Chapter One: Scientific Success and Realism

1.1 The Success of Science

The instrumental success of science--the ability of certain bodies of scientific theories to make accurate predictions--is striking. While some have warned against the carefree use of such phrases as 'the success of science,' we can give this phrase a relatively unproblematic interpretation. In referring to 'the success of science,' I mean to point to the (mostly) accurate predictions of well-established bodies of scientific theories about observable phenomena in a wide variety of particular instances. Examples of this success range from the mundane (the ability of Newtonian mechanics to accurately predict the velocity of an object after a collision) to the surprising (Fresnel's prediction of a bright spot in the center of a circular disc's shadow). It is one task of philosophers of science to explain why it is that our accepted scientific theories enjoy such success.

One reading of the question, "Why are scientific theories instrumentally successful?" yields a trivial answer. An important criterion for acceptance of a scientific theory is its instrumental success--if a candidate for a new theory gives obviously incorrect predictions about what we observe, then it will soon cease to be a viable candidate. Asking why scientific theories are successful looks like asking why professional basketball players are so good at dribbling and shooting: if an accepted scientific theory wasn't successful, it wouldn't be an accepted scientific theory in the first place. But there is a deeper question: are we justified in expecting further success from an already-established theory, and, if so, why? A basketball player would not be a basketball player if he had not established his proficiency at such skills as dribbling and shooting, but this is not, in and of itself, what justifies our expectation that he will continue to be proficient in the future. That justification is found in an examination of the player's physical and mental

capabilities in basketball-related skills. The analogous question for philosophers of science, then, is, what is it about our accepted theories that makes them predictively reliable? Just as labeling an athlete as a 'professional basketball player' does not *endow* him with any athletic ability (though it does give rise to expectations of athletic ability), the labeling of a theory as 'an accepted scientific theory,' though giving rise to the expectation of instrumental reliability, does not endow the theory with any reliability-creating features. What, then, *are* the features of a scientific theory that make it instrumentally reliable? The type of answer available to a philosopher depends heavily on her views about the status of scientific theories: whether such views are best characterized as realist or anti-realist.

1.2 Realism and Anti-Realism

One point of difference between realism and anti-realism concerns the interpretation of theoretical terms in claims of science. By 'the interpretation of theoretical terms,' I simply mean the way in which claims about unobservable entities (such as magnetic fields or electrons) are to be understood. A central feature of realist philosophies of science is that such claims are to be interpreted realistically; that is, as simply describing the world around us. Terms used to designate unobservable entities are treated in the same way as terms designating readily observable objects like chairs and tables. If a succesful scientific theory utilizes the theoretical term 'neutrino,' then the realist infers that such objects exist, as described by the theory, in the world.

Anti-realists, on the other hand, typically deny that theoretical terms can be understood as actually referring to real objects in the world, though many (such as Bas Van Fraasen, for instance[1]) insist that scientific theories must be understood as

[1] cf. Van Fraasen, B., *The Scientific Image*, Oxford, 1980, p. 10: "[The anti-realism I shall adovcate] holds that the language of science should be literally construed...."

purporting to describe real objects in the world. The use of theoretical terms in science, rather than being legitimated by such terms reflecting the real nature of the world, might instead be justified by extra-systematic concerns. The use of 'neutrino,' for instance, might be justified by the fact that theories which employ that term are more instrumentally reliable (that is, are better guides for making predictions) than theories which do not employ the term. This, though, is by no means the only anti-realist route available: some anti-realists may employ simplicity as a justificatory device, or any one of a variety of other concerns. What binds anti-realists together under a single heading is their rejection of the idea that the theoretical terms of an accepted scientific theory actually describe the constituents of the world around us (as it exists independently of and prior to any theorizing).

A second point of contention between realists and anti-realists concerns the confirmation of scientific theories. Realists hold that "scientific theories, interpreted realistically are confirmable and are in fact confirmed as approximately true by ordinary scientific evidence in accordance with ordinary methodological standards."[2] We have, according to the realist, very good reasons to suspect that are current scientific theories are at least approximately true. A prediction by a theory, borne out in experiment, gives us warranted grounds for preferring that theory to another. Anti-realists, however, tend to deny that our scientific methodologies give us good reasons for preferring our chosen theories to any of an infinite number of alternatives. Our standard methodology in science minimally tells us that observed events that accord with observable consequences of a theory provide grounds for accepting or preferring that theory, at least provisionally. But, the anti-realist argues, there are an infinite number of possible theories, with radically different

[2] Boyd, R. “The Current Status of Scientific Realism,” in *Scientific Realism*, Leplin, J. (ed.), p. 41

claims about unobservables, which could entail the observed consequences. Thus, we must remain agnostic about the claims a theory makes about unobservable entites, for that which we can observe speaks nothing about that which we cannot observe. This argument, in its various guises, is often referred to as the *evidential indistinguishability thesis* (EIT), and has been employed by Bas van Fraasen, among others. In his *The Scientific Image*, van Fraasen writes: "Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate."[3] In other words, to accept a scientific theory is to accept that it makes accurate predictions about observables, without accepting any claims the theory makes about the realm of the unobservable.

I stated earlier that one's position in this realism/anti-realism debate affects the resources one has to explain the success of science; the reason for this should now be coming clear. The realist can point to the way in which successful scientific theories, by referring to real objects and processes in the world, mirror the functioning of the world. Instrumental success, then, is to be expected. Hilary Putnam is credited with terming such appeals 'no miracles' arguments.[4] This straightforward explanation is not available to the anti-realist. On the issue of the success of science, then, the usual anti-realist responses are constructivism (which holds that the world is, in crucial part, *constituted* by our concepts, and so success is to be expected) or silence.

