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Ordinary Empirical Judgments and our Scientific Knowledge: An Extension of Reformed Empiricism to the Philosophy of Science

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A thesis submitted in partial fulfillment of the requirements for the degree in Doctor of Philosophy

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ABSTRACT

The present essay examines the relationship between ordinary empirical judgments and our scientific worldviews. It is concerned with how ordinary judgments (and the primitive frameworks in which they are formulated) might be usefully integrated into an account of epistemological progress, both of our personal views and scientific theories, so that the sciences (especially modern theories of space and time) can reasonably be thought as being informed by, and evolving out of, at least some of the various pre-scientific views they have replaced. We examine our normal perceptual judgments of magnitude, position, orientation, and displacement in the hope of uncovering the logical, conceptual, and empirical relations that exist between such judgments (as well as the views of the world they presuppose) and our sophisticated understandings of space, time, and motion in physical theory.

This research contends that experience and a rich type of conceptual analysis—one that examines the presuppositions that make possible the application of concepts in empirical contexts—together provide the framework within which a rational account of such relations can be proposed. The project thus defends a form of empiricism, but one distinct from classical forms (be they British empiricism, Russellian empiricism, or logical empiricism)—rather a slightly modified version of Anil Gupta's "Reformed Empiricism". This empiricism is capable of avoiding the logical excesses and errors of earlier forms, whilst providing an account of how a set of basic empiricist principles might be extended from their context in general epistemology to recalcitrant problems in the philosophy of science, such as the problem of our formal knowledge, the problem of the communicability of observation, and the rationality of theoretical progress. Such an extension offers a comprehensive account both of our ordinary and scientific knowledge.

Keywords: classical empiricism (British and post-Kantian); logical empiricism; Reformed Empiricism; the *given*; experience; perception; perceptual knowledge; empirical judgment; geometry; space and time; scientific progress; conceptual analysis; conceptual revision; dialecticism; history and philosophy of science.

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INTRODUCTION

The Question of a Comprehensive Empiricism

Construed broadly, this dissertation is about the relationship between ordinary empirical judgments and empirical scientific knowledge. More specifically, it is concerned with how our ordinary empirical judgments (and the primitive frameworks in which they are formulated) might be usefully integrated into an account of epistemological progress, both of our personal views and scientific theories, so that the sciences (especially modern theories of space and time) can reasonably be thought as being informed by, and evolving out of, the various pre-scientific views they have replaced. To this end, we examine our normal perceptual judgments of magnitude, position, orientation, and displacement in the hope of uncovering the logical, conceptual, and empirical relations that exist between such judgments (as well as the views of the world they presuppose) and our sophisticated understandings of space, time, and motion in physical theory. It is our contention that nothing other than experience and a rich type of conceptual analysis—one that examines the presuppositions that make possible the application of concepts in empirical contexts—is necessary to provide a rational account of such relations. Based on such considerations, we think it is possible to develop an empiricism that overcomes the logical deficiencies of classical forms of empiricism whilst also offering a unique characterisation and analysis of recalcitrant problems in the philosophy of science, including the aforementioned relation of pre-scientific to scientific worldviews, and also the rationality of theoretical progress in the sciences.

The purpose of these introductory remarks is to give a brief account of the central question that motivates the essay, and an even briefer overview of the arguments to come. Our claims here fall into three parts, each devoted a section below: §I addresses the question of a "comprehensive empiricism", including the obstacles encountered by an empiricist project, like this one, that speaks to issues of general epistemology and scientific knowledge. Included in our discussion is an historical remark regarding the origins and eventual decline of comprehensive

empiricism. §II offers a brief overview of a new form of empiricism, namely Gupta's "Reformed Empiricism" as developed in his [2006a], with a brief claim as to the merits of Reformed Empiricism, and why it may be an ideal candidate for a new comprehensive account of our (empirical) knowledge, both ordinary and scientific. The remarks here are very general; the technical details of Gupta's empiricism are more fully examined in Chapter One. §III offers a brief overview of the main points of each chapter to follow.

§I: The Question of a Comprehensive Empiricism

Can empiricism account both for our common sense and scientific knowledge? This was once a central question for empiricists going all the way back to Locke, and it was arguably the most dominant amongst philosophers (and many scientists) for nearly a century and a half: from Kant's positing a single *a priori* framework that makes possible empirical knowledge both in common sense judgment and in science, to Helmholtz's assimilation of the Kantian *a priori* to the psychology of perception and the mechanics of motion; from Mach's naturalised phenomenalism, to the new theory of knowledge as it was explored by the logical empiricists in light of the considerable advances in 19th century geometry and early 20th century physical theory. Not only was a "comprehensive empiricism" (if we can call it that) a desideratum of the highest order, it seemed to be promised by new developments in physics and psychology.

Despite its central role in many past empiricist projects, this desideratum is not prevalent in the contemporary literature. Empiricism is arguably healthier than it has ever been, considering the ubiquity of views that profess the advancement of this or that particular empiricist thesis, and considering what may be called an empiricist *spirit* in general epistemology and the philosophy of science. But this piecemeal empiricism only masks the fact that there has been no attempt since the 1950s to propose a comprehensive empiricism that offers a systematic understanding of all of our knowledge (common sense and scientific) on basic empiricist principles. This is perhaps quite surprising given our remarks in the

previous paragraph, and even more surprising with a cursory investigation of the roots of modern empiricism and the question of scientific knowledge. This history, if traced back to the classical British empiricists, exhibits the great benefit of developing an epistemology which can deal with questions of a general epistemological sort (such as the explication of the *way of ideas*, and the concern of scepticism regarding the reality of everyday perception, and the nature of our visual perception) and a scientific sort (such as the epistemic standing of the mechanical philosophy and Newton's new physics, the "problems" posed by Newtonian gravity and other action-at-a-distance phenomena, and the proper "rules of reasoning" for empiricism in light of Humean scepticism about the ultimate causes of things).

Let us take Locke as our example of the fruits of congress between empiricism and the sciences. For Locke, a new epistemological program was required to explain not only our common sense perceptual judgments, but also (importantly) our scientific judgments—in particular, the findings of the new experimental sciences. Given the limitations imposed on us by our faculties, the Aristotelian paradigm of *scientia* (that scientific knowledge is only of necessary truths) seemed unachievable: if scientific knowledge must meet such high epistemic demands, and our senses (even when organized by experiment) seem to grant us nothing like necessary truths, then properly scientific knowledge would seem to escape our grasp. Of course we have such knowledge when it comes to formal matters (such as geometry), but the Aristotelian account of *scientia* was far too restrictive to provide a proper epistemological account of the new empirical sciences *as sciences* (especially Boyle's radical corpuscularian mechanical philosophy and experimentalism, both major influences on Locke's thought). Natural philosophy needed to be divorced from *scientia*, but to do so required a new epistemological framework: one which did not demand the grasp of necessary truths.

Locke's epistemology was deft in its handling of this problem. Acknowledging that *scientia* seemed to be the mark of theoretical sciences (especially geometry and ethics), yet far too stringent a model for the new experimental method in natural

philosophy, Locke preserved *scientia* as an ideal, but at the same time loosened the imperative to meet this ideal in natural philosophy, which could be based on purely sensitive knowledge (experiment). (Kochiras [2009]) Human knowledge is thus able to account for scientific knowledge, but only after we see that scientific knowledge is *not* a systematic understanding of necessary truths—of formal causes and the essences of things. Rather, natural philosophy has to be content with an understanding of the merely nominal essences, understood through the regular co-existence of certain collections of perceptual properties in things, learned by repeated representation of those properties as being conjoined. (ibid)

While a more complete analysis of Locke's philosophy and its relation to the new sciences must wait for Chapter Two, we can already sketch some of its central points: (1) natural science, whatever it may be in detail, is based on sensitive knowledge (at least for limited beings like us); (2) while our senses put us in causal congress with the world (says Locke), our epistemic limitations are such that we cannot arrive at universal and necessary truths about it; (3) since we cannot determine essences (outside of demonstrative sciences like geometry), and since natural science still avails itself of general claims based on particular experience/experiment, we can only approach the ideal of *scientia* if we are able to refine and better apply our empirical tests, so that we can trust our inductions more and more, and arrive at fundamental principles (like those of Boyle's mechanics, Galileo's natural philosophy, or Newton's nomic systematisation of motion and gravitational effects).

We will see in Chapter Two that there were very real philosophical limits to how far such inclusion of scientific concerns might go in the tradition of classical British empiricism, and that certain parts of Newton's system were rejected (or at least radically re-contextualised) by Locke and Hume on what we would now consider purely philosophical grounds. Hence, we cannot anachronistically impose on the early modern empiricists a kind of naturalism they did not hold. However, for now, the preceding remarks suffice to make the more general point—that even in its infancy, empiricism was to account for our common sense *and* scientific beliefs

about the world. Empiricism was intended to be comprehensive. This idea was maintained throughout later empiricist projects, including Kant's transcendental attempts to ground our synthetic judgments in sensible intuition (overcoming the gap between *scientia* and induction vis-à-vis Newton's laws of motion), and the post-Kantian empiricist attempts of figures like Helmholtz, Mach, and Russell, and various logical empiricists, to show just how much of the synthetic *a priori* may be replaced by purely empiricist principles of reasoning and (in the case of Russell and the logical empiricists) the use of new logical tools which allowed for a more nuanced understanding of the analytic—including how the analytic might facilitate our empirical reasoning in ways once thought to require the doctrines of the Transcendental Aesthetic and Transcendental Analytic. Such matters will be discussed in more detail in Chapter Two, but are mentioned in gloss here to show just how prevalent was the desideratum for a comprehensive account of our normal perceptual judgments and our scientific principles.

The prevalence of comprehensivism from the 17th century to the first half of the 20th century might lead one to think that its absence in contemporary epistemology is quite bizarre. What could have led to the demise of the desideratum? The real answer is quite complex, and explored in more detail in Chapter Two, but the quick answer is this: such a broadly construed normative empiricism became unpopular (and considered by many as untenable) by the middle of the last century in light of the (real and perceived) problems of logical empiricism. While empiricist projects continued in general epistemology and the philosophy of science (in fact, such projects flourished), none were ever so systematic in nature as their predecessors from the 1950s onward. This is due to two main factors (though likely a whole host of other concerns could be cited as reasons for the decline of comprehensivism, so we propose these factors as non-exhaustive). Firstly, the 1950s saw a marked decline in the popularity of normative empiricism, due in large part to three immensely influential criticisms: (1) Quine's critique of Carnap's [1928/2003] and the proposal of his replacement naturalism (a naturalised empiricism based on the grounds of behavioural psychology) in Quine

[1951/1961]; (2) Wittgenstein's critiques of propositional reductionism and ostensive definition (and his proposal of a natural language philosophy) in Wittgenstein [1953]; and (3) Sellars' critique of the myth of the given in Sellars [1956/1997]. These critiques, whatever their actual philosophical worth,[1] were decisive in bringing to an end the comprehensive stage of the logical empiricist tradition.

Secondly, the discipline of philosophy, like all disciplines in the academy, came to be more and more fragmented after the war; the need for broad, philosophical accounts of knowledge that had once seemed so central to philosophy were no longer *de riguer.* However, fragmentation alone does not explain the decline of comprehensivism. For that, we must discuss a more specific historical development. The second half of the last century saw a parting of the ways between general epistemology and the philosophy of science, partially as a by-product of the specialisation that accompanied fragmentation, but also (in the specific case of empiricism) as a result of the diaspora of positivism out of Germany, Austria, and the Czech Republic after (respectively) the Nazi seizure of power, the *Anchluß*, and annexation of the Sudetenland. In the context of the academy in North America, the specialised research projects of (especially) Reichenbach, Hempel, and Carnap came to define logical empiricism as a narrower project than it was perceived to be in the Halcyon Days of old Vienna. The comprehensive projects of the 1920s and 30s—here, Schlick's [1925/1974] and Carnap's [1928/2003] and [1934/2002] are prime examples, as is Russell's structuralism as expressed in his [1927][2]—were replaced by more focused researches on topics such as the foundations of quantum mechanics (Reichenbach [1944]), Hempel's and Carnap's development of inductive logic (Hempel [1945], Carnap [1950/1962], [1952]), and a whole host of other even

[1] We provide a critical analysis of Quine's arguments in Chapter Two.

[2] Schlick and Carnap get later mention, but we refer only to Russell's phenomenalist program of [1914], not his later Lockean representationalist approach and structuralism in [1927] and [1948/2009]. This later project does not attempt, as the [1914] project had attempted, to reduce scientific concepts to logical constructions of sense-data. Rather, it attempts to show that what matters to our physical knowledge is not the intuitive contet given in acquaintance, but the mathematical structure of perception. Once we realise that physics only cares about structure, we are in a position to extend what we know about the causal structure leading from external events to percepts to find the general mathematical laws which hold of the world that lies beyond perception.

more particular problems in the analysis of mathematical physics. This process of specialisation is best seen as a natural development—a maturation of a very sophisticated brand of philosophy. Nonetheless, the fact that concerns of a general epistemological nature were often largely ignored in favour of more esoteric studies in the philosophy of science is undeniable, and the mature projects of the logical empiricists offer our best example of this trend away from comprehensivism.[3]

Also, specific theses developed within the logical empiricist tradition (about the stipulative nature of theoretical frameworks, and the view that basic judgments are at least partly the domain of psychology) meant that questions about the relationship between simple factual judgments and scientific theories were no longer thought of as in need of epistemological explanation. The logical empiricists came to think that there wasn't much to be said about how our normal empirical judgments might bear on scientific theories.

We are in the position of providing an account of the merits of comprehensivism in this highly fragmented and specialised context. But what motivates a reintroduction of comprehensivism in empiricism? Some may argue that the regulative ideals of simplicity, systematicity, and unity are goals in themselves. *The more expansive the parts of our knowledge that can be brought under a smaller and smaller set of empirical principles the better*. While there is certainly value in simplicity, systematicity, and unity of thought, we do not think that these ideals function as the primary (much less sole) motivation for comprehensivism. We hold that the history of empiricism offers us our best motivation: the fruitful congress that has existed between philosophers and scientists (of various stripe and colour) who have sought to explain physical phenomena without recourse to extra-

[3] Friedman [1999] and Carus [2007] have discussed the ways in which these specialised projects drew emphasis away from not only the essentially political character of early logical empiricism, but also its deep connections with the post-Kantian intellectual traditions in European thought, including most significantly the later 19th century and early 20th century development of a kind of scientific empiricism in the work of Helmholtz, Mach, Avenarius, and Russell, but also the congress between logical empiricism and less empirically-minded thinkers, from Marburg neo-Kantians like Natorp and Cassirer to phenomenologists like Husserl and Heidegger. Such particularities are not of our immediate concern; we merely hope to establish here the final stages of comprehensivism, and the general conditions which led to its demise. See Friedman [2000] for an analysis of the complex relationship between different strands of the post-Kantian tradition, through his recounting of the famous conference in Davos, Switzerland, March 17-April 6, 1929.

empirical principles. These philosophers and scientists, as we shall see, developed nuanced ways of mobilising novel experiment and empirical data to answer some of the most complicated questions about the nature of space, time, and motion—including, among other historical episodes, Galileo's critique of the Aristotelian theory of motion (based on Aristotle's elemental conception of "natural motion") in his exposition of relativity, and Newton's critical analysis of the Cartesian account of "philosophical motion" based on dynamical considerations in his famous "bucket experiment". Also, more recent examples can be explored in the development of relativistic physics, such as Einstein's reflection on the role of infinite signaling in classical electromagnetic theory, leading to the formulation of special relativity. These success stories of empirically motivated conceptual analysis in physics highlight the ways simple experiments might be brought to bear on the justification of new scientific theory, and how our personal, and perspectival views of the world might therefore be better extended, with a few misconceptions and parochial excesses corrected along the way, to ever more sophisticated contexts. Such considerations of the intimate relationship between our common sense, personal perspectives and well-organised scientific pictures of the world do much to motivate the reintroduction of comprehensivism, even in light of the anatomy of comprehensivism's demise.

Part of the present essay is therefore dedicated to an examination of the work of previous philosophically motivated scientists and scientifically motivated philosophers. Much of Chapters Two and Three discuss the insights of thinkers like Helmholtz, Mach, Poincaré, Einstein (and a host of others) who saw the value in making philosophy more scientific on the one hand, and, on the other, bringing a kind of sober empiricism to bear on existing scientific conceptions. Such thinkers are significant in their own right, but also deeply important to the current study for the influence they had on the development of logical empiricism. It is our contention that we can learn much from the logical empiricists' readings of these 19th and early 20th century philosopher-scientists. While their understanding of scientific theory as based on a thorough-going conventionalism may have taken them away from the

kind of comprehensivism we wish to pursue here, we argue that there is much to learn not only from their reading of 19th and early 20th century figures, but also from their commitment to that tradition's concern with the formal parts of our knowledge and its relation to empiricism. Further, we argue that what was really missing from the logical empiricist picture of our scientific knowledge was a brand of conceptual analysis that eschewed the Kantian-cum-positivist dichotomy between analytic and synthetic statements as too coarse a framework within which to reconstruct the "language of science". This form of conceptual analysis, most pronounced in Poincaré, though operating in some primitive way in Helmholtz and Mach, has been characterised by Demopoulos [2000] as "the practice of recovering a central feature of a concept in use by revealing the assumptions on which our use of the concept depends." (220) We will see that this form of conceptual analysis has pride of place in any new proposal of an empiricist account of the shift from common sense to scientific accounts of the world.

§II: Gupta's Empiricism as a Comprehensive Empiricism

In this essay, we will investigate an interpretation of empiricism that may be up to the task of bridging the current gap between general epistemological research and the philosophy of science, namely Gupta's Reformed Empiricism as developed in his [2006a]. In this work, Gupta offers a novel account of the rational import of experience to knowledge. Gupta argues that experience (or more aptly "the *given*" in experience, i.e. the rational import of experience to knowledge) has traditionally been viewed by empiricists as offering, by itself, epistemic license to a set of perceptual judgments based on the special epistemic status given to experience. Gupta takes a general sense-datum theory of knowledge to be an exemplar of just such an interpretation of empiricism. Under this classical account, experience acquaints us with a set of primitive facts of an essentially private and privileged sort: *private* in that they are facts about an agent's occurrent mental states (or about subjective entities private to the agent) , available only to the agent (at least available *only in their primitive form* to the agent), and *privileged* in that knowledge

of such facts is thought to be prior to knowledge of other sorts of facts, such as those which describe an external and public world. It is quite common within classical empiricism to treat these private and privileged data of sense as the incorrigible basis on which all of our substantive knowledge rests: all of our factual knowledge is either a product of incorrigible direct awareness, or derivative of such—got by having the right kinds of logical connections with "basic empirical beliefs". Here, the assumption is that experience must make its contributions to knowledge in the form of propositions, since only propositions are able to have the right kinds of logical relations with our common sense and scientific judgments, and since the given must come in the same general logical form as such facts if it hopes to justify them. This account of the given is thought to be ubiquitous in empiricism from Locke to Carnap.[4]

Reformed Empiricism rejects the classical empiricist claim that experience on its own offers epistemic license to a set of perceptual judgments. While the details and mechanics of Gupta's view must wait for Chapter One for a full explication, we may survey in outline some of its main claims (and direct the reader to Chapter One, especially Sections 1 and 3, if this short survey proves inadequate). Gupta holds that experience does have a rational bearing on knowledge, and that experience is indeed "our principal epistemic authority and guide." (Gupta [2006a], 3) However, from such claims, he views it as a false assumption that experience must make its epistemic contribution in the form of propositions. He acknowledges the naturalness of such an assumption, especially if one adopts (as the classical British empiricists did) the Cartesian model of experience—the model already hinted at in the preceding paragraph, which views experience as providing us (through direct awareness) not with a world of external objects and their relations, but only with a subjective realm of ideas (impressions, sense-data, etc.).

Gupta's task is to show that this model of experience is not thrust upon the empiricist out of necessity, but is rather the result of contentious philosophical assumptions regarding the proper logical grammar of the given. His work is in large

[4] We challenge just how far such a conception of the *given* can be extended, arguing that it starts to find dissenting voices in the scientific empiricism of the 19th and early 20th centuries.

part a proposal of another model of experience, the *hypothetical* model of the given, which places experience in an altogether different logical category. Experience does not "speak" to us in propositions (or proposition-like entities), rather it operates as a *function*: experience operates by taking one of a possible infinity of what Gupta calls "worldviews" (as a function takes an argument) to generate epistemic license to a set of perceptual judgments. This hypothetical model of the given avoids the pitfalls of empiricist foundationalism and the problem of the myth of the propositional given. It assumes that epistemic license to a set of perceptual judgments is owed to experience and view (the epistemic agent's current, yet revisable, understanding of the world and self). As a result, there are no such things as "empirically basic beliefs" on such a model, nor does it make any sense to associate experience with conceptual/propositional contents, much less truth-values (since experience is not of the right logical category to be attributed truth-values). This is, of course, quite different from the logical empiricist view, which holds that there is such a set of beliefs, connected with theoretical structures via "meaning postulates".

A problem remains—one addressed by Gupta in his work, and one that will be addressed in Chapter One of this essay. The factorisation of experience means that the subjective character of experience, i.e. the way things seem to be to the agent, is a product of two factors: the world and the subject's situation and constitution. From the subjective character of experience (say, the seeing of a red apple), one cannot deduce the rational contribution of experience. This is because many such world-self combinations (in fact, perhaps an infinity of possible such combinations) can lead to an experience of this subjective character. Perhaps it is a product of a common sense view that assumes (correctly) the lighting conditions are normal, and that the agent's perceptual system is functioning well, etc. In such a case, experience will yield the expected judgment (or set of judgments) about the apple. But the agent's current experience could also be of a yellow apple (though she sees the apple as red), as she remains unaware that lighting conditions are not ideal, or that she is wearing red-tinted glasses, or that her perceptual system is not

functioning properly, perhaps because of some recent trauma, etc. Experience is always open to multiple such factorisations. This does not, however, diminish the importance of the fact that experience has a rational role, though it goes some way to calling into question its ability to function as our sole (or at least ultimate) epistemic authority, since we have (as of yet) no way of utilising experience to show that our current view of the world is correct.

For the purposes of this Introduction, we speak only in a rough way about how an empiricist might go about constructing her view using the hypothetical model of the given. It will be realised immediately that the possibility of multiple factorisations of experience leads one to a sceptical worry. If one has the correct view of the world, then it will easily be granted that experience will yield true perceptual judgments. However, how is an empiricist to know she has the correct view? How is she to justify an assertion that her view is correct, save through experience—the very same experience she knows to be veridical only if she is justified in thinking her view correct? It is out of this epistemic circle Gupta hopes to break by arguing that the essential interdependence of experience and view does not preclude the possibility of one arriving at *categorical* entitlements from the hypothetical given.

The picture looks something like this. Allow a number *n* of epistemic agents to undergo a series of experiences *E*. Assume that amongst these agents is an assortment of views $\{v_1, v_2, v_3, \ldots v_{m\leq n}\}$, where each agent has one and only one view, though we need not assume that each agent's view is distinct or unique; we merely need to assume that at least two distinct views in the set are distributed amongst our epistemic agents. The empiricist's problem of extracting categorical entitlements from hypothetical information is then one that is solved by a convergence between the views in the set $\{v_1, v_2, v_3, \ldots v_{m\leq n}\}$ (or the views in the relevant subset, which, as mentioned, must include at least two of the views from $\{v_1, v_2, v_3, \ldots v_{m\leq n}\}$). If it can be maintained that there exists a revision process for each initial view (revised at each stage of enquiry in light of the series of experiences *E*), and that such "revision processes" converge on a common conception of the

world (and a common core of perceptual judgments Ω), then all of the epistemic agents have categorical entitlement to Ω (assuming that $\{v_1, v_2, v_3, \ldots v_{m \leq n}\}$ contains all admissible views), whilst we still maintain that the given in experience is hypothetical. The justification for one's assent to the perceptual judgments contained in Ω rests on the *dynamical properties* of revision processes, and the convergence of all initial views on Ω. (Once again: we speak here only in broad outline. The details of view revision in light of experience, the notion of a revision process, and the technicalities of convergence are explored in more detail in Chapters One and Three.)

The extension of this Reformed Empiricism to scientific conceptions of the world (as opposed to our common sense conceptions) follows if Ω contains not only "normal" perceptual judgments, but the sorts of empirical judgments one is used to seeing in, say, physics. Such an extension is not straightforward, and many an issue about the relationship between experience and our formal knowledge needs to be addressed. However, we argue that Reformed Empiricism is a basis for such an extension given the possible parallels that exist between the essential interdependence of experience and view, and the interdependent character of at least some of our empirical judgments and the theories we use to arrive at them. Further, since views are allowed to include principles of a formal nature (at least insofar as such views which contain such principles can converge with other views which may not), we have a novel response to a long-standing empiricist problem that seemed to block extensions to the philosophy of science: the problem of offering an empiricist justification for formal principles, or at least showing how this kind of knowledge is consistent with empiricism.

In spite of its promising new logical categorisation of experience, and in spite of the plausibility of the new empiricism that Gupta constructs using the logic of interdependence, one may be concerned that we are too narrowly focusing on this model as a unique candidate for a comprehensive empiricism. Two comments must be made regarding our use of Gupta's new empiricism in the present essay. The first is that it may not be the only form of empiricism that is up to the task of accounting

for our common sense and scientific knowledge. We here only pursue it as *a* candidate for a comprehensive empiricism, not as *the unique* such candidate. Secondly, many of the other prominent forms of empiricism in the contemporary literature seem to have deficiencies which preclude them from the kind consideration we offer here of Gupta's work. This should come as no surprise, given what we have said about the decline of comprehensivism. Most forms of empiricism seem tailored to problems of either a general epistemological sort, *or* a philosophy of science sort. For example, recent work in Bayesian Confirmation Theory seems to provide us with a probabilistic account of general claims based on evidence, whether such claims be of common sense or science, and whether or not "evidence" be construed as simple observation of one's surroundings, or something more restricted and formalised. However, the notion of evidence clearly is not neutral in this way, betraying its decidedly scientific origins, for the notion of evidence employed by the Bayesian comes far down-stream from the sorts of general epistemological concerns we will see addressed by Gupta. Confirming hypotheses (especially general claims) using evidence presupposes that there is some rational account to be offered of how sense-experience may yield observational evidence, and we wish to hold that no such assumptions ought to be made by a comprehensive empiricist. One of the central features of comprehensive empiricism is that it offers us an account of how experience may have a rational import for knowledge. Confirmation Theory assumes that a probability approaching 1 ought to be granted to particular sentences that are confirmed by experience, but the more primitive question is this: why does seeing a white swan count as confirmation of the sentence, "That is a white swan"? More specifically, since there are a number of ways in which undergoing an experience with the subjective character of seeing a white swan might not be owed to the seeing of a white swan, the idea that such experiences count as evidence is perhaps in need of further explanation.

We should note that this is no fault of Confirmation Theory. Even if Confirmation Theory (or some other variant of empiricism) turns out to be problematical as a comprehensive empiricism for this or that reason, we should

note that it is "problematical" in a very restricted sense: only as a candidate for comprehensive empiricism, not problematical *simpliciter*. Using Confirmation Theory as our running example, it is immensely fruitful in the domain for which it was designed. It is the dominant theory we use when assessing the empirical adequacy of theories, and the relation of hypotheses to their evidence bases. Such successes should not be trivialised by the lack of attention they receive in the present essay. We do not presume here that all empiricist projects need strive for comprehensiveness, and that falling short of that mark is some inherent deficiency; such a claim is far too strong. We merely hold the weaker thesis that there is an important role for a comprehensive empiricism in the contemporary literature, and that this role needs to be addressed; further, we hold that Gupta provides us with a framework within which this address may occur.

§III: Overview of Chapters

In Chapter One, we offer a thorough explication of Gupta's logical recategorisation of experience, the mechanics of the logic of interdependence (as developed by Belnap and Gupta, principally in their [1993]), and Gupta's minimal framework for normative empiricism as captured in a set of truisms and constraints. There are two truisms: The first is the "Insight of Empiricism", which we have already seen. It states that "experience is our principal epistemic authority and guide." (Gupta [2006a], 3) The other truism is "Multiple-Factorizability". We have alluded to it as well. This is the thesis that the "subjective character of experience – how things seem to be in experience – is a product of two factors: how things are, and our state and position in the world." (Gupta [2006a], 5) There are also four constraints: *Existence* commits one to the rational contribution of experience; *Equivalence* asserts that subjectively identical experiences are epistemically equivalent; *Reliability* that "[t]he given in an experience does not yield anything false or erroneous"; and *Manifestation of the given*, which ensures that a certain class of models of experience that trivially satisfy the first three substantive constraints are blocked. We examine how these basic commitments of any normative empiricism

whatsoever were thought to force on the empiricist a propositional model of the given, but how this model follows from an overly strong Cartesian reading of the truisms and constraints, not the truisms and constraints themselves.

Having established the basic machinery of Gupta's view, we examine in detail Gupta's novel interpretation of empiricism based on the essential interdependence of experience and view glossed above. In Chapter One, the logical details of the relevant notions (revision process, convergence) are elaborated. We also provide historical context for many of Gupta's arguments. These historical forays more fully motivate Reformed Empiricism, as it will be seen that many of the problems it solves are not only of a contemporary sort, but have been part of the philosophical landscape since the ancients.

In Chapter Two, we examine a variety of historical forms of empiricism with an eye to showing that there exists a bifurcated taxonomy of empirical views: on the one hand, we have classical British empiricism and its more modern sense-datum theory counterparts. Such views, while in congress with the sciences (as we have already seen in these introductory remarks), were able to account for our scientific conceptions only to a point—only to a point where the scientific conceptions were consistent with the Cartesian way of ideas that was adopted, in some way, by all major British empiricists (even by those like Hume who rejected the Cartesian-cum-Lockean conception of "idea"). On the other hand, it was not until Kant and post-Kantian forms of empiricism began to tackle difficult problems in the conceptual analysis of some of our key physical claims that such Cartesian theses are challenged, based largely (for post-Kantian empiricists) on empirical evidence and the demands of a better conceptual understanding of the key assumptions which govern the application of physical concepts (especially those about space, time, and motion). The existence of this camp of modern scientific empiricist thinkers (such as Helmholtz, Mach, and the logical empiricists) challenges Gupta's monolithic conception of "classical empiricism". We argue that Cartesian conceptions of experience and knowledge (as based on foundational certitude) were not so nearly as ubiquitous as Gupta assumes, and that arguments against the Cartesian

conception of experience begin with such thinkers. We in fact argue that a proper understanding of logical empiricism will show that its main figures, especially Carnap, held views which, while lacking Gupta's logical re-categorisation of experience, nonetheless militated against Cartesian foundationalism and the idea that the given in experience is incorrigible. This historical understanding of the development of empiricism from Locke to Carnap shows Gupta that he may encounter sympathetic voices in the history (at least in regards to some of the commitments of modern scientific empiricism).

In Chapter Three, we assess Gupta's view against a much more complex historical picture of empiricism. As an alternative to a generalised sense-datum theory of knowledge, his critical work is unassailable, but it is unclear how it might relate to the more scientific empiricism of the late 19th and early 20th centuries. Most significantly, it remains unclear how Reformed Empiricism might address a whole host of concerns which seem entirely divorceable from the Cartesian assumptions Gupta takes to be definitive of previous normative empiricism. In particular, we argue that while Gupta is right to say that his empiricism need not address issues regarding mathematical truth, what else may be said about how mathematical assumptions operate in our normal empirical judgments is not given much attention in Gupta [2006a]. More precisely for our study, Reformed Empiricism cannot be thought of as a candidate for comprehensive empiricism until such an integration of our formal knowledge can be proffered, and the account can be shown to extend to some of the critical questions associated with the kinds of mathematical assumptions required to even formulate a set of judgments of a mechanical (or proto-mechanical) type, such as judgments about relative distances of objects or the motion of objects through space (however construed).

We argue that Reformed Empiricism can only be extended to our scientific conceptions of the world if we add an admissibility criterion for initial views that respects the assumptions we make when applying spatial and temporal concepts in real contexts. This is captured in what we call the *Principle of Scientific Empiricism*. This principle blocks any purported initial view that does not have (as part) a

minimal and very weak conception of a shared space of interaction in which simple empirical judgments may be embedded. If such a primitive space is granted, then it can be shown how reflection upon empirical evidence (if not experience itself) may lead us from primitive to sophisticated understandings of space and time. We show this by focusing on several key instances of theoretical progress in modern physics, including the development of Galileo's modern mathematical physics (and its relation to Aristotle's natural science); the development of the Newtonian framework based on an extension and revision of Galileo's and Descartes' mathematical accounts of nature; the origins and development of the concept of an inertial frame; and Einstein's development of special relativity.

The rest of Chapter Three is devoted to a defense of the Principle of Scientific Empiricism as part of a genuinely empiricist account of knowledge. While it can be shown that such a principle is able to ensure the possibility of convergence upon sophisticated physical accounts of space and time, we might worry that such a principle is *ad hoc*, or (what is worse) non-empirical. We allay such worries by reflecting on the Principle of Scientific Empiricism vis-à-vis the truisms and constraints outlined by Gupta.

Our Conclusion is far from a general account of terrain covered. Instead of such general remarks, we discuss the relationship between Reformed Empiricism and contemporary work in the philosophy of science. We specifically survey two dialectical accounts of the development of theories of space and time from Newton to Einstein: Friedman's neo-Kantian conception of scientific progress in modern physics, and DiSalle's more focused account of dialectic argument as based on the unique form of conceptual analysis discussed above. Friedman [2001] argues that a dialectical opposition regarding long-standing debates between philosophical outlooks (such as the absolute-relational debate) are an arena for the rational discussion of alternative theories which are themselves too "rigid" to host such debates. DiSalle shifts focus away from the central significance such debates have been given in 20th century philosophy of science, arguing that dialectical analyses of connections between existing concepts and their empirical basis offer us an account

of the rationality of key instances of theoretical progress from Newton to Einstein.

Why such a discussion of dialecticism is pursued in the present essay is perhaps not immediately clear. Firstly, we examine these dialectical accounts because they study the process of revision on a large scale, making sense (at least in the case of DiSalle's dialecticism) of the role experience can play even when "views" (in this case, scientific theories) have a very complex character. The conceptual and historical scale on which such revisions are studied is relevant to our considerations here; we hope to show that similarly rational developments not only function within the sciences, but between the sorts of view studied more typically in general epistemology and the sciences. Secondly, we will argue that similarities between Reformed Empiricism and dialecticism regarding revision propose an interesting conjecture: that DiSalle's more focused and empirically sensitive dialecticism might be reconstructed using the logical machinery of Reformed Empiricism. In other words, Reformed Empiricism (plus the Principle of Scientific Empiricism) may go some way to illuminating the underlying logic of theoretical progress, even when thought of as occurring on large scales between "paradigms"—the sort of progress thought hitherto only amenable to a dialectical analysis. This confluence only lends credence to the flexibility and (comprehensive) applicability of Reformed Empiricism.

CHAPTER ONE

Gupta's Proposal for a Reformed Empiricism

The conceptual and technical problems that have hampered canonical interpretations of empiricism are well known. As to what sorts of views are considered "empiricist" outside of this canon, and as to the historical development of an "empiricist tradition," there is much less consensus. Beginning with the work of the so-called British empiricists, namely Locke, Berkeley, and Hume, and including more recent positivist and post-positivist positions in the philosophy of science—many of which have attempted to explain our scientific knowledge without the aid of extra-experiential or extra-evidential notions—"empiricism" includes a rich class of doctrines indeed, with varying commitment, goals and methods. The purpose of this present chapter is not to discuss such intricacies. Chapter Two will provide an historical account of empiricist doctrines so that we may highlight the nuanced differences between the projects of the British empiricists and their more modern counterparts. The purpose of this chapter is to offer an exegesis of a *new* proposal for empiricism, one that is based on radically different logical foundations than its predecessors.

We refer here to Gupta's Reformed Empiricism. The goal of Reformed Empiricism is to preserve many of the key empiricist commitments of old without recapitulating the theses that brought trouble and embarrassment to earlier empiricist projects, be they classical, Russellian, or logical empiricist. As we shall see, Gupta's work is immensely fruitful and insightful, allowing one to avoid many of the criticisms that were characteristic of the mid-20th century shift away from classical empiricism discussed in the Introduction, specifically those found in the work of Quine, Wittgenstein, and Sellars. But Gupta's careful analysis of what has gone wrong with previous empiricist projects allows us to see that many of the errors highlighted by such critiques do not call into question the basic commitments, tenets or truths of empiricism, only secondary theses of various

classical interpretations of empiricism. Empiricism is the proverbial baby thrown out with the bathwater.

An empiricism that can respond to such critiques is already worth exploring, but we will argue here that Gupta's proposal is most significant for other reasons. Firstly, Gupta's empiricism explicitly reintroduces the fundamental empiricist commitments (to be studied in detail below) that define empiricism as a normative epistemology, making clear (unlike previous empiricist projects) what views are central to any empiricism, and what views are idiosyncratic to an individual thinker (or, at most, central only to a specific school). Secondly, Gupta's proposal has a wide *prima facie* scope; not only is empiricism thought to capture our common sense knowledge of the world, but our theoretical and scientific knowledge as well. This makes it ripe for our application of it to the question of comprehensive empiricism. Thirdly, Gupta's empiricism shows great flexibility; unlike previous empiricist doctrines, Reformed Empiricism offers an epistemological account of our ability to revise concepts in light of experience (and therefore eschews a kind of "conceptual rigidity" in regards to fixing, e.g., the concepts of "direct awareness"). For Gupta, the very concepts of a privileged realm of experience may be altered based on the course of experience itself, leaving no mythical fixed realm of meaning as a kind of rationalist residue in his empiricism.[5] Lastly, Gupta's proposal builds on perhaps the greatest methodological commitment of late 19th century and early 20th century empiricism: the application of the most recent and refined formal tools to gain a better understanding of how our experience relates to our knowledge.

What exactly are the reforms that Gupta proposes in his Reformed Empiricism? The model attempts to discern the logical relationship between

[5] Conceptual rigidity is given its best expression in the empiricist tradition by Russell's wedding the notion of the given in experience to his epistemological distinction between knowledge by description and knowledge by acquaintance. All knowledge must begin with, or be grounded in, knowledge by acquaintance. Though "acquaintance" has a common sense meaning in Russell's earlier work, it begins to take on a very restrictive meaning when understood in relation to his developing sense-datum theory of perception. According to this theory, we are only acquainted with sense-data, their relations and their properties. Their non-relational properties are, however, fixed (by making them akin to Platonic universals), so that we have immediate knowledge of a private object's shape, colour, and size (relative to the visual or visio-imaginative field). The moral of the story is, of course, that no individual experience could upset such Platonic concepts.

experience, and what Gupta calls a "world view" (or "view," for short)—the epistemic agent's (often merely implied) understanding of the self and its relation to the world, what Gupta refers to as a "world-self combination." Any experience can always be the result of a potential infinity of world-self combinations. Given these possibilities, and given that at least some of them can be mutually inconsistent, a problem arises: we think that experience justifies (at least some of) our beliefs, yet experience cannot play such a role without the epistemic agent having some picture of the world. Empiricism is in the difficult position of closing this circle: our experiences can justify our beliefs only if we take its dictates to be true, i.e. only if we have a correct picture of the world; but we can only know that such a picture is correct via experience. If empiricism is to be viable, it must, therefore, explain how we can move from the conditional entitlements which anyone[6] can see are granted by an experience (given a view), to categorical entitlements, i.e. the knowledge that our view is correct and that our perceptual judgments are (by and large) true. It is this model that we will examine in more detail here.

We should pause on the nature of this very difficult problem in order to understand just why it has been such a hard philosophical nut to crack. The problem stretches back to the origins of philosophical scepticism, though its persistence or solution need not be tied to that family of doctrines. One popular problem posed by scepticism goes back at least as far as Carneades (214-129BCE), the founder of the Third Academy.[7] He developed several arguments that precluded the traditional grounds of the Stoic on which beliefs were thought reasonable. Following Zeno, the Stoics believed that one could achieve a state of wisdom through the cognitive grasp of a particular set of impressions that were self-warranting. Because these impressions were self-warranting, assent to them had a twofold effect: we

[6] A note on scope: only those who think there is a rational given in experience will assent to such a claim. Many have rejected this position since Sellars' sustained attack against the "myth of the given." (For the most straightforward expression of such a view, see Davidson [1988/2001].) We will see below that Gupta's empiricism can overcome Sellars' attack against this myth, establishing a different logical grammar for the given. For now, the remark that our claims are restricted only to those who think experience is rational will have to suffice.

[7] Carneades did not record any of his own philosophy, engaging, like Socrates, in active dialogue instead. His students, especially Clitomachus, did record some of his doctrines. While the works of Clitomachus have been lost, they were discussed by Cicero and Sextus Empiricus.

expressed our belief that they were true while also grasping their truth in one fell swoop. Such impressions could not be the stuff of mere opinion; to possess them was to also have knowledge of them. Basing knowledge on such impressions, and rejecting any opinion (i.e. assent to impressions which were not self-warranted) could allow one to arrive at certain knowledge via the senses.

Cognitive impressions, on this model, are (a) true, (b) properly caused by the object(s) represented by them, and (c) very precise and rich so that they can preclude the possibility of being about objects that do not properly cause them. To use modern parlance, they are the principal component in a direct and causal representationalism (but, because of their direct and causal nature, such impressions need not be regarded as representational at all). Like Socrates and Plato, the Stoics assumed that epistemology was concerned with certainty, and one's possession of a kind of wisdom or certain knowledge. Unlike Socrates and Plato, the Stoics believed that certainty could be achieved through the senses in special cases—cases in which one had cognitive impressions.

The first sceptical response to such a view came from Arcesilaus, the founder of the Second Academy.[8] He granted that some impressions may be true and caused in the right sort of way, but he denied that any impression is ever so rich as to preclude the possibility that it was caused by the wrong objects. Carneades developed these sorts of arguments more fully, positing that perceptually indistinguishable objects prevent the Stoic from establishing (c). The richness criterion of the Stoics, like other proposed "guarantees" of certainty, was an academic chimera. What kind of representational precision could the Stoics grant to perception—how veridical did impressions need to be—to discern between nearly identical objects?[9] If the Stoics cannot satisfy their own richness criterion, the entire

[8] Although there is much debate regarding the nature of Arcesilaus' scepticism, as a result of his doctrine being multiply interpreted, his primary concern was formulating arguments against Stoic epistemology. We can happily leave aside differences in the historical scholarship, which focus on the nature of Arcesilaus' positive philosophy. Interpretations of his work as a critic are much less contentious.

[9] The classical examples of nearly identical objects which challenge the Stoic notion of richness are eggs, grains of sand, or even multiply reproduced artifacts.

edifice of their epistemology collapses.[10] (Of course, one could deny altogether that any notion of richness could guarantee veridical representation. What is claimed here is much weaker: the sceptic is saying simply that the Stoical notion of richness is not met; however, the examples provided can be generalised so as to show that no notion of richness can guarantee veridical representation.)

The sceptical insight operating here is this: where object and appearance aren't in immediate and guaranteed correspondence, there can be no certainty, and *experience itself provides no indication whether this correspondence has been established*. The problem for modern empiricism, beginning with Locke in the 17th century, thus becomes a problem of answering this sceptical charge regarding our experiential knowledge. How can knowledge be based on experience when knowledge requires certainty and experience seems to grant us nothing like certainty? In response to this problem, empiricists adopted a Cartesian view of experience: one is directly aware of one's own impressions, however the objects of such impressions are not taken to be external, but phenomenal, or, if external, only known by *in*direct representation.[11] The very act of receiving an impression provides its object, but not as a matter of (direct) representation; empiricism is able to respond to the sceptical problem by avoiding the Achilles' heel of the Stoics: a commitment to direct realism. However, the rejection of such a commitment comes at a cost, for the truth of our claims about external, material objects (should we wish to make any) now rests on a very shaky foundation, for it seems impossible to provide an empiricist justification for the inference to external objects. The only other viable option is to reject both realism and scepticism in one fell swoop, as

[10] These arguments influenced different sceptics in different ways. Some sceptics believed that we needed to purge ourselves of any and all substantive commitments, since a lack of certainty would leave us forever frustrated and unable to act. Others denied the very possibility of truth. Arcesilaus and Carneades, from what we have seen, thought there was a notion of truth left unscathed by such arguments (as their assumption to grant the Stoic the truth of impressions indicates), but that our access to such truth was at best *probabilistic*. In order to explain action as a result of our beliefs (without the possibility of certain knowledge) the attractiveness of a probabilistic epistemology is clear, and it was this epistemology that Carneades developed in response to the Stoic's infallibly known cognitive impressions.

[11] One is always making some sort of inference, whether sometimes licit (as Locke would have it) or always illicit (as it was for Berkeley) when she begins to talk about the material causes of ideas.

Berkeley did, by adopting a thoroughgoing idealism. Empiricism is thus left in a dilemma: either scepticism or idealism.

