28/30 excellent.

Brendan Shea
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Carnap and Kuhn on Language, Theory, and Observation

1. Introduction

One of the primary goals of philosophy of science is to provide an account of what type of activities/investigations are "scientific" and which are not. For example, the philosopher wishes to explain why astronomy is a science and astrology is not. In *The Structure of Scientific Revolutions*[1], Kuhn argues that normal scientific activity (within a specific area of science) is characterized by the presence of a "paradigm" that is shared by the scientists in the relevant field. While Kuhn is not entirely clear about what this amounts to, it is clear that this shared paradigmatic commitment includes a common language, a commitment to the truth of certain axioms, and a shared sense about the directions for future research. So, people researching problems in Newtonian mechanics are doing legitimate science, while those doing astrology most likely are not (at least insofar as there is no shared paradigm within astrology). This much any philosopher of science would agree. However, Kuhn's claim about the dependence of scientific practice paradigms is much more radical than this. Specifically, Kuhn argues that it is impossible for a scientist to make paradigm neutral observations about the world – that is, a normally practicing (i.e. rational) scientist will never observe things which falsify fundamental claims of her paradigm. Kuhn argues for this conclusion with appeals to the history of science as well to psychology.

It is widely accepted that *The Structure of Scientific Revolutions* marked a sea change for not just philosophy and history of science, but for epistemology and the methodology

[1] Kuhn, Thomas. *The Structure of Scientific Revolution.* London and Chicago: University of Chicago Press, (1970).

of social sciences more generally. At least part of the perceived importance of Kuhn's book lay in its claim that there was no way to draw a coherent distinction between our theoretical commitments and the things we observe in the world. If Kuhn is correct about this, then the logical empiricist project of "reducing" theoretical statements to claims about the "observed world" is a failure. In this paper, I would like to take a second look at the observation-theory distinction, its perceived import in providing a foundation for science, and the adequacy of Kuhn's criticisms. I hope to suggest that some version of the observation-theory distinction remains both viable and desirable, especially given the problems faced by Kuhnian accounts.[2] good

2. Verificationism and its Problems

Kuhn's account of the nature of the scientific enterprise stands in stark contrast to the then dominant "positivist" views of philosophy of science. According to the early logical positivists (or logical empiricists), a claim was "scientific" just in the case that its truth or falsity would have observable consequences. The positivists hoped that such a criterion could clearly separate science from "metaphysics" and in doing so, provide a guide for the formulation of new theories. The positivist "ideal" of a scientific theory might be the special theory of relativity or quantum mechanics – in both cases, the posited theoretical entities (photons, wave-functions, etc.) have clear empirical consequences. In contrast, the claims of traditional philosophy (about the nature of being per se, the existence of monads etc.) did not have empirical consequences at all. Instead, it seemed that any experience whatsoever was compatible with the truth or falsity of the given theory.

[2] In this paper, I will be considering what I think to be the most plausible interpretation of Kuhn's *Structure of Scientific Revolution.* In Kuhn's more recent writings, there is not much difference between him and the Carnapian structural-realism I am advocating. However, I think that considering the more "radical" Kuhn is worthwhile because of the impact it has had on philosophy of science.

Of course, this view was not unique to the positivists. Thinkers as Hume and Mach[3] were notable predecessors. The insight of the positivists was to *define* as meaningful only those sentences that had specifiable observational consequences. According to the early positivists, the sentences and terms of a language received their meaning from their verification conditions. Sentences could then be divided into "analytic" sentences, whose truth followed from solely the rules of logic specified for the given language and "synthetic" sentences which required reference to the observable world.

This sharp distinction between analytic and synthetic claims was in reaction to Immanuel Kant, who had argued that certain truths about the world were synthetic a priori – that is, they were contentful claims about the world that could not be deduced from experience. Kant thought that claims about the structure of space, time, causation, and arithmetic were all based in synthetic a priori knowledge. While such a position had intuitive appeal (especially given Humean empiricism's failure to make much sense of these notions), it ran into problems in the late nineteenth century with the appearance of non-Euclidean geometries, Einstein's and Poincares' operational definitions of simultaneity, and Frege and Russell's incompatible (but seemingly analytic) definitions of numbers in terms of logic and set theory.[4] Given the mass falsification of almost every synthetic a priori claim, the positivists thought the positing the truth of statements in this manner could not be justified.