The waters, then, have been thoroughly muddied. On the one hand, the anti-realist can provide a seemingly cogent argument against realism via the EIT, but appears to lack a plausible explanation for the success of science (the inadequacy of the constructivist position will be discussed later). The realist, on the other hand,

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[3] *The Scientific Image*, p. 12

[4] cf. "What is Realism?" in *Scientific Realism*, p. 140: "[T]he typical realist argument against idealism is that it makes the success of science a *miracle*." (emphasis original)

has an entirely plausible explanation of instrumental success, but seems suspiciously silent about the EIT. A Putnam-style 'no miracles' argument does not counter the epistemic weight of the EIT and provides no indication of what, if anything, might be wrong with the anti-realist position. Richard Boyd, in such articles as "Scientific Realism and Naturalistic Epistemology" and "The Current Status of Scientific Realism," addresses the shortcomings of traditional realist arguments and attempts to provide an argument for realism that will not only explain why science is instrumentally reliable, but that will also show why EIT-based arguments are unsound.

1.3 Boyd's Realism

The central claim in Boyd's argument for realism is that only a realistic interpretation of scientific theories can account for the instrumental reliability of scientific methodology. He begins by asking the same question asked by constructivists: "What must the world be like in order that a methodology so theory-dependent as ours could constitute a way of finding out what is true?" The constructivist response (which Boyd attributes to various epistemological threads found in Kuhn's *The Structure of Scientific Revolutions*) is that the world must therefore be, at least in significant ways, defined by or created by our methodology. Boyd points out, though, that another, perhaps more epistemically responsible, answer is possible: the world is constituted such that the claims made in our scientific tradition are at least approximately true, and the methodologies which arise out of this theory-laden tradition can function dialectically to guide us to an even more accurate approximation of truth. This more accurate approximation of truth would give rise to even more reliable methodologies, and the convergence upon truth would continue.

So, then, the realistic interpretation of scientific theories is "the only

scientifically plausible explanation for the reliability of ... theory-laden methodological principles."[5] This seems to be a recasting of the no-miracles argument, but with the emphasis now on procedures and methods, rather than specific claims and theories. No explanation of the success of scientific methodology grounds such success as well as the realist explanation given by Boyd; in fact, Boyd's explanation seems to be the only one which is "scientifically plausible." The issue of whether this realist explanation is truly to be preferred to a constructivist one will be explored later; but regardless of its particular content, Boyd claims that there is "no scientifically plausible explanation of the *instrumental* reliability of actual scientific methods ... which does not portray those methods as reliable for the acquisition of theoretical knowledge as well."[6]

How does this response fare better than standard no-miracles arguments? The central problem for those arguments was that, while motivating a realist interpretation of scientific claims, they did nothing to show why the anti-realist arguments (based on the EIT) were faulty. Boyd claims to avoid this difficulty, for on his account, the EIT must be false. Our methodology is defined, for the most part, by our theoretical tradition. Since this tradition embodies at least approximately true theories, it must be considered *evidentially* when considering rival hypotheses; that is, a new theory which arises out of our actual theoretical tradition is to be preferred over one which does not, even assuming that both theories yield identical observational consequences. It is a mistake, Boyd argues, to make the common assumption that accord with observation is the only relevant criterion for judging a proposed theory's plausibility. Once it is shown, as Boyd tries to do, that our theoretical tradition consists of approximately true theories, then

[5] Boyd, R. "Scientific Realism and Naturalistic Epistemology," in *PSA 1980*, vol. 2, Asquith and Giere (ed.), p. 622 (emphasis original).

[6] "Scientific Realism and Naturalistic Epistemology," p. 617

how well a theory 'fits' with this tradition is an evidential consideration. Two theories may have identical observational consequences, but how well each accords with the theoretical tradition of that science weighs decisively (as *evidence*) in favor of one or the other.

How is it, then, that the approximate truth of our theoretical tradition can be established? Boyd, having argued that no scientifically plausible account of the (presumably undisputed) instrumental reliability of the methodologies of science can be given which does not also show these methods to be reliable for the acquisition of theoretical knowledge, invites us to examine some specific instances of this reliability, rather than consider the relatively vague issue of 'the success of science.'

1.3.1 Boyd's Illustrations: The Instrumental Success of Theory-Laden Methodologies

First, consider the way in which proposed theories are arrived at. As the anti-realist rightly points out, there are always an infinite number of possible candidates consistent with any finite set of observations, yet we choose one on the basis of just such a finite number of observations. So some extra-experimental criteria, other than mere consistency with observation, must be at work. One such criterion is 'simplicity': we prefer theories that are relatively 'simple' modifications of existing theories. We prefer that previous ontologies be maintained, that previous laws be preserved (at least as limiting cases), etc. Simplicity is thus a profoundly theory-dependent notion, and serves to eliminate an infinite number of possible theories from contention (before they can even be experimentally tested) based on their relation to current theories. One might expect that such outright elimination of so many seemingly viable theories would detract from the instrumental reliability of science, and yet it actually seems to *contribute* to said reliability. Why, Boyd asks, is such a theory-dependent strategy so useful in finding instrumentally reliable

theories?[7]

Second, consider a puzzle of measurement. Let T(*t*) be a well-confirmed theory containing the theoretical term '*t*'. Alleged measurements of the value of *t* have only been possible through measurement procedures m_1, ..., m_r whose reliability is a consequence of 'mini-theories' M_1, ..., M_r. Now suppose that a new theory, T'(*t*), which is only distantly related to T(*t*), is confirmed by an entirely different set of observations. A consequence M_{r+1} of T'(*t*) is that a new measurement procedure m_{r+1}, distinct from $m_1 ... m_r$, is suitable for the measurement of *t*. Now, when we employ T(*t*) with this new measurement procedure m_{r+1}, we are confident of getting at least approximately true predictions--and we do! Why should we be justified in confidently expecting accurate predictions from [T(*t*) & M_{r+1}], when no observational prediction of this pair has ever been tested?[8]

Finally, what Boyd calls "The Fundamental Rule of Experimental Design." In testing a theory T, we first subject T to theoretical criticism. We ask, in light of existing theories, where the mechanisms and / or objects posited by T might conflict with accepted mechanisms and objects, etc. When this theoretical criticism is complete, we test T in exactly those circumstances where the theoretical considerations indicated T might go wrong. That is how we design experiments, and, as Boyd says, "that's as theory-dependent as you can get."[9] Why does this work? Why does the theoretical criticism T is subjected to turn out to be such a reliable indicator of where T actually goes awry in practice?