So much for the Stoical project. What we propose to study in the present essay is the epistemological *legacy* of such debates, and the prospects for an empiricism that, unlike British empiricism, rejects altogether any commitment to these Stoical themes—and the dilemma of scepticism or idealism to which they lead. Gupta's work offers us just such a prospect.[12] In what follows, we will provide a close reading of Gupta's reformed model of experience. In §I, we will look at the antecedent commitments that inform any rational model of experience. These commitments come in the form of two truisms which need to be preserved by any model of experience, what Gupta calls the "Insight of Empiricism" (the truism that experience is epistemically primary and authoritative) and "Multiple-Factorizability" (the truism that a given experience can be the product of many different world-self combinations), and four constraints imposed by the given that must not be transgressed by any empiricist account of experience.

In §II, we will introduce the Cartesian model of experience and its attempt to satisfy these truisms and constraints, and survey Gupta's criticisms thereof. We will also examine some of the ways in which Descartes' model of experience was adopted by early empiricists, though a detailed discussion of such issues will be reserved for Chapter Two.

§III offers a thorough recounting of the mechanics of Gupta's model of experience, specifically focusing on the logic of interdependence and his notion of a "revision process," in an attempt to understand the rational contribution of experience. Gupta argues that experience plays a principal epistemic role, but that it only makes its "pronouncements" in an essentially *interdependent* way. Just how experience relates to a "view" will be discussed, as will be the formal framework within which belief revision takes place as a result of experience.

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[12] This is not to say that other forms of empiricism fail to move beyond the framework set in place by ancient scepticism and the Stoics. In fact, as we will see in Chapter Two, much of logical empiricism of the first half of the 20th century simply ignored the pull of such long-standing views in the hopes of creating philosophy anew as a scientific discipline, and their position was anticipated (in some ways) by post-Kantian empiricists like Helmholtz and Mach.

We should also pause here to make a terminological note. Any mention of "previous empiricism" or "classical empiricism" made in this chapter intends to pick out a general sense-datum theory of perception wedded to a notion of certain knowledge: a sense-datum foundationalism as it were. No more precise notion is required to motivate Gupta's project. As Chapters Two and Three make clear, a more precise and complete account of empiricism's development in the last two centuries is required for the proper assessment of Gupta's project, but such matters are of future concern.

§I: The Basic Commitments of Empiricism

The following will be a gloss of the minimal commitments that are required by any empiricist model of experience that grants experience and its possible contents a normative role in justifying our beliefs. Where necessary, I will motivate these commitments by putting them in their philosophical context. We should immediately note that not all epistemological models afford such a normative role to experience. Such views, particularly as they have been proposed by Quine and later developed by Donald Davidson and others, will not be addressed in detail. In what follows, this essay will be concerned only with models of experience that attribute to experience the ability to make rational contributions to our knowledge.

I.1: Gupta's Two Truisms

Gupta starts his [2006a] by establishing the framework of commitments within which he will develop his reformed model of experience and his Reformed Empiricism. The work begins with a discussion of what Gupta calls the "two truisms," or two theses which he thinks are in tension with one another. The first truism is the "Insight of Empiricism," the second "Multiple-Factorizability." According to the Insight of Empiricism, "experience is our principal epistemic authority and guide." (Gupta [2006a], 3) The scope of such authority and guidance is wide according to the Insight: "It is experience that pronounces on the validity of our concepts, conceptions and theories about the world." (ibid) It is misleading to

Gupta draws a physical analogy: if two forces simultaneously act on a body, and we know those forces, we are able to determine the resultant force; however, given only the resultant force, one can't recover the initial forces acting on the object. In short: "No experience carries with it its own genealogy." (Gupta [2006a], 7) This may seem to put us in a rather sceptical position. Given Multiple-Factorizability, it seems as though one could never be justified in making even the most mundane, everyday perceptual judgments. Similarly, one would be unable to justify even the most basic, common sense beliefs about her position in a world of medium-sized objects. What Lewis called "Moorean facts"[13]—statements like, "Here is a hand, and here is another"—seem in dire straits if Multiple-Factorizability is true. The *prima facie* truth of such judgments is often taken to be the basis our common sense view of the world, and such judgments are especially important in philosophical defenses of our common sense knowledge.

Take Moore's proof of the external world. This proof is based heavily on the soundness of the following simple argument:

> *Premise*: "Here is one hand, and here is another," (said while making particular gestures and looking in the relevant places).
> *Conclusion*: (At least) two human hands exist at this moment.

By Moore's proof standards, this argument must (a) not beg the question, (b) have the performative component performed by someone who looks in one place and sees a hand, then looks in another place and sees another hand, and (c) have the conclusion follows from the premise.

The argument, according to Moore, satisfies all three criteria. Because the conclusion could be true while the premise false, each clearly expresses a different proposition, so the non-question-begging criterion is satisfied. If the premise is true, then so is the conclusion, so the argument is valid and avails itself of an inference that naturally connects the contents of the premise to the conclusion. If the sceptic is

[13] The explication of Moorean facts is owed to Lewis [1996]. He says a Moorean fact is "one of those things that we know better than we know the premises of any philosophical argument to the contrary." (549)

to find fault in the argument, it must rest with criterion (b). One must show that Moore doesn't have knowledge that there is one hand here, and another hand there. The sceptical argument is in the form of *modus ponens*:

(P1) If *A* does not know that he is not dreaming (hallucinating, etc.), then *A* does not know that there are hands.
(P2) *A* does not know that he is not dreaming (hallucinating, etc.).
(C) Therefore, *A* does not know that there are hands.

Moore responds by offering what has since been called a "Moorean shift." One simply "reverses" the argument of the sceptic, putting it in the form of *modus tollens*:

(P1) If *A* does not know that he is not dreaming, then *A* does not know that there are hands.
(P2) *A* does know that there are hands.
(C) *A* knows that he is not dreaming (hallucinating, etc.).

The use of *modus tollens* here shows, in stark logic, the epistemic standing Moore gives to our common sense judgments. He elaborates this view in his [1925] by offering more than a Moorean shift, rather a sustained dialectical argument against the sceptic. Moore starts the argument by showing that it is perfectly reasonable to assert simple judgments about external objects, and to hold the relevant common sense beliefs on which such judgments rest; however, one must rely on various philosophical intuitions, implausible thought experiments, and idealistic theories of perception if one is to be in a position to found any sceptical belief which can call our common sense judgments into question. All of these sceptical pre-conditions and arguments are far more contentious and epistemologically suspect than the common sense beliefs and judgments that Moore defends. The fact that I have hands is more evident than the possibility that I am a brain in a vat, a deceived Cartesian ego, etc.[14]

[14] What Moore has really done is set the standard for knowledge much lower than philosophical certitude. The Cartesian tradition is based on the foundational notion that belief not based ultimately on certain knowledge is epistemically suspect. Moore is giving us common sense grounds on which we can establish the reasonableness of doubt. Wittgenstein [1969/1972] develops these ideas. For

The sceptic to whom Moore is speaking is employing an *argument from assumption*: the demonstration offered in the premise for the existence of the external world is not a *proof* based on *certain* foundations. Thus, the sceptic will not grant Moore knowledge of the "fact" that he sees two hands. Moore relaxes the restraints on demonstration, arguing that the sceptic can provide no reason for him not so to relax them.

We ought not dwell on such matters further. This debate about our common sense judgments has already done the job we want it to do, which is to highlight three important facts about Multiple-Factorizability. Firstly, it can be seen that Multiple-Factorizability is independent of all of the ancient sceptical modes. Multiple-Factorizability does not avail itself of the mode from assumption, as Moore's sceptic does. Multiple-Factorizability merely states the fact that judgments about one's hands, in spite of their antecedent rationality, are not rational *come what may*. The sceptical mode from assumption says that such judgments are *never* reasonable, but this is much too strong. There is a superficial similarity between Multiple-Factorizability and the sceptical mode from relativity that should be noted. According to this mode, the object—for example, one of Moore's hands—"appears thus or thus in relation to the thing judging and the things perceived along with it," (Sextus Empiricus [2002], 372) and therefore one should not judge regarding the true nature of the object. But, as Gupta is quick to point out (Gupta [2006a], 9), the relativity of sense perception holds that one object can lend itself to many different kinds of experiences, whereas Multiple-Factorizability states that one experience can have many different objects. Multiple-Factorizability is logically independent from the sceptical mode from relativity, or its more recent reincarnations, e.g. the relativity of sense perception.

Wittgenstein, doubt is something that can only arise in a language-game, in which the speakers are using the words with a constant meaning. The sceptic is in the odd position of not being able to give meaning to her notion of doubt, for she can't say what it means to doubt if she can't also say what it means not to doubt something. A doubt can only make sense if an example can be given of a non-doubt, i.e. "doubt" has a sense only when its negation has a sense, but the sceptic has set up the game in such a way that the negation cannot have a sense, and we are merely to assume with the sceptic what it would be like to doubt at such a point. (See §24, and also §§354-375.)

Secondly, Moore's response assumes a stark dichotomy. Either the sceptic is correct or common sense realism is correct. What Gupta hopes to show is that this dichotomy is falsely thought to be exclusive. In fact, one can believe (with Moore) that one's regular, present experiences can rationally entitle one to make such judgments, but this in no way contradicts Multiple-Factorizability:

> What we call "ordinary judgments of perception" do not issue from experience alone. They are founded in experience *and* conception. Ordinarily, we have no need in our discourse to question or to vary the conception, and we speak as if the justification for perceptual judgments lies solely in experience. The truth is, however, that every experience has multiple factorizations, and no experience *by itself* provides any justification for ordinary judgments of perception. (Gupta [2006a], 7)

Given that Multiple-Factorizability is independent of the ancient sceptical modes, and given that it is not inconsistent for one to hold both a common sense view of the rationality of simple perceptual judgments and Multiple-Factorizability, we arrive at the third important fact about Multiple-Factorizability: the problem of providing an account of the rational import of experience is independent of any sceptical philosophy or classical common sense alternative. The problem is not one of scepticism, and, as we shall see in §II, empiricism was for far too long wedded to the following two Cartesian views: (a) the sceptic had to be neutralised and responded to, and (b) the problem of experiential knowledge is solved once one has been successful in such a response to the sceptic. There are, of course, many ways of building a philosophy that side-steps the sceptic. One way, favoured by Davidson, Rorty, and others, is to tell the sceptic to "buzz off." Gupta wants to show that empiricist commitments do not force one to engage with the sceptic on the sceptic's own terms, but neither should we completely ignore what the sceptic has to say. We can learn much about how to view empirical rationality by reflecting on sceptical positions.

We are thus left with a very serious problem if we hope to offer an empiricist account of how our knowledge can be adjudicated and guided by experience. On the one hand, the Insight gives broad berth and scope to the normative role of

experience in all epistemic matters. On the other hand, Multiple-Factorizability seems to undercut the Insight. Experience alone cannot adjudicate our claims to knowledge. (Gupta [2006a], 10) If one had a true conception of the self, then one could, based on experience, arrive at true perceptual judgments. However, at the beginning of enquiry, we have no guarantee that our conception of the self is true, and no guarantee that experience is providing us with such truths.

I.2: Four Constraints on Any Normative Account of Experience Whatsoever

A model of experience need not only contend with the two truisms, it must also satisfy four minimal constraints—constraints which express the content of an analysis of what one could possibly mean by "the given".

The first constraint, *Existence*, commits one to the rational contribution of experience. "Something is given in each experience; that is, each individual experience makes a rational contribution to knowledge." (Gupta [2006a], 19)[15] This commitment is in one sense very strong. It blocks from consideration all purely causal accounts of experience, i.e. all accounts within which experience plays no rational or normative role in our knowledge. In another sense, it is very weak. Aside from acknowledging the rational contribution of experience to knowledge, it makes no commitments as to the nature of experience, nor does it commit itself to the magnitude (or, as Gupta puts it, the "logical weightiness") of such a contribution. (Gupta [2006a], 19-20) Experience, as far as this constraint is concerned, may make its pronouncements in the form of propositions, or may bring before the mind external objects or representations of those objects, or merely raw, formless contents which the mind must categorise (the so called "bricks and mortar" view of experience), *etc*. The motivation for imposing such a constraint is to capture the

[15] A related, though independent, thesis is held by empiricists: "*the given in experience can logically force our knowledge of the world*." (Gupta [2006a], 21 – italics in original) This thesis is obviously a strengthening of the *Existence* constraint for the purposes of developing a specifically empiricist account of our knowledge. This should not be confused with other empiricist theses, such as the classical psychological thesis of British empiricists that all of our knowledge has a causal origin in experience, the more modern verificationist version of that classical thesis that states all of our *substantial* knowledge is based in experience, or, as Gupta notes, methodological theses regarding the primacy of empirical data in the sciences. It is merely a logical thesis about the rational import of experience.

minimal rational role of experience on any model of experience that affords a rational role to experience.

The second constraint is *Equivalence*. This constraint asserts that subjectively identical experiences are epistemically equivalent. "If *e* and *e'* are subjectively identical experiences of an individual, then the given in *e* is identical to the given in *e'*." (Gupta [2006a], 22) This constraint is agnostic about the veracity of such experiences; *e* may be veridical, *e'* hallucinatory. As Gupta says, the constraint "places a restriction only on the epistemology of experience, not its metaphysics." (ibid) Gupta is quick to note two interesting features of this constraint. The first is that it speaks only to the reasonableness of a subject's beliefs based on the course of her experience, not on the truth of those beliefs. Such a constraint is concerned only with the internal perspective of the cognitive subject, not an external evaluation of the beliefs one is warranted to have given what facts hold about that subject's environment.[16] This feature of the constraint is motivated by the basic empiricist commitment that two relevantly similar rational agents undergoing the same experiences should not be able to have radically different views of the world. (Gupta [2006b], 184) If this were the case then something other than experience must be referenced to account for the difference between their views. Rationalism can countenance such a scenario, empiricism cannot.

The second interesting feature of *Equivalence* is its applicability to possibly subjectively identical experiences in different subjects. While Gupta prefers to individuate experiences by building the subject of the experience into the experience itself, meaning that *e* and *e'* cannot be identical if experienced by different subjects,[17] he thinks we can modify *Equivalence* so that two "similarly endowed" subjects will have the same set of reasonable beliefs based on subjectively identical experiences. One may think that such claims contradict each other. If

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[16] Gupta's notion of reasonableness is an internalist one, though not contentiously so. As he states on pg. 29, fn. 17, the debate between internalists and externalists is really about epistemic warrant, and his account of reasonableness can be happily agnostic about such matters. Gupta is only concerned about the rational relationship of experience to knowledge, which need not avail itself of the notion of warrant.

[17] See Gupta [2006a], footnote 16.

experiences are individuated in the first place by the subjects that experience them, then how could two subjects share a subjectively identical experience? To answer this question we should draw attention to a priority relation that is implied in Gupta, but never explained in detail, which dissolves the seeming contradiction. The following argument hopes to make matters explicit:

(1) Represent "*e* is an experience of subject *X*" (for any value of *X*) as e_x.
(2) If *e* is experienced by *A*, then e_A.
(3) If $A \neq B$, then e_A cannot be an experience of *B*, i.e. $e_A \neq e_B$.

Together, (1)-(3) represent the subject-relative identity condition on experiences.

(4) For any subject *X*, if *Ree'* (where *R* is an identity relation ranging over experiences and *e* and *e'* are experiences), then if *e* makes a set of judgments, Γ, reasonable for *X*, then *e'* will make Γ reasonable for *X*.

It should be noticed that (4) is of the form of *Equivalence*, though the nature of identity in relation *R* is left undefined. This is important for the next part of our explication.

As we have seen, Gupta allows for the experience of different subjects to be subjectively identical; insofar as those subjects are similarly endowed (with relevantly similar sensory organs, cognitive systems, and views), subjectively identical experiences will yield the same entitlements to the same set of judgments Γ for both subjects. Such a state of affairs meets *Equivalence*. We represent this as follows:

(5) For any two subjects *A* and *B* (where $A \neq B$), if $R' e_A e_B$ (where *R'* is an identity relation ranging over experiences, and e_A and e_B are experiences) then, by (3), the relation $R \neq R'$.

What (5) tells us is that there is an asymmetry, or better yet a priority relation, between the identity relations *R* and *R'*. Call *R* "identity of experience" and *R'*

"subjective identity of experience." These relations must satisfy the following conditions:

(6) *Ree* implies *R'ee*
(7) *R'ee* does not imply *Ree*

Thus, while the experiences of different subjects can never be identical (in the broader sense), they can be subjectively identical to one another. As to what sorts of other conditions in addition to subjective identity *R* would have to satisfy, we make no claim here. Such matters are entirely avoided, for *R'*, i.e. *subjective identity*, is the only relation required to establish *Equivalence* or its intersubjective variant.

The third constraint is the *Reliability* of experience, or the view that "[t]he given in an experience does not yield anything false or erroneous; in particular, it does not yield a false proposition." (Gupta [2006a], 27) Gupta is quick to remind us that this constraint is concerned only with the rational given in experience, not the causal role of experience. It is certainly true that at least some of our false beliefs are *caused* by experience, but this is no fault of the given in experience. But even when properly construed as an epistemological claim, purged of any causal notions, the constraint is strong. Often, such a constraint has been thought too strong. A model of experience which can satisfy this constraint must make clear how it can be the case that the given can never be "wrong". We will see how the Cartesian model attempts to meet this constraint along with the philosophically troublesome theses it musters for the task in the next section. In the fourth and final section, we will see how Gupta offers a novel re-characterisation (or re-categorisation) of the logical grammar of experience in order to satisfy this constraint in a much more plausible way, avoiding the pitfalls of the "framework of givenness," as Sellars called it, and remaining true to *Reliability*.

These constraints, if followed, are sufficient enough on their own to account for a great many philosophical views. However, as they stand, too many an absurd model could be devised under their auspices, meaning that the set of constraints must be strengthened. The fourth constraint does just this. Gupta refers to it as

Manifestation of the given: "The given in experience must be manifested in that experience; that is, it must depend systematically upon the subjective character of the experience." (Gupta [2006a], 30) This constraint is necessary to block a family of models which would simply assign some material truth to every given in experience. Gupta uses the example, "The Earth's core is, was or will be hot." A model which employs this proposition (or others like it), assigning this content to the given of any experience whatsoever, will satisfy *Existence*, *Equivalence*, and *Reliability*. The proposition exists; everyone with the same subjectively identical experience will be able to make the same judgments and inferences (supported as they are by this measly content); and, since the proposition is always true, it is reliable—no given in experience will ever yield a falsehood. *Manifestation* is required to block such assignments, and to make real our commitment to the notion that experience tells us something about the world that cannot be captured by the arbitrary coordination of meaning. (We do often engage in arbitrary meaning assignments, especially in the sciences, but this is not the only way experiences can be related to meanings.)

One "natural" way of satisfying these constraints is found in the work of Descartes, who, applying the methods of the sceptics surveyed above, arrives at unmistakable impressions of the sort that the Stoics once sought. This model is indeed a powerful one. It bases itself on a foundationalism that assumes that a rational insight can guarantee, prior to any experience whatsoever, that one's beliefs are (at least by and large) true, and some of the most foundational of them known with complete certainty. In the next section, we will not examine in depth Descartes' rationalist arguments, but rather his view of experience. The model of Cartesian experience, it will be seen, was a potent instrument in the development of British empiricism, and completely divorceable from Descartes' rationalism. One could reject the rational foundations of that view and still preserve the model of experience as privileged access to a realm of indubitable subjective data. To that model we now turn.

§II: A Cartesian Model of Experience

When one describes this or that philosophical position as "Cartesian," two concerns should always be present. Firstly, to what extent is such a description *pejorative* as opposed to historically and philosophically accurate? Secondly, in what way is the position Cartesian—or, in other words, what part of Descartes' philosophy is being recapitulated or replicated? We will see Gupta's account of how the principal British empiricists—Locke, Berkeley, and Hume—adopted, without significant alteration, a Cartesian model of experience. In this section we will examine these Cartesian themes as they relate to the development of early empiricism. We shall also see how such themes culminated in a general sense-datum theory of perceptual knowledge and the shortcomings of that theory.

II.1: Cartesian Doubt and the Subjectivity of Experience

Descartes' epistemological program in many ways sets the stage for all of modern epistemology, establishing in the modern period the program of indirect representationalism, the epistemic role of certainty and doubt, and the significance of empirical claims within a system of justification that eschews the sort of treatment such claims received under the old Scholastic and Platonic models. On the Aristotelian model of human knowledge, empirical claims require the potentiality of the mind to be actualised by the senses. The senses are essential to the mind. As Aristotle wrote, "What [thought/mind] thinks must be in it just as characters may be said to be on the writing-table on which as yet nothing actually stands written: this is exactly what happens with thought." (Aristotle [1984], III.4. $430^{a}1$) This is in stark contrast to the view of the mind held by Plato. For Plato, the mind (as perfect, unchanging and eternal) is essentially divorceable from the senses; in fact, the senses function only as epistemic barriers to achieving true knowledge of the Forms via *dialectic*. For Plato, all sensory experience is "imperfect," and our empirical judgments only have sense (though are not true or certain) relative to a set of

perfect concepts known through *Anamnesis*—i.e. held innately and eternally by the soul.[18]

The Cartesian "method of doubt" attempts to establish the certainty of at least some of our beliefs by rationally discerning them from beliefs that can be subject to doubt. Insofar as this part of the project is of primary importance for Descartes, he follows in the rational tradition of Plato regarding the innate possession of certain knowledge. However, Descartes' rationalism is intended to buttress an account of experiential knowledge so that information from the senses can be viewed as more than a barrier to pure rational insight. The method of doubt thus serves a twofold purpose: firstly, it establishes those propositions about which we cannot be mistaken, and, secondly, it shows how such rational insights also secure for us an account of how our normal and dubitable empirical judgments are, notwithstanding their defeasible nature, trustworthy. The first purpose is Platonic, and betrays Descartes' commitment to the mind as (*pace* Scholasticism) fully actualised without the senses. The second purpose is decidedly non-Platonic; it establishes empirical beliefs as genuine (though not foundational) parts of our knowledge. This is done in two steps: firstly, the *cogito* establishes a foundational basis for knowledge that cannot be doubted (even by the most enthusiastic sceptic), and secondly by the ontological argument, which establishes the existence of an omni-benevolent deity, corresponding to (though strictly "greater than") my concept of such a deity. Since such a deity is good, He would not allow me to be globally deceived: while any specific empirical belief I possess may be incorrect, the deity would not allow for global error.

We rehearse these Cartesian arguments here not to establish their validity or accuracy. Rather, we hope to tease out of them some key epistemological themes

[18] Plato's "equality argument" in the *Phaedo* establishes this epistemology, though earlier dialogues (such as the *Meno*) present similar positions. For Plato, all empirical judgments of "equality" between two objects are imperfect in the sense that no two objects are actually equal. We recognise this imperfection in our application of the concept when asked to reflect upon such judgments. Thus, the empirical relation of equality is (by our own recognition) imperfect, yet grasp of an imperfect concept implies that we also have a grasp of a corresponding perfect concept. Since this perfect concept can't have come from experience, it must be possessed by the mind prior to experience, and accessed by some faculty other than empirical perception, e.g., direct rational insight.

which, when taken out of the context of Cartesian rationalism, capture many commitments we have to the study of knowledge more generally, but especially to the study of empirical knowledge—including its nature, its sources, and its justification. For Descartes, knowledge must have a certain source. Like Plato and the ancient sceptics, Descartes holds that no measure of certainty can be granted to the information that comes to us via the senses (at least not thought of independently from rationally arrived at guarantees). No sensory impression is apodictically certain. From this tenet, however, he does not infer, as did Plato, that experience is in some way epistemologically suspect. Within the correct framework, our "normal" experiences can be taken as veridical, and our empirical judgments can be taken as at least not in global error. Even sceptical presuppositions lead one to accept the very same account of empirical judgment that would accompany a common sense understanding of our experiential interaction with the world.

Very much at the heart of these commitments is the Cartesian model of experience as a kind of subjective theatre within which representations, perhaps accurate but perhaps merely illusory (or delusory), take centre-stage. (We here focus on Descartes' model of experience, but the theatre also houses imaginings, memories, and representational beliefs of a non-experiential kind, such as the belief in the existence of the self or God.) As the Cartesian picture has it, experiences fail to carry with them any indication of their veracity, and thus any subjective episode may be the product of any number of factors. Take an experience of looking at what (at least seems to be) a red apple. The episode could be the product of actually seeing a red apple in the environment. Call this a "veridical experience". The episode could be the product of seeing something in the environment that looks like an apple, but is not—e.g. a large plum, or a plastic decorative apple, *etc*. Call such an experience a "semantically imprecise experience". Lastly, the episode could be the product of a dream (or even just simple illusion). Call such an experience an "illusory experience". What Descartes is committed to, and what empiricists after Descartes are also committed to, is the assumption that there is no immediate epistemological difference between these episodes because there is no way for us to

discern the differences between them by mere introspection. Given that knowledge is principally a matter of our introspective access to experience on the Cartesian model, this follows almost as a corollary from the *way of ideas*. Such distinctions, whatever their metaphysics, are epistemologically (representationally) neutral.

The subjective nature of experience in Cartesian (and Cartesian inspired) epistemology rests on two distinct arguments. The first is the "dream argument," with a rich philosophical history going back to the ancients,[19] and given several modern re-statements.[20] The dream argument exploits our supposed inability to distinguish between veridical, semantically-imprecise, and illusory experiences when said experiences are subjectively identical. According to this argument, our normal perceptions (and perceptual beliefs) are possible products of a dream-state, one in which the subject is undergoing the sorts of perceptual states as if she is awake (and in "normal" contact with reality via her senses). Because we could never say that such a scenario is an impossibility, we must be suspicious of the senses and their ability to give us certain knowledge (i.e. the sort of knowledge that does not admit of such possibilities).

The second argument is the "argument from illusion," used to reach an independent conclusion. The argument from illusion asks us to take some example of a perceptual illusion—for example, the apparent bending of a stick when it is partially submerged in water. Now we suppose that a stick does not actually bend when placed in water, and ask why it appears to bend (and what the nature of this apparent bending may be). Since we have assumed that the actual stick (whatever that may be) did not bend, we conclude that the thing being bent is some representative (or perceptual proxy) of the stick (an idea, sense-datum, etc.). The

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[19] Both Plato and Aristotle mention the dream argument. In Plato [2004], Socrates talks about not only dreams, but disease and madness, as instances where our senses cannot be trusted, leading to a critique of the Heraclitean theory of flux entailed by Theaetetus' proposal that knowledge is nothing other than perception. Aristotle mentions dreams in *Metaphysics* $1011^{a}6$ during his discussion of truth and appearance, and chastises those that use the dream argument for "seek[ing] a reason for that for which no reason can be given."

[20] There are far too many sceptical hypotheses in the modern literature to cover here, but such a list would surely include simulation hypotheses (such as Nozick's experience machine and its variants), evil demon or scientist scenarios (including ones where brains end up in vats), and, perhaps the most recent variation of the dreamer sceptical hypothesis (proposed by Ernst Sosa in his [2007]) in which dream contents are not beliefs but *imaginings*.

argument concludes by asserting that our perception of the world is always mediated by indirect representations. Notice, however, that this is a much stronger conclusion than the one reached by the dream argument. The dream argument has as a premise the proposition that it is possible that our current mental states are nothing but the product of dreams. In conjunction with the presupposition of the way of ideas we arrive at the conclusion that our senses cannot be the basis of certain knowledge (because certain knowledge cannot admit of even the possibility of illusion/semantic imprecision). It does not establish, nor even attempt to establish, the way of ideas. The argument is, therefore, only attempting to speak to those who already adopt something like a Cartesian epistemology and model of experience. The argument from illusion attempts to establish the truth of the way of ideas to the uninitiated or unconvinced. On this score, it has not been as successful in the past fifty years,[21] during which time the dream argument and its many modern variants have played a central role in the development of not only epistemology, but externalist semantics and anti-individualist philosophies of mind.

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[21] Such commitments were central for almost all epistemologists until the ordinary language philosophers of the 1950s and 1960s. As we have already seen in footnote 14, Wittgenstein challenged the very possibility of a global scepticism, for it makes no sense to doubt all things. Similarly for Ryle, the argument from illusion in the *Meditations* relies on something of an inconsistency. For Ryle, "just as it makes no sense to talk of counterfeit coins when there are no genuine ones to contrast them with, so it makes no sense to talk of illusory experiences like dreams without waking and veridical ones to contrast them with." (Ryle [1954/1960], 94-95) The dream argument concludes that there is no subjective comparison to be made between dreams and waking experiences, but its premises ask one to imagine that all of what one perceives is a dream. Austin likewise thinks an inconsistency is lurking in the very idea that we can't discern normal from illusory experience in his posthumous [1962]. In that same text, he is also sceptical regarding our supposed inability to recognise the difference between dreams/illusions and normal experiences. Regarding the distinct argument from illusion, taken to establish the perceptual primacy of ideas or sense data, Austin attacks the key inference (from the proposition that during illusory states of perception we are only acquainted with our sense-data to the proposition that we are *only ever* perceptually acquainted with our sense-data).

Contemporary disjunctivists grant the Cartesian idea that nothing in introspection will settle whether an experience is illusory/semantically-imprecise or veridical. However, they do not agree with Descartes that the veridical experience has the same rational content as non-veridical experience. For disjunctivists, the relevant object (or external state of affair) enters into the content of veridical experience, but not into the content of the non-veridical experience. See Hinton [1973], McDowell [1982], and Martin [2006]. We address McDowell's disjunctivism in Chapter Three, with particular attention to this contention that veridical and non-veridical perceptions have different givens (an idea which runs contrary to Gupta's *Equivalence* constraint).

Setting aside the contemporary role of the dream argument or argument from illusion, we can establish a more modest (yet still very interesting) thesis. Early empiricists, namely Locke, Berkeley and Hume, adopt much of this Cartesian outlook in the development of their doctrines. While a more thorough analysis of those doctrines must wait for the next chapter, it is clear that foundationalism, the transparency of ideas to the mind, the fundamentally indirect representational nature of ideas, and the subjectivity of experience are all commitments that form a central core of early British empiricism.

II.2: A Cartesian Interpretation of the Truisms and Constraints

Utilising the truisms and the constraints offered by Gupta (glossed in §I above), we can see why the empiricist may be drawn to these Cartesian themes. Firstly, if knowledge is taken to be foundational in character, and based on a string of reasoned justifications, then it is natural to assume we are dealing with propositions and their contents. All of our claims to knowledge, from the lowliest and simple judgments of perception to the claims of sophisticated science, are propositional in form, and we take justification to be a rational procedure of showing how such claims are known. However, if we take *Reliability* as a serious constraint we also notice that the sorts of perceptual judgments that may serve as empirical bases for such procedures must be of a different character than simple judgments about objects in our environment and their relations. Because normal judgments such as, "That apple is red," "The CN Tower is taller than my house," or, "This coffee mug is currently warmer than this water glass," are all judgments about which we may be mistaken, either through mistaken reference (and here, dreams and illusions need not be, though perhaps sometimes are, the culprits), or as a result of other contingent factors (e.g. perhaps lighting conditions are not ideal to establish the colour of apples in a definitive way, or a new—and surprisingly tall!—addition has been added to my house without my knowing it, *etc*.).

The real concern is not over whether or not such judgments can be given a sense, or can be found to be true; the real concern stems from an empiricist version

of foundationalism which adheres to two distinct principles, one of which is an interpretation of the Insight, the other a substantive account of the nature of the given—an account which forces a particular interpretation of all of the constraints, but here we will especially examine the interpretation of *Equivalence* and *Reliability*. The Insight, as we may recall, encapsulates the truism that "experience is our principal epistemic authority and guide". However, the interpretation of this truism (when wedded to foundationalism and the way of ideas) is slightly different. It is represented by the following thesis:

> *The Principle of Empiricist Foundationalism*: Knowledge is immediately given in experience (or derived from experience in a way that preserves the certainty of the immediately given in experience). Such knowledge is unassailable.[22]

Firstly we should note that this principle is much stronger than Gupta's Insight. The Principle of Empiricist Foundationalism, like the Insight, grants the given in experience a primary epistemic role, but, unlike the Insight, commits us to the position that such knowledge is certain, unassailable, and unrevisable.

The classical empiricist response to the *Equivalence* and *Reliability* constraints makes sense only when understood in light of the replacement of the far weaker Insight by the Principle of Empirical Foundationalism. Because experience is thought to do all of the epistemic heavy lifting, the following argument leads one to a rather substantive thesis regarding the metaphysical nature of the given in experience:

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[22] This is a purely epistemological version of a fairly common thesis. Some empiricists, Schlick for example, held a very closely related epistemological/semantic hybrid version of this thesis: that the truth of statements about the "immediately perceived" was given with the understanding of such statements. If one understands a statement such as, "I (seem to) see a blue patch, now," one also knows the statement to be (certainly) true. While such statements are the "ultimate origin of all knowledge" (381) they ought not be "designated as its foundation" (ibid). Schlick thus held that there was a difference between statements about the immediately given and the so-called "protocol sentences" or "protocol propositions" that were thought foundational for science by the Vienna Circle. As far as Schlick was concerned, statements about direct observation – which he called "affirmations" *(Konstatierungen)* - were distinct from the foundational claims of science. Where nothing could lead one to give up her own "observation propositions under any circumstances," (380) one could very well come to reject a foundational scientific claim. We will explore such matters in more detail in Chapter Two.

(1) Experience, if it is to be epistemically significant, justifies our beliefs;
(2) beliefs are propositional in form;
(3) only a proposition can justify another proposition;
(C) the given is propositional in form.

We represent this conclusion as a thesis, perhaps the most central thesis of classical empiricism:

Propositional Given: the rational import of experience is propositional in form.

The thesis of the Propositional Given satisfies the *Equivalence* and *Reliability* constraints, but only by restricting the sorts of objects of which one may have immediate awareness, and thus restricting the type of propositions which may form the basis of empirical foundationalism. By *Equivalence*, any two rational agents who have the same experiences must be provided with the same propositional contents (perceptual facts) by experience. If one is hallucinating while the other is undergoing veridical experience, then the propositions that are attached to experience must be of a certain special sort. By *Reliability*, the truth of propositions given in experience must be known with certainty. As with *Equivalence*, only a very special class of propositions (ones that are warranted when grasped) can be accepted. It seems that the empiricist is calling upon something akin to the cognitive impressions of the Stoics. But, as we have already seen, the sceptic has a response to the positing of such entities; no epistemic temple can be built on the foundation of something akin to cognitive impressions. The classical empiricist seems in no better position to respond to the sceptic than were the Stoics.

The empiricist's response to the Stoical problem is to take refuge in a theory of indirect representationalism of the Cartesian variety surveyed above. What is immediately given is not a proposition to the effect that just *anything* is such and such. What is given to the mind is a very narrow kind of content, one that need not (and, from the normative epistemology that supports classical empiricism, ought not to) make reference to any state of affair in a world separate from the *ideas*

immediately given to the mind. The empiricist response to the sceptic is to argue that the Stoics went wrong not in their epistemic aims, but only in their assumption that they could establish with certainty a class of beliefs that went beyond the representations immediately given to the mind. The sceptic found a way to doubt even the most basic and unassailable of cognitive impressions by showing that the Stoic could always be mistaken about subjectively similar (even "identical") states which nonetheless failed to refer to the same objects or states of affair. The empiricist simply restricts the class of terms from which empirically basic statements may be culled, allowing reference only to the subjective qualities of experience (the *ideas* themselves) and not, as the Stoics allowed, terms of normal discourse with their natural reference.

This certainly does offer a response to the sceptic. However, in so responding, the empiricist has inadvertently given far too much to the sceptic. Such an epistemology will have difficulty in establishing the credence we give to normal and scientific judgments that go beyond the narrow contents from which certain judgments about one's private mental space may be made. Such judgments stand in merely inferential relation to the sorts of judgments that can be made regarding our ideas, and scepticism here wins the day (by the empiricist's own strict Cartesian standards), or, empiricism radically re-thinks the nature of the non-subjective. The first horn yields so much to the sceptic that it is tantamount to scepticism itself (though scepticism not about experience, but about inference); the other horn leads one to an idealism of a Berkeleyan variety. Neither scepticism nor idealism establishes what the empiricist originally sought to establish: an empirical basis for our naturally interpreted claims to knowledge, both of a common sense and scientific variety, for they permit such knowledge to be justified only with recourse to theories arguably even more radical than the scepticism they are thought to replace.

We have yet to even address the conceptual problems exposed by the typical Wittgensteinian, Quinean, and Sellarsian critiques (critiques that call into question the semantic and epistemic assumptions of the thesis of the Propositional Given).

Can the basic beliefs of an empiricist framework be generated without a rich conceptual framework? How would such concepts be justified so as to be palatable to an empiricist? How do languages based on terms that refer only to private objects (ideas, sense-data) get a public meaning? Does the empiricist's infallible semantics capture what we really mean by such locutions such as "… looks like…" or "it appears to me that…"—or do such claims have a sense only because our normal reports on our internal episodes cannot always be trusted? We need not address these arguments in detail, but it is worth mentioning them here if only so that we can establish how cumbersome the propositional given can be, and how beneficial it might be to find another interpretation of empiricism that does not succumb to such conceptual difficulties. Any theory of the given that can preserve the basic empiricist commitment that experience is epistemically primary and authoritative without succumbing to these historical and conceptual concerns is one worthy of further exploration.

§III: Gupta and the Given

So far, we have said little about the positive program Gupta offers, except to say that it satisfies the truisms and constraints. Just how Reformed Empiricism is able to do this requires an examination of some of its key technical innovations, based largely on the logic of interdependence developed by Gupta and Belnap in Gupta [1982], Belnap [1982], and Gupta and Belnap [1993].

III.1: Gupta's Logic of Interdependence

The logic of interdependence attempts to develop fundamental tools (revision rules and revision processes) for "dealing with phenomena containing interdependencies" (Gupta [2006a], 73). Their original application in Gupta [1982], Belnap [1982], and Gupta and Belnap [1993] models the kind of reasoning we encounter when confronted by the liar's paradox (whilst staying within a two-valued semantic system). The Revision Theory of Truth attempts to make sense of the circularity of the truth concept by showing how both the intuitive and

paradoxical features we associate with the concept of truth can be countenanced for languages, such as natural English, which contain their own truth-predicate, and therefore do not admit of the solution proposed by Tarski for artificial languages.

The technical details of the Revision Theory of Truth are beyond the scope of this work. Nonetheless, a basic understanding of the key ideas in the theory is required if we are to see how Gupta utilises the logic of interdependence to account for epistemic (as opposed to semantic) interdependence.

Firstly, we must understand what it means for there to be "phenomena containing interdependencies". In brief, such phenomena exhibit the frightening logical property of *circularity*. Gupta proposes mutually dependent definitions as a sort of simple and neutral "setting" for studying the phenomenon of interdependence, and we here follow suit. Gupta asks the reader to imagine a conversation regarding arithmetic where we can assume the universe of discourse to be the natural numbers between 0 and 6 (i.e. {1, 2, 3, 4, 5}). The conversation is between the reader and an interlocutor, in which the interlocutor establishes the following definition for a new general term *G*:

(1) *x* is *G* $=_{\mathrm{Df}}$ *x* is an even number and x is *H*.

Unfortunately, the interlocutor is interrupted and must leave before providing a definition for *H*. Definition (1) is incomplete, and the meaning (or the extension) of *G* is not known. Nonetheless, you do have some "significant information" about *G*. (Gupta [2006a], 60) What you have is *conditional* information of the extension of *G* given the extension of *H*. Gupta introduces the terms "antecedent extension" of *H* and "consequent extension" of *G* to show that fixing the extension of *H* will, given the information in definition (1), fix (or at least home in on) the extension of *G*. We have hypothetical knowledge of the meaning (extension) of *G*.

But what about systems in which the meaning of *H* is not only unknown, but dependent upon the meaning of *G* (which is also dependent on the meaning of *H*)? Such a system is produced by taking (1) and adding to it the following definition

(2) x is $H =_{Df} x > 3$ or x is G

to arrive at

(3) x is $G =_{Df}$ x is an even number and x is H
x is $H =_{Df} x > 3$ or x is G.

Such a system shows how the meaning of G can be dependent on the meaning of H, and the meaning of H on the meaning of G, yet we still have a great deal of information that helps us to fix our understanding of G (and H). Such a system exhibits the phenomenon of interdependence. Logically, it is no different from the epistemic circle we find ourselves in when we admit that our conception of the self is dependent on our conception of the world, and our conception of the world dependent on our conception of the self. Neither is fixed, and both are dependent on one another. Rather than seek to remove ourselves from these sorts of circles, we should welcome them, and the kinds of information they can give us about the concepts entangled therein:

> When terms belong to a system of interdependent definitions, we are unable to determine in any direct way the objects of which they are true. With the system in (3), for example, to determine the objects of which G is true, we need to know the objects of which H is true; and to determine the objects of which H is true, we need to know that which we are trying to determine in the first place—namely, the objects of which G is true. We are caught in a circle. Interdependent definitions get us entangled in circles such as these. They also yield, however, vital information—information that allows us to exploit the circles in which we find ourselves. (Gupta [2006a], 63-64)

We can exploit this entangled information by taking as hypotheses not our original assumptions about the extension of G and H, but the consequent extensions of G and H given our original assumptions. Consequent extensions are *better* (or at least equally good) hypotheses regarding the meaning of the general terms introduced by the system than are the antecedent extensions.

Now assume for system (3) the antecedent extension of *G* and *H* to be ⟨{}, {}⟩. If this is the case, then the consequent extension will be ⟨{}, {1, 2, 3, 4, 5}⟩. Gupta holds that the consequent extension of *G* and *H* ⟨{}, {1, 2, 3, 4, 5}⟩ is a better hypothesis than ⟨{}, {}⟩. But the true power of the account developed by Gupta is that we need not stop here. We can capture this move from antecedent extension to consequent extension as a "revision rule"—a rule which can now be applied to our system again. Using Gupta's notation, call a hypothesis *h*, and a revision rule *ρ,* then

ρ(h)

is a better (or equally good) hypothesis than (as) *h.* But, applying rule *ρ* to *ρ(h)* will yield

$\rho(\rho(h))$.

as a better hypothesis, and so on. The repeated application of the revision rule is called a "revision process", and it is revision processes that will be the focus of our enquiry here.

This account of revision shows us that information in any system of interdependencies can be exploited so as to develop more sophisticated hypotheses regarding meaning. But how can we use revision to arrive at conclusions regarding the meaning of general terms (or, for our purposes, the epistemic standing of views) if all we know is *conditional* on our original assumptions? Here, Gupta wants to point to certain features of revision processes. We turn our attention not to Gupta's treatment of interdependent definitions, but to the matter more closely at hand: the revision of epistemic views in light of experience.

III.2: Gupta's Account of Epistemic Revision

Rather than search for arguments to fix the nature of the interdependence of experience and belief (i.e. Cartesian "guarantees" via rational argument or mere

assumption that one's experiences are veridical because one's conception of the self is known to be true), one merely embraces the flexibility and multiplicity that interdependence brings to the table. Only relative to a view is one entitled to make (or hold as true) perceptual judgments in light of experience. For any single experience we have only a *conditional* entitlement to the beliefs that are a result of that experience. Of course, this is unsurprising. If I am justified in believing that my eyes are working properly, that the lighting conditions are optimal, and that no particularly odd conditions hold, then my perceptual judgments to the effect that I am now seeing black type on a white paper will be justified. Even a sceptic would have to yield this conditional claim, assuming, of course, that she does not yield the truth of the antecedent.

The logical machinery surveyed in §III.1 does not yet solve the problem posed by the tension between the Insight and Multiple-Factorizability, nor the interdependence of our conceptions of the self and of the world. Whereas the Insight tells us that experience has a broad epistemic scope, Multiple-Factorizability seems to preclude experience from being able to play such an epistemic role. Experience can only provide us with true judgments *if* our view of the self is correct; our view of the self can only be considered correct *if* justified by true judgments. Given a view, one can see how an experience can take one from that view onto a new view. However, this set of perceptual judgments is only rational (justified, warranted, etc.) *if the initial view one holds is rational* (justified, warranted, etc.). Given that any individual experience gives the epistemic agent only conditional entitlements to her beliefs, conceptions, and theories, how could the agent ever be in the position of saying that her perceptual judgments are absolutely or categorically justified? In other words, our epistemic predicament requires us to into account the interdependency of view and (perceptual) judgment.

Empiricists specifically, but epistemologists more generally, have been guilty of thinking that a belief (set of beliefs/entire view) is justified by some experience in a fundamentally *synchronic* way. No belief is up to this task. If there are genuine epistemic interdependencies, then such "solutions" would be akin to fixing (without

warrant) our hypothetical antecedent extensions in the definitional example glossed in §III.1. Just as we have no grounds based on the theory of definitions to posit a privileged antecedent extension for a general term, we have no epistemic grounds to privilege a particular conception of the self so that the dictates of the senses get their "natural" meaning. This would be to utterly ignore the truism of Multiple-Factorizability.