[3] See Hume's *Enquiry Concerning Human Understanding* sec. 1-3 and Mach's *The Analysis of Sensations* Chapter 1.

[4] Kant argues for the synthetic a priori status of space and time in *A Critique of Pure Reason.* See especially the sections in "The Transcendental Aesthetic." Frege lays out his definition of number in the *Begriffsschrift* section 26, and Russell in *Principia Mathematica.* For more on Poincare and Einstein see Galison's *Einstein's Clocks, Poincare's Maps.*

Perhaps the most prominent defender of such a view was Rudolph Carnap. For example, in his article (the first article ever published in *Philosophy of Science*), "On the Character of Philosophical Problems" Carnap claims:

> The metaphysicians wish to seek their object *behind* the objects of empirical science; they wish to enquire after the essence, the ultimate cause of things. But the logical analysis of metaphysical propositions has shown they are not propositions at all, but empty word arrays, which on account of notional and emotional connections arouse the false appearance of being propositions.[5]

Carnap's claim here is that the statements of traditional metaphysics are analytic at best (if they are generously interpreted into a formal language), and cannot be taken as contentful claims about things happening in the world. The statements of science on the other hand, are synthetic statements.

The problem immediately faced by the positivists was to understand the primitive *terms* of science in a way that allowed for a line to be drawn between synthetic, scientific statements and empty metaphysical claims. For example, "The wave function of this system is X" should turn out to be a scientific claim; "The phemonology of time is rooted in being-toward-death" should not. But it does not seem that 'electron' refers to anything inherently more "encounterable" or "sensible" than 'being-towards-death' does –we cannot have direct sense experience of either object.

The problem faced by Carnap's project was one of meaning. Traditionally, it had been thought that each word "picked out" some object or process in the world, and

[5] Carnap, Rudolph. "On the Character of Philosophical Problems." *Philosophy of Science, Vol. 1, No. 1 (1934)* p. 5

thereby received its meaning. Thinkers such as Russell had proposed that sentences were true just in the case the state of affairs they described actually pertained in the world.[6] However, this picture of meaning was problematic for Carnap. After all, we do not have direct sense perception of electrons, nor do we think that we having such sense perceptions are possible. In this sense, 'is an electron' seemed more similar to problematic predicates such as 'is a monad' than to acceptable predicates such as 'has such and such a mass.'

In response to this problem, Carnap proposed that terms were meaningful insofar as we could logically deduce certain "verifiable" or "observable" propositions from them. In one of his first treatments of the problem, he wrote:

> What gives theoretical meaning to a proposition is not the attendant images and thoughts, but the possibility of deducing from it perceptive propositions,...the possibility of verification. To give sense to a proposition the presence of images is not sufficient; it is not even necessary. We have no actual image of the electro-magnetic field, nor even, I should say, of the gravitation field. Nevertheless the propositions which physicists assert about these fields have a perfect sense, because perceptive propositions are deducible from them.[7]

Carnap's claim is that statements containing the term 'electron' will have "perceptive propositions" as consequences; that is, statements about states of affairs that we can directly verify.

This early positivist account of the meaning of theoretical terms has been called "verificationism." Verificationism directly identifies the meaning of a theoretical term

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[6] See the chapter on truth in Russell, Bertrand. *The Problems of Philosophy.* Indianapolis: Hackett. (1990)
[7] Carnap, Rudolph. *Philosophy and Logical Syntax* .London : Kegan Paul (1935). Chapter 1.

with its observational or verifiable consequences. Claiming that a system's wave-function is described by such and such an equation is just to claim that if I carry out a certain procedure specifiable in the observation language (shine so much light at such and such angle; read number off a certain instrument, etc) I will observe a certain result.