Often, a pragmatic answer to at least the first and third of these questions is given--we test relatively simple theories in light of currently existing theories not

[7] "Scientific Realism and Naturalistic Epistemology," p. 619
[8] Ibid., p. 620
[9] Ibid., p. 621

because we have good grounds for thinking this will work, but because it is more pragmatically sound to test theories you already have (or are close to ones you already have) before proceeding to the infinite number of alternatives. But this leaves a key question unanswered: why does it work? Why are we able to get satisfactory results from testing these less esoteric theories, without having to radically depart from our existing theoretical tradition?

This question is easily answered if one assumes that the background tradition of theories on which these three theory-laden methodological principles are based are approximately true descriptions of the world. Indeed, Boyd argues, such an explanation is the only scientifically plausible one available. The first question asked why constraining ourselves to proposed theories 'close to' existing theories contributes to the instrumental reliability of science. If these existing theories are approximately true descriptions of the world, then staying 'close' to them ensures that future theories will also be approximately true, and thus instrumentally reliable. By the same token, if well-confirmed theories of measurement are really about measuring things in the world, then applying such a theory to an unrelated but also well-confirmed theory in another area of science is really no different than "applying the lens-makers equations to design a microscope and then using the microscope to observe bacteria."[10] Finally, if theoretical considerations are reflective of the 'furniture of the world,' then it is to be *expected* that theoretical criticism would guide us to the areas in which a proposed theory will fail. When the theory-ladenness of principles operative in establishing the instrumental reliability of science is fully considered, then, Boyd says, we see that only by accepting the approximate truth of these theoretical considerations can we explain this instrumental reliability.

[10] Ibid., p. 622

1.4 Summary

Boyd's arguments are indeed an improvement on the more traditional arguments for realism, and should give the anti-realist pause. They have shown that the standard anti-realist ammunition, the EIT, is not true, and the arguments that rest on it therefore unsound. To rescue the anti-realist position, then, I will do two things: first, I will show that, even lacking a positive argument for anti-realism, Boyd's (and other forms of) realism are not viable insofar as they involve a correspondence between things in the world and things referred to in scientific theories. This would show that the success of scientific methodologies must be grounded in something other than their 'mirroring' of the external world, and, specifically, that Boyd's claim that this 'mirroring' (i.e., approximate truth) is the only scientifically plausible grounding for the instrumental success of science is false. Thus, a different, scientifically plausible grounding of the success of science must be found. I will then attempt to provide such a grounding.

Chapter Two: Conceptual Idealism

The primary considerations in providing an anti-realist explanation for the instrumental success of science stem from Nicholas Rescher's 1973 *Conceptual Idealism*. The term 'idealism' generally is taken to apply to any system wherein the mind and its functionings play an active role in the constitution of the external world. Rescher's idealism falls under this description, but is quite unlike more familiar varieties of idealism. It is not an ontological idealism, which holds that the existence of the external world is dependent upon minds, nor is it a (trivial) epistemological idealism, which merely holds that knowledge of the external world requires mental functionings. Rather, Rescher's idealism is of the conceptual sort, in that it holds that the concepts we deploy in learning about and describing the world are, at bottom, mind-dependent. That is, the conceptual schemes we employ in science make (implicit) reference to mental functionings. This has the implication that the knowledge gained through use of such conceptual schemes cannot be knowledge of external things in and of themselves, but rather of things as they are known by mental agents. Before that result is explored, however, I will review and assess Rescher's arguments for his conceptual idealism.

2.1 Possibility as Mind-Involving

The cornerstone of Rescher's arguments about the mind-involvement of our standard conceptual apparatus is the status of unrealized possibility. Throughout this paper (as throughout *Conceptual Idealism*), it will be maintained as an unargued assumption that there is an external world whose existence and functionings are independent of any mental functionings. Thus, actual, existent objects (i.e., the objects of science) admit of the following distinction:

(1) The actual, objectively existing thing or state of affairs

(2) The thought (hypothesis, assertion, supposition, etc.) of this thing or state of affairs.[11]

It is a nearly trivial point that our only access to (1) is by way of (2)--that our access to the real consituents of the world is constrained by what we know or think of them. But (and this is what has kept scientists busy for several hundred years) there is a logical gap between the two: the actual properties or behavior of the state of affairs may quite different from what we think them to be. We therefore engage in scientific inquiry in an attempt to bring (2) in line with (1), to make the world-as-we-know-it as much like the world-in-itself as possible.

Consider, though, an attempt at making the same distinction with a thing that exists only as an object of thought (say, a nonexistent cat). We have:

(1) the cat-of-my-thoughts

(2) the *purported* cat *as I think of it*.

In such a case, the gap between (1) and (2) has been closed.[12] It simply makes no sense whatsoever to say, "I was imagining that there was a brown cat on the mat, but it turned out to be a white one," unless there is an *actual* state of affairs which is being described. If there is no actual cat-on-the-mat, then to speak of differences between the object (the cat-of-my-thoughts) and the conception thereof (the cat-as-I-think-of-it) is to commit literal nonsense. The only existence (1) has is as an object of thought, and so it is in principle impossible to have a mistaken conception of it.

One might object that it is perfectly sensible to speak of mistaken conceptions of objects of thought. For instance, we may be discussing the possible features of the 60th state of the U.S., and one might wonder if it would come before or after the 61st state--surely, the objection goes, to think that the 60th state came after the 61st state would be to have a mistaken conception of it. But this is not to have a mistaken

[11] Rescher, N. *Conceptual Idealism*, Basil Blackwell: 1973, p. 30
[12] Ibid., p. 31

conception of the 60th state of the U.S., but to be mistaken about something else entirely: namely, the way in which numbers are used. To say, "I think the 60th state would come after the 61st state," is to admit that one has no idea how to utilize numbers according to linguistic convention, and so to remove oneself from the very discussion of this possible 60th state.