Gupta's solution is to think of the justification of our beliefs as a fundamentally *dynamical* matter—as a product of the very process of revision itself. No single experience justifies the view of the world we hold (or even some part of it, such as an individual belief), but the revision of our views based on the experiences we have (in a stepwise and cumulative way) can lead us from any number of mistaken (though antecedently rational) views to a true conception of the self and world.[23] Even if we all begin from radically different (perhaps even contradictory) conceptions of the self and world, we can, given a relevantly similar course of experience, come to agree on a stable view. If all initial views, no matter their initial differences, converge on the same view (given a sequence of experiences *E*), then we can take ourselves as more than conditionally entitled to hold such views. In other words, certain dynamical properties of revision processes allow us to move from merely conditional to *categorical* entitlements—from mere hypothesis to knowledge.

The following is a toy example. Imagine two individuals, Fred and Jesse, admiring Sally's new car. Fred believes that Sally's car is purple (P), Jesse believes Sally's car is red (R). Both reasonably believe that their respective viewing

[23] Some of Gupta's critics still make this error. Peacocke [2009] argues that Gupta's experience function produces a rational connection between old and revised views, but argues that the function does not meet a "Ratifiability Condition." This condition has it that, "[w]henever a mental transition is rational, there is a condition of soundness that it meets. This soundness condition involves the notion of truth, and it is a condition that concerns the correctness or fulfillment of the contents of one or more of the mental states involved in the transition." Thus, we only know that a transition from true premises to a true conclusion in deductively valid inference is rational *because it is truth-preserving*. Because Gupta gives no content to experience, it cannot meet this condition. However, Gupta never argues that an experience makes a mental transition rational, only that a revision of a view is rational if such revision converges. This means that any version of the Ratifiability Condition would need to apply to full revision processes, not primitively to each experientially motivated step in the revision process. Gupta shows that the process of revision can be ratifiable in this way. We offer a more complete response to Peacocke in Chapter 3.

conditions are ideal, having seen the car from the comfort of their respective living rooms through their front windows in what they both believe to be normal, well lit conditions. Both rightly believe that their respective sense organs are working properly. Fred has had an experience of the car as purple, Jesse an experience of the car as red. Fred has a conditional entitlement to (P), Jesse a conditional entitlement to (R). Now imagine Fred leaves his house after having a conversation with Jesse about Sally's car. He thinks Jesse is wrong to hold (R). Upon a closer viewing, Fred's experience matches Jesse's. The car is red. Fred remembers he just had new windows put in his home, and that they have a subtle, blue UV-blocking tint on them. His initial view was incorrect. In spite of their very different initial views, Fred and Jesse now agree on the colour of the car.

Using Gupta's notation, let us represent in symbols the general epistemic contribution of an experience *e* as

$$\Gamma_e$$

and the perceptual judgments one is entitled to hold in light of Γ_e given some view *v* as

$$\Gamma_e(v).$$

Now, imagine a rational agent at stage 0 of a revision process. At stage 1 of the revision process—the first link in a chain of constant revisions of view *v* (or the "input view") given the set of perceptual judgments $\Gamma_e(v)$ indexed to the previous revision level—a new view is generated based on some new experience. More formally, $\Gamma_e(v)$ is the class of perceptual judgments generated at stage 0 by the combination of experience and world view. Thus, we have our first revision:

$$\textit{New View}_1 = R(v^*, (\Gamma_e(v)).$$

New View$_1$ is the result of revising v in light of $\Gamma_e(v)$. It is also the "output view" of stage 0 and the view the rational agent brings to stage 1, where the rational agent undergoes experiences and will need to revise this view to

$$New\ View_2 = R(R(v^*, (\Gamma_e(New\ View_1))))).$$

There may not be a significant difference between *New View*$_1$ and *New View*$_2$; there are no epistemic guarantees that each experience will force a rational change in our view of the world, only the commitment that experience *can* force a rational change in view. If the process continues, it can be represented in the following way. A sequence of views

$$V = <v_0, v_1, v_2, \ldots v_n, \ldots>$$

is revised (starting with v_0) as a result of a sequence of experiences

$$E = <e_0, e_1, e_2, \ldots e_n, \ldots>$$

to generate

$$v_1 = R'(v_0, e_0)$$

and on up to value n for the definiendum occurrence of "v", and value n-1 for the definiens occurrence of "v" and "e".

If, in the course of revision, the epistemic agent arrives stably at a belief Q as part of view v_x (as a result of revising V in light of E), and if all other revision processes with alternative initial views also converge on Q (in spite of their initial differences) in light of E, then we have categorical entitlement to Q.[24]

[24] A sequence of views V is stable iff there is s stage n such that, for all stages $m \geq n$, V_m is fundamentally equivalent to V_n. Views are fundamentally equivalent just in case they are "the same in all relevant respects." (See Gupta [2006a], 91.) Two revision sequences, V and V', converge iff there is

The strength of such a view should be apparent. The Insight is now preserved, since the revision process itself can yield categorical knowledge, utilising experience as our "principal epistemic authority and guide." Multiple-Factorizability is preserved because no view has been privileged without being properly guided by experience. The constraints are also met. *Existence* is satisfied; we have actually gone beyond this constraint to offer a full logical characterisation of the given in experience. *Equivalence* is satisfied even when two agents may disagree about the meaning of their experience because we can make sense of such disagreements while keeping the rational import of experience fixed. *Reliability* is satisfied vacuously. Since the given in experience is logically characterised as a function, it is not of the right logical category to be truth-evaluable, and therefore it does not provide us with false propositions. Lastly, *Manifestation* is respected; rational changes of view are based on the subjective character of experience, not some semantic trick that simply assigns the desired content to experience in an arbitrary or *ad hoc* fashion.

Gupta places only a limited set of admissibility criteria on initial views: they may not be *pathological*, i.e. you cannot genuinely begin an epistemic revision process with a rigid view that is immune to epistemic revision; they must be *receptive*, i.e. they must yield different perceptual judgments when experiences are subjectively distinct; and, lastly, they must be internally coherent. Solipsism and scepticism (where "scepticism" refers to some severe form of the doctrine, one that does not allow any experience to produce any perceptual judgments, and which does not, therefore, allow for revision of view in light of experience) are blocked as initial views in a revision process because no possible experience can upset them. (Gupta [2006a], 172-175) Notice, though, that solipsism and scepticism are not blocked entirely, only as *initial* views in a revision process. The sceptic is free to show how, from some innocent and revisable view of the world, we can converge on the view that we are brains in vats. This, however, is a much harder task (and not a

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a stage n such that, for all stages $m \geq n$, V_m is virtually identical to V'_m. The relation of virtual identity holds between two views, V and V', just in case an agent with V and an agent with V' have nearly the same view. (See Gupta [2006a], 93.)

very sceptical conclusion, anyway). Merely highlighting the possibility does nothing to mitigate the reasonableness of our non-sceptical views. Like Moore, Gupta has shown that the ball really is in the sceptic's court—it is the sceptic who owes us reasons for agreeing with her, not *vice versa*. Unlike Moore, Gupta is able to arrive at this point in the dialectic with the sceptic whilst still observing Multiple-Factorizability.

§IV: Concluding Remarks

Gupta has taken up a reasonable position against the sceptic, avoided the propositional given, and remained true to the Insight, Multiple-Factorizability, and the constraints. However, we do not yet have a full appreciation of his account. In Chapter Three, we will examine some of the main criticisms of Gupta's account, specifically the criticisms offered by Neta, Peacocke, Berker, and Schafer. We will isolate certain common themes in these criticisms (to the effect that Gupta's system is rationalism hiding in empiricist's clothing), and dispel common assumptions at the heart of such mis-readings.

Before we get to such concerns, we will stop in Chapter Two to discuss Gupta's characterisation of "classical empiricism", which, as we have seen, he identifies with the general sense-datum account of perceptual knowledge. Chapter Two will show that Gupta's alternative to classical empiricism can be supported by insights of earlier post-Kantian criticisms of classical empiricism. Our critical analysis of Reformed Empiricism, which follows the survey of existing critiques in Chapter 3, makes sense only in the context of the extension of Gupta's empiricism into the philosophy of science. The late stages of post-Kantian empiricism surveyed in Chapter Two motivate our contention that exigencies of scientific knowledge (particularly its inherently formal character) demand slight alterations be made to Gupta's new interpretation of empiricism. The proposed extension (and subsequent assessment of Gupta's account in light of that extension) will comprise the final two sections of Chapter Three, as well as our Conclusion.

CHAPTER TWO

An Historical Taxonomy of Empiricism

In the preceding chapter we were introduced to a new interpretation of empiricism, *viz*. Reformed Empiricism. The details of this interpretation were characterised in relation to an idealisation of what Gupta refers to as "classical empiricism", specifically a generalised sense-datum theory of perception and perceptual knowledge—one that is wedded to a propositional model of the given. We saw that the adoption of the propositional model of the given was the outcome of certain Cartesian theses and themes that came to influence particular strands of British empiricism in the seventeenth and eighteenth centuries, including the commitment to indirect representationalism, idealism, the significance of sceptical arguments (and the need to address such arguments), as well as the epistemological commitment to foundational certitude.

In this chapter we will examine more closely this Cartesian model of experience, and its historical significance to empiricism. In §I, we will attempt a limited, though illuminating, overview of the Cartesian legacy in empiricism—an examination of the principal British empiricists (especially Locke) in regard to Cartesian representationalism and the Cartesian-cum-Lockean *way of ideas*. In §II we will examine the relationship between the developing sciences (in particular, the mechanical philosophy, optics, and Newton's physics) in relation to the development of empiricism in the 17th and 18th centuries. In §III we will look at a *break* in empiricism in the 19th century, partly as a response to new developments in non-Euclidean geometries, partly in response to developments in physics, and partly in response to new empirical sciences of perception (in particular the physiology of visual perception). We will focus on Helmholtz's and Mach's turn to naturalism in the 19th century, paying special attention to Helmholtz's characterisation of our mathematical knowledge as based on basic mechanical "facts", and Mach's empiricist critique of Newton's notion of absolute motion. We will argue that Helmholtz and Mach offer us an insight into just how different post-Kantian forms of

empiricism were from their classical British counterparts. In §IV, we examine the influence of such considerations on later forms of empiricism, particularly the relationship between Helmholtz's naturalism and conventionalism, with a brief examination of Poincaré's interpretation of the convex mirror thought experiment in Helmholtz [1868/1977a]. We then survey the impact of conventionalism on the early Vienna Circle. We will argue that what are often taken to be the most clear expressions of Cartesianism (or classical empiricism) in the logical empiricist tradition, specifically the reliance on protocol sentences, and Carnap's phenomenalist reduction of scientific concepts in his [1928/2003, hereafter *Aufbau*], are best viewed as non-Cartesian (perhaps even anti-Cartesian) in character. Carnap's "construction of the world" is best understood as a radical rethinking of the relationship between empirical concepts and formal/theoretical concepts, one that Carnap developed in various ways throughout his career. The Quinean misinterpretation of Carnap's "external world project", which Quine sees as the culmination and epitomous expression of Cartesianism-cum-empiricism, will be critiqued. We will argue that Quine's interpretation of the *Aufbau* and its significance to epistemology since the middle of the last century (a) belies the very new course for philosophy which Carnap (and the Vienna Circle more generally) pursued, and (b) has set the stage for a number of misconceptions regarding logical empiricism in the contemporary literature.

In spite of the historical scope of this chapter, its purpose is narrow and focused in light of our studies thus far: we hope to get clear on Gupta's characterisation of "classical empiricism" as being definitively committed to the propositional character of the given, especially if that model is thought (in its ideal case) to be akin to a sense-datum theory of perceptual knowledge. We are arguing for a decisive break between what we may call "Classical British Empiricism" ("CBE", for short), including the principal British empiricists, Locke, Berkeley, Hume, and, what may best be called "Modern Scientific Empiricism" ("MSE", for short), a tradition beginning in the 19th century and culminating in the work of the logical empiricists; what can be said about CBE regarding a commitment to Cartesian

themes can very rarely be said about MSE. In fact, MSE is often very hostile to Cartesian accounts of experience, perception, and knowledge; it is rather an attempt to conceive of empiricism anew—as a branch of philosophy concerned with understanding how our scientific knowledge could inform philosophical concerns, not merely the *vice versa*. While Gupta rightly sees the need to reform the logical confusions at the heart of CBE, his taxonomical category of "classical empiricism" blurs the distinction here proposed and obscures just how much of his account of Reformed Empiricism, specifically its critical part, finds similar expression in the careful work of scientifically minded philosophers in the late 19th and early 20th centuries.

§I: Epistemology as a General Theory of Representation

The idea that Cartesianism is central to modern epistemology is not novel in Gupta, though his characterisation of Cartesianism (based on its logical categorisation of experience) is. Critical examinations of the Cartesian influence in empiricism are due mostly to the post-World War II critiques of empiricism, beginning with veiled remarks in Wittgenstein [1953, 1969/1972], Quine's sustained attack on phenomenological reductionism in his [1951/1961] and [1969/2004], as well as his attribution of Cartesian epistemology to Carnap's *Aufbau* in the introduction of Quine [1936/2004]. Gupta focuses much of his attention on Sellars' analysis of the mythical aspects of the given—the given as a pure and neutral conceptual realm, free from the concepts and categories we employ in common sense and scientific discourse.[25] The argument from Sellars is that such a myth extends as far back as Descartes, and as far forward as sense-datum theories of perception. However, Gupta is mostly silent regarding just how far the "classical empiricist" tradition is to extend. The work of Russell is often mentioned, especially in regard to key Cartesian themes (direct awareness, reductionism, and scepticism), though other 20th century figures are little discussed in the book. Reichenbach is well (though limitedly)

[25] Gupta surveys Sellars' criticism of the classical view of experience in a subsection of the 5th chapter of his [2006a], "5B The Sellarsian Criticism". For Gupta's critiques of the Sellarsian position, see especially 124-130. For a full account of the overview of Sellars' theory of perception, see Gupta [2012].

utilised to give full expression to Hume's "empiricist principle" (that reason can only make analytic contributions to our knowledge), but nothing is said of his nuanced account of the "neutrality" of experience, a view which seems (at least on the surface) to be amenable to Gupta's own project.

Carnap too is sparsely dealt with. He is encountered in only three footnotes: the first of which expresses Quine's indebtedness to phenomenalism of the sort thought to be expressed in Carnap's *Aufbau* (Gupta [2006a], 47fn); the second, a reference to §64 of the *Aufbau*, which Gupta takes to be Carnap's expression of a phenomenalist foundationalism (akin to Russell's sense-datum basis of knowledge in his [1914/2009]) (Gupta [2006a], 109fn); the third and final reference to the similarity between Quine and the mature Carnap regarding the commitment to see ontological questions as largely reducible to "manners of speaking" (Quine [1981], 474) or acceptance of a "certain form of language" (Carnap [1950], §2). While the footnote on page 47 does make reference to recent work that has challenged Quine's misreading of Carnap's *Aufbau*—citing Friedman [1999], Richardson [2003], and Sauer [1989]—, it is clear that Gupta comes at the logical empiricist tradition only obliquely through Sellars and Quine. As a result, the assumption that Carnap (especially the Carnap of the *Aufbau*) fits well into the Cartesian tradition goes unchallenged. We wish to call attention to the Quinean misreading as it relates to Gupta's taxonomy.

As we shall see below, the common thread Gupta finds running throughout Cartesian models of experience is an account of direct awareness of an internal, subjective realm. CBE, and a strain of British empiricism coming into the 20th century, surely did hold such a view. As we shall see, it played little to no role in the development of MSE, and is not the source of modern variants of classical projects (in particular, Carnap's reconstruction of the external world in the *Aufbau*).

I.1: Classical Empiricism—Beyond Aristotle and Descartes

As Jesse Prinz [2002] has noted, the imagistic basis of our conceptual knowledge runs deep in the philosophical tradition, from Aristotle to the British empiricists.

(See especially Prinz [2002], Chapter 2, section 2.1) Prinz is explicitly concerned with discussing the nature of our concepts; from the context of our inquiry, this focus is somewhat tangential. From what we have already seen in Chapter One, our concerns are much less with the nature of concepts than with our account of how they are known, and how they may relate to the more primary matter at hand: our knowledge of (empirical) judgments. Nonetheless, imagism was a central component of the Aristotelian picture that Descartes was looking to replace, and, almost paradoxically, a central part of the Lockean view of experience that Descartes inspired. Because of its centrality to British empiricism, imagism is worthy of some discussion here.

It was Aristotle who first argued that thinking is essentially imagistic: "To the thinking soul images serve as if they were contents of perception… That is why the soul never thinks without an image." (Aristotle [1984], 431a15) The view is that all thinking is at least mediated by images, if not directed upon images present to the mind (as actualised by the senses). This imagism was in marked contrast to Plato's formal theory of knowledge; for Plato, images could not perform the central epistemic role attributed them by Aristotle. This is for two reasons. Firstly, we simply have no relevant images to associate with many of the most significant abstract concepts we use, such as virtue, justice, the good, etc. Secondly, images always carry with them a *particularity*, and an empirical genealogy. How could we ever come to understand (and think with/about) *universal* concepts based on such a meager *empirical* basis? (For Plato's most sustained disagreements with the empirical theory of concepts see the *Meno* and *Phaedo.*) Moreover, how could a particular come to stand in for the general class of things the universal concept is thought to capture?

Aristotle's response was essentially to allow some images to be abstractions from particular cases, so that we may arrive at a general or universal idea based on abstracted similarities of the particular instantiations of the concept that we have come across in experience. This is especially pronounced in the case of mathematics and "first philosophy", as compared to the case of physics. For Aristotle, physics is

concerned with all of the characteristics, active and passive, of bodies. (Aristotle [1984], 403b12) Physics thus requires little in the way of abstraction to get its proper object, though Aristotle does note that the subject matter of physics is relatively more abstract than the subject matter in, say, carpentry or even medicine. On the other hand, Aristotle views mathematics as concerned with number and geometrical shape, each "inseparable [from bodies] in fact, but… separable from any particular kind of body by an effort of abstraction". (Aristotle [1984], 403b13) Metaphysics, because it is associated with general knowledge and wisdom, cannot be a matter of sensual experience, since, for Aristotle, "sense-perception is common to all, and therefore easy and no mark of wisdom". (Aristotle [1984], 982a11) The true mark of knowledge and wisdom is for Aristotle as it was for Plato: formal and universal. Aristotle writes:

> Such and so many are the notions, then, which we have about wisdom and the wise. Now of these characteristics that of knowing all things must belong to him who has in the highest degree universal knowledge; for he knows in a sense all the subordinate objects. And these things, the most universal, are on the whole the hardest for men to know; for they are furthest from the senses. (Aristotle [1984], 982a20)

But Aristotle only agrees with Plato superficially; yes, knowledge and wisdom are associated with universals, but such knowledge, while furthest from the senses, nonetheless finds its genesis in the images actualised in the mind by the senses (as opposed to some rational faculty of recollection). He responds to Plato's criticism that it is impossible to get a universal from particulars by taking full advantage of a *broad-scope* conception of abstraction: since one of the characteristics of any body is that it is perceptible, we merely abstract from our perception of the particular its perceptibility. In fact, this is how mathematical objects can be countenanced by a theory of abstraction: some "images" of objects are not perceptible, for the perceptibility of the object has been abstracted away by an action of mind—in particular, an action of the mathematician's mind. The metaphysician goes one abstraction further, according to Aristotle—by discovering

what is separate from body, i.e. the universal which it instantiates. Call this "Broad-Scope Abstractionism". We will see below that Locke, availing himself of a notion of abstraction much more narrow in scope, is able to avoid some of the pitfalls of ancient epistemology by moving toward a particular kind of modern imagism: *the way of ideas*.

Before making such comparisons, we should stop to note that while we have attributed to Descartes the general theory of representationalism adopted by Locke ("the way of ideas"), Descartes' theory of representation is a hybrid of imagism and descriptivism, attributing to the human mind a distinct intellectual faculty which can address concepts directly. This faculty operates when we conceive of things that cannot be represented to us *as such*. *God* is obviously such a concept for Descartes, but the faculty is operating more subtly in the Wax Argument of Meditation II and Descartes' comments about the impossibility of imagining (representing in sensual imagination) a chillagon. Ayers has noted that "[h]ostility to Descartes' conception of intellect pervades the *Essay,*" (Ayers [1991], 48), but this does not yet establish for us that Locke was, as Ayers makes him out to be, an unequivocal imagist (of an Aristotelian sort).

Imagism can best be thought of as beholden to three theses: (1) objects are presented in thought as they are presented in sensation; (2) there is no rational faculty which mediates this presentation; (3) only memory and sensation are required to explain the acquisition and character of ideas. Locke makes it clear that he holds theses (2) and (3) in the beginning of Book II of the *Essay*, but it is only via Locke's method of abstraction that (1) is established, and only tenuously at that. In Book II, 1, 3, Locke comes closest to conveying (1) when introducing the faculty of sensation: "First, *Our Senses,* conversant about particular sensible Objects, do *convey into the mind*, several distinct *Perceptions* of things, according to those various ways, wherein those Objects do affect them…" But what then of complex ideas, which seem to fail to be "conveyed" or presented to the mind in sensation—what of our abstract idea of *whiteness* (to use one of Locke's examples) as that property which is shared by the idea of milk, the idea of snow, and the idea of chalk (much less ideas such as

uncle-ness, *perfect justice*, or *virtue*)? Like Aristotle's imagism, Locke's imagism must countenance such ideas, as they seem to be present to the mind (often in ways not dissimilar to simple ideas), and their genesis may not be located, by theses (2) and (3), in an intellectual faculty. (Other problematic ideas include "the powers" of material things to cause in us simple ideas, non-hedonic ideas pertaining to morality, ideas of existence, etc.)

Locke's account of abstraction, like Aristotle's, is meant to explain how at least some of these ideas may be present before the mind whilst not *seeming* to be images. In the *Essay*, Locke argues for a principle of abstraction whereby a given idea is separated from all other ideas that accompany it *in existence*. (Book II, 11, 9) So far, Locke shares with Aristotle an account of abstraction that classifies certain ideas (namely abstract ideas) as those that are free from the full reality and context of their original sensation. In the case of the first class of complex ideas, ideas of *Substance*—"… the combination of simple *Ideas*, as are taken to represent distinct particular things subsisting by themselves" (Book II, 12, 5)—there is still an attachment to a notion of *real* essence as distinct from *nominal* essence of the thing. Another class of complex ideas, the *Modes*, is populated by ideas separable from the context of their original sensation—ideas which "contain not in them the supposition of subsisting by themselves, but are considered as Dependencies on, or Affections of Substances…" (Book II, 12, 4)

As Emily Carson [2006] has noted, the key distinction between Modes and Substances, therefore, is that Modes best exemplify the free activity of the mind exhibited in all construction of complex ideas from simple ideas, since, unlike Substances, such constructions are not restricted in any way to the original context of the simple ideas—the context of "real being" presupposed by Substances. Whereas Substances are complexes of ideas that we know are in constant conjunction in experience, Modes are voluntary and free activities of the mind, constructed "without the help of any extrinsical Object, or any foreign suggestion". (Book II, 13, 1; quoted in Carson [2006], 5) Carson introduces an analogy to make clearer the distinction between Substances and natural kinds (on the one hand), and

Modes on the other, the construction of which owes nothing to "the reality of things" (as Locke puts it), and very much to the "Thoughts of men".[26] Modes are not beholden to being (or even its supposition), but are rather the free association of ideas (so long as those ideas are consistent to the understanding). Locke's paradigm examples are *beauty*, *triangle*, *gratitude,* and *murder* ("*murther*"). Unlike Substances, which have (or at least may have) a distinct and separate real essence and nominal essence, Modes are, as it were, entirely nominal: they are nothing more, nor do they purport to be anything more, than abstract categories based on appearance.

However, it is entirely unclear whether Locke would accept something akin to the Aristotelian level of abstraction that we saw earlier was necessary to make certain types of very abstract knowledge (e.g. mathematics and metaphysics) possible within Aristotle's imagism. Recall that Aristotle allowed for an abstraction from the perceptibility of the original image (as a mere property) so that we may arrive at perfectly general conceptions of number and figure (as required by arithmetic and geometry). Locke, on the contrary, takes advantage of his division of complex ideas between Substance and Mode to explain our mathematical knowledge (as distinct from metaphysics). Whereas natural philosophy (including metaphysics) presupposes a notion of Substance which it cannot demonstrate (because of the cleavage between the real essence of a Substance and its nominal essence), mathematics is concerned only with the Modes (whose real and nominal essences must coincide). Therefore, because mathematics is "only of our ideas", its propositions can be certainly demonstrated.

This foray into mathematical knowledge will not only inform the discussion of mathematical concerns, a dominant theme in what is to follow later in this chapter and the next; it also calls to our attention a key point of Locke's epistemology, specifically his notion of foundational certitude. For Locke, certain knowledge rests on the degree to which it is purely *ideal*. There are three standards by which ideas can be judged as certain or uncertain: their reality (as opposed to

[26] Strictly speaking, Locke holds that all ideas, even Modes, have *some* attachment to real being, since they are complexes of simple ideas which are always "real" in the sense that they "represent" the powers in external objects to cause them. More will be said regarding such matters below.

illusoriness), their adequacy (as opposed to inadequacy), and their truth (as opposed to falsehood). Simple ideas of sense are real (in that they are passive and always effects of the powers of external causes), adequate (in that they always faithfully represent the powers of objects which are their causes), and true (in that they are in the mind as they are because of the powers in objects that cause them). Ideas of Substance, on the other hand, always make reference to an external reality, and can be fantastical (because substances require activity of the mind and such combinations may not refer to the reality of things), inadequate (because they are mere "Copies of those Originals, and Archetypes…" and are thus imperfect and inadequate), and untrue (since they cannot be reduced to the simple ideas whose combinations we come to know them by). Modes are the other paradigm of certain knowledge. Because of their ideality, they are real (in that their real and nominal essences correspond to one another), adequate (in that they are not copies, but originals and archetypes unto themselves) and they cannot be referentially false (in that they refer to nothing but their own nature). Here we have the essentials of Locke's way of ideas (his indirect, causal theory of representation): all knowledge is of ideas, and all certain knowledge comes either from simple ideas of sense or the arbitrary and voluntary constructions of the mind (using only simple ideas), which purport no reference to reality. Simple ideas and Modes are the benchmark for knowledge, for they are either the stuff that is immediately given to the mind or the innocent fabrications of the mind based on what is immediately given. We cannot be mistaken about either category.

Unlike Aristotle, then, Locke countenances two kinds of certain knowledge: the ideas of immediate sensation, and the Modes that are abstracted from them. Lockean abstraction, in a sense, reverses the Aristotelian epistemic order while remaining within an imagistic framework: certainty is not gotten by moving further away from the data of sense, rather by only trusting the data of sense and the free and voluntary actions of the mind to combine said data into complex ideas which make no reference to a reality beyond themselves. It is here where modern empiricism makes its greatest turn from the ancients. Whether simple or abstract,

ideas may be the foundational stuff of certain knowledge; *but they must not attempt to represent any externalities if we wish them to maintain this epistemic status*. Simple ideas are representationally connected to reality, of course; but only in the sense that they "represent" the powers in objects to cause them.[27] Modes have even less attachment to external reality, and are certainly known by the mind because they are pure fabrications of the mind. From the perspective of this new way of ideas, the Stoical reliance on "cognitive impressions" (highlighted in the first chapter) can easily be remedied. The new empiricist version of "cognitive impressions" make no reference to the "real" (at least no reference to a material world of things), no reference to a world which might be different from the representations of it that are present to the mind. The basic "facts" of modern empiricism are thus purged of such references, and thus rendered immune to classical sceptical attacks.

I.2: Empiricism—after Locke

This, of course, is not the end of the story. Problems with Locke's imagism (and his causal theory of simple ideas of sense) are not essential to empiricism. Berkeley was the first to call into question all abstract ideas whatsoever, arguing that any and all ideas that can be present to the mind must be *particular* ideas—a strict form of imagism that Prinz calls "picturism". Also, Hume attacked the very notion of causation required by Locke's theory of representation as a mysterious *something* that could not be represented by the mind (and therefore could be no part of an empiricist account of knowledge which wished to avoid all commitments which leave room for the sceptic). Both Berkeley and Hume rejected Locke's ideas of Substance as well; we may have ideas that purport to be about an external material realm, but nothing in these ideas could ever represent the materiality of material objects (even inexactly and imperfectly).[28] But these paraphernalia, as important as

[27] Of course, Berkeley offers a purely idealist interpretation of this Lockean view, holding that nothing but an idea can be like an idea. Hume rejects at least two of its fundamental assumptions: 1. That the mind can come to distinguish between real and fantastical ideas. 2. That there exists a causal relationship between external powers and impressions (which Locke requires to get his representationalist theory off the ground).

[28] Hume seems to be in general agreement with Berkeley on such matters. This passage from the *Treatise* is telling of such agreement:

they are to Locke's philosophy, mask the essential shift we have here isolated. Descartes's sceptical philosophy made for modern epistemology a framework of foundational certitude. His focus on the immediate contents of the mind as the basis for any epistemology introduced the way of ideas. However, it is Locke who takes these Cartesian themes and makes of them a radical and new form of empiricism, wedded to the importance of two sources of knowing: sense, and free and voluntary operations of the mind. It is Locke who divorces certainty from intellect, relying merely on sensation and memory (pertaining to internal representations) to reconstruct our claims to knowledge—a reconstruction that Aristotle and the Stoics never achieved.

Leaving aside the particulars of Locke's view, we may now briefly examine some of the key commonalities holding together the CBE tradition—principal among them the Cartesian conception of knowledge as a kind of access to a subjective realm. As we saw with Locke, the way of ideas emerges from the classical empiricist critique of Descartes' rationalism in such a way that it is capable of acting as a foundation for our knowledge without positing any extra-sensory faculties other than memory and (limited) imagination. As Gupta notes, Locke gives best expression to his Cartesianism in the introductory remark of Book IV of the *Essay*: "Since *the Mind*, in all its Thoughts and Reasonings, hath no other immediate Object but its own *Ideas*, it is evident, that our Knowledge is only conversant about them." This reliance on the immediately given objects of sensory awareness—as the only

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> Now since nothing is ever present to the mind but perceptions, and since all ideas are deriv'd from something antecedently present to the mind; it follows, that 'tis impossible for us so much as to conceive or form an idea of any thing specifically different from ideas and impressions. Let us fix our attention out of ourselves as much as possible: Let us chace our imagination to the heavens, or to the utmost limits of the universe; we never really advance a step beyond ourselves, nor can conceive any kind of existence, but those perceptions, which have appear'd in that narrow compass. This is the universe of the imagination, nor have we any idea but what is there produc'd. (Hume [1739/2000], 1.2.6.8)

Many have challenged the once dominant idea that Hume is an idealist, stressing those parts of the *Treatise* and *Enquiry* where Hume is in a more naturalist mood. Recently, Stephen Buckle [2007] has argued that Hume is best characterised as a "sceptical materialist", stressing that Hume's scepticism should be seen as a modern variant of the ancient rejection of claims to the essence of things. It is thus amenable, he argues, to materialism (which "denies any special status to human rational powers").

primitive epistemic entities—holds together the British empiricist tradition, even amongst those (like Hume) who challenge the Cartesian-Lockean (and Berkeleyan) conception of *ideas*. We call this common bond of classical empiricism the "Subjectivity Thesis":

> *Subjectivity Thesis:* sensual experience furnishes the mind with subjective entities, or brings about in the mind certain states, of which the self is directly aware. The relationship of direct awareness is the basis of knowledge.

Even when Hume rejects the fundamental Lockean position that simple ideas of sense are causally related to their objects, he does not reject the Subjectivity Thesis, as the following passage makes clear:

> Nothing can ever be present to the mind but an image or perception, and… the senses are the only inlets, through which these images are conveyed, without being able to produce any immediate intercourse between the mind and the object. (Hume [1748/1999], 12.1—quoted in Gupta [2006a], 16)

There are, of course, many disagreements regarding the particular interpretations of the Subjectivity Thesis, particularly (as Gupta notes) regarding the nature of the subjective. Gupta points to several such disagreements: Are ideas (sense-data, impressions, etc.) identical with appearances? Are we only directly aware of universals, or particulars as well? Is the self an object of awareness? Are we aware of things, or merely states? Are phenomenal properties merely adverbial, or do they have a more primitive place in our ontology? Nonetheless, the idea that knowledge (especially at its most certain) is direct awareness of mental entities or states is the common thread running throughout Cartesian accounts of experience and sensual imagination. Gupta says that this commonality allows us to pass happily over the various differences we encounter within Cartesianism. These differences do not challenge the basic Cartesian model of experience as "direct awareness".

We are thus in a position to offer an account of the peculiar constellation of interpretations of empiricism known as "British empiricism". In spite of their very real differences, they seem to hold two distinct commitments: the view that

knowledge is a matter of direct awareness (the Subjectivity Thesis), and the view that there is some cleavage between the world as given in direct awareness and the world as it does (or may) exist independently from the mind. As Gupta says, "[i]n direct awareness, the self has transparent and immediate access to a special realm of facts, distinct from the realm of familiar public facts… The facts revealed by direct awareness are subjective and internal; familiar public facts, on the other hand, are objective and external." (Gupta [2006a], 17-18). Further, as Gupta notes, the primary problem for any Cartesian account of knowledge is to explain the wide gulf that exists between internal facts and external facts—to justify our common sense knowledge using only those propositions licensed by direct awareness.

The problem is therefore not one of discerning how it is we may come to represent an external world, rather the problem of discerning the truth of ordinary claims based on a class of very peculiar and restricted claims about a subjective realm. This grand empiricist project, the "problem of the external world" (as it came to be known), can be viewed as the defining concern of what Gupta calls "classical empiricism", a project he sees as common to Locke, Berkeley, Russell, and Carnap (among many others). As is clear from the introduction, we wish to challenge this claim, arguing that the problem of the external world is separable from Cartesianism and the Subjectivity Thesis.

In what follows, we will look more closely at the historical progression of a certain strain of empiricism, dominant in the 19th and early 20th centuries. We will show that this brand of empiricism turns away from the program of foundational certitude alive in Descartes, Locke, Berkeley and Hume, putting in its place a particular brand of naturalism and deference to the formal and empirical sciences. Firstly, we will look at the reception of Newton's physics in the CBE tradition to show that there were limitations to how far CBE would extend itself to accommodate the new sciences. Following that, we will look at Mach's empiricism, and his critique of Newton's notions of absolute space and absolute time, comparing his rejection to Berkeley's. The similarities are, of course, informative; but the key differences between Mach and Berkeley are telling of a radical shift in empiricist

thought in the 19th century. We will then examine Helmholtz's attempt to ground our conceptions of space and time in basic "facts" of perception—primarily the free mobility of objects in space. Lastly, we will examine how these naturalist considerations informed the conventionalism of Poincaré and the logical empiricists, with special focus on Carnap's contribution to the external world project, and the rather anti-classical commitments of the *Aufbau*.

Of course, this history will be brief, leaving out many significant details; directions for further information will be offered when a more full explication, explanation, or analysis of matters under discussion are made impossible by space limitations. However, even a rough and very selective history of various forms of empiricism will offer us an account of the bifurcation between CBE and MSE mentioned above.

§II: CBE and the New Sciences

As we have seen, CBE had as its main focus a delimitation of that part of our knowledge about which we can be sceptical from that part of our knowledge about which we can be certain. While the lines were drawn differently for each empiricist, the broad idea was Cartesian in the sense that any epistemology worthy of (and useful for) study must make clear its notion of foundational certitude, and must have a response to the sceptic based on that notion and its consequences. The focal point of such views was the individual knowing subject, with certain assumptions about her position in, and relation to, the world, the epistemological virtues of certainty, the epistemic transparency of ideas, and a commitment to some form of the Subjectivity Thesis.

Part of the project of CBE was to offer stable epistemic foundations for the new sciences, or to show the ways in which parts of these sciences could not be justified. Of course, Locke was heavily influenced by corpuscularian mechanics and Berkeley by developments (many of them his own) in optics, but such sciences played a central part in empiricism only insofar as they could be made to cohere with the way of ideas. Newton's surprising results in physics were, quite

interestingly, received with great suspicion by Berkeley, and ambivalence by Hume; even Locke, who trumpeted the "incomparable Mr. Newton", was sceptical as to the nature of Newton's laws and forces, and their impact on the theory of knowledge. To be clear: the CBE principals were all willing to take up parts of the new scientific philosophy, even parts of Newton's system, but only insofar as the new sciences could be made consistent with their philosophical views. That Locke adopted and defended a rather radical form of mechanics is indicative of the point being made here: it was such a mechanics which could offer a bridge between the qualities of material things and the ideas which they caused. Only because external objects were reasonably stable (i.e. exhibited nomic behavior), and only because our ideas of sensual perception (unlike memory or imagination) were caused without our volition, could we even begin to consider bridging the gap between ideas as given and a material world beyond them. Locke had philosophical reasons to accept the mechanical philosophy: philosophical reasons which precluded his accepting fully a science which made central a notion of force as action at a distance. (Of course, Hume's scepticism about the external world allowed him to adopt aspects of Newton's empiricism, but here only to a point as well. See below.)

Berkeley's hostility to Newton's use of absolute space, time, and motion in his physics is a prime example of the primacy of idealism over Newtonian science within CBE. (Berkeley [1721/1993], especially 52-65) Likewise Hume, though he agrees substantially with Newton's "rules of reasoning", and adopts Newtonian principles of gravitation, elasticity, cohesion of parts, and impulse as "probably the ultimate causes and principles which we shall ever discover in nature" (Hume [1748/1999] 4.1.12), nonetheless thinks none of these "causes" real. While Hume respected Newton's form of empiricism, thinking it superior to Boyle's mechanical philosophy, at least in regard to its more careful characterisation of laws, which Newton based only on experiment and not mechanical assumption (see volume 6 of Hume [1778/1983]), and while he appreciated Newton's reticence to commit himself (Newton) to an interpretation of the ultimate cause of what such laws described, for Hume, the laws are nothing but a mathematical contrivance, helping

us to organise the appearances, but themselves unverifiable. As to the argument that the subtitle of the *Treatise* ("Attempt to Introduce the Experimental Method of Reasoning into Moral Subjects") exposes Hume's Newtonianism, it has been argued by Eric Schliesser [2008] that Hume's education was actually more heavily influenced by Boyle's mechanical view, and that "experiment" in the subtitle has far less to do with Newtonian experiment (restrained as it was by the desire to make a connection between properly organised observation and mathematical structure, see Smith [2002]), and much more to do with Boyle's use of the term in the titles of several of his writings. "Experiment" holds no reference, therefore, to Newton's methodology—a methodology to which Hume only sparingly subscribed when it suited his scepticism.

Locke, of all the CBE principals, was perhaps the most sensitive to the close relationship between theoretical advances in the sciences and traditional epistemology and metaphysics. His remarks in the "Epistle to the Reader" from the *Essay* make clear his reverence for the advancement of scientific theory:

> The commonwealth of learning is not at this time without master-builders, whose mighty designs, in advancing the sciences, will leave lasting monuments to the admiration of posterity: but every one must not hope to be a Boyle or a Sydenham; and in an age that produces such masters as the great Huygenius and the incomparable Mr. Newton, with some others of that strain, it is ambition enough to be employed as an under-labourer in clearing the ground a little, and removing some of the rubbish that lies in the way to knowledge … (Locke [1690/1959], 14)

Early 18th century Newtonians were happy to view Locke as a "card-carrying Newtonian *sans* the math"[29] because of such statements. In fact, it was quite common (until recently) to find Newtonian readings of Locke in the literature.[30] In the last 25 years, this view has been challenged. Downing [1997] has proposed

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[29] This pithy phrase is due to Michael J. White. As of yet, he has not used it (to my knowledge) in a published essay. It can be found in Michael J. White (2010) "*Locke on Newton's Principia: Mathematics or Natural Philosophy?*" from HOPOS 2010: The International Society for the History of Philosophy of Science (Budapest, 24-27 June). (The conference paper is available through PhilSci Archive.)

[30] In her [1997], Downing cites some examples of this reading of Locke, including Feingold [1988] and Buchdahl [1961].

(along with Alexander [1985], McCann [1985], and Atherton [1991]) that Locke's commitments to Boyle's radical and reductive corpuscularianism prevented him from accepting certain key tenets of Newton's physics, most significantly action at a distance. According to this view, Locke thought of Newton's treatment of gravity (as a prime example) to be a mathematical feature, not a real quality of matter, and therefore not a natural quality. From what we have seen in our earlier discussion of Locke's abstractionist account of mathematics, a central part of Newton's physics (the law of gravitation) is nothing but an arbitrary construction of the mind. For this reason, it may help us to organise the phenomena, but is not true in any substantive (read: *mechanical*) way. The natural philosophy at the centre of the *Essay* remained a radical corpuscularianism, in tension with the traditional reading of Newton's natural philosophy that the phenomenon of gravitation is real, in spite of our lack of knowledge about its underlying cause.

If we are to be a bit too simple, we can take from such details the conclusion that Locke's excitement about Newton's new physics never had the impact on his epistemology and metaphysics it was thought to have. Far from being a card-carrying Newtonian, Locke reverted to a (meta)physics which better explained his causal account of empirical knowledge (a mechanical cause of indirect representation via the "Powers" of external things) and, for almost all intents and purposes, reserved his admiration for Newton's development of the physics to remarks like the those of the *Epistle*. High reverence in rhetoric did not transform itself into philosophical integration of Newton's physics, even to the extent that Hume adopted Newton's rules of reasoning and rules for constructing "principles" or causal laws, leaving Locke's epistemology firmly entrenched in the Cartesian way of ideas (supported by a familiar, if non-Cartesian, mechanistic physics).

Speaking more generally, then, many of the scientific advancements of the 17th and 18th centuries failed to be integrated by the British empiricists – failed to be counted as genuine parts of our knowledge. The fundamental problem seems to be the Cartesian commitment to epistemology as a *ground* for our knowledge, including the sciences, and therefore a methodological wedge between what can be

established philosophically (from first principles) and what cannot be so established (i.e. the sciences—at least certain central parts of Newton's physics). While it would be wrong to characterise this distinction as following from "first philosophy" (i.e. speculative metaphysics of the Cartesian sort), the distinction can be characterised as following from a "philosophy-first" principle: because the empiricists had made clear their commitment to foundational certitude based on purely subjectivist grounds, and because it was thought that parts of the new physics could not be represented to the mind (e.g. the cause of gravitation), the reality (material or ideal) of physics was put aside.

We do not here presuppose that empiricism must have something positive to say about the sciences as particular bastions of knowledge and method that must be integrated into any proper epistemology. Such claims would be naïve, both historically and philosophically. However, we should pause to make sense of what we have seen so far regarding the historical and conceptual development of the way of ideas. The empiricists received the way of ideas through an admixture of Aristotelian and Cartesian influences. Both Aristotle and Descartes used the way of ideas to explicate the nature of our mathematical and physical knowledge. For Aristotle, images formed the stuff in which all of our thought (even non-imagistic thought) found its origins. It was those images before the actualised mind that allowed for the relevant abstractions to take place, facilitating the ever more general studies of physics, mathematics, and metaphysics. For Descartes, the way of ideas was at once both an epistemic limitation and epistemic boon for embodied creatures like us; Descartes embraced this duality of embodiment. Unlike Plato's, Descartes' rationalism is not an attempt to show how far knowledge can progress when we free ourselves from our bodies and our senses, but rather how much we can learn from empirical phenomena once the data of sense has been put on the *terra firma* of the cogito and ontological arguments. In short, both Aristotle and Descartes constructed their respective epistemological systems to make sense of our mathematical and scientific knowledge.

While this desideratum is not necessarily inherited by the wholesale adoption of the way of ideas, one popular reason why empiricism fell out of favour by the end of the 18th century was because of its inability, or at least unwillingness, to account for key parts of Newton's physics. Given the widespread success of Newton's system, which had become unassailable, philosophical reticence to regard the epistemic centrality of physics, much less the occasional aggressive attack on Newton's ideas based on "dogmatic empiricism", ceased to be attractive. Kant's critical work became *de rigueur* at least in part for its ability to more adequately characterise the significance of the new physics than did Locke's or Hume's empiricism. In spite of Locke's and Hume's adoption of those parts of Newton's work that found an *a priori* "fit" with their philosophies, and in spite of passages in both of their work which show reverence for Newton and Newton's philosophy, there was no attempt by either to engage in anything more than what we may call "piecemeal Newtonianism". Newton's project to put knowledge of the world on a secure mathematical footing, following principles gained from induction from the phenomena, would have to wait until Kant for a full philosophical explanation. With Kant, and with the post-Kantian return to empiricism, this piecemeal Newtonianism had to be addressed; no longer could empiricism fail to incorporate the sciences.

