophy* (1974) *71*: 5-19; Philip Kitcher, "Explanation, Conjunction, and Unification", *Journal of Philosophy* (1976) *73*: 207-212.

Now radical empiricists do not believe that there is an epistemic rationale for the quest for explanation. Since Einstein is a hero of radical empiricists, my suggestion is perhaps surprising that Einstein's greatest worth was in his pursuing rationalist goals for knowledge. But in fact, in the task of comprehending disparate parts of physics together in a logically unitary way, radical empiricism and radical rationalism work hand-in-hand. The disparate parts of physics that are at first not unified, are unifiable, and because of this must be held apart by something otiose in their respective conceptual bases. The task of unification is therefore advanced by identifying otiose content in the respective conceptual bases, and this is a task that radical empiricists zealously perform. Thus one may find similar arguments and conclusions in the writings of Mach and Leibniz on space and time. Leibniz's proposed refinement of the concepts of space and time was motivated by his rationalism, and Mach's by his empiricism. But these thinkers both wanted to have the barest conceptions of space and time that can ground the known facts, and the arguments and methods of Leibniz and Mach are on some points strikingly similar.

This, I believe, explains why in seeking to unify mechanics and electrodynamics Einstein's work was for a while slung between positivism and the quest for explanation. It is a complicated and confusing business deciding what epistemological position best illuminates Einstein's early work. I believe that the early period was epistemologically somewhat confusing for Einstein too. As evidence for this I shall use the fact that the word "relativity" attaches to Einstein's theory. This label, I shall argue (in section 5.8), Einstein felt was apt only given a formulation of his doctrine that he could not understand to be explanatory. Such was Einstein's own formulation of 1905: it was presented as thoroughly positivistic, and Einstein as late as 1907 explicitly stated that he found he could not imagine a world that could conform to it. But the positivism and the unintelligibility of his early formulation came in Einstein's own eyes to seem defects of his formulation, defects that could be removed. The turning point, I shall argue, was reached in 1908, when Hermann Minkowski provided his geometrical reformulation of Einstein's theory. From that point the name "relativity theory" seemed to Einstein no longer apt. Einstein, in 1909, referred to his theory as the "so-called relativity theory", and let it be known that he would have preferred to have called it exactly the opposite: "*Invariantentheorie*". I shall argue that this was a turning point for Einstein's understanding of his doctrine *as* a theory, for his understanding of that theory, and for his epistemological position.

First, however, I wish to examine Einstein's tacit method for "finding the path that leads to fundamentals". For this was none other than Newton's method of making deductions from phenomena.[19] In section 5.5 I examine how

[19]This has been argued before, most notably by Jon Dorling, to whose article "Einstein's Methodology of Discovery was Newtonian Deduction from the Phenomena" (1987, privately published) I am much indebted in this chapter.

Einstein, in order to deduce the kinematics of special relativity, selected a "safe" set of phenomena from the observational consequences of a theory he knew was faulty.[20] Einstein then adapted certain high-level theoretical constraints to accommodate the demand that symmetries inherent in the phenomena must be reflected in theoretical principles, and on this basis proceeded to the deduction of the kinematics of special relativity. In section 5.6 I discuss Dorling's characterization of this deduction. (In an appendix to this dissertation, Appendix 2, I present the deduction in a geometrical way made possible by Minkowski's 1908 geometrical reformulation of Einstein's work, a way that is key to properly colligating and rendering intelligible the novel phenomena Einstein's 1905 phenomena drew to our attention. I make reference to Appendix 2 several times in this chapter.) In section 5.7 we will examine the heuristic and unificatory virtues of special relativity theory.

5.4. Why "On the Electrodynamics of Moving Bodies"? The title of Einstein's 1905 paper, "On the Electrodynamics of Moving Bodies", has seemed mysterious to many commentators. With this title Einstein made no claim that he was propounding a *theory*; he made no mention of *relativity*; and he drew attention in

[20]Einstein then deduced a novel theory from these "old" facts. This was possible since he employed non-classical theoretical constraints -- constraints adapted to his own novel philosophical preconceptions. By contrast, in his early work on quantum theory Einstein employed "new" facts (the established experimental evidence against classical theory) and a "safe" subset of old theoretical constraints. See Dorling's "Einstein's Methodology", op. cit., for more on this point, and for an account of the latter deduction.

a way whose significance has escaped most commentators, on the one hand to electrodynamics -- a concern with fields -- and on the other to bodies. It will prove relevant to understanding Einstein's methods to look first at why none of these features of the title "On the Electrodynamics of Moving Bodies" is really surprising.

It was widely recognised by Einstein's day that, Maxwell's own mechanistic predilections notwithstanding, no physics adequate to the facts embraced in Maxwell's Equations could be based solely on the mechanistic set of conceptions. Einstein, however, could not follow Lorentz, the thrust of whose programme was to abandon the first set of conceptions for the second (the Hertz-Lorentz "electromagnetic world picture") for two reasons at least. First, from very early days relativity considerations were for Einstein an important mark against the Lorentz programme, which assumed a stationary ether. Second, and even more importantly, by 1905 Einstein realized that the facts embraced in *Planck's Law* could not be accommodated to Maxwell's Equations and so completely undermined the Lorentz programme. Thus in a letter to Max von Laue of 17 January 1952 Einstein recollects his early discontent with "Maxwell's Theory":

> When one goes through your collection of verifications of the Special Relativity Theory, one gets the impression Maxwell's Theory may be unchallengeable. But already in 1905 I knew with certainty that it [Maxwell's Theory] leads to wrong fluctuations in radiation pressure, and hence to an incorrect Brownian movement of a mirror [suspended in] a Planckian radiation cavity. In my opinion one can't get around ascribing to radiation an objective atomistic structure, which of course does not fit into the framework of Maxwell's Theory. Naturally, it is comforting that the

> Special Relativity Theory in essence rests only upon the constant c, and not on a presupposition of the reality and fundamental character of Maxwell's fields. But unhappily the 50 years which have elapsed since then have not brought us closer to an understanding of the atomistic structure of radiation. On the contrary!

So already in 1905 Einstein believed that Maxwellian field theory, while it had a commanding grasp on the facts, was strictly false, and needed to be bought into better relation with the forms of thinking in physics that emphasized point particles. Because of the rich complex of veritable facts embraced in Maxwell's equations, false though Maxwell's equations may be, Einstein believed that there is no way forward in physics that does not heed Maxwell's Theory as a clue. We will come presentely to what, for Einstein, the clue was, and how he heeded it. (Initially Einstein took this clue to be a large portion of electromagnetic theory, but before long he saw that only a simple and very general result -- he called it a "principle" or a "postulate" -- sufficed for the deduction of a new doctrine. We see it mentioned in the above quote: it is the principle of the constancy of the light-velocity, *c*, a principle Einstein called his "light-postulate".)

First let us remark that Einstein's 1905 relativity papers afforded tantalizing suggestions about the connectedness of the phenomena of fields and particles. "It is remarkable that the energy and frequency of a light complex vary with the state of motion of the observer in accordance with the same law," Einstein remarks after some calculations in "On the Electrodynamics of Moving

Bodies'. This was support for his light-quantum hypothesis, by then already announc 1, in that it underlined the fundamental significance of the relation $E = h\nu$. After discovering, late in 1905, that Newtonian dynamical principles reinterpreted in the light of the kinematics of special relativity imply $E = mc^2$, Einstein wrote:

> *If a body gives off the energy L in the form of radiation*, its mass diminishes by L/c^2. The fact that the energy withdrawn from the body becomes energy of radiation evidently makes no difference, so that we are led to the more general conclusion that ... [t]he mass of a body is a measure of its energy-content ... [and] radiation conveys inertia between the emitting and absorbing bodies.

Einstein clearly has some basis in this result for believing connected the phenomena of fields and particles.

These were general reasons for highlighting "electrodynamics" and "bodies" in the title of his paper. We should acknowledge, moreover, that Einstein had very good reasons not to call his contribution a "theory", for he was as yet far from understanding the physical meaning of the relativistic kinematics he set forth. It was one thing to have, from that kinematics, the tantalizing suggestions of a connection between the phenomena of fields and particles, and quite another to have a satisfactory, systematic conception of the world that makes sense of the new kinematics. In 1907 Einstein candidly admitted that "for the time oeing we do not possess a world picture corresponding to the relativity principle".[21] For this reason Einstein did not offer "On the Electrodynamics of

[21]From "Über die vom Relativitätsprinzip geforderte Trägheit der Energie",

Moving Bodies" as a *theory*. Einstein in all his early papers was very careful to restrict his use of the term 'theory'. He avoided it wherever what *most* impressed him was the incompleteness of knowledge. Here are the titles of his earliest papers: "Consequences of Capillarity Phenomena" -- no mention of theory. "Concerning the Thermodynamics of Potential Difference between Metals and Fully Dissociated Solutions of their Salts, and Concerning a New Method to Investigate Molecular Forces" -- no mention of theory. "Kinetic Theory of Thermal Equilibrium and the Second Law of Thermodynamics" -- mentions a theory of presumably very general scope and defends its adequacy in a specific domain. "A Theory of the Foundations of Thermodynamics" -- Einstein is really onto something. "On Molecular Theory of Heat" -- ditto. In 1905 Einstein proposed no "light-quantum *theory*". Rather he titled that paper "On a Heuristic Point of View ...".

This well-founded reticence to use the word "theory" too liberally recalls to mind the expression "so-called relativity theory" by which Einstein referred to the special relativity kinematics in 1909. In German his expression was "*die sogenannte 'Relativitätstheorie'*". In 1912 Einstein changed this to *die gegenwärtig als "Relativitätstheorie" bezeichnete Theorie*. In fact the German, "*die sogennante 'Relativitätstheorie'*", is neatly ambiguous between "the so-called 'relativity' theory"

Annalen der Physik 23: 371-372. Quoted in Holton, "Einstein's Scientific Program", op. cit., p. 55.

and "the so-called relativity 'theory'". Einstein had misgivings about both terms. We will examine in the next section Einstein's misgivings about applying the term 'relativity'. Why Einstein had misgivings about the term 'theory' can be understood by reference to his high standards for admitting a physical doctrine as a full-fledged "theory". In 1907 Einstein said that "On the Electrodynamics of Moving Bodies" had concerned "the unification of the Lorentzian theory with the relativity principle". Lorentz's ideas, in their day (that is, before Planck) stood up well as a nearly complete physical world picture; Einstein readily applied the term 'theory' to them. But precisely because he could provide no "world picture" corresponding to the relativity principle, Einstein withheld the label 'theory' from his 1905 work. He had no physical picture of the novel kinematics from his 1905 work. Moreover Einstein lacked an adequate framework for fathoming the connections his results manifested linking the phenomena of fields and particles. The 1912 quote leaves no doubt about his willingness to call the 1905 work a theory, although unease is still evident concerning the term 'relativity'. This indicates Einstein's assimilation of Minkowski's work -- and a change of mind (that I shall discuss further in section 5.6) about the realistic significance of the new kinematics.

5.5. The "light-postulate". Let us look next at what the clue was from Maxwell -- or rather, from "Lorentzian theory" -- from which Einstein deduced a new doctrine of space and time. Earman, Glymour and Rynasiewicz[22] have argued cogently that the principal paper on special relativity that Einstein published in 1905 drew on an earlier draft in which, in Einstein's words, the full "Maxwell-Hertz equations for empty space together with the Maxwellian expression for the electromagnetic energy of space" had served as the starting-point for his arguments.[23] From his work on light quanta Einstein knew that this starting point was not strictly valid; doubtless he was not satisfied with the unsound character of his argument. Then Einstein discovered that a very much simpler and more restricted result sufficed[24] for the derivation of his conclusions. This was the "light-postulate" -- the principle of the constancy of the speed of light.

The principle of the constancy of the speed of light is immediate if one accepts Maxwell's equations along with the relativity principle. But classical kinematical concepts make it impossible to do this. We know that Einstein saw

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[22]J. Earman, C. Glymour and R. Rynasiewicz, "On Writing the History of Special Relativity", *PSA 1982* (Philosophy of Science Association Proceedings), vol. 2, pp. 403-416.

[23]See Earman et al., "On Writing the History of Special Relativity", op. cit., pp. 411-12; the quotation is from the beginning of the second 1905 relativity paper, "Does the inertia of a body depend upon its energy-content?" in Einstein, et al., *The Principle of Relativity*, W. Perrett and G. B. Jeffrey (trans.), New York: Dover, 1952.

[24]On an assumption concerning isotropy that finds its rationale only in the developed theory of Minkowski space-time. In 1905 Einstein presented this assumption as a mere *convention*, and signalled the possibility of alternative conventions according to which the speed of propagation of light is not isotropic.

his way to a satisfactory accommodation of Maxwell's equations with the relativity principle by realizing that in the classical axiomatic expression of Maxwell's theory, time is a suspect concept. We may reconstruct his thinking as follows:

If a light flashes at the point of coincidence as A and B, which are in relative motion, spatially coincide, then *if* the principle of relativity holds and Maxwell's Equations are assumed also to hold then it seems that we must consider not one but two expanding light shells, one "seen" by A spherically around A, the other "seen" by B spherically around B. This seems to be a contradiction, since only one light flash has occurred. If, however, the light-sphere around A is considered simply as a particular spatially spherically symmetric electromagnetic pattern, or set of electromagnetic "point events" simultaneous for A, and the light sphere around B is considered in like fashion, then we have a way of removing the seeming contradiction, or physical disparity in the electromagnetic field. We can choose to deny not the coherence of the theory of electromagnetic fields, but rather the supposition that the point events spherically about A, because "simultaneous for A", are also "simultaneous for B".

From the point of coming to some such realization about the concept of time, Einstein hastily modified the concept of time in the way necessary to remove the seeming contradiction. Unfortunately, he expressed the modified concept in terms of his well-known light-signals operational definition of simultaneity, showing that from this definition the desired relativity of simultaneity follows. However, his derivation of the Lorentz transformations, later in the paper, does not depend on his operational definition of simultaneity.[25] The latter derivation proceeds directly from the light postulate, the principle of relativity, and various innocuous high-level theoretical constraints such as that appropriate coordinate transformations be linear.[26] Looking back to the draft paper in which, tendentiously, the truth of Maxwell's equations had been assumed, Einstein now saw that only the light postulate was needed as a phenomenal basis for the deduction of the novel kinematics. From the draft paper he already had to hand numerous purely kinematical solutions to a variety of outstanding difficulties concerning the electrodynamics of moving bodies. Accordingly, these took their place in the final draft, which he was able to complete, after discovering the key fact about simultaneity, in a mere five weeks.

5.6. Einstein's Derivation of the Kinematics of Special Relativity as a Newtonian Deduction from Phenomena. Einstein's derivation of the kinematics of special relativity from the light postulate, the principle of relativity, and sundry other innocuous high-level theoretical assumptions can be made perhaps the most pedagogically perspicuous deduction from phenomena of any that have played

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[25]This is pointed out by Dorling in "Einstein's Methodology", op. cit., p. 7n.

[26]This is pointed out by Dorling in "Einstein's Methodology", op. cit., p. 5.

an important role in the history of science. Dorling does not go into the details of this deduction, but simply writes:

> That the structure of this argument can be made formally rigorous, no competent authority has denied. To recognize it as formally a Newtonian-style deduction from phenomena, it is only necessary to recognize that it is the Lorentz transformations which must explain the constancy of the velocity of light and not vice-versa; as is clear from the fact that the Lorentz transformations are generalizations over all velocities (and also over all length and time intervals) as well as over all processes which propagate with the particular velocity *c*, and not just over one such process. ...So Einstein really has derived an explanans from one of its own explananda in the classical Newtonian manner. Contrary to popular belief Einstein's own operational definition played a merely heuristic and not a logical role in this argument.[27]

Dorling also notes, however, a point against calling special relativity theory *explanatory* until after Minkowski's geometrical reformulation of it as an *invariance* doctrine. We shall discuss this point later.

In Appendix 2 I show how easily Einstein's deduction can be made the basis for *teaching* what is called the special theory of relativity. The distressing fact is that special relativity theory is most often taught in terms that involve Einstein's superfluous operational definition of simultaneity, terms that are cumbersome and inperspicuous and that completely fail to convey the geometrical form that Minkowski in 1908 provided for the theory. In the standard terms the name "relativity theory" seems to make perfect sense, and people are led to

[27]Dorling, "Einstein's Methodology", op. cit., pp. 5-7.

the false belief that Einstein, like the later quantum theorists, "brought the obersever into the very laws of physics".[28]

5.7. Heuristic and Unificatory Strengths of Relativity Theory. Einstein's heuristic was unity-seeking; it was premissed on the assumptions that high-level theoretical physics should (1) give a coherent, unified, harmonious, simple, organically compact world-picture, and (2) aim (for this reason) to replace any theory that does not explain symmetries in phenomena as the manifestations of deeper symmetries, by a theory that manages to do this.[29] This heuristic is illustrated in the initial sentences of "On the Electrodynamics of Moving Bodies". Einstein complains

> that Maxwell's electrodynamics -- as usually understood at the present time -- when applied to moving bodies, leads to asymmetries which do not appear to be inherent in the phenomena. Take, for example, the reciprocal electrodynamic action of a magnet and a conductor. The observable phenomenon here depends only on the relative motion of the conductor and the magnet, whereas the customary view draws a sharp distinction between the two cases in which either the one or the other of these bodies is in motion. For if the magnet is in motion and the conductor at rest, there arises in the neighbourhood of the magnet an electric field with a certain definite energy, producing a current at the places where the parts of the conductor are situated. But if the magnet is stationary and the conductor in motion, no electric field arises in the neighbourhood of the magnet. In the conductor, however, we find an electromotive force, to

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[28]I have made a pretty thorough critique of this view in my unpublished paper "Einstein's Opposition to Indeterminacy".

[29]This summary of Einstein's "positive heuristic" comes from Elie Zahar's Lakatosian discussion, "Why did Einstein's Programme supersede Lorentz's?", *British Journal for the Philosophy of Science* (1973) *24*: 95-123, 223-262. See especially pp. 224-225.

> which in itself there is no corresponding energy, but which gives rise -- assuming equality of relative motion in the two cases discussed -- to electric currents of the same path and intensity as those produced by the electric forces in the former case.[30]

This complaint against classical electrodynamics also involves a suggestion how electric and magnetic forces may be conceptually unified, that is, conceived of as manifestations of one underlying force of electromagnetism. Consider an electron that is constrained (within a wire, say) to move uniformly within an electromagnetic field. What force due to the field acts on the electron? If we can transform the field to a system of coordinates in which the electron is at rest, we may ascertain the force on the electron due to the field by attending simply to the calculated value of the *electric* force due to the field. Then by our invariance (or "relativity") principle, we know that in any other system of coordinates the field acts on the electron with this very same force. We are spared the need to resolve the force on the electron due to the field into separate components, one electric in origin, one velocity-dependent and magnetic in origin. Thus, once Einstein has deduced the the Lorentz transformations from his relativity principle and light-postulate, he applies these transformations to the Maxwell-Hertz equations for empty space in order to study the nature of the electromotive forces occurring in a magnetic field during motion. He finds that he is able to replace the first of the following two modes for conceiving these

[30]"On the Electrodynamics of Moving Bodies", in Einstein, et al., *The Principle of Relativity*, op. cit., p. 37.

forces with the second, and that this amounts to the complete conceptual unification of electric with magnetic forces:

> 1. If a unit of electric point charge is in motion in an electromagnetic field, there acts upon it, in addition to the electric force, an "electromotive force" which, if we neglect the terms multiplied by the second and higher powers of v/c, is equal to the vector-product of the velocity of the charge and the magnetic force, divided by the velocity of light. (Old manner of expression.)
>
> 2. If a unit electric point charge is in motion in an electromagnetic field, the force acting upon it is equal to the electric force which is present at the locality of the charge, and which we ascertain by transformation of the field to a system of coordinates at rest relatively to the electrical charge. (New manner of expression.)
>
> The analogy holds with "magnetomotive forces." We see that electromotive force plays in the developed theory merely the part of an auxiliary concept, which owes its introduction to the circumstance that electric and magnetic forces do not exist independently of the state of motion of the system of coordinates.
>
> Furthermore it is clear that the asymmetry mentioned in the introduction as arising when we consider the currents produced by the relative motion of a magnet and a conductor, now disappears.[31]

Minkowski deepens the analysis by considering the action on the field of a charged particle in any kind of motion. This was the subject of the laws concerning retarded potentials set down by A. Liénard and E. Wiechert. Minkowski shows that the basis for these laws is fundamentally geometrical -- that is, by reference to the geometry of Minkowski space-time one understands them immediately. In Minkowski's demonstration of this, the fundamental unity of the electric and magnetic forces again is made clear: "in the description of the field

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[31]"On the Electrodynamics of Moving Bodies", in Einstein, et al., *The Principle of Relativity*, op. cit. Pp. 54-55.

produced by the electron we see that the separation of the field into electric and magnetic force is a relative one with regard to the underlying time axis".[32]

We have already examined the unifications of space and time and of kinematics and geometry that were consequent upon the advent of special relativity theory. The new theory showed that time is measurable in centimetres, and showed how it takes its place along side the three dimensions of space in a unified four-dimensional semi-Euclidean space-time structure. The new theory also dissolved the chief tensions between electrodynamics and mechanics. For it showed that the failing programme for a mechanical reduction of electrodynamics is badly misconceived. It also gave new direction and impetus to the reverse programme, of understanding mechanics through electrodynamics. By deriving the relation $E = mc^2$, Einstein connected the electromagnetic field with the very stuff of particulate matter. That there is this connection Einstein further underlined by showing the basis in relativity kinematics for the fundamental quantum-theoretic relation, $E = h$. Einstein noted in 1905 that the relation $E = mc^2$ could potentially help dissolve the mystery of the enormous energy-yields of nuclear processes. Einstein was also first led by the relation $E = mc^2$ to his key initial insights concerning the relation between gravity and

[32]From "Space and Time", in Einstein, et al., *The Principle of Relativity*, op. cit. P. 89.

inertia.[33] According to Newtonian physics, vertical acceleration due to gravity is independent of horizontal velocity. But Einstein had established that inertial mass depends on velocity. This apparently implies that, for Einstein, the acceleration of a falling body is, contrary to Newton, dependent on its horizontal velocity (and also on its internal energy). Einstein was not prepared to accept this conclusion. Einstein thought firmly established the principle that the acceleration due to gravity depends only on the gravitational field and in no way on the masses, velocities, or internal-energy states of the bodies accelerated. So rather than accept the conclusion that the acceleration of a falling body is dependent on its horizontal velocity and also on its internal energy, Einstein identified gravitational with inertial mass. He colligated the phenomena that support this conception under the rubric of the "principle of equivalence". In this way, Einstein was led to investigate gravitation as an inhomogeneous *inertial* field.

Einstein in any case knew that the classical, Newtonian gravitational theory is not compatible with the kinematics of special relativity. Some accommodation would have to be found that corrected the classical gravitational theory. (Minkowski showed in 1908 that were gravitational theory accommodated to the new kinematics, the corrected theory would still be closely conformable to the

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[33]See Einstein's "Notes on the Origin of the General Theory of Relativity", *Ideas and Opinions*, op. cit. Pp. 285-290.

phenomena from which Newton had deduced his law of gravitation -- Kepler's laws.) It is noteworthy that Einstein maintained as his heuristic for determining the form of the new gravitational theory exactly the unity-seeking heuristic that had guided him to the theory of special relativity. In the new work on gravitation, the suggestive symmetry was the principle of equivalence, which again classical theory failed to explain as a manifestation of deeper symmetries. As is well-known, Einstein followed this clue through to the successful treatment of gravitation as an inhomogeneous inertial field conditioned by the distribution of matter-energy.

The advent of what is called general relativity theory unmistakably amounted to a "progressive problem shift". It is well-known that the theory made novel predictions that were empirically confirmed. (Zahar argues that even the prediction of the correct perihelion motion of Mercury was a confirmed "novel" prediction, though the motion had been discovered long antecedently to Einstein's discovery of his theory; certainly the successful prediction of the well-known eclipse-results of 1919 made Einstein's programme in Lakatos's sense empirically progressive.) Still more important, however, in Einstein's view, than the empirically progressive problem-shift, were features of the new theory that suggested that a potential existed for further conceptual unification. According to Einstein an important part of the warrant for his theory consisted in the promise it held for further progress, as judged by the lights of his well-tested and well-proven unity-seeking heuristic. General relativity theory subsumed gravitation within a space-time structure conditioned in its form by the laws of electromagnetism. Thus it connected the forces of gravity and electromagnetism, albeit without yet comprehending them together as manifestations of a single underlying structure. Rather the new theory begged Einstein to undertake such further unificatory work. The promise seemed very real that the project could succeed (where electromagnetic field theory had not succeeded) in fully embracing the facts of mechanics and the particulate nature of matter and radiation. By working to comprehend gravity with electromagnetism, Einstein hoped that he could complete his grand task of dissolving the deep-going opposition in physics between field and particle pictures. Thus the title of a 1919 paper by Einstein: "Do Gravitational Fields Play an Essential Role in the Structure of the Elementary Particles of Matter?". Einstein's idea was the old one, from Lorentz, of reducing the very stuff of ponderable matter to wrinkles in a field. Gravity was to help explain the stability of the wrinkles that are elementary particles. The theory that would embrace gravitation and electromagnetism together would concern a unified field. The stochastic-seeming phenomena from the inchoate quantum mechanics of the day Einstein thought might reflect a kind of "over-determination" within the unified field, just as the behaviour of a gear that is "overdetermined" within a system of gears appears stochastic. (An overdetermined gear is one that is mechanically driven both one way and the opposite way. Straightforward mechanical reasoning cannot predict what will "give" and thus which way the overdetermined gear will move.) Initially Einstein was supremely optimistic about this approach:

> Can one do justice to this [quantum mechanical] knowledge about natural processes, to which indeed we must ascribe general significance, in a theory based upon partial differential equations? Quite certainly: we need only "overdetermine" the field variables by the equations. That is, the number of differential equations must be greater than the number of field variables determined by them.[34]

But Einstein did not succeed with this approach. It is well-known that Einstein's general quest for a unified field theory did not succeed, and that to the end of his life Einstein remained unable to explain field-theoretically the phenomena embraced within the empirically progressive, developing theory of quantum mechanics. Toward the end of his life Einstein entertained serious doubts about the prospects for the eventual triumph of field theory:

> In present-day physics there is manifested a kind of battle between the particle-concept and the field-concept for leadership, which will probably not be decided for a long time. It is even doubtful if one of the two rivals finally will be able to maintain itself as a fundamental concept.[35]

> I consider it as entirely possible that physics cannot be based upon the field concept, that is on continuous structures. Then *nothing* will remain

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[34]From Einstein's "Does Field Theory Offer the Possibility for a Solution of the Quantum Problem"; quoted by J. Stachel in his "Einstein and the Quantum: Fifty Years of Struggle", in R. G. Colodney (ed.), *From Quarks to Quasars: Philosophical Problems of Modern Physics*, Pittsburgh: University of Pittsburgh Press, 1986. P. 379.

[35]Einstein to Herbert Kondo, August 11, 1952, Einstein Archives item 14-306; quoted by J. Stachel in his "Einstein and the Quantum", op. cit, p. 380.