But perhaps there are substantive ways of being mistaken about such mere possibilities. The debate about the 60th state of the U.S. may center around whether or not it would be located in North America, with reasonable positions being taken on opposite sides. Couldn't a participant in this debate be mistaken about the 60th state with respect to its geographic location? The mere fact of disagreement about the nature of a possibility does entail that there is a 'right' conception of it and a 'wrong' one, for possibilities, unlike actual things, have an element of property-incompleteness. If I consider the possible IRS auditor that would have visited had I not completed my taxes, and the *full extent* of my picture is that of a balding, pudgy man, it is not as though his wallet *really* is in either his left pocket or his right, and we can debate about which is correct. Rather, it is simply neither true nor false that he has a wallet at all--the conception of the possible IRS auditor is incomplete on that point, as well as an infinite number of others. It may be worthwhile to consider the IRS auditor as having a wallet in various locations, but to do so is to *change* what is at issue, for we would no longer be discovering features of the balding, pudgy male auditor, but changing the object of conception from "a balding, pudgy male auditor" to "a balding, pudgy male auditor with a wallet somewhere on him." Thus, one could be mistaken about the location of the 60th state of the U.S. (insofar as that is the total dsescription of the possibility) only if there is an actual 60th state, at some point in time, to which our conceptions could be compared. In any event, the claim that mistaken conceptions of mere possibilities are impossible remains untouched.

This rather simple point has far-reaching effects. For what separates a mere possiblity from an actual, existent thing is the independent foothold in reality that actual things have and merely possible things lack. The sphere of the actual encompasses only what is mind-independently real--it is just not feasible to hold that there is some sort of Platonic realm wherein the unrealized possibilities of the world reside. Unactualized possibilities, then, do not exist in the world of the mind-independently real, but only as objects of thought. To make reference to an unrealized possibility, then, is to make reference to a mentalistic realm, and so to transcend the realm of the actual.

Following Rescher, then, the current line of argument can be summarized as follows:

> (1) The world only contains, as its real constituents, that which is actually existent.
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> (2) Unrealized possibilities, *ex hypothesi*, are not part of the realm of the actual, but exist only as objects of thought.
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> (3) Possibility-talk is pointful only when such unrealized states of affairs are operative.[13]
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> (4) Thus, to refer to a possibility is to invoke the functionings of mentality, in that mental functionings, and *only* mental functionings, can bring unrealized possibility on the scene.[14]

It is this last point that brings out the mind-dependency at issue here. To speak of possibilities is necessarily to transcend what is actually existent, and so a world of which possibilities are a part is a world that does not exist (*as* the world it is) independently of mental functioning.

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[13] That is to say, what may be called 'epistemic possibility' is of no concern to this position. For instance, the 'possibility' operative in, "It is possible that his plane crashed, since I haven't seen him since he was due to arrive," is not a true possibility in the sense relevant here, since there is (presumably) an objective fact of the matter: either the plane crashed or it didn't, even though I don't know which is the case.

[14] This is my recasting of Rescher's original argument; compare with the argument on p. 50 of *Conceptual Idealism*.

2.2 Lawfulness as Mind-Involving

Having established the conceptual mind-involvement of possibility, Rescher next turns to lawfulness. Laws, everyone agrees, are universal generalizations: in their simplest form, statements of the form "All X's are Y's." It is crucial, though, to distinguish between lawful generalizations on the one hand and accidental generalizations on the other. Consider the difference between "All ants have six legs" and "All creatures in my yard have six legs." While both statements may be considered true, there are drastic differences which make the former useful for prediction and explanation in a way that the latter is not. The former is a lawful generalization not only because it is true of the world (for the latter is also true), but because it has nomic necessity and hypothetical force. Nomic necessity is the element of the 'must': not only do all currently existing ants (under normal developmental circumstances, etc.) have six legs, but it *must* be the case that they do. This relates to the hypothetical force of the law: if this spider, which is not an ant, were an ant, then it would have six legs. As Rescher says, "it is preeminently this element of hypothetical force that distinguishes a genuinely lawful generalization from an accidental" one.[15] We are prepared to accept that this spider would have six legs if it were an ant, but not that this spider would have six legs if it were in my yard.[16] This is not intended to point out what informs us that a generalization is a law, rather than an accidentally true statement; instead, it points out the difference in *treating* something as a law rather than as an accidentally true universal statement.

Lawful claims are thus marked not only by being universally true, but by

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[15] *Conceptual Idealism*, p. 60

[16] cf. Hempel on Ryle, in Hempel, C.G., *Aspects of Scientific Explanation*, Free Press: 1965, p. 354: "[Laws] characteristically function as inference licenses authorizing inferential moves from the assertion of some factual statements to the assertion of others."

making claims about the unactualized. The claim that "Copper conducts electricity" does not speak merely of what happens to exist in the world, but about unactualized possibilities: about states of affairs which, *ex hypothesi,* are not the case. It tells us not only that these pieces of actual copper conduct electricity, but that if this piece of rubber were a piece of copper, then it too would conduct electricity. This actuality-transcending element of laws is absolutely essential for their status as laws. It has already been established that the sphere of the possible is mind-invoking, for possibilities lack the foothold in reality which would make their existence more than as mere objects of thought. By necessarily invoking the domain of the possible, then, Rescher concludes that laws themselves are conceptually mind-involving--they cannot be fully understood without reference to the products of the workings of minds.[17]

2.2.1 Causality as Mind-Involving

The concept of a scientific law, and of lawful behavior, is as pervasive a concept as there is in our commonly deployed conceptual framework, and so the mind-involvement of scientific laws has far-reaching implications. For instance, it is one of few points of almost-universal agreement among philosophers of science that the concept of causality is such that lawfulness must be invoked: there is no causality except under the rubric of a causal law. Since causality makes such a reference to lawfulness, it is a mind-involving concept. To say 'The force imparted by A caused B to move' is to make an actuality-transcending, and so mind-invoking, statement.