§III: The break with CBE

The main bifurcation in the empiricist tradition occurs in the latter half of the 19th century. During this period, empiricism becomes ever more associated with a kind of methodological naturalism, breaking with the Cartesianism of CBE. The idea that traditional philosophical views (usually pertaining to the Kantian *a priori*) can be amended using more sophisticated scientific theory (be it a better understanding of the formal sciences, or individual sciences like physics, physiology, or psychology) replaces the way of ideas. What we see in MSE is an increasingly vigorous attempt to divorce empiricism from its Cartesian commitments. No longer is foundational certitude the over-arching epistemic virtue, and we find a reversal of the idea that one must have a clear philosophical picture of knowledge *before* one assumes

commitments of a scientific nature. Facts of physics, biology, psychology and optics can (and perhaps even need) be employed to explain our empirical knowledge, to upset what were once thought to be necessary and *a priori* philosophical points of view. Of course, each modern empiricist tackled the problem of *a priori* knowledge in different ways. Some held, with Kant, that formal principles were essential to our epistemological situation, though such principles were no longer transcendentally given by the structure of our sensible intuition (see Helmholtz in the section to follow), while others proposed epistemologies that gave no special standing to formal or scientific principles (e.g. Mill, and associationists working in the tradition of Mill and Bain). The latter maintained most of the commitments of CBE (though, as was the case with Mill, foundationalism and deductivism were often replaced by inductivism). The former produced a collection of views that comprise the MSE tradition,[31] with its distinct philosophical outlooks and epistemological programs.

III.1 Mach's Naturalised Phenomenalism

Various forms of the post-Kantian MSE tradition turned to a naturalised understanding of the preconditions for the possibility of experience. Perhaps the best example of this sort of shift to naturalism is found in Mach's "Anti-Metaphysical" introductory remarks at the beginning of his [1897]:

> The splendid results achieved by physical science in modern times, a success which is not restricted to its own sphere but embraces that of other sciences which employ its help, has brought it about that *physical ways of thinking and physical modes of procedure enjoy on all hands unwonted prominence,* and that the greatest expectations are associated with their employment. (Mach [1897], 1)

In this book, we are given a promise of the use of the physical analysis of sensation to answer traditional philosophical questions about knowledge. Mach's meta-

[31] As with all naming conventions, this one has its weaknesses. One should not assume that because we place Mill in the CBE tradition, for example, that he did not have a great deal of impact on MSE, or that he wasn't a "scientific empiricist" of a sort. Of course his work on inductive methodology and his empiricist treatment of number are but two examples of his deeply scientific commitments. The line between CBE and MSE is a blurry one – but no less useful or correct for that fact.

philosophical position is not a rejection of philosophical notions such as the *a priori*, but it is a marked shift from first philosophy (or philosophy-first) to naturalism—in Mach's case, a biological account of how *a priori* knowledge is made possible for an evolving organism. As Paul Pojman has put it, "[i]n as much as Kant used the *a priori* to explain how knowledge is possible, Mach uses the knowledge of the new sciences to explain how an *a priori* is possible. One more patch of philosophy, it was thought, yielded to science." (Pojman [2011])

Science was not taken as an unproblematic epistemic starting point, as evidenced (for example) by Mach's criticisms of Newtonian physics, or his rejection of the atomic theory as mere "dogma".[32] Rather, what we see in this period is a move to discern what parts of physical theory ought to be expunged on the grounds of perspicuity and simplicity (economy) based on the information coming from the sciences themselves, and a desire to pursue only those scientific theories that are empirically motivated—i.e. only those theories that are nothing but an expression of empirical relations. Whether the object of such empirical study be scientific or philosophical (or, as was mostly the case, some admixture of the two), the desire to purge a theory of any unnecessary or unwarranted parts, getting to its descriptive core, was prevalent.[33]

What counted as a more "simple" and "perspicuous" theory was, of course, determined relative to Mach's own empiricism, and, of course, his account of sensation. For Mach, the picture starts off very much in the CBE tradition: (1) our *ideas* are nothing more than the combination of sensory elements, and (2) everything we know is known through sensation. The first thesis commits Mach to

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[32] For Mach, theorising is replaced by a thorough-going "descriptionism". The job of the scientist is not to give an ultimate account of reality, but to explain how the world impinges on her senses. Theories really help us do nothing more than capture and describe the relations existing between phenomena. Mach rejects the atomic hypothesis for going beyond the mere expression of empirical relations allowed by his descriptionism, since it posits unobservables rather than merely stating the laws.

[33] That descriptionism, in some variant form, became popular by the turn of the 20th century is evidenced by the number of learned congresses which devoted time – often keynote addresses – to descriptionist themes, including the famous 1900 International Congress of Physics, held in Paris—devoted to taking stock of the advancement of science in the 19th century. For a brief history of descriptionism as originating in the work of Gustav Kirchhoff, its popularity, its detractors, and the debate between Planck's development of a "world picture" and Mach's descriptionist phenomenalism, see J.L. Heilbron [1986], 47-60.

phenomenalism, the second to a form of empiricism, and together (1) and (2) commit him to something very much like the Cartesian-cum-Lockean way of ideas. However, the affinities with the classical picture are only superficial; the sensory elements at the "bottom" of this picture can be (and are) given a scientific account based on the findings of experimental psychology. No more is the way of ideas mysterious, or in any way Cartesian (i.e. founded on first principles); ideas no longer need to be explained by some metaphysical insight, nor need we posit a Cartesian theatre of consciousness. In this way, Mach ensured that a causal account of perception did not need to work towards a theory of indirect representation and the way of ideas, as it did for Locke. Contrary to Locke, sensation is, for Mach, fully naturalised, and causal through-and-through. The last vestiges of Cartesianism (indirect representationalism) are jettisoned.

Mach's naturalism did not stop with his physiological theory of sensation. He also held a "biologised" scientific-pragmatic conception of the sciences. For Mach, physical theorising should be based on sensible things only, and should reduce unobservables in previous theories to observables in a new theory (or eliminate such superfluities altogether). Theories should be as economical as possible, and should only describe the functional dependencies that exist between facts; and science too should exhibit an economy through naturalism—as a more sophisticated variant of the sorts of connections made between sensations by primitive men, or even animals. This contiguity with the sensual ability of animals also helps us make sense of how it is that our abilities to negotiate our environment are not mysterious at all; in fact, contiguity of sensation in the animal kingdom preserves the economy of science, and fits well with Mach's professed "biological-economical theory of knowledge" (Mach [1910], 30-31), or the "anti-metaphysical" commitment to purge science, in all of its forms, of any unnecessary vestiges of past philosophical views. It is clear here that Mach is interested in pruning our account of perception to its minimal physiological core.[34]

[34] Mach makes the connection between his minimal (descriptionist) accounts of physical and physiological knowledge explicit: "One and the same view underlies both my epistemological-physical writings and my present attempt to deal with the physiology of the senses—the view,

This general methodological naturalism became manifest especially in Mach's critique of the Newtonian conception of absolute space. It was thought by Mach that Newton's absolute notions could be replaced by a theory of completely relative motion, and phenomena thought to be explained only by the positing of absolute space, time, and motion should be seen as emerging from the large-scale distribution of mass (and energy) in the universe.[35] The sorts of criticisms leveled by Mach against Newton have an affinity with those provided by the CBE tradition (especially Berkeley's criticisms of absolute space), but with Mach such criticisms find their origins in the naturalised conception of sensation introduced above. Firstly, unlike Berkeley, Mach held that all sensation is spatial in character, based on the extension of the sensory organs in space. (Mach [1901/1906], 13-15) Like Berkeley, Mach held that there were no significant coincidences between sensory spaces (for Mach, "physiological spaces"), though, unlike Berkeley, the inherent spatiality of tactual sensation as well as the disparity of binocular visual sensation allowed for associations to be made between the senses, especially regarding the spatiality of sensation. (Mach [1901/1906], 15-16) So, like Berkeley, Mach holds that the space of normal experience is essentially a matter of correlation between tactual and visual sensation (which are not physiologically correlated), but he views each sensory organ as exhibiting its own peculiar spatiality. The skin is a particularly good example of an organ that is itself extended in space, though all organs have this feature. Our spatial understanding is thus more than just the discovery of correlations between non-spatial visual and tactual *signs* (as Berkeley has it with his semiotic), but the development of the more primitive sensations of space inherent in physiological spaces to a space of experience. In short, Mach's account of sensation characterises it as a species of physical interaction, subject to

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namely, that all metaphysical elements are to be eliminated as superfluous and as destructive of the economy of science." (Mach [1886/1897]).

[35] Of course, as Stein was the first to point out, Mach offered no real solution to the "problem of rotation", and we now understand (after Weyl) that even general relativity avails itself of a "world structure" without which even the idea of relative motion would be meaningless – i.e. general relativity is not thoroughly relativised. In fact, much of what Mach thought to make Newton's framework an "abusive empiricism" can also apply to general relativity.

the laws of physics and biology, completely contrary to Berkeley's purely semiotic view.

Mach's naturalism is also committed to studying space within its proper "biological relationship" of sensation to the function of an organism, including the irritation of an organism's sense organs relative to conscious and unconscious bodily movement and the organism's memory of previous similar irritations and movements. Central to this biologised account are "feelings of motion," i.e. proprioception.[36] Without such sensations of motion, our understanding of space would be greatly diminished. Other factors that inform our view of external space include "biological needs" and (most especially) "willed movement" of the organism, through which we arrive at our robust notion of space (as an arena of transformations and interactions).

Mach's critique of Newton's conceptions of absolute space, time, and motion in Mach [1901/1960] can be read either phenomenalistically (as a continuation of the project in Mach [1886/1897] just glossed), or as fairly independent of his phenomenalism. Taking the first option as an interpretive framework for Mach's critique makes it seem much more akin to other phenomenalist critiques, especially Berkeley's. Since absolute space, time, and motion make no difference to the phenomena (i.e. since all we can perceive are relative motions), they are not empirical notions, and ought to be removed from our physics. Physics should only avail itself of what is found in sensation. But far from being a crude attack of this sort, Mach's critique is (a) a careful analysis of the role absolute space played (or did not play) in Newton's own physics, and (b) a critique of Newton's abstract formulation of the laws of motion (a critique based not on a dogmatic reductionism as many, since Stein [1977], have maintained, but on a careful dialectic with Newton's own physics regarding an illicit, empirically unmotivated, abstraction in Newton's laws of motion). Mach held that insofar as Newton's concept of inertia was empirical, i.e. insofar as the laws of motion had an empirical content, it was only

[36] See Mach [1906], pp. 25-30. Mach was responsible for a lot of the pioneering work done on balance and the inner ear. Independently of one another, Mach and Josef Breuer discovered in 1873 how fluid in the semicircular canals of the inner ear function to produce a sense of balance. The theory now goes by the name "Mach-Breuer Theory of Endolymph Shift".

relative to the largest frame of reference within which measurements could be made. For Mach, the most general (yet empirically motivated) spatial frame of reference is that provided by the fixed stars, and the most general (yet empirically motivated) reference by which time can be measured is the rotation of the Earth. Mach's real bug-bear is not the Newtonian conception of absolute space as a metaphysical entity. Rather, Mach's concern is that Newton privileges one of a set of dynamically equivalent inertial frames—a move that is empirically unmotivated *within Newton's own system*. For Newton, empirical reasoning could only ever countenance *relative* motions. Absolute space and time serve the purpose of helping us conceptualise what it is we are doing when we measure relative spaces and times. They help us conceive of the "aims of measurement" (As George Smith has put it): in other words, they help us to conceive what our preferred measurements approximate, though they do not enter into empirical reasoning, as do the preferred measurements. The phenomenal and reductive reading of Mach's critique thus loses sight of the careful dialectical position that Mach takes up in his *Mechanics*. It also fails to acknowledge the nuances in Mach's mature empiricism.

Neumann was the first to observe that in order to give any kind of empirical content to the laws of motion, one must first postulate a reference frame and time scale in order to express the law of inertia. Neumann posited that there exists a rigid body (or point system) somewhere in the universe, relative to which a given particle moves in a straight line. Such a body would privilege a reference frame, but the definition was regarded as obscure, and thus replaced by the laws themselves. Mach, however, questions such counterfactual speculation. All of our empirical measurements, and all of our experiences of motion, have been and continue to be performed in the specific context of the fixed stars (and the mass distribution they realise). For Mach, the laws of physics do not necessarily rest on phenomenal grounds; however, the historical development of the key concepts in those laws, and therefore the laws themselves, must not engage in empirically unmotivated forms of

abstraction, no matter how natural and useful such abstractions may be.[37] If one joins Mach in reading the laws as expressions of empirical relations, then one needs only the most general frame (the one in which the most phenomena reasonably fit)—that of the fixed stars. The relative motions described in Mach's law of inertia are elaborate constructions, though the evidence for them is reducible (in principle) to basic sensory elements—in other words, we can make sense of relative motions as they impact the sensorium, and no extra-experiential entities need be admitted.

But if Mach's critique of Newtonian absolute space is *not* indebted to, but merely consistent with, his phenomenalism, and is nonetheless motivated by his mature empiricism, then what does this tell us of his empiricism? Mach's phenomenalism is clearly indebted to the general framework of the Cartesian-cum-Lockean view; there still remains an individualistic, almost solipsistic, view of knowledge, and one may certainly make the argument that Mach's phenomenalism is merely a naturalised expression of the Subjectivity Thesis. However, certain aspects of Mach's mature view challenge this categorisation. Firstly, as we have seen, Mach's key critiques of our received theories of space, time, and motion show a sensitivity to empirical considerations which are not solipsistic, which can be characterised relative to physical constructions which are not a species of coarse phenomenalism. Secondly, even Mach's phenomenalism fails to share one key characteristic with its CBE counterparts, since Mach rejects the *a priori* grounds on which one may posit sensations as the basic stuff of an epistemological theory. Instead, as we have seen, he considers sensations central to such a picture *because we have a scientific (physiological, biological, and physical) understanding of them*. Mach remained some kind of phenomenalist throughout his career, surely; but this phenomenalism never seeks to reduce science to the phenomena, nor does it pretend to be able to characterise the phenomenal realm from first principles (the

[37] See DiSalle [2002c]. Mach was, according to DiSalle, not attempting to reduce complex constructions of relative motion to phenomenalist elements, but rather attempting to reformulate the law of inertia *so that* it reflected the real historical conditions in which motions had been measured. DiSalle argues that for Mach, "[e]xpressing the laws more abstractly might enable us to apply them to imaginary situations, but it would not increase our knowledge of the functional dependence among phenomena." (DiSalle [2002c], 173)

phenomena are never viewed as an unanalysable starting place). We know that sensations are the sole gateway to knowledge because the empirical sciences of physiology and psychology have established them so.

Further, recall that Gupta argues that "classical empiricism" is best understood as beholden to a view of direct awareness and the gap between internal "facts" of perception and the external world. While Mach does hold a naturalised version of the Subjectivity Thesis (and therefore a notion of direct awareness), he does not "mind the gap", as it were. His phenomenalism is rather based on there being no gap between sensations and the sciences—between sensations and the empirical sciences which organise and describe the phenomena. Where Locke and Berkeley erred, by Mach's standards, was in thinking that sensation was able to provide for us a "picture" of the external world (either as material thing, or as mind of God), and that such a picture, as formulated by science, was in some way separable from, though hopefully reducible to, sensation. Naturalised phenomenalism rejects the gap altogether. The scientist's job is not to construct a "blue-print for reality" (seeing if such a blue-print can be made from sensory elements, or, what amounts to the same thing, seeing if the blue-print matches our intuition), but to be aware of how it is she can have a conception of the world of objects, including scientific objects, based on sensation. Science need not be phenomenalistic in the sense that it must be reduced to phenomena—it must be phenomenalistic in the sense that every abstraction it makes, every construction it performs, and every law it utilises must be conceived relative to the sorts of observations we have made and continue to make. (Science, in short, must be empirically motivated and empirically sensitive; it must be economical.)

But already we see a nuanced view of empiricism that falls outside of Gupta's characterisation of "classical empiricism". Holding a variant of the Subjectivity Thesis need not entail the problem of the external world. We have to be more precise. What produces the problem of the external world is a non-naturalised notion of direct awareness (non-naturalised in that it is necessarily pre-scientific and necessary for the study of science), along with the assumption that scientific

laws purport to make reference to more than the functional dependencies we see between the phenomena. While it is clear that Locke, Berkeley, and Hume held all of these positions in combination, we must recognise that such a constellation of theses is in no way definitive of empiricism. As we see with Mach, there are alternative interpretations of empiricism which reject the subaltern status of the sciences vis-à-vis first philosophy, and which reject altogether "the problem of the external world". To reject either, it seems, is enough to challenge the framework of CBE, and to fail to fall under Gupta's category of "classical empiricism". As we shall see in the next section, Mach was certainly not the only (nor the first) modern scientific empiricist to challenge such a categorisation.

III.2: Helmholtz's Empirical Analysis of the Concept of Space

A different kind of naturalised empiricism operated in the work of Helmholtz as well, especially in his empiricist critique of the Kantian Transcendental Aesthetic.[38] The Transcendental Aesthetic aims to establish the pure intuitions of space and time as the necessary structures of our intuition. Our spatial intuition is, according to the Aesthetic, necessarily Euclidean, allowing for only those geometrical constructions which accord with the postulates of Euclid's geometry. The alternative hyberbolic and elliptical geometries that were developed throughout the 19th century would be regarded by Kantians and neo-Kantians as mere technical elaborations of consistent mathematical systems, and therefore would be seen by Kantians and neo-Kantians as offering no reason to overturn the privileged position of Euclidean geometry

[38] I do not wish to gloss over here the very real differences between Mach's and Helmholtz's programs by focusing on their common move toward naturalism. For one, Helmholtz was more in line with traditional empiricism in assuming that the human mind was a *tabula rasa*. Mach often flirted with a kind of nativism. For Mach, certain autonomic actions are hard-wired into the organism that allow for the eventual correlation of tactual and visual space required for our full notion of space. Mach's example in his [1906/1943] is of a newly born chick. The chick will notice an object, and peck at it. From this correlation of visual space and tactual space the organism can develop a system of spatial sensation on what Mach calls "powerful impressions". (19-20) As we have seen, the picture here is not unlike Berkeley's semiotic, in which visual sensation lacks the optico-geomtric properties attributed to it by Descartes and Malebranche. See Descartes' *Optics* in Descartes [1985], and Malebranche's [1980], Book 1, Chapters 6-9. Berkeley also rejected the Lockean idea that sensations of space belong to more than one modality. For Berkeley, we must come to coordinate the modalities in a way to better predict the course of sensation through the active association of sensual signs from which our notion of space arises.

(amongst the growing family of possible geometries) as *the* structure of our spatial intuition. Helmholtz was the first to challenge this privileged status based on the mutual sensibility of alternative geometries. Rather than suppose, as did Kant, the necessity of Euclidean space as the structure of our sensibility, Helmholtz develops a philosophy of mathematics that finds as its basis not the pure intuitions of space and time but our empirical observations of the spatial displacement of rigid bodies.

After discussing at length the properties of spherical and pseudo-spherical geometries in his [1868/1977a], Helmholtz turns to spaces of positive, negative, or zero constant curvature. However, rather than following Riemann's abstract account of such spaces (and the metric tensors that determine their geometries), Helmholtz begins from "facts" about our perception of objects in space, and notes that the displacements of rigid bodies can act as representations of ideal physical processes for spaces of constant curvature. Riemann was concerned with a large class of possible geometries, and therefore had to countenance a large class of possible physical processes which empirically determined the physical geometry. A Riemannian metric defines geometrical notions for a space, such as angle, area, length and curvature. However, the principle of free mobility operates in exactly the same way for homaloidal spaces and other spaces of constant curvature. Determining which of the relevant spaces we are in becomes an empirical matter. From the observation that bodies are rigid through spatial relocation we are able to establish the isometries (in the case of homaloid space, a notion of *congruence*) that make measurement possible.

Whereas Riemann began his inquiry from the most general account of space, asking what conditions must be in place for us to consider something a space at all, Helmholtz begins with the elementary experiences that *acquaint* us with space, asking what conditions these experiences impose upon the structure of space once the experiences have been given the appropriate mathematic formulation: "if one… adheres from the outset to the demand that figures fixed in themselves should have unconditionally free mobility, without distortion, in all parts of space, then

Riemann's initial hypothesis is derivable as a consequence of much less restricted assumptions." Helmholtz continues:

> My starting point was that the primary measurement of space is entirely based upon the observation of congruence… However, one cannot at all speak of congruence unless fixed bodies or point systems can be moved up to one another without changing form, and unless the congruence of two spatial magnitudes is a fact whose existence is independent of all motions. So I presupposed from the outset that the measurement of space through ascertaining congruence was possible, and set myself the task of looking for the most general analytical form of a severally extended manifold in which motions of the kind thus demanded are possible. (Helmholtz [1868/1977b], 41)

From this observational fact, we have the basis from which measurement of space can occur, and thus the general geometrical features of those spaces that admit of rigid bodies (i.e. spaces of constant curvature) are established. The trick is to see that no special transcendental feature of spatial intuition is required to arrive at these general geometrical features of actual space. Surely we must intuit space (i.e. we must be able to represent it to ourselves), but its geometry owes nothing to the structure of our cognition: rather, its structure is owed to a very simple fact of mechanics observable by anyone (even, Helmholtz argues, the blind).

The distinctions between Riemann's general and algebraic method and Helmholtz's empirical method are not of immediate concern here. Our concern is with the impact of these Helmholtzian considerations on Kant's transcendental philosophy and their subsequent impact on the development of MSE. Helmholtz thinks these considerations from the observation of rigid bodies force the Kantian into a dilemma. Either such discoveries are truly empirical, in which case we can explain our spatial perception (and measurement of objects therein) without reference to necessary *a priori* claims,[39] or the "Kantian might certainly look upon

[39] Helmholtz always maintained that sensation itself was synthetic *a priori*. His argument only extends to the geometrical structure of space, which is not determined uniquely by any necessary feature of our sensible faculty. Determining that space has an external structure (and determining the structure it has) requires us to, over long stretches of experience in early childhood, make certain unconscious inferences, based primarily on what we learn as we move through space.

the geometrical axioms as propositions given *a priori* by transcendental intuition which no experience could either confirm or refute, because it must first be decided by them whether any natural bodies can be considered as rigid." (Helmholtz [1876], 320) But, says Helmholtz, there seems to be nothing *synthetic* about such propositions: "they would merely define what qualities and deportment a body must have to be recognised as rigid." (Ibid[40]) Neither horn in the dilemma can save the specific geometry thought to be secured by the Transcendental Aesthetic and both are thus acceptable to an empiricist.

More specifically, what makes Helmholtz's philosophy of geometry an *empiricist* philosophy of geometry is his commitment to the view that mechanical facts can be determined by the normal course of visual (and perhaps tactile) perception, and that such facts (which establish for us a notion of "rigid body") can act as the foundation of geometry. Because we know certain facts of mechanics (specifically, that there are sufficiently rigid bodies in the world that allow for the measurement of space), geometry follows from mechanics, and not some transcendental source. Kant and many Kantians (before Helmholtz), thought such a transcendental source was necessary to move beyond the belief, held by Locke, Berkeley, and Hume, that geometry was nothing more than a contrivance of mind, established at best as a kind of system to bring order to the appearances. Helmholtz, like Mach, takes full advantage of his naturalism to attack both the CBE tradition and Kant: what we know (if we know anything) are facts about the motion of medium-sized bodies in immediate space, and the congruence of some of these objects allows measurement of space to occur. The CBE principals were wrong to think that space

[40] In the original version of this paper, Helmholtz [1868/1977a], the matter is stated in basically the same terms: "One could admittedly also take the concepts of fixed geometrical spatial structures to be a transcendental concept, which is formed independent[ly] of actual experiences and to which [these structures] need not necessarily correspond, as in fact our natural bodies are already not even in wholly pure and undistorted correspondence to those concepts which we have abstracted from them by way of induction… [A] strict Kantian certainly could then regard the axioms of geometry as propositions given *a priori* through transcendental intuition, ones which could be neither confirmed nor refuted by any experience, because one would have to decide according to them alone whether any particular natural bodies were to be regarded as fixed bodies. But… the axioms of geometry would certainly not be synthetic propositions in Kant's sense. For they would then only assert something which followed analytically from the concept of the fixed geometrical structures necessary for measurement…" (Helmholtz [1868/1977a], 24-25)

was a mere contrivance; Kant was wrong to think it was necessarily established by the structure of our cognition, and wrong to think that we could not intuit non-Euclidean spaces. Space is, for Helmholtz, an existing structure (or set of possible structures) constrained (though not uniquely determined) by our observation of rigid bodies (or Lie group of free motions).

Helmholtz's analysis of the concept of space has established that it is not synthetic *a priori* in Kant's sense by showing that there is nothing *a priori* about the geometrical structure of space. More precisely, while spatial awareness given to us in intuition (the amorphous and as yet undifferentiated two-dimensional manifold) may be considered synthetic *a priori*, this intuition is separate from the geometrical structure(s) of space (*pace* Kant). In his reply to Land in *Mind*, Helmholtz explicates the narrow focus of his critique:

> … it is a misunderstanding on Prof. Land's part if he thinks I wished to raise any objection to the notion of space as being for us an a priori and necessary, or (in Kant's sense) transcendental, form of intuition. I had no such intention. It is true, my view of the relations between this transcendental form and reality… does not quite coincide with that of many followers of Kant and Schopenhauer. But space may very well be a form of intuition in the Kantian sense, and yet not necessarily involve the axioms. (Helmholtz [1878], 213)

The geometrical structure of space is formed from this amorphously given intuitive manifold through movement in space, tactile perception, and the coordination of visual sensation with certain facts about medium-sized objects in our environment (as we come to understand that environment through interaction).

Ultimately, Helmholtz's empiricism hinges on our ability to found geometry on a domain of geometrical objects with which we have acquaintance. However, Helmholtz calls into question such acquaintance, perhaps nowhere more explicitly than in the introduction to his [1868/1977a]:

> … in geometry we deal constantly with ideal structures, whose corporeal portrayal in the actual world is always only an approximation to what the concept demands, and we only decide whether a body is fixed, its sides flat

> and its edges straight, by means of the very propositions whose factual correctness the examination is supposed to show.

As DiSalle points out, this comment (and similar remarks regarding the definitional character of arithmetical concepts and the definitional character of arithmetical propositions in Helmholtz [1887/1977]) calls into question the synthetic nature of the axioms of geometry:

> If geometry, like arithmetic, merely explores the consequences of a certain set of fundamental concepts, if its application to the world consists not in providing empirical evidence for its basic principles, but merely in identifying instances of its fundamental concepts, then Helmholtz's empiricism and the synthetic a priori would appear to be in the same difficulty. (DiSalle [2006b], 132)

Both Kant and Helmholtz attribute to geometry a synthetic character: Kant finds the basis of geometry in sensible intuition, Helmholtz finds it in what he takes to be simple mechanical judgments about the behaviour of bodies. Neither saw that the certainty of geometrical principles resides in their role as *criteria* for the application of geometrical concepts.

In the same paper in which Helmholtz introduces his empiricist account of geometry (Helmholtz [1868/1977a]), he also proposes his famous convex mirror thought experiment. In this thought experiment, we are asked to consider a convex mirror in which we find the images of several objects placed in front of the mirror, diminished and flattened the further the corresponding object is placed from the mirror. Now consider a man in front of the mirror measuring the length of a line with a trustworthy measuring rod; as the line stretches away from the mirror, the image of the line will "shrivel up" (diminish in the dimension of depth), but so will the image of the man measuring, along with his measuring rod. (Helmholtz [1868/1977a], 20) The diminution in the dimension of depth will, in the mirror-world, yield the same results, and all of the same congruence relations, that exist between objects in the object-world.

> In short, I do not see how the men in the mirror could bring it out that their bodies were not fixed bodies and their experiences [not] good examples of the correctness of Euclid's axioms. But if they could look out into our world, as we look into theirs, without being able to cross the boundary, then they would have to pronounce our world to be the image of a convex mirror, and speak of us just as we of them. And as far as I can see, if the men of the two worlds could converse together, then neither would be able to convince the other that he had the true and the other the distorted situation. I cannot even recognise that such a question has at all any sense, as long as we introduce no mechanical considerations. (ibid)

What separates the men in the mirror from those of us outside of the mirror, then, is the final set of considerations about the mechanics of objects in the actual world. Without such mechanical considerations, both worlds are not only equally intuitable (contra Kant), but equally right to proclaim their geometry correct. As we have seen, Helmholtz holds that mechanics provides us with an empirically verifiable understanding of rigid bodies—it establishes that the objects in the world in front of the mirror are rigid (at least approximately so), and that the empirical criteria used to determine this fact are not satisfied by the objects in the mirror (unless the mirror people possess, after all and to our surprise, the right mechanical notions, and we do not). The broader message is this: given a single world, with a single set of mechanical laws, the situation couldn't arise of two equally good interpretations of geometry.

However, the way that Helmholtz presents the relationship between the relevant facts and geometry leaves the view open to two different interpretations: the first interpretation sees Helmholtz as flirting with the obvious conventionalism in his account of the principle of free mobility within homogenous geometry. Congruence (based on free mobility) constrains the possible spaces to only these, but does not determine a privileged member among them, i.e. does not determine Euclidean space as *the* space over non-Euclidean spaces (of constant curvature). This interpretation begins with Poincaré, and is heavily influential on the logical empiricists, as well as later thinkers like Coffa and Friedman. Interpretations of this sort hold that Helmholtz's empiricism was taken to the brink of conventionalism (which is implicitly in the theory), but that he failed to recognise the final "move" in

the dialectic. If we look more carefully at Helmholtz's remarks, we see that he was coming to terms (or, rather, not quite coming to terms) with the constitutive principles of modern geometry, which had neither an empirical nor a transcendental content. They exhibit all of the markings of *stipulative conventions*. As DiSalle puts it, Helmholtz, according to this interpretation, merely "stopped short of recognizing the principle of free mobility as a convention" (DiSalle [2006b], 132) though its conventionality is clearly exhibited by Helmholtz. On this interpretation, Poincaré completes the move to an explicit conventionalism by showing that there is no way of determining (empirically or otherwise) between geometries (all of which can be made to describe our experience, so long as one produces the stipulations that define congruence and straightness), and therefore there is a free choice among them. All geometries are "relative", and there is no question as to which is "correct", so other considerations (such as simplicity of the geometry itself, or the preservation of mechanical laws) determine pragmatically which geometry will be chosen.

This interpretation surely captures at least some of the content of the convex mirror thought experiment. If we do not privilege our mechanical laws, there really is no way of speaking across the divide created by the convex mirror—no way of settling who is in a "normal" and who is in a distorted space. However, such an interpretation has much to explain. Firstly, though less interestingly, why did Helmholtz not characterise conventionalism explicitly? We set aside this question in the interest of brevity. Secondly, and more interestingly, it ignores two central aspects of the paper: (1) Helmholtz is attempting in the paper to show that we can imagine a non-Euclidean space, and therefore that the question whether space is Euclidean or not has an empirical sense, and (2) that Helmholtz did not think geometry a matter of conventional choice because that would mean that mechanical laws are not decisive. Thorough-going conventionalism was far too arbitrary on this front; Helmholtz did not think mechanics was a matter of arbitrary choice, as neither group of geometers has any choice but to base their geometry on the behaviour of bodies that seem to be rigid. If they live in the same physical world,

then there would be a fact of the matter as to what geometry was correct. Helmholtz seems to be making this further assumption, though for him it is not an assumption: mechanics gets its privileged role in virtue of its role in the genesis of geometry.

Further, as DiSalle has noted, that we can even make the sorts of distinctions required to formulate the convex mirror thought experiment presupposes that the same mechanical laws are operating for folks both outside and inside of the mirror. DiSalle thus offers us our second interpretation of the thought-experiment. Helmholtz is not pursuing a thorough-going conventionalism, he's pursuing rather a novel analysis of the concept of space. This analysis does not conventionally assume the principle of free mobility; that is far too arbitrary. Rather, it is a conceptual analysis "of what is implicit in our theoretical and practical judgments about geometrical measurement. In this respect it precisely follows the pattern of Helmholtz's analysis of what it means to 'imagine' a non-Euclidean world." (DiSalle [2006b], 136-137) When we say that Euclidean space can be imagined or visualised, we also admit that non-Euclidean space can be imagined or visualised. Our ability to make sense of how our world would look to those living inside of the mirror, and our ability to imagine how they could see themselves as living in a Euclidean space (even though our mechanical laws determine they live in distorted conditions) establishes this point.

For the purposes of this chapter, we should note that Helmholtz's naturalism was focused on producing an adequate analysis of the central concepts required by our intuitive understanding of space, showing that without such concepts one could not have an understanding of space as an external realm of interaction in which empirical judgments could be embedded. This form of conceptual analysis would have been entirely foreign to the British empiricists, since geometrical acquaintance was, for them, a matter of direct awareness of ideas of certain sorts, and not a condition on external objects (and which of them could be privileged as "geometrical objects"). Something here of Kant's transcendentalism survives the day, beyond the mere agreement that there is a synthetic spatial intuition: our "ideal" notion of geometrical objects is not satisfied in fact or in image, but as what is

presupposed by our application of formal mathematical systems in measurement, and empirical judgments involving space.

Thus, while Helmholtz agrees with Kant that we bring certain synthetic *a priori* principles to experience, and while he (like Mach) is committed to some form of the Subjectivity Thesis as a result, the problem of the external world is for him (as it is for Mach) completely separable from the adoption of the Subjectivity Thesis. Once again, it is a naturalised conception of our knowledge of the physical world (in this case as having a specific geometrical structure based on mechanical considerations) that seems to prevent the move from the Subjectivity Thesis to the problem of the external world. Naturalised empiricism is in the position to take certain facts as primary to inquiry, even when preserving the view that what is given in experience is something fixed. As we can see with Helmholtz and his use of the fact of rigid bodies, this fixed experience can, nonetheless, admit of multiple-factorisations based on what bodies we take to be rigid as a matter of fact. In a way, we see in Helmholtz that the Insight of Empiricism and Multiple-Factorizability can both be respected by a view of experience that also observes the Subjectivity Thesis.

§IV: Conventions, Protocols, and the Logical Construction of the World

Even though we have pointed to the ways in which the logical empiricists may have misinterpreted Helmholtz, it is clear that their thorough-going conventionalism was also a very decisive move away from the commitments of CBE. No longer did one need to find the epistemological "grounds" in order to say that some body of knowledge (whether empirical or formal/mathematical) was properly founded. "Foundations" for one's formal framework (including the relevant concepts) are not established, but stipulated. The first task of philosophy is to discern how such stipulations are to be made so as to accurately capture our pre-theoretical intuitions about what parts of our knowledge are factual, and what parts are non-factual (since different stipulative decisions may draw this line differently). The second task of philosophy is examine the consequences of our choice of theoretical framework, including its conceptual commitments and empirical application.

Theories allow us to define a given concept only relative to a theoretical framework; the task of epistemology is, first and foremost, the isolation of those theoretical principles that allow one to employ the theory to make empirical predictions and explanations of natural phenomena. One can empirically justify a concept, but only relative to the fruitfulness and empirical success of the (conventionally chosen) theory as a whole. This presupposes that the concept in question can be implicitly defined relative to the theory itself, and therefore that the theory is complete. However, this procedure is inadequate if we wish to justify the initial generation or substantial revision of concepts (via experience and analysis) as Helmholtz seems to be doing in the DiSalle interpretation of the convex-mirror thought experiment. The logical empiricists thought that any such explanation of how such a concept is gained must exceed epistemology, at least partially, requiring recourse to some psychological faculty such as imagination, representation, intuition, etc. Helmholtz's analysis of space necessitates a move beyond the framework of a given theory since it attempts to analyse the concept of space that is contained within Euclidean as well as alternative geometries of constant curvature. However, as we have seen, there is nothing flawed or "external" about such a breed of conceptual analysis. We would do well to preserve it rather than the logical empiricist view that conceptual analysis can only occur once we have become clear on which theory the concept being analysed belongs to.

IV.1: From Protocols to the Logical Construction of the World

As we have seen, MSE thinkers may hold the Subjectivity Thesis while disregarding the classically conceived problem of the external world. But are they wedded to the propositional given? That the logical empiricists held that the given entered into scientific arguments through representation via propositions is undeniable. But this is certainly not akin to the general sense-datum view that the given makes its pronouncements in the form of propositions. The famous *protocol sentence debate*, which dominated discussion in the Vienna Circle's final public phase, shows that much. We must therefore be careful to note the ways in which the logical empiricist

conceptions of protocols differ from the generalised sense-datum view, for the departures are, in many a surprising way, anti-Cartesian. We start by noting three interesting characteristics of the most popular theories of protocols in the Vienna Circle.

(1) The basic statements—"protocol statements"—were often not considered indubitable and certain (Neurath and Carnap).
(2) They were certainly not limited to the subjective reports in the first-person (Schlick, Neurath, Carnap).
(3) As a result of 1 and 2, basic statements tended to avoid the problem of the gap between the internal and external world by allowing the very concepts of the sciences to enter into the basic statements.[41]

These features seem to challenge the received view that the Circle was mainly concerned with foundational issues in epistemology. This is with good reason. In regard to foundational certitude, with the exception of Schlick's notion of affirmations *(Kontstatierungen)*—"statements" about the immediately perceived—, the early logical empiricists, particularly Neurath and Carnap, rejected the idea that the basic sentences of the sciences were *certainly known*. (It should also be noted that Schlick agreed with Carnap and Neurath that protocols—which were not identical with affirmations in his philosophy—were refutable and revisable.[42]) Schlick comes closest to embodying the sort of caricature of logical empiricism, and comes closest, therefore, to offering a modern variant on our Cartesian themes of the primacy, privacy, and foundational character of the given. But even here, the Cartesian label holds only to a degree. Firstly, affirmations are "statements" of sorts for Schlick, but they are essentially ineffable and fleeting ("momentary"), and therefore only loosely thought of on the model of statements. Affirmations are "not identical with statements written or remembered. i.e., with what could properly be

[41] We use phrases such as "often" and "tended to" in recognition of the very long debate that took place in the Vienna Circle from Carnap's entry into the Circle (1926) to the end of its public phase (1935-36). During this period, a number of Circle members entered the debate, and held very nuanced views that need to be ignored here. For an analysis of the multiple and interconnected changes within the Circle, see Uebel [2007].
[42] For an account of Schlick's "affirmations", their distinction from protocols, and the debate between Neurath and Schlick regarding radical physicalism, see Uebel [2007], 304-315.

called 'protocol propositions'…" (Schlick [1934/1979b], 381) This position is already at odds with Gupta's characterisation of a canonical sense-datum view.

Secondly, we can see that a sense-datum model of perceptual knowledge is far too simple to account for Schlick's complex (even convoluted) epistemology. Affirmations, like sense-data reports, are foundational, but unlike such reports they are not really statements at all. Protocols, like sense-data reports, are statements, but not really foundational. We can see, here, Schlick's tenuous Cartesianism at work—his desire to "utilize stretches of the Cartesian road" to make sense of what it means to be "immediately observed" without "losing ourselves in the cogito ergo sum and similar absurdities". (Schlick [1934/1979b], 380) Schlick was looking for an unshakeable foundation on which the edifice of knowledge could be built—a Cartesian program indeed; but even if we assume that Schlick's views regarding affirmations were canonical within the Circle, and even if we also establish by fiat (for history is no friend here) that affirmations were able to be expressed as statements *simpliciter*, Schlick still radically breaks with the sort of generalised sense-datum theory that Gupta takes to be the paradigmatic interpretation of classical empiricism for two reasons: (a) Schlick argues that affirmations are not really foundational at all, rather the "framework" within which foundational statements can be generated,[43] and (b) Schlick's epistemology seems to come closest of all of the principal protocol sentence debaters to the Cartesian model (since affirmations are absolutely certain, undeniable, and subjective), though even here he inverts the Cartesian epistemological order—affirmations play their vital epistemic role at the *end* of inquiry (in verification), not in giving us the first principles which make inquiry possible and its dictates true:

> Observation plays the part of absolutely certain knowledge, not when it is taken as a starting point and stands at the beginning of science, but when it is

[43] Schlick never faced head on the Cartesian conundrum in which he found himself: if affirmations are to be foundational, then they cannot be so fleeting and so ineffable, yet if our knowledge is to be given a proper and unshakeable foundation, then it must come from affirmations (the only proper and unshakeable bits of knowledge we have). Neurath and Carnap avoided the Cartesian conundrum by arguing that the purpose of protocols is to serve as a sufficient evidentiary basis for science, not to express our incorrigible epistemic beliefs (if such things even exist).

> arrived through science… These ultimate propositions stand, not at the beginning, but at the end." (Schlick [1986/1987], 92—quoted in Uebel [2007], 308)

For these reason, it is hard to consider even affirmations (let alone protocols) as fitting into the Cartesian model, at least not without a great deal of gerry-mandering. As a received historical view, even Schlick's affirmations (as we have seen) run contrary to the Cartesian conception of experience in significant ways—ways that do not call into question Schlick's foundationalist aims, but rather the limited sense in which such aims were "Cartesian" (in both Schlick's and our senses of the term).

The attribution of Cartesianism to the logical empiricist tradition as a whole is even more dubious. Such an attribution ignores the central desideratum of intersubjective agreement in scientific and epistemological matters, a desideratum which was central to Neurath's and Carnap's development of physicalism and "liberalised empiricism" from the publication of the *Aufbau* to the end of the Vienna Circle's public phase. This desire to account for the essential communicability of scientific knowledge was a motivating factor for Neurath and Carnap throughout the early phases of the protocol sentence debate, and on up through Carnap's syntactic and then semantic turns. In its first guise, *physicalism*, intersubjective communicability of scientific results was taken as a matter that could not be ignored. The 19th and early 20th centuries saw the development of increasingly more fragmented and disparate scientific projects, from fundamental physics to the social sciences. Given the radically different languages used by, for example, chemists and economists, how were common problems to be effectively discussed and ameliorated? Less important than the desire to unify science for unity's sake, or the desire to reduce the softer sciences to the harder ones, unification was meant to further the sciences as a collective body of knowledge, and allow for particularly

difficult and often recalcitrant problems to be solved by mutual congress between the sciences.[44]

But surely unification, even if laudable, committed the logical empiricists to a form of phenomenal reductionism and the Subjectivity Thesis. As Friedman has noted, the so-called "First Vienna Circle", with its focus on Mach's phenomenalism and mechanism, set the stage for the Circle's revival under Schlick as principally concerned with phenomenalism. Carnap's entrance (through Schlick) into the Circle in 1926, and the warm reception of the *Aufbau* in 1928, are best understood relative to this backdrop of Machian phenomenalist-empiricism.[45] Under this view, the Circle was so taken by Carnap's research precisely because it realised a long-standing, classical empiricist project: the construction of the external world based purely on the data of sense. Where Locke, Mach, Avenarius, and Russell had made only promises and piecemeal progress, Carnap was developing a pure system of construction, within which the method of "quasi-analysis" could be employed to construct ever more sophisticated classes of objects from the elementary experiences of a single cogniser, up through the development of sense classes (i.e. the construction of individual sense modalities based on dimensionality considerations), to colour classes, to the construction of sense-data themselves, and, eventually, their coordination with space-time points external to the cogniser. The project of phenomenal reductionism, it was shown, was possible; what was required—what was not available to earlier empiricists—was a proper logic for the enterprise: Russell's theory of relations.

This is certainly the received view of the *Aufbau*. However, the *Aufbau* itself tells a different story about knowledge, one that is decidedly against the phenomenalist-reductionism and foundationalism of Schlick and Waismann's neo-

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[44] Carnap states in his Intellectual Autobiography that "one of the most important advantages of the physicalistic language is its intersubjectivity…" ([1963], 52) Further, Carnap says on the same page (in the following pargraph), that the principle of the unity of science "says that the different branches of empirical science are separated only for the practical reason of division of labor, but are fundamentally merely parts of one comprehensive unified science."

[45] Friedman [1992/1999], section IV, 141-152 (especially 146 onwards). Friedman gives a brief (though accurate) account of the *Aufbau's* reception as a piece of phenomenalistic foundationalism by Schlick and Waismann, and the subsequent non-phenomenal and non-foundational defense of its ideas by the growing "left wing" of the Circle (including Carnap himself).

Machianism, while preserving a form of phenomenal reductionism that Friedman perhaps too quickly rejects in his [1987/1999] and [1992/1999]. The *Aufbau* establishes a construction system in which all concepts can be defined either by their phenomenal character or the step-wise ways in which they can be derived from more basic phenomenal concepts. However, this is just one of many different possible construction systems. Carnap also mentions (though does not pursue in detail) the construction of psychological objects using only a physical or material basis, reversing what a classical empiricist (fully entrenched in the way of ideas) or phenomenalist (such as Mach) would deem an irreversible order. (See §§57-60.) In fact, Carnap discusses the possibility that the constructional system can have a hetero-psychological, i.e. a non-solipsistic psychological basis (§§58, 63); physical bases (§59), for which Carnap offers three possible starting points: 1. an atomic basis (atoms and their relations), 2. a basis of space-time coordinates (and their relations), and 3. a basis of world lines in Minkowski space-time. (§62); or even a possible, though "problematic", construction system with cultural objects at its base—reducing psychological and material objects to cultural ones. (§62)

While phenomenal reductionism is pursued, the only explicitly Cartesian aspect of this project is the recognition that the epistemically significant constructional system will reconstruct the material system of concepts using the auto-psychological concepts. (This amounts to nothing more than the claim that those interested in the theory of knowledge are primarily interested in how the individual knower comes to construct physical concepts based on those concepts available in experience—as a matter of historical record, this is plainly true.) Because the constitution of knowledge—its rational reconstruction—can be done in any way we please, Carnap's *Aufbau*, far from being the most sophisticated version of phenomenal reductionism, and the realisation of an ancient empirical project, is actually anti-foundationalist, or rather *multi-foundationalist*, since any system of concepts—insofar as it has a significantly rich internal structure—can be used to reconstruct any other (adequately structured) set of concepts. The *Aufbau* is an attempt to make sense of the great flexibility we exhibit in conceptual matters, not

an attempt to rigidly fix through some reduction scheme all of our concepts in the immediately given.