Verificationism obviously requires a clear distinction between which terms or predicates denote observables, and which do not. This distinction is crucial to the positivist program in at least two ways. First, as mentioned above, it provides a criterion for separating "meaningful" synthetic statements, from "empty" analytic statements. This allows not only a separation of science from metaphysics, but accounts for the possibility of co-existing rival scientific systems. Since the content of a given theory is exhausted by its observable consequences, it is not necessarily the case that two theories with opposed theoretical entities are logically at odds with one another. Rather, each has chosen a different theoretical language as a "tool" to predict and describe the behavior of the world.

A clear-observation theory is also necessary if one hopes to provide a formal account of what it means for observations to confirm or disconfirm a theory, as many positivists hoped to do. More specifically, a theory of formal confirmation requires that we spell out the logical relations between theoretical claims and the observational statements that will serve as evidence for their possible truth or falsity. For example, Newtonian mechanics was confirmed by the observation of the planet Neptune. Newtonian mechanics itself contains theoretical terms such as 'gravity.' However, the truth of statements containing these terms could be indirectly confirmed because the theoretical statements had observational statements as logical consequents. So, the distinction between observation and theory provides a way to explicate how "reasonable" it is to think that a certain scientific theory is true.

The problems with Carnap's early formulations of verificationism are numerous. For instance, the theory seems to require that each theoretical term is identified with either 1) a specific experiment or instrumental procedure or 2) a specifiable set of such procedures. This seems to rule out the possibility that new experimental procedure discovering new "properties" about old entities. Instead, new procedures would necessarily be about new entities. This seems inconsistent with the actual practice of science. For instance, it used to be the case that disease-causing microbes could be detected only by their effects on the host. So, a verificationist might claim that the meaning of the term 'disease-causing microbe' was cashed out in terms of certain observed properties about the host (i.e. 'A disease-causing microbe of type B or C is present' is true iff The host's temperature is between 101.1F and 103F). The problem arises when scientists find new ways of "detecting" disease-causing-microbes; for instance, the use of a microscope. The verificationist seems to be committed to claiming that microscopes cannot tell us anything about disease-causing microbes, at least not if we hold the meaning of 'disease-causing microbe' to remain constant. We are free to claim that "looking-through-microscope" constitutes an additional part of the definition of 'disease-causing microbe,' but in doing so we have not "discovered" anything new about an old entity. Rather, we have given a new meaning to an old word. But this is obviously wrong – the microbes we see through the microscope are the same microbes we were talking about as causing disease in people.

There are at least two other problems with verificationism that are commonly mentioned. First, it has proved highly problematic to assign any probability other than 0 to a universal statement. That is, no finite quantity of observations of brown horses (combined with the observation of no non-brown horses) will ever raise the conditional probability that "all horses are brown." This objection, while it may raise problems for positivist confirmation theory, does not directly suggest the impossibility of an observation-theory distinction. It does, however, raise problems for the positivist claim that it is more "probable" that one theory (with its attendant theoretical terminology) is true than another (with a different theoretical terminology). In this case, it seems that the probability of most theoretical statements will be 0.

A more significant problem for the verificationist distinction between theory and observation lies is the "continuum" between clearly theoretical and clearly observational statements. For example, a verificationist would no doubt wish to claim that statements about dogs are observable, since dogs are directly observable, and that statements regarding quarks are non-observable, since we can only infer their existence from the results of numerous experiments. However, it does not seem that we can separate statements into two neat classes of observable and unobservable. Rather, there is a spectrum: ~~for example~~: direct sight, eyeglasses, binoculars, magnifying glass, normal micrscrope, electron microscope, cloud chamber, etc. It seems obvious that states of affairs observed while wearing eyeglasses should count as observables, but what about while using a microscope, etc.? The problem is that there does not seem to be a principled distinction between the observable and non-observable. We have some intuitively clear examples, but this may not be enough to serve our purposes.