[17] Rescher often uses the phrase "the workings of minds" rather than "the products of the workings of minds," which I have used here. Rescher's word choice doesn't seem to be warranted, since it is the unrealized possibilties themselves which introduce the element of mind-involvement. These possibilities are the results of mental functioning, and so referring to them is no more to refer to the mental process than referring to a building is to refer to its construction. This, though, does not affect the crucial claim that these possibilites are entirely mentalistic in nature, as seen by the inseparable link between the possibility and the conception thereof.

Granted, some have tried to hold that, since the goal of science is to explain what is actually before us, such actuality-transcending elements of science are irrelevant.[18] The mark of a law, on such a view, is its universal truth, and not its hypothetical force. Such views, though, are plagued by difficulties, especially when statistical laws are operative: it may be an entirely acceptable law (by any reasonable standard) that an electron in a certain state has a .5 probability of jumping orbits, even though one set of experiments showed the electron jumping six out of ten times, and another showed a jump on four of ten occasions. Statistical laws are not true universally--that is, it is not the case that any set of observed phenomena will behave as described by the law--and so accomodation of statistical laws provides powerful motivation to view laws as actuality-transcending. Even though *this* set of observations did not show a .5 probability of jumping orbits, it is still the case that *if* more observations were made, *then* the observed probability would be (closer to) .5.

The mind-involvement of causality is but one ramification of the mind-involvement of lawfulness generally. Throughout this chapter, more of these ramifications will come to light. In the sections ahead, we will see how it is that particularity, space, time, and empirical properties are all rendered mind-involving through their ties to lawfulness, and often for other reasons as well.

2.3 Particularity as Mind-Involving

2.3.1 Particulars as Property-Having

How is it that we dub a collection of matter as a 'thing'? The standard way of doing so is by reference to the properties of the thing: for example, we may identify a lump of wax as 'that thing in Descartes' hand,' 'that yellowish blob,' or 'the only

[18] cf., Ludwig Wittgenstein in his middle period, as quoted by H. Speigelberg in *American Philosophical Quarterly*, vol. 5 (1968), p. 256: "Physics wants to establish regularities; it does not look for what is possible."

thing on the table that melts at less than 200 degrees Farenheit.' The fact that all of these descriptions are true of one thing is, obviously, a contingent one. The world might well have been constituted in such a way that there was one thing in the room which was yellowish, and another which had such a melting point. Essential to the thing being what it is ('a lump of wax'), is the *lawful concatenation* of (at least some of) these properties.[19] The very identifiability of things in the world is crucially dependent on the lawful behavior of their effects on us. To use Rescher's example, if something we considered to be an apple did not "'behave like an apple'--if on the inside it were leaden, or if it looked triangular from above, etc., etc.--then we simply could not maintain the claim that it was an apple."[20] For a collection of matter (or, less theory-ladenly, 'stuff') to be a *particular thing*, it must behave lawfully: it must affect our sensory apparatus in regular and (at least in principle) predictable ways. However, this necessary involvement of lawfulness in our conception of particularity brings us back to the realm of the mind-involving. Lawfulness, through its counterfactual-supporting nature, is conceptually mind-involving, so particularity, by utilizing lawful behavior as a defining element, makes reference to the objects of the workings of minds in its conceptual unpacking.

2.3.2 Particulars as Ostended

It may be objected, though, that although the lawful concatenation of properties is perhaps the most common way of defining a particular, it is not the only way: we may *ostend* the object in question by actually pointing it out. Even granting the possibly controversial point that ostension alone can be sufficient to identify a thing (Quine's 'gavagai' comes to mind), this objection fails to affect the

[19] This is not to adopt an essentialist construal of things-in-themselves; but for the thing to be the *thing that it is*, some of its properties (i.e., those operative in defining what sort of thing it is) must hang together lawfully.
[20] *Conceptual Idealism*, p. 107

mind-involvement of particularity, for the mere act of ostension is something which necessarily involves a mind. By its very nature, ostension is an attention-directing device--it calls upon a mind to focus upon one thing rather than another. Lacking this mental involvement, the concept of ostension is not viable: in a universe denuded of mental activity, nothing can ever be 'pointed out.'

This, Rescher shows, brings out an even deeper level of mind-involvement in the concept of particularity. The act of identification, whether through physical ostension or descriptive concepts, is inherently mentalistic: to identify something is to focus the attention of one's mind upon it. Objects must be thought of, much as they must be seen, from a certain perspective. Identification, then, is relative to a certain intellectual 'point of view.' In the act of identification, the mind's attention is brought to focus upon certain framework-relative features of an object, features which distinguish that object from others and therefore make it an *individual*. Thus, the conception of individuality, or particularity, is mind-involving insofar as it requires that things be *considered from a certain point of view*. Without such a 'point of view,' we have no resources to distinguish one part of our world from another.

This last point may be seen as a purely epistemic one that has no bearing on the ontological claims being made. That is, the fact that such a 'point of view' is required to make sense of particularity does not seem to affect the existence of particular things. But as the current line of argument is directed at showing the mind-involvement not of the world itself, but rather of the world-as-we-know-it, there is an inevitable link between the epistemological and the ontological. For the scientific realist claims that the world-in-itself and the world-as-we-know-it are essentially the same, a claim that is denied by these arguments. If the world-in-itself is to correspond to the world-as-we-know-it, and the world-as-we-know-it is one of particular things, then one had better be able to explain how the world-in-itself is

composed of things. If there is no way of understanding 'thinghood' without invoking the mental, then the claim that the world-in-itself is one of things (while, again, remaining denuded of mental activity) seems highly dubious. It becomes far more likely to suppose that minds are active in the constitution of the world of things, rather than passive in receiving sensory reports of the 'things' out there.