> of my whole castle in the air including the theory of gravitation, but also nothing of the rest of contemporary physics.[36]
>
> I must confess that I was not able to find a way to explain the atomistic character of nature. My opinion is that if the objective description through the field as an elementary concept is not possible, then one has to find a possibility to avoid the continuum (together with space and time) altogether. But I have not the slightest idea what kind of elementary concepts could be used in such a theory.[37]

Einstein had found no principle akin to the light postulate or the principle of equivalence, to guide him in the development of a unified field theory. He altogether lacked a basis for a new deduction from phenomena. Instead he fell captive to one after another tantalizing formal suggestion of deeper-lying unity in nature, and conducted his investigations in the formal depths of a succession of only partially worked-out theories. In this way he searched along hundreds of fruitless paths, attempting to achieve, with a revelatory mathematical break-through, a way of turning the various tantalizing hints in his possession into a full-fledged physical theory. This was not the method of his early years, and he vainly wished for some phenomenal key, some notable symmetry not properly reflected in theory, to guide him in his later work. "One thing I have learnt in a

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[36] *Besso Correspondence*, p. 527, letter of August 10, 1954; quoted by J. Stachel, "Einstein and the Quantum", op. cit., p. 380.

[37] Einstein to Bohm, October 28, 1954, Einstein Archives item 8-050; quoted by J. Stachel in "Einstein and the Quantum", op. cit., p. 380.

long life," he wrote to Besso in 1950, "it is devilishly hard to get closer to 'Him', if one doesn't want to remain on the surface."[38]

5.8. The Error of Conventionalism about Space-Time Structure. That the interpretation and refinement of subtle, suggestive mathematical features of physical theory can be a confusing business is well illustrated by the interminable misunderstandings and misplaced efforts in philosophy of physics concerning the question whether space-time structure is *conventional*. We have seen that Einstein introduced a symmetry into physical theory (his special relativity principle) under the guidance of symmetries that appeared to be inherent in the phenomena. A consequence of the introduction of this theoretical symmetry was the doctrine of the "theoretical equivalence of all inertial frames". This seemed to be a weakening of theoretical space-time structure, say from one involving a rest frame for the ether (or equivalently, one involving the doctrine of absolute space), yet a weakening that did not diminish the empirical adequacy of physical theory. Likewise, Einstein's later apparent introduction of a *general* principle of relativity, ostensibly showing the theoretical equivalence of all frames, inertial or non-inertial, seemed a further such weakening. These impressions were quite mistaken. The space-time structure of special relativity theory is not "weaker" than classical space-time structures. It is simply very different from them, for

[38]*Besso Correspondence*, p. 439, letter of April 15, 1950; quoted by J. Stachel, "Einstein and the Quantum", op. cit., p. 382.

example in possessing a unique invariant finite velocity, c. (In the limit as one lets the invariant velocity c go to infinity, classical Galilean-relativistic space-time can be recovered from Minkowski space-time; classical Galilean-relativistic space-time *is* weaker than the classical Newtonian space-time structure involving absolute space, but is not a space-time structure within which Maxwell's equations can hold.) Likewise, the advance from special to general relativity theory involved no "weakening" of theoretical structure. On the contrary, special relativity theory describes the degenerate, homogeneous Minkowski space-time structure. General relativity theory describes space-times that are inhomogeneous and typically infinitely richer in structure than Minkowski space-time. And general relativity theory contains special relativity theory as a limiting case, in the limit as the regions considered become arbitrarily small, and also in the limit as the distribution of matter-energy becomes completely homogeneous. So general relativity theory gives up not any of the theoretical structure described by special relativity theory.

Nevertheless, the impression was that theoretical structure not needed from an empirical standpoint had been abandoned without loss. People agreed, in fact, that, far from there being losses, there had been gains, particularly in the epistemological perspecuity of physical theory. The example held some fascination. Hans Reichenbach was much worked upon by this example. He discovered in the operationalism of Einstein's 1905 paper an apparent basis for doing something similar all over again, still within the confines of special relativity. By varying the light-signals definition of simultaneity with which Einstein had (unnecessarily) prefaced the kinematical discussion in his paper, Reichenbach found (1) that he could define, roughly speaking, certain collections of "*non-standard* inertial frames", and (2) that he could emulate Einstein by showing that collections of these frames could be held "equivalent" without diminishing the empirical adequacy of physical theory. There was, however, an essential difference between Einstein's work and Reichenbach's. Einstein's work responded to symmetries inherent in the phenomena that had suggested the need for new symmetries in theory. Reichenbach's work proceeded, instead, from formal elements of a particular mathematical presentation of a theory, elements that suggested to Reichenbach (mistakenly) that there should be a way of generalizing the equivalences to which Einstein's work had drawn attention. At no point was Reichenbach animated by empirical considerations. On the contrary, he emphasized again and again that none of his manipulations made any difference so far as experience is concerned.

Reichenbach's work on non-standard simultaneity accomplished absolutely nothing. People who think that Reichenbach accomplished something are confused about what it is to formulate a space-time theory such as special relativity in coordinate-dependent terms. If Reichenbach showed anything he showed that one can impose additional structure (spatial anisotropy), and then remove it again (through an equivalence doctrine), all without sacrifice of empirical adequacy. This is not to say that Reichenbach showed even this much clearly. Reichenbach thought that he was displaying a broader equivalence than Einstein had displayed. Reichenbach actually found an ally in Einstein for this mistaken view. At that time Einstein was struggling to learn how to formulate a space-time theory in coordinate-independent terms. Although Einstein eventually sorted himself out tolerably well on the issues (see note 28, above, and citations) he never took the time to reevaluate his earlier encouragement of Reichenbach. Reichenbach's work was too unmotivated physically -- proceeding as it did on the basis of formal rather than phenomenal hints -- to hold Einstein's interest.

Supposedly Reichenbach helped to demonstrate an ineliminable element of conventionality within the theoretical structure of special relativity. Let us briefly identify the confusion in this, referring to anti-conventionalists A. A. Robb[39], David Malament[40], Michael Friedman[41] and John Winnie[42] for fuller discussions.

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[39]A. A. Robb, *A Theory of Time and Space*, Cambridge: University of Cambridge Press, 1914.

[40]D. Malament, "Causal Theories of Time and the Conventionality of Simultaneity", *Nous* 11: 293-300, 1977.

[41]M. Friedman, *Foundations of Space-Time Theories*, op. cit.

[42]J. Winnie, "The Causal Theory of Space-Time", in J. Earman, C. Glymour and J. Stachel, eds., *Minnesota Studies in Philosophy of Science*, vol. 8, Minneapolis: University of Minnesota Press, 1977; "Invariants and Objectivity: A Theory with Applications to Relativity and Geometry", in R. Colodney (ed.), *From*

In his coordinate-dependent presentation of special relativity theory, Einstein expressed the equivalence of inertial frames in terms of the Lorentz-covariance of standard coordinate-dependent expressions of physical laws. A particular class of coordinate-systems is picked out as special according to this way of thinking, namely, those adapted (according to Cartesian conventions, conventional choice of unit, and an assumption of spatial isotropy) to inertial frames. We may think of this class of coordinate-systems as a particular "distinguished" class of coordinate systems in the following sense. Consider a coordinatization to be a bijective map of space-time points to elements of $\mathbf{R}^4$. Then one can move from the one coordinatization of space-time to another by composing three maps: the inverse of the coordinatization map, an automorphism of the space-time structure, and the coordinatization map. Call "distinguished" the class of all coordinatizations that can be reached in this way from one particular coordinatization. It can be shown[43] that the distinguished classes of coordinatizations partition the most general class of space-time coordinatizations. Moreover, it can be shown[44] that for any distinguished class of coordinate systems, the symmetries of the group of transformations relating coordinate systems in the distinguished class are just the symmetries of the space-time structure. This

Quarks to Quasars, op. cit.

[43]See J. Winnie, "Invariants and Objectivity", op. cit., p. 101.

[44]J. Winnie, "Invariants and Objectivity", op. cit.; Corollary 2.26, p. 102.

latter point makes clear what it is to formulate a space-time theory successfully in coordinate-dependent fashion: it is to capture, in the transformations between coordinatizations in some restricted class of coordinatizations ruled admissible, all the intrinsic space-time symmetries and no other symmetries. And the former point shows that the class of coordinatizations ruled admissible may not be *broadened* without this changing what is implied about intrinsic structure. Without changing what is implied about intrinsic structure, the class of coordinatizations ruled admissible, say the standard coordinatizations, may simply be *exchanged* for another distinguished class, say some non-standard distinguished class of coordinatizations that is generated by the intrinsic space-time symmetries from some one non-standard coordinatization. (A. Ungar[45] in effect keeps the standard and the various non-standard classes of coordinatizations thus separate.) Clearly, however, simultaneity-conventionalists of Reichenbach's ilk have typically had in mind to effect not such an *exchange*, but a *broadening* of the class of admissible coordinatizations. Simultaneity-conventionalists have proposed the inclusion in one big class of admissible coordinatizations all the standard coordinatizations and all the non-standard coordinatizations consequent upon choosing a direction *d* of maximal spatial anisotropy and choosing a value $\epsilon \neq ½$, $0 < \epsilon < 1$ that establishes the degree anisotropy in that direction (standard simul-

[45]A. Ungar, "The Lorentz Transformation Group of the Special Theory of Relativity without Einstein's Isotropy Convention", *Philosophy of Science* (1986) *53:* 395-402.

taneity, involving no anisotropy, corresponds to $\epsilon = \frac{1}{2}$). (In effect, the proposal has been to take the union of all the classes of coordinatizations that Ungar keeps separate.) Let us call a formulation of special relativity theory that accomplishes this an 'ϵ-formulation'.

By the former point from Winnie, no ϵ-formulation can agree with the standard formulation about intrinsic structure. If special relativity theory really is ϵ-formulable then the standard formulation in some way involves otiose structure. Now, in 70 years of discussion, no proponent of ϵ-conventionalism has ever managed to say what the otiose structure in the standard formulation is supposed to be. Having taken the unmotivated step of imagining d's and ϵ's into the formal apparatus of the theory, their chief concern is to show, by formal manipulation and doctrines of equivalence, that theirs is *not* a different theory from standardly formulated special relativity theory, at least so far as empirical consequences are concerned. The discussions are heavily laden with ill-chosen terminology and subtle confusions, and it is made to seem that the doctrines of equivalence get rid of more structure than was first artificially added in, thus "improving" on the standard formulation of the theory. It is completely unclear, however, whether ϵ-conventionalists have ever coherently imputed some different intrinsic structure to space-time than is imputed by Einstein or Minkowski. Neither Friedman nor Winnie believes that ϵ-conventionalism is coherent. In their view the pursuit of ϵ-formulations of special relativity theory cannot succeed. They base their conviction in part on a result by Malament, that helped refute a claim which ϵ-conventionalists have traditionally deemed important to their programme. Malament reproduced Robb's result that standard simultaneity is definable from the "topological" primitives of special relativity theory that ϵ-conventionalists had traditionally accepted as objective. Malament then proceeded to show that without the artificial imposition of unmotivated additional structure (such as the ϵ-conventionalists' *d*'s and ϵ's), standard simultaneity is, on certain very weak "natural" constraints, the *only* simultaneity relation thus definable.

Not even Malament's proof has stopped the steady proliferation of ϵ-conventionalist tracts. Friedman and Winnie have attempted to strengthen the case that ϵ-conventionalism is incoherent. Friedman introduces in his general discussion of methodology an independent basis for rejecting conventionalism. Winnie pursues the idea presented above of what it is to formulate a space-time theory successfully in coordinate-dependent fashion, and in terms of this idea attempts[46] to deepen the significance of the Malament result. The significant point for our purposes is that the ϵ-conventionalism debate arises, in Einstein's terms, from a "devilish" confusion consequent upon not "remaining on the surface" -- from attempting not on the basis of phenomenal or physical reflec-

[46]At pp. 142-158 in Winnie's "Invariants and Objectivity", op. cit.

tions, but on the basis only of suggestive formal considerations, to improve the presentation of physical theory and the characterization of physical concepts.

5.9. Einstein's Laws. If the principles that Einstein set forth in his space-time theories are not conventions, we should consider whether they may be objective laws, or at least some among the leading candidates for such a status that empirical science has yet set before us. From our discussion so far we certainly can see that space-time principles simply do not fit the description of laws that Cartwright urges upon us in her simulacrum account of theoretical explanation. Einstein's laws do not describe the behaviour of objects in idealized models, so much as they describe the possible space-time *form* of *any* physical system, whether idealized, or modelled, or real. Models had nothing to do with Einstein's inference to the kinematics of special relativity, and it was (as I argue in Appendix 2) in fact initially impossible for Einstein to conceive any kind of models of physical systems whose space-time form was as his theory required. Moreover, the burden of Einstein's space-time theory is not *ontic* but *structural*, so that when we ask in what the objectivity or physical significance consists of the laws that are the core content of this theory, we find that we must answer in the positivist, empiricist spirit of relationalism: the structure that the laws specify is not that of an existent thing, space-time, but of experience, as colligated by that richly over-connected, bootstrap confirmed, synthetic body of doctrine, general physics. The mistake of thinking ontic the commitments of space-time theory has been underlined by recent arguments (deriving from an argument by Einstein) against "space-time substantivalism".[47] The arguments demonstrate that it is incompatible with the possibility of causal determinism to hold that the points in space-time possess any individuality outstripping their place within the metric structure defined over them. Thus space-time is not an entity (the manifold) *carrying* a certain structure, but rather *is* the structure we conceive the manifold as carrying. The question in what the objectivity or physical significance consists of the structure laid before us by space-time theory must not be answered in an ontological vein (as does Michael Friedman, whose 1983 book *Foundations of Space-Time Theories*[48] adopts a clearly-stated realist stance about the manifold, a stance which helped to sharpen the subsequent critical discussion of this position). The answer rather must concern the structure's importance within a corpus of physical theory that engages the empirical evidence in all the ways (including the bootstrap way) that we studied in Chapters 3 and 4 and in the present chapter. The objectivity or physical significance of space-time structure is just that we cannot lay this structure aside or even amend it and still retain what systematic knowledge we have of nature's workings.

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[47]See John Earman and John Norton, "What Price Spacetime Substantivalism? The Hole Story", in *British Journal for the Philosophy of Science* (1987) *38*: 515-525; John Norton, "Einstein, the Hole Argument and the Reality of Space", in J. Forge (ed.), *Measurement, Realism and Objectivity*, Dordrecht: D. Reidel, 1987, pp. 153-188.

[48]Op. cit.

The philosophical doctrine of space-time that I am advancing here is conformable with certain core doctrines of *relationalism*, but it is not the position of any historical relationalist. While in the sense just given relationalism is in my view the right philosophical doctrine of space-time, those relationalists who hold that *conventionalism* about space-time structure is a consequence of their position, are wrong. They are in error (in particular) about the underdetermination thesis underlying their conventionalist view; scientific experience shows again and again that this thesis is (on any interesting reading) just plain false. Those relationalists are wrong who think that, in some way supposedly consequent on the truth of relationalism, the doctrine of general relativity of motion can be maintained. This leaves no historical relationalist in the right. Nevertheless, relationalists are correct to hold that an *empiricist*, not a traditional *realist* answer is required to the question in what the objectivity or physical significance consists of the structure that is called to our attention by our leading space-time theory. A realist answer will not do, at any rate, if "realism" demands that the commitments consequent upon accepting a theory are always at least partly ontological. The answer I have just given (an answer I shall call *structuralist* rather than *relationalist*, because of the historical confusions in the relationalist camp) actually *is* realist in another sense, since it is an account of the objectivity of space-time principles, and argues (against a consequence of the underdetermination thesis) that space-time principles may carry the value "true". Not only the failure of "space-time substantivalism" makes empiricistic relationalism the most reasonable philosophy of space-time physics. The improvement of our conception of empiricist methodology, and the realization that method itself is empirically based, augment the credentials of empiricism, and eliminate its association with such dubious doctrines as underdetermination, conventionalism, and relativism.

From the vantage point of such empiricist structuralism, Einstein's space-time principles or laws are best conceived of in precisely the "deductivist" terms of the positivists. Einstein's work on relativity theory was thoroughly "deductivist" both in intent and execution. That is to say, Einstein sought axioms from which to derive by deductive logic conclusions of the form that if such-and-such physical conditions obtain, then such-and-such other physical conditions must also obtain; he heeded clues that could be stated as simple principles, and that led by deductive steps to the axioms that he wanted; and he appraised the physical significance of his theory in terms of the logical conditions it placed on the form of physical concepts within a broader framework of theory. The example gave pause to all those interested in the nature of a scientific theory. It was no leap of fancy for the positivists to move from this example to the conclusion that physical theories are like deductive mathematical systems, except that conclusions follow from physical theories but not from purely mathematical theories concerning observable, physical situations. It is no surprise that the positivists could move from this example to the conception that physical theories, like mathematical theories, are most perspicuously conceived of as logically articulated sets of sentences.

I intend to argue that space-time principles are the clearest example known to us of laws of nature. If that is so then Cartwright's position is refuted. But my discussion why space-time principles are definitely laws of nature really requires the resources of Chapter 6, in which I adopt the empiricist conception of laws of J. S. Mill, F. P. Ramsey and D. Lewis that recently has been defended by John Earman.[49] Given that conception, it is automatic that space-time principles are among the laws of nature; they are not to be ascribed some different status -- say, the status of conventions -- on account of the fact that they strongly condition what other general principles may be regarded as laws. For on the empiricist account of laws that Earman defends, precisely such conditioning of what other general principles may be regarded as laws is to be expected of the highest-level theoretical laws. Thus we may examine what may be meant by calling a proposed structure for space-time objective or "true" knowing that, if the empiricist conception of laws from Chapter 6 is accepted, this illustrates a definite sense in which we may mean something by calling a law objective or "true". The sense I have offered in this section is *empiricist*, but (in denying underdetermination) *realist* (though not in any sense that involves ontic

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[49]J. Earman, *A Primer on Determinism*, Dordrecht: D. Reidel, 1986, chapter 5.

commitments). When in Chapter 6 I defend against Cartwright the "facticity" view of theoretical laws, this is the conception of the laws' truth or facticity that I have in mind.

5.10. **Principle and Constructive Theories.** Einstein himself knew better than to generalize about all science from the example of his space-time theory. Not all physical theorizing is as formal, precise, or sure as that surrounding, say, special relativity. It is a mistake to draw too general a lesson about the nature of scientific theorizing from this example. In 1919 Einstein explained this in terms of his distinction between "constructive" and "principle" theories:[50]

> We can distinguish various kinds of theories in physics. Most of them are constructive. They attempt to build up a picture of the more complex phenomena out of the materials of a relatively simple formal scheme from which they start out. Thus the kinetic theory of gases seeks to reduce mechanical, thermal, and diffusion processes to movements of molecules -- i.e., to build them up out of the hypothesis of molecular motion. When we say that we have succeeded in understanding a group of natural processes, we invariably mean that a constructive theory has been found which covers the processes in question.
>
> Along with this most important class of theories there exists a second, which I will call "principle-theories." These employ the analytic, not the synthetic method. The elements which form their basis and starting-point are not hypothetically constructed by empirically discovered ones, general characteristics of natural processes, principles that give rise to mathematically formulated criteria which the separate processes or the theoretical representations of them have to satisfy. Thus the science of thermodynamics seeks by analytical means to deduce necessary conditions, which separate events have to satisfy, from the universally experienced fact that perpetual motion is impossible.

[50]In "What is the Theory of Relativity", a 1919 newspaper article for *The London Times*; in *Ideas and Opinions*, op. cit., pp. 227-232. See p. 228.

> The advantages of the constructive theory are completeness, adaptability, and clearness, those of the principle theory are logical perfection and security of the foundations.
>
> The theory of relativity belongs to the latter class.

Einstein's description of constructive theories is sketchy. It can be enriched by incorporating "constructivist" elements from present-day philosophy of science. It is perfectly consonant with Einstein's intentions to do this. In 1919 Einstein remained receptive to a positivist reading of his special and general theories of relativity.[51] Yet he warned (see above) that most of the rest of physics is not best illuminated by the emerging positivist conception of theories. We know that philosophers of science in the last three decades have greatly fleshed out the grounds for thus resisting positivism. Yet we must also keep in mind the lesson from section 5.9, that precisely the positivist conceptions are needed for the illumination of some theories.

That positivism must be kept in the picture is made the more certain by the consideration that in the development of physical theories Einstein's distinction between principle and constructive theories proves unrobust. We have seen that many features of general relativity theory suggest that a further generalization of the theory should be possible, one that would give a unified theory of the gravitational and electromagnetic fields. Success in this endeavour would

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[51]See D. Howard's "Realism and Conventionalism in Einstein's Philosophy of Science: The Einstein-Schlick Correspondence", in *Philosophia Naturalis* (1984) *21*: 616-629.

make a principle into a constructive theory. Such a transformation would not surprise Einstein; it was exactly what he sought. Einstein's early result that $E = mc^2$ already implied that the stuff of the world and its energy (in whatever form) and hence all the forces that animate it are all somehow one. The implication seemed bizarre. What can it mean for energy, the animating principle of matter, to be somehow equivalent to matter itself? General relativity theory for the first time made this bizarre-seeming implication intelligible. Within the terms of general relativity theory, all forms of energy are equivalent, and are measured in terms of curvatures in space-time. Because the theory does not embrace non-gravitational forces, the equivalence of energy forms is mis-stated (I will not say overstated) by general relativity theory. But, bracketing this fault of the theory, we find in relativity theory a very startling suggestion about the ultimate nature of matter's animation. For relativity theory comprehends activity in structural terms only, not in the traditional terms of things with animating energy-principles being structured in certain ways. "Gravitational interaction" amounts to timeless space-time structure satisfying certain conditions of geometry. So with mass and energy both resolved in structural terms, in terms of curvatures in space-time, the equivalence of mass and energy becomes intelligible for the first time. The suggestion is very strong, in fact, that so far as gravity is concerned at least, the curvatures of space-time can be taken as all that matter is. In the special theory time is shown to be measurable in centimetres. Now in the general theory mass and energy become measurable in the same units as space and time; mass and energy are measurable in centimetres, in terms of the curvature of space-time. So far as gravity is concerned, the curvatures of space-time can be considered to be all that there is to the universe. Mass and energy are effectively measurable by reference to geometrical properties of the field. Thus a principle theory (general relativity theory, whose burden is wholly structural, and whose objectivity or physical significance is best understood in terms faithful to empiricism) seemed to Einstein poised to become (on further generalization) a theory of Reality, a theory of all that there is, and thus certainly a constructive theory in Einstein's sense.

5.11. **Conclusions.** Einstein showed that Newton's method still works despite the demise of classical physical principles on which it had been partly based. Einstein used deductions from phenomena to infer to space-time principles; there are no good grounds for doubting the objective truth of the highly theoretical principles thus established. In particular, while space-time principles are doubtless "abstract", they are so neither in Cartwright's nor in Duhem's sense of that word. In Duhem's sense, the "abstractness" of space-time principles should mean that they are merely symbolic, and conventional because of the supposed empirical underdetermination of choices of symbolic frameworks. I hope that I have argued adequately against that view. In Cartwright's sense, the "abstractness" of space-time principles should mean that they apply only to unreal objects in highly idealized models. This view is false; I have argued, moreover, that were it adopted, its emphasis on models would altogether block the reconstruction of Einstein's reasoning to the kinematics of special relativity. Rather, the terms required for such a reconstruction are the deductivist terms of the positivists. The proper sense in which space-time principles are "abstract" is to be understood in relation to the character of one's commitments when one accepts a space-time theory as true. I have argued that these commitments are structural not ontic, and that the traditional, positivist, empiricist perspective is the best perspective from which to understand their objectivity or physical significance of this structure. In light of non-hypothetico-deductive methods such as bootstrapping which we take to be empirically learned, I have dispensed with the traditional positivist fascinations for conventionalism and underdetermination; accordingly, the empiricist position I have outlined is rightly also deemed a "realist" one. Unlike the hypothetico-deductivist realist that Cartwright attacks in arguing for her theoretical anti-realism, this realist position does not consist of straw. In the next chapter I argue, in the face of all the evidence Cartwright has given for the contrary view, that this empiricist-cum-realist picture is an appropriate regulative ideal for the rest of science, and affords the best understanding of the relationship between theoretical systems, laws, and causes.

Chapter 6

Laws, Causes, and the Systematicity of Nature

The historical cases studied in Chapters 2 through 5 tell us something about science: they tell us that science has (at least apparently) discovered, in bootstrapping, an enduringly useful method of investigation and appraisal. These historical cases likewise tell us something about nature: they tell us that nature is (at least apparently) such that the bootstrap method is useful for investigating it, and such even that this usefulness is likely long to endure. I ha e said that the historical case studies *apparently* tell us these things because the theses just stated about science and nature are empirical theses and can never be positively established. The historical cases that support these theses, however numerous and varied, could in the end turn out to misrepresent the true potential for science's bootstrapping its way to theoretical conclusions about nature. We need not, however, make ourselves overly concerned about the possibility of such a negative outcome. Science so far has roundly indicated that bootstrapping is a method that works, in inferences to low-level causal conclusions, right through to inferences to the highest elements of theory. In theoretical physics, not only did the bootstrap method ground Newtonian principles and warrant various extensions and refinements of classical physics through to the end of the nineteenth century, but it also survived into the present century even when other high-level Newtonian principles did not. We have seen that these developments were progressive by the lights not of any one, but of numerous alternative empirical methodologies. We have seen, then, that one need not adopt bootstrapping's own standards of progress to judge that its application in these cases conduced to progress. Supposing our conclusion concerning method to be adequately secured, we can proceed to investigate the conclusion concerning nature. We ask: *what* (exactly) *have we said about nature* when we say that nature is well-suited to study by the bootstrap method?

In this chapter I argue that what we have said is that nature is systematic, and we have said this clearly and on good evidence. This prepares the way for a reply to Cartwright that is not piecemeal but wholesale: not only this or that element in her conception of science can be faulted, but its basic theme, its guiding idea, should be thrown away. I argue that this can be done without loss: I indicate that the phenomena concerning scientific practice from which her position draws apparent strength in fact can be explained even once this guiding idea and the whole scepticial conception of theoretical science that surrounds it is rejected. I argue further that there are gains to be made, gains long sought by empiricists: we may rid science of a special metaphysics of laws and causes. Cartwright flatly disagrees with this, but I argue that she is wrong to do so.

6.1. Bootstrapping and Systematicity. Clark Glymour thinks it a virtue of his bootstrap account of empirical confirmation that by its lights scientists' preference for the simplest, most systematic or unified, or best-explaining theories can be explained as an *epiphenomenon* of their preference for the empirically most bootstrap-confirmed theory. According to the bootstrap account, these preferences are indirectly warrantable even though methodology must not (in Glymour's empiricist view) ever accord evidential weight directly to extra-empirical considerations about simplicity, unity, or explanatory power. Critics of Glymour have complained that he has simply built into his account of "empirical" confirmation these extra-empirical desiderata. The objection: because Glymour's method conduces to increased simplicity, unity, and explanatory power of theory, it favours as directly as you please theories that possess such qualities, and for this reason is not a wholly empirical method after all. Glymour's calling his account an account of empirical confirmation is, on this view, mere verbal subterfuge, and however ingenious his attempt may be to *seem* empiricist, Glymour in fact forsakes empiricism. Glymour (on this view) pays lip-service to empiricism but union dues to rationalist metaphysics.