That such a project fails for technical reasons is not our concern here; we are concerned with the historical fact that, in their earliest guise in the logical empiricist tradition, the so-called "basic phenomenal elements" of "auto-psychology" fail to be Cartesian. Firstly, the basic auto-psychological elements in Carnap's *Aufbau* system are not akin at all to the canonical sense-data of other empiricists, such as Mach, Russell, or Ayer. For Carnap, sense-data must be constructed (using the method of "quasi-analysis") from "elementary experiences". Elementary experiences are unanalysed cross-sections of experience, which are *basic*, not in the sense that Carnap takes them to be immediately known, or unquestionable, but in the strict technical sense that they are the primitive stuff out off which higher-order constructions ("quality classes", "sense classes", and, later in the construction, "sensory data") are made, using only the single two-place predicate "recollection of similarity". Their "elementarity" is understood through their role in the constitutional system. As Friedman has noted ([1987/1999], 91), Carnap was heavily influenced by holistic and Gestalt psychology at the time of writing the *Aufbau* (§§67, 75), rejecting the sensory "atoms" at the base of CBE models of experience (i.e. Locke and Berkeley's ideas, Hume's impressions, or Mach, Russell, and Ayer's sense-data).

Further, Carnap seems in his very methodology (his constructional voluntarism) to be rejecting the way of ideas, and therefore the Subjectivity Thesis. Similarly to Mach's principled phenomenalism, Carnap's voluntaristic phenomenalism begins with the best scientific understanding of the day regarding our experience, and not some Cartesian notion about the epistemic primacy of the immediately given. In this way, even though Carnap is reconstructing the concepts of physics using elementary experiences, he is not attempting to "ground" physical concepts in the immediately given. Strictly speaking, the *Aufbau*, as we have seen, countenances no notion of "immediate object of experience", and certainly no talk at all of direct awareness—its elementary experiences are nothing like the stuff of

which we take ourselves, in moments of empiricist naiveté, to be aware, such as colour patches, visual shapes, auditory "images", particular smells, or even worldly objects, etc. Carnap receives his notion of "basic elementary experience" from Gestalt psychology,[46] not from *a priori* reasoning, so it should perhaps be no surprise that we see none of the typical Cartesian arguments to buttress the sort of representationalism we discussed earlier in this chapter.

IV.2: Sources of the Confused Reading of the Aufbau

From where do the confusions about Carnap's commitments in the *Aufbau* arise? One candidate for the source of the confusion is certainly Carnap himself. In the *Aufbau* §3, Carnap adopts Russell's "supreme maxim in scientific philosophizing", the methodological principle which guided Russell's external world program: "Wherever possible, logical constructions are to be substituted for inferred entities." In fact, Carnap tells us that he will "employ this principle in an even more radical way than Russell", offering a construction of unobservable entities based on those which are observable. To the extent that Russell's external world program was a continuation of the grand empiricist project begun by Locke, Carnap seems to be inducted into the tradition as well. Further, in the same passage, Carnap compares his construction theory to earlier forms of phenomenal reductionism (specifically Mach and Avenarius' strict descriptionism) which reduce scientific concepts to the given in experience. From the mention of such earlier projects, one could certainly be forgiven, especially if one were to view the *Aufbau* from the context of the first Vienna Circle, for adopting a straightforwardly phenomenalist reading of the *Aufbau* (and the auto-psychological construction system expanded upon within its pages).

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[46] A.W. Carus [2007] has challenged the idea that it was Gestalt psychology acting as a primary influence on Carnap while writing the *Aufbau*. Gestalt thinking did influence Carnap throughout the mid-20s, but it is far more likely that Carnap was much better acquainted with the philosophical criticisms of associationist psychology, especially the work of Husserl, Natorp, Cassirer, and Vaihinger. (See fn. 7 on 147.) Carus argues that it was actually Carnap's Kantianism that led him away from classical empiricism to a "structural empiricism" or "logical empiricism", applying Russell's theory of relations and Husserl's phenomenology to Vaihinger's "chaos of sensations" to reduce the number of possible construction systems (what Vaihinger called "fictions") to something more manageable, eventually just the auto-phenomenological and hetero-phenomenological systems discussed in the *Aufbau*. (145-148)

Moreover, in his Preface to the second edition of the *Aufbau*, Carnap focuses much of his discussion on phenomenal matters. In his description of the book, he isolates as its main concern "the possibility of the rational reconstruction of the concepts of all fields of knowledge on the basis of concepts that refer to the immediately given." (Preface, v) In response to the technical problem with his choice of basic element and basic relation, Carnap says, "I should now consider for the use as basic elements, not elementary experiences… but something similar to Mach's elements, e.g. concrete sense data, as, for example, a red of a certain type at a certain visual field place at a certain time", (vii) and the choice of basic relations the relations existing between such data, such as "earlier than". Nonetheless, even though Carnap is offering here a particularly phenomenalist reading of the *Aufbau* in retrospect, he still includes a lengthy section on the possibility of alternative, non-auto-psychological forms the construction of concepts may take, with reference to the relevant passages (already discussed above). Such considerations make a completely phenomenalist reading of the *Aufbau* untenable; if we base our readings of the book solely on Carnap's interpretation of his own work, at least something of the nuanced constitutionalism (as opposed to narrow phenomenalism) of the work becomes undeniable, as does the notion that Carnap held anything like the Subjectivity Thesis, for direct awareness is never assumed to be *the* basis of our knowledge of physical things, only one of the possible frameworks for the reconstruction of physical concepts.

The real source of confusion seems rather to lie in Quine's reading of the *Aufbau* in Quine [1936/2004], [1951/1961], and [1969/2004]. The massive influence of each of these works, especially the latter two, on analytical philosophy in the second half of the twentieth century (and even today), account for the scope and nature of the misreading of Carnap as a phenomenal reductionist and classical empiricist.

Firstly, there is a tendency, beginning with Quine, to over-state the similarity between Carnap's *Aufbau* and previous incarnations of the external world program, particularly Russell's (from Russell [1914/1963], and, especially, Russell

[1914/2009]). Quine conflates the two when he views the projects as essentially similar. Here is a taste of Quine's "Two Dogmas" view:

> Radical reductionism, conceived now with statements as units, set itself the task of specifying a sense-datum language and showing how to translate the rest of significant discourse, statement by statement, into it. Carnap embarked on this project in the *Aufbau*… He was the first empiricist who, not content with asserting the reducibility of science to terms of immediate experience, took serious steps toward carrying out the reduction. (Quine [1951/1961], 39)

And, here is an example from "Epistemology Naturalized":

> To account for the external world as a logical construct out of sense-data – such, in Russell's terms, was the program. It was Carnap, in his *Der logische Aufbau der Welt* of 1928, who came nearest to executing it. (Quine [1969/2004], 262)

Others have pointed to such passages in an attempt to show that Quine misunderstood the *Aufbau* as (a) a piece of radical phenomenological reductionism, and (b) the completion of the CBE project of accounting for the external world using only the objects/concepts of immediate experience.[47] While this is certainly true, it often goes unrecognised that such a reading of Carnap is merely a symptom of a much deeper Quinean misunderstanding of Carnap's work, a misunderstanding that began in Quine [1936/2004]: the mis-interpretation of Carnap as following in the Cartesian tradition of epistemological foundationalism. Quine makes this interpretive error when he asserts that Carnap's project is essentially a completion of the Tractarian (and Cartesian) attempt to answer the question, "How is logical certainty possible?" (Quine [1936/2004], 64). However, Carnap never adopted such a project as his own. In a sense, Carnap was concerned (as were other "left wing"

[47] See Friedman [1987/1999], [1992/1999], and [2007]; Richardson [1990], [1992], and [1998] (especially Chapter One, "Reconstructing the *Aufbau*"). For a view that challenges the Quinean critique by arguing that Russell too was not a reductive empiricist, see Pincock [2002] and [2007]. For an account regarding the relationship between Russell's and Carnap's structuralism (as opposed to the similarities or lack thereof regarding phenomenal reductionism), see Demopoulos and Friedman [1985] and Demopoulos [2003a] and [2003b].

members of the Vienna Circle) with showing that the *Tractatus* was far too restrictive in assuming that there must be (a) a single logical language (which could never be the object of its own utterances), and (b) that there was one, and only one, correct logic for that language. Of course, such a contrary view to the *Tractatus* was undeniable by the time Carnap published his *Logical Syntax and Language* [1934/2002], making Quine's 1936 remarks otiose; but, even in 1928, the *Aufbau* makes clear it is not concerned with establishing *the* correct logic, or *the* unmistakable simple atoms of a *single* logically perspicuous language.

In regard to our empirical knowledge, Cartesianism is supposed to be operating in Carnap's work in both what Quine calls a "conceptual" sense (reducing all material concepts to concepts of experience) and a "doctrinal" sense (offering an experiential justification for all of our substantive knowledge). The *Aufbau*, Quine tells us, attempts to reduce all physical statements to statements couched in a language of observation, logic, and set theory. (We have already seen here that such a "reduction" need not imply foundationalism or reductionism, and also that Carnap was not utilising an "observation language", but we can set such matters to the side for now.) For Quine, the conceptual ingenuity exhibited by the Aufbau means little, however, since there is no way to follow through with the doctrinal side of Cartesianism:

> … the mere fact that a sentence is *couched* in terms of observation, logic, and set theory does not mean that it can be *proved* from observation sentences by logic and set theory. The most modest of generalizations about observable traits will cover more cases than its utterer can have had occasion to actually observe. The hopelessness of grounding natural science upon immediate experience in a firmly logical way was acknowledged. (Quine [1969/2004], 262-263)

Quine then asks what "could have motivated Carnap's heroic efforts on the conceptual side of epistemology, when hope of certainty on the doctrinal side was abandoned?" (ibid) But this only shows Quine's confusion about Carnap's aims. Carnap was not concerned with offering a justification (or proof) of our empirical and scientific knowledge in the *Aufbau*. No such justification is required. He didn't

see his new development of construction theory as an expression of Cartesianism, as a way of overcoming the deficiencies of earlier forms of empiricism. He saw construction theory as a way of making philosophy more scientific, in the sense of showing how the basic concepts of what was once thought to be a purely philosophical domain (the auto-psychological) could be reconstructed using the "system form" of physics. Carnap still maintained that it was an interesting question as to how the physical world could be constructed from the solipsistic experiences of a single cogniser, but such a basis was no longer thrust upon the philosopher as a necessary starting point for all conceptual inquiry. Many of the old empiricist concerns about the "problem of the external world", the *gap* between the subjective and the objective, evaporate; what is left is the use of the best psychology of the day to characterise what we mean by "basic element of experience", and the use of the best logic of the day to explain how our received physical view of the world may be constituted by such elementary experiences. Little of "first philosophy" or "philosophy-first" thinking remains. The ghosts of Cartesianism have been banished.

If Carnap were Cartesian in the requisite sense, he would never have acknowledged that the order of constitution could be reversed. He was interested rather in exhibiting the flexibility of our concepts and conceptual systems, and the mutual ways in which such systems could be put into relations of translation with one another—to show that there is no special division between the concepts of science, the concepts of elementary experience, the concepts of partially constituted experience, and the concepts of "hetero-phenomenology". Stating the project in light of Cartesian concerns, and asking what Carnap's motivation could have been to develop such an empiricism after Humean scepticism showed such pursuits to be quixotic, only misses the point we have laboured here: that Carnap was indeed not concerned in the slightest with Cartesian foundational certitude, but with developing a new scientific philosophy quite removed from the concerns of Cartesianism.

§V: Concluding Remarks on the relation of MSE to Reformed Empiricism

But what can we say now of Gupta's view that "classical empiricism" suffers from the Cartesian idea that experience is a kind of direct awareness of facts of an undeniably certain and subjective sort? Our examinations in this chapter, preliminary as they may be, lead us to the conclusion that there exists a multiplicity of interpretations of empiricism, and that the historical development of these different interpretations is required if we are to understand how new proposals for empiricism (like Gupta's) *fit* into the tradition, and what set of commitments from previous forms of empiricism are worthy of keeping, and which we may have to discard as inessential, technically or conceptually confused, or just simply wrong.

However, this chapter had another purpose; to isolate a key historical shift in the meaning of "empiricism", from the philosophy-first views of Classical British Empiricism to the commitments and concerns of post-Kantian Modern Scientific Empiricism. Much of what Gupta says regarding the methods, theses, and errors of the former cannot be truthfully said of the latter. At this point, Gupta could simply claim that his target is limited, and that his relative lack of reference to MSE thinkers exhibits this focus. However, such a move would be unwise, both for methodological and strategic reasons. Firstly, regarding methodology, Reformed Empiricism does itself a disservice if it only speaks to the general epistemological concerns of the CBE tradition, for the MSE tradition shows a desire to put empiricism in congress with the best sciences of the day. Also, there is a tendency in the existing literature, too heavily informed by Quine, to take for granted connections between themes in general epistemology and the philosophy of science; Reformed Empiricism can act as an inoculation to such tendencies.

An empiricism which speaks only to the concerns of the principal CBE thinkers fails to take into account the various ways in which a serious attempt to incorporate our scientific knowledge into empiricism offers empiricism its most interesting moments for reform and revision, be it the non-Euclidean geometries of the 19th century, the non-classical physics of the early 20th century, or, in our present day, the constant integration of knowledge from relativistic and quantum

physics, biology, chemistry, the social sciences, and medicine which invite us to challenge our received view of knowledge. If Reformed Empiricism is only thought of as an alternative to CBE, it need say nothing about the sciences or the problems posed by the sorts of formal knowledge that the sciences at least seem to presuppose. But it is the discussion of such matters that has given us the most reason to suppose that our pre-conceived notions of the self and the world are indeed mistaken. This is why the question of how our scientific theories and concepts relate to experience, and how they can be revised by experience, will be a major topic of the next chapter.

Secondly, regarding strategy, there is many an ally to be found in the canon of the MSE tradition. Many of the key tenets and theses of Reformed Empiricism find prototypes within this tradition. Let us examine but a few here. (1) Reformed Empiricism is intended to be a brand of empiricism that takes the Insight and Multiple-Factorizability seriously. Unlike CBE, Reformed Empiricism does not attempt to fix a notion of the self (as privileged authority over its current mental states and their objects) so that the Insight may be preserved. But this means we must make sense of what it means to have experiences that are, in a certain sense, underdetermined. Experience by itself, we will recall, does not give epistemic license to any set of judgments. However, such a view already shares much with the logical empiricist's idea that experience itself is in need of coordination if we are to use it to generate and justify substantive judgments. The relativised *a priori* frameworks of Schlick, Reichenbach, and Carnap are all instances of the central, yet limited, role given to experience in the logical empiricist tradition. The interdependence between experience and view overcomes many of the restrictions of this relativised *a priorism*, but it also, we argue, has something to learn from this strand of post-Kantian philosophy of science.

(2) Carnap's voluntarism, of which we have seen an early example in the *Aufbau*, holds that there are no rational grounds on which to preclude the sorts of formal commitments that are necessary to account for our theoretical knowledge. While "tolerance" operates in Carnap's work in a way that finds no technical

analogue in Gupta's work, the spirit of tolerance (if we may be so bold) is certainly shared. For Carnap, there is no principled way of precluding one choice of formal system over another, though the choice of some formal system or another is required as the relativised *a priori* framework from within which we may attribute content to empirical (synthetic) claims. (We explore Carnap's notion of tolerance, including the Principle of Tolerance, in the next chapter.) For Gupta, there is no argument to be given regarding the starting points of our empirical inquiry, no "right" or "wrong" choice in world-view, though some choice of view must be made to begin inquiry (to make empirical judgments possible) within his interdependent system. The spirit of tolerance is found in Gupta's commitment that any view is a respectable starting point for inquiry, provided that the view is reasonably open to revision. For both Carnap and Gupta, there is nothing to be said about the rightness or wrongness of said views until they are chosen and utilised. A merit in Gupta's work is his offering a perspicuous logic for how such revision may go, a process which remains ill-defined in Carnap's philosophy.

(3) Whereas Carnap held in the *Aufbau* that a plurality of construction systems are permissible, and whereas he held in his later work that a plurality of syntactical system (during his syntax phase) or language forms (during his mature phase) are admissible based on one's interests in inquiry, Gupta recognises the *prima facie* rationality of an infinity of possible views, precluding on *a priori* ground only those views that are pathological. This plurality of possible systems of coordination is certainly a similarity between the logical empiricists and Gupta, though it is obvious, especially given what we have seen in the last chapter, that a great merit in Gupta's work is in making the revisions from one view to another itself sensitive to experience. The logical empiricists held that such a change, even though perhaps motivated by an epistemological critique, was ultimately conventional (according to Schlick and Reichenbach), or a pragmatically motivated shift in view (according to Carnap). We don't pretend here that the logical empiricists had the requisite logic necessary to make sense of the complete interdependence of experience and view, only that they attempted to make sense of

such an interdependence as far as their logic allowed. Reformed Empiricism may offer us a way of completing the logical empiricist program of accounting for the interdependence of experience and view without defaulting to an extra-empirical (and extra-epistemological) notion of voluntarism.

(4) As we have seen in the last section, Gupta finds a prototypical version of his view that privileged access to one's subjectivity plays no role in generating knowledge. Even as early as the *Aufbau*, Carnap moved away from what he clearly saw as a troublesome claim—that the theory of knowledge was equivalent to offering a justification of our knowledge based on experience. If anything, epistemology (in the *Aufbau*) is about conceptual clarification, so that we can begin to ask (with something like scientific precision) the right sort of clear questions because we can see how all sciences, even solipsistic ones (like auto-psychology), might fit together in one common system of concepts/objects. This tactic, of course, presupposes the kind of unity Gupta thinks we will find regarding empirical matters after a long process of convergence (based on experience). The differences here are vast, but they mask a characteristic common to Reformed Empiricism and MSE, setting both apart from the philosophy-first methodology of the CBE. For both Reformed Empiricism and MSE, the assumption is that no matter what particular system of concepts one uses to think or speak about the world, if that system (theory, view, etc.) is sufficiently rich, then no differences between system (theory, view, etc.) will block communication among practitioners, or the eventual understanding based on access to a shared domain of interest. For Carnap, this eventual agreement resided in what he supposed to be facts about the structure of the world, a structure which exceeded any differences or particularities of view. With Gupta, we have a new understanding of how convergence and agreement can be found that does not rely on this troublesome supposition.

But possible allies always come to the table with their own battles and strategies. The more connections we find between Reformed Empiricism and MSE, the more we are led to think that perhaps some more of the commitments of MSE deserve explicit mention in Gupta's work. As we shall explore in the next chapter,

Gupta makes very little reference to the sorts of concerns of most of the MSE tradition: the special place of formal concerns in the science (and how our formal knowledge could best be explained and justified); the presuppositions necessary to make the communication of scientific findings, or even plain common sense empirical judgments, possible; and the role of conceptual analysis in the explication, and revision, of physical theory. Reformed Empiricism doesn't completely play its role in the tradition without considering these problems. If it is a successful comprehensive empiricism, it ought to be extendable to embrace these problems.

CHAPTER THREE

Empiricism, Formal Knowledge, and Communicability

The previous chapter attempted to show the increased importance of accounting for our theoretical and formal knowledge in the development of empiricism from its British roots to the post-Kantian projects of Helmholtz, Mach, and (partially through Poincaré) the logical empiricists, principally Schlick and the early Carnap. As will hopefully be clear, the history glossed in the previous chapter is thought to contain several desiderata that may inform more contemporary interpretations of empiricism, especially Gupta's Reformed Empiricism (as the nexus of our discussion here). We saw that in spite of particular problems with particular methods and theses of MSE, many central concerns of enduring interest remain and are worthy of attention. We also saw that a certain Quinean critique of the empiricist tradition's "problem of the external world" (focused on Carnap's *Aufbau*) was the result of a mis-reading, not only of the rich history examined in outline in Chapter Two, but also of the role Cartesian themes played in the latter stages of MSE; in further detail, we saw that Carnap's *Aufbau* is actually better read as a rejection of the Cartesian picture (a) that we are only ever acquainted with essentially private data of sense, (b) that all knowledge is justified by the extent to which it can be reduced to phenomenal experience, and (c) that foundationalism is therefore the only proper method for philosophy. All of the modern empiricists we examined in some way threw off at least one of these shackles; Carnap was merely in the favourable position of doing away with all of them, replacing traditional theory of knowledge with the constructional system ("constitutional theory"*)* of his *Aufbau*.

That such a project was ultimately untenable is not of our immediate concern. As Goodman [1951/1977] argued, the notion of analysis (both "proper" and "quasi") employed in the *Aufbau* suffers from ineliminable problems.[48] Quine

[48] Goodman [1951/1977] showed that there were two distinct technical problems at the foundation of the *Aufbau's* notion of *analysis*. For Carnap, quality classes are analysed via proper-analysis or constituted via quasi-analysis based on "part-identity" for members of a group. We depart here from Goodman's analysis in slight detail, though not spirit. Imagine being given a set of "erlebs" (momentary time-slices of an individual cogniser's experience), each possessing some combination

[1951/1961], on top of his more broad concerns with Carnap's empiricism, argued that sentences of the form "Quality q is at x;y;z;t" can never be translated into "Carnap's initial language of sense-data and logic." (40)[49] Friedman [1987/1999] questions whether the only primitive logical relation of the *Aufbau*, namely *Rs* ("recollection of similarity") can be replaced by a relation (or relation variable) that is both "objective" and "founded", i.e. he argues that the *Aufbau* cannot capture the empirical relations of science in a purely structural way.[50] In the last chapter, we saw that such technical problems were indeed divorceable from the Cartesian commitments that Quine (and others) thought dogmatic to empiricism. Thus, we are in the unique position here, having joined the chorus of voices in the contemporary literature which challenge the Quinean wisdom, to see if some of the central themes, concerns, commitments, and desiderata of MSE can be integrated into more recent

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of elements (call the elements a, b, and c). We want to know these elements, and how they function as constituent parts of the erlebs. For the purpose of critique, take these five entities: *ac*, *a*, *b*, *ab*, and *abc*. Because c is systematically related to a (wherever c occurs, a occurs), we cannot (at least in such contexts) know what c is. (Goodman calls this the "companionship problem".) Goodman also raises the "problem of imperfect community". This occurs when we look at a set of erlebs, each related to the others in the set, but no element running through the whole of the set. Such a set will constitute a quality class by Carnap's system, though such a quality class does not seem to map onto any intuitively known quality. (See §§V.3, V.5 of Goodman [1951/1977].)

For an argument that Goodman's critique can actually be used to strengthen Carnap's *Aufbau* (by supplying the work with a formal characterisation of empirical phenomena such as illusion and under-determination) see Paprzycka [1994], especially section 3. Mormann [2003] also challenges Goodman, arguing that he (Goodman) failed to recognise the geometrical background of the *Aufbau*, against which many of the technical "problems" of analysis disappear. Also, just as Proust [1989] had showed, Mormann contends that quasi-analysis remains useful as a general method of constitution, even if we accept the shortcomings of its application in the constitution of external space based on phenomenal elements.

[49] Mormann [2004] offers just such a translation of the "is at" relation, calling into question Quine's claims.

[50] Such structural definite descriptions replace *Rs* with a variable ranging over relations, and there is no non-arbitrary way to limit the range of the variable, i.e. no way of determining a set of unique relations holding between empirical facts. Thus, the problem arises for how to limit the range of the variable; but here, we seem to rely on a procedure which privileges "experienceable, 'natural' relations" (*Aufbau*, §154). The objectivity of the sciences is in no way secured for us, since the reconstruction of the relevant relations still requires an appeal to *foundedness*, and thus relies ineliminably on individual intuition. Friedman puts the problem the following way: "We are motivated to pursue a program of complete formalisation by a conception of scientific objectivity that seeks to disengage objective meaning entirely from ostension. We now find that to reach our goal we need to introduce the class of *founded* relations as a primitive notion of logic, where the founded relations are just the 'experienceable, "natural" relations.' But what can 'experienceable, "natural" relations' be except precisely those relations somehow available for ostension? Our original motivations, in other words, have been totally undermined by Carnap's final move." (Friedman [1987/1999], 103)

interpretations of empiricism, *sans* the technical difficulties. It will be our contention in this chapter that such integration of MSE desiderata with Reformed Empiricism is not only possible but also fruitful. It extends Gupta's novel view for general epistemology to a set of recalcitrant problems in the philosophy of science, often with surprising and satisfying results.

However, we come to such an extension of Reformed Empiricism only by way of a critical exposition of Gupta's reticence to discuss in detail matters formal and scientific in his [2006a]. In §I, we will explore some of the existing critiques of Gupta's empiricism in the literature. Some have argued that Gupta's empiricism, in spite of its novel approach to re-thinking the rational contribution of experience to knowledge, masks rationalist presuppositions and *a priori* distinctions, betraying its purported commitment to the Insight. We will argue against such critiques. We will pay special attention to Schafer's [2011] contention that Gupta views experience less on the model of empiricism, and more in line with the Kantian tradition. Yes, Gupta's empiricism has some elements in common with Kantianism, but only those that were found useful by the MSE tradition. Gupta does not hold that experience is a product of the understanding and the sensibility, nor does he think there a transcendental argument to be given about the nature of our view of the world. He does hold with Kant (and most of the principal logical empiricists) that experience alone does not give us epistemic license to a set of judgments. Here, the false dichotomy between Kantianism and empiricism (construed broadly to include MSE) begins to evaporate.

In §II, we will explore Gupta's discussion of our formal knowledge. Because Gupta attempts to obviate all general discussion of mathematics until certain foundational issues in the philosophy of mathematics are settled, he does not recognise the extent to which mathematical and mechanical assumptions are operating in our normal judgments about extensive magnitudes. We will argue that it is a mark of classical empiricism to view mathematics as separable from empirical judgment, and that Gupta's Reformed Empiricism should avoid the pitfalls of such a view if at all possible, perhaps by examining possible revisions to the theory which

find their inspiration in the nuanced ways scientific empiricists attempted to account for our geometrical assumptions about space.

In §III, we will assess a central problem with Gupta's logical re-categorisation of the given as a function. This categorisation is of utmost importance for the successful defense of empiricism against the myth of the propositional given and other related problems. As such, it is central to Reformed Empiricism, but it also has undesired ramifications when put in the context of the experimental sciences, and, we argue, has difficulty accounting for scientific knowledge and the communicability of observation. If experience operates like a function, then it remains unclear how different scientific practitioners might engage in co-operative experiment, i.e. how they might organise their observation in such a way that their findings are communicable to one another. The issue is not dissimilar to the sorts of concerns within the classical empiricist tradition about the underdetermined relationship between experience and, as Russell put it, a world of "public neutral objects". (Russell [1912/1997], 20-21) While it is certainly no empiricist's duty to offer a proof of the external world, any empiricism that accounts for the rationality of our common sense beliefs must have something to say about the ways in which we come to assess empirical judgments about ordinary states of affair in our environment, and how such an assessment informs (and is informed by) common sense and scientific discourse. Gupta does not explicate how such an account may proceed. We will explore some of the ways Gupta might respond to this problem.

In §IV, we will offer a slight revision of Gupta's system, which, in light of the historical taxonomy of the previous chapter, will argue that all admissible initial views must be able to formulate, with the right experiences of course, a set of ordinary empirical judgments about magnitude, position, orientation, and displacement. Such judgments, while they do not force any substantive account of geometry, presuppose a revisable public space of interaction. We call the principle which governs admissibility in this way the Principle of Scientific Empiricism. We will then explore whether or not this is too substantive a principle, and whether or not an account that imposes it as an admissibility criterion on initial views can really

be considered "empiricist". We will argue that it does not upset the Insight, Multiple-Factorizability, or any of the constraints outlined in Chapter One, so that if these principles and constraints really do capture the essence of normative empiricism, then our revision of Gupta's empiricism is also empiricist. While no formal equiconsistency proof will be offered, the dependence between the system of principles and constraints offered by Gupta and his system plus the Principle of Scientific Empiricism will be shown. In fact, it may be that our Principle of Scientific Empiricism, which demands that all initial views possess at least a primitive, weak, and revisable conception of public, objective space, is nothing more than a particular characterisation of Gupta's initial condition that no *solipsistic* views be allowed (because of their pathological nature) as starting points in a revision process, though characterising the Principle of Scientific Empiricism as such is not necessary for its defense.

§I: The Role of Reason in Gupta's Empiricism

Critiques of Gupta's empiricism have largely been concerned with three aspects of his work. The first set of critiques is concerned with how it can be that convergence of views delivers us a true conception of the self and world. These criticisms come in two varieties. The first variety claims that even if a set of disparate initial views do converge on the same view in light of a series of experiences *E*, such convergence fails to deliver us the *uniquely true* and non-circuitously justified conception of the self and world we want (Martínez Fernández [2009]; Valor Abad [2009]). Call such criticisms of convergence "strong convergence criticisms", since they hold that even if convergence does occur, it does not grant any epistemic entitlements. The second variety of critiques of convergence argues that that there is no way to block (on *a priori* grounds) a set of initial views which are solipsistic or sceptical. (Neta [2009]; Berker [2011]; Schafer [2011]) Call such critiques "weak convergence criticisms", since they do not call into question the entitlements that convergence would grant the epistemic agent were convergence actually to occur—they merely question the

possibility of such an occurrence given the rationality and admissibility of (at least some modified) solipsistic and sceptical initial views.

Secondly, there are critiques that focus on alleged infelicities with one or more of Gupta's constraints, usually with reference to an alternative account of the rationality of our perceptual judgments. Call such critiques "architectonic criticisms", since they argue against the very formulation of Gupta's empiricism relative to the constraints.

Thirdly and lastly, there are a group of criticisms that accept Gupta's constraints and his account of convergence, but argue that the system itself is not faithful to empiricism. This family of critiques questions the substantial role for rationalism within Reformed Empiricism. Call these the criticisms "rationalism criticisms".

I.1 Weak Convergence Criticisms

We begin this subsection with an examination of Neta's [2009] critique of Gupta's notion of convergence. Neta argues that Reformed Empiricism cannot adequately ground Gupta's notion of unconditional rational entitlements because there is a class of sceptical positions that will always upset convergence. Here is Gupta's definition of convergence:

> *V* and *V' converge* iff there is a stage *n* [of the revision process] such that, for all stages m ≥ n, V_m is virtually identical to V'_m – that is, $V_m \approx V'_m$; the least such *n* will be called the *convergence point* of *V* and *V'*. (Gupta [2006a], 93)

Convergence is thought to give us unconditional entitlement/obligation if the empiricist can (i) "formulate and justify specific requirements of admissibility on [initial] views" and (ii) "maintain that views that meet these requirements will converge under the force of experience." (Gupta [2006a], 160) Neta argues that it is impossible to maintain (ii) in light of the failure of (i), and Gupta has not come up with adequate admissibility requirements that will block the relevant class of solipsistic and sceptical views. In short, not all admissible views will converge, and

therefore convergence cannot be taken to provide us with a core of judgments to which we have unconditional epistemic entitlements. This is because not all solipsistic and sceptical views are *pathological*.

Recall that Gupta rejects solipsism and scepticism as initial starting points for revision since, he argues, both are thought to be rigid and insensitive: no matter what the course of experience, such views will not change in any substantial way. Nothing can convince the solipsist that the world she sees is anything but a creature of her own mind; nothing can convince the sceptic that there is a real world acting on her senses. However, Neta argues that Gupta has too narrowly defined solipsism and scepticism for his purposes. Solipsistic and sceptical views come in a variety of forms, and some are not insensitive to empirical revision based on experience. Take a classical solipsistic view. According to this view, the world is nothing except what exists in my own conscious experience. There are no external objects, no other persons, and no other minds: only phantasms of my own imagination (or some other mental faculty). Call this view S. By Gupta's standards, it is clear that no experience (or chain of experiences) can upset S. It is rigid, and therefore inadmissible as a starting point for a revision process, since, no matter what the sequence of experience, all views in the revision process will be fundamentally similar to one another. But, Neta argues, the solipsist may modify her position. He gives the following two possible modifications, S' and S'' (Neta [2009], 485-486):

> Let S' be a view that says the following: if I have a visual experience of a particular shade of orange uninterrupted for precisely 10 seconds, then there is an omnipresent, sempiternal divine being, and otherwise there is nothing but my own experiences.
>
> Let S'' be a view that says the following: if I have a visual experience of a particular shade of orange uninterrupted for 10 seconds, then there are things distinct from my experiences that are causing me to have those experiences; if I have a visual experience of a particular shade of orange uninterrupted for 20 seconds, then the only things that exist are my present experiences; and otherwise, there is no basis for deciding between these two possibilities.

Both S' and S'' are revisable in light of experience, and therefore count as admissible views. However, their existence now ensures that not all antecedently rational views will converge given a sequence of experiences. This puts in jeopardy the convergence of all possible initial views given a series of normal experiences *E*, and thus calls into question Gupta's move from the conditional entitlements of any view whatsoever to the unconditional entitlements licensed by convergence. Convergence fails, and so does Reformed Empiricism: experience does not privilege any view as unconditionally rational.

Neta's criticisms are strong, for they put Gupta into something of a dilemma. Either Reformed Empiricism admits of modified solipsistic views, making categorical judgments impossible, or it blocks such modifications. If Gupta chooses the first horn of the dilemma, then Reformed Empiricism only gives us hypothetical entitlements, not knowledge; if he chooses the second, then Reformed Empiricism seems arbitrary, since there are no principled grounds for preventing such views from entering into possible revision processes as initial views. Neta has been careful to show that S' and S'' are sensitive to experience, that experience can occasion a change in view with the consequence that the view after experience *e* (the 10 second sensation of orange) is fundamentally different from the view before *e.* Reformed Empiricism is in the unenviable position of not being able to arrive at unconditional entitlements without some empirically unmotivated decision to make such views inadmissible.

But have the modified solipsist views S' and S'' shown what Neta thinks they have shown? Gupta thinks not. In his [2009b], Gupta argues that Neta shows S' and S'' to be non-rigid (sensitive to experience); however the views are still *non-receptive*. A view is receptive only when it yields different perceptual judgments when experiences are subjectively distinct. S' and S'' are non-receptive since there are still a large class of subjectively distinct experiences which do not yield substantially different judgments. Gupta can block any non-receptive views as inadmissible, therefore convergence is not jeopardised by S' and S'', unless Neta has some independent problem with the receptivity admissibility criterion. Further,

these sorts of view are more than merely "silly" (as Neta grants) they are actually incoherent, and Gupta holds that internal coherence is also an admissibility criterion. That S' and S'' are incoherent becomes apparent when we note that they both fail to provide an epistemic means of individuating temporal experiences of the right shade of orange and other experiences. Such a view can provide no reason to think that, in some instances, *this* shade of orange has such and such epistemic consequences, while slightly different shades of orange (or even different colours) do not, and why 20 second bursts of orange are epistemically so different from 19.9 second bursts.

It is clear that Gupta will not accept any view, therefore, which does not respect *Manifestation*. Neta's examples of S' and S'' fail in this regard, for they do not respect the phenomenological character of the given; views such as these are equivalent to assigning some propositional content to a sensation on purely arbitrary grounds. They are held together by fiat, not by internal coherence. Both S' and S'' offer no distinction as to why sensations of colour are sometimes epistemically weighty, and other times not, so they lead to a set of judgments of the following sort (using S' as our basic view): "Colour sensations of orange are both grounds and not grounds for believing in a deity." But the view can offer no reasons as to how or why such differences depend on the subjective character of the given. The coherence of a view which thinks sensation of a particular shade of orange for 10 seconds obliges one to believe in a deity, while the same sensations for 9.9 seconds oblige one to its contradictory, and which do not provide any reason why the slight difference in time-interval would matter, are incoherent not because small phenomenological differences can't make big epistemological differences; lots of times, especially in the sciences, the smallest of phenomenological differences can lead to the development of new theories and views. (One need only think of one of countless examples, perhaps Brownian motion.) However, there are reasons offered by the view. In such a case as S', we would need to develop a theory of why the existence of a deity (or at least belief in the existence of a deity) should find some functional correspondence with phenomenal experiences of colour, and also why

minute differences in shade of orange and minute differences in duration of sensation would correspond to such big differences in epistemic entitlement and obligation.

If we start to flesh out these functional dependencies, S' (and by parity of reasoning, S'' and all such "silly" views) will either be incoherent (and inadmissible), or they will start to be robust enough to converge with non-solipsistic views in regards to judgments about a whole host of matters. Neta's critical examples only work because they show so little content that they do not, on their face, seem incoherent, nor do they seem robust enough to ever oblige the view holder to move beyond the solipsistic view. When put in the context of other central parts of a view, including beliefs about the differences between colour sensations, the related beliefs one may have about deities and their natures, as well as our abilities to know them based on sensation, and a whole host of other *framework beliefs*, S' becomes either incoherent or plausibly capable of convergence.

I.2 Strong Convergence Criticisms

Strong convergence criticisms argue against Gupta's notion of convergence on purely logical grounds. They argue that even if Gupta establishes (i) and (ii), convergence does not bring with it rational entitlement or obligation. It is simply not up to the task. This is because there is a much more severe sceptical worry Gupta has not addressed: scepticism about the epistemic entitlement one has to perceptual judgments $\Gamma_e(v)$ even if we can establish the truth of v and can establish the rational connection between v, e, and $\Gamma_e(v)$. This is because the rational entitlement to $\Gamma_e(v)$ is licensed only when the Insight is established, but, says Valor Abad, there is no empirically rational support for the Insight that is non-circular. All proofs of the Insight presuppose the Insight. Valor Abad argues that Gupta's account fails to provide a justification for the claim that the given has normative weight—that Γ_e imposes rational constraints on us no matter what we may say about some particular view which it takes as argument. He argues that any attempt to show that experience imposes upon us a rational constraint will presuppose the very fact. His

comparison is with the circularity of justification for the rational force of *modus ponens*:

> The modus ponens (MP) schema – like any other valid argument schema – seems to impose rational constraints on us. If S holds A and A → B, S must accept B. According to Gupta, something similar occurs with Γ_e, the given in *e*: If S holds a view *v* while having an experience *e*, S must accept the propositions in $\Gamma_e(v)$. But, do MP and Γ_e really impose rational constraints on us? Why is it reasonable or normatively compelling to follow MP and Γ_e? We could say that all instances of MP are valid and that all conditionals of the form 'If S holds *v* while suffering *e*, then S must accept Γ_e (*v*)' are true, but the worry still remains: how can we establish this? In the first case, we would need to argue for MP without using or assuming MP, otherwise we would fall into a vicious circle. Unfortunately, it seems impossible to argue in favour of MP without assuming it at some point. (Valor Abad [2009], 326)

We have two options: if we assume a transcendental argument that shows the normativity of the given, then we have transgressed the Insight. If we turn to experience, then we merely assume the Insight. The Insight is either false, or viciously circular; "it seems impossible to avoid this dilemma in justifying the normativity of Γ_e." (Ibid.)

However, Valor Abad has missed the dialectic in which Gupta is involving himself. The sceptical standard here is rather high; if we allow that it is reasonable for the sceptic to call into question patterns of logical inference because they are merely axiomatic, and cannot be supported by non-circular justifications, then we have simply given the sceptic too much. The Humean sceptic who asks how we think we know *p* when the logical connections between what we actually know and *p* are tenuous, poses a concern that needs to be addressed; the sceptic who tells us that the Insight is unjustified need not be addressed for two reasons. Firstly, Gupta proposes the Insight as a *truism*, which, if we develop our account of perceptual knowledge correctly, can be *preserved*; he nowhere speaks of the Insight as something in need of proof, but rather something that is central to, or constitutive of, empiricism. Secondly, the Insight need not be given in experience to be known true. The old dichotomy that every proposition we hold true is either verified by

some experience or is itself a logical truth is a central part of the classical empiricist doctrine that Gupta seeks to replace. The Insight is rational because of its central role in empiricism—because a consistent form of empiricism can be established which preserves it. Valor Abad has the significance of the analogy with *modus ponens* wrong. He should argue that *just as modus ponens* is rational in spite of the circularity of explicit justifications of it, the Insight is rational in spite of the circularity of explicit justifications of it. What this shows is that the Insight, like *modus ponens*, is not something for which it is rational to demand the sort of proof that would quiet the sceptic.

This last point should make clear one of the key distinctions between Gupta's Reformed Empiricism and its classical predecessors, at least its CBE predecessors. While previous empiricists held that all of our knowledge should be based on, grounded in, or verified by experience, Reformed Empiricism is committed to no such absolute claim. Gupta holds rather that it can be shown that an agent's belief set (or, more generally, her *view*) can be rendered rational by some sequence of experiences. This means that Reformed Empiricism need not avail itself of anything like a classical principle of verification, and all of the technical and conceptual problems associated with our knowledge of said principle. Valor Abad seems to be confusing the Insight with such a basic principle (indeed, his characterisation of it as "basic" and as a "principle" betrays his confusion), but this is to confuse the entirety of the project with the sorts of problematic empiricisms to which Gupta is offering an alternative. Gupta's empiricism neither takes the bald sceptic seriously, nor does he construct an account of experience as offering a verification of every belief, as if we may turn to some particular experience (or set of experiences) as the justification for, and meaning of, each belief/judgment in our view. The mistake is to think that just because we cannot do this for some particular belief (even one so central as the Insight), that the belief is not rendered rational (as part of a belief set or view) by experience as our principal epistemic authority and guide. It is to (a) not think of experience as functioning holistically, and (b) not think of justification via experience as something that is dynamical and diachronic. The Insight is not a "basic

principle" at all, and it is certainly (as Gupta would admit) not the sort of thing that can be "grounded" in the classical sense—but no belief/claim can be grounded in the classical sense according to Gupta, so this should come as no surprise.

I.3: Architectonic Criticisms

A second set of criticisms focuses on the constraints for normative empiricism which Gupta takes as guiding the development of his Reformed Empiricism. John McDowell, for example, has argued that Gupta offers us no reason to adopt the *Equivalence* constraint, since it is entirely possible that subjectively identical experiences can have differing content. In fact, this is a central tenet of McDowell's disjunctivism. For McDowell, we begin by accepting that experience can rationally and normatively support perceptual beliefs. In order to maintain this position, we must divorce our account of experience from mere sensibility as it has been traditionally construed—that is, experience as non-conceptual. Experience is, according to McDowell, our principal epistemic authority and guide, but only when construed as involving conceptual capacities that one normally associates (to use the Kantian vocabulary) with the *understanding*, not *receptivity*. Conceptual capacities belonging to a faculty of reason (McDowell [2008], 2) operate in experience itself.[51] As McDowell once put it, "we can coherently credit experiences with rational relations to judgment and belief… only if we take it that spontaneity is already implicated in receptivity; that is, only if we take it that experiences have conceptual content." (McDowell [1996], 162) These concepts are the very concepts that operate in judgments also.

Such a view can only remain tenable if one argues that there are two kinds of experiences: ones which represent the world thus and so, and ones which only *seem* to represent the world thus and so. The difference is between veridical perception and real content, and a mere *seeming* content in the cases of non-veridical perception. This disjunctivism only becomes tenable if one rejects *Equivalence*, since

[51] McDowell once held the view that experience, already conceptualised, provided one with all of the *propositional* content required to account (non-inferentially) for a subject's experiential knowledge. McDowell now rejects this view. See his [2008].

subjectively identical experiences may not have the same rational contents, though, according to McDowell, they can both make a subject's belief (that the world is thus and so) rational. For McDowell, the mental states which accompany veridical and non-veridical perceptions differ because of their connections to the world—this in spite of the experiences themselves being indistinguishable.

Gupta asks the following question: "… how do we account for the fact that in both cases, veridical and illusory, the subject is equally rational in her belief [that *p*]?" (Gupta [2009b], 491) McDowell grants that both veridical and illusory experiences are rational (McDowell [1009], 470), but given that he has granted this, Gupta asks "how do we move… from seeming givenness to actual rationality?" (Gupta [2009b], 491) In other words, what is it about *seeming* that provides the epistemic agent with a different proposition/conceptual content than the one she is provided with in veridical cases? Either McDowell has an account, in which case he will need to avail himself of *Equivalence* (perhaps the equivalence of propositional/conceptual contents), or he denies that there is anything similar given in experience in veridical and illusory cases, in which case he fails to account for how they both entitle an epistemic agent to a set of judgments. Recall that Gupta characterises *Equivalence* in just this way: if *e* and *e'* are subjectively identical, then the given in *e* is identical to the given and *e'* (where the given is merely the rational contribution of experience to knowledge). McDowell may want to define the given in more robust terms, but it remains to be seen why he wants to do so, especially given that he already yields the point that what really matters to the rationality of experience is how it *seems* to the epistemic agent.