Rescher rightly emphasizes the importance of focusing on the particularity of things and not, say, on their perceivability or describability. For while perceivability and describability are mind-involving through their invocation of possibility, they are only certain features among others, and one might still hold out hope that mind-independent features of things might come to light. However, by focusing upon the very *thinghood* of things, the issue is necessarily different. The *very existence of an individual as the individual it is* involves reference to the workings of minds. We cannot hope to discover completely mind-independent truths of a thing, when its existence *as a thing* is mind-dependent. It is worth re-emphasizing at this point that this argument is not directed towards showing that minds somehow bring physical objects into existence. The 'stuff' of the universe, out of which things are constituted, is out there, existing independently of any mental functioning. But *things* do not exist independently of mental functioning, and the way we know about the world depends, at its most basic level, on the idea that the world is composed of things that interact with each other.

2.3.3 Summary

To summarize, then, the present line of argument has shown that any route one attempts to take at understanding the concept of particularity is conceptually mind-involving. It is central to our notion of what it means to be a thing that the thing in question behaves in certain ways; specifically, that its behavior is law-governed. Since lawfulness has been shown to be mind-involving, thinghood is as

well. Even without the argument that lawfulness is mind-involving, one can still arrive at the mind-involvement of particularity. To be a thing is to be either identified or identifiable: identification or ostension is, necessarily, a mentalistic act, and identifiability, by invoking possibility, also makes reference to mental functionings. Thus, our conception of what it is to be a thing is a mind-involving one.

2.4 Time as Mind-Involving

It is *prima facie* difficult to accept the idea that time is a mind-involving concept, for one may immediately object: 'The assertion that time is mind-dependent means that in the absence of minds, there is no time. But since the concept of change requires the concept of time, to say 'Without minds, there is no time' implies 'Without minds, there is no change,' which is patently absurd.'[21] It is agreed that any philosophical theory which does not provide for the possibility of change outside of the sphere of the mental is not to be preferred, but the present theory does not fall prey to this. The way out lies in a careful examination of the premise that since change requires time, without time there would be no change.

Rescher offers a distinction between *bare temporality* on the one hand, and full-blooded *time proper* on the other. Bare temporality is constituted by two conceptual considerations:

(1) the idea of different occasions or junctures at which varying states of affairs obtain. Rescher labels this as *occasion diversity*.

(2) the idea that these different occasions fall into a one-dimensional ordering of earlier / simultaneous with / later. Rescher calls this *occasion ordinality*.

Time proper involves two additional concepts:

[21] *Conceptual Idealism*, p. 119-20.

(3) the idea that not only do different occasions fall into a one-dimensional ordering, but into a sequence which is *measurable* and in which chronometry is possible. This is termed *temporal measurability* by Rescher.

(4) the concept of 'the present' or 'now' as one outstanding juncture in the sequence. Rescher calls this *temporal interiorization*.[22]

The two conceptual cornerstones of time (i.e., time proper) each admit of mind-involvement, though in distinctly different ways. First, consider the requirement of temporal measurability. Note that this component is distinct from the ordinality of basic temporality: a well-ordered succession of occasions could still be quite chaotic insofar as intervals between occasions may not be well-defined at all. A much higher degree of order is called for for chronometry to be possible--specifically, Rescher claims, the 'existence of duly correlated periodic processes.' In order for these periodic processes to be used as a basis for *measurement* of change, they must behave *lawfully*. Chronometric measurement depends crucially upon statements of the form 'If one were to do X, then the observed result would be Y' being true. These possibility-invoking laws bring us back to earlier arguments, arguments which showed that the concept of lawfulness itself is mind-involving. By requiring lawful behavior, time makes reference to the domain of the purely mental.

The second element of time, that of temporal interiorization, is not only conceptually mind-involving (due to the fact that identification of temporal *particulars* is involved) but outright mind-dependent. It is an ineliminable feature of our concept of time that there is one characteristically outstanding moment from within the sequence of junctures: the 'now-of-the-present.' The identification of the 'now' as a moment of time functions in essentially the same way as generic identification of particulars, with the qualification that the 'now' can *only* be

[22] Ibid., p. 120-1.

distinguished in an ostensive (i.e., experience-involving) way. Only by experiential participation in the flow of time, Rescher claims, can a certain moment be picked out as 'now' or 'the present.' This presentness of the now is inherently experiential, and is thus mind-dependent. Again, though, this mind-dependence is not *causal* (that is, it is not being argued that at first there was bare temporality, and with the evolution of minds, time came into being) but rather *conceptual*: an understanding of the concept of 'presentness' requires, on the very face of it, a mind.

However, though the mind-involvement of these key elements of time may be conceded, Rescher's distinction is still troublesome. Bare temporality is said to be marked by a plurality of junctures ordered one-dimensionally, which seems to involve the introduction of particulars on the scene; for instance, a 'plurality of junctures' implies that the junctures have separate existences as particular sections of temporality. For Rescher to hold that an allegedly non-mind-involving conception of temporality involves particular junctures with certain features is for him to contradict his own arguments. Thus, the initial objection of this section, which Rescher resolves through the non-mind-involving conception of temporality, is not fully dealt with. But the insight behind Rescher's faulty solution, that there can be change even though our full-blooded conception of time is a mind-involving one, remains as a resource for answering that objection.

2.5 Space as Mind-Involving

The argument that our concept of space is mind-involving runs parallel to the previous argument about time, and involves analogous distinctions. Specifically, we can distinguish between what Rescher calls *minimal spatiality* and *full-blooded space*. Minimal spatiality has two primary features:

(1) A plurality of positions, and

(2) That these positions are located relative to one another within a certain

ordering-structure.