The objection seems to be this: the bootstrap method presupposes that nature is fundamentally simple, systematic, comprehensible; according to rationalist metaphysics nature is fundamentally simple, systematic, and comprehensible;

thus the bootstrap method is rationalist metaphysics. But this argument is invalid, and its conclusion is false; the bootstrap method is not rationalist metaphysics. Glymour can champion the bootstrap method and nonetheless remain faithful to empiricism, and can mean something not merely metaphysical, but something clear and empirical, when he implies (as he must) that nature is fundamentally simple, systematic, and comprehensible. For he can insist that the credentials of the method are empirical, that science has learned on the basis of experience that nature is such as to be well studied by this method. And when he implies that nature is fundamentally simple, systematic, and comprehensible, he can mean precisely that nature will *continue* to prove amenable to study by the bootstrap method. This is an empirical thesis about the future of science. The thesis indirectly concerns nature. Because of the indirectness it concerns not noumenal nature, nature-as-it-is-in-itself, but phenomenal nature, nature as experienced by us and as we (will) conceive it by the present (and future) lights of our empirical science. The idea that nature will continue to prove amenable to study by the bootstrap method implies that phenomenal nature is simple, systematic and comprehensible, for it says that science's quest for *better* theories (better bootstrap-confirmed theories) is inevitably a quest for theories that are more unitary and better explaining. Whether this view is correct can never be demonstrated, but only *shown* more or less compellingly by science.

My chapters 2 through 5 argue that science from the time of Newton seems to have been showing this, and showing this ever more compellingly. Add the studies by Cartwright and others that discern bootstrap methods very widely in the sciences, and the conclusion seems inescapable that nature is simple, systematic and comprehensible. At least (in a manner of expression from my Preface) it is seriously *unpragmatic* to doubt this. It is unpragmatic to consider least sure the principles by which we judge most sure various "established" elements of our presumed knowledge. For that would necessitate doubt about the elements that we would doubt last, and so would necessitate doubt about all of our presumed knowledge, and thus would entail the thoroughgoing scepticism about knowledge to which the pragmatist objects on the grounds that we must always set out from where we are (in any task, and not least in epistemology). The pragmatic view is that science is actual and we should study not whether but how it is possible. But how science is possible is to be discovered in principles that are part of science itself, principles of method that science has empirically learned. It is no fault of science that science cannot establish, but can only tentatively display in its practice, what methods work for the advancement of our knowledge of nature. It is, at any rate, pragmatic to accept this, and to regard the glass that is thus half-filled to be half full rather than half empty. We should not make the impossible demand of science that the reliability of its methods be conclusively proven; if we are to be pragmatic, we rather should assume that science will move forward using precisely those methods that by the present lights of science have borne the best and most abundant fruit.

Granting then that the warrant for bootstrap principles, though forever inconclusive, is excellent, we have a clear case for the thesis of the systematicity of nature. Cartwright has helped provide grounds for this conclusion. She certainly holds up as most sure the principles in science that she sees as bootstrap warranted. But the conclusion that nature is systematic is one that she never draws, and given other things she says, is one she can never allow. On Cartwright's stated view, the evidence all points quite the other way; as Cartwright sees it, the actual practice of science implies that nature is unsystematic. I shall next review the piecemeal criticisms we have so far made of Cartwright's stated view, and then consider what may be said against her position wholesale, consequent upon rejecting her central conviction of the unsystematicity of nature.

6.2. Recapitulation: Piecemeal Disagreements with Cartwright. We have already assailed Cartwright's position in a piecemeal way. Against her sharp distinction between theoretical and causal explanation, we have shown by historical examples (in particular, in the work of Copernicus, Kepler, Newton, Maxwell and Einstein) that the distinction is often impossible to make out. Against her methodologically separating high-level theoretical science from the kind of inference she says scientists make to low-level conclusions, we have argued that the method in terms of which she reconstructs the low-level inferences has its clearest most cogent applications (and historically had its earliest applications) in inferences to high-level theoretical conclusions. Against her doubtful, idiosyncratic theses about component forces and consequent special sceptical theses concerning fundamental force laws, we have posed an alternative conception of the "resultant" force, a conception that dissolves the problem upon which Cartwright bases her scepticism; and we have argued that the truth of the principles of theoretical mechanics, which Cartwright grants, very positively assures (on the basis of deductions from phenomena that Newton made in their light) the truth of Newton's fundamental force law concerning universal gravitation. Against Cartwright's pretension that she has described science post-positivistically, systematically, and as it is actually practised, we have examined a variety of theories of method or of empirical confirmation and found all of them illuminating of historical developments in science; we have argued that whereas much work in science is not well illuminated by positivism, the development and the significance of space-time theory is well understood only in positivist terms; and on the basis of these considerations we have illustrated that it is impossible to give a systematic description of how science is actually practised, and that rather we philosophers must idealize scientific practice, and tolerate pluralism by embracing many philosophical accounts of science that each partly illuminate how science works. We have shown, against Cartwright's *Laws*, that "constructed" content in scientific theory (though doubtless present at most levels of theory) is precisely what the great system-seeking thinkers in science (such as Newton, Maxwell and Einstein) tend to eliminate. In the process of their work, we have shown, radical empiricism and radical rationalism work hand-in-hand; it is partly for this reason that the developments for which they are responsible are well described by positivism. We have shown, against Cartwright's *Capacities*, that the resulting theories are not "abstract" in anything like her sense of that word. Moreover, we have shown that they are not "abstract" in Duhem's sense either. In showing this, we attacked two traditional sources of scepticism about high-level theory, the underdetermination thesis and the thesis of conventionalism; in so doing, however, we have also illustrated how the realism-empiricism dispute may simply disappear at the highest levels of theory. Thus we found ourselves underwriting certain themes from positivism with which Cartwright disagrees, even as we dispensed with the anti-realist themes from positivism with which Cartwright is in some sympathy. Among the themes from positivism that were supported is the Kantian one limiting knowledge to the empirical. Nothing we said committed us (as Cartwright is committed) to metaphysical realism, or in particular to the thesis that a cause's being real or a law's being true is, as Cartwright claims, a metaphysical rather than broadly empirical matter. Indeed, we found in space-time theory a clear case of laws or principles whose content seems limited to the structure of experience.

6.3. Against Cartwright's Metaphysics. Cartwright's most basic conviction about the world is that it is unsimple through and through. Cartwright often resorts to theological metaphors when discussing this conviction in general terms. We know, however, what Cartwright says about the aims of science and the methods of appraisal of scientific theories, and we may try to understand her conviction that nature is unsystematic in this appropriately methodological connection. Cartwright says that "theoretical explanation" is always a counterfeit form of understanding; that theories achieve what explanatory power they have only by backing painstakingly away from the real, nitty-gritty nexus of singular causes among unique particulars, to unreal heights of fictive abstraction; that science neither aims to achieve nor could ever achieve truth in its explanatory theories; that rather science achieves what truth it has in its experiment- and measurement-based explorations of the complicated, unsystematic nexus of causes. (Cartwright likens her view to Aristotle's, but this, as we saw in Chapter 2, involves a conception of Aristotle as a pluralist about "natures". Aristotle says much that supports this conception, but it is not easily reconciled with Aristotle's strong emphasis on rational unity in his theory of science.) Against all these elements of Cartwright's view we have made piecemeal criticisms, reviewed last section. We might add here that science advanced beyond Aristotelianism, and the rejection of those conceptions in Aristotle (or perhaps, rather, in classical Greek common sense) to which Cartwright attempts to return was no incidental part of this advance: our study in Chapters 2 and 3 of the relevant history showed that quite the reverse is true. Science's bootstrapping to exceptionless high-level quantitative physical laws was the essence of this advance.

Cartwright's pluralism about causal capacities involves metaphysical commitments of precisely the sort that traditional empiricism attempted to escape. Traditional empiricism attempted to do without any special metaphysics of causation, through connecting causal necessity to the systematicity of theory. On Cartwright's view, the practice of science does not square at all adequately with the empiricists' anti-metaphysical picture. For the systematicity simply is not there, she contends, in light of which causal necessity can be understood structurally rather than ontically. Such links as theoreticians can actually fashion between causes and theoretical laws, she says, are by methods so diverse, and employ assumptions so various, that properly, the laws ought not be regarded as general truths concerning the world, but rather as pure artifacts of theoretical oversimplification. So Cartwright attempts to embrace on its own terms the patchwork nexus of causes from which she says the theoretician backs away: Cartwright argues the acceptability in science of causal talk that has no basis in theoretical law, and she assumes ontic, metaphysical commitments accordingly.

For Cartwright, the relation between causes, laws and theory is as follows. First, there are singular causal facts -- this aspirin relieved this headache. These ground capacity claims -- aspirins carry the capacity to relieve headaches. General theory (e.g. "an aspirin will relieve a headache") studies causal relations in abstraction from the natural impediments that can prevent a capacity's being fulfilled. Thus theoretical laws hold good only in idealized contexts. When theoretical principles are read as laws, laws describing regularities in nature, they are one and all false (because of the impediments). However, the same principles, read as ascriptions of causal capacities, are true. Read whatever way, the principles do *not* (in Cartwright's view) confer the status of being *causal* upon such singular facts as that this aspirin relieved this headache. Rather, such truth as the theoretical principles possess (capacity-ascription truth) they possess (Cartwright contends) because of the *prior* existence of singular causes such as this aspirin relieving this headache.

All this is diametrically opposed to the traditional empiricist view, according to which system is prior to law and law prior to cause. Whereas traditional empiricists attempt with their emphasis on system to avoid any ontic commitment to causes, Cartwright simply has no desire to avoid metaphysics. She is explicit about being a "metaphysical realist". Though Cartwright is "anti-realist" about the theoretical laws around which the sciences are ostensibly systematized, she holds that laws are quite capable of being true or false. For Cartwright, theoretical laws are determinately false, and thus certainly do have truth-values. In fact, Cartwright thinks that were laws true this fact would be in part ontic.

She contends (*Capacities*, p. 199) that the ontic conception of laws developed by D. M. Armstrong[1], F. Dretsky[2] and M. A. Tooley[3] (laws as objective relations among universals) provides the best picture of what laws would be were they true.

With John Earman, I believe that the Armstrong-Dretske-Tooley conception of laws is obscure and unnecessary metaphysics. Earman argues that the objections Armstrong and others posed against the empiricist regularity account of laws can be satisfactorily resisted. Empiricism about laws is the more defensible, I believe, because of the developments in empiricism (discussed in Chapter 3) that argue against the empirical underdetermination thesis. I grant that the empiricist account of laws (defended by Earman) by Mill, Ramsey and Lewis is terribly glib about the prospects for adequately axiomatizing real theories in natural science, and about the deductive links that are supposed to connect the laws with experience. On the latter head, we have seen a way free of the principal difficulties in the distinction between data and phenomena. In a moment I shall discuss, with reference to the work of Mark Wilson, the exact

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[1]*What is a Law of Nature?*, New York: Cambridge University Press, 1983.

[2]"Laws of Nature", *Philosophy of Science* 44: 248-268, 1977.

[3]"The Nature of Laws", *Canadian Journal of Philosophy* 7: 667-698, 1977.

way in which the Mill-Ramsey-Lewis deductivist image of scientific thinking is glib, but I shall argue that despite its glibness, the Mill-Ramsey-Lewis empiricist account of natural laws is perfectly respectable. It rests, it is true, upon a considerable idealization of the role that laws *could* play in human thinking, and upon a very great idealization of the role that presumed laws *actually* play in our thinking; but its task is to make clear in epistemic terms the notion of something which, judged from the standpoint of knowledge, is clearly an aim or an ideal; so its dependence upon idealization is hardly to be held against it. I gave in chapter 5 a good example of laws in this sense. In my discussion of space-time theory I showed that physical science does arrive at principles that satisfy all the conditions for being laws in the empiricist, Mill-Ramsey-Lewis sense, and are pretty obviously objective, given the strength of their bootstrap warrant and the weakness of all attempts to defend conventionalist theses concerning them.

Cartwright in my view is incorrect to suppose that a proponent of laws must go over to the ontic conception. Cartwright suggests that her own ontic commitment to causal capacities is a "smaller metaphysical price" than the traditional commitment to the objective truth of laws. I have argued that, on the contrary, one can believe in the objective truth of laws without assuming metaphysical commitments. Next I shall argue that Cartwright is mistaken to assume an ontic commitment to capacities

-- there is no good reason to adopt her special metaphysics of causation. I first survey some extant positions on the issue, and then defend a new position (making use of work of Mark Wilson's).

6.4. Cartwright, Russell, Salmon, and Kitcher. Cartwright contends that there fundamentally *are* in the world the singular causes from which her account of natural necessity makes its start. Her position is exactly opposite to Bertrand Russell's; in "On the Notion of Cause"[4] Russell attacks the very idea that science studies causes. Russell's main contention was that it is the way of science to reduce the description of things to functional laws; but functional laws are invertible, so in their light the asymmetry between cause and effect cannot be sustained. Since the asymmetry between cause and effect is part of the very notion of cause, the scientific interest in questions concerning causality is supposed to evaporate. Cartwright in effect levels two book-length replies to Russell's main contention: it is scarcely the way of science to *reduce* the description of things to functional laws; laws rather are the endpoints of artificially systematizing, fictive "theoretical" explanation, and the bulk of explanation in science is not purely theoretical but concerns causes. We need not look again at Cartwright's reasons for saying this. Suffice it to say that Cartwright has the

[4]"On the Notion of Cause with Applications to the Free-Will Problem", in H. Feigl and M. Brodbeck, *Readings in the Philosophy of Science*, New York: Appleton-Century-Crofts, 1953.

most direct reply of all to the asymmetry problem. She denies that the laws are even true, by reference to which Russell and others have argued that there is a problem with explanatory asymmetry.

Cartwright claims for her account the advantage that it smoothly accommodates other features of scientific practice than those surrounding asymmetries in explanation. She contends that the bulk of scientific work comprises low-level measurement- and experiment-based inferences to the reality of specific singular causes. Can her traditional empiricist adversaries themselves make proper sense of this practice? Cartwright thinks not.

In *Laws* Cartwright chooses to illustrate her causes-first, laws-and-theory-second metaphysical conceptions in the context of statistical explanation, mounting a case against Patrick Suppes' idea of probabilistic causation that is also an argument against assimilating causes to regularities. Cartwright highlights Simpson's paradox, which concerns the fact that correlations that suggest causation can always be removed by some partitioning of the sample space. For example, Berkeley University rejected women job applicants at a much higher rate than it rejected men job applicants, so that being female positively correlated with rejection. Department by department, however, the rejection rates for women and men were roughly equal -- women simply tended to apply for jobs in departments with high overall rejection rates. The fact that partitioning by department removes the correlation strongly suggests that there is no gender bias in hiring practices at Berkeley after all. It suggests this because we know that the partitioning of the sample space by department is *causally relevant* -- for we know that hiring decisions are made within departments. "Simpson's paradox" is that there always will be some partitioning (perhaps not "causally relevant", however) that similarly removes any given correlation. What the "paradox" establishes is that there is no non-circular understanding of causation in terms of correlation; for correlation establishes causation only when there are no *causally relevant* partitionings of the sample space that remove the correlation. Suppes' probabilistic view of causation supposes that identifying (stochastic) causes is a relatively simple matter of identifying probabilifying factors. Simpson's paradox shows that, on the contrary, inference from statistics to causes is something requiring a background of "already causal" assumptions.

In *Capacities* Cartwright illustrates with case studies concerning several sciences how scientists make bootstrap inferences to causal conclusions. Here again she emphasizes "already causal" assumptions -- that is, strong, high-level background assumptions that express prior commitments to the reality of singular causes -- assumptions without which (she contends) the bootstrap deductions would not be possible.

In the context of statistical explanations in the special sciences, Cartwright's case for the acceptability in science of causal talk that admits no coherent or immediate reduction to theoretical law is especially persuasive. Wesley Salmon, whose work on statistical explanation is well known, in fact has been quite turned around by Cartwright's work: whereas in his own earlier work Salmon had eschewed special attention to causes, he now admits, with Cartwright, that statistical arguments *explain* only in light of special assumptions concerning *causal homogeneity*. In his *Scientific Explanation and the Causal Structure of the World*,[5] Salmon attempts to redress the inadequacies of his earlier position, and, through a return to some ideas of Reichenbach, to show how the requisite causal talk in science can stand on its own two feet, independently of any basis in theoretical law.

Philip Kitcher is perhaps the foremost philosopher to attempt detailed arguments against the Cartwright-Salmon view.[6] Kitcher's opposition to Cartwright and Salmon is informed by an interpretation of Kant, and in particular,

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[5]W. Salmon, *Scientific Explanation and the Causal Structure of the World*, Princeton: Princeton University Press, 1984.

[6]Philip Kitcher, "Two Approaches to Explanation", in *The Journal of Philosophy* (1985) *85*: 632-639; also his monograph-length essay "Explanatory Unification and the Causal Structure of the World", in P. Kitcher and W. Salmon (eds.), *Scientific Explanation, Minnesota Studies in the Philosophy of Science*, vol. XIII, Minneapolis: University of Minnesota Press, 1989. Pp. 410-505.

by his interpreting the Appendix to the Ideal of Pure Reason (in which Kant discusses theoretical systematicity) as Kant's "constructivist completion" of Kant's own "radically nonconstructive" reply to Hume in the Second Analogy.[7] The lessons that Kant, on Kitcher's interpretation, is teaching, Kitcher thinks remain important to philosophy of science. Kitcher calls the Kantian position a "top-down" understanding of necessity in natural science, for on this understanding laws are prior to causes, and the system of theoretical knowledge is prior to law. The Cartwright-Salmon understanding Kitcher calls "bottom-up", since it reverses these priorities. Of course the nature of the relationship between system, laws, and causes, is conceived very differently in these two accounts. Kitcher thinks hopeless Reichenbach's mark method for demarcating causal from non-causal processes. With Kant, he insists that there cannot be determinable events in a determinable time-order -- events about which the *question* of lawful sequence could arise -- without there already being laws that systematize these events. With Kant, Kitcher argues that there is a thoroughgoing dependency of what are the phenomenal *events* in nature on what (in light of our best systematization of experience) we take to be the *laws* of nature. Kant implies that the Hume world, in which there are events but no causes, is impossible (that is, is a world which we could never experience). On the strength of the same arguments,

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[7]Philip Kitcher, "Projecting the Order of Nature", in R. E. Butts (ed.), *Kant's Philosophy of Physical Science* (Dordrecht: D. Reidel, 1986), pp. 201-235.

Kitcher thinks impossible the Cartwright world in which there are events and singular causes but no systematizing laws.

Kitcher's Kantian conception of science in effect emphasizes inference-to-the-best-explanation. It also employs a conception of explanation that is based on unification. Kitcher asserts that "[u]nification consists in the derivation of many conclusions by using a few, stringent, argument patterns".[8] In the course of defending his basically epistemic, deductivist, anti-metaphysical theory of causation, Kitcher successfully shows that such a theory is capable of dling many cases thought damaging to epistemic conceptions of causality. He shows in particular (in a wealth of interesting detail) that an epistemic account does not necessarily succumb to the old problem of asymmetry.[9] There is much in Kitcher's position that I would like to assimilate to my own. But on two points just mentioned Kitcher collides head-on with Cartwright's case for her position. There is enough about the practice of science that seems to me to conform with Cartwright's general picture that Kitcher's impacting her position head-on seems to me a mistaken way to oppose Cartwright. Whereas Cartwright has spurned

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[8]P. Kitcher, "Projecting the Order of Nature", op. cit., p. 228.

[9]See especially his "Explanatory Unification and the Causal Structure of the World", in P. Kitcher and W. Salmon (eds.), *Scientific Explanation, Minnesota Studies in the Philosophy of Science*, vol. 13, Minneapolis: University of Minnesota Press. Pp. 410-505.

inferences to the best theoretical explanations, Kitcher says that that form of inference is absolutely fundamental to science. Whereas Cartwright insists that the argument patterns linking theoretical principles to phenomena are very diverse and never "cover" all phenomena, Kitcher insists that the essence of science is to make these patterns "few and stringent" and yet covering of phenomena. If that is so, Cartwright is thoroughly mistaken even about the features of scientific practice that she sees as evidence for her position. In my view, Cartwright is *not* so thoroughly mistaken about these features, and Kitcher's position is for this reason inadmissible.

6.5. Elements of an Alternative Epistemic Conception of Causation. I think that a basically epistemic, deductivist, anti-metaphysical theory of causation *can* be defended, and defended on the supposition that systematizing theoretical laws are true. Unlike Kitcher, however, I shall simply concede to Cartwright that there *is* the widespread dependence in science on theoretically unsupported causal notions that she says that there is, and that philosophers of science, whose paradigm examples of scientific explanation are typically trite, stock, nomological-deductive cases, (1) have failed to account for the very great diversity of the causes that science brings into view, and (2) have so far failed to account for how such diversity of causes is possible, if causes are to be understood by reference to general laws. Here I shall state what elements I think must go into such an account, and in the next sections I shall argue that an account with these elements can succeed (where Kitcher's does not) as a reply to Cartwright.

In place of Kitcher's Kantian inference-to-the-best-theoretical-explanation conception of scientific practice, I emphasize bootstrap inference. The credentials for this inference pattern I have proposed (in a non-Kantian, Mill-like spirit) are empirical. Bootstrap inference is an inference pattern which I have argued works not only at the levels at which Cartwright herself discerns it, but all the way up to high-level theory. However, I accept (in the face of the phenomena concerning scientific practice from which Cartwright's position derives apparent strength) that the high-level theoretical principles thus bootstrap inferred may link to such phenomena as they do "cover" not in "few and stringent" but in diverse and subtle ways, and that we will often find it impossible in practice to link phenomena back to laws at all. In the face of often insuperable difficulties in the task of linking phenomena back to laws, we will rather achieve by low-level, experiment- or measurement-based bootstrap inferences to quite specific, theoretically unsupported causal conclusions what understanding we can achieve of the workings of some systems that we study. That is, our investigations will often proceed by inferences that are precisely of the sort that Cartwright makes basic to her account of science.

Why I think we should expect that much science will be like this is not (as Cartwright says) because nature is unsystematic, but rather because of underappreciated features of the mathematics pertaining to fundamental, systematizing laws that I have argued are true. These underappreciated features are well discussed by Mark Wilson, and I turn to a short discussion of his work in the next section. After discussing Wilson, I shall argue that Cartwright's empirically inferred causes (when they are real) derive their natural necessity from the obtaining of some theoretical law, even though in general it is beyond our mathematical abilities to fathom this. Though the position I sketch involves a thesis that is untestable and in that sense metaphysical, it removes ontic commitments to singular causes and so in another sense is anti-metaphysical. Moreover, it accommodates those features of scientific practice to which Cartwright has drawn special attention, and makes the idea of causal necessity as perspicuous as I think it can be made.

6.6. The Diversity of Causes in a Law-governed World. The phenomena concerning scientific practice from which Cartwright's conception of science derives apparent strength point to pervasive and important features of science that philosophers of science have indeed tended to neglect; they concern low-level experiment- or measurement-based inference to causes, and the great diversity and singularness, from a theoretical standpoint, of the causes thus brought to light. But these phenomena I believe can be explained even on the "deductivist" image of science that Cartwright (in *Laws*, at any rate) attacks, and on an assumption about laws that is opposite to Cartwright's, namely, that theoretical laws presently accepted as "fundamental" are (at least approximately) true. The explanation looks to some underappreciated facets of the mathematics appertaining to fundamental physical laws, and the gist of it, in cases where the mathematics *is* tractable, can be gleaned from recent work, directed to quite different philosophical conclusions, by Mark Wilson[10]. Wilson highlights the fact that existence and uniqueness proofs play an underappreciated role in real-life applications of physical laws to physical systems, and shows with a wealth of examples that the world may be wonderfully rich in its causal characteristics though simple fundamental laws be true. That is, the diversity to which Cartwright draws our attention of the ways by which scientists link particular causal patterns to theory may have its source simply in the mathematics pertaining to laws presumed true.

In light of Wilson's work, I believe one can see Cartwright as significantly right about scientific practice but wrong in her conclusion that nature is ununi-

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[10]Principally "Honorable Intensions", unpublished; and also: "The Double Standard in Ontology", *Philosophical Studies* (1981) *39*: 409-427; "The Observational Uniqueness of Some Theories", in *The Journal of Philosophy* (1980) *77*: 208-233; "What is this Thing Called 'Pain'? -- The Philosophy of Science behind the Contemporary Debate", *Pacific Philosophical Quarterly* (198) *66*: 227-267; Review of D. M. Armstrong's *What is a Law of Nature*, *The Philosophical Review* (198) *96*: 435-441; Review of J. Earman's *A Primer on Determinism*, *Philosophy of Science* (1989) *56*: 502-532.

fied. For Wilson shows how, on the assumption that the fundamental laws of physics are true (so that, contrary to Cartwright, nature has a relatively simple basic nature), natural science must nevertheless bring diverse *properties* into view. Such diversity of properties, Wilson's work makes clear, is compatible with the positivists' regulative ideal of theoretical unity and the theory-first, laws-and-causes-second conception of causality. But its implications for theory and practice in science are quite another matter. Concerning practice, there is no guarantee, and much good reason to doubt, that practice in the diverse fields of scientific inquiry will ever be appreciably interlinked, or ever (in particular) adequately linked back to fundamental physics. Concerning theory, Wilson argues that a strong physicalist thesis can nonetheless be maintained: no theory in science concerns matters autonomous in principle from physics. But Wilson is no physical reductionist: the fields of scientific inquiry are undeniably many, and physics is no umbrella for them all. Typically there is no possibility of making out the relation to fundamental physics of the causal patterns that are brought to our attention by a given field of science. Thus the conceptions of a veritable *deductive system* of knowledge to which the Mill-Ramsey-Lewis account of laws and the Kitcher account of causality refer as ideals, Wilson's work implies *must* forever be idle pipe dreams. But Wilson's work also shows how causal necessity could for all we know always link back simply to the obtaining of fundamental physical laws. If the fundamental physical laws are true and there is no further necessity in the world than what follows from all natural systems falling under them, still the causal characteristics of the world would be just as bewilderingly diverse as all the special sciences collectively say that they are.

Wilson complains that there has been insufficient "appreciation of the complicated ways in which a differential equation may express 'causation'". In particular, philosophers have altogether missed the key role played by property existence and uniqueness proofs. Wilson's complaint can be laid, with some irony, against Russell, whose attack on the very idea that science studies causes we briefly discussed above. The way one works from a differential equation to various conclusions about the motions of a physical system is very different from the way Russell supposed one works from functional physical laws to such conclusions. Wilson's discussion makes Russell's main contention (concerning explanatory symmetry in laws) in "On the Notion of Cause" seem very naive. Wilson shows how the asymmetry requisite for causation has after all not disappeared: the asymmetry is there in the step of establishing property-existence claims. The mathematical reasoning from fundamental laws underlying a physical system to its properties, and thence to its "effects" or later states, is evidently far subtler than Russell imagined. This is ironic given Russell's high-handed assertion that "the reason why the old 'law of causality' has so long continued to pervade the books of philosophers is simply that the idea of a function is unfamiliar to most of them, and therefore they seek an unduly simplified statement".[11]

I have already stated my agreement with Cartwright that science *does* study causes; Russell's colourful contention to the contrary cannot survive the evidence that Carwright mounts against it. I also agree with Cartwright that the causes brought to light by low-level measurement- or experiment-based inferences in the various special sciences are wondrously diverse. I see Wilson's work as establishing, however, that this in no way argues for the falsity of systematizing laws. In earlier chapters I argued, against Cartwright, that the high-level theoretical laws in physics are empirically every bit as well-supported as the specific causal claims that Cartwright assumes are true. Wilson's work supports the further conclusion that, on the assumption that these high-level laws are true and are the ground of all natural necessity, the practice of science can seem very much what Cartwright describes in her bottom-up conception.