3. Kuhn on Paradigms and Observations

It is widely accepted that the publication of *The Structure of Scientific Revolution* represented the end of positivism as the dominant model of philosophy of science. In the book, Kuhn not only presents arguments purporting to show the impossibility of making "theory-neutral" observations, but proposes a new model for differentiating "scientific" from "non-scientific" modes of reasoning. In this section I will briefly lay out Kuhn's idea of a paradigm, and consider his claim that members of different paradigms will have different (and perhaps incommensurable) observations.

Kuhn's statements about what constitutes a paradigm are notoriously vague. It may be easier to say what a paradigm is not, rather than what it is. Paradigms are not, at least according the early Kuhn, things "that can be reduced to [their] logically atomic components"[8] or things "that imply…the existence of an underlying body of rules and assumptions that additional historical or philosophical investigation may uncover."[9] Statements such as these suggest that Kuhn is consciously attempting to separate himself from any form of verificationism. After all, the vericationist would be perfectly happy to admit that Newton and Einstein were speaking different languages where 'mass,' 'space', and 'inertia' have different meanings. She may even be willing to grant that the Newtonian and Einsteinian languages contain different "physical truths" we must hold resistant to falsification.[10] The verificationist claim would simply be that since all theoretical terms are defined by language-independent observations, one could in principle "translate" from one language into another (and thus bring the confirmation mechanism in the language to bear on the claims in question). For example, the early

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[8] Ibid. p. 11.
[9] Ibid. p. 46
[10] Its hard to say what these might be, but perhaps equations regarding gravity or the speed of light.

Carnap seems to be involved in a project like this in "Psychology in a Physical Language."[□] In this case, he is trying to translate the terms of a higher science (psychology) into the terms of physics (a more basic science). However, there is nothing in the verificationist program which rules the possibility of a similar translation between the languages of two competing theories.

Kuhn explicitly compares his notion of a paradigm to Wittgenstein's idea of a language game, where it is impossible to give an explicit account of the meaning of individual terms or the rules by which they are related. Instead, the meanings of terms in a given language are "constituted by a network of overlapping and crisscross resemblances."[12] Given the impossibility of locating the meaning of an individual term, we should not be surprised that Kuhn thinks we cannot "translate" between different languages. More surprisingly, Kuhn claims that the content of one's paradigm cannot be expressed even in the language of the paradigm holder: "paradigms may be prior to, more binding, and more complete than any set of rules for research which could unequivocally abstracted from them."[13] It is this last claim which is especially significant for the possibility of an observation-theory distinction, since Kuhn sees our implicit commitment to paradigms as prohibiting our ability to make observations that conflict with these paradigms. For Kuhn, all observations will end up being theory-dependent, whether they are of electrons and tables. Moreover, even within a paradigm, it is impossible to draw a sharp distinction theoretical and observational terms. In any case, one does not learn

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[□] Carnap, Rudolf. 1932/33. "Psychology in Physical Language." *Erkenntnis* 3. Reprinted (in translation by George Schick) in *Logical Positivism*. Ed. A. J. Ayer. New York: The Free Press, 1959: 165-98.

[12] Kuhn, p. 45

[13] Ibid, p.46.

terms by hearing a word and having it associated with a particular experiment or observable. Rather, one learns the "game" of science by participating in research.

According to Kuhn, the relation between observation and theory is a symbiotic one – in fact, he suggests that it is impossible for one to have "observations" that disagree with one's paradigm unless one already has an "alternative paradigm" in place. As a clarification of his view, Kuhn considers the "discovery" that oxygen was a gas[14]. He argues that this "discovery" was in fact a gradual process, and that oxygen was not fully "discovered" until there was a new paradigm which contained "oxygen" as a concept. For those committed to the phlostigon theory, there were no observations of oxygen, but merely anomalies. Early thinkers such as Priestly and Schelle constructed experiments that distilled what later theorists would call "oxygen," but they themselves reported observations in terms of "air" with more or less phlostigon (that is, they never "observed" the presence of a purified gas at all). Lavosier, in contrast, could observe oxygen, but only because he had in mind a new paradigm where such an observation made sense.