Further, even though McDowell's brand of disjunctivism may preserve the Insight, Multiple-Factorizability is jeopardised by his account, giving us independent grounds on which to find his disjunctivism unattractive. Like Sellars, McDowell believes that the concepts in our language are what allow us to have the sorts of perceptual judgments we do, i.e. these concepts allow us to take the content we do from experience, which is some kind of conceptual (if not propositional) content. Thus, we have privileged a set of inherited concepts, but it seems we have done so

for no good reason. We have given experience its rational weight, but either (a) we argue, with Kant, that we must remain with this set of concepts come what may or (b) we argue, with Sellars, that changes in our concepts occur, but how these changes are attached to experience is not clear. If we pursue (a), we do not preserve Multiple-Factorizability. If we pursue (b), we do great damage to the Insight, damage that McDowell thought could be avoided by his disjunctivism, for experience is no longer the principal epistemic authority regarding the development of our concepts. Sellars makes no claim that experience plays such a role, but, McDowell is very much committed to the idea that experience offers rational support for our beliefs, even though his disjunctivism seems unable to preserve it.

Perhaps the view can be salvaged so as to do justice to the truisms, or perhaps Multiple-Factorizability can be shown to be flawed in some way, and not worthy of its lofty truism status. This is not our concern, for even if the truisms are honored, McDowell's position runs aground on *Reliability*. Take his example of non-inferentially seeing a cardinal. (McDowell [2008], §4) While he admits that not all people have the conceptual capacity to make perceptual judgments of the sort, "That's a cardinal," many do, and the CARDINAL concept is operating at the level of experience itself. (Here we see the spontaneity in receptivity mentioned earlier.) But now, experience can yield (for such a person) a conceptual content that is false (if propositional or proposition-like) or erroneous (if merely a conceptual content). *Reliability* captures the fact that what we would like to say is that experience isn't to blame in such cases (for example, if I am seeing a Blue Jay in poor lighting conditions); what is to blame is my view of the world. However, according to McDowell, the concepts of the understanding (for beings like us) inextricably operate in experience; experience takes on all of the many faults that can be found in our view of the world and our position in it, and the given therein can, as a result, yield false propositions (or erroneous contents). Of course, McDowell is always free to take recourse in his bifurcation between the given in veridical experience versus the given in illusory experience. However, such a move, in light of what we have just seen in the last paragraph, is illicit. There is no reason to reject Gupta's *Equivalence*

constraint, and therefore no way for McDowell's disjunctivism to preserve *Reliability*.

Further, there are no grounds to accept any of the other constraints if one adopts McDowell's disjunctivism: *Existence* is rejected since in illusory cases, nothing is given in experience; *Equivalence* and *Reliability* are rejected for the reasons given above; finally, *Manifestation* is rejected, since the given does not systematically depend on the subjective character of experience, but on the connections between some experiences (veridical ones) and the world. McDowell is thus merely speaking past Gupta; the two have very different notions of the given, but Gupta's seems to better accord with the Insight and Multiple-Factorizability, while McDowell's seems to only accord with his disjunctivism. In any case, Gupta is free to set up any constraints he likes. The strength of the view is that it accords with a variety of constraints that empiricists have hitherto taken to be true. Gupta wants to show that he can develop a defensible empiricism that preserves the plausibility of these constraints without falling into the propositional given and Cartesian conceptions of experience. McDowell has also missed the dialectic of the work if he thinks anything else is at stake.

Another architectonic critique comes from Peacocke [2009]. Peacocke argues that Gupta has not adequately characterised his *Reliability* constraint.[52] According to Gupta, experience never yields anything false or erroneous. Where there is something wrong about our perceptual judgments, it is not experience that is to blame, but some defect in our view. Peacocke says that

> …this position is false to the phenomenology, to the epistemic status, and to the metaphysics of experience. The content of apparently misleading experiences cannot be attributed to perceptual judgements or beliefs, because in some cases the subject knows he is experiencing an illusion; yet the experience still has the false content. (Peacocke [2009], 477)

Peacocke's example of the Müller-Lyer illusion makes the point more salient. In this case, we know that the lines are of the same length, yet this does not prevent

[52] Schafer [2011] also offers a critique of the Reliability constraint, though we will not address it here.

experience from offering representational content to the contrary. Here, it would seem, our view is perfectly well established and it is experience that continues to impart false or erroneous contents.

Further, if Gupta's view is adopted, then there is no sense in which experiential contents can ever be taken at "face-value", to use Peacocke's term. Undergoing an experience would be divorced from having the world presented to you as being a certain way. All of my judgments to the effect that the world is a certain way would either be "an irrational leap in the dark, or something inferential." (Peacocke [2009], 478) Gupta will have lost the rational connection between experience and judgment. However, here Peacocke is mistaken. He construes the hypothetical given as requiring some inference to impart its contents. This is not the case. There is, for Gupta, spontaneity of experience. Given a view *v*, experience operates as a function, which takes us (immediately, intuitively, naturally, and spontaneously) to a judgment. It is not as if we ever encounter the neutrality of the given in experience itself (say in accordance with Husserlian phenomenological *epoché*) in spite of its logical categorisation. We experience the world as thus-and-so immediately in experience, but this does not mean that the given in experience does the conceptual heavy lifting. Thus, the hypothetical given does not put in danger the phenomenology of experience, nor its epistemic status. A hypothetical given is perfectly consistent with phenomenology, especially when we take into account Gupta's explicit inclusion of the *Manifestation* constraint. (As to being true to the metaphysical nature of experience, Gupta is wisely silent.)

But even if experience is spontaneous, Reformed Empiricism offers no account of how transitions from one view to another in light of experience can be rational. Surely there has been a change in our mentality (our belief system at the very least) in so far as two views, *v* and *v**, are not the same. And certainly this seems to be a result of the intervening experience *e.* But what makes such a change in view *rational*? Peacocke introduces the "Ratifiability Condition" which must be met if such transitions are thought to be rational:

> *Ratifiability*: Whenever a mental transition is rational, there is a condition of soundness that it meets. This soundness condition involves the notion of truth, and it is a condition that concerns the correctness or fulfillment of the contents of one or more of the mental states involved in the transition. (Peacocke [2009], 478-479)

But this notion of truth presupposes a more robust account of content than Gupta is willing to countenance in his hypothetical account of the given. It presupposes that at least some experiences have a "face-value" content, and that such contents can be true: what Peacocke calls "content as required to elucidate rationality". Without it, we are left with no way of ensuring that the transition is rational, since no step in the transition has a content that is true. Gupta, after all, likens his hypothetical model of the given to an argument schema, which takes us from a view to a proposition (or set of propositions). But, unlike the example of *modus ponens*, which is a valid inference form, and which therefore has truth-evaluable premises (and thus is able to be ratified—it is truth preserving), the transition from one view to another based on a non-propositional given cannot be ratified. (Similar arguments have been made by Berker [2011], who argues that you can only get categorical entitlements out of revision if categorical entitlements are put into the revision process; otherwise, there is no way to secure the rationality of the move from the hypothetical to the categorical.)

It seems that Peacocke's analysis has not taken full account of the logical reforms at the centre of Reformed Empiricism. Reformed Empiricism does not show how our conception of the self and world can be considered true, even in the face of the harshest sceptical criticism; any empiricism that accepts this task is playing a fool's game. Reformed Empiricism is an attempt to show how, using only empiricist principles, one might justify her current set of beliefs, exhibiting that the set is by and large *rational* because she has met certain reasonable conditions for thinking so—because her view has converged with other very different views, perhaps those of her community members, as a result of experience. It answers the sceptic, but only after the sceptic has been neutralised. It shows the sceptic that experience can rationally force a change in view, and asks the sceptic to now formulate her doctrine

in light of this epistemic fact. Gupta is not concerned with offering an account of how an individual shift in view, irrespective of the possible convergence of all views in light of experiences *E*, might satisfy *Ratifiability*. Gupta holds that the rationality of any shift in view is established by its ability to converge with other views, no matter how radically different they may have been at the beginning of inquiry (so long as admissibility criteria have been met). Of course, there are no guarantees that convergence will occur, but only relative to even merely possible or idealised convergence can we "ratify" shifts in view. Peacocke's criticism does not set Reformed Empiricism in this broader perspective, and thus loses some of its sting as a result, for it is in this broader perspective that Gupta's account of ratification is formulated: as dependent on the essentially dynamical character of view revision. This is why Gupta can admit that there is perhaps no individual stage of a revision process that will meet *Ratifiability*, but this does not prevent revision prcoess from being ratifiably rational.

I.4: Rationalism Critiques

The third and most interesting family of criticisms challenge Gupta on what they take to be the overly rationalist assumptions in his position, and, more often than not, focus their attention on Gupta's admissibility criteria glossed in §I.1 above. The argument takes the form of those expressed by Neta and Valor Abad, though rationalism critiques follow the reasoning exhibited in those critiques to its natural end: Gupta's reforms are merely rationalism in empiricism's clothing. Berker [2011] and Schafer [2011] both argue for this thesis, though in slightly different ways. In this subsection, we will explore their criticisms, and then show how such criticisms miss the mark: Gupta's empiricism deserves to go by that name. Nonetheless, such criticisms have hit upon, if only obliquely, an underlying deficiency in Gupta's account of the antecedent rationality of initial views and the set of admissibility conditions which we may need to expand upon to solve a certain class of solipsistic views from upsetting convergence. In sections 2-4 below, we will propose a way that we may enrich Reformed Empiricism's conception of what counts as a

pathological view: its limited set of conditions, including receptivity, internal coherence, and non-rigidity, are not jointly sufficient to block all forms of solipsism. For now, however, we will focus on the charge that Gupta's admissibility criteria are too rationalistic, and that they betray his empiricism.[53]

Berker argues that Gupta has not done an adequate job dispelling the solipsist or the sceptic. He introduces a bit of formalism to help him develop the point. Take the set of propositions possessed by all convergent views after being revised in light of any given finite sequence of experiences *E*:

$$\Omega_E = \{P : (\forall \text{ admissible view } v)\ (P \in \rho_E(v))\}$$

where $\rho_E(v)$ is the convergence point for all admissible views given *E*. The set Ω_E contains all of those propositions upon which all of the admissible views will come to agree given *E*, so Berker calls it "the common core of admissible outcomes generated by *E*". (Berker [2011], 24) Any proposition that is a member of Ω_E is one to which we have unconditional entitlement. Of course, we can only preserve the viability of Ω_E if we preclude from our set of starting points those views that put such convergence in question. This, as we have already seen, must be done on some principled ground that is acceptable to empiricism, i.e. must preclude unacceptable views as initial starting points in revision by some relatively innocent admissibility criteria. Gupta, as we know from our concluding remarks in the previous subsection, has three such criteria: receptivity, internal coherence, and non-rigidity. Utilising these criteria, Gupta can block the sorts of solipsistic and sceptical views which put in danger the viability of Ω_E as a common core of propositions, and thus put in danger the set of propositions to which we have unconditional entitlements. The

[53] For a distinction between rationalist and empiricist *a priori* commitments, see Gupta [2009b], 334: "The general 'revision-and-convergence picture' is neutral, it should be observed, on the debate between the empiricists and the rationalists…Both parties can accept the general picture; their disagreement centers on requirements of admissibility. Rationalists will argue that we know a priori some substantive truths about the world; that these truths constrain admissibility; and that without this additional constraint, we cannot make sense of empirical rationality. Empiricists, on the other hand, will deny that we have any a priori insight into the nature of the world. They will insist that all admissibility constraints must be grounded solely in epistemological considerations (such as those motivating non-rigidity)."

project of giving an empiricist account of the rationality of our common sense view of the world would be completed: based only on the hypothetical given we would be entitled to a set of categorical judgments.

But Berker is not convinced. He argues that Gupta's notion of experience may not actually respect the Insight.

> Gupta has certainly provided an account of categorical justification in which experience plays an important role. However, in order to vindicate empiricism we don't just need experience to play some role in the justification of our beliefs; it is difficult to see how any plausible account of justification, whether rationalist or empiricist, could deny that. Rather, what we need is for experience to play an exclusive or primary role in the justification of our beliefs. And it remains to be seen whether experience bears the brunt of the normative work in Gupta's proposal. In particular, we need to ask whether there are places in Gupta's account where reason (rather than experience on its own) makes a substantial contribution to the justification that we have for our beliefs. (Berker [2011], 26)

Berker argues that reason plays three substantial roles in Gupta's Reformed Empiricism, and only one of these roles is recognised by Gupta. Firstly, there is the *a priori* use of reason to discern the admissible from the non-admissible views. (This is the use of reason that Gupta does acknowledge, and I will not address it here.) Secondly and thirdly, Gupta uses reason to demarcate the contours of the $\Gamma_e(v)$ and $\rho_E(v)$ functions respectively.

For Berker, Gupta must use reason to determine *a priori* the proper extension of the $\Gamma_e(v)$ function. In fact, Berker calls such statements about the extension of $\Gamma_e(v)$ synthetic *a priori*. The charge is strong indeed, for Gupta needs to prevent his hypothetical account of the given, and the interdependence of experience and view, from suffering the pitfalls of Kant's similar project. This is only possible if Reformed Empiricism can generate, using only experience and view, the sorts of judgments about the self and the world that were thought to require a transcendental source in Kant's system. If Berker's criticism is apt, then Gupta too has proposed an account of the rationality of experience that presupposes too significant a role for reason. Assume, with Gupta, that the first use of reason (to

determine which views are admissible and which are not via convergence) is legitimate. Even so, convergence will not help us determine the proper extension of $\Gamma_e(v)$. Because convergence requires specific values for the $\Gamma_e(v)$ function, this criticism comes prior to convergence. But, having precluded convergence as a useful tool to establish the contours of the $\Gamma_e(v)$ function, it seems that there is no way for Gupta to prevent someone from embedding into the $\Gamma_e(v)$ function any theory of the given they so choose, even a propositional account of the given. This is because we can use the two-argument function $\Gamma_e(v)$ (which takes experiences and views and maps them onto perceptual judgments) to construct a one-argument function from experiences to classes of propositions (as with sense-datum theory). Berker argues that Gupta can only rule out such a possibility through some illicit use of reason. Gupta can try to take refuge in the constraints, but he can only determine whether the constraints hold via some illicit use of reason too.

Reason is also operating to determine the extension of the $\rho_E(v)$ function. Berker asks us to suppose a rather simple case of revision: a rational agent with view v undergoes experience e, and $Q \in \Gamma_e(v)$. (Berker is here assuming for the purposes of argument that $\Gamma_e(v)$ has been unproblematically fixed, *contra* his previous criticism.) Perhaps Q merely needs to be added to v to generate some new view v^*, but, the more interesting cases are when v^* is a substantial revision of v. How are we to determine when a substantial revision is to occur, and how that substantial revision should occur? In other words, how can we preclude the possibility that one can begin her epistemic life with a completely common sense view, and, in light of some experience, simply revise her position to a solipsistic or sceptical one? Either Gupta offers no principled reason why such revisions ought to be precluded, in which case even the initial blocking of rigid views does not save the day against solipsism or scepticism, or Gupta does offer principled reasons, but of an illicit and rationalist sort (since no appeal to convergence can prevent such a possibility, given that such possibilities do irreparable harm to the epistemic force of convergence).

Before examining Berker's analysis of the $\Gamma_e(v)$ function (which we shall do

after examining similarities between his critique and Shafer's critique), let us pose a problem for his analysis of $\rho_E(v)$. It is unclear why Gupta must be in a position to block the possibility of a given view being revised in light of a given experience into a radically different, even sceptical view, because it is unclear why this poses a problem for said sceptical view, so arrived at, from partaking in the categorical judgments contained in Ω. This is because it seems impossible for a severe form of scepticism to be formulated in light of experience, and this is independent of any admissibility constraint whatsoever. The sceptical view in question, no matter what sort of lunacy it may profess, has built into itself a rational role for experience based on the revision process (here a rather short one, containing only one experience and two views, one input and one output). Imagine agent *A* possessing initial view v_0. Pretend that this view is similar in nature to our normal common sense picture of the world. Now imagine *A* undergoes experience e_1, which (*ex hypothesi*) leads her to revise v_0 into a harsh Pyrrhonian "view" of the world (in as much as a Pyrrhonian sceptic can have a *view* of the *world*)—call it v_p. This view includes the minimal sceptical commitment to *Diaphônia* (the mode from disagreement), including the accompanying belief that one should suspend all belief. (Allow this in itself to be a consistent position for the sake of argument.) However, the rationality of view v_p rests on the revision process from v_0 in light of the rational contribution of e_1. Thus the holder of v_p either admits (a) that experience can rationally motivate a change in view (calling into question the coherence of the view, in which case it should be rejected by *A*, whom we assume rational), or (b) *A* modifies her scepticism so that it is a milder form of scepticism, allowing for at least the occasional rational revision in light of experience. But now what exactly is so scary about such a view, and why is it necessarily the case that v_p does not partake in Ω at $\rho_E(v)$? The only condition operating on revision in this case, which Gupta does not make explicit, is this: no revision from a coherent view to an incoherent view can be licensed by experience. (This is more or less a corollary to the *Reliability* constraint, since we must blame our view of the world, not experience, if we find ourselves involved in incoherence.)

Before we look at a rely to Berker's criticism that Gupta has no non-

rationalist way of delineating the contours of the $\Gamma_e(v)$ function, we should pause to looks at the ways in which the charge of rationalism is developed by another critic: Schafer. Schafer [2011] also argues that Reformed Empiricism appeals to reason in ways that betray the Insight. He argues that the admissibility constraints placed on initial views "seem to give us a basis for at least some deeply contingent *a priori* knowledge" (§3), specifically the substantive *a priori* knowledge that none of these inadmissible views obtains (i.e. is antecedentally rational). For such reasons, we should think of Gupta's project as more indebted to the Kantian tradition than the empiricist tradition:

> … Perhaps the real lesson for someone like Gupta to draw from these concerns is that empiricism is only sustainable when combined with a substantial degree of rationalism. For perhaps we can only do justice to the empiricist's ideal of experience as the highest epistemic authority by accepting a set of a priori constraints that amount to a tacit endorsement of a fairly robust form of rationalism. If so, it might be best to regard Gupta's response to skepticism as most similar, not to traditional forms of empiricism, but instead to the sort of combination of empiricism and rationalism that we encounter in work in the Kantian tradition.

This, according to Schafer, is for two reasons. Like Kant, Gupta thinks the only successful response to the sceptic accepts constraints that are both empirical and rational in nature. Secondly, if Gupta is a rationalist (which Schafer thinks he must be), it is clear he is of a Kantian sort. As Schafer recognises, theoretical reason in and of itself can grant us no positive view of the world, no *a priori* insight into the structure of reality: a commitment Gupta shares with Kant.

According to Berker and Schafer, we may grant that Gupta offers an adequate response to the sceptic, and that his system gives experience an important and ineliminable epistemic role. However, the system only works because he avails himself, either explicitly or implicitly, of certain illicit rationalist elements. His view is novel, though not empiricist. However, there are reasons to doubt such a criticism. Let us begin with Schafer's main point, that Gupta commits himself to a Kantian sort of view by holding that reason can adjudicate between admissible and inadmissible

views prior to experience. As we saw, Schafer thinks this amounts to *deeply contingent* knowledge regarding which views do or do not obtain. However, this is not the case. Gupta offers no grounds on which an internally coherent view can be rejected as an inaccurate view of the world. As he is at pains to show, we *can* arrive at solipsistic or sceptical views of the world as the result of experience; the prohibitions on solipsism, scepticism, or any other rigid view only have jurisdiction at the initial stage of a revision process:

> … the rigidity constraint rules out solipsism as a starting point of revision. But this leaves it completely open whether the revision of a view—even an admissible view—can result in solipsism. So admissibility constraints do not yield a priori knowledge that solipsism is false. They do not even yield a priori directives on belief, e.g., that one ought to believe that solipsism is false. (Gupta [2011], 49)

Also, as Gupta argues, the affinities between his view and Kant's should not be over-stated. Critically, Kant's "empiricism" (if we may call it that) argues that experience is a product of the understanding and sensibility. For Gupta, experience is (to use the Kantian terminology) pure receptivity. (Gupta [2011], 50) While Gupta, Kant, and Neo-Kantians all agree that mere receptivity is not enough to grant one epistemic license to a set of claims, they disagree about whether this fact precludes experience from being mere receptivity. Gupta's functional account of the given makes this clear.

Gupta's response to Berker is less straightforward. Gupta argues that his $\Gamma_e(v)$ function is fully determined by its arguments (experience and view). As he makes clear, views are more than just collections of judgments or beliefs; a view contains not only a set of judgments, but "also links between experiences and perceptual judgments: the view determines how the subject is to respond to an experience." (Gupta [2001], 49) Berker's criticism that one can represent (or even reconstruct) the dreaded sense-datum model of experience by fixing the extension of $\Gamma_e(v)$ as a one argument function that maps experience to perceptual judgment is not troubling, since we can also fix the extension of $\Gamma_e(v)$ as a two-variable function which takes us from sense-datum theorists' view of the self as argument to a class of

perceptual judgments. While Gupta is not so explicit in his own defense, it is clear that he does not want to preclude the formulation of the sense-datum view. That would be unwarranted. He wants to show that there is no reason why one should view the extension of $\Gamma_e(v)$ as a one-variable function, and no reason why sense-datum theory should not be viewed as merely one particular view which experience can map onto a set of perceptual judgments. There is nothing antecedently wrong with the sense-datum view, so we shouldn't want to block its construction based on the formal structure of the theory; we merely want to say, with reference to the $\Gamma_e(v)$ function, that there are a number of possible ways of filling in the view-variable—and that experience need not be thought of as licensing only a set of very restricted statements about sense-data or our present mental states. That Gupta's function can allow for the non-privileged and non-unique construction of the sense-datum view is a boon, not a flaw of the function.

In as much as Gupta maintains a central role for reason in his empiricism, and in as much as he thinks experience unable to account for the rationality of our beliefs on its own, it is clear that he has moved far away from the CBE tradition (including the simple sense-datum theories that are its most recent incarnation). However, the dichotomous choice, CBE or Kant, is far too coarse, as is the dichotomy between the view of reason as purely analytic (and therefore trivial) on the one hand, or as a form of synthetic *a priorism* on the other. Reason can play a substantive role in determining the admissibility of initial views, or in testing the coherence of revised views, in such a way that the Insight is not violated; it is a rather old view of the *a priori* that holds reason can only make contributions to our knowledge by explicating the meaning of a concept, or by showing how certain concepts might be said to "contain" others, as *bachelor* contains the concepts *unmarried* and *adult man*. Gupta is right to move beyond this limited view of the role of reason within empiricism.

Of course, there is a way of seeing the hypothetical model of the given as compatible with rationalism; however, there is little reason to think that this is a deficiency of the model. One's model of experience, after all, shouldn't be able to

settle the debate between rationalism and empiricism. It is a sign of strength that an abstract model of the given be compatible with both. Here, again, it is quite clear that Berker and Schafer have misunderstood the point of the work, which is not to prove rationalism inherently flawed, incoherent, or otherwise incorrect. Gupta's model of the hypothetical given rather

> provides a framework for conducting the debate. The burden on the rationalist in this debate is to produce some synthetic a priori principles and show that they are essential for empirical rationality. The burden on the empiricist is to produce a rich and well-motivated conception of admissible views, one sufficient to undergird empirical rationality. (Gupta [2011], 48-49)

And, Gupta continues, "Berker is right that the abstract model, when taken by itself, does not rule out rationalism. I want to insist, however, that the model is more hospitable to empiricism than its traditional counterpart." (Gupta [2011], 49) Further, the burden is still on the rationalist to show that there are substantive truths of reason (or for the Kantian to show that there are synthetic *a priori* truths); the abstract formulation of rationalism within Gupta's model gives us no reason to suppose such truths can be found. The empiricist is forced to give reason only a formal role on the propositional model of the given, because all rational import on that model must be propositional, and to allow for non-analytic uses of reason is to countenance substantive truths of reason. On Gupta's model, reason can have a "robust role" which does not assign to reason the task of unveiling metaphysical truths required to explain empirical rationality. (Ibid.) Experience remains our principal epistemic authority and guide: reason is used merely to "remove obstacles that would hinder experience in its exercise of its epistemic authority. The role of reason is to serve experience." (Gupta [2011], 50) Reason determines the admissibility of initial views based on their dynamical behaviour in the course of revision processes under different possible courses of experience. This means that it has a substantial role to play in explaining empirical rationality, even though Reformed Empiricism can maintain (like all forms of empiricism) that reason can

provide us with no substantive truths about the world or the nature of the self.

§II: Proto-Mathematical Presuppositions and Experience

As we have seen, Gupta is able to respond to his critics by arguing that his Reformed Empiricism draws on richer conceptions of both experience and reason than previous accounts of empirical knowledge. Unlike rationalists, who view experience as subordinate to the dictates of intellect or some other non-receptive faculty when characterising the logical import of experience to knowledge, and unlike previous forms of classical empiricism (CBE and MSE) which unnecessarily denigrate reason, Reformed Empiricism maintains that experience can be our principal epistemic authority and guide (preserving the Insight) and also that reason plays an ineliminable and nontrivial role in allowing for experience to fulfill its epistemic role.

What we wish to explore in the section is whether or not this must be the sole operation of reason in Gupta's model. We will explore some questions about the formal character of at least some of our empirical judgments about extensive magnitude, motion, and relative position. We also question whether such judgments do not necessitate that reason play something like the role it played for MSE thinkers, a role which Gupta seems to eschew—a rich kind of conceptual analysis. This form of conceptual analysis, which we will explore in more detail in the Conclusion, examines the presuppositions which are necessary for the normal application of our basic pre-scientific-cum-scientific concepts in empirical contexts. In short, is it possible for reason to have at least two tasks within our empiricism: to (a) adjudicate over the admissibility of initial views based on the sorts of dynamical behavior they will exhibit in light of empirical revision, and (b) to discern what assumptions are at play in even the most minimal pre-scientific view when we make judgments about extensive magnitudes, spatial position, orientation, and displacement? It will be our contention that these two tasks are reason's, and reason's alone; further, the second task is nothing but an interpretation of the first task, since we will argue that the greatest obstacle for an empirical understanding of

the world is confronted unless reason can play a nontrivial role in revising our views. Reason is thus responsible to remove those initial views that are not amenable to revision in light of various possible "normal" courses of experience.

This is a novel analysis of Reformed Empiricism. Unlike the existing criticisms, it does not hold that there are any flaws in the formulation of Reformed Empiricism as an account in general epistemology, only that the view may encounter trouble when extended to certain topics and problems in the sciences and philosophy of science—problems which call into question Reformed Empiricism as a comprehensive empiricism, as outlined in the Introduction. Reformed Empiricism has no problems accounting for how our common sense view of the world is a convergent core shared by various revision processes from disparate initial views based on experience. We will argue here, however, that Reformed Empiricism is in need of slight reform itself if it is to explain the development of scientific theory as contiguous with pre-scientific, yet systematic, views of space. We hold that Reformed Empiricism, with the amendments offered here, is more than up to the task, and that the amendments offered in no way transgress the Insight, Multiple-Factorizability, or the constraints.

II.1: Mathematics in Gupta's Empiricism

Gupta very clearly states, early in his [2006a], that he is not concerned with giving an account of mathematics, unless it can reasonably be shown that mathematical truths are about the world:

> I will set aside mathematics in this discussion. Note, however, that it is plausible to include mathematical claims within the jurisdiction of experience insofar as they are *genuinely about the world*. The difficult philosophical questions about mathematics concern the contents of its claims. Are these claims vacuous in the way certain logical claims are? Or are they about broad structural features of the world, or perhaps of language? Or are they about a special Platonic realm that is a genuine part of our world? Whether the jurisdiction of experience extends to mathematics depends on how such questions are answered. (Gupta [2006a], 4fn)

Gupta is within his rights, and very likely right, to set aside such issues. No empiricist need talk about the reference, truth, or meaning of mathematical claims unless certain foundational matters are settled. Any form of empiricism that avoids such thorny issues is likely more than justified in this regard. Nonetheless, the decision to set aside foundational concerns in mathematics still leaves a further task, one motivated by the assessment of geometrical and mechanical concerns in the late MSE tradition: the role of mathematical assumptions in normal empirical judgments. We hope to show that questions about our formal knowledge (as implied by such simple judgments) are relevant to the philosophy of science, and that they illuminate the connection between general epistemology and the philosophy of science.

As we saw, this kind of role for mathematical assumption was recognised by Helmholtz and Poincaré, though perhaps best highlighted by Carnap. For Carnap, mathematical claims are not descriptive in the way that would force an empiricist to show how our knowledge of mathematics is experiential; rather some mathematical claims are interpretations of mathematical theories that, because they are empirical interpretations, describe the world and are open to empirical investigation. None of this Carnapian approach has any bearing on the *truth* of mathematical claims, but it is closer to our concerns here: that certain elementary empirical judgments employ primitive mathematical (or at least proto-mathematical) assumptions. For example, ordinary empirical reasoning assumes a certain *additivity* in many typical judgments about spatial relations, and this is independent of a view about why or whether the principles of arithmetic are true. These judgments employ implicit formal principles that cannot be reduced to logic, yet seem to operate much in the same way as logical principles do.

Two observations must be made. Firstly, as the taxonomy will have made clear, the ontology of mathematical objects is (for the most part) a separate issue from how we view the truth of mathematical claims, and this is separate still from the role mathematics plays in science and the very central role it has in our epistemology of scientific knowledge. This much was made very clear by Carnap in

his [1934/2002], and emphatically maintained even once the move was made from syntax to semantics (see his [1950]), but it finds its origins much earlier in Poincaré's conventionalism. Even if the foundational questions find no answers, and even if we cannot settle on a specific scheme (platonism, logicism, intuitionism, or formalism) for the assessment of the truth of mathematical claims, the epistemological significance of mathematics still stands. What was new, after all, in Carnap's researches on "foundational" matters was the importance he gave to the formal differences and similarities between differing foundationalist camps regarding mathematical concepts and theorems. His work expressly focuses on what can be done in philosophy and mathematics *without* definitive answers to the questions of mathematical ontology or truth. We hold that is too strong in his claim that such matters cannot ever be settled, save by some kind of pragmatic assessment, but the picture is mostly correct: whether we have an answer to foundational questions is largely independent of the role of mathematical assumptions in empirical reasoning.[54]

We do well to pause on this issue. In the early 1930s, the foundations of mathematics were still mired in crisis. Firstly, as Ricketts [2007] has noted, there were a number of different (and competing) solutions to the logical (and semantic) paradoxes, including both impredicative and predicative type-theoretical approaches, and various axiomatic set-theoretical approaches. The wrangling between philosophers and mathematicians representing these different approaches risked slipping into empty and regressive debate. Secondly, Gödel's Incompleteness Theorems had shown that any consistent formalisation of arithmetic (save for the formalisation of very trivial systems) is necessarily syntactically incomplete, calling into question Hilbert's attempt to find a complete set of axioms for mathematics. Lastly, there were disputes over the role of non-constructive proof in mathematics. Intuitionists like Brouwer, and then Heyting, defended the position that only those mathematical proofs that take us from simpler mental constructions to more

[54] The post-Kantian stipulation that mathematical structure can be divorced from mathematical content makes such considerations possible. Of course, on the Kantian view, the application of mathematics to sensibility does bear on the truth of mathematical principles.

complex mental constructions are legitimate. Logicists and formalists rejected this restriction. Logicists held that all logical laws and mathematical principles were either "laws of thought" or derived from laws of thought, and even the notion of number followed from the laws of thought. They eschewed the role of any intuition whatsoever in arithmetic (and, in the case of Russell, Whitehead, and Wittgenstein, though not Frege, geometry as well). Formalists held that mathematics was merely concerned with the consequences of certain strings of symbols based on rules for manipulating those symbols, with no regard to any possible content (intuitive or otherwise) that may be associated with the formalism.

Logical empiricists were in the unenviable position of taking stock of these seemingly intractable disputes, given that their philosophical position (following first Russell and Whitehead and then, increasingly, Wittgenstein's *Tractatus*) was an attempt to associate all of our substantive knowledge with experience, and all of our formal knowledge with empty and tautological symbol systems. Intuitionism gave too much content to mathematics (and restricted the application of mathematics to strictly human forms of representation); naïve logicism was susceptible to paradox; and Russell's type theoretical logicism (like various set-theoretical approaches) required that the logical empiricist countenance substantive existence axioms which themselves were not experiential or tautological in nature.[55] By default, Hilbert's formalism seemed the most plausible approach (or, as in the case of Carnap's pre-Gödel researches, a mixture of Russell's and Whitehead's logical apparatus and Hilbert's metamathemtics). With the arrival of the Incompleteness Theorems, logical empiricism seemed on the precipice. The mixture of classical empiricism's regard for experience as our sole source of substantive knowledge and Tractarian logicism was untenable.

Carnap sought to solve (or rather dissolve) these foundational problems in his [1934/2002] (hereafter simply *LSL* for the *Logical Syntax of Language*). As with our analysis of Carnap's *Aufbau* in the last chapter, our concern is not with whether or not the philosophy of mathematics in *LSL* is ultimately tenable. We are merely

[55] In Russell's type theory, the axioms of infinity, choice, and reducibility were of this character.

concerned here with the method by which Carnap sought to prevent these foundational concerns from interrupting the progress of mathematics and our broader philosophical understanding of its role in scientific knowledge. The method is to propose a logical pluralism (or rather a pluralism of possible languages, each with its own vocabulary, formation rules, and transformation rules) along with a voluntaristic pragmatism which holds that different language forms serve different purposes, though no such language proposal may be viewed as having hit upon "the correct" or "the true" logic sought by Frege, Russell, and Wittgenstein.

These innovations (a pluralism of as-of-yet uninterpreted calculi, and a purely pragmatic means of choosing between them) form the core of Carnap's notion of *tolerance*: the next big step for logic (and, by extension, for philosophy, which, for Carnap, was to be viewed as nothing but the "logic of science" or the "logical syntax of the language of science"). The goal is to "eliminate" the standpoint that new logics which deviate from their classical cousins "must be justified—that is, that the new language-form must be proved to be 'correct' and to constitute a faithful rendering of the 'true logic'." (Carnap [1934/2002], xiv) Tolerance allows for this:

> … we have in every respect complete liberty with regard to the forms of language; that both the forms of construction for sentences and the rules of transformation… may be chosen quite arbitrarily. (Carnap [1934/2002], xv)

This liberty is best expressed by what Carnap sees as the reversal of the order for language construction. In the tradition of Frege, one begins by assigning a meaning to the logical symbols of the language and then sanctions only those inference forms that preserve the truth of the fundamental sentences of the language. The construction approach from the "opposite direction" is this:

> let any postulates and any rules of inference be chosen arbitrarily; then this choice, whatever it may be, will determine what meaning is to be assigned to the fundamental logical symbols. By this method, also, the conflict between the divergent points of view on the problem of the foundations of mathematics disappears. For language, in its mathematical form, can be

> constructed according to the preferences of any one of the points of view represented; so that no question of justification arises at all, but only the question of the syntactical consequences to which one or other of the choices leads…" (Ibid.)

What governs this method? The *Principle of Tolerance*: "it is not our business to set up prohibitions, but to arrive at conventions." (Carnap [1934/2002], 51—italics removed from original) Carnap further explains the methodology of this principle:

> *In logic, there are no morals*. Everyone is at liberty to build up his own logic, i.e. his own form of language, as he wishes. All that is required of him is that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules instead of philosophical arguments. (Carnap [1934/2002], 52)

Mathematics is viewed as central to such language forms (not surprisingly, since each possible language form, from the point of view of syntax, is nothing but the proposal of a calculus, and the exploration of its logico-mathematical properties). Even empiricism itself is affected by *Tolerance*, since it is transformed from a substantive thesis about the origins of knowledge to a mere family of proposed language forms for the philosophy of science in which the relation of logical consequence for the language ("L-consequence") will determine all sentences which utilise only logical and mathematical terms as either logically valid within the language ("L-valid") or contravalid ("L-contravalid"), meaning that Carnap has basically supplied the empiricist with a family of languages in which bivalent truth-values apply determinately to all logical, mathematical, and logico-mathematical sentences (assuming that sentences containing observation terms are "L-indeterminate"). Logic and mathematics are nothing but "notational auxiliaries" to the substantive L-indeterminate (empirical) sentences.[56]

Given our substantial agreement with Gupta, it will perhaps be obvious that such an account of empiricism is undesirable. This is for at least two reasons. Firstly,

[56] Whether or not such a project is tenable is, once again, not of our concern. There is obvious concern that such a partition of the language between its logico-mathematical and empirical part cannot survive Gödel's researches, but we will not examine such matters here. For accounts of *LSL's* relationship to incompleteness, see Awodey and Carus [2001; 2004] and Goldfarb [2005].

empiricism as a general commitment to only use those languages that treat logical and mathematical sentences as L-valid or L-contravalid entails that such sentences can never be revised in light of experience, which seems to do great harm to the Insight of Empiricism.[57] There is no reason why we shouldn't expect that some experience might upset our conception of the world in such a way as to cause a rational revision, even regarding logic and mathematics (and even if logic and mathematics are considered non-factual). This view shares some superficial affinities with Quine's account in his [1951/1961], but it should not be conflated with Quine's confused and mistaken imagery of the "core" and "periphery", nor his confused notion that "total science is like a field of force whose boundary conditions are experience". Quine is right to hold that "[a] conflict with experience at the periphery occasions readjustments in the interior of the field," but wrong to think that such a field only meets with experience at the periphery. Quite often, what we are looking at when we engage in scientific experimentation is just how experience impacts the core, come what may at the periphery.

Secondly, the inflexibility of the language in its logico-mathematical part is also a feature (more troublingly) of its empirical part. Carnap's language forms offer us a way to engage in empirical testing of physical theories, but seemingly no way of rationally revising said theories in light of our tests (even though the outcome of such a test might, for Carnap, inform a switch from one theory to another, when experience makes it convenient). Thus, the syntax project (and its later semantic variant) is far too arbitrary to be considered a viable form of empiricism for our purposes, if only for the fact that it leaves far too much to choice. Pluralism and voluntarism may make sense of the Multiple-Factorizability of Experience, but they fail to give proper weight to the fact that we at least seem to rationally revise our scientific theories in light of experience (and that one of experience's key epistemic purposes is to motivate and guide such changes). In short, Carnap's theory of theories (both the *LSL* and mature variants) jeopardises the Insight.

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[57] Carnap does think that experience can give you some pragmatic reason to adopt a new language form with new L-rules, but this is different than the empirical revision of such rules—for which Carnap makes no allowances.

What these cursory remarks are meant to show is not that any such arbitrariness should be added back into the mix. One of the main strengths of Gupta's argument is that it can account for changes (often very substantial changes) in view based on experience, not the arbitrary choice of new formal system. Nor is it our desire to reduce empiricism to an ethos or commitment. It is more than a matter of free choice whether or not one adopts the Insight. No view purged of the Insight would be worth calling "empiricism", nor would it be a proper philosophy of empirical science. So why the excursion into this very late project of MSE; what is it we can take from Carnap's discussion of matters logical and mathematical? Carnap's work (first during his syntactical phase, and then his post-Tarski shift to semantics) is significant to our study because it is continuous with the MSE tradition studied in the previous chapter—the outcome of an historical progression of thought which recognised (a) that a key obstacle for empiricism was accounting for our mathematical knowledge in a way that showed how it could be *a priori* and generally applicable, and (b) that some of our basic empirical judgments required some kind of formal framework within which judgments about extensive magnitudes could be made (i.e. frameworks which make possible the empirical testing of theories). These are themes that we wish to explore in more detail below.

II.2: Simple Empirical Judgments and Proto-Mathematical Presuppositions

As we have seen above, no empiricist should think it is her duty to make pronouncements regarding the truth of mathematical claims. Our concern is different, and it shows that there is still something mathematical to say about experience, even if there is nothing empirical about mathematics. Setting aside the thorny issues of mathematical truth, the ontology of mathematics, and the general nature of our mathematical knowledge, we are still left with empirical principles that have some mathematical content, or at least avail themselves of primitive, proto-mathematical assumptions: at the very least, a conceptual structure that, while perhaps quite elementary, posseses formal aspects that implicitly involve a

mathematical idea. Let us take a simple claim about the movement of people in a shared environment:

> (†) "John ran to his left because Bill was gaining on him faster from the right than was Sandy from behind him."

Let us assume that the speaker who utters (†) has a view (v^*) very much like our own, and that the experience that generated the belief (e^*) whose propositional content is expressed by (†) are of having watched John change the direction of his run as the result of Bill closing in on him, so that

$$(\dagger) \in (\Gamma_{e^*}\ (v^*)).$$

Let us also suppose that we are dealing with an utterer who has a perceptual system that has been reliable in making such judgments about relative distances in the past. Also assume that there are a number of spectators who have also watched John's run from Bill and Sandy, and that all spectators agree with the propositional content expressed by (†). Talk of "other spectators" is meant to imply that the space in which (†) is embedded admits of an infinity of possible points of view or perspective from which (†) can be observed, and does not commit us to any kind of bizarre communalism—e.g. the view that our conception of space relies on intersubjective agreement. We do hold the much weaker, logically distinct, claim: that quasi-mathematical assumptions make possible intersubjective agreement on a certain class of (spatial) judgments.

However, the spontaneous generation of (†) in light of experience e^* is only possible if certain features hold of view v^*. On top of the utterer having the relevant concepts (which, because v^* is very much like our view of the world, we may assume she has *ex hypothesi*), (†) relies on many tacit assumptions about the shared space of the spectators. One such assumption is that the space in which the pursuit takes place is uniform, i.e. there can be a family of seamless translations of position as John, Bill, and Sandy move through it. Another assumption is that topological

structure is preserved: Bill and Sandy cannot converge upon John without also converging on one another, and differences in perspective do not upset (†). In general, the space is such that it allows for the sorts of judgments about relative distance that we may make when describing the pursuit. The space is uniform, homogenous, and isotropic—or at least locally so. Here we can see how quasi-mathematical assumptions operate in even very simple judgments, including those that do not call for specific and quantified measurements.

This is significant because these sorts of judgments are exactly the sort we make in even the most primitive of settings. They are the sorts of judgments one could see a primitive human making about his or her surroundings while foraging for food, engaging in the collective hunt, running from predators or enemies, or in following simple directions to get from one place to another. They may even be consistent with the high priest's proclamations about gods in the heavens, and their interactions, as well as the flights of fancy we entertain in dreams and imagination. We want our empiricism to be able to make sense of such worldviews and such sets of perceptual judgments, even if they employ naïvely unscientific notions like "left", "right", and "behind". Even though some views may include the misconception that space has a privileged set of such directions (just as Aristotle's physics thought that "up" and "down" were genuine, physical qualities of movements), this does nothing to mitigate the wealth of information we possess, even in such primitive examples, about the spaces we inhabit. This much is clear from even a cursory analysis of the presuppositions that make such judgments possible.

A second observation deals with these implied mathematical assumptions that are presupposed by such judgments. There exists a set of empirical principles in which some mathematical structure is implicit. Such principles, as we have seen, are not set aside with the foundational worries Gupta rightfully hopes to bypass. We here make a further and stronger claim, however: without considering these principles, an extension of Reformed Empiricism to relevant issues in the philosophy of science is impossible. Recall that, given the commitments of MSE, our main concern is with the mathematical content of at least some of our ordinary

empirical claims about magnitude, position, and relative motion. Thus, we hold with a preponderance of thinkers in the MSE tradition (in opposition to the CBE tradition) that such assumptions must be accounted for, and that such an account is conceptually prior to even those simple claims that the CBE tradition thought we could make about our "ideas" (impressions, sense-data) of moving objects.[58]

The distinction is made most clear by an examination of the debate between Russell and Poincaré regarding the meanings of geometrical primitives. The debate began with Russell's [1897], in which he argues that the principles shared by Euclidean and non-Euclidean geometries are *a priori*, but that those uniquely Euclidean principles are empirical. The first to respond to this position was not Poincaré, but Louis Couturat in his critical review of Russell [1897] (Couturat [1898]). The review was quite positive. Couturat praised Russell's careful treatment of Euclidean and non-Euclidean geometries, especially his account of the aprioricity of geometry (and the useful ways in which symbolic logic might be applied to traditional philosophical questions about our knowledge of geometrical axioms) but did not think Russell had established that Euclidean geometry was empirical. Russell [1898] is a response, mostly cordial, explicating the difficulties with offering a complete account of the empirical nature of Euclid's axioms. However, it is a passage at the beginning of the piece that led to the debate proper—a claim Russell makes against the conventionalism of Poincaré:

> Are Euclid's axioms capable of being verified or refuted empirically? In order to discuss this question, we must first of all reject completely the theory of M. Poincaré, according to which these axioms are pure conventions like the metric system, and consequently are not capable of being true or false, verified or refuted… I shall suppose… that the Euclidean axioms are either true or false. The question is then of determining the nature of the reasons which we have for accepting them. (Russell [1989/1990], 325)

[58] There are elementary experiences, perhaps of pain or of relative intensive magnitudes like *warmth*, which may not require us to have prior quasi-mathematical notions. These differences may very well be "given" in experience. I do not address such issues. I am concerned only with those experiences of spatial magnitude, position, orientation and displacement.

Russell held that if a geometrical proposition was meaningful, then we understood this meaning by grasping each of its constituent terms.[59] But this assumes some cognitive access (let's assume acquaintance through experience) to the constituent parts of a geometrical proposition. Let us look at an example that calls into question such empiricism about geometrical propositions.