[11]Russell himself simplistically suggests that if *determinism* be true, yet not a trivial doctrine, the function assigning states of any given physical system to various times must be sufficiently simple to be capturable within language. Incidentally, Kemeny extracts this (falsehood) as the principal lesson from Russell's paper, though he asserts, against Russell, that the thesis that the relevant functions are capturable in language is a very bold one. Wilson's paper is rich with correctives to the mistakes Russell and Kemeny are making here.

Just as I accept an account (that John Earman has carefully defended) of lawhood that I acknowledge involves an idealization about scientific practice, so the view that I accept about causes is idealized; it is contrary to Cartwright's, and *more or less* the "top-down" Kantian view set out and defended by Kitcher. Into Kitcher's account, however, I add the generous caveat that the deductive relation of causes to fundamental laws will often be beyond our (mathematical) abilities to make out. Kitcher has done an impressive job[12] showing how an epistemic account of causation in fact is capable of handling many cases thought damaging to it, and I accept Kitcher's good work on this front as helpful to my own view. But I hope to avoid the glibness about systematicity in Kitcher's conception of knowledge, a glibness that in light of Wilson's work we may link to a naivete about the mathematics pertaining to physical laws. Against Kitcher I do not assume that the store of argument patterns linking fundamental theoretical principles to phenomena are "few and stringent". They are rather, as Cartwright says, thoroughly diverse, and thoroughly unable to "cover" the phenomena (that is, the causal facts).

In proposing, diametrically contrary to Cartwright's view, that fundamental physical laws are true, and are, moreover, the ground of all natural necessity, I am certainly proposing something untestable. It is in one sense of "meta-

[12]Particularly in his "Explanatory Unification and the Causal Structure of the World", op. cit.

physical" a metaphysical thesis that I am advancing, that the necessity in every genuine cause links back (probably in no way fathomable by us) to the obtaining in the system in question of fundamental physical laws. However, my thesis absolves me of ontic commitments; I say that neither singular causes nor causal capacities are in the world, that the natural necessity in causation relates to the obtaining of general laws, and that laws are structural -- no ontic doctrine is needed of the nature or objectivity of laws. In this way my thesis is anti-metaphysical. Cartwright claims that her metaphysical commitment to the prior reality of singular causes is a "price worth paying" for the purpose of adequately reconstructing scientific practice. Because I am able to explain in my own terms the phenomena concerning scientific practice from which Cartwright's conception derives its apparent strength, I reject this view. Cartwright's special metaphysics of causation seems to me an avoidable weakness in her position.

6.7. Conclusions. Whereas Cartwright is a pluralist about the natural world, holding that there are innumerable separate natures or causal capacities in things, I have argued that the world is one system. My argument is empirically based, its evidence the historical success of the bootstrap method. I have argued that Cartwright's doctrine of the diversity of causes is nonetheless true, and the reason why it is true can be found in the mathematics pertaining to fundamental physical laws. In place of Cartwright's ontology of diverse singular causes and causal capacities, I have argued for the reality of the deep-lying structures that fundamental physical laws describe to us. I have argued for accepting the untestable thesis that all that is genuinely necessary in nature is so wholly because of the reality of these structures. This thesis rids us of superfluous metaphysics, and (contrary to Cartwright's view) there is no legitimate reason to reject it in order to account for the practice of science.

Afterword

A recurrent theme in my discussion opposes a somewhat incongruous contrary theme in Cartwright. Despite arguing for the systematicity of nature, I have urged that *science is not itself systematic.* In Chapter 2 I discussed how Cartwright in effect argues that the world has not a single systematic nature for physics to describe. I have also noted, however, that Cartwright in effect assumes that *science* has a single systematic nature for philosophy of science to describe. Just as Cartwright claims that there is no single nature of nature, that is, that there is no one coherent underlying order in the physical world for our physical theories to grasp (a claim I have argued we should reject), so Richard Rorty[1], Ian Hacking[2], Nancy Nersessian[3] and others have argued that there is no single nature of *science* that a theory of science can grasp. Science is not a "natural kind", and so we must expect our knowledge of it to remain forever piecemeal and ununified. I think that there is much truth in this latter claim: at least the relevant empirical evidence so far testifies for its truth. Certainly this claim is true when we take science as an object for *naturalistic* study. For

[1]*Philosophy and the Mirror of Nature*, Princeton: Princeton University Press, 1979.

[2]*Representing and Intervening*, op. cit.

[3]*Faraday to Einstein: Constructing Meaning in Scientific Theories*, Dordrecht: Nijhoff, 1984.

example, the generalizations so far proferred by "strong programme" sociologists of science, who aim to explain the development of science wholly in terms of causes, are vacuous (because they involve catch-all, tell-nothing terms such as "social negotiation"), and although their case studies are fascinating for their always lurid and sometimes accurate and illuminating discussions of select nitty-gritty sociological details of certain select episodes of scientific work, we certainly are not pointed by them in the direction of a general understanding of science. I think that philosophers' various methods of reconstructing science, methods that aim to explicate scientific *reasoning*, have handled science from a more satisfyingly general point of view. However, it remains impossible, I believe, to find a unitary general conception of the workings of science. I think the philosophical investigation of science is important because it achieves a measure of success in illuminating science in a general way, while scientific studies of science achieve very little such success. But I think that because the subject of study itself has no unified character, this philosophical work must be left quite open-textured, and the question of its relation to naturalistic studies of science must also be left open.

Philosophers of science, including Cartwright, have tended to advance philosophical conceptions that are too singular and definitive to be true to the subject of study. This is true of the two schools emphasizing "rational reconstruction" of science. Carnap emphasized the logical or rational reconstruction of established theoretical science, as a way of exploring the "foundations" of scientific theories. Carnap began formal studies of the confirmation of theories by empirical data. Popper and Lakatos sought, in a more historical vein, first to demarcate science from non-science by a philosophical theory concerning the methods that constitute the rationality of science. Armed with this, they proposed that history of theoretical science be written reconstructively, to display its points of conformity with the philosophical theory. On their historiography, one regards as rationally explained all elements of history deemed rational by the lights of the theory of method, and relegates to study by sociologists and psychologists all other elements of actual history. In my historical chapters I have argued that rational reconstructions of these two sorts can be illuminating; but I in no way wish to imply that rational reconstruction reveals science's true *nature*. Again, I believe that it is unlikely that science truly has a nature to be revealed; there is only a *pragmatic* rationale for rational rather than pragmatic or naturalistic (social/psychological) reconstruction of science. Socio-psychological illumination of science is too difficult to be achievable, and would in any case not deliver useable generalizations about science. There are a variety of approaches to the pragmatic reconstruction of science, but I think my pragmatic point holds against all of them. I have already instanced strong-programme sociology of science. Another example is the German "constructivist" school, whose proponents seek to illuminate the content of physical theories by reference to "life-world" concepts supposedly fundamental to the conceptualization of social action. It is easy to see, from perusing their work, that, legitimate as the task may be, to complete it would require far greater genius and effort than went into the development of the physical theory under study. There seems, once again, a pragmatic rationale for settling for something less, namely, an admittedly idealized and potentially spurious reconstruction in terms of supposed canons of method or reasoning.

It in fact sits well with this view of why philosophy is important in the study of science, that philosophical theories pertaining to science comprise a somewhat eclectic jumble. Variety is needed in philosophers' theories of method because no single theory picks out all and only the methodologically salient facts in scientific practice. Cartwright is in my view correct when she says that philosophers' concentration on the theories of science has led them to miss important elements. Although I argue against Cartwright's scepticism about laws and theoretical explanations in *physics*, I believe that my general perspective on *philosophy* of science should be deemed a logical extension of Cartwright's view, although, surprisingly, Cartwright does not herself adopt this perspective. Ironically, Cartwright attempts to make a unified system of much post-positivist philosophy of science by introducing a novel, sceptical view of the systematicity of scientific theories. At best she actually succeeds only in creating a partly new, mostly old, important but incomplete set of partial insights into science.

I have argued that *science itself is not systematic*, but at the same time have argued against scepticism concerning the theories in science. I have argued that *it is silly or unpragmatic to entertain thoroughgoing doubts about theoretical science*. My aim has been to argue against Cartwright's theoretical scepticism in terms that should seem well-considered from her own point of view. This aim I think is important because for broad socio-historical reasons, times are evidently ripe for Cartwright's sceptical message about theoretical science. Today it is modish to argue the impossibility in principle of comprehending nature through science. Philosophical conferences are held to proclaim that science is at an end, its traditional aims unfulfilled, its pretense to unity gone forever. The view attracts favour. Hostility to the authority of science is part of the very spirit of our age. As my final word on Cartwright, I will briefly summarize my view of these trends, their causes, and their relation to her. In ending in this way I in no way mean to suggest, as a parting shot, that Cartwright deliberately capitalizes on modern anti-science sentiments. My impression is that there would be no truth in such an accusation. But if Cartwright has initiated a new current of philosophical thought, the significance of her views must be understood partly in terms of other currents, some of them dangerous, that Cartwright's joins and strengthens.

The causes of the modern hostility to science are not far to seek. Ours is an age of exponential curves, curves whose forward projection, however, soon simply must at the very least inflect. Riding high as we are on those curves, we are feeling in mounting degree the peril of our trajectory but also our relative powerlessness in resetting its parameters. It is obvious that we would not be where we are without science. Anti-science thinking therefore naturally arises from the pessimism and frustrations of our age.

No-one today trumpets the 1950s view that science acts mainly to free humanity of natural ills and constraints. Today what puffs the sails of crowd-pleasing academics is a strongly anti-science intellectual wind, a wind that bemoans any idealizing of detached comprehension, that protests rationality, that screams against science for a litany of alleged crimes. Science (so it is said) has urged into infantile hands tools for wreaking havoc in nature and weapons capable of totally destroying the globe. Science has glorified numbers and neglected values, dehumanized the thinking of policymakers, fallen into icy league with the cold, calculating establishment. There is, within science itself (so it is said), an ill-starred macho eagerness for technical control of nature. Environmental problems are thus basically wrought by science -- at least, by short-sighted scientistic meddlesomeness in nature, under the guise of technical "control", a false guise because however we meddle there always are undesirable consequences which we do not control.

It is clear that these sentiments have considerable basis in fact, and it is clear also where one's heart and mind must be in order for one to have embraced them completely. One must want to liberate general culture from its domination by false and perilous scientistic ideology. One must be sufficiently open-minded to recognize, as legitimate, "alternative modes of knowing" that science does not embrace. It is modish today to proselytize a strange kind of hope, a hope that, through our collectively putting science somehow firmly in its place -- in short, through our achieving a radical "change in consciousness" -- our world's problems can be corrected. The 1950s thought science could free us of ills and constraints, the 1990s see science as the source of our ills and constraints.

Times ahead are unlikely to lessen the appeal of the modish new sentiments against science. However inane the leading idea may be that a change in consciousness is the key to solving humanity's problems, it remains perfectly possible that consciousness will change. Nothing guarantees us or any accustomed element of our culture safe passage through the crunches that many known exponential curves assure us lie close up ahead. Riding high, as we are, on those curves, we live in times that are more wondrously rich than ever with human accomplishments, even as our exuberance is darkened by unprecedented problems. The pace of new accomplishments is in itself cause for bewilderment, and shakes our confidence in the perspective on such accomplishments that we inherit from earlier times. Nothing has prepared us, mentally, for the explosion that is the present day Nothing has prepared us, moreover, to ponder such unspeakable thoughts of injustice and destruction and futility as our present condition sets before us -- about historically unparalleled, ever-worsening inequities in the quality of human lives, about our undermining the health on a global scale of the natural environment, and about our species possibly not surviving, and all its accomplishments consequently being lost. No wonder people's faith is severely shaken in traditional conceptions of what we *have* accomplished in the long march to the present day. Finally, and most significantly for Cartwright herself, the pace of accomplishment in the present day overwhelms our individual comprehension, and in this way excites scepticism about the very ideal of unitary knowledge. The old ideals for knowledge prejudge the possibility of a steady perspective on all knowledge from on high. But steadiness and perspective are just what we do not have in our exuberant times.

It is possible in the years ahead that the new currents of general intellectual thought will overwhelm the traditional rationalist current from which science was born. That would be a momentous change in the conception of nature and knowledge, and would wreak thoroughgoing change in the fabric of our culture. Anti-science may, I believe, in our day, for the first time become successfully well-concerted, if world calamities presently unfolding are successfully blamed on science. Yet the calamities would not thus be avoided, and in fact a powerful perspective would be lost for understanding them, not in the reactive terms that our frustrations will foster, as evils of some appropriately defined out-group (for the reactionaries, the "lazy poor", say, or the burgeoning Third World; for the radicals, the ever more unbelievably inhumane Establishment), but as biogeochemical exigencies of ecological overshoot, caused by a basically innocent mistake: the premising of societies East and West on the erroneous notion of limitlessness. For quite some while after the inception of these societies the condition of our world has made this erroneous notion seem tenable. To miss these points, as I believe people's increasing reactiveness against science is helping people to do, is a positive encouragement to one of the greatest evils of our age: vilification and scape-goating, in the face of frustrations that are in fact largely ecological in origin. Right when we need steadiness and perspective we may be turfing out the very ideal.

Proponents of disunity want a change of consciousness away from the ideal of rationality embodied in theoretical science. They may get their change of consciousness, but I am worried about the wider effects that will come with it. I am sure that I know no more worthy author to appraise than Cartwright, in order to test the weight of argument for our jettisoning the traditional conceptions of the unity of nature and of the importance of seeking unity in knowledge. Thus I have targetted Cartwright as the chief, most worthy proponent of the disunity of nature view (with which I disagree completely) and disunity of science view (with which I partially disagree). And I say that criticism of those views *matters*, because of the potential influence of the new school of philosophical thinking with which they connect.

Appendix 1

Disputing "Newton and the Fudge Factor"

The following calculations concern pp. 756-757 of Richard Westfall's "Newton and the Fudge Factor". They address the question that Westfall has posed concerning the sincerity and defensibility of Newton's arguments under Propositions XXXVI and XXXVII in Book III of the *Principia*.

Under Proposition XXXVII, Newton reports Samuel Sturmy's observations that spring tides at the time of the equinox run to 45 feet, and neap tides to 25 feet, "[b]efore the mouth of the Avon, three miles below Bristol". Westfall's remarks about the variability around these values of another observer's measurements (Samuel Colepresse's) seem hardly to the point: the variability that Westfall reports is quite small. (This is as one would expect, since high accuracy can be achieved in simple procedures for measuring the tides, and moreover, the *random* contribution of extraneous factors -- factors other than the sun and moon -- to ocean levels is in most places typically very small.) More serious is Westfall's general suggestion on pp. 756-757 that Newton worked by a series of fudges from the 9:5 ratio to a salutary figure for the ratio of the solar and lunar tidal effects -- salutary for his calculation of the earth's axial precession.

My purpose is two-fold: to calculate from present-day values for the various astronomical and physical constants, whether this 9:5 ratio is reasonable at latitude 50 degrees, and to examine directly the physical reasoning on which each of Newton's corrections, that Westfall alleges are fudges, is based.

I show that the observed 9:5 ratio does indeed seem reasonable, *just in case* I apply Newton's three corrections, relating to special features of the equinoctal case and of the Bristol tides -- the corrections that Westfall charges are fudges. In a small way, I think these calculations confirm the integrity of the observational data used by Newton, and confirm the integrity of his reasonings concerning special corrections to be made in basing on these data an assessment of the ratio of the solar and lunar tidal effects.

I show also that each of Newton's three corrections is based on deep, ingenious physical reasoning. Except for the way Newton reaches an average in respect of the second correction, his reasoning is perfectly sound. There is therefore no good basis that I can see for Westfall's allegation that the three corrections are fudges.

Newton's Reasoning, in brief. Wishing to calculate the ratio L:S of the lunar to the solar tidal influence on the earth, Newton initially related the ratio (L+S)/(L-S) to (equinoctal) spring and neap tides (when the sun declines not at all from the equator). Let SH = spring-high-tide height, SL = spring-low-tide height, NH = neap-high-tide height, and NL = neap-low-tide height. Newton began by equating (L+S)/(L-S) with the observable ratio (SH - SL)/(NH - NL).[1] As mentioned, observations of equinoctal tides near Bristol fixed this ratio at approximately 9:5.

Newton then applied various corrections, the first relating to the fact that, at the site of the observations, high (low) tides occur three tides after the moon is in syzygies (quadratures); the second relating to the fact that, when the earth is in the equinoxes, the moon declines on average 22 degrees 13 minutes from the equator; and the third relating to the fact that the moon's distance from the earth is on average greater in the syzygies than in the quadratures. After applying his corrections, Newton deduced that L/S = 4.4815.

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[1]The legitimacy of this as a first approximation can be explained as follows. Newton knew that the seas may both be raised and depressed by the sun or moon, and that the maximum raising effect is twice the maximum lowering effect. Therefore we can call the moon's maximum raising effect $^{2}/_{3}L$, and its maximum lowering effect $-^{1}/_{3}L$, and the sun's maximum raising effect $^{2}/_{3}S$ and maximum lowering effect $-^{1}/_{3}S$. In that case SH ("spring high") results from $^{2}/_{3}L$ summed with $^{2}/_{3}S$, and SL ("spring low") results from $-^{1}/_{3}L$ summed with $-^{1}/_{3}S$. Moreover, since the effect of the moon dominates the effect of the sun, NH ("neap high") results from 2/3L summed with $-^{1}/_{3}S$, and NL ("neap low") results from $-^{1}/_{3}L$ summed with $^{2}/_{3}S$. Thus (SH - SL)/(NH - NL) = $((^{2}/_{3}L + {}^{2}/_{3}S) - (-^{1}/_{3}L + -^{1}/_{3}S))/((^{2}/_{3}L + -^{1}/_{3}S) - (-^{1}/_{3}L + {}^{2}/_{3}S))$ = (L + S)/(L - S). That Newton evidently followed some such reasoning also assures us, as is fairly clear in any case from Newton's wording, that the rises of 45 and 25 feet respectively are rises over the course of a quarter of a day, and not absolute displacements from some lowest-ever tide mark -- that is, 25 feet measures not NH - SL but NH - NL.

Outline of the comparative calculations. The modern value for L/S is in fact 2.2. The following calculations, based on modern values for the masses of the moon and sun, the average earth-moon and earth-sun separations, and the radius of the earth, are in close agreement with this. L/S equals 2.2 implies that (L+S)/(L-S) should equal 2.67. We show how, when no corrections are made (ideal conditions are assumed), (SH - SL)/(NH - NL) does indeed equal 2.67 at various latitudes, including latitude 50. We then examine how the three corrections that Newton urges for the latitude 50 case alter this result.

I calculate directly the difference between the acceleration due to the sun or moon of a point on the earth's surface, and the acceleration due to the sun or moon of the centre of the earth. This difference, which I call the "differential acceleration" due to the sun or moon, at most points on the earth's surface has both a vertical component and a horizontal component. Newton incorrectly considered that only the vertical components are significant for the oceanic tides. I concern myself with the vertical components, and also, where this is possible, for purposes of comparison, with the horizontal components. Because, due to the moon's greater proximity to the earth, the differential accelerations due to the moon form a slightly less symmetrical pattern over the earth's surface than do differential accelerations due to the sun, the results concerning ratios of solar and lunar horizontal components are not quite identical with the results concerning ratios of solar and lunar vertical components. But the differences prove to be slight. Newton's approach to determining L:S is therefore not compromised by the incorrectness of his conviction that the vertical components only are significant for the oceanic tides.

My calculations display only very slight variations with latitude in the ratio (SH - SL):(NH - NL) for the idealized case (involving no corrections). So Newton was correct to think that in determining this ratio, with appropriate corrections, at latitude 50 degrees, it was unnecessary to worry about a dependency on latitude of the result. I also show that while the calculated ratio (SH - SL):(NH - NL) for the idealized case differs markedly from the observed 9:5, Newton's corrections have a markedly salutary effect. The corrections bring the ratio from 2.67 to 1.849, and thus within the general ballpark of the observed 1.8 = 9:5. As I have said, I think this argues, in a small way, for the integrity of Newton's corrections. The more cogent argument is just that these corrections make complete physical sense: as far as I can see Westfall really has no good basis for alleging that they are fudges. Along the way to establishing my quantitative results, I examine the physics underlying Newton's three corrections, and argue (with one partial exception) that his reasoning was sound.

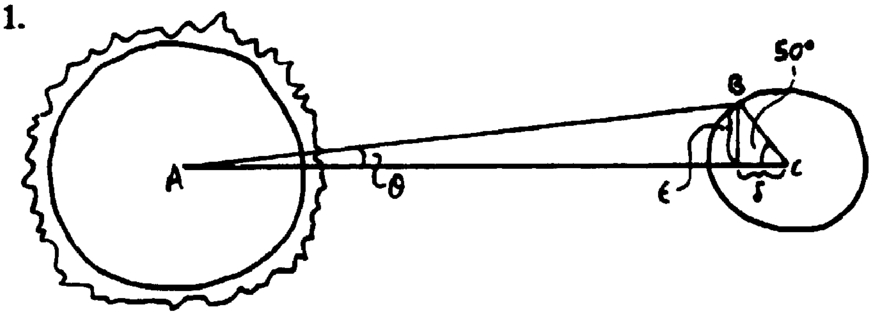
The calculations

constants (cgs units)

$r_{mc} = 3.844 \times 10^{10}$
$r_{es} = 1.496 \times 10^{13}$
$r_e = 6.378 \times 10^8$

$m_s = 1.96 \times 10^{33}$
$m_m = 7.36 \times 10^{25}$
$G = 6.670 \times 10\text{-}8$

Result 1.



$\delta = BC \cos 50^\circ = 4.09969923481 \times 10^8$ cm

$\epsilon = \sqrt{BC^2 - \delta^2} = 4.88583143211 \times 10^8$ cm

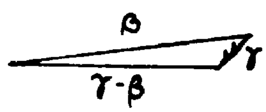
$\overline{AB}$ $\overline{AC} - \delta = 1.49525900301 \times 10^{13}$ cm

$\theta \approx \sin\theta \approx \tan\theta = \epsilon/(\overline{AC} - \delta) = 3.26601960500 \times 10^{-5}$ radians
$= 0.00187129139^\circ$

acceleration along $\overline{AB} = GM_s/(\overline{AB})^2 = .584174410812$ cm s^{-2}
acceleration along $\overline{AC} = GM_s/(\overline{AC})^2 = .584142383254$ cm s^{-2}

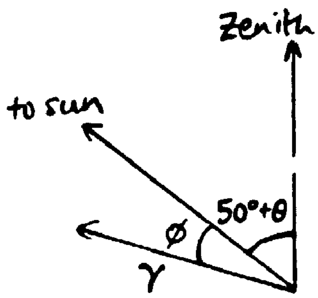
let vectorial acceleration along $\overline{AB} = \beta$, vectorial acceleration along $\overline{AC} = \beta - \gamma$.

vectorially calculate γ as follows: adopt coordinate system with B at origin, centre of sun A in negative x-direction:



coordinates of β are then (-.584174410812, 0),
and of $\gamma - \beta$, $.584142383254(\cos\theta, -\sin\theta)$
i.e. $\gamma - \beta = (.584142382942, -0.000019078205)$
so $\gamma = (-0.000032027870, -0.000019078205)$
so $|\gamma| = 3.729516 \times 10^{-5}$ cm s^{-2}.

Now resolve γ into vertical and horizontal components as follows.

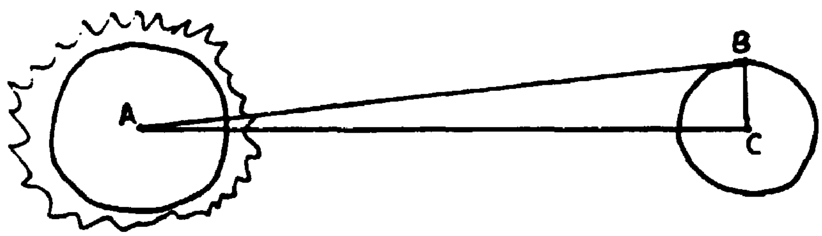


$\phi = \cos^{-1}(0.000032027870/0.000037279516)$
$= \cos^{-1}(0.859127824)$
$= 30.781207^\circ$

component of γ toward zenith
$= 3.7279516 \times 10^{-5} \cos(50^\circ + \theta + 30.781207^\circ)$
$= 3.7279516 \times 10^{-5} \cos 80.783078^\circ$
$= 5.971161 \times 10^{-6}$ cm s^{-2}

component of γ parallel to earth's surface
$= 3.7279516 \times 10^{-5} \sin 80.783078^\circ$
$= 3.67982 \times 10^{-5}$ cm s^{-2}

Result 2.



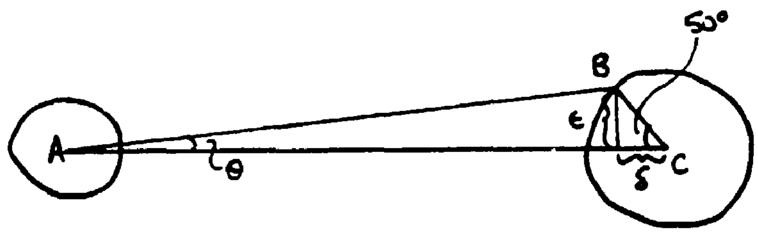
$\overline{AB}$, $\overline{AC}$ are to all intents and purposes equal
$\theta \approx \tan\theta = \overline{BC}/\overline{AC} = 4.263369 \times 10^{-5}$

vectorially subtract acceleration along $\overline{AC}$ from its scalar-equal acceleration along $\overline{AB}$; result is parallel to $\overline{BC}$

differential acceleration at B due to sun is then

$$GM_s/(\overline{AC})^2\tan\theta = 2.4904 \times 10^{-5} \text{ cm s}^{-2} \text{ (downward)}$$

Result 3.

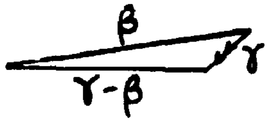


$\delta = BC\cos 50° = 4.099699234814179 \times 10^8$ cm
$\epsilon = \sqrt{\overline{BC}^2 - \delta^2} = 4.885831432104569 \times 10^8$ cm
$\theta = \tan^{-1}(\epsilon/(\overline{AC} - \delta)) = \tan^{-1}(0.01284747299)$
$= 0.7360555°$
$\overline{AC} - \delta = 3.803002946616702 \times 10^{10}$ cm
$\overline{AB} = (\overline{AC} - \delta)^2 + \epsilon^2 = 3.80268908489335 \times 10^{10}$ cm

acceleration along $\overline{AB} = GM_m/(\overline{AB})^2 = 3.3902483939922904 \times 10^{-3}$ cm s^{-2}
acceleration along $\overline{AC} = GM_m/(\overline{AC})^2 = 3.317771063137709 \times 10^{-3}$ cm s^{-2}

let vectorial acceleration along $\overline{AB}$ = ß, vectorial acceleration along $\overline{AC}$ = ß - γ.