Kuhn offers two distinct arguments supporting his claim that observation is theory dependent. The first is based on psychology, the second based on the history of science. Kuhn cites several experiments in psychology which supposedly demonstrate the "inability to observe," "anomaly," "new paradigm sequence." One experiment showed that people shown playing cards that were black hearts, red spades, etc. were likely to initially misreport the suit, and soon could make no report at all. It was only after a great many trials that they "noticed" these cards. Once they "discovered" these suits, however, they could easily identify them.

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[14] Ibid, p.53-56

Of course, the psychological argument does not carry much weight by itself, but Kuhn thinks it illustrates something about the way scientific are carried out. In the end, his argument succeeds or fails with actual scientific practice. If it really is the case that new discovering new observables requires a paradigm shift, the history of science should show long periods of relative stability (where the meanings of scientific terms are stable) interrupted by short "revolutions" (where no one is sure about what is going on), succeeded by another long period of stability with a new set of terminology.

While neither of these arguments are entirely convincing, they do pose some significant problems for the possibility of an observation-theory distinction. Gettier examples (e.g. duck-rabbit drawings of the type described by Kuhn) show that what we report as observed depends to some degree on the conceptual scheme we bring to bear. For example, it seems unlikely that people who have never seen stairs will recognize our "3-dimensional" drawings of stairs as 3-dimenional at all, but will instead see it as a 2-dimensional collection of lines. Of course, one might claim that there still is a "truth" about what one observes: namely, that that there were black squiggles on such and such point in the visual field at such and such time. But this is just to embrace verificationism, with all its attendant problems. Specifically, it does not seem that we can identify the meaning of terms with a propensity to see a certain pattern of squiggles. More, Kuhn presents some evidence to think that even reports of squiggles can be theory influenced (as in the case of the playing cards).

Kuhn's analysis of the history of science is more debatable, and I will give some criticisms of it in the next section. However, it does have several important insights. First, Kuhn's notion of "normal science" as taking place within a paradigm seems to fit much closer with actual scientific practice than either the naïve inductivism of the verificationist program[15] or Popper's falsificationism. On these views, it is difficult to explain why one would adopt an unconfirmed theory at all (i.e. Copernicus vs. the Scholastics) or why one would not immediately abandon a theory that made a false prediction. On Kuhn's account, our willingness to adopt and maintain a paradigm is in some sense linked to its perceived productive "potential" which seem accurate. Also, Kuhn highlights the fact that the meanings of our theoretical terms are in some important sense dependent on the theory or paradigm in which they are embedded. It is simply not the case that 'mass' has a stable meaning across the Scholastic physics, Newtonian mechanics, and special relativity. Finally, it seems that the reasons for which scientists change paradigms are in same significant way different from the reasons for which they endorse or deny claims within a paradigm, and that any adequate theory of scientific practice must take account of this.

4. *Kuhn Criticized*

So far, I have outlined Kuhn's arguments against the theory-observation distinction and indicated those parts I find most compelling. However, I think that are severe problems with what I take to be his positive views. According to the Kuhnian account I have presented, we cannot make any distinction between theoretical and observational terms. This would have several major implications for the way we think about scientific practice, all of which are problematic. First, as I have already suggested, it would render

[15]By "naïve inductivist" I mean the view that all predicates can be defined in verificationist terms, and science is done by cataloging observations and applying the laws of statistics. For example, we define mass of an object as being the ~~weight to~~ which it moves a needle on a certain scale, its volume by the amount of it water displaces. We then "discover" that the density of certain material is X based on our weighings and dunkings in water of many objects. Along with problems raised earlier, this view cannot account for Goodmaned predicates such as 'grue' or 'bleen.'

translation between languages all but impossible, since we only understand the meanings of our terms through our commitment to a paradigm whose content we are unable to articulate. Second, the Kuhnian account sees any choice between competing scientific theorems as being a fundamentally "irrational" choice, insofar as Kuhn suggests that our paradigms are the sources of not only our term meanings, but also of our logic and implicit values. Finally, it could be argued (although Kuhn denies this) that the identification of mature science as an activity with a shared paradigm is a flawed one, which both excludes some "real" sciences and includes many non-scientific activities. I will offer some reasons for thinking Kuhn is wrong about all of these claims.