Take a general sentence that makes clear some of the conceptual commitments required to assert a judgment about a geometrical observation, e.g. the principle of free mobility:

(Δ) "Rigid bodies can be moved in space without change in shape."

According to Russell, for one to grasp the meaning of the proposition expressed by (Δ) would require one to grasp each of the terms in (Δ) independently of one another. Let us assume for the sake of argument that Russell's theory of meaning gives us acquaintance with all of the terms of the proposition expressed by (Δ), except for the final term: *shape*. Russell will say that shape is gotten by direct acquaintance too: it is the simple (unanalysable) datum that we grasp, giving the proposition its sense. But, argues Poincaré, this cannot be the term as it arises in geometry, for geometrical principles have a *publicity* which allows them to be grasped by all those who can reason with the principles, and no two persons will have the same datum of sense (such as a yellow triangle) before them, so this is not what "shape" can mean. Different intuitive access to a concept cannot explain our uniform use of the concept, which relies on all users to have the same formal criteria for the application of the concept. What is required, therefore, is that the concept be given a more precise formulation—that formulation is given by (Δ) itself, which ought to be read not as a substantive general claim about the actual behaviour of bodies in motion (or our intuition of same), but as an *implicit definition* ("definition in disguise") of the concept of shape (or, arguably, the concept of space, or the concept of rigid body). In other words, (Δ) shows us how the concepts employed in

[59] This follows from Russell's general theory of propositional understanding, which argues that to understand a proposition is to be acquainted with its constituents.

(Δ) are systematically related, so that if we take a class of objects to be our rigid bodies (say measuring rods), then we can make sense of what we mean by "space" and "shape" (and here, the *we* of communal knowledge is key).

Another way to think of the issue is this: if Russell provides us with a report about his occurrent visual sensation, and he says that he is currently experiencing a triangle moving from left to right in his visual field, then Poincaré can say, "How do you know it to be a triangle at all, unless you assume it is a rigid body moving through a uniform space and not some non-rigid entity undergoing systematic spatial distortions of the sort that make it appear as if a triangle moving through uniform space?" This question is enough to upset Russell's position. All of our formal principles, including the principles of geometry or even of physical theory, have this character: *they seem empirical*, but really are not—at least not in the same sense as normal empirical claims.[60] While they have the syntactical appearance of synthetic claims, they do not express what we have come to know empirically about the concepts they contain, but rather such concepts are defined by the very principles themselves: they determine for us how precise and formal concepts are to be empirically interpreted. In this way, they may be empirical, since they are only understood through their empirical content; but what they are not are substantive claims about the world. Here is the dilemma for the (classical) empiricist: they are not purely formal, and therefore cannot be lumped together with logical principles; but (as we have seen) they are also not empirical in the sense that they are given in the unanalysable data of sense. If they are empirical in some sense, that sense is not Russell's.

So the worry is this: our formal principles are tied to their empirical content in systematic ways that we may reason about said concepts collectively and arrive at agreement about our empirical judgments. Such principles, for Poincaré, take the place of Kant's "forms of intuition" precisely because they allow for the formulation of, and agreement about, such simple judgments. What remains unclear is how

[60] Here, "empirical" should be read in its more classical sense, i.e. empirical statements are statements that are synthetic (at least in appearance) and empirically verifiable or falsifiable. In the sense of Reformed Empiricism, such claims may be revised in light of experience.

Gupta's view accounts for such agreement if mathematical assumptions and formal principles are not given more attention in his account. In §IV we will argue for a way in which he can address some of these concerns about formal principles. But before we look at the issue in more detail an equally pressing problem must first be tackled.

§III: The Problem of Communication and Communal Knowledge

The basic problem to be addressed in this section has already been motivated by our discussion in the previous section, so our remarks will be brief. One of the most pressing issues for the empiricist tradition, as made clear in the Russell/Poincaré debate, concerns the very real problem of inter-personal communication. If our claims about an external world of objects embedded in space are all actually claims about subjective entities in psychological space, then how could it be possible for different people to converse about the same matters? How do we coordinate abstract formal concepts (say, those of geometry) with spatial intuition (assuming, contrary to Kant, that geometrical structure is not necessarily determined by the structure of intuition)? Later empiricist thinkers, including Russell and the logical empiricists, thought the problem could be overcome by focusing on *structure*. Schlick is a good example. We do not (and cannot) communicate about the subjective contents of mind, according to Schlick. However, we can communicate about the implicitly defined abstract concepts of formal science. Whereas we have "acquaintance" (*Kennen*) with sensory images, we have "knowledge" (*Erkennen*) through concepts.[61] We are said to *know* only when we have produced "a successful designation of reality" by means of concepts. Rather oddly (for an empiricist), Schlick holds that we have genuine empirical knowledge via sophisticated scientific

[61] Schlick thought it a confusion of the "philosophy of intuition" to conflate acquaintance and knowledge. Here, Schlick is offering a more Kantian analysis than Russell's classically empiricist account of knowledge, which is centred upon knowledge by acquaintance. "Intuition is mere experience, but cognition is something quite different, something more. Intuitive knowledge is a *contradictio in adjecto*. Even if there were an intuition by means of which we could insert ourselves into things, or things into us, it would still not constitute knowledge… What we obtain [through intuition] is an acquaintance with things, but never an understanding of things. It is the latter alone that we aim at when we search for knowledge in science and in philosophy." (Schlick [1925/1974], 83)

theory precisely because such theories are conceptual and non-intuitive. Scientific knowledge counts as knowledge because it is formal, and this is what makes scientific theories communicable. (The influence of Poincaré is obvious.)

But in what sense are scientific theories empirical if they are so non-intuitive? Here Schlick relies on what he calls "the method of coincidences". Take various intuitive spaces of an experiencer (co-numerous with the number of sense modalities of the experiencer). Each will have its own qualitative character, such that one cannot compare a smell with a touch, or a visual sense image with an auditory sense image. Nonetheless, we can experience a "singularity" in two sense-modalities. To use Schlick's example, I can touch *and* see the point of a pencil. This produces for us a "coincidence" which can be embedded in objective space by abstracting away the particularities of the tactile and visual intuitions. When we piece together these coincidences (when we have enough singularities) a system of points in objective space is produced. We can communicate with one another about the structure of this objective space (the structure of point-coincidences)—specifically its topological structure. This topological space is a projection of intuitive (or psychological) space—the former dispensing entirely with the incommunicable subjective content of acquaintance.

> Everything in our world-picture that *cannot* be reduced to these coincidences is deprived of physical objectivity and can just as well be replaced by something else. All world-pictures that agree with respect to the laws of these point coincidences are physically absolutely equivalent. (Schlick [1917/1978], 241)

It is to the structural aspects of experience that we refer when we communicate with one another, allowing for the practice of science.

This general line of thought was also employed by Carnap in his *Aufbau*, though here the structure of the physical system of concepts is not a projection of intuitive space on an objective space, rather external three dimensional spatiality is constructed (via quasi-analysis) from colour sensation, which is itself quasi-analytically constituted by the construction of colour subspace, which is itself

constituted by the construction of the different sense-modalities based on their dimensionality differences (e.g. visual sense modality is the only one with five dimensions), which is constituted by "sense classes", which are further constituted by quality classes, which are defined as those cross-sections of the experience of a single cogniser that agree on some particular sensation.

We do not have the space here to offer a critical analysis of Schlick's method of coincidences. We have already discussed the problematic move from the structure of auto-psychology to the structure of physics in the *Aufbau* at the beginning of this chapter, though not in great depth. Thankfully, no such analysis is required. This is because Gupta sidesteps the problem of giving a structural account of communication. This reliance on structure is a result of viewing experience as having itself a structure and an intuitive part, the latter of which cannot be spoken of in public language save via coincidences. The worry is one of making a private language (a sense-datum language, say) correspond to a public language, and here there seems no satisfactory way of progressing. Gupta avoids this problem altogether by showing that experience has no proposition-like structure, and that experience itself can be of public objects. However, we have not yet done away with Poincaré's problem. Unless we assume that we all possess at least some of the primitive geometrical principles operating in normal judgment, an equally frustrating obstacle to communicability is encountered: because perceptual judgments are a function of experience and view, either we assume some basic similarity in view or lose any hope of conveying to others what it is we are given in experience. Agreement regarding perceptual judgments is not enough in itself, for two epistemic agents, A and B, could agree on perceptual judgments Γ in ways that were not systematically attached to the same course of experience *E*: where A has license to Γ as the output of his view and course of experience, and B has Γ as a result of a very different view and a completely different set of experiences. Such problems are only removed if we can presuppose that A and B have the same experiences or considerable overlap in their views.

As will easily be granted, the empirical sciences require the comparison and integration of reports of empirical judgments from a variety of personal and spatio-temporal perspectives. Science, at least its experimental arm, is therefore a communal enterprise. If experience is viewed propositionally, there are no special concerns about how observation judgments may be conveyed between practitioners (though empiricists, as we have seen, may encounter difficulties with the non-publicity of reports of private sensation). We may have to suss out the nature of our scientific "protocol sentences", and such issues can get quite messy, but the program itself is straightforward enough. There are collections of scientific judgments that are public and communicable. This was indeed how the left wing of the Vienna Circle, especially Neurath and Carnap, conceived of "physicalism"—not expressly as a reduction of all subject matter to physics in accordance with an abusive scientism, but as a way for practitioners in sundry fields to communicate with one another. Physicalism was a first (flawed) attempt by the left wing of the Vienna Circle at producing an interdisciplinary approach to the sciences, to overcome unnecessary wrangling through genuine communicative practice. Protocols (couched in the vocabulary of formal science) were thought so valuable because they were meant to guarantee that discussion could take place: a neutral language within which all other sciences can congregate.

However, those (like us) who agree with Gupta that the whole business of assigning propositional contents to experience should be avoided must replace this program, and that means losing a straightforward model for the communicability of observation. But, then we must contend with a consequence of the logical recategorisation Gupta proposes: the functional given makes inter-practitioner communication—so central to experimental science—if successful, entirely mysterious unless the practitioners share substantial overlap in courses of experience or views. If practitioners were to drop the propositional given they would also drop the ability to speak directly about experience (i.e. they would not be able to produce neutral observation reports). They can still produce theory-laden judgments, but how they might know they are using the same theory is not

explained, and where such a theory came from is also of concern: if it is historically given, or arbitrarily chosen, then it seems to lack the kind of justification (via experiential revision) which Gupta demands of views. The only other option, that it is a product of observation, fails to make sense in light of Multiple-Factorizability (which, if it is to make communicable pronouncements at all, must already be functionally paired with a view of the world).

Gupta may respond to such a claim by arguing that scientists are far more likely than the average group to share a theoretical perspective (a worldview). In fact, he says almost as much, arguing that the extension of his Reformed Empiricism to questions about scientific knowledge is unproblematic once scientists share a "core" of beliefs (we may suppose something like Ω above). Here is what Gupta says:

> There is a further idea here with which I shall not be much concerned: that the succession [of experience] renders reasonable our scientific conceptions also. The philosophical problems posed by our empirical knowledge concern the very core of our ordinary conception—a core that humans acquired before they began inscribing marks on clay tablets and that most children acquire before reaching the age of three. Once the core is secured, the further idea that experience renders reasonable the refinements of the sciences presents few fundamental difficulties. The bearing of experience on the sciences (and other disciplines) can be understood in the standard way: experience issues perceptual judgments and can thus serve as a tribunal for them. (Gupta [2006a], 103-104)

But more needs to be said about how this common core is sufficient to guarantee that perceptual judgments are adequate to serve as the tribunal for the sciences. From what we have examined, that seems at least to be an open question. Without such an account, we are left wondering how we can justify our scientific theories.

Here is another way to conceive of the problem. We can only know that we share a view if we fix either experience or view—i.e. if we can show that the sequence of experiences endured by one cognitive agent are the same as another's (or at least they have the same subjective character), or if we can show that a number of cognitive agents have fundamentally similar views. So, in the case of communal observation, either (a) we fix experience, or (b) we fix views. But now we

encounter a dilemma. We cannot fix the relevant set of experiences in accordance with (a) if the given in experience is a function, since (*ex hypothesi* for the cases of communal observation) such a move would require inter-practitioner communication and practitioners cannot communicate their experiences without assuming some view first. (Recall the central assumption here is that there exists a set of relevant observations that escape any single observer, and are only arrived at by coordinated, communal practice.) We can only communicate our experiences, therefore, if we fix the view being used by all relevant practitioners in accordance with (b)—but here, since by (a) experience cannot be what fixes views, we must arrive at the same view via some other faculty than experience, doing great damage to the Insight. If horn (a), then the essentially communal character of the empirical sciences seems to be jeopardised, since we have no reason to assume that scientists could engage in the requisite communication (about observation) to engage in theory development. If horn (b), then in what sense is the existing scientific view *justified*?

To be clear, we do agree with Gupta that the common sense view of the world and the scientific view of the world are not two separate views, rather that they are intimately intertwined; but his cursory remarks about how a common sense core of beliefs, commitments, and anticipations will lead (via experience) to the "refinements of the sciences" with "few fundamental difficulties" leaves the reader in want of further explanation. The problem is even more pronounced if the reader does not share his view that common sense views and scientific theories share a core.

Take Newton's second law of motion as an example of the difficulty: the acceleration of a body is parallel and proportional to net force *F*, and inversely proportional to the mass. Is this an empirical generalisation? Arguably, yes. But it and the other two laws of motion together constitute a coherent framework within which empirical judgments about force and motion can be made. This was an insight taken into consideration by Newton himself, Kant, Poincaré, and the logical empiricists. They all held (in some way) that certain principles are *constitutive* of

scientific theory, even though there is some disagreement about the necessity and apodictic certainty of such principles. As with the law of free mobility discussed earlier, the second law of motion is an implicit definition (a "definition in disguise"), in this case of a new Newtonian concept: *force*. One can utilise such a rich conception to criticise other incoherent accounts of force and motion, as, for example, Newton criticises the Cartesian distinction between "proper motion" (motion viewed as the translation of the neighbourhood of contiguous bodies) from "vulgar motion" (motion as change of internal place), and he can point to the "normal experience" of water in a rotating bucket to point out the problems with the Cartesian position (the so called "Bucket Experiment"). But it is not clear how one could come to the laws of motion as a result of a series of experiences without highlighting the presuppositions of Descartes' physics, and showing the ways in which those presuppositions (to the chagrin of Cartesians) already hand to the Newtonian much of her conception of motion in light of the relevant experiences (experiments). It is not enough merely to point to experience, though it plays a central role; Newton also shows that all of the historical discussants, including Galileo, Descartes, Leibniz, and Huygens, presuppose the laws in making sense of elementary dynamical problems, even if they don't think they do. Such theorising requires a particular kind of refinement in conceptual *system*, and it remains to be seen how such a system could, on Gupta's model, come about as a result of experiential revision unless the role of mathematical assumptions is made more central.

Firstly, there is the problem of indeterminacy, though, since this is not a problem solely for Gupta, and since he seems more able than most to cope with indeterminacy (given that convergence overcomes the problem of the more primitive problem of the relativity wrought by Multiple-Factorizability), our remarks will be brief. Recall that perceptual judgments are of the form $\Gamma_e(v)$. Therefore, empirical claims of a theory (if we may be permitted some new formalism) can be represented in the following way:

$$\Gamma(T) = \varphi(O)$$

where $\Gamma(T)$ is the empirical claims of the theory (a class of systematised sentences); φ is the theory itself (a class of systematised sentences that may or may not be equivalent to $\Gamma(T)$, but also anticipations—i.e. hypotheses—and perhaps rules for conducting experiments, and what may count as evidence as opposed to mere data, etc.); and O the observation (or set of observations) of a practitioner. Now, assume Multiple-Factorizability. It is entirely possible—i.e. not determinately precluded by $\Gamma(T)$—that $\Gamma(T)$ is the result of other theory/observation combinations, so that (for example)

$$\Diamond (\Gamma(T) = \phi(O^*)).$$

An historical example might be Lorentzian electrodynamics and special relativity, both of which were compatible with evidence available in 1905 (e.g. Michelson-Morley), but we set aside the particularities of examples to get at the crux of the matter: scientific views are not determined by experience or observation. (This in itself is not a problem for Gupta since he merely wants to account for the ways in which experience can rationally inform a view, not uniquely determine a correct view. However, these considerations enter into the following argument.)

The real problem for Gupta comes by way of what may be considered a quantum leap from common sense conceptions of the world to the sorts of sophisticated scientific views of the world we currently possess. Taking the judgments of our leading views on space, time, and motion as a central part of Ω, will all admissible views converge on Ω? What about views that do not even countenance at least a primitive notion of external space in which empirical judgments may be embedded? Can Gupta's existing admissibility criteria license, for example, the blocking of phenomenal accounts of experience, like Husserl's, which leave no room for such judgments (as a result of sustained *epoché*)—a view that interprets all experience as intensive magnitudes—as feelings and sensations not

even directed at themselves as intentional objects? What about Heideggerian conceptions of time? (Examples do not need to be further multiplied.) If these views are admissible, then there will be no convergence point at which all admissible views will agree upon Ωφ (Ω enriched to include not only common sense judgments, but scientific judgments about space, time, and motion).

Science requires communication between practitioners about observation. Because observation is theory-laden (as we have shown to be a corollary of Multiple-Factorizability), communication requires that we have in place views which are either (a) already commensurable with one another, or which (b) can be revised to be commensurable with one another. Assuming that practitioners already share a substantive scientific view prior to observation means that we do damage to the Insight, for the origin of our scientific world conception will be merely accidental, historical, or arbitrary. For (b) to be possible, a rational scientific conception of the world must be a substantial part of Ωφ, and all admissible views must converge on Ωφ (lest practitioners only possess conditional entitlements). Scientific knowledge would be either arbitrary or merely conditional (and likely both). Unlike our nicely convergent common sense views of the world, our scientific views of the world would grant us no categorical knowledge.

§IV: Reformed Empiricism as a Model of Scientific Knowledge

There are at least two plausible ways Gupta could reply to such results. I will examine both. The first is to admit that the bifurcation poses a problem for his view, and limit the applicability of his model to general epistemology only. This would amount to saying that Reformed Empiricism is not a replacement of classical empiricism full stop, but only of CBE and its more modern variants—for example, sense-datum theories of perceptual knowledge. While this move saves the position, it blocks the view from having a fruitful applicability to the philosophy of science, which would constitute a great loss. This would be to admit that Reformed Empiricism is not a possible candidate for a comprehensive empiricism. As we have

made clear, this dissertation is an attempt to defend the opposing thesis: that Gupta's framework is an ideal candidate for a comprehensive empiricism.[62]

A second tactic allows Reformed Empiricism to overcome the deficiencies we have isolated from the perspective of MSE. Nothing in the way of technical change is proposed (we do not, for example, revert back to a propositional model of the given), but stronger conditions on an acceptable view must be imposed in order for the connection to be possible between ordinary views and scientific ones.

Gupta tells us that there should be no prior restrictions on the initial view of a revision process except that any initial view should not be pathological/rigid, insensitive, or internally incoherent. By this he means that we should preclude any initial view that has as a central feature an inability to be revised *no matter what the course of experience may be*. Which views are *not* acceptable as initial starting points and why are they precluded? Of course, we know by now that Gupta's early examples are solipsistic and/or sceptical views, but he also explores ([2006a], Chapter Four) other types of unrevisable views, such as certain religious views, chauvinistic views which shun outsiders and prevent experiences of/with outsiders from taking place, mystical views which hold that "normal" experiences carry no epistemic weight, or the sorts of views held by drug addicts who think they have fulfilled their epistemic obligations only when engaging in "enhanced experiences". (Gupta [2006a], 205)

These *a priori* restrictions, as we have already noted, do not preclude pathological or insensitive views from forming as a *result* of revision. One can come, via a course of experience, to recalcitrant religious beliefs, or to reasonably consider herself a brain in a vat, or to the belief that all others she encounters are un-minded phantasms of a fantastical hoax by some deceiving demon or evil scientist. These

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[62] Empiricists may decide to hold the view that science is different from, and discontinuous with, common sense. In fact, many empiricists have done this. The dialectic I wish to pursue will not address those who think there is such a discontinuity. It seems at least *prima facie* plausible that common sense informs our scientific worldview, and nearly absurd to think that science does not have congress with our common sense. To use Sellars' terminology, the *Manifest Image* and the *Scientific Image* interact with one another in ways that are hard to deny. In any case, those who do not share the desire for what we here call a "comprehensive empiricism", what Sellars called "synoptic philosophy", are not the direct audience for such a study.

positions may be reasonable in light of experience, though it may be hard for us to see (from our current epistemic position) what sorts of experiences would lead us to such beliefs and how they may survive subsequent experience. But a boon for this tactic is already quite clear. For Gupta, it is completely unnecessary to engage in one or another philosophical argument against scepticism or solipsism, given that they are (strictly speaking) allowed. The stronger claims that scepticism is committed to a larger set of unproven assumptions than is common sense (Moore), or that it is conceptually/semantically confused (Austin, Putnam, *etcetera*), or unworthy of epistemic consideration (Davidson, Rorty, *etcetera*) are irrelevant. Scepticism does not need to be "blocked"—we merely need to point out to the sceptic that she may not propose her view as a genuine starting point in a revision process. (How, before experience after all, are we to accept a view that says that experience can have no impact on epistemic revision? There is no basis for such a claim, and the empiricist need not accept it.)

But are these the only sorts of views that are inappropriate as starting points for revision? I suggest that there is a class of views that will fail to extend to a scientific picture; the kinds of views with which there cannot even be Galilean dialogue, and, as a result, which will not converge with our contemporary accounts of space, time, and motion. These unacceptable initial views must be barred, even if they are not (strictly speaking) rigid, insensitive, or incoherent. Any view, in fact, which does not have as a central feature some primitive conception of the world as a common and objectifiable realm in which empirical judgments can be embedded cannot be accepted. If we accept that Gupta's model can account for the step-wise development of our beliefs from the primitive and incorrect "lore of our fathers" to the sophisticated and true conceptions of spacetime we currently possess, then empirical fact alone will not be enough. What is required is an analysis of the presuppositions that accompany even our most primitive views of the world as a shared arena of interaction.

As we have already seen, what is required is an application of conceptual analysis—a conceptual analysis which shows the essential agreement we have

regarding presuppositions that are necessary for the empirical application of spatial and temporal concepts.[63] Such presuppositions may include primitive versions of some minimal, irreducible conception of elementary spatial operations (and their relations to one's own perspective)—e.g. iteration, subtraction, and composition. Any failure to countenance the applicability of at least these elementary operations would indeed be "pathological" in the specific sense that such a view would find it impossible to provide any extension of the personal to the communal (and, eventually, the scientific). But now we demand more than that a view be non-pathological; we demand also that it countenance a common arena for experience. (Perhaps we can construe Gupta as meaning nothing more when blocking solipsism as an initial view; if that is the case, then here we are giving a more complete characterisation of why solipsist views are pathological.)

If all initial views possess at least this minimal commitment to a revisable view of an arena of communal interaction, then it is plausible we can explain how experience (along with the occasional ingenious bit of conceptual analysis) can account for the development of more sophisticated theories of space, time, and motion. We have examined (in a rough way) some of the key points in the history of such developments, and a more complete (though brief) discussion will take place in the concluding remarks. For now, we should see that such a minimal conception of a shared arena of interaction serves the purpose of blocking those views that could never allow us to converge upon sophisticated physical theories. Further, this seems legitimate, for all proper scientific thought should, as Gupta might agree, remove those obstacles which prevent experience from playing its essential role as our epistemic authority and guide.

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[63] We intend our account of conceptual prerequisites to be quite general indeed, maybe even allowing for all kinds of "communal spaces" which appear quite unscientific, such as grand theological pictures of the nature of reality, or even "new wave" conceptions of the ultimate nature of reality. Such system building, we argue, should not be blocked. However, we argue that such views are revisable, in so far as they are not rigid, insensitive, or incoherent (which they very well may be, and in which cases they should be blocked by Gupta's original criteria). It is our contention that a great many of their metaphysical commitments will find no purchase at our hypothetical convergence point, and the set of judgments $\Omega\varphi$.

But a question remains: is the enrichment of acceptable initial views here proposed amenable to empiricism? We argue that enrichment must not transgress the Insight or Multiple-Factorizability or any of the constraints. In other words, we can accept this enrichment only insofar as such principles that may be found within our initial views are revisable in light of experience. Also, we think we are capturing both a methodological commitment of scientific empiricism and a substantive claim about the historical development and justification of our understanding of space, time and motion. On the methodological side of things, we are saying that it makes no sense to consider oneself a scientific empiricist if one does not at least agree that there is a common arena in which empirical judgments can be made and in which judgments about extensive magnitudes can be assessed. On the substantive side of things, we hold that our current sophisticated understanding of space, time, and motion could not have come to be if not for some initial conditions that made it possible for subjects in a community to understand one another and point to salient features in the environment which gave physical theorising (even very primitive versions of it) some purchase. The following thesis can thus be read both methodologically or historically:

> *The Principle of Scientific Empiricism (SE Principle):* All initial views must admit of a minimal conception of a shared, objective arena of interaction.

The SE Principle thus amounts to a commitment that all initial views must allow and account for basic interaction. Of course, this does not guarantee that all such views can be made scientific, only that all scientific views possess this kind of structure. Advanced physical science, not epistemology, must decide which views may end up counting as scientific—our job in epistemology is merely to block those views which are necessarily destructive to convergence on a scientific conception.

The SE Principle is weak in the sense that it makes no commitments to what sort of physical processes may be referenced when establishing this or that geometrical notion. For example, the SE Principle makes no commitment, as Helmholtz did, to congruence as the basic geometrical notion for spaces like the

ones we are thought to inhabit. The SE Principle is strong, however, in that it demands that empiricism give us an account of what presuppositions are operating in our empirical application of spatial and temporal concepts (even if we are not yet knowingly employing "spatial" and "temporal" concepts).

The SE Principle also does not commit us to a kind of rationalism, if "rationalism" means there exists some faculty other than experience that provides us with insight into which geometrical conception is true of the world or any theoretical insight into how any aspect of the world must be, including its geometry. Notice that, aside from saying that we cannot consider ourselves scientific empiricists without countenancing an arena of interaction, the SE Principle says nothing about the nature of visual or physical space, much less does it place upon us some fixed conception of how the world is. We are not born, as it were, with innate geometrical structures to which all experience must conform—or, at the very least, the SE Principle supposes nothing of the sort. This is a matter for cognitive or developmental psychology to study, and not something imposed on us by epistemology through the SE Principle**.** All of these assumptions, typically tied to a notion of the *a priori*, are here seen as further issues to be settled by a normal sequence of experiences (coupled with the tools of conceptual analysis). The SE Principle should thus be read as the most minimal of constraints on adopting the view that our scientific knowledge can be empirically justified. If one were to consider it *a priori*—a characterisation we do not promote—then one would have to admit that it is the most minimal and malleable of *a priori* principles one could imagine.

The SE Principle is therefore consistent with a vast number of initial views, and also with the ways in which those views can be enriched. Imagine starting with a primitive view that allows us to make judgments of a sort like (†). As we discussed, such a judgment carries with it presuppositions about the (local) uniformity, homogeneity, and isotropy of space. Though our primitive speakers would very likely not be able to characterise their presuppositions in these ways, it is clear that at least proto-mathematical variants of principles we commonly use in mechanics

are at the heart of communal life for such people, exhibited by the successful negotiation of the environment by such folk. The principle of free mobility (the modern, mathematical principle) tells us that arbitrary continuous motions (transformations) can occur without distortions in a body. But something like this is already assumed when we attempt to manipulate objects in our environment, or when we provide directions to a traveler (which presuppose elementary variants of basic group-theoretic transformations). Such simple exercises exhibit a tacit understanding of our environment, functioning as an initial view of our world as extended (in more or less locally uniform space), an initial view that can reasonably be thought an ancestor of our current conceptions of space, time, and motion via a (long and very complicated) series of empirical and formal revisions, especially when those revisions result from seeing how this local conception fares when put into new and surprising contexts (such as the celestial realm, high-energy fields, or at velocities approaching the speed of light).

Similar points have already been made in the literature, though they are decidedly more *dialectical* in character than the purely empirical project we have examined here. DiSalle and Friedman have offered the best dialectical accounts of theoretical and conceptual progress in the sciences (especially theories of space, time, and motion). By way of explication of some of the merits of an empirical approach to such questions, I will examine their dialectical positions in my concluding remarks. What are the differences between empirical revisionism and dialectical revisionism? How close might the two be in their historical and philosophical claims? We will argue that the gap is not wide at all, and extending Gupta's logical apparatus to questions about the historical development of theories of space, time, and motion might not only allow us to make distinctions between two broad types of dialecticism in the philosophy of science, but might also characterise (with more formal precision) some of the key transitions in the development of physical theory.

CONCLUSION

An Essay on Empiricism and Dialecticism

As we hope is made clear in the preceding chapters, this dissertation has two substantial parts. The first is critical, the second more constructive. In its critical part, we argue that two incompatible views emerge from Gupta's Reformed Empiricism. The first is that his view is an alternative to "classical empiricism" (construed as a tradition which includes not only CBE, but also MSE); the second is that Reformed Empiricism can be extended to account for our scientific knowledge without issue. The exegetical work and taxonomy are thought to motivate this critical thesis. The constructive part of the project proposes a way that we may amend Gupta's framework so as to make it more sensitive to questions of a formal and conceptual character, especially regarding how we may have license to a class of perceptual judgments which seem to rely, at least in part, on proto-mathematical presuppositions. We argue that initial views must satisfy the SE Principle if they are to be admissible initial views. We thus show that proto-mathematical commitments of a very elementary sort operate in our normal experience of the world as a space of interaction—i.e. that certain elementary proto-mathematical principles (empirical principles or physical principles with an elementary mathematical structure) are presupposed in views that allow for a (perhaps only loosely) structured space of experience. While we agree with Gupta that we may set aside issues pertaining to the specific interpretation of our mathematical knowledge (and the classical foundational questions about how such interpretation is to go), we identify principles (or, rather, a set of possible elementary proto-mathematical principles consistent with the SE Principle) which are relevant to empirical judgment (especially judgments involving spatiality).

According to such an account, Reformed Empiricism, we argue, is preserved. Also, Reformed Empiricism has the resources to resolve some classical difficulties with MSE. For example, because we have maintained with our amendments the logical recategorisation of experience, the famous protocol-sentence debate within

the Vienna Circle, with its secondary hypotheses about *physicalism* (to facilitate communication amongst scientists) and corresponding epistemological theses regarding the fallibility of such statements, is now entirely avoided. Another example, namely empirically motivated theoretical revision and theoretical development in the sciences, will be our main focus below.

For now, a general remark about the wide applicability of Reformed Empiricism is in order. Reformed Empiricism gives an account of empirical judgments, which means it can be extended to the philosophy of science (with only slight modification). In short: Reformed Empiricism is a comprehensive empiricism; it makes clear the ramifications of adopting a comprehensive framework to explain all of our knowledge.

Like empiricists before him, Gupta utilises all of the most modern technical tools to address epistemological questions, preserving the merits of previous empiricist projects without succumbing to the folly hidden in their logical assumptions. Where this dissertation differs with Gupta's own account of his theory is in emphasis. What is new in Reformed Empiricism is not its critical suspicion of Cartesian foundationalism, as Gupta thinks (though his particular critiques of Cartesianism are innovative); nor is Reformed Empiricism the first to think that experience by itself is epistemically neutral (i.e. experience alone does not provide us with epistemic license to a set of perceptual judgments). Anti-foundationalism and the neutrality of experience were theses already held by many of the logical empiricists, and (before them) Poincaré (to the extent that he fits into the MSE tradition). As the remarks in Chapters Two and Three will have made clear, Reformed Empiricism is not the first normative empiricism to question these Cartesian theses and themes, and thus Gupta may find fellow travellers amongst the MSE tradition, with its focus on a new kind of empiricism: one which had already adopted as a central desideratum a sensitivity to the most pressing concerns in the formal and physical sciences, and which set itself the task of developing new frameworks within which these concerns (as well as those concerns of classical British empiricism worth saving) could be fruitfully addressed.

So, then, what is truly *sui generis* in Reformed Empiricism, and why, over the past five years, has it become the focus of so much scholarship? Firstly, as many critics have noted, even if it fails as an empiricist account of knowledge (which we have argued in Chapters One and Three is an incorrect view), it will have nonetheless offered us a novel way of conceiving the relationship between experience and knowledge, one hitherto hidden from the view of normative epistemologists entrenched in their assumption of the propositional character of the given. Secondly (though relatedly), Reformed Empiricism shows the cleavage between incidental features of particular brands of empiricism and what we may consider a minimal form of empiricism—the type of empiricism that follows from the truisms (minimally construed) and the constraints. Gupta allows us to see the various ways in which the Insight, Multiple-Factorizability, and the constraints (especially *Reliability*) have been too strongly interpreted (based on a Cartesian conception of experience and the *way of ideas*). This negative part of Gupta's work is itself of great importance for epistemology and the history of philosophy. Thirdly, Gupta is able to uncover the very intricate ways in which our existing beliefs organise our enquiry, and establish our expectations for experience, thus preserving what is true in the theses of doxastic holism and the "theory-ladenness" of perception—without assuming that experience itself is infiltrated by (and thus subordinate to) our existing conceptual frameworks—though, as we have seen with the SE Principle, this does not preclude the necessity to factorise experience in accordance with some reasonably rich, though not determined or fixed, conceptual framework.

But very importantly, Gupta's most significant contribution to empiricism is found in his utilisation of contemporary logic. The logical framework is that of interdependence, sketched in Chapter One, for which Belnap and Gupta have made the most significant contributions. The logic of interdependence, we have maintained, is a technical innovation which marks a sea-change in the analysis of experience, and a sea-change in empiricism—it, more than any other technical innovation in the past century, allows for a proper logical account of experience,

which (when wedded to the chorus of anti-Cartesianism already alive in the MSE tradition) marks exciting new territory for the empiricist to explore, and a new framework within which the empiricist/rationalist debate can be conducted in such a way that the rationalist doesn't simply win the day because of the way empiricism is interpreted. (This, more than anything, is the true virtue of the work: one on which Gupta continues to place his focus, but which, as we saw in the first half of Chapter Three, critics continue to overlook.)

§I: Reformed Empiricism and the Sciences

The promise of this essay is not a general defense of Reformed Empiricism, but an attempt to extend the view to recalcitrant problems in the philosophy of science. The general extension of the theory has already been framed within the context of two such problems, as we explored them in Chapter Three: the *problem of our formal knowledge*, and the *problem of the communicability of observation*. The problem of formal knowledge is this: at least some revisable proto-mathematical principles of spatio-temporality, motion, and iteration (of a completely elementary sort) must be part of our view of the world in order for experience to converge upon normal and scientific judgments about spatial position, orientation, and displacement—indeed, must be in place for us to even be in a position to make a large class of judgments about spatial position, orientation, and displacement whatsoever. We saw that something like a primitive version of the principle of free mobility, perhaps we could call it "quasi-free mobility", provides for us a revisable notion of space that, at least at the beginning of enquiry, is homogenous, with some vague idea of size constancy.

The problem of communicability of observation is far more complex. Stability and convergence are the sorts of properties we hope to see in the revision of our scientific concepts and theories, not just in our common sense concepts and views of the world. As we have seen, although Gupta is largely silent on the issue, he does hint that his account for general epistemology would be applicable to conceptual and theoretical changes in the sciences. As we also have seen, the extension of his

account from general epistemology to the epistemology of science is not immediately clear. It would be fortunate if there were a clear correspondence between the general epistemological case and the scientific case (between experience and view as they are in general epistemology, and observation and theory as they are in the philosophy of science). There is a *prima facie* similarity between scientific observation and normal experience, but observation is often far more restricted than normal experience. (One need only think of even the most basic methodological restraints on observation, like those of Bacon or Mill, to see that experience and observation are not equivalent.) Also, experience belongs to a single cogniser—though, as we saw in Chapter One, §I.2, two (or more) epistemic agents may have subjectively identical beliefs. This is different from scientific "observation", where often observational data are collected by a number of different practitioners, working over extended periods of time. What about views and theories? They too seem to have a *prima facie* similarity that fades on further reflection. View are not communal things by their nature, even though many agents can come to have fundamentally similar views. However, theories are communal. This is because all but the most toy theories quickly become extremely complicated. Unless we assume scientists are perfectly rational agents with infinite imaginations, no restrictions on memory, etc., we can see that a working scientific theory will require social coordination of data gathering, interpretation, and theoretical revision. As a result of the complexity of scientific theories, we cannot rely on the experience of one cogniser, nor can we think it adequate to view theories as things possessed by individual practitioners (in the way that a view may be so construed). This means that we need to describe observation in such a way that its rational significance is communicable to others, precisely what Gupta says we cannot do (since the given in experience is a function, not a proposition).

The SE Principle helps overcome both of these difficulties when added to the truisms, constraints, and admissibility conditions of Reformed Empiricism proper. Because it demands that every admissible view include at least the prerequisites for some space of interaction within which empirical judgments may be embedded, all

views have at least some minimal and revisable proto-mathematical presuppositions. Even the most primitive local perspectives define an objective local space that allows for interaction with other perspectives that may lead to something that begins to look like a theory of space. What matters most, is that we are all able to have a spatial perspective on the experience of those we communicate with (that we are able to make judgments about how the world must appear to them). Of course, they must be able to consider our experience in the same way for communication to be possible. In this way, our understanding of space (even our primitive understanding) is communicable in a way that perhaps the rudimentary experiences of space we share with animals is not—in spite of the fact that some animals also surely anticipate the experience of others, e.g. predators, though not in a way that can be marked and codified.

In this way, we can see how local judgments about the tendency for heavy objects to move downward and for fire to move upward may have come to converge on something like the Aristotelian conception of the universe, where earthen bodies are attracted to like substance (with its natural elemental place at the centre of the universe), and for fire to move upward to its rightful place between the Earth and the Moon. It is barely a jump from this point to assume that, because the stars don't fall toward us, then they must be made of a different element: not Earth, Air, Fire, or Water. Such judgments become public without difficulty, in spite of their misconceptions. These normal, intuitive judgments are then called into question when local perspectives are compared to relative motions. Under such an examination, Aristotle's theory of motion (and its causes) breaks down. If Aristotle is correct, then we should not observe relative motions as we do. This much is shown in Galileo's exposition of the classical notion of an inertial frame of reference in his famous thought experiment of an observer below deck of a ship, in which it makes no difference to the relative motion of free-falling bodies—or other accelerating systems—whether the ship is at rest or in uniform motion. Because Aristotle's theory of motion was central to bringing together local perspectives into

one worldview, that worldview is now in question, or at least unmotivated, lacking the metaphysics that gives it sense.

As such preliminary considerations show, the communication of observation does not require any specific physical theory be agreed upon (before hand) because of such empirical considerations. Of course, not all theories can be tested like this; however, our concern is merely with how experimental science may impact theory revision without having to first assume the theory which will be the output of said revision, *not* that all theories are in fact empirically testable.[64]

A further remark on common sense views and scientific theories: Views and theoretical perspectives also seem to differ in the degree to which they are extended to new (and possibly surprising) contexts. Whereas we are often surprised by our normal course of experience, finding that it guides us to revise our common sense view of the world in this or that way, it is not normally a part of our views (or, at least, not most views) that we direct our experience with the active, conscious intention of extending our current view to different contexts (be such contexts new regions of space, high energy fields, the very small world of the quanta, or ever larger reference frames beyond our locality, our planet, our solar system, and our galaxy). This is exactly what scientific experiment (and critical examination of scientific concepts) is meant to do—to design scenarios that extend scientific concepts to new domains that may be quite different than the domain(s) in which the theory was developed. Whereas experience always has the potential to alter our views of the world, our views are very rarely directed at such extensions.

This is not to be confused with "falsification"; that specific thesis holds that we produce meaningful and useful theories only if there is a clear (if only implicit) indication of how such claims may be defeated by experience. Our claim here shares with that claim the idea that scientific theories are only acceptable if defeasible, but

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[64] Some theories may not be empirically testable for practical reasons (e.g. cosmological theories which cannot fix variables in the right way so as to be amenable to any kind of experiment), or different theories about the basic constitution of matter (which can perhaps only be "tested" based on their internal logical consistency, and other regulative principles like simplicity). Some theories may only differ counterfactually, and they may be empirically equivalent as an accident of how our world happened to turn out. These sorts of issues are not of our concern here.

it does not share any specific doctrine of meaning or epistemic significance. The present claim is rather a methodological one, inherited from Newton: one should develop a framework within which the phenomena can be properly characterised; the true value of this framework is found in its stability as it is applied to more and more contexts for which it was not designed.

In this way, scientific theories can be understood as attempts to preserve our common sense perspectives, or, better still, attempts to make such perspectives *fit* into new scientific conceptions. Theories show how such intuitive views, while locally plausible, may not extend to all circumstances and all cases without alteration. They show how our naïve conceptions about space have remained locally plausible (space as having privileged "places", and privileged directions like "up" and "down") in spite of their often revolutionary re-contextualisation within ever more sophisticated theories (like Newton's physics, or even special and general relativity) that reject such concepts because they have no physical application. Normal views are notorious for employing such local and perspectival concepts as if they have a more than local and perspectival character. More often than not, such assumptions are innocent. These views will still allow us to generate perceptual judgments with no real difficulty—judgments about which we can have much agreement. (Teammates of a football player who are watching him attempt to beat defenders and pass the ball will do well to anticipate his perspectival experience, even if modern physics tells us that there is a fundamental misconception at work in thinking that "up", "down", "left", "right", "behind", and "forward" are real things.) Theories set themselves apart by imposing a rigorous framework that attempts to rid itself of these parochial vestiges, applying in a unified and systematic way to more and more phenomena, though much information about the world we inhabit can be got at, in spite of (and often even *because of*) the misconceptions.

§II: The Arbitrariness Problem and the Progress of Scientific Theory

In what is to follow, we will extend Reformed Empiricism to another problem that has plagued post-positivist philosophers of science: a conception of rational

progress in the sciences, especially in theories of space, time, and motion. As we have seen, the logical empiricist theory of theories attempts to account for the flexibility of our scientific knowledge, but at a high cost. Logical empiricism accounts for the radical changes in 19th century geometry and early 20th century physics by positing theories as relative *a priori* frameworks against which "internal" questions can be asked. The move from one framework to another becomes an arbitrary, free choice. This account of our theoretical knowledge shows it to be analytic (i.e. an innocent by-product of our choice of formal framework), but the arbitrariness of the choice of formal framework belies the historical development of the sciences. Call this the "arbitrariness problem"—*there can be given no empirical justification for the move from one theory to another, since such justifications are always specific to the formal framework of a given theory*.[65]

II.1: Arbitrariness Expounded

In connection with theory change in science, much has been said about the testing of rival theories by observation and experiment. However, the generation of new (or substantially revised) concepts was largely relegated by traditional logical empiricism to psychology. Using Reformed Empiricism as our guide, we argue that something philosophically interesting can be said about conceptual change that accompanies theory change in the sciences, and that such matters have much more to do with empirical justification than the psychology of practitioners. An account of conceptual change in the sciences that attempts to capture our generation (or substantial revision) of concepts in a way that allows for conceptual innovation to be empirically justified can be offered—one that is both historically accurate to the development of modern physics and which follows the logic of interdependence.

[65] Whereas Kuhn interprets the arbitrariness problem as "inter-paradigm incommensurability", one need not read it so strongly. In fact, the logical empiricist tradition viewed the choice between theories as a pragmatic choice, one for which arguments could be made for the choice of theory relative to one's aims, and relative to epistemological considerations; however, such arguments did not have the kind of rigor exhibited by properly theoretical justifications. Nonetheless, the logical empiricists held that theory choice was arbitrary by our definition, since there were no empirical considerations that could rationally force such a choice.

The problem of conceptual innovation in science has a history that goes back to the roots of logical empiricism. It was given form in Reichenbach's distinction between the context of discovery and the context of justification. Theoretical principles impose an interpretation of a given concept (e.g. "straight line") even if that concept already has a received, intuitive meaning. The principles (implicitly) define a given concept, but only relative to a theoretical framework; the task of epistemology is, first and foremost, the isolation of those theoretical principles that allow one to employ the theory to make empirical predictions and explanations of natural phenomena. One can empirically justify a concept, but only relative to the overall empirical success of the theory as a whole. This presupposes that the concept in question can be completely defined relative to the theory itself, and therefore that the theory is already complete. This is surely one way of empirically justifying a concept. However, this procedure is inadequate if we wish to analyse the empirical application of a concept (or set of concepts) without the aid of a completed theory. An explanation of how such a concept might be arrived at was thought by the logical empiricists to exceed epistemology, at least partially—requiring recourse to some psychological faculty such as imagination, representation, intuition, etc. Such an account would necessitate a move beyond the framework of a given theory—since we attempt to justify the generation or revision of a concept, say, before a theory in which it features is complete—and thus beyond the reach of epistemic justification via theory application.