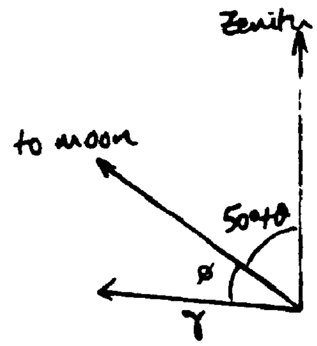
vectorially calculate γ as follows: adopt coordinate system with B at origin, centre of moon A in negative x-direction:



coordinates of ß are then (-3.390248393922904, 0),
and of γ - ß, $3.317710631\,37709 \times 10^{-3}$ (cosθ, -sinθ)
i.e. γ - ß = ($3.31749726 \times 10^{-3}$, $-4.26208507 \times 10^{-5}$)
so γ = ($-7.27511339 \times 10^{-5}$, $-4.26208507 \times 10^{-5}$)

so $|\gamma| = 8.43164539 \times 10^{-5}$ cm s^{-2}

Now resolve γ into vertical and horizontal components as follows.

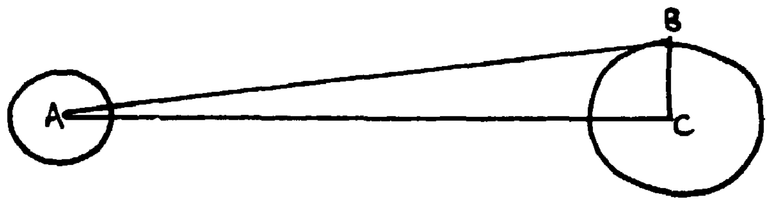


$\phi = \cos^{-1}(7.27511339/8.43164539)$
$= \cos^{-1}(.8628343643)$
$= 30.3636696°$

component of γ toward zenith
$= 8.43164539 \times 10^{-5} \cos(50° + \theta + 30.3636696°)$
$= 8.43164539 \times 10^{-5} \cos 81.0997269°$
$= 1.3045 \times 10^{-5}$ cm s^{-2}

component of γ parallel to earth's surface
$= 8.43164539 \times 10^{-5} \sin 81.0997269°$
$= 8.3301 \times 10^{-5}$ cm s^{-2}

Result 4.



$\overline{AB} = \sqrt{\overline{AB}^2 - \overline{BC}^2} = 3.8445291 \times 10^{10}$ cm
$\theta = \tan^{-1}(\overline{BC}/\overline{AC}) = 0.9505696°$

acceleration along $\overline{AB} = GM_m/(\overline{AB})^2 = 3.3168579 \times 10^{-3}$ cm s^{-2}
acceleration along $\overline{AC} = GM_m/(\overline{AC})^2 = 3.3177711 \times 10^{-3}$ cm s^{-2}

let vectorial acceleration along $\overline{AB} = \beta$, vectorial acceleration along $\overline{AC} = \beta - \gamma$.

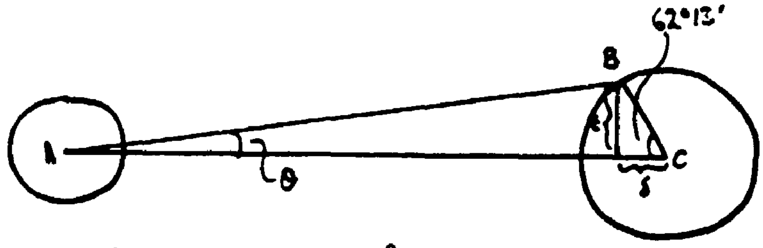
vectorially calculate γ as follows: adopt coordinate system with B at origin, centre of moon A in negative x-direction. Then

$\beta = (3.3177711 \times 10^{-3}, 0)$
$\gamma - \beta = 3.3168579 (\cos\theta, -\sin\theta)$
i.e. $\gamma - \beta = (-3.3164014 \times 10^{-3}, -0.055026 \times 10^{-3})$
$\gamma = (.13697 \times 10^{-5}, 5.5026 \times 10^{-5})$

so the vertical component of the differential acceleration at B due to the moon is 5.5026×10^{-5} cm s^{-2}

Note that there is also a small horizontal component to the differential acceleration at B due to the moon. This component is $.13697 \times 10^{-5}$ cm s^{-2}.

Result 5.



$\delta = \overline{BC}\cos 62°13' = 3.1680898 \times 10^8$ cm

$\epsilon = \overline{BC}^2 - \delta^2 = 5.5355299 \times 10^8$ cm

$\theta = \tan^{-1}(\epsilon/(\overline{AC} - \delta)) = \tan^{-1}(0.0145201)$

$= 0.8319411°$

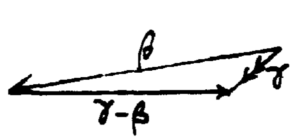
$\overline{AC} - \delta = \underline{3.81.23191 \times} 10^{10}$ cm

$\overline{AB} = \sqrt{(\overline{AC} - \delta)^2 + \epsilon^2} = 3.8212721 \times 10^{10}$ cm

acceleration along $\overline{AB} = GM_m/(\overline{AB})^2 = 3.3619227 \times 10^{-3}$ cm s^{-2}

acceleration along $\overline{AC} = GM_m/(\overline{AC})^2 = 3.3222850 \times 10^{-3}$ cm s^{-2}

let vectorial acceleration along $\overline{AB}$ = ß, vectorial acceleration along $\overline{AC}$ = ß - γ.



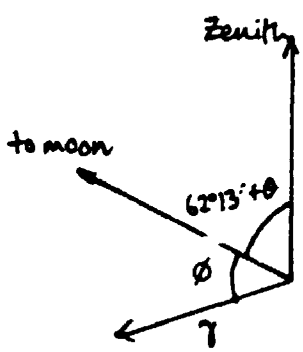
vectorially calculate γ as follows: adopt coordinate system with B at origin, centre of moon A in negative x-direction: coordinates of ß are then $(-3.3619227 \times 10^{-3}, 0)$, and of γ - ß, 3.3222850×10^{-3} (cosθ, -sinθ)

i.e. γ - ß = $(3.3219348 \times 10^{-3}, -4.82382 \times 10^{-5})$

so γ = $(-3.99879 \times 10^{-5}, -4.82382 \times 10^{-5})$

so |γ| = 6.265745×10^{-5} cm s^{-2}

Now resolve γ into vertical and horizontal components as follows.



$\phi = \cos^{-1}(3.99879/6.265745)$

$= \cos^{-1}(0.6381986)$

$= 50.3424°$

component of γ toward zenith

$= 6.265745 \times 10^{-5}\cos(62°13' + \theta + 50.3424°)$

$= 6.265745 \times 10^{-5}\cos 113.391°$

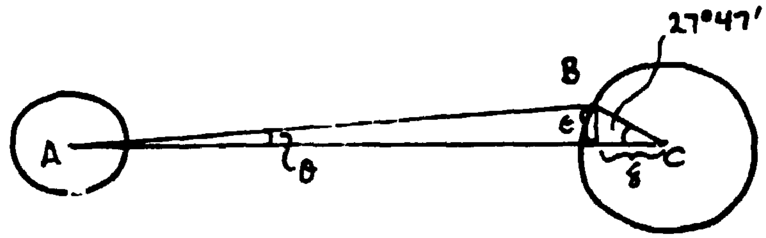
$= -2.4875 \times 10^{-5}$ cm s^{-2}

component of γ parallel to earth's surface

$= 6.265745 \times 10^{-5}\sin 113.391°$

$= 5.7508073 \times 10^{-5}$ cm s^{-2}

Result 6.



$$\delta = \overline{BC}\cos 27°47' = 5.64272231 \times 10^8 \text{ cm}$$

$$\epsilon = \sqrt{\overline{BC}^2 - \delta^2} = 2.9729728 \times 10^8 \text{ cm}$$

$$\theta = \tan^{-1}(\epsilon/(\overline{AC} - \delta)) = \tan^{-1}(0.007849282559)$$

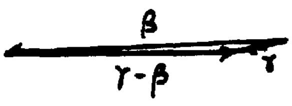
$$= 0.44972153°$$

$$\overline{AC} - \delta = 3.787572715875056 \times 10^{10} \text{ cm}$$

$$\overline{AB} = \sqrt{(\overline{AC} - \delta)^2 + \epsilon^2} = 3.7876894 \times 10^{10} \text{ cm}$$

acceleration along $\overline{AB} = GM_m/(\overline{AB})^2 = 3.4218024 \times 10^{-3}$ cm s^{-2}
acceleration along $\overline{AC} = GM_m/(\overline{AC})^2 = 3.3222850 \times 10^{-3}$ cm s^{-2}

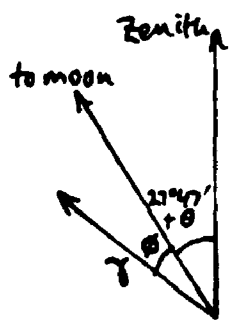
let vectorial acceleration along $\overline{AB}$ = ß, vectorial acceleration along $\overline{AC}$ = ß - γ.



vectorially calculate γ as follows: adopt coordinate system with B at origin, centre of moon A in negative x-direction:
coordinates of ß are then (-3.4218024, 0),
and of γ - ß, 3.3222850×10^{-3} (cosθ, -sinθ)
i.e. γ - ß = (3.3221827×10^{-3}, -2.60759×10^{-5})
so γ = (-9.95174×10^{-5}, -2.60759×10^{-5})

so $|\gamma| = 10.287694 \times 10^{-5}$ cm s^{-2}

Now resolve γ into vertical and horizontal components as follows.



$$\phi = \cos^{-1}(9.95174/10.287694)$$

$$= \cos^{-1}(0.9673441)$$

$$= 14.68275°$$

component of γ toward zenith

$$= 10.287694 \times 10^{-5} \cos(27°47' + \theta + 14.68275°)$$

$$= 10.287694 \times 10^{-5} \cos 42.915805°$$

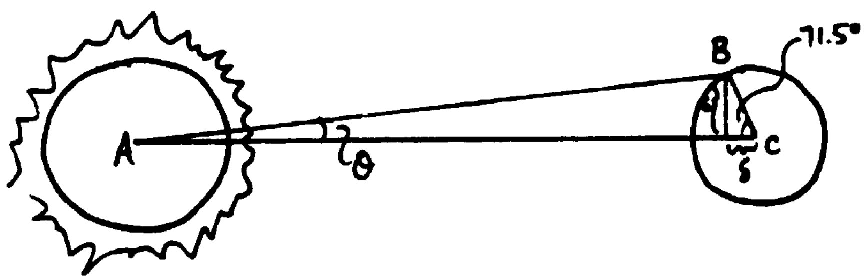
$$= 7.5432 \times 10^{-5} \text{ cm s}^{-2}$$

component of γ parallel to earth's surface

$$= 10.287694 \times 10^{-5} \sin 42.915805°$$

$$= 7.0051 \times 10^{-5} \text{ cm s}^{-2}$$

Result 7.



$\delta = \overline{BC}\cos 71.5 = 2.023769029557 \times 10^{8}$ cm

$\epsilon = \sqrt{\overline{BC}^2 - \delta^2} = 6.04840818016 \times 10^{8}$ cm

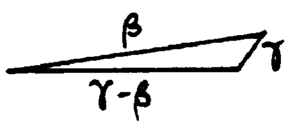
$\overline{AB} \approx \overline{AC} - \delta = 1.49597976231 \times 10^{13}$ cm

$\theta \approx \sin\theta \approx \tan\theta = \epsilon/(\overline{AC} - \delta) = 4.043108291 \times 10^{-5}$ radians
$= 0.00231653°$

acceleration along $\overline{AB} = GM_{\odot}/(\overline{AB})^2$
$= .584158198067$ cm s^{-2}

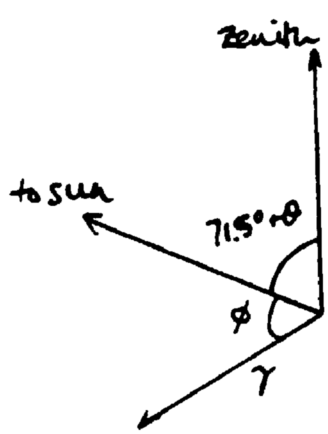
acceleration along $\overline{AC} = GM_{\odot}/(\overline{AC})^2$
$= .584142393344$ cm s^{-2}

let vectorial acceleration along $\overline{AB} = \beta$, vectorial acceleration along $\overline{AC} = \beta - \gamma$.



vectorially calculate γ as follows: adopt coordinate system with B at origin, centre of sun A in negative x-direction:
coordinates of β are then $(-.584158198067, 0)$,
and of $\gamma - \beta$, $.584142393344(\cos\theta, -\sin\theta)$
i.e. $\gamma - \beta = (.58414239287, -2.3617509530)$
so $\gamma = (-1.580520000, -2.3617509530)$
so $|\gamma| = 2.841814755 \times 10^{-5}$ cm s^{-2}.

Now resolve γ into vertical and horizontal components as follows.

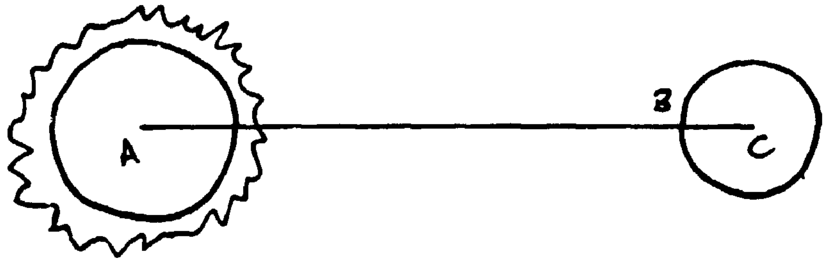


$\phi = \cos^{-1}(1.5805200/2.8418148)$
$= \cos^{-1}(0.55616575)$
$= 56.2098957°$

component of γ toward zenith
$= 2.8414148 \times 10^{-5}\cos(71.5° + \theta + 56.2098957°)$
$= 2.8414148 \times 10^{-5}\cos 127.71221°$
$= -1.7381 \times 10^{-5}$ cm s^{-2}

component of γ parallel to earth's surface
$= 2.8414148 \times 10^{-5}\sin 127.71221°$
$= 2.2478 \times 10^{-5}$ cm s^{-2}

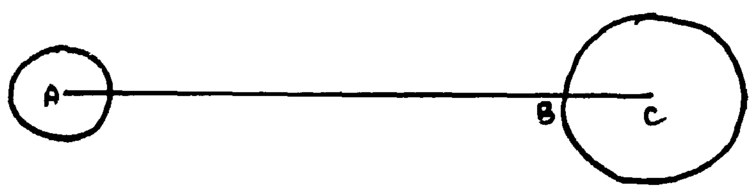
Result 8.



$\overline{AB} = 1.49593622 \times 10^{13}$ cm s^{-2}

differential acceleration at B $= GM_s/(\overline{AB})^2 - GM_s/(\overline{AC})^2$
$= 4.983 \times 10^{-5}$ cm s^{-2}, upward

Result 9.



$\overline{AB} = 3.78022 \times 10^{8}$ cm
differential acceleration at B $= GM_m/(\overline{AB})^2 - GM_m/(\overline{AC})^2$
$= 11.29 \times 10^{-5}$ cm s^{-2}

Analysis of Tides

Results 2 and 4 concern cases where the sun and moon respectively are approximately "on the horizon" from the perspective of B. Results 1 and 3 concern cases where, at the time of the equinox and at latitude 50 degrees, the sun reaches its highest point in the sky (very nearly 40 degrees up) or the moon reaches its average highest point in the sky. Results 8 and 9 concern cases where the sun and moon respectively are directly at zenith for some point on the earth's surface.

The configuration used below for calculating SL ("spring low") is sun "on horizon", moon "on horizon". The configuration used for calculating NL ("neap low") is sun at highest point in sky, moon "on horizon". The configuration used for calculating NH ("neap high") is sun "on horizon", moon at highest point in sky. The configuration used for calculating SH ("spring high") is sun at highest point in sky, moon at highest point in sky.

We principally examine, at some length, the latitude 50 degrees case at the time of the equinox.

For purposes of comparison, however, we first briefly examine the case concerning a point (in the tropics, clearly) that may see the sun and moon at zenith. For simplicity we take this point to be on the equator. In that case the time of year must be an equinox. (This ensures that, at the syzygies, the moon's highest point in the sky will also be zenith -- or within 5 degrees 9 minutes of zenith, the inclination of the moon's orbit to the ecliptic. At the quadratures the moon's highest point in the sky will be $23°27' \pm 5°9'$ from zenith. For the purposes of the present calculation we ignore all this -- effectively we assume that the ecliptic, equator, and moon's orbit lie all in one plane.) Here we treat the seas as responsive to the vertical components only of the differential accelerations due to the moon and sun. We also assume, for the moment, that the seas are ideally responsive to these accelerations.

*Equinoctal tides at the equator, ideal case (vertical components)**

configuration	*combined effect of sun and moon*
SL	-7.9930×10^{-5} cm s^{-2}
NL	-0.5189×10^{-5} cm s^{-2}
NH	8.7995×10^{-5} cm s^{-2}
SH	16.274×10^{-5} cm s^{-2}

$$(SH - SL)/(NH - NL) = 2.604 \approx 2.67$$

---

*In the present case the calculation simply cannot be re-preformed using horizontal components -- the horizontal components are all zero in Results 2, 5, and 6. That the horizontal component is not zero in Result 4 -- concerning the moon when angle BCA = 90 degrees -- and that the vertical component in Result 4 is thus slightly reduced, apparently however does not explain why (SH - SL)/(NH - NL) is less than the expected 2.67. If, rather than the vertical component 5.5026×10^{-5} cm s^{-2} in Result 4, we use the total differential acceleration 5.5043×10^{-5} cm s^{-2}, our table becomes

configuration	*combined effect of sun and moon*
SL	-7.9955×10^{-5} cm s^{-2}
NL	-0.5206×10^{-5} cm s^{-2}
NH	8.7995×10^{-5} cm s^{-2}
SH	16.274×10^{-5} cm s^{-2}

$$(SH - SL)/(NH - NL) = 2.604.$$

The difference perhaps does partly concern the geometrical difference between the cases of the sun and moon when angle BCA = 90 degrees. The moon is nearly a degree below the horizon when angle BCA = 90 degrees. Also, and I believe more significantly, L:S (as determined by the ratios of magnitudes of the differential accelerations in Results 2 and 4 (2.210), and Results 5 and 6 (2.265)) is not exactly 2.2 (giving rise to the expectation that (SH - SL)/(NH - NL) should equal 2.65 according to Results 2 and 4, or 2.58 according to Results 5 and 6). The match between the result 2.604 and expectations is certainly close.

We now turn to our sustained examination of the latitude-50 case. We first briefly look at horizontal components, ideal case. Turning then to the ideal case of vertical components, we see that the calculated ratio (SH - SL) /(NH - NL) is very nearly the same in this case as it is in the ideal case concerning horizontal components.

Equinoctal tides at latitude 50 degrees (horizontal components), ideal case

configuration	*combined effect of sun and moon*
SL	-0.1370×10^{-5} cm s^{-2}
NL	3.5428×10^{-5} cm s^{-2}
NH	8.3301×10^{-5} cm s^{-2}
SH	12.0099×10^{-5} cm s^{-2}

$$(SH - SL)/(NH - NL) = 2.79 \approx 2.67$$

We now recalculate, using vertical components.

Equinoctal tides at latitude 50 degrees (vertical components), ideal case

configuration	*combined effect of sun and moon*
SL	-7.9930×10^{-5} cm s^{-2}
NL	-4.9055×10^{-5} cm s^{-2}
NH	-1.1859×10^{-5} cm s^{-2}
SH	1.9016×10^{-5} cm s^{-2}

$$(SH - SL)/(NH - NL) = 2.66 \approx 2.67$$

(From results 1 and 3 we can calculate $L_{vertical}/S_{vertical} = 2.185$, $L_{horizontal}/S_{horizontal} = 2.264$, $L_{resultant}/S_{resultant} = 2.26$ -- all approximately 2.2, all leading to an expected value for (L + S)/(L - S) = (SH - SL)/(NH - NL) of approximately 2.67.)

With Newton, we will from now on consider vertical components only, in the conviction that this in no way seriously affects the determination of the ratio L/S.

Newton's corrections

(1) Because at the true time of the observed SL, NL, NH, SH, the sun-earth-moon angle differs by $18^1/_2$ degrees from that on which we based our calculations, Newton urges that we correct the differential accelerations due to the sun by cos37°=0.7986355. An independent check (Result 7) shows that Newton's correction is indeed approximately valid:

sun "on horizon"	-2.4904
sun $18^1/_2$ degrees away from horizon	
Newton would use	-1.9889
correct value (Result 7)	-1.7381
sun at highest pt. in sky	5.9712
sun $18^1/_2$ degrees away from highest pt. in sky	
Newton would use	4.7689

(2) When, at the time of the equinox, the moon is 18.5° past the quadratures, Newton says that it declines from the equator by approximately 22°13'. This assertion is correct. (At the time of the equinox, the moon at the *syzygies* can decline from the equator no more than the inclination of the plane of the moon's orbit to the ecliptic -- 5°9'. This angle is small, and Newton rightly ignores it: he applies no correction in the syzygies. Supposing, then, as is approximately correct, that the moon's orbit is inclined not at all to the ecliptic, then at the time of the equinox, the moon at the quadratures will decline from the equator by 23°27', the inclination of the earth's axis to the ecliptic. Moreover, the moon's declination from the equator will vary sinusoidally from zero at syzygy to +23°27' at one quadrature to zero at syzygy to -23°27' at the other quadrature. Thus at $18^1/_2$° past the quadratures the moon declines cos$18^1/_2$° times 23°27', that is, 22°13', from the equator.)

When the moon is "on the horizon" (Result 4), no correction is required owing to the declination of the moon from the equator. For the angle BCA will be 90° twice per day regardless of any declination of the moon from the equator.

When the moon is at its highest point in the sky, Newton urges that we correct as follows for the declination of the moon from the equator. He suggests, correctly, that the influence on the tides of differential accelerations due to the moon are reduced as the cosine squared of the moon's declination to the equator -- that is, by a factor of 0.8570327. Thus Newton implies that, rather than the figure 1.3045 from Result 3, we should use the figure 1.1180 for the differential acceleration due to the moon in this case.

This consideration, however, takes no account ot a further, very significant, fact. This is, that with the moon declined to the equator, one high tide each day will be *increased* in size, and the other *decreased* in size, for reasons Newton himself sets out in the discussion following Proposition XXIV (see especially p. 438). The reason is that, when the moon is at quadratures, the morning maximal differential acceleration due to the moon felt by a point at latitude 50° will differ from the evening maximal differential acceleration due to the moon felt by the same point. One of these will equal that of a point seeing the moon 62°13' above the horizon, the other will equal that of a point seeing the moon 17°47' above the horizon. I have calculated these in Results 5 and 6. The results (for vertical components) are summarized here:

moon at highest pt. in sky		1.3045
Newton would simply use		1.1180
the average, however, of the following two figures is relevant:		
	27°47'	7.5432
	62°13'	-2.4875
average:		2.5279
corrected average:		2.1665

(3) Newton calculates that, because the moon is on average further from the earth in syzygies than in quadratures, we should apply corrections 0.98 in the syzygies and 1.02 in the quadratures. (Newton had of course calculated very closely the deviations from Keplerian motion of the orbital motion of the moon, and in particular, had closely determined how the disturbing influence of the sun affects the earth-moon distance over the four quadrants of the moon's orbit. When Newton corrects, therefore, for systematic differences in the moon's distance at syzygies and quadratures, he knows what he is talking about. The corrections are not fudges!)

Newton's own corrections would thus alter our previous table as follows:

Equinoctal tides at latitude 50 degrees (vertical components), incorporating corrections for non-ideality

configuration		*combined effect of sun and moon*
SL	$(0.98 \times -5.5026) + (-1.9889) =$	-7.3814×10^{-5} cm s^{-2}
NL	$(1.02 \times -5.5026) + (0.4769) =$	-5.1358×10^{-5} cm s^{-2}
NH	$(1.02 \times 1.1180) + (-1.9889) =$	-0.8485×10^{-5} cm s^{-2}
SH	$(0.98 \times 1.3045) + (0.4769) =$	1.7553×10^{-5} cm s^{-2}

$$(SH - SL)/(NH - NL) = 2.131 \not\approx 1.8 \text{ even very roughly}$$

However, in the spirit of Newton's corrections, but using our more accurate figure relating to correction 1 and our averaged figure relating to correction 2 (assuming that the 25-foot rise reported by Sturmy was the average of the two neap-tide rises that day), we may rather write:

Equinoctal tides at latitude 50 degrees (vertical components), incorporating corrections for non-ideality

configuration		*combined effect of sun and moon*
SL	$(0.98 \times -5.5026) + (-1.7381) =$	-7.1306×10^{-5} cm s^{-2}
NL	$(1.02 \times -5.5026) + (0.4769) =$	-4.3333×10^{-5} cm s^{-2}
NH	$(1.02 \times 2.1665) + (-1.7381) =$	0.4717×10^{-5} cm s^{-2}
SH	$(0.98 \times 1.3045) + (0.4769) =$	1.7553×10^{-5} cm s^{-2}

$$(SH - SL)/(NH - NL) = 1.849 \approx 1.8$$

We next examine how each of the three corrections separately affects the ratio (SH - SL)/(NH - NL) (equal, as we have seen, to 2.66 uncorrected). We see from these calculations that corrections 1 and 3 diminish the calculated ratio (SH - SL)/(NH - NL), and so bring this ratio closer to the observed 1.8. Correction 2, as Newton proposes to apply it, slightly increases the calculated

ratio, but according to our own more detailed calculations, a correction for the moon's declination from the equator should decrease the calculated ratio.

Equinoctal tides at latitude 50 degrees (vertical components), applying correction 1 only

configuration	*combined effect of sun and moon*
SL	-7.4915×10^{-5} cm s^{-2}
NL	-4.7853×10^{-5} cm s^{-2}
NH	-0.6844×10^{-5} cm s^{-2}
SH	2.0218×10^{-5} cm s^{-2}

(SH - SL)/(NH - NL) = 2.320

Equinoctal tides at latitude 50 degrees (vertical components), applying correction 2 only -- Newton's version

configuration	*combined effect of sun and moon*
SL	-7.9930×10^{-5} cm s^{-2}
NL	-4.9055×10^{-5} cm s^{-2}
NH	-1.1964×10^{-5} cm s^{-2}
SH	2.0218×10^{-5} cm s^{-2}

(SH - SL)/(NH - NL) = 2.700

Equinoctal tides at latitude 50 degrees (vertical components), applying correction 2 only -- our version

configuration	*combined effect of sun and moon*
SL	-7.9930×10^{-5} cm s^{-2}
NL	-4.9055×10^{-5} cm s^{-2}
NH	-0.3239×10^{-5} cm s^{-2}
SH	2.0218×10^{-5} cm s^{-2}

(SH - SL)/(NH - NL) = 2.186

Equinoctal tides at latitude 50 degrees (vertical components), applying correction 3 only

configuration	*combined effect of sun and moon*
SL	-7.8829×10^{-5} cm s^{-2}
NL	-5.0155×10^{-5} cm s^{-2}
NH	-1.1598×10^{-5} cm s^{-2}
SH	1.8755×10^{-5} cm s^{-2}

(SH - SL)/(NH - NL) = 2.531

Conclusions. Newton's approach to determining the ratio L:S of lunar to solar influences on the tides was brilliantly conceived. His data were well chosen: by examining the equinoctal case, Newton avoided the problem of correcting for declination of the sun from the equator. However, Newton realized that the physical considerations relating even to these well-chosen data are complex. Newton believed rightly that corrections were *not* necessary for latitude, or for absolute heights of tides -- he rightly reasoned that the ratio L:S is independent of latitude, or of local conditions determining absolute heights of tides. Also, the relation of the ratio L:S to the observable ratio (SH - SL)/(NH - NL) is almost completely insensitive to whether vertical or horizontal differential accelerations determine the tides. So Newton's reasoning was in no way compromised by his false conviction that only vertical components determine the tides.