As I outlined it earlier, Kuhn's argument regarding the incommensurability of paradigms seems to go something like the following: we cannot explicitly define any term within our paradigm, and therefore we cannot translate between one paradigm and another. Therefore we cannot "compare" the predictions of one paradigm with the predictions of another. While Kuhn is certainly right about this in some weak sense, it is simply incoherent if one takes the claim at face value. In many cases in the history of science, it seems that there are shared observational terms between paradigms, and that predictions ~~that contain~~ these contain these terms are can be seen as "evidence" for the superiority of one paradigm over another. For example, consider Fresnel's white spot experiment. Fresnel, a defender of the wave theory, wrote a dissertation on the behavior of light. Poisson, an advocate of the then accepted particle theory of light, deduced the following from Fresnel's calculations: if a beam of light was directed at a disk of sufficient size, we should observe a small white spot in the middle of the disc's shadow. This was of course immediately confirmed, and taken as evidence that the wave theory might be correct. While this example might seem simple minded, it does show that "observation terms" are in some sense theory independent – Fresnel and Poisson might disagree about what the "white spot" was in a theoretical sense, but they both agreed that it was present.[16] Of course, this is not an instance of Popperian falsification, since Poisson and the particle theorists did not immediately abandon their view, but it does seem that predictions of this sort give the particle theorists "reasons" to think that the wave theory might be correct.

This relates directly to the next problem with Kuhn: his (implicit) claim that the choice between scientific paradigms is somehow necessarily irrational (or at least unscientific). While it may not be that one can provide a specific probability for the truth of a paradigm (since an explicit probability calculus can only be articulated within a paradigm), it does seem to be the case that we can make rational choices among competing theories based on criteria such as simplicity, successful predictions (of shared observables!), and perceived fruitfulness. In a related point, Kuhn provides no account for how the theoretical terminology of one paradigm is related to that of another, besides claiming that we cannot translate between the two languages. But there obviously is some relation. While the ontology behind 'inertia' changes vastly between Descartes (relational space, vortices), Newton (absolute space, conserved momentum) and Einstein (general relativity), it remains recognizable insofar as all three theories contain very similar equations. While Kuhn may be correct in claiming that the ontologies/paradigms of Cartesian, Newtonian, and Relativistic mechanics are in a way "completely different

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[16] See Worall, John (1989) "Fresnel, Poisson and the White Spot: the role of successful predictions in the acceptance of scientific theories", in *The Uses of Experiment* (ed. by D. Gooding, T. Pinch, and S. Schaffer), Cambridge University Press, 1989. Worall points out that this experiment did not convince Poisson of the truth of the wave theory.

worlds" it remains the fact that the equations surrounding inertia remain almost unscathed. This suggests that despite the theory-laden character of our observations, we have consistently observed the world behaving in some definite way. Moreover, each succeeding theory augments these equations so that they more and more accurately predict observations. It does not seem unreasonable to think that there is some paradigm independent observations that these equations describe successfully.

A final point against Kuhn concerns his identification of mature scientific practice with a shared paradigm, a condition that seems neither sufficient nor necessary. In the case of sufficiency, one can imagine that Freudian psychology conquered its behaviorist rivals, and became the shared language of all practicing psychologists. We would expect speakers of this language to agree on some research program (e.g. "the hysteria complex in people of a certain economic class," etc.), and we would expect there to agreement on what counts as acceptable results (no denying the unconscious exists, etc.). Since Kuhn suggests that it impossible to lay out the rules of given language, we cannot simply "throw out" Freudian psychology because its predictions are not logical consequence of its theoretical assumptions. In other words, if Freudian psychologists display the social behavior of physicists, then Kuhn would have to say that Freudian psychology is just as "scientific" as physics.