Our full concept of space involves two additional considerations, the concepts of *orientation* and *distance-mensuration:*

> (3) Orientation is analogous to temporal interiorization, in that it gives meaning to 'here' and 'there' in the same way that meaning was given to 'earlier' and 'later.' The concept of 'thereness' involves the notion of connectedness: that 'there' is connected to 'here' in a way that makes it possible for a particular to get from here to there. Just as a point in time is not 'in the future' if it is in principle impossible to reach that point by progessing through time, so a point in space is not 'there' (or part of space at all) if it is impossible to get there from 'here.'
>
> (4) The condition of distance-mensuration implies that for any two points in space, it is possible to determine a well-defined, numerical *distance* between them through use of a measurement procedure.[23]

Rescher's argument for the mind-involvement of full-blooded space is essentially the same as that for time, so I will only review it briefly. For measurement to be possible, certain regularities must obtain: namely, the space being measured must behave lawfully. It must not only be the case that a certain quantity describes the distance between two points, but that *if* the points were situated differently, then certain results would follow. This essential component of lawfulness brings this feature of space into the sphere of the mind-involving, by way of earlier arguments. And as in the arguments about the nature of time, spatial positioning is mind-invoking not only because of the ostensive and fundamentally experiential nature of orienting oneself in space (that is, of locating oneself 'here'), but also because the location of *particulars* is at issue.

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[23] Ibid., p. 134-5.

Again, Rescher seems to fall prey to the same difficulty that undermined his discussion of time, for minimal spatiality, insofar as it involves *particular* positions, seems shot-through with mind-involvement as well. And, as was the case last section with time, the mind-involvement of full-blooded space remains, but the intuitive objections to such a position gain force. Something like minimal spatiality is needed to avoid the objection that this position ties together the limits of the universe with the limits of our conception thereof, but, unlike Rescher's construal of it, such minimal spatiality must be free of mind-involvement.

2.6 Empirical Properties as Mind-Involving

In this section, the ambitious thesis that *all* properties are necessarily mind-invoking will not be argued, for the current position is quite comfortable with the notion that the stuff-of-the-world might well have perfectly real, mind-independent properties. What *will* be argued for, as in previous sections, is a kind of *de facto* mind-involvement: that the properties which we standardly employ in describing the world (namely, empirical ones) make tacit reference to mental functioning. The mind-involvement of empirical properties--here defined as 'properties which are experientially manifest'--is twofold. First, and most straightforwardly, consider what is being said when it is held that empirical properties are those 'experientially manifest features of things.' To describe an experiential feature of a thing is to describe a *relation* between the thing and an observer. That is, empirical properties, by their very nature, report how it is that a thing affects a (presumably mind-endowed) perceiver, and so have an essential reference to the workings of minds. One might immediately object:

> I grant that empirical properties, by reporting effects on perceivers, do make reference to minds when considered as such. But, as it is allowed that there can be mind-independent, objective properties of things, when an empirical

> property *P* is attributed to a thing, it is because there is an objective property *P'* in virtue of which attributing *P* to the thing in question is correct. It is this property *P'*, and not the mind-involving *P*, which is really at issue.[24]

Rescher disputes this objection by questioning the assertion that "there is an objective [mind-independent] property *P'* in virtue of which attributing *P"* is correct, for the feature of the thing which gives rise to the attribution of empirical property *P* is not an objective property at all, but instead a relational one. We are simply not justified in holding that there is some mind-independent property which correlates with a mind-involving one--we are only licensed to infer that a feature of the *transaction* between object and mind gives rise to the property *P*. The relationship between object and observer is crucially important, and cannot simply be reduced to features of the object itself.

An analogy may be useful: a painting with a jarring mixture of colors may make me uneasy, and so we can say it has the property of 'D-uneasiness' (meaning, 'makes Dennis uneasy.') The strategy of the above objector would be to say that, though the property of 'D-uneasiness' is clearly mind-referential, it is in virtue of the (for this example, mind-independent) color-properties of the painting that 'D-uneasiness' is correctly attributed to it. But there is something lacking in this reduction, for my reactions to colors are clearly contingent upon a great many things, and I might have been so constituted as to enjoy such mixtures. It is not merely because of the colors that the painting is D-uneasy, but because of the relational matter of the effects the colors have on me. This relational matter encompasses not only the (again, for this example, objective) properties of the painting, but also the way in which these properties interact with the conceptual scheme brought to bear on them. The case with empirical properties generally is

[24] Ibid., p. 145.

much the same, in that the functionings of minds have an essential, ineliminable role, but an important disanalogy should be made clear: it is not the idiosyncratic, personal transactions with real properties that give rise to empirical properties, but rather the generic way in which our minds *in general* interact with the world.

Not only are empirical properties mind-involving through their reporting effects on percievers, but through their dispositional nature as well. As Nelson Goodman points out in *Fact, Fiction, and Forecast*, not only are such predicates as 'flexible' or 'soluble' dispositional, but also such run-of-the-mill empirical properties as 'hard' or 'is a dog.'[25] For all such properties have a component of 'what-would-happen-if': the statement 'The table is hard' says more than just that all prior actual occurences of contact with the table have resulted in considerable resistance, but also that if one was to have touched the table yesterday, when in fact one did not, considerable resistance still would have been met with. Thus, dispositional properties correlate with lawful behavior through their encompassing of not only the actual, but the unrealized as well. The arguments for the mind-involvement of lawfulness can then be brought to bear on dispositional empirical properties. Insofar as describing something as, say, being a 'conductor of electricity' attributes lawful behavior to that thing, such a description is mind-involving.

2.7 Summary

This chapter has reconstructed Nicholas Rescher's arguments about the mind-referentiality of our standard conceptual scheme. Rather than being done in a transcendental way (like the more familiar Kantian arguments about the mere possibility of experience), this was shown in a *de facto* manner: a point-by-point examination of the key concepts of our standard framework shows that the

[25] Goodman, N., *Fact, Fiction, and Forecast*, Harvard Press: 1955 p. 40-9.

meanings of these concepts are tied to mental functionings. That is, what these concepts *say* about the world they describe invokes, in crucial part, the workings of minds. To say that something behaves lawfully, for instance, is to locate it against a backdrop of unrealized possibility, and so to describe it in terms that refer to more than what is actually existent. The ramifications of these arguments will be fully explored in the next chapter, but, for now, the kind of effect these arguments have on a robust scientific realism should be coming clear: if, as the realist claims, science describes the world as it is, then the terms of such description should not go beyond what is actually in the world. But, as our standard concepts go beyond the realm of the actual (and invoke the realm of the mental) in significant ways, our standard methods of description are not up to the task of realistic theorizing.