Newton reasoned that three corrections were important. The physical relevance of these corrections is incontestable. The determination from them of specific correction factors involves no erroneous physics, although in the case of one correction, concerning the average equinoctal declination of the moon-in-quadratures from the equator, Newton's way of calculating the changed average effect of the moon is faulty. Jointly Newton's corrections bring the ratio (SH - SL)/(NH - NL) for the Bristol tides that may be calculated from known modern values for the relevant physical constants, within the general ballpark of the observed 1.8.

Appendix 2

Einstein's Inference to the Kinematics of Special Relativity was Newtonian Deduction from Phenomena

(A Geometrical Presentation)

Immediately following the logically superfluous (but, for Einstein, heuristically and psychologically significant) first section of "On the Electrodynamics of Moving Bodies" (the section in which Einstein discusses the light-signals operational definition of simultaneity), Einstein sets out the basis for his deduction as follows:

> The following reflections are based on the principle of relativity and on the principle of the constancy of the velocity of light. These two principles we define as follows:--
>
> 1. The laws by which the states of physical systems undergo change are not affected, whether these changes of state be referred to the one or the other of two systems of coordinates in uniform translatory motion.
> 2. Any ray of light moves in the "stationary" system of coordinates with the determined velocity c, whether the ray be emitted by a stationary or by a moving body.

I shall call 1 and 2 "Einstein's guiding notions". I shall follow Einstein in calling the first notion the principle of relativity, even though the name "principle of invariance", for which Einstein later expressed a preference, is no less apt.

As is well known, in classical mechanics 1 holds good for all coordinate systems adapted to frames in which in their most simple, coordinate-dependent expression the laws of mechanics hold good, that is, for all "inertial" frames. In classical electrodynamics, however, this principle does not hold good. For in classical electrodynamics one meets with cases in which the laws one must choose in order to explain electrodynamical phenomena vary according to the choice of reference frame. From early days Einstein noted this fact, and attached considerable significance to it. Einstein, as we have seen, was among a handful of physicists for whom the question of the relation of mechanics to electrodynamics was moot. And in considering this question himself, it did not seem right to Einstein that one half of physics should float free from a principle so deep within the other half. Einstein was guided by the felt need to reconcile Maxwell's equations with the first guiding notion. In this work, Einstein fastened upon the second guiding notion (2), the principle of the constancy of the speed of light, which states that the speed of electromagnetic radiations in a vacuum has a constant value, irrespective of the relative motion of their source. This surprising notion was an immediate result of combining Maxwell's equations with the relativity principle.

We have seen (in section 5 of Chapter 5) how the second guiding notion led Einstein to the conclusion that in the classical axiomatic expression of Maxwell's theory, time is a suspect concept, and how he came thus to write the superfluous first section of his paper, and in a hasty presentation obscure the true form of his deduction. Now, as I have said, I wish to consider first not the hasty formulation Einstein gave to special relativity theory in 1905, but rather the geometrical reformulation of this theory given by Minkowski in 1908. In Minkowski's geometrical terms, the form of Einstein's deduction can be most perspicuously presented. In keeping with my pedagogical purpose, I shall approach the Minkowski formulation by simple steps, starting with the notion of a co-ordinate system. Co-ordinate systems can be used to describe the frames of reference with which relativity theory is concerned. By exploring the notion of a co-ordinate system we can forge a link between geometry, and Einstein's results concerning space, time and motion.

Co-ordinate systems are like grids on maps. When we picture co-ordinate systems it is easy to think of graphs. But co-ordinate systems are not graphs, and it will be useful, in order to clarify the concept of a co-ordinate system and later to introduce some ideas from relativity theory, to outline the differences that there are between co-ordinate systems and graphs.

Figure 1 (overleaf) is a picture of a graph. Like all graphs it is a plot of the relationship of variables; in this case, a distance variable (the distance, in a specific direction, that an object is from some reference position) and a time variable (the time elapsed from some reference time). Very often in the case of two variables the value of one variable depends on the value of the other; when this is so, the first is called the dependent variable, and the second the independent variable, and generally the independent variable is plotted along the horizontal axis. In Figure 1, time is plotted along the horizontal axis. This is because every physical object satisfies the property that it can be at one and the same place at two different times, but it cannot be one and the same time be at two different places. Thus the temporal position of an object is independent of its spatial position, but the spatial position of an object depends on its temporal position. This, at least, is the idea behind plotting time along the horizontal axis.

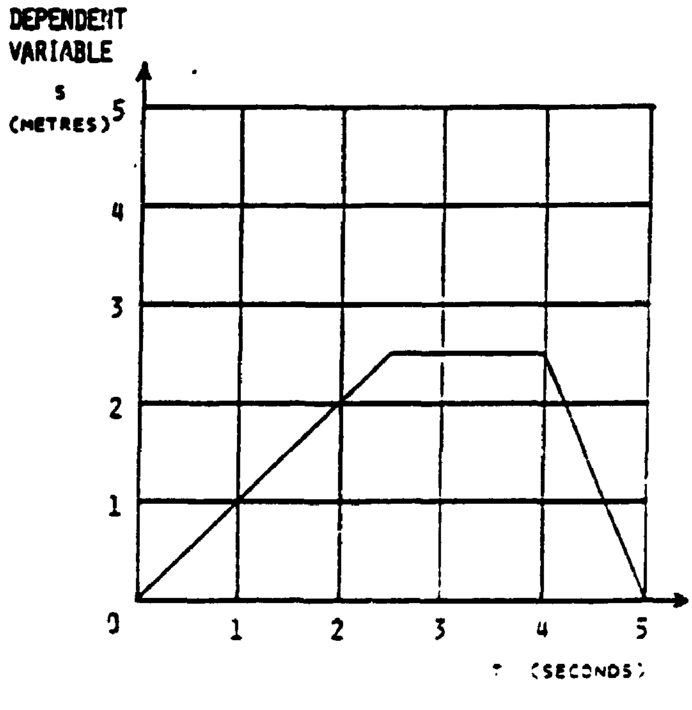


Figure 1

But in the context of a discussion of Einstein's first guiding notion, one must be suspicious of this reasoning, and in particular, suspicious of the notion of an object's being in one and the same place at two different times. For whether or not an object is at one and the same place at two different times depends entirely on the reference frame that is chosen for describing the position of that object. I am in the same place as I was in two second ago in the reference frame of this room, but not in other reference frames, say that of a car passing outside or an aircraft passing overhead. Einstein's first guiding notion calls into question the reasoning on the basis of which time is said to be an independent variable. In Figure 2 the graph is redrawn so as to remind us no longer to think of time as an independent variable.

Relativity theory not only teaches us not to think of time as an *independent* variable; it also teaches us (though this is jumping ahead historically somewhat) not to think of time as a *variable* at all, but rather as a fourth dimension over and above the three dimensions of space. If time is thought of in this way, the line in Figure 2 cannot be a plot of the relationship of two variables, but must be regarded instead as the *world-line* of an object in a two-dimensional region of space-time. If we consider Figure 2 in this way we are well on the way to seeing it not as a graph, but as a co-ordinate system, describing a two-dimensional region of space-time. But there are two features of Figure 2 which prevent its properly being regarded as a co-ordinate system.

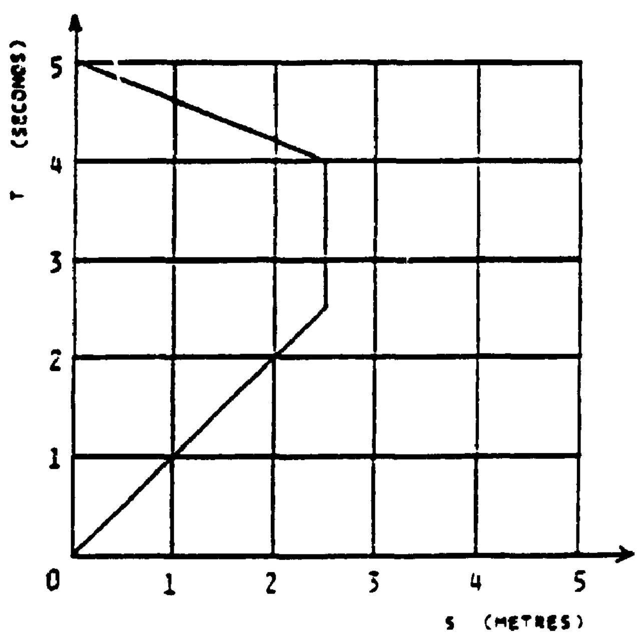


Figure 2

One problem, which is really very important since the co-ordinate systems we want to draw are to help us to understand the special theory of relativity, is that the world-lines of fast-moving objects (with speeds significant with respect to the speed of light) cannot be depicted satisfactorily in Figure 2. They would all be indistinguishable from horizontal lines. Another, more serious, problem, stems from the fact that a co-ordinate system helps us to locate points in a *region*. In any region, it ought to be possible to make sense of the notion of the magnitude of the separation of any two points in that region. But this is not possible in Figure 2. We can say, in Figure 3, that the separation of points A and B is two metres, and that the separation of points B and C is two seconds. But what the separation of points A and C is, we cannot say. So we have not yet depicted a co-ordinate system.

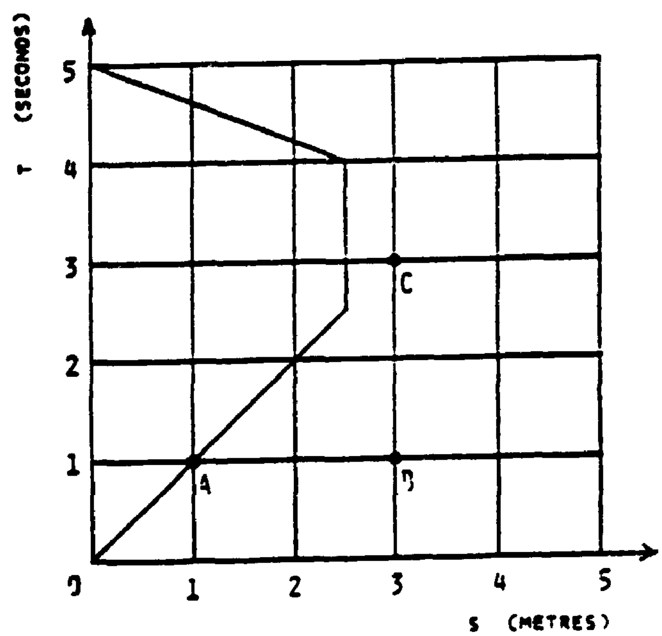


Figure 3

Now both of these problems can be solved if we follow physicists in performing a couple of tricks. The basis for performing these tricks will be indicated later. The tricks are shown in Figure 4, and relate to Einstein's connecting the concepts of space and

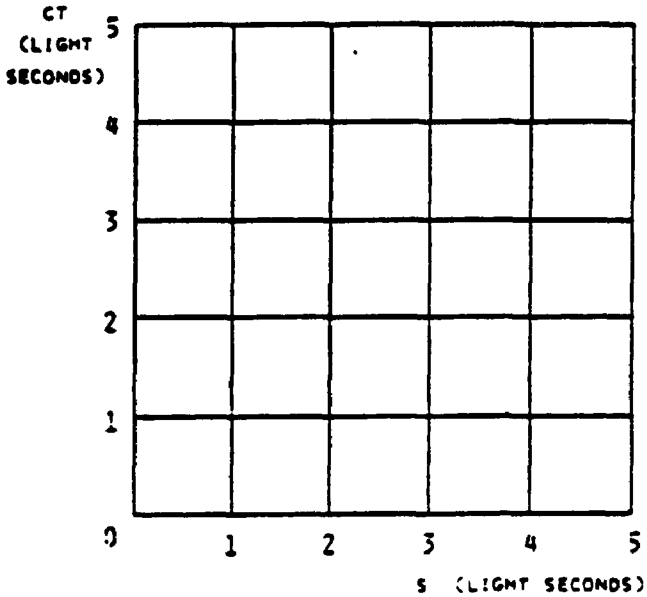


Figure 4

time to the theory of electromagnetism. First, the units along the horizontal (distance) axis have been greatly expanded. They were metres before, and now are light-seconds, or units of approximately 300 000 000 metres. This allows us to depicts world-lines of fast-moving objects. In Figure 5 the world-line of a photon is depicted. Objects traveling at less than the speed of light will have steeper world-lines than this, and objects, if there were any, traveling faster than the speed of light, would have relativity less steep world-lines. The other trick employed in Figures 4 and 5 is multiplication of what is measured up the vertical axis (time) by the speed of light, *c*. This converts the units along this axis to light-seconds, the very same units as

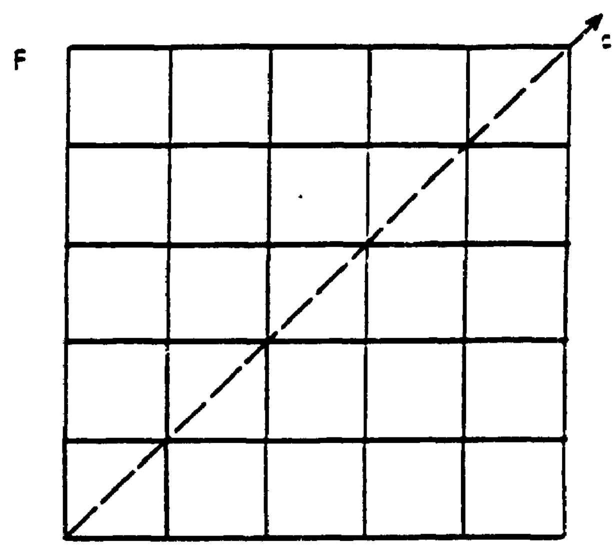


Figure 5

are along the horizontal (distance) axis. This solves the problem about measuring the separation of points in the region. Whereas one can say that no obvious *metric*, or mathematical device for measuring the separation of points, suggests itself in the case of Figures 2 and 3, this is not so now. In Figures 4 and 5 we can employ the standard Pythagorean metric for determining the magnitude of the separation of any two points, and express the result in light-seconds. What is depicted in Figures 4 and 5 can now properly be regarded as a co-ordinate system (a co-ordinate system which describes the frame of reference *F*).

In Figure 6 the world-line of an object traveling at half the speed of light relative to reference frame *F* is depicted on the co-ordinate system describing *F*. This object defines its own stationary frame *F'*, which moves with it at half the speed of light relative to *F*. The project we shall now undertake is to depict on the graph-paper lines of the co-ordinate system describing *F*, the graph-paper lines of the co-ordinate system

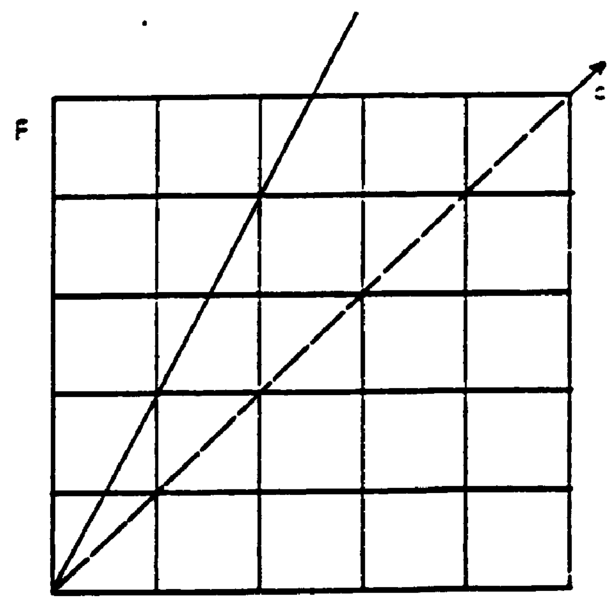


Figure 6

describing F'. For then we will be in a position to compare descriptions of physical systems from the point of view of F with descriptions of the same physical systems from the point of view of F'. This will allow us, then, to "do" some special relativity.

We can begin with the observation that any object which is stationary in F will have a vertical world-line in F. Thus an object at (0,0) and stationary in F will have (0,1), (0,2), (0,3) etc. as points on its world-line, and thus will have a vertical world-line in F. By the same reasoning, any object which is stationary in F' will have a vertical world-line in F'. But of course the very object whose world-line we are considering is stationary in F', since F' is its stationary frame. Thus we can take its world-line to be the vertical axis of the co-ordinate system describing F'. In Figure 7 it has been labelled accordingly.

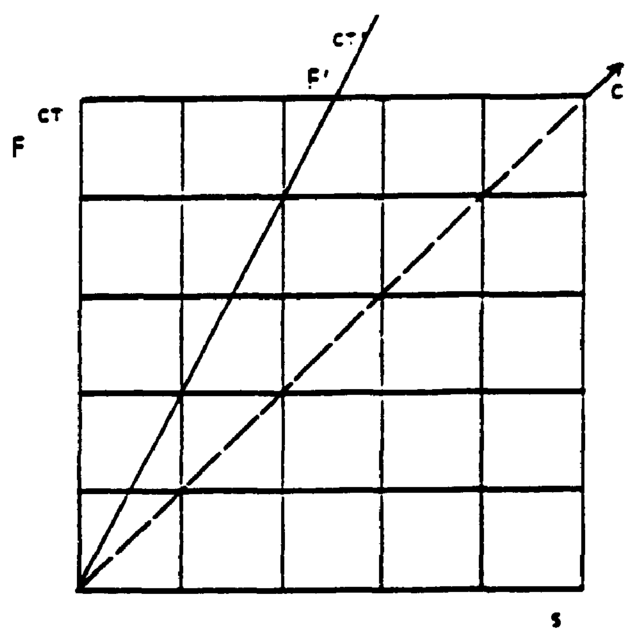


Figure 7

It remains to determine the direction of the horizontal axis and the length of the units along both axes of the co-ordinate system describing F'. Let us assume that the units along both axes of the co-ordi nate system describing F' will be the same length. (Here we make use of the innocuous high-level theoretical constraint that appropriate coordinate transformations will be linear. Any assumption to the contrary would introduce tremendous complexities into the mathematical transformations taking descriptions of physical systems from the point of view of F onto descriptions of the same physical systems from the point of view of F'.) On this assumption, Einstein's second principle ((2), above) determines that the "horizontal" axis of the co-ordinate system describing F' will be a reflection, about the diagonal in F, of the "vertical" axis of the co-ordinate system describing F' (Figure 8.) For only in this case will the speed of a photon traveling at c in F be c in F' also.

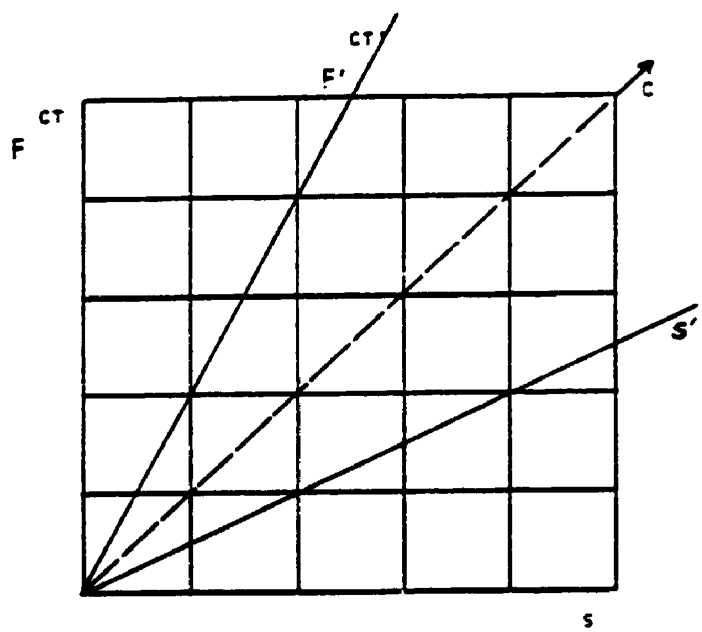


Figure 8

In order to complete the graph paper lines of the co-ordinate system describing F', we require to know the length of the units within the new co-ordinate system. Now since our origins in F and F' coincide, we know that x', ct' must be related to x, ct in such a way that the space-time points determined by $x^2 = c^2t^2$ (this corresponding to the equation $x^2 + y^2 + z^2 = c^2t^2$ for a light sphere expanding about the origin in *F*) are also determined by $x'^2 = ct'^2$ (this corresponding to a light-sphere expanding about the origin in *F'* -- that is, the very same light-sphere as is expanding about the origin in *F*). Now the simplest relation between x', ct' and x, ct that meets this requirement is that in which $c^2t^2 - x^2$ and $c^2t'^2 - x'^2$ (both equal to zero over the points just discussed) in fact have the same value *everywhere* in the region. We shall assume that this is the case. Once again, this assumption is based on our innocuous high-level theoretical assumption that the appropriate transformations are linear. Any other assumption would introduce tremendous complexities into the mathematical transformations taking descriptions of physical systems from the point of view of *F* onto descriptions of the same physical systems from the point of view of F'.

So we have that $c^2t^2 - x^2$ and $c^2t'^2 - x'^2$ have the same value everywhere in the region. In particular, the point $(x', ct') = (0,1)$ must lie on the hyperbola defined by $c^2t^2 - x^2 = 1$. Thus by drawing this hyperbola (Figure 9) and taking its point of intersection with the ct' axis of *F'*, we can determine the length of the units for the graph-paper

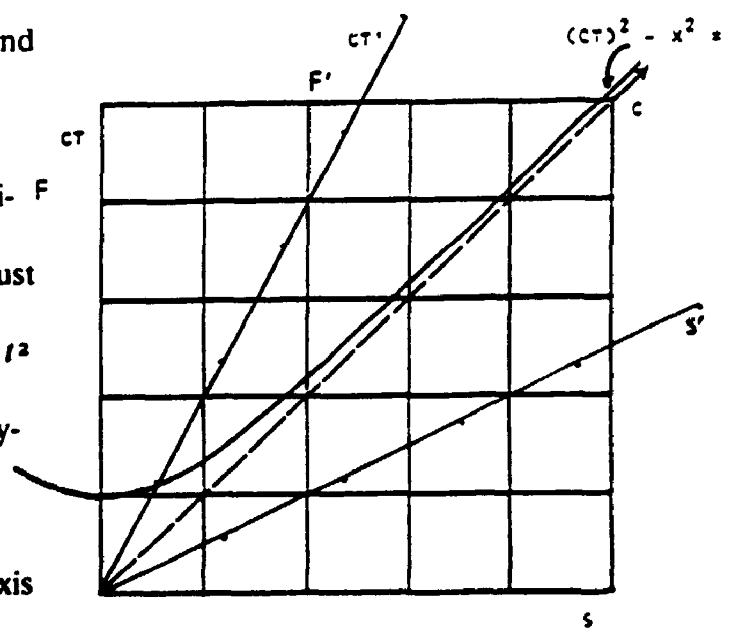


Figure 9

squares of the co-ordinate system describing *F'*.

Now the hyperbola depicted in Figure 9 has this particular shape because the unit along both axes has been chosen in a manner sensitive to the value of *c*, and the equation determining the shape of the hyperbola includes *c*. If we were to leave the units along both axes as they are, but were to imagine the speed of light to be increased to twice its actual value, the hyperbola would be flattened out somewhat, as shown in Figure 10. In the case where the imagined value of the speed of light is ten times its actual value, the hyperbola appears as in Figure 11. In the limit as the imagined value of the speed of light is allowed to increase without bound, the hyperbola becomes indistinguishable from the

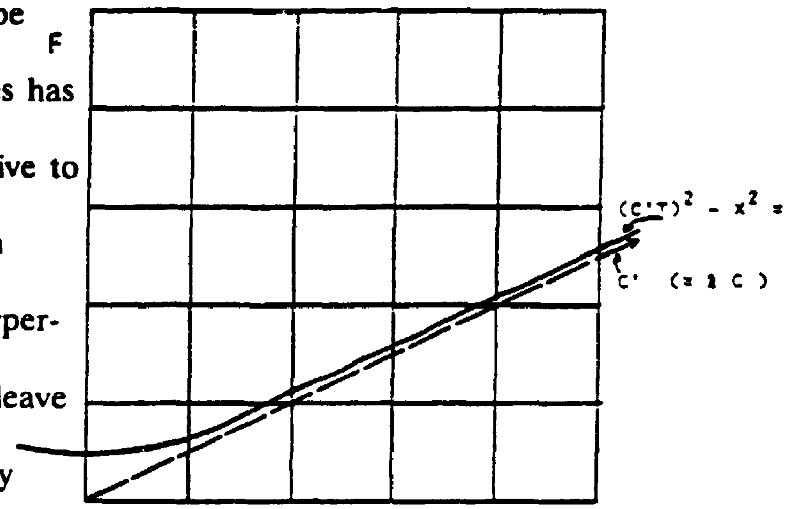


Figure 10

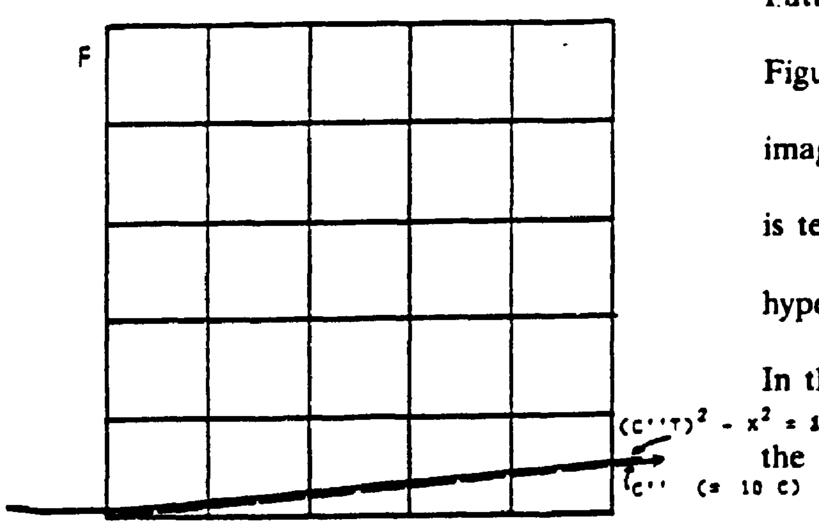


Figure 11

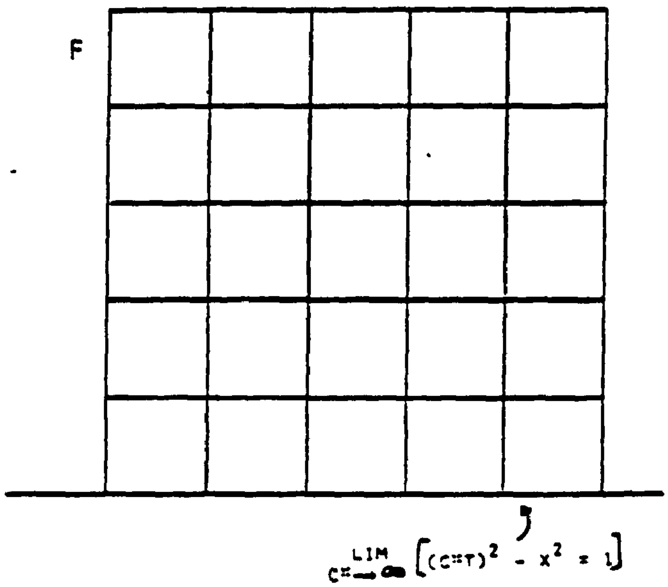


Figure 12

horizontal axis (Figure 12). Clearly, therefore, the line of thought we are following requires that the value of c is finite.