In the case of necessity, it does not seem to me that Kuhn has made the case that individual scientists are engaged in a fundamentally different activity than those in normal science. For Kuhn, it seems to be a miracle when a pre-paradigm scientist makes any discoveries at all. But this does not fit with the history of science. Thinkers such as Archimedes and Descartes made significant advances in physics and mathematics, yet their theoretical "tools" do not seem to have been shared by their contemporaries. Moreover, neither inspired a substantial research program. While Descartes did have some followers, this does not seem essential in figuring out whether he was involved in normal science, since he obviously was. He had an axiomatic theory with implicitly defined primitives that made predictions about the outside world. Even if no one had adopted Descartes language, he would have been a scientist in a way that the Freudian psychologists could not be.

5. Observation Revisited

So far, I have described two possible approaches to the observation-theory distinction: verificationism and Kuhnian historicism. I have argued that the first fails because it makes it impossible to learn anything new about theoretical terms, while the second fails because it makes observations so theory dependent that is impossible to have any rational discourse about the value of competing theories. In this section, I want to suggest that a middle ground is possible. Specifically, I want to claim: 1) that while we cannot explicitly define the primitive terms of our language in terms of observation, there are some "real things" which correspond to them, and 2) it is our ability to perceive these theory independent things which makes it possible for us to choose between theories in a rational (though not necessarily "scientific") way.

If one is talk coherently about theory and observation it is important to differentiate between our informal, "everyday" language and the formalized language of our scientific theories. In everyday language 'mass' denotes something like heft or weight. That is, it refers to the "movement resisting" property on an object. In Newtonian mechanics, however, mass is an undefined primitive which is related to certain other primitive terms by means of certain equations (e.g. F=MA). So, it cannot literally be said that 'mass' in Newtonian mechanics is the same thing as 'mass' ordinarily conceived. However, there is an important relationship between the two. The scientific term 'mass'(and the equations in which it appears) is meant as an *explications*[17] of our pretheoretic understanding of mass. By *explication,* I mean the process by which one replaces a vague, pre-theoretical term with an explicitly defined theoretical one. This theoretical term is an explication insofar as it serves some of the same roles that term serves in natural languages (i.e. the larger the mass, the harder to accelerate). Of course, it is perfectly to have multiple explications of the same term, each in a different theoretical language. For example, the scholastics, Newton, and Einstein all offer different explications of mass. Following, we might say that the term 'mass' is used by people in different paradigms, and that the uses of this terms are in some sense incommensurable (that is, there is no term or set of terms in scholastic philosophy that denotes the same thing Newtonian 'mass'). However, the fact that one cannot directly translate between paradigms does not mean that the terms of the paradigms do not have some relation to another. Each paradigm's use of 'mass' is intended as an explication of our pretheoretical understanding of mass.

Given this understanding of the relationship between scientific theories and ordinary language, we are in a better position to see how theory choice can be a rational process. Picking between theories is just to choose between different explications of our pre-theoretic observations. Pretheoretically, it may not be the case that by 'mass' we mean something that has a quantifiable relation to force and acceleration. However, if

[17] The notion of explication means different things to different philosophers. Here I follow Carnap's account in Chapter one of *Meaning and Necessity.*

adopting such an explication allows us to discover new things about the world, and better predict its behavior, we might demand that future explications of the term mass also include this relationship to force and acceleration. In this sense, we can say that our scientific theories allow us to "discover" things about the world. At the same time, we can avoid the claim of the old correspondence of truth, which demands that our scientific theories are either absolutely true or absolutely false. We adopt our theories based on criteria such as simplicity, intuitiveness, and perceived fruitfulness, and not on empty criteria such "we think they are true." However, our theories are not merely instruments to predict observation – they can tell things about the non-theoretical world.

In any case, it seems likely that the best solution to the observation-theory distinction (as well the realist vs. non-realist dilemma) must involve some solution of this type. For it seems obvious that science does make progress, and that rival scientific paradigms are talking about the same world. However, we must also account for the fact that it is impossible for us to design a theory-neutral language of observation. Given this, we might say that our best scientific theories work partially because of our creativity and partially because of the way they accurately describe the word.

1.) Length 5/5
2.) Grammar 6/8
3.) Content 17/17

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