Thus, while we can show that the logical empiricists developed an extremely insightful and nuanced theory of theories, their focus was mainly placed on the role of a given theory as constituting the object of said theory, so that epistemological questions become significant relative to a choice of what Reichenbach termed "axioms of connection" and "axioms of coordination."[66] Such choices ought to be

[66] Reichenbach first introduced these notions in his [1920/1965]. Axioms of connection are empirical laws that employ empirically well-defined concepts. Axioms of coordination are principles which offer an empirical interpretation of the theory. Thus, what is assumed *a priori* valid by one theory may be an empirical question in another. In classical mechanics and special relativity, the metric of space-time is Euclidean, and assumed as an axiom of coordination. In general relativity, the space-time metric is empirically verifiable, and it is an empirical question, based on matter and energy distribution, whether space is flat or not.

seen as constituting the relativised *a priori* against which empirical claims can be justified. Similarly, Schlick held that certain conventional choices needed to be made regarding constitutive *a priori* principles of a given theory.[67] Thus, the logical empiricists, contrary to many of their critics, did develop a comprehensive epistemological account of scientific theories and their relevant differences, but such an account is inadequate as an *empirical* account of theory change. Logical empiricists held that concepts cannot be evaluated *in vacuo*, rather only as part of a change in theoretical framework, and changes between theoretical framework are never rationally forced by experience.

Given that the logical empiricists held the relevant concepts of a theory to be defined only relative to that theory, we should suspect this result. However, we argue that this is a false dichotomy—there are ways in which a concept can be applied to generate empirical judgments that do not necessarily owe anything to an already completed theory, but which are also not the mere *sui generis* intuition of an otherwise inspired mind. We argue that dialecticism offers just such a middle-way between completed theory and no theory at all.

II.2: Obstacles to a Conception of Rational Theory Change

If we wish to propose an account of how we develop some of our scientific concepts, at least two obstacles must be overcome. The first obstacle has often been falsely identified with theoretical "incommensurability". The difficulty it proposes is better captured by what we call the *Non-Evaluability Thesis*:

> *Non-Evaluability Thesis (NE thesis)*: there are no rational grounds on which to compare empirical theories.

This obstacle is largely a chimera. The problem posed by the NE thesis is negotiated by the logical empiricists via a comparison of the adequacy of the coordinations on which differing theories rest. Also, Confirmation Theoretical accounts of the relative impact of evidence on disparate theories have not only shown that theories can be

[67] See Friedman [1994/1999] on the relation between Reichenbach's relativised *a priori* and Schlick's conventionalism.

mutually evaluated, but that differences in evidence can even be quantitatively (i.e. probabilistically) measured. Some theories accord better with the evidence than others, and this is an empirical fact. Such comparisons are not restricted to dealing only with the rational apparatus provided by this or that theory, but rather encompass a wide range of empirical views.

Even though logical empiricist and Confirmation Theoretic accounts of theory choice offer us empiricist justifications of concepts given a complete theory, neither overcomes our second obstacle. This obstacle is best expressed by the following thesis:

> *Conceptual Application Thesis (CA thesis)*: the application and empirical revision of existing concepts can never be justified outside of a theoretical framework.

Comparison of views does not seem to recover for us the process by which we revise some of the most central concepts of the theories under comparison. If we are to have an empiricist account that responds to both theses, then a view that complements logical empiricist and Confirmation Theoretical responses to the NE thesis must be found. In short, we must have an adequate empiricist response to the CA thesis, an obstacle to any account of how concepts can be empirically justified outside of the framework of a theory.

Thus, we see that *arbitrariness* is not merely a problem about the rationality of theoretical progress, but also a putative limitation regarding the scope of our conceptual abilities, and a related severe limitation regarding the flexibility of our concepts. Empiricism has always had such a tendency—to explain our great conceptual flexibility in matters common sensical and scientific by fixing the set of concepts given in experience. This tendency became manifest in Russell's concept-empiricism, the view that central concepts of physics—space, time, matter, and causality—can be logically constructed using concepts pertaining to sense-data.[68]

[68] For a distinction on Russell's epistemological aims, see Grayling [2003]. Grayling draws the distinction between epistemology as a task of justifying knowledge claims on the one hand, and "as being an explication of the relation between what the claims are about and the nature of experience." (450) Russell, according to Grayling, was much more concerned with the latter task.

Notions such as space, time, matter, and causality are not transcendentally presupposed in order to make possible any empirical knowledge, in fact such concepts are actually "constructed". For example, while we may never be acquainted with matter in its metaphysical reality, we need not dogmatically assume it to be real in order to do physics. Instead of such dogmatic assumption of "real" matter, one

> can construct a logical fiction having the same formal properties, or rather having formally analogous formal properties to those of the supposed metaphysical entity and itself composed of empirically given things, and that logical fiction can be substituted for your supposed metaphysical entity and will fulfill all the scientific purposes that anybody can desire. (Russell [1918/1998], 144)[69]

In this way, Russell believes philosophy can do away with the assumptions of dogmatic metaphysics as well as Kant's Transcendental Aesthetic: space and time become empirically constructible concepts. Our experiences of sense-data, which are immediate and therefore have a "primitive certainty", can then be coordinated with physical objects in such a way that our coordinations can be mistaken, re-thought, or at least further examined (tasks that modern physical theory may do), while still being grounded in an act of sensation which secures for us objects about which we cannot be mistaken.[70]

But such an empiricism does great harm to both the Insight and Multiple-Factorizability. It transgresses the Insight because it assumes that there is a set of basic concepts that cannot be revised in light of experience (since they are provided

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[69] This basic idea was proposed in a number of essays produced by Russell after 1910. See Russell [1912/1997], especially "the Existence of Matter" and "The Nature of Matter; Russell [1914/1963]; Russell [1914/2009]; and Russell [1915/1963].

[70] As we saw in our discussion of the Russell/Poincaré debate (Chapter Three, the end of §II), Russell's view is much stronger than conventionalist accounts of geometry. Russell thinks that one of the things we can be acquainted with are specific objects (sense-data), and thus there is a geometry in the given of experience. Poincaré thought that sense-data had no such epistemic primacy; even given a manifold of experience, certain assumptions needed to be made, most significantly the principle of free mobility. We also saw that Schlick, adopting Poincaré's conventionalism, criticised Russell's "knowledge by acquaintance" for assuming a kind of knowledge that did not need to be subsumed under concepts that only get their meaning in a system of inter-related judgments. See Chapter Three, §III.

by experience itself). It transgresses Multiple-Factorizability since it holds that there is a privileged factorisation of experience—the necessarily incorrigible and immediate data of sense. While such a picture may explain how we apply concepts without a particular theory at hand (in fact, it shows us how we can use the concepts of the immediately given to construct the concepts of physical theory, one-by-one), it does not do so in a way consistent with our project here because of the transgression of the truisms. We will see in the next section that only a move to dialectical accounts of conceptual application and conceptual revision can satisfy the truisms (and constraints) whilst also offering a response to the NE and CA theses.

§III: Dialecticism in the Philosophy of Science

A dialectical approach to theory change in the exact sciences holds that the arbitrariness problem is misconstrued. Arbitrariness follows from a far too rigid and formal account of what a given scientific theory is: a collection of sentences, each with a distinct role. Some sentences express the underlying logico-mathematical assumptions (and may be common to theories with different empirical interpretations). Such sentences are analytic. Other sentences determine the empirical application of the theory. These are likewise analytic (the so-called "formal auxiliaries" of one's preferred framework or language), though they are coordinated with properly empirical sentences. Lastly, there are those sentences which are truly empirical—those statements which are discovered true or false by observation. This view of theories comes out of the logical empiricist's commitment (especially amongst the left wing of the Vienna Circle) to remove from philosophy fruitless debate on metaphysical matters by replacing philosophy (as it had traditionally been practiced) with the program of rational reconstruction—the construction of formal systems which could then be proposed as the language of science. All scientific languages have these three parts.

Rather than engaging in the rational reconstruction of theories (in the logical empiricist sense of "reconstruction"), dialectical approaches seek to establish the sorts of scientific and philosophical arguments that have occurred in (and perhaps

beyond) the sciences—to show the various ways in which scientists (and others) have engaged in conceptual analysis, philosophical debate, and empirical enquiry to the end of reaching some kind of rational consensus regarding the structure of theories and concepts and the applicability of formal systems to physical phenomena. Far from there being some sort of incommensurability between theoretical frameworks, the history shows the nuanced ways in which reflection upon empirical evidence, thought-experiments, and fundamental concepts has been the cause for conceptual change in the sciences—even of a revolutionary sort. Here, we wish to explore two very different ways of carrying out this project: Friedman's neo-Kantian dialecticism and DiSalle's more narrow dialecticism. We will hold that while both adequately address the arbitrariness problem, speaking to the NE and CA theses, only DiSalle's more empirically sensitive form of dialecticism can be thought to satisfy the truisms and minimal constraints of Gupta's empiricism. In fact, we will argue that there is a very happy congress between Gupta's Reformed Empiricism and DiSalle's account of the history and philosophy of modern theories of space and time—Reformed Empiricism actually makes perspicuous the logic underlying empirically sensitive dialecticism. This means that insofar as DiSalle offers a correct picture of the dialectically rational progression of modern physics, Gupta's logical machinery can be used to account for the rationality of said progress.

III.1: Friedman's Wide Dialecticism

In his [2001], Friedman argues that philosophy of science acts as a dialectical space in which discussants (both scientists and philosophers) can propose different *a priori* principles (and thus, theoretical frameworks) for the study of nature. Friedman argues that the traditional role of philosophy of science, as exhibited by Kant's adherence to constitutive *a priori* principles and the logical empiricists' adherence to conventionalism and Tolerance, served a similar purpose: to provide meta-level discussion on a number of salient topics in the history of a given discipline in order that one can evaluate proposals of constitutive, coordinating principles that give physical theory (for Kant) or theor*ies* (for the logical

empiricists) an empirical interpretation. Kant thought the matter settled with a thorough account of the transcendental conditions which give Newton's physics its object, but such conditions were found to be too restrictive given the mathematical and physical developments of the late 19th and early 20th centuries (surveyed in Chapter Two). The logical empiricists, while more flexible than Kant, also confronted intractable difficulty. Schlick's conventionalism, Reichenbach's relativised *a priori,* and the Carnapian notion of Tolerance, while possible accounts of the dialectical space in which conceptual debates can occur, are not strong enough to definitively block worries, says Friedman, about Kuhnian incommensurability.[71]

But according to Friedman, we need not surrender to Kuhn so easily. We can challenge Kuhn on his insistence that no intellectual discipline can be up to the task of hosting meta-level discussion of central theoretical concepts. Kuhn held that the sciences are exemplars amongst all intellectual disciplines in their ability to achieve "paradigm stability". Yet, this paradigm stability also entails paradigm *rigidity*, leading to the problem of incommensurability. Kuhn's reasoning is represented by the following simple argument:

(1) If any disciplines are capable of quelling the instability of revolutionary moments, it is those that do not themselves exhibit paradigm instability.
(2) The most stable sciences are unable to discuss meta-level questions about their conceptual differences;
(C) Therefore, such questions are not answerable.

Friedman argues that this is an invalid inference. From the fact that the sciences are the most stable sorts of intellectual enterprise we have, we ought *not* infer they should be up to the task of showing how theoretical progress and conceptual revision are possible. Rather, the *lack* of paradigm stability in a discipline like philosophy makes it (not the stable sciences) a more likely candidate to revise or

[71] Kuhn's notion of incommensurability is actually a semantic variant of arbitrariness and the NE thesis. For Kuhn, radically different scientific theories model the world in different ways, and there is no paradigm neutral space for inter-communication between these theories, since there is no basis on which we can translate one theory into the terms of another. (The actual employment of this concept in Kuhn's work is not nearly so coherent as Friedman assumes it to be.)

generate central theoretical concepts. Philosophy provides the space for conceptual innovation, where philosophical arguments are provided for the development of concepts that are outside of prevailing scientific theory and practice (at least explicitly). Science is then free to develop a more rigid theory around the concepts once they have been proposed.

The existence of such philosophical dialogue provides a kind of communicative rationality that refutes the CA thesis and its Kuhnian cousin: incommensurability.[72] For Kuhn, debates on matters of deep principle are inevitable and necessarily inconclusive, since one's attitudes are determined by the philosophy in question (along with other "subjective" factors). Friedman's view sheds light on why this Kuhnian account is not true. The development of some of our central theoretical concepts is done outside of rigid theoretical frameworks, but it is (at least in some sense) *rational*—it inherits the communicative rationality of the meta-level debates between "philosophically-minded scientists" and "scientifically-minded philosophers". This is a kind of rationality that Kuhn does not adequately address, but it does a great deal of the heavy lifting in Friedman's explanation of the rational character of scientific progress.

Philosophy of science (though philosophy more generally, and even the humanities and social sciences more generally than that) thus have a key role to play in ensuring that scientific progress can occur (that scientific "paradigms" do not become too stagnant), and that such progress is organised by the regulative demands of reason to always be engaged in a revision of one's scientific theory (once again, to avoid stagnation). The following passage sums up the wide dialectic that continually occurs between philosophy and science:

> Science, if it is to continue to progress through revolutions… needs a new source of ideas, alternative programs, and expanded possibilities that is not itself scientific in the same sense—that does not, as do the sciences themselves, operate within a generally agreed upon framework of taken for granted rules. For what is needed here is precisely the creation and

[72] Of course, Friedman believes he is responding to the problem of incommensurability, not the more precisely formulated NE and CA theses.

> stimulation of new frameworks or paradigms, together with what we might call meta-frameworks or meta-paradigms—new conceptions of what a coherent rational understanding of nature might amount to—capable of motivating and sustaining the revolutionary transition to a new first-level or scientific paradigm. Philosophy, throughout its close association with the sciences, has functioned in precisely this way. (Friedman [2001], 23)

Under such a picture, central parts of Reichenbach's context of discovery (specifically the parts which require the free play of inspiration from some non-epistemic source to explain conceptual innovation and revision) are replaced by philosophy itself, which structures "discovery" as the product of a certain kind of debate that follows certain strictures of communicative rationality.

Friedman here exploits the cleavage between Kant's conception of constitutive *a priori* principles and merely regulative principles. For Kant, constitutive *a priori* principles offer "rules of synthesis" which determine the apprehension of an object in appearance, and which also fix for us the object of science. Constitutive principles supply a concept of a real object. (Kant [1781/1787/1998], A306/B363; A648/B676) In this way, Newton's laws of motion determine the object of Newtonian physics; the laws are the necessary conditions for the very possibility of objective knowledge. However Kant also held that reason also makes demands upon us of a non-determinative or non-constitutive sort: what Kant called *regulative principles.* Regulative principles (which Kant introduces in the Appendix of the Transcendental Dialectic) are those principles (or ideals) of reason that demand a "systematic unity". Here, Kant is arguing that the understanding's ability to think of an object owes much to reason's demand that such thought be unified, especially if such thought is supposed to proceed from an unsystematic sort to scientific theory. (Kant [1781/1787/1998], A651-52/B679-80) Knowledge is unified systematically by reason's demand that we find fewer and fewer laws under which empirical phenomena can be subsumed. The ideal of reason here is a unified knowledge, or a "whole of knowledge", which will systematise all contingent judgments about matters of fact (supplied by the understanding) so that they may be coherently held together, as branches of the same science, nomically connected.

Of course, for Kant (and later neo-Kantian thinkers like Cassirer), such an ideal can never be reached. Its value lies in ensuring that science is always in a position to progress, not in terms of its constitutive principles, which are entirely fixed as we have observed, but in the sense that regulative principles bring with them a kind of theoretical convenience. Without unification, and the related assumption that nature conforms to our demands for unification, our scientific researches would lack focus and direction, and, as Friedman points out, they would quickly become stagnant or impossible. Regulative principles are even more important in those sciences which Kant thought to lack constitutive principles, especially chemistry and biology—here, it is the unification of theory that not only gives such studies direction, but forms for them a revisable understanding of their objects.

What Friedman proposes is this: reject constitutive principles (in Kant's sense) altogether, and replace them with regulative principles when thinking about the sciences. No longer is physics beholden to a determinate object based on Newton's laws, rather science becomes a succession of historically related revisions of physical theory convergent upon regulative ideals of simplicity, unity, and systematicity. (Friedman [2001], 64) This account of theoretical progress takes it as wrote that deep conceptual revolutions or "paradigm-shifts" occur in much the same way as Kuhn thought them to; it adopts as its task the offering of some account of why such shifts in paradigm do not lead directly to the arbitrariness problem—why such shifts aren't merely instances of "conversion". It admits that there are no constitutive principles in Kant's sense—no necessary, apodictic, universal principles which determine (once and for all) the features of objects (or of the necessary spatial and temporal background in which we come to represent said objects).

But with the loss of such necessities also comes relativism (or so the worry goes). Friedman's solution: in place of constitutive principles of the Kantian sort, we substitute the regulative principle that the evolution of scientific thought itself is approaching an ideal, and that at each stage we may revise those principles

constitutive of our current outlook. Friedman phrases it as a matter of convergence (though in a very different sense from Gupta's notion of convergence):

> We can imagine... that our present constitutive principles represent one stage of a convergent process, as it were, in that they can be viewed as approximations to more general and adequate constitutive principles that will only be articulated at a later stage. We can thus view our present scientific community, which has achieved temporary consensus based on communicative rationality erected on its present constitutive principles, as an approximation to a final, ideal community of inquiry (to use an obviously Peircian figure) that has achieved a universal, trans-historical communicative rationality on the basis of the fully general and adequate constitutive principles reached in the ideal limit of scientific progress. (ibid)

In fact, we must employ this regulative use of reason "for only so can the required inter-paradigm notion of communicative rationality be sustained". (Friedman [2001], 65)

According to Friedman, this regulative ideal should be manifest in historical instances of paradigm-shift, so that it satisfies three criteria. Firstly, the new conceptual framework should contain the conceptual framework of the old theory "as an approximate limiting case". (An example is the way that Newton's system is the limiting case of taking invariant velocity *c* to infinity, or collapsing the past and future light-cones together as surfaces of absolute simultaneity.) Secondly, "that the new constitutive principles should also evolve continuously out of the old constitutive principles, by a series of transformations"; and thirdly, "this process of continuous conceptual transformation should be motivated and sustained by an appropriate new philosophical meta-framework..." (Friedman [2001], 6) Since there are any number of possible transformations at any point in this convergent process, and since many roads may approach the ideal, there is nothing necessary about the nature of our scientific development. (Friedman [2001], 68) The progress of the sciences is perfectly contingent, and we cannot read off of our currently dominant conceptual frameworks and philosophical debates what the new transitions will be; at best we have "educated guesses" for how the next step will look.

It is clear that Friedman keeps the arbitrariness problem at bay, and that he argues successfully against the NE and CA theses. The NE thesis is successfully refuted because all new conceptual frameworks contain the old conceptual framework at some well-specified and defined limit. Not only can different conceptual frameworks be compared, but, as a matter of fact, new conceptual frameworks must always possess the ability to construct the frameworks they are replacing within their own systems. What such findings show is that there is at least no such thing as what Friedman calls "retrospective irrationality" (one half of Kuhn's notion of incommensurability): there are no new scientific conceptions of the world which make the understanding of a previous paradigm impossible. The other half of Kuhnian incommensurability, what Friedman calls "prospective irrationality" (how those working within a previous paradigm might view a new paradigm) is refuted because new conceptual frameworks grow out of (are a continuous transformation of) old conceptual systems. Here, Friedman gives as his example Einstein's formulation of the principle of the constancy of the velocity of light against the backdrop of the indistinguishability of different inertial frames via optical and electrodynamical considerations. As Friedman argues, Einstein merely raises it to the status of a "coordinating principle", calling into question the classical coordination based on the laws of motion and the possibility of the propagation of a causal signal with no upper limit. (Friedman [2001], 102)

Friedman also claims that Einstein did something very similar with the principle of equivalence. All physical theories going back as far as Aristotle held that it was a very well-established fact that there must be some fundamental state of natural motion, and that deviations from that motion must be caused by some force. (ibid) Galileo transformed Aristotle's natural place and natural motion with a conception of inertia; the precise mathematical concept of force was then articulated by Newton in the context of his laws of motion (as a privileged inertial frame). As a matter of experiment, there is no difference between inertial and gravitational mass. Once again, Einstein "elevates" this empirical fact to the status of a coordinative principle. The gravitational field is physically equivalent to a

corresponding acceleration of the reference frame (and thus gravitation is no longer considered an external force). This radically transforms what may count as a "natural motion"—which is now uniquely determined for a given space by the trajectory followed by a particle in free fall, i.e. a "geodesic". But, in spite of the fact that a particle in free fall cannot constitute a natural motion in classical physics, Einstein has framed his coordinative principle in such a way that practitioners starting from a classical model could make perfect sense of general relativity (and be in the position to adopt it not merely as an excellent black-box predictor, but as a physical theory they grasp as having evolved from their classical model).

The above considerations offer a refutation of the CA thesis as well, for the development of some particular concepts may very well be theory-bound, but a great many concepts (such as *natural motion*) are not bound to any theoretical framework—they rather characterise the neutral "facts" countenanced by all relevant theories, including those with radically different domains. We develop these concepts in an evolutionary way, building meaning upon them as we go, always striving toward some ideal and always respecting previous applications of the concept. Convergence on a common picture of the world is thus the never completable *telos* of theoretical science—including, we may assume, consilience amongst different domains, should they employ some of the same concepts. Agreement increases certainty.

Torretti has built upon this general account of consilience, arguing that it produces effects beyond the mere belief that some inductions may be more certain than others. Torretti conceives of consilience as a convergence process of different domains, the effects of which seem *irreversible*. (Torretti [1999], 222) Consilience brings unifications that collect and explain the phenomena in such a way that we cannot simply "go back" to a time before such systematisation, nor can we imagine introducing an entirely new concept (in an entirely new theory) which did not cover the extension of the concept as convergence has established it to this point. Torretti uses the example of gravitation:

> ... no one today would dream of placing different subsets of the so-called gravitational phenomena under disparate conceptions. We naturally expect every purported successor to Newton's theory of gravity to account for the fall of heavy bodies *and* the circulation of the planets, not forgetting the tides and the precession of the equinoxes... (ibid)[73]

However, one rather large disanalogy between Torretti's neo-consilience conception of convergence and Friedman's neo-Kantian use of regulative ideals is that Friedman's project is, as we have indicated, teleological, whereas Torretti's avails itself only of conceptual critique and inductive consilience. The rationality of progress in the sciences is *anticipatory*, not regulative; it is based on the fact that we have seen unifications bring more and more phenomena under a smaller and smaller set of principles and concepts, not a by-product of some regulative use of reason and its accompanying model of communicative rationality that holds that such unifications must be sought.

Torretti does not (to our knowledge) explore this non-teleological model of consilience in detail. However, DiSalle proposes what we take to be a cognate theory—a dialectical account which dispenses with reference to regulative ideals by showing that "revolutionary" moments in the history of physics have actually been well-founded responses to existing (though perhaps not yet well-defined) problems based on uncontested empirical claims and the presuppositions of those concepts required by physics. By taking focus away from the historically distorting grand narratives and their endless quarrels (e.g. the absolute-relational debate), DiSalle is able to focus his approach on showing that dialectical arguments (in the philosophy of science) start with a principle that is in use, even if only implicitly, in the old scheme, only to show that the principle is incompatible with fundamental parts of that scheme. This more minimal dialecticism (or "narrow dialecticism", as we are calling it) offers a solution to the arbitrariness problem and a rebuke of the NE and CA theses (as did Friedman's wide dialecticism), but it does so without the excessive

[73] Of course, some quantum theories of gravity that use a flat background metric do attempt to undo the unification of inertia and gravity. The general rule still applies, even if Torretti's absolute language finds a counter-instance or two.

Kantianism that precludes Friedman's approach from preserving the Insight of Empiricism (since his view of rational progress does not provide the given in experience any rational role at all). We hold that this minimalism makes DiSalle's position amenable to a more formal presentation using the logical framework of Reformed Empiricism.

III.2: DiSalle's Narrow Dialecticism and Reformed Empiricism

Friedman's wide dialecticism is, as we have just seen, not only an account of the rationality of theoretical progress, but also an account of how it is philosophy factors into the historical and conceptual development of the sciences. DiSalle preserves the first desideratum, but not the second. For DiSalle, we have a "philosophy of space and time" not because philosophy operates as a more or less autonomous discipline in which grand debates about age-old questions can take place (as Friedman holds), offering us the kind of conceptual freedom required to bring about the next revolution in theoretical physics. DiSalle argues that the absolute-relational debate has played an important role in "motivating principles of physical speculation". However, it is not the central concern (much less the only concern) for DiSalle's researches. The debate plays a far more limited role in DiSalle's dialecticism than it does in Friedman's, due in no small part to a further concern: that a too narrow focus on this question alone has obscured much of 20th century philosophy of physics, and "clouded" a more nuanced understanding of the historical development of philosophical and scientific thought regarding the concepts of space and time. (DiSalle [2006a], 1-2)

Those debates don't explain the significant developments of theories of space and time. Firstly, they do not motivate theoretical progress, at least not directly, because (as Friedman shows) they stand outside of the physical application of the relevant concepts in question. The development of this or that concept does not itself motivate anything like a change in theory. What is required is some application of the concept—or some indication that the concept cannot be applied as assumed by the old scheme (such as the application of the implied Newtonian definition of

simultaneity in light of concerns about light signaling). Secondly, those debates are not, by their very nature, sensitive to empirical evidence. If Friedman is correct, then such debates must be beyond the reach of empirical evidence in much the same way that interlocutors in such debates are not constrained by the empirical application of concepts in particular physical theories.

By framing philosophy of physics through the absolute-relational debate, analyses (like Friedman's) have put far too much emphasis on the role of a more or less autonomous philosophical discussion as determinant in a field of study which has (as a matter of recent and not-so-recent historical fact) become more and more indebted to physical modes of thinking.[74] (We saw an early push in this direction in Chapter Two, where we quoted Mach's anti-metaphysical introductory remarks to his [1886/1887].) Our main focus is not, however, to make claims about the direction of 20th century philosophy of science, but to argue (*pace* Friedman) that changes in scientific theories are instances of empirically-driven conceptual revision, not an application of general philosophical principles.

Friedman has missed a critical point to which DiSalle draws our attention: we have a philosophy of space and time not because philosophers made physics more philosophical, but because physicists (over the course of 500 years) made

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[74] For this reason, some of the remarks in Nick Huggett [2009] seem to miss the mark. Huggett fails to acknowledge the very real differences in the use of Kantian and positivistic themes regarding a prioricity in Friedman's and DiSalle's accounts, thinking them to be entirely complementary. Here is an example:

> In this project DiSalle is closely in step with Friedman's [2001] neo-Kantian program, which also seeks to show how changes in the most fundamental principles of mathematical physics can [be] made by a process of rational deliberation–indeed exactly the process described by DiSalle. (The connection is no coincidence; both authors cite the other's influence.) (Huggett [2009], 414)

While the connection is certainly not coincidental, we must not let it draw our attention away from the fact that DiSalle avails himself of much less of the Kantian framework, and much less of Reichenbach's idea of a relativised *a priori* than does Friedman. A lack of focus on Huggett's part also means he conflates DiSalle's narrow dialecticism with Friedman's wide dialecticism, asserting that DiSalle is proposing (like Friedman) that the primary dialectic is between physics and philosophy. (Huggett [2009], 405) This is not the case. DiSalle holds that science fruitfully engages in a dialectic with empirical discovery by using some philosophical tools (i.e. conceptual analysis), but this is a very different position, as we hope to make clear by the end of this section.

philosophy more sensitive to empirical and physical concerns (especially those regarding measurement).

> ... the physics of space and time has not earned its place in philosophy by suggesting empirical answers to standing philosophical questions about space and time. Instead, it has succeeded in redefining the questions themselves in its own empirical terms. The struggle to articulate these definitions, and to re-assess and revise them in the face of changing empirical circumstances, is the history of the philosophy of space and time from Newton to Einstein. (DiSalle [2006a], 1)

For DiSalle, the history of the revolutionary changes we saw in physics during this modern period owes much to a special kind of conceptual analysis.[75] This variety of conceptual analysis starts by analysing what physics presupposes about space and time, and then "how these presuppositions must confront the changes in our empirical knowledge and practice." (DiSalle [2006a], 2) Overlooking this role for conceptual analysis (and the philosophical arguments pertaining to such analyses) has obscured our understanding of how rational change in physical theory has occurred as a result of empirical findings. Whereas philosophical hypotheses, speculation, and even grand-system building have a role to play in the development of physics, such a role is far less significant than this kind of analysis of concepts of space and time.[76]

III.3: Einstein's Analysis of Simultaneity

We have already seen above Friedman's brief account of the development of relativistic physics. Relativity, on this score, has two constitutive principles: the principle of light, and the principle of equivalence. These two principles (along with an explicated principles of relativity in special and general relativity) were

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[75] It is a version of a kind of conceptual analysis explicated by Demopoulos in his [2000] and [2003a], already quoted in the Introduction to this paper. It is a conceptual analysis of "our theoretical knowledge in general, and of our knowledge of physics in particular, one that clarifies and reveals those assumptions that are implicit in our basic judgements involving the theoretical vocabulary..." (Demopoulos [2003a], 399)

[76] DiSalle first gave an account of this new form of conceptual analysis in his [2002a], in the context of geometrical conventionalism in the Poincaré-cum-logical empiricist tradition.

countenanced in some way by all modern physicists going back to Galileo, but the key shift in Einstein's thinking was to see the light and equivalence principles not as empirical facts, but to "elevate" them to the status of conventions in the sense of Poincaré or coordinating principles in the sense of Reichenbach—the relativised *a priori* constitutive principles which allow the abstract mathematical structures of general relativity to be coordinated with actual sensory experience. While both principles may have had an empirical origin, they have now been "exalted" (to use Poincaré's turn of phrase). Once such coordinations are made, a whole new host of empirical claims follow (for example, explaining the advance of the perihelion of Mercury in general relativity). But the coordinative definitions and the background mathematical framework are not sensitive to empirical considerations in the same way because of their exalted status; they make empirical claims possible, but are not themselves empirical in the same sense.

Such a picture looks far too much like the conventionalism of the logical positivists for two reasons. Firstly, it gives too little consideration to the *empirical* development of physical geometry, even though it may account for the rational progress of theory change in physics. The reasons have already been given. This view preserves too much of the naïve Kantian picture that scientific progress must be held together by something more than just conceptual analysis and experience. Of course, Friedman replaces the rigid *a prioristic* framework of Kant with a nuanced account of the relativised *a priori*, but such a project only preserves the rationality of scientific progress by postulating a regulative ideal towards which physics strives. We have no grounds on which to take such a *telos* seriously (save wishful thinking, and a desire for systematicity which, as we saw with Torretti's notion of consilience, is better explained by the anticipatory nature of our theorising than by an orientation toward some always moving regulative ideal).

As we have seen, DiSalle sees the issue quite differently. For him, Einstein's transformation of the concept of simultaneity was not a result (*pace* logical empiricism) of some deep reflection on the relational character of all motion—some attempt to take Mach's principle and transform it into a thorough-going physics

which had relieved itself of the metaphysical baggage of spacetime structure. Such a positivist reading ignores the ways in which both special and general relativity avail themselves of a world structure no more nor less metaphysical than Newtonian spacetime. Even if Einstein was inspired by such epistemological critique, it is not sufficient, and only necessary in an historical sense, for his development of either the special or the general theory of relativity. Rather, Einstein's development of the theory of special relativity came about as a sustained dialectical critique of the implied Newtonian conception of absolute simultaneity based on instantaneous signal propagation in Newton's physics. On such a reading, Einstein did not attack Newton based on the philosophical need for a clear and precise notion of simultaneity where Newton had no conception at all; rather Einstein engaged in an examination of what Newton could have meant by "simultaneous", or "… happened at the same time", given his understanding of absolute time and the use of light signaling as a proxy for instantaneous gravitational signals, given gravity's complete inadequacy as practical signal because of the inverse-square law (and the very weak, almost non-existent, gravitational forces operating between even fairly large masses at great distances, in other words at precisely those scales for which one would want to exploit instantaneous signaling), and because it is a bad signal indeed that cannot be turned off and on.

Einstein's analysis starts rather with the recognition that there is a problem in the theory of electromagnetism of moving bodies, and that the problem has something to do with presuppositions about time and length. DiSalle examines Einstein's example in Einstein [1905] of the production of an electric current, which relies only on the relative motion of a magnet and a conductor (the so-called "magnet/conductor problem"). The phenomenon (current) can be represented in two ways: if the conductor is thought at rest, there is an electric field around the magnet; if the magnet is assumed to be at rest, there is an electromagnetic force in the conductor. The measurable magnitude is the same in both (DiSalle [2006a], 103-104) This may seem merely as if it is an innocent asymmetry, at most troublesome at a theoretical level (since two separate descriptions can be given for the same

phenomenon). But the problem is one of an "ontological asymmetry" (ibid) that accompanies the theoretical asymmetry: different fields exist depending on which of the magnet or the conductor is considered at rest, and therefore the distinction between the kinds of field is actually owed to something deeper in the theory of electromagnetism—that electromagnetic forces are mediated by waves in the ether, and, therefore, the structure of space and time supposed by the ether. The magnet/conductor thought experiment now poses a major problem, since the competing descriptions call into question the (Galilean/Newtonian) principle of relativity. (More generally, as Joseph Larmor and Hendrick Lorentz were to show, Maxwell's equations are only invariant if time dilation and length contraction are allowed for at relative velocities approaching the speed of light. In other words, the Maxwell equations are only approximately invariant at low speeds, where the Galilean transformations they realise approximate Lorentz invariance.)

There are different ways of framing the problem. The Lorentzian solution argues that the Maxwell equations do privilege an inertial frame—the luminiferous ether. The null result of Michelson-Morley is explained by holding that the ether has distorting effects on the objects which pass through it. More precisely, bodies moving through the ether with velocity *v* contract in the direction of motion by a factor of

$$\sqrt{1-\left(\frac{v}{c}\right)^2}.$$

When, as in most "normal" cases, *c* is much larger than *v*, contraction is almost non-existent, as in such cases

$$\sqrt{1-\left(\frac{v}{c}\right)^2} \approx 1.$$

This is now called the "Lorentz-FitzGerald contraction", since both Lorentz [1882] and George FitzGerald [1889] independently proposed this solution in light of

Michelson-Morley. Time contractions were also necessary (since under discussion were motions through the ether), so Lorentz also introduced the notion of "local time", it taking

$$\frac{1}{\sqrt{1-\left(\frac{v}{c}\right)^2}}$$

times the actual time elapsed between events. What was thought to be the cause of these contractions (dilations)? The "molecular forces" transmitted through the ether causing uniform effects on the microstructural properties of objects as they move through the ether.

Einstein takes an entirely different tack; not one that seeks to *explain* Michelson-Morley's null result via fine-grain distortion effects on bodies as they move through the ether, but to call into question the very framework of space and time that ether theories rely on. But we can see from such considerations that the dialecticism operating here is distinct from Friedman's. Under wide dialecticism, as we saw, philosophers (and philosophically-minded scientists) engage in meta-framework debates that then offer conceptual resources that can be used to create new scientific theories, related to their counterparts by the long-standing meta-framework debates. DiSalle holds the alternative view, that "physics [itself] could cause us to reconsider the very principles by which we define spatial and temporal measurement." (DiSalle [2006a], 154) In the case of special relativity, it was seen that an adequate definition of "simultaneity" was able to resolve several problems arising from the shaky foundations of classical electromagnetism, replacing the principle of rigid displacement with dynamical principles (the Maxwell equations under Lorentz transformations).

This narrow form of dialecticism gives a considerably larger role to empirical matters—in fact, empirical concerns motivate and guide each step in the dialectic history:

> for the revolutions we have considered, what has overthrown a given theory of space and time has been, not the "seeds of its own destruction" that it has carried internally, but the confrontation with unexpected contingent facts—facts which, on careful analysis, could be seen to undermine the concepts on which the theory had staked an entire spatio-temporal framework. The contradictions occur because it is the nature of such theories—as Newton often suggested, by words and by example—to extrapolate far beyond the empirical evidence that originally motivated them, and so to *expose* themselves as contradictions that arise from unexpected empirical circumstances. (DiSalle [2006a], 155)

Scientific theories, as we have seen, can go beyond the evidence that motivates them; here is one instance where the extension of the Newtonian theory of space and time runs afoul of experience and experiment when applied back to empirical circumstances. This is not particular to Newton's physics. It is a central feature of scientific theorising (at least since Newton), and it begins with our very first attempts to make sense of the spatial and temporal features of (firstly) our own world of experience, and then a space of common objects (that is eventually seen as obeying mathematically precise laws).

This is partly why we posit the SE Principle in the preceding chapter as a necessary pre-requisite for engaging in the empirical revision of scientific theories. This principle captures the basic presuppositions that operate in simple empirical judgments: the proto-mathematical assumptions which we take at first to be universal and stable, but which we eventually find to be limited, excessive, misconceived, and in need of revision and theoretical recontextualisation.

§IV: Final Remarks

Our discussion of narrow dialecticism unveiled the empirical nature of theoretical revision in the sciences. On such a picture, empirical considerations (such as Michelson-Morley, or observations of the behaviour of magnets and conductors) can rationally compel a revision in view (though in very complex ways). However, our considerations have also shown that such revisions are not entirely determined by said observations: they also require a deep form of conceptual analysis motivated by empirical findings—a form of conceptual analysis which does not see its purpose as

the prior generation and systematisation of a mathematical framework yet to be empirically applied, nor an impoverished form of conceptual analysis based on the analytic/synthetic distinction. This form of conceptual analysis does not start off in *vacuo*, or at some random (arbitrary) point of our own free choosing, but with the existing views we employ at any given moment to make sense of our world. Our task is to determine what presuppositions are in place to engage in the kind of empirical enquiry we engage in, and how these presuppositions might need to be revised in light of our empirical findings. More generally, in the context of Reformed Empiricism, we need to establish an adequacy condition for all initial views that have these (or similar) presuppositions regarding the application of elementary or prototypical spatio-temporal concepts, and then examine their dynamical behaviour as they are revised in light of empirical findings and further theoretical developments.

The logic of interdependence is certainly up to the task of accounting for such changes, for they exhibit (in a more complicated way) the kinds of epistemological interdependence we see between experience and view in the pre-scientific cases. At least on the narrow dialectical account of DiSalle, we employ theories to give empirical meaning to some physical process or concrete experience, but we also bring these empirical judgments to bear on the view that generated them. Successive revisions bring us closer and closer to our goal of a conception of the world that both shows the plausibility of our elementary conceptions, but also their limitations, and how such conceptions can be contextualised to fit with a newer and more sophisticated theory that explains what experience would be like in extreme contexts (even though "normal" experience seems not to have changed much at all).

But this merely shows us that we have empirical and conceptual reasons to revise our theories. This does not yet fully answer the question of how such revisions can be thought to offer us an account of rational progress in the sciences, i.e. we have not fully overcome the arbitrariness problem. To overcome that problem, we need to be able to show that our current theories do not merely license a certain set of (theory-laden) judgments, but that such judgments are rational.

Here, the story is much the same as it is in general epistemology: convergence on a set of core judgments gives us categorical entitlements as opposed to mere theory (or "paradigm") dependent claims. Thus, we capture what is rational about scientific progress—the mostly slow, though periodically punctuated, development of our theoretical concepts in light of empirical considerations generated by the very theories being revised. The considerations we have supplied here show that there is at least one historically contingent way (and maybe many others) by which any view with the kind of elementary principles of space, time, and motion (even so weak and ill-defined as quasi-free mobility) may be revised and corrected (through empirical considerations, and conceptual analysis), perhaps eventually converging on something like our contemporary accounts of space, time, and motion.

But what justification could be given for the SE Principle, or the set of primitive spatio-temporal views it licenses as initial starting points? What justification could be given for blocking the class of views that it blocks as initial starting points—those views which lack the relevant spatio-temporal structure it imposes, or which simply maintain a purely philosophical (metaphysical) understanding of space and/or time which cannot, even putatively, be coordinated with any physical processes? Here, our justification follows the limitations set by Gupta for any initial view whatsoever. Gupta, we recall, maintains that reason has a nontrivial role to play in epistemology; its role in Reformed Empiricism is far more robust than its role in the classical British or logical empiricist traditions (with their consignment of reason to the merely analytic). For Gupta, reason's task is to examine the dynamical properties of various views in light of a sequence of experiences *E*, and admit only those that do not put in jeopardy convergence and the epistemic entitlements it brings. The SE Principle thus does for scientific world conceptions what non-rigidity, receptivity, and coherence do for common sense views of the world. Non-rigidity, receptivity, and coherence remove the obstacles that preclude experience from fulfilling its epistemic role as our principal epistemic authority and guide, and the SE Principle does the same for scientific views: it ensures that epistemic views allow for a shared space of relative orientation (and

possible orientations) for infinitely many perspectives. Seen in this way, the SE Principle merely demands that theories of space and time be couched in physical terms, which hardly seems much of a demand at all.

Of course, in science, there is no "normal" course of experiences, at least not in the sense that any reasonably endowed scientific community ought to be expected to predict what sorts of experimental data will be proffered at some future point (though, it should be said, there are no such guarantees that "normal" experience will hold in the general case either). Nonetheless, scientific theories, with their precise logico-mathematical frameworks, have implications for extreme situations in ways that common sense views do not; at least in this sense, scientific theories are much better prepared and equipped to handle unexpected experience. They are rarely upset by such extensions of their framework, and, when they are, the revisions they undergo follow a logic arguably even more clearly than do revisions of common sense views based on experience.[77]

But ultimately the SE Principle is justified by the fact that we are right to demand that all scientific theories be public, i.e. that they make clear the ways that their concepts and principles can be applied so that other practitioners may make sense of them in light of their empirical application. This is why much of what we have said here focuses on the communal practice of scientific investigation. This communal endeavour, though it demands restrictions like the SE Principle, has a beneficial consequence: scientific communities are likely in a better position than their individual epistemic counterparts to overcome some key limitations of epistemic resources. Because inter-communication is facilitated by the SE Principle, knowledge production falls on to the community as a whole. This may be considered a boon, since the scientific community more closely approaches/approximates the *Raimex* in Gupta's idealisations than does any individual knower. But regardless of how such scientific communities work, and regardless of how much the SE Principle might help us to characterise such communities, these are completely tangential

[77] We think such a picture much better captures what might be meant by "normal science", which is not the regular testing of theories in order to accumulate data about their applicability, but the constant extension of the concepts of the theory to new and exciting (though also perhaps upsetting) terrain. This conception of normal science owes much to Newton's method.

concerns. Our main concern is in using the SE Principle to construct a picture of how scientific conceptions of the world find their origin in primitive conceptions of space and time based on limited and parochial perspectives; in fact, scientific theories may be seen as an extension of such primitive views, and thus as having the function of correcting earlier excesses, misconceptions, and oversimplifications.

This account of the development of scientific theories (as arising more or less contiguously from pre-scientific views) preserves one of the benefits of Friedman's wide dialecticism which one might have (mistakenly) thought lost by moving to DiSalle's narrow dialecticism—a special role for philosophers in advancing our scientific knowledge. Here, the philosopher does not get her special status because of her disciplinary access to long-standing debates in the philosophy of science, but by the fact that philosophical modes of argument (conceptual analysis, dialectical critique) have become central to scientific practice, and because there are essential and interesting connections between her traditional areas of study (general epistemological studies of perceptual knowledge) and scientific knowledge. The philosopher, far from being a victim of some monolithic scientism, is now invited to participate in the analysis of scientific theories, especially their pre-scientific origins.

More to our point here, we have seen the ways in which Reformed Empiricism might inform the role of the philosopher of science—how, when done carefully, the philosophy of science (or, more aptly, the *history* and philosophy of science) can be motivated to show the various ways in which our scientific development has been sensitive to experience and its more structured cousins: empirical observation and empirical evidence. That the view (with only the most slight modification) is ripe for application to matters both of a general epistemological and scientific nature is, we hope, an unassailable fact. While the extension of the view to scientific matters has here been only done in a piecemeal way, we hope that such considerations are at least enough to spur further research, and that the examples themselves are only secondary to the general claim that the logical machinery of Reformed Empiricism offers us fresh insight into how scientific advancement may be accurately modeled, as reliant on empirical considerations and

conceptual analysis only. No other factors (such as regulative ideals of an "always incompletable" science) need be considered to capture what is rational about such progress. This may be shown quite simply through convergence on a common scientific conception through a number of revision processes for initial scientific theories $\{T_0, T_1, T_2, \ldots T_n \ldots\}$, which, even if the views are as disparate as Aristotelian physics and general relativity, may still converge (so long as we are able to do some philosophical brush-clearing, removing those theories which make no real attempt to be physical theories in the first place).

Thus, the scientific empiricist need only establish as much as the reformed empiricist: she must be in a position to show her non-empiricist counterparts that (i) she may formulate and justify admissibility criteria on possible initial scientific views (the SE Principle may be just one such criterion among many), and (ii) maintain that all scientific views which meet such criteria will converge in light of experience and conceptual analysis on something like our modern theories of spacetime. We argue that this essay has put us well on the way to establishing both (i) and (ii), and that, as a result, Reformed Empiricism is the most promising candidate for a comprehensive study of our knowledge.

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