We are finally in a position to complete the graph-paper squares of the co-ordinate system describing F' (Figure 13), and hence to compare descriptions of physical systems from the point of view of F with descriptions of the same physical systems from the point of view of F'. Without getting technical, we will see that we have, in effect, deduced, or "forced out", the Lorentz transformations. Thus in Figure 14 (p. 298) segments of the histories of three separate objects are depicted, and the characteristics they manifest in the two different frames of reference can be read off from the graph-

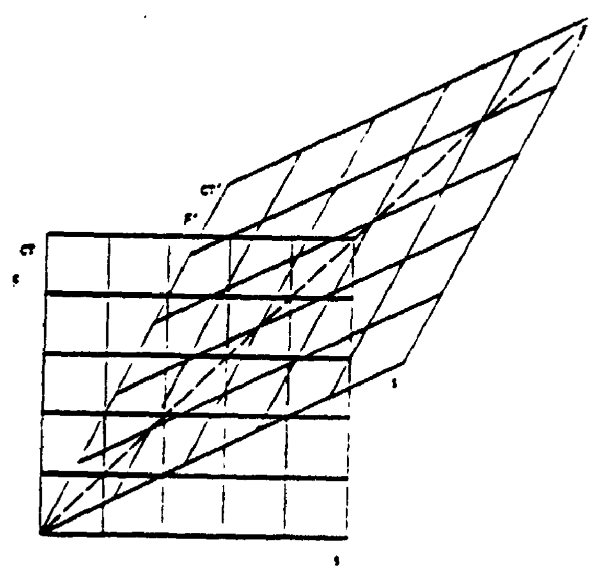


Figure 13

paper lines of the two co-ordinate systems. AB and EG depict objects which are traveling at half the speed of light in *F* and are stationary in *F'*, and CD depicts an object stationary in *F* and traveling "backwards" at half the speed of light in *F'*. Note that the time-like length of AB is two seconds in *F*, but is less than two seconds in the object's stationary frame, *F'*. This is an instance of

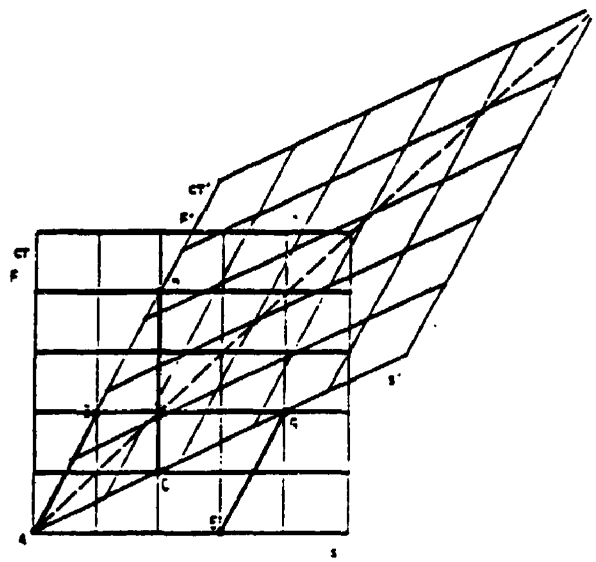


Figure 14

time dilation, and we could, simply by using some analytic geometry, show that the dilation factor is the familiar $1 - v^2/c^2$ found in the traditional treatments of special relativity. It is important to note that the time-like length of CD is similarly shorter in the stationary frame of that object, which in this case is *F*. The time-like length of CD is three seconds in *F*, and more than three seconds in *F'*. Again less time has elapsed in the stationary frame, so that "the moving clock runs slow". The dilation factor can be shown to be the same familiar $1 - v^2/c^2$. Thus we have ended up with a certain symmetry of transformations, which in fact is a requirement of Einstein's first principle.

At the points B and G in Figure 14 we have, in F, the simultaneous arrival of two objects at two separate points in space. But these events are not simultaneous in F'. The events occur at t equals two seconds in F, but at separate times t' equals 1.6 and t' equals zero seconds in F'. This is an instance of the relativity of simultaneity, an especially fundamental result as Einstein first formulated special relativity theory in 1905.

Now these are some of the bizarre results of the special theory of relativity, results which made it offensive to common sense. But in the light of the foregoing geometrical portrayal of these results, they should no longer seem so bizarre. For they can now be seen as consequences of geometrical relationships that exist, and should be expected to exist, between descriptions in alternat co-ordinate systems. Whenever two co-ordinate systems are employed in describing one and the same set of physical facts, the two sets of descriptions will be related by geometrical laws. If Fred chooses a co-ordinate system for describing the streets of London, Ontario, and Felix chooses a co-ordinate system for describing the streets of London, Ontario, then Fred's descriptions will be related to Felix's by geometrical laws. Our Minkowskian, geometrical presentation of special relativity simply shows that descriptions in alternative co-ordinate systems are related by geometrical laws even in the case in which the co-ordinate systems describe frames of reference in motion with respect to one another.

Another supposedly bizarre phenomenon of special relativity is known as "length contraction", and Minkowski's geometrical insight makes short work of explaining it. In Figure 15 the world-paths of two spatially extended objects are depicted. One object is stationary in F, the other in F'. Both objects have spatial extensions of less than one light second in the frame of reference in which they are not at rest.

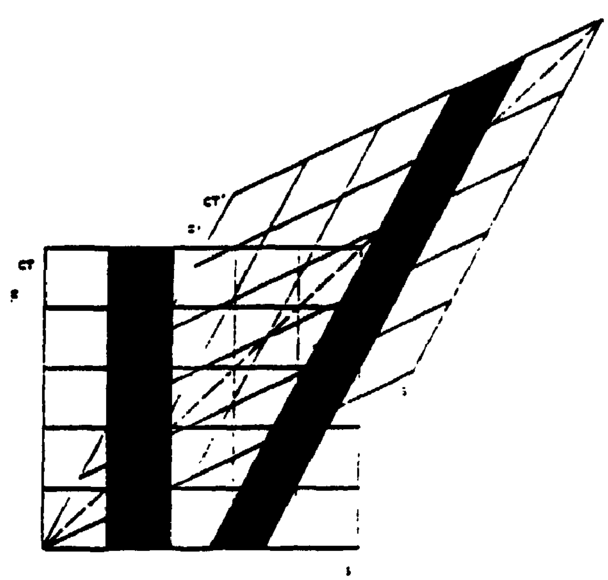


Figure 15

This can be determined by examining the x' and x axes, respectively. The "contraction" factor in each case can be shown to be the familiar $1 - v^2/c^2$, but it can be seen that both objects' world-paths are simply *there*, and there has been no "contraction" at all.

The example of Fred and Felix can be employed to help make explicit the nature of the geometrical relationship of descriptions in F and F'. The important point is that the mathematical transformations that take Fred's descriptions onto Felix's are precisely the transformations that take Fred's co-ordinate axes onto Felix's. In the simplest case, where they use the same units and the handedness of the axes is the same, the transformation will be rotations and translations of co-ordinate axes. If the origins coincide, the transformation will be a rotation only. The simplest Minkowskian transformation relating reference frames in relative motion is simply a special kind of rotation (a "hyperbolic" rotation) of the axes of a co-ordinate system. I shall explain in a moment why the rotational transformation is hyperbolic. Let us first look more closely at how this rotation is expressed in two dimensions. In Figure 13 the co-ordinate system describing *F'* can be seen as an ordinary sheet of graph paper, *viewed from an angle*. A rotation from one co-ordinate system to the other is involved here, and we see the two-dimensional expression of it.

In Figure 16, overleaf, this expression of a rotation is illustrated further. The relative velocities of the frames whose coordinate systems are superimposed is 0.2*c*, 0.3*c*, ... , 0.9*c*, and *c*. When the relative motion of the two frames is equal to *c*, no descriptions of physical systems are possible in the non-stationary frame. From this it can be seen that a relative motion of *c* between frames of reference is not possible. This, of course, is a familiar result from special relativity.

That the Minkowskian transformation is a rotation of co-ordinate axes can be illustrated in another way. Figure 17, on page 303, shows the graph-paper lines of the co-ordinate system describing *F'*. Figure 18, on page 304, is a photograph of this, with the camera rotated out of the plane of the figure. The Minkowskian transformation is reversed by this rotation, and the rhombuses of

Figure 16

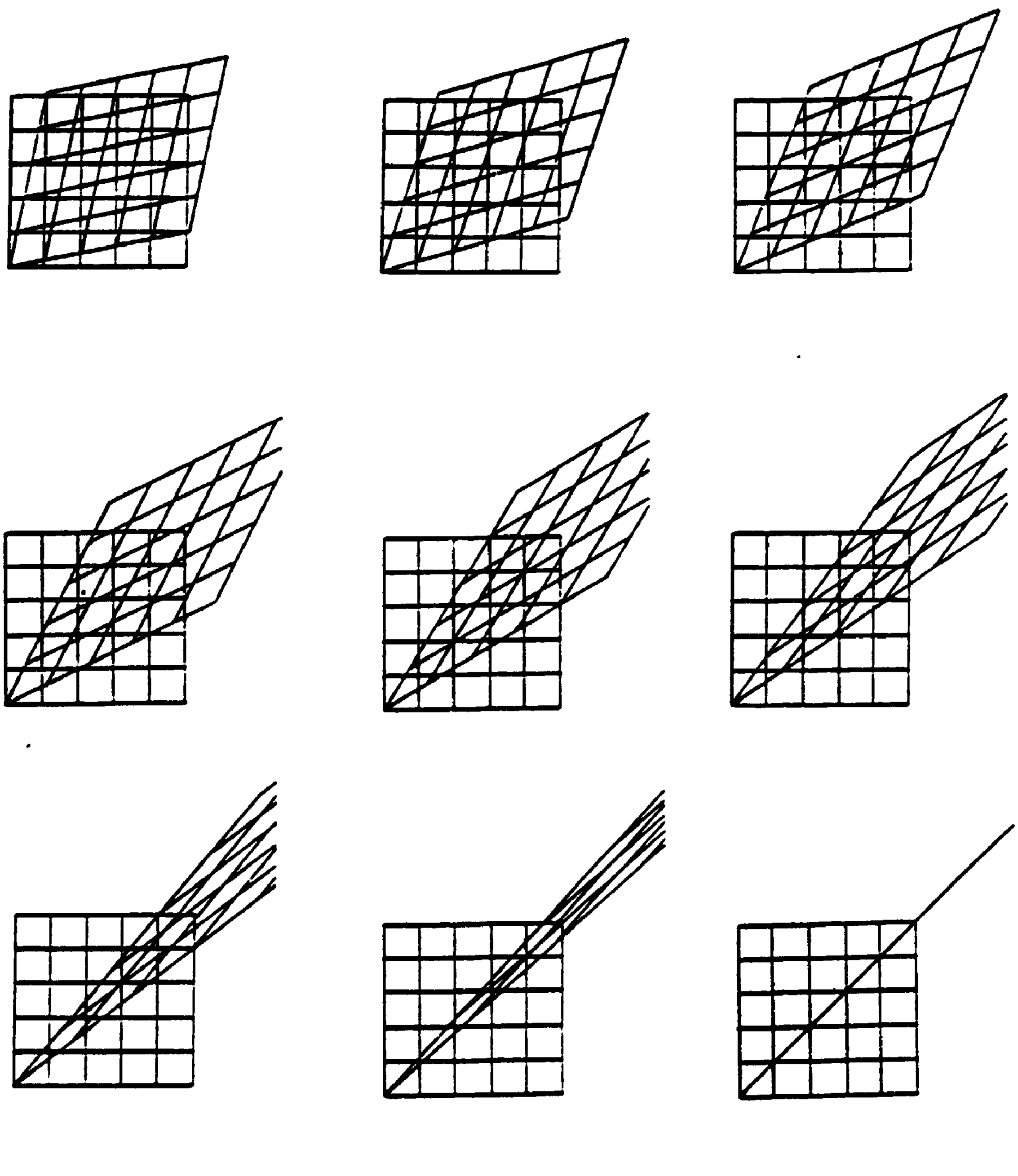


Figure 17

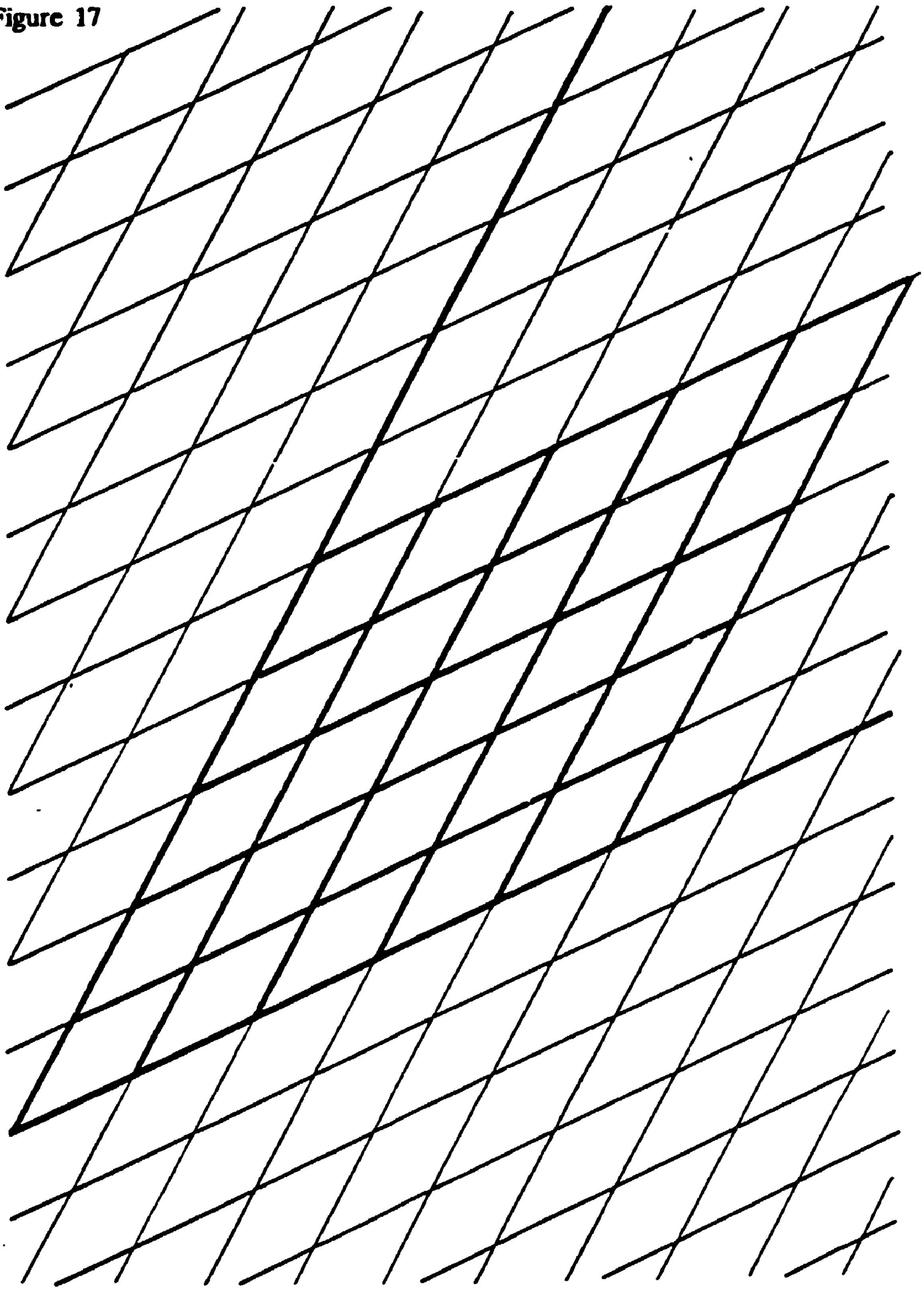
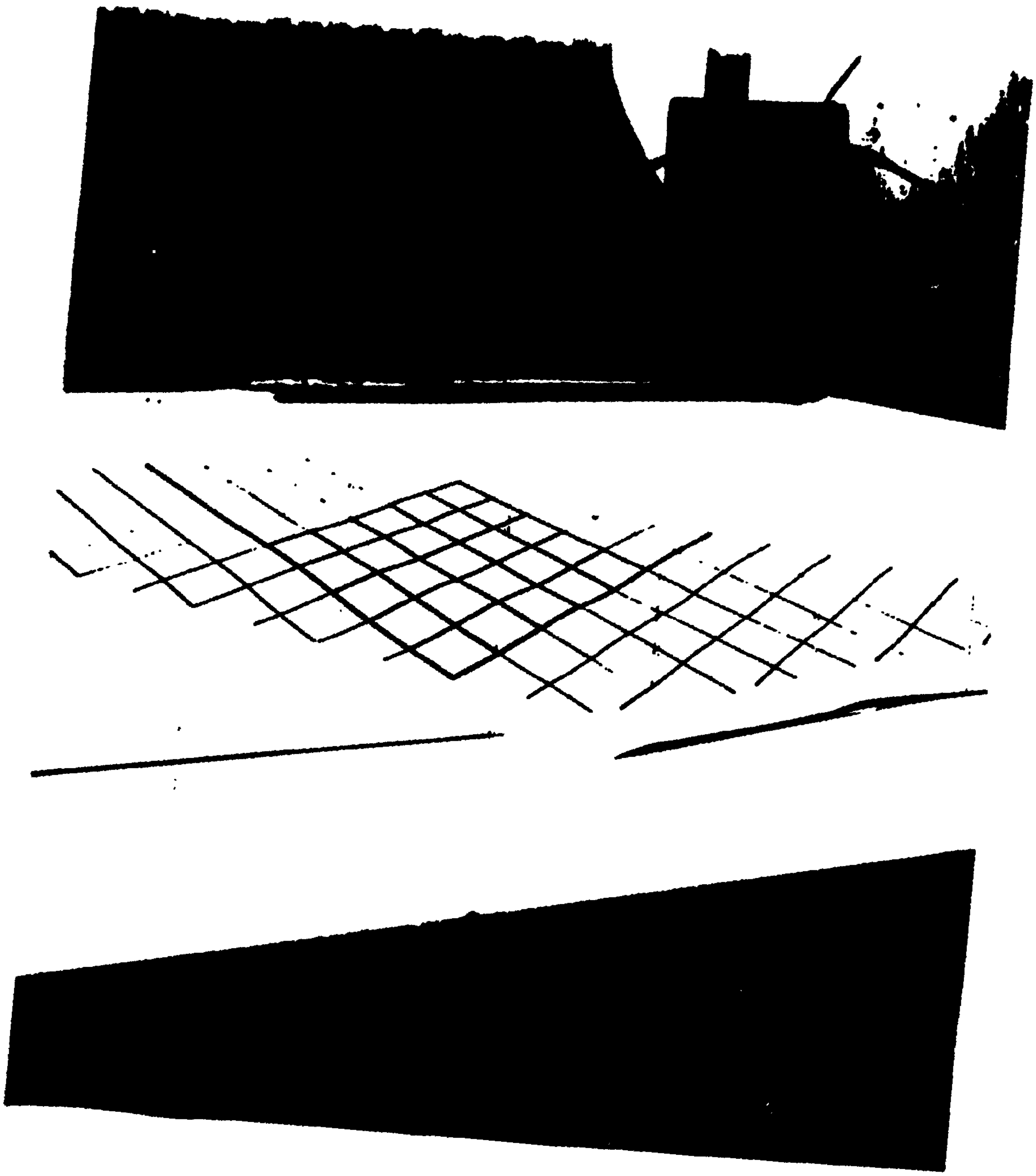


Figure 18



the graph-paper lines of the co-ordinate system describing *F'* are converted to the squares of a stationary frame. Figure 19 (page 306) shows the co-ordinate systems of both *F* and *F'*. Figures 20 and 21 (pages 307, 308) are photographs of Figure 19, with the camera again rotated out of the plane of the figure, and, in the case of Figure 21, with the camera tilted also. In Figure 21, *F'* appears clearly as the stationary frame, and *F'* rather than *F* has orthogonal axes. *F* is the moving frame of reference, moving "backward" at half the speed of light, as we would expect; and it now has the rhombus graph paper.

Interestingly, Figure 19 shows the units of *F'* to be longer than the units in *F*, but Figure 21, which is a photograph of Figure 19, shows the units in *F* to be longer than the units in *F'*. In each case the stationary frame has the shorter units, and the units in the non-stationary frame can be determined by drawing in the hyperbola of Figure 9. This confirms the fact that that hyperbola, given by $c^2t^2 - x^2 = 1$, is also given by $c^2t'^2 - x'^2 = 1$ in terms of the changed co-ordinates; this being a special case of our requirement that *in general* $c^2t^2 - x^2 = c^2t'^2 - x'^2$. The symmetry afforded by this requirement relates in a profound way to the unity of space and time.

Let us examine this unity in more detail. We have found that the principles underlying special relativity theory determine that the four-dimensional quantity $c^2t^2 - x^2 - y^2 - z^2$ is invariant under choice of reference frame.

Figure 19

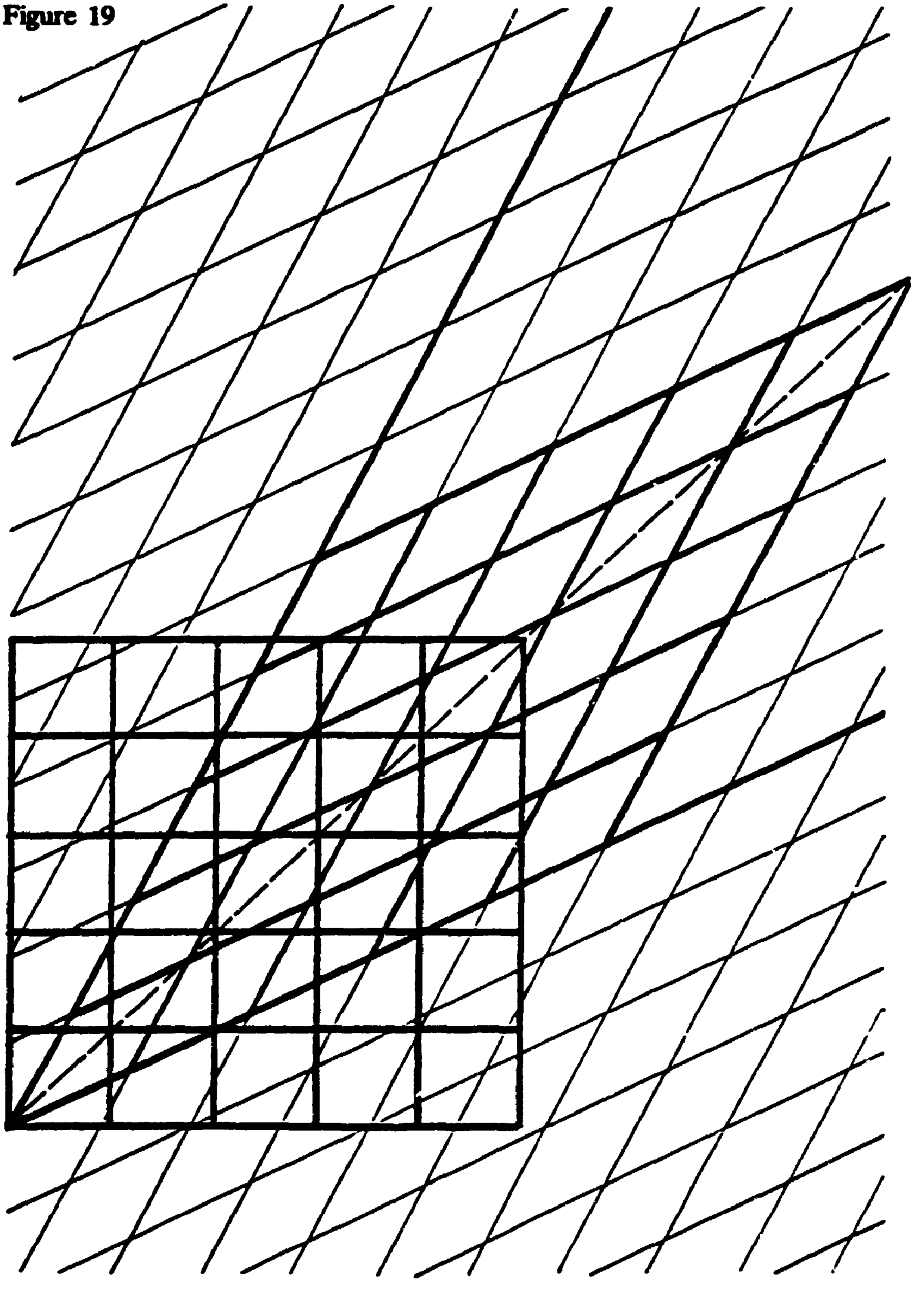


Figure 20

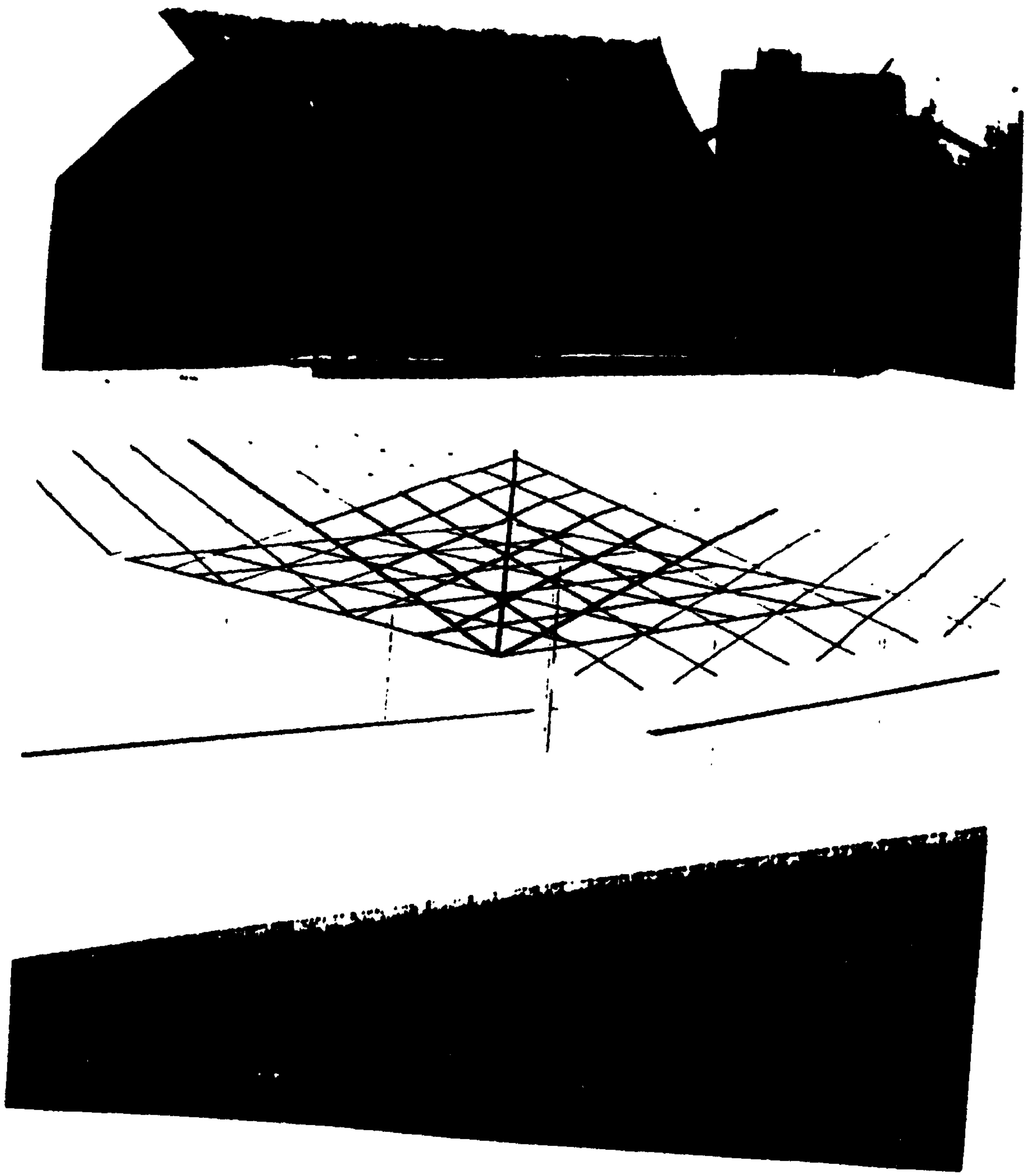
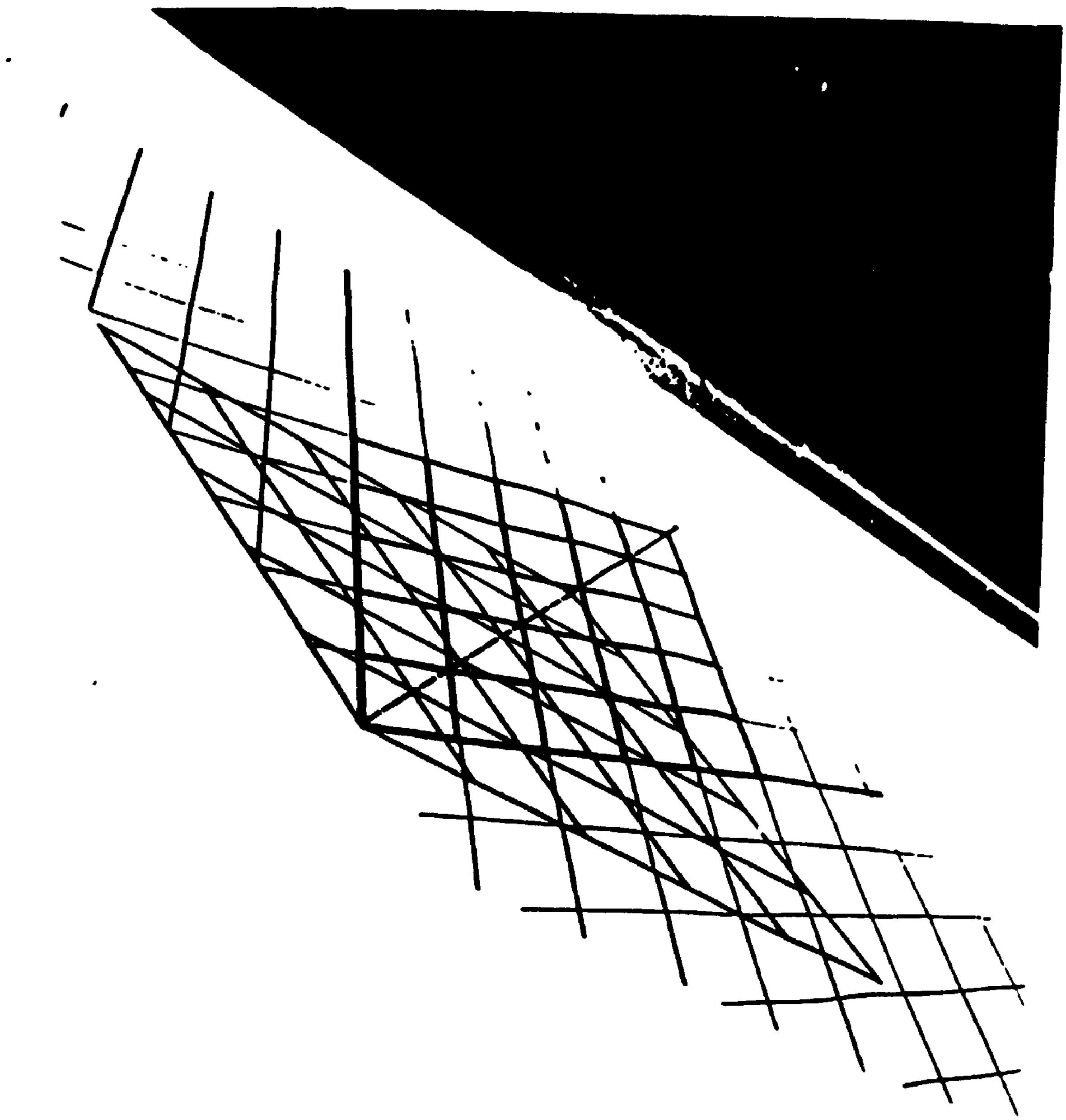


Figure 21



Equivalently, we have that the quantity $x^2 + y^2 + z^2 + (ict)^2$ is invariant under choice of reference frame, where $i = \sqrt{-1}$. Now this result contains as a special case the familiar geometrical fact that $x^2 + y^2 + z^2$ is invariant under ordinary transformations of spatial co-ordinates (in any given reference frame). The invariance of $x^2 + y^2 + z^2$ under the transformation of spatial co-ordinates is the formal expression of the unity of the three dimensions of space. The invariance of $x^2 + y^2 + z^2 + (ict)^2$ requires, of course, alteration of our concepts of space and time; for in essence it is the formal expression of the unity of the four dimensions of space-time.

The Minkowski thesis of the unity of space-time allows us, for what it is worth, to *picture* the world in which the relativity principle holds good. Formally, it allows us to express the Lorentz-Maxwell theory of electromagnetism consistently with the doctrine of invariance, that is, with the "relativity" principle; in this regard Minkowski's formulation of special relativity theory is remarkable for embracing, as a special case, the familiar invariance of physical laws under transformations of spatial co-ordinates. But Minkowskian geometry also allows us to *visualize* what "special relativistic effects" amount to in the objects themselves. Minkowski teaches us to ascribe a four-dimensional, space-time structure into the "space" and "time" of a given reference frame. Minkowski teaches that a change of reference frame is simply a change of *perspective* on an absolute, four-dimensional world. "Time dilation" and "length contraction" are consequently resolved under the heading of *perspectival* change (which is, of course, within the four-dimensional structure, no change at all).

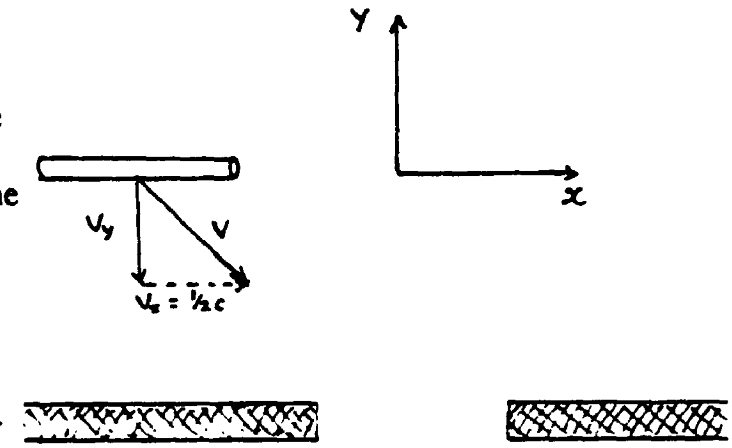


Figure 22

In this regard one further text-book example of a "relativistic effect" is worth considering. Suppose we have a stick with proper length one metre and a slit of proper width one metre, initially stationary relative to one another, in the configuration shown in Figure 22. Then an enormous velocity is imparted, in a direction that should take the stick, obliquely, directly through the slit. Suppose that the x-component of the relative velocity between the stick and the slit is ½c. Then within the frame of reference of the stick, the width of the slit will be only 0.866 metre, which seems to imply that the stick will not get through. But within the frame of reference of the slit, the length of the stick will be 0.866 metre, which seems to imply that the stick will pass through the slit without even brushing its edges. The apparent contradiction here results from our neglecting the unity of space and time. Change of reference frame is a rotation in space-time. More careful analysis shows that the slit presents itself to the stick at an angle, an angle whose dependence on the relative velocity is precisely such as to ensure that the stick just passes through the slit (brushing both edges). Correlatively the stick presents itself to the slit at an angle, again of exactly the right measure to ensure that the stick just passes through. See Figure 24, below. The slit "appears" rotated to

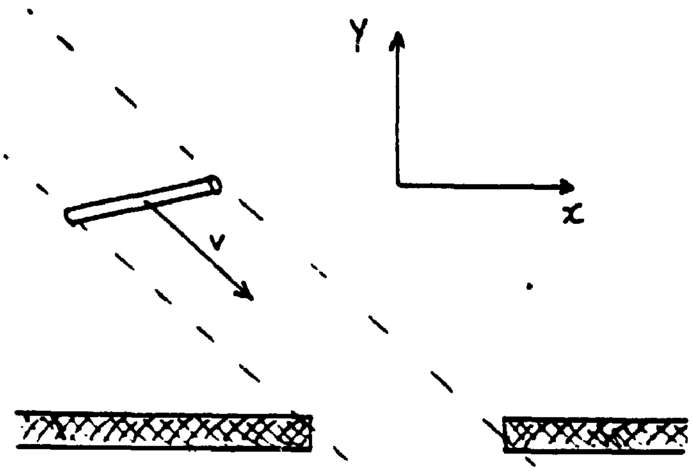


Figure 23

the stick, and the stick "appears" rotated to the slit. But talk of "appearances" is otiose, and misleading. It is more revealing to say simply that there *is* a rotation here. Each reference frame is associated with a perspective on the events in questions, a perspective from which we project these events into the "space" and "time" of our reference frame.

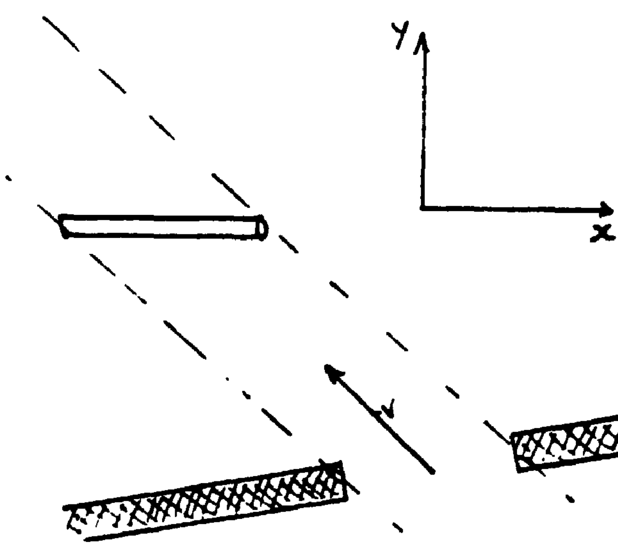


Figure 24

In projecting the four-dimensional structure of events into our chosen "space", we find that obliquely moving objects are rotated. These moving objects are also "contracted", and to understand why, we must think further about the four-dimensional structure of these events. For in the projection of these events into our correlatively chosen "time", there is an explanation of the "length contraction". We have projected a diminished *spatial* length, but an increased *temporal* length for the moving object. The stick does not pass through the slit in an instant; rather, it passes through over an interval, with its ends brushing the edges of the slit non-simultaneously. The rotation and the "length contraction" and the non-simultaneity of the ends hitting edges are all expressions of the unity of space-time.

That the rotations that relate to a change of reference frame are *hyperbolic* has still to be explained. We have already seen a mathematical expression of this: in two dimensions, when origins coincide, the family of hyperbolas given by $c^2t^2 - x^2$ = constant is exactly reproduced by replacement of x, t with x', t'. Likewise in the four-dimensional case (assuming throughout the origins coincide) the family of hyperbolic hypersurfaces given by $c^2t^2 - x^2 - y^2 - z^2$ = constant (or equivalently, by $x^2 + y^2 + z^2 + (ict)^2$ = constant) is exactly reproduced by replacement of x,y,z,t with x',y',z',t'. This result contains as a special case of familiar fact about the geometry of space: viz., that the family of spheres given by $x^2 + y^2 + z^2$ = constant is exactly reproduced by replacement of x,y,z with

$x'y'z'$. Mathematically, what is the difference between hyperbolic geometry and the familiar spherical geometry we associate with space? It comes down to the presence in the formalism of i ($= \sqrt{-1}$). In describing rotations in space, we use trigonometric functions; hyperbolic rotations are described by hyperbolic functions, which differ from trigonometric functions by the constant factor $i(=\sqrt{-1})$.[1] Because *ict*, rather than *ct*, takes its place alongside the spatial coordinates, our freedom in projecting variously the four-dimensional structure of events into "space" and "time" is in an important way limited. Whereas $x'y'z'$ could, for example, under one possible transformation, simply replace y, z, x respectively, x' could under no possible transformation simply replace *ct*. This, as it seems to me, in no way diminishes the warrant for the thesis of the unity of space-time. One may balk at the use of the "imaginary" constant factor i. But this number picked up its name before it was found useful in physics. And it is physics that should decide whether or not its name is apt. We have discussed the grounds for postulating a unified, four-dimensional structure for the world. The constant factor i in no way diminishes the warrant for this postulate.

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[1]The hyperbolic sine and cosine functions are related to sine and cosine by the formulae $\sinh A = [\sin(iA)]/i$ $\cosh A = \cos(iA)$. I am indebted to a discussion by D. Bohm in his *The Special Theory of Relativity*, W. A. Benjamin, Inc., New York, 1966. Chapter XXVII. However, I do not agree with Bohm's arguments, given principally in Chapter XXXI, against the verisimilitude of Minkowskian geometry. Ironically, these arguments are operationalistic, and it is my argument, below, that Minkowski's reformulation of special relativity theory has advantages which discredit the operationalism of the 1905 formulation.

It simply happens in our world that, in Minkowski's words,[2]

> ... the essence of this postulate may be clothed mathematically in a very pregnant manner in the mystic formula
> $$3 \cdot 10^5 \text{ km} = \sqrt{-1} \text{ secs.}$$

(In fact the same geometrical structure that Minkowski characterized as Euclidean in x, y, z, and ict can alternatively be characterized, without introducing complex numbers, as semi-Euclidean in x, y, z, and ct. Under this characterization, the magnitude of the separation of "light-like" related events is *nil*, of "space-like" related events is *negative*, and of "time-like" related events is *positive*. This structure is generated by the so-called Minkowski metric tensor, and the generalization of special to general relativity theory amounts to making this tensor a function of position in a way reflecting the distribution of matter-energy.)

Now let us turn our attention, by steps, back to 1905, and consider Einstein's original formulation of special relativity theory. How far removed was Einstein's thinking then, from the picture of the world afforded by Minkowski in 1908? Well, in 1908, Einstein was present when Minkowski first delivered his remarkable lecture "Space and Time", which began as follows:[3]

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[2]H. Minkowski, "Space and Time", contained in Einstein, et al., *The Principle of Relativity: A Collection of Original Papers on the Special and General Theory of Relativity*, trans. W. Perrett and G. B. Jeffery, Methuen, London, 1923. P. 88.

[3]Minkowski, "Space and Time", op. cit. P. 75.

> The views of space and time which I wish to lay before you have sprung from the soil of experimental physics, and therein lies their strength. They are radical. Henceforth space by itself, and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality.

Einstein reacted at first to Minkowski's lecture with awed incomprehension: "I don't know what this man has done with my theory!" Einstein remarked after the lecture. It is clear from this that Einstein did not grasp, by himself, the warrant from special relativity theory for the thesis of the unity of space-time. This, indeed, is exactly what Minkowski contended in his lecture:[4]

> [T]he credit of first recognising clearly that ... t and t' are to be treated identically, belongs to A. Einstein. Thus time, as a concept unequivocally determined by phenomena was first deposed from its high seat. Neither Einstein nor Lorentz made any attack on the concept of space, perhaps because in the above-mentioned special transformation, where the plane of x', t' coincides with the plane of x, t, and interpretation is possible by saying that the x-axis of space maintains its position. One may expect to find a corresponding violation of the concept of space appraised as another act of audacity on the part of the higher mathematics. Nevertheless, this further step is indispensable for the true understanding of [the invariance of $x^2 + y^2 + z^2 + (ict)^2$], and when it has been taken, the word *relativity postulate* for the requirement of [this] invariance ... seems to be very feeble. Since the postulate comes to mean that only the four-dimensional world in space and time is given by phenomena, but that the projection in space and time may still be undertaken with a certain degree of freedom, I prefer to call it the *postulate of the absolute world* (or briefly, the world-postulate).

I shall come presently to a discussion of Minkowski's contention that Einstein had not "made any attack on the concept of space". And I shall want then to amplify Minkowski's implied contrast between the notion of "relativity" and the

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[4]Minkowski, "Space and Time", op. cit. Pp. 82-3.

notions of "invariance" and an "absolute world". For the moment, let me remark that Minkowski's ideas, their initial incomprehensibility to Einstein notwithstanding, before very long won Einstein's complete approval. By 1909, in fact, Einstein referred to his own theory as the "so-called relativity theory", and let it be known that he would have preferred to call his theory exactly the opposite: "*Invariantentheorie*".[5] Now I do not believe that, before 1908, Einstein felt that *invariance* was a more central notion in his theory than *relativity*. It is true that Planck, not Einstein, first called Einstein's theory "relativity" ("*Relativtheorie*"). But Einstein, far from protesting, in 1907 used in print the term '*Relativitäts-theorie*' for his own theory.[6] And in 1905, after all, in his original paper introducing this theory, Einstein termed "the principle of relativity" what could more aptly have been called "the principle of invariance".[7] In my view Einstein began to see *invariance* as a more worthy centrepiece for his theory precisely when he had assimilated Minkowski's notion of an absolute four-dimensional world.

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[5]See Arthur I. Miller, *Albert Einstein's Special Theory of Relativity: Emergence (1905) and Early Interpretation (1905-1911)*, Addison-Wesley, London, 1981. P. 173. See also p. 78.

[6]A. Einstein, "Die vom Relativitätsprinzip geforderte Trägheit der Energie", *Annalen der Physik 23*, 1907. Pp. 371-384.

[7]This is the principle that I have called '(1)', above. See Einstein, "On the Electrodynamics of Moving Bodies", in Einstein et al., op. cit., pp. 37-65. See in particular p. 38 and p. 41.

From 1905 to 1908, Einstein had no way of *picturing* the world, and for this reason *relativity* seemed to him the more worthy focal idea within his theory.

From a methodological standpoint, this emphasis (that is, Einstein's own emphasis) on *pictures* is both vague and problematic. What is it to require that the world be "pictured" by a theory? Why expect in the first place that the world is picturable? It is doubtful whether clear answers can be given to either of these questions. Nevertheless, Einstein discussed the intelligibility of physical theories in these terms, and his position was tolerably clear in certain respects that interest us here. We have seen that Einstein was vitally concerned to achieve a new, satisfactory "world-picture" for physics. This meant that he was deeply disturbed that there were two incompatible basic conceptions in physics (fields versus particles), each with its own strengths; while Einstein would say that the incompatibility concerns the demands these different conceptions place on physicists' ways of *picturing* the world, what matters is that the difficulty to which Einstein alluded of formally reconciling the two conceptions was real. When, in 1907, Einstein conceded that he lacked a world-picture corresponding to the relativity principle, this clearly expressed a major dissatisfaction. Einstein probably took the problem to be no fault of his own, but rather to reflect the general problem concerning the duality of physical conceptions and thus the *general* lack of a satisfactory "world-picture" for physics. It was thus undoubtedly very significant *psychologically* for Einstein that Minkowski showed that a world conforming to Einstein's kinematics is, in Einstein's terms, picturable. The new space-time conceptions, Einstein could now see, were perhaps not so encumbered with the difficulties facing physics generally as he had at first thought. One effect that Minkowski's work, once Einstein had assimilated it, seems to have had, is to convince Einstein to regard his kinematical and electrodynamical results as comprising a veritable *theory*. Another was his coming to conceive *invariance* to be the leading idea of this theory. This was *heuristically* very important for Einstein. The new kinematics implied that gravitation cannot be an instantaneous action-at-a-distance force; yet gravitation was not yet formally embraced within Einstein's theory. In undertaking to generalize his theory to embrace the phenomena of gravitation, Einstein's concern with invariance was a key to his eventual success. Einstein sought some symmetry in the phenomena that should be reflected in the theory, and thus would guide its development. He thus fastened on the "equivalence principle" (a colligation of the well-known Eötvös results) which proved to be precisely the needed clue for the successful development of the new theory.[8] Finally, without the geometrical conception of

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[8]Against my general contention that Einstein, after 1909, saw *invariance* rather than *relativity* as the leading idea of his theory, it could be pointed out that Einstein saw his work on gravitation as generalizing the principle of relativity. This is indeed how Einstein writes, but his meaning is subject to various interpretations. Certainly Einstein always connects his discussion of relativity very closely to discussion of invariance, that is, to discussions of the symmetries of the theory. Many commentators (among them Michael Friedman) criticize Einstein for thinking that there is a principle of *general* relativity at all, and for confusing questions about the relativity of motion with questions concerning the theory's "general covariance". Friedman takes "general covariance" to imply just the admissibility of general coordinates, and points out that *every* space-time

the four-dimensional absolute world, and without the clue from the Minkowski

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theory can be formulated in such a generally covariant way. Einstein did not altogether miss this point; at least, by 1934 he could write, in *Mein Weltbild*,

> Is it true that the equations which express natural laws are covariant with respect to Lorentz transformations only and not with respect to other transformations? Well, formulated in this way the question really has no meaning, since every system of equations can be expressed in general coordinates. We must ask: Are not the laws of nature so constituted that they are not materially simplified through the choice of one *particular* set of coordinates?

Then concerning the general theory of relativity Einstein says:

> ... our empirical law of the equality of inert and gravitational masses prompts us to answer this question in the affirmative.

There is a contrast here with classical space-time theories, including Minkowski's; for in the case of these others, we unproblematically answer this question in the negative.

Einstein is calling our attention, here, to the unproblematic fact that the special theory of relativity is quite simply expressed in coordinate-dependent terms. In these terms, which I have followed in this appendix, a restricted equivalence among coordinate systems is brought into view, namely, the equivalence among coordinate systems adapted to inertial, that is, unaccelerated, frames. All inertial frames are equivalent according to special relativity. Does the general theory of relativity generalise this equivalence, so that all frames, accelerated or unaccelerated, become equivalent according to the general theory of relativity? In a certain sense, *pace* Friedman, it does; but the sense is somewhat restricted and disappointing. For according to general relativity theory, there simply are no frames. The empirical law of the equality of inert and gravitational masses prompts us to adopt a non-Euclidean geometry of space-time, in which the notion of frame has no purchase, except locally, where according to the general theory, the special theory of relativity holds good.

For a defence of an interpretation more favourable to Einstein than Friedman's of Einstein's remarks on relativity principles, admissibility of general coordinates, and the equivalence of inertia and gravity, see the numerous recent papers by John Norton that I have included in my Bibliography. Here, I shall mention just two key elements of Norton's discussions. One is that "general covariance" seems for Einstein to have had a much richer meaning than "admissibility of general coordinates", which is the sole sense Friedman gives to this notion. The other is that special relativity theory is to be viewed as a limiting case of general relativity theory not only in regions of arbitrarily small size, but also in the limit as the gravitational field becomes completely homogenous.

metric tensor's describing a special, flat, homogeneous and isotropic inertial structure of how that geometrical conception might be generalized to embrace the phenomena of gravitations and thus the realistic case of non-flat, non-homogeneous and non-isotropic inertial structures, Einstein would never have reached the general theory of relativity.

Setting aside the psychological and heuristic value of Minkowski's formulation, is it superior to Einstein's 1905 formulation also in respect of its explanatory power? This is not an easy question. While Minkowski's formulation of the theory, as I have attempted to show, makes short work of rendering intelligible the relativistic phenomena of time dilation, length contraction, and so on, these results seem in no wise intelligible, but merely predictable, from within the 1905 formulation. The 1905 formulation (which is empirically equivalent to Minkowski's 1908 formulation) seems merely to systematize these results and to provide a quantitative apparatus for predicting them.

It may be overly harsh to judge Einstein's 1905 formulation of his theory non-explanatory. Perhaps, however, Minkowski's contention in 1908 that Einstein had not "made any attack on the concept of space" supports such a judgement. How could Minkowski say this, when Einstein in his 1905 paper had derived the Lorentz transformations for space and time? The answer is that Einstein's results concerning lengths and spatial co-ordinates are all derivative within his early formulation of his theory upon his operationalism and his definitions pertaining to time. His early theory, it can be argued, cannot inform us about space. For it is operationalistic, and really is *about* its observational predictions only. The understanding it provides of phenomena pertaining to space, it provides only in terms of the repercussions that it can predict within the business of our measuring lengths and distances, of our accepting the revised notion of simultaneity. The 1905 formulation of special relativity theory does *not* provide a means of conceiving the world otherwise than in space. Nor, therefore, does it make *intelligible* its surprising predictions about phenomena pertaining to space. Minkowski, by contrast, supplied what was missing for these predictions to be intelligible. And his work involved, by contrast, a *veritable* "attack on the concept of space". For Minkowski established that the "world in-itself" must be conceived otherwise than in space, in order for the predictions of Einstein's new physics to be rendered intelligible.

Dorling also contends that the Minkowski formulation is explanatory where Einstein's 1905 formulation, properly speaking, is not. Dorling's contention is that until Minkowski's work the Lorentz transformations were at least as mysterious as the light postulate for which they are supposed to provide an explanation, and a good deal more complex than the Galilean transformations that they are made to replace. Dorling writes,[9]

[9]"Einstein's Methodology", op. cit., pp. 5-6n.

> This objection remained a serious one ... until Minkowski (1908) pointed out that the combined Lorentz and Euclidean groups are mathematically more natural than the combined Galilean and Euclidean groups. ...[I]f one starts with a standard Euclidean axiom system for spatial geometry, and merely weakens the axioms the minimal amount necessary in order to allow in lines corresponding to "temporal" intervals (all we need actually assume is that such intervals are not congruent to spatial intervals) in addition to those corresponding to ordinary spatial intervals, then one astonishingly obtains, without making any additional assumptions, precisely Minkowski geometry, and hence the Lorentz transformation equations as perspective laws. Newtonian space-time geometry with the Galilean transformations as perspective laws, is here excluded because it violates further Euclidean axioms. ...[Thus from the fact] that in the geometry of space the interval between two points is path-dependent, it is inductively more reasonable to suppose that in the geometry of motion the interval between two events will also prove to be path-dependent, than it is to deny this. ... As Minkowski remarked, with staircase wit, a good mathematician should thus have anticipated the Lorentz transformations in advance of any direct physical evidence. Einstein certainly did not see this in 1905.

On the other hand, Einstein's 1905 formulation centres on a successful deduction of the Lorentz transformations, and (unlike Lorentz's Theory of Corresponding States) it treats any two coordinate systems related by these transformations as equally natural. This is the whole basis for the Minkowski geometry, and the whole basis for the unificatory successes of the theory that we will discuss in the next section. If one understands explanation wholly in formal terms relating to unification, one cannot coherently claim that the 1905 formulation lacked some or all the explanatory power of the 1908 formulation. One would have to say that the difference in "intelligibility" between the two formulations is merely psychological.

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