Chapter Three: Explanations of Scientific Success

3.1 Explanations Generally

Let us return now to the issue of how the success of science ought to be explained. As it is an *explanation* that we seek, we find ourselves exercising not deduction or induction, but rather, as Peirce emphasized, *abductive inference*. To make an abductive inference is to adopt (tentatively and defeasibly) an explanation for a known state of affairs--thus, the frequent labeling of abduction as 'inference to the best explanation.' The general form of an abductive inference is this:

(**P1**) The surprising fact, C, is observed;
(**P2**) But if A were true, C would be a matter of course,
(**AC**) Hence, there is reason to suspect that A is true.[26]

Obviously, not just any candidate for explanation (i.e., any A) is acceptable--for instance, suggesting that airplanes are observed to fly because of mass hallucinations seems to be far less adequate an explanation than invoking the various laws of physics operative in sustaining heavier-than-air travel. What, then, consitutes a viable explanation? Peirce himself was rather vague on this subject, saying that the more "facile and natural" explanation, "the one that instinct suggests,"[27] is to be preferred. We do not need to make this much more precise in order to arrive at reasonable criteria for adjudicating among competing explanations (as I do not wish to tie the preference of one explanation over another to any particular theory of explanation, such a lack of precision in criteria is desirable, so long as the criteria remain clear and workable).

First, a proposed explanation must, obviously, actually explain the phenomena in question: "If [the explanation] were true, [the phenomena] would be a matter of course." That is, the conjunction of the explanation and the relevant

[26] Peirce, C.S., "Abduction and Induction," in *Philosophical Writings of Peirce*, Buchler (ed.), Dover: 1955, p. 151
[27] Ibid., p. 156

background information must give us sufficient reason to expect the phenomena. Moreover, Peirce's remarks about the 'naturalness' of an explanation seems to suggest that the explanation must not conflict with propositions we are not prepared to give up; that is, with the 'known facts.' In the above example, since we are relatively deeply committed to the idea that widespread hallucinations of mysterious origin do not systematically occur when airplanes are preparing to take off, we reject any explanation that conflicts with it. However, trade-offs are possible: we may abandon previously held beliefs in order to accomodate a particularly powerful or otherwise appealing explanation. Thus, if the hallucinatory explanation of airplane flight led to astoundingly accurate predictions over a wide range of conditions, we may be compelled to give up our old beliefs about flight.

Even if two proposed explanations do not *conflict* with any accepted facts, we can adjudicate between them according to their *fit* with the facts. For example, for a theistic, scientifically-minded person, explaining airplane flight through divine intervention does not seem to *contradict* any previously-held beliefs, but the traditional physical explanation coheres with our body of knowledge in a way that the divine explanation does not. Clarifying this notion of 'fit' or 'coherence' is a project unto itself, but, fortunately, we do not need a very precise definition to continue the current project. Let it simply be noted that better explanations tend to have a relationship of mutual support with relevant background facts in explaining a phenomenon, while suspect explanations tend to be increasingly *ad hoc*, ignoring what appears to be relevant information or viable alternative avenues to explanation in favor of more idiosyncratic claims.

Finally, an acceptable explanation should be *simpler* than any known rivals of approximately equal explanatory power. As Boyd reminds us, simplicity is a profoundly theory-dependent notion, but this need not deter us from utilizing it here; after all, we have to start somewhere, with some (theoretical) beliefs forming

the background against which current work is judged. The preferable explanation is one which postulates the fewest number of entities or processes in whose existence we lack independent grounds for believing.

We have, then, a reasonable list of criteria for judging explanations:

> (**C1**) The proposed explanation must, together with all relevant background information, provide reason to expect that thing which is explained.

This requirement emerges straightforwardly from Peirce's formulation of abductive inference generally. There are a number of different ways in which this requirement can be spelled out; perhaps the most famous is Hempel's formulation of deductive-nomological (D-N) explanations:

$$\begin{array}{ll} C_1, C_2, ..., C_n & \\ & \text{(Explanans)} \\ L_1, L_2, ..., L_m & \\ \hline \quad E & \text{(Explanandum)}^{28} \end{array}$$

In such an explanatory structure, the statements $C_1 ... C_n$ represent the particular facts invoked, while $L_1 ... L_m$ are the general laws upon which the explanation rests. The conjunction of the C_i's and L_i's entail E, a statement of the phenomenon to be explained. The laws themselves, key premises in the deduction, are not arrived at through such a deductive process, however: they are the result of various abductive inferences. This is one example of the relation between the various modes of inference when it comes to explanation, and it highlights the way in which an explanation is *arrived at* in one fashion (i.e., abductively) and is *utilized* in another (i.e., through deductive or otherwise reliable arguments).

> (**C2**) The proposed explanation must not conflict with more deeply-held beliefs, and should cohere with these beliefs better than its known rivals.

Logical truths are the paradigm instances of what an explanation must be consistent

[28] Hempel, Carl G., *Aspects of Scientific Explanation*, Free Press: 1965, p. 336

with, and explanations must usually also be consistent with accepted empirical knowledge. As Quine reminds us, though, anything and everything is potentially revisable in light of experience; and so if experience compels us to sacrifice a truth of logic to accomodate a scientific explanation (e.g., the suggestion that the distributive law be abandoned in quantum-physical explanation), so be it. As a general rule, though, (**C2**) suffices in most cases.

> (**C3**) The proposed explanation should be simpler than any rival of comparable power.

With these criteria in hand, let us revisit previously considered explanations of the success of science.

3.2 Boyd's Realism and Kuhn's Constructivism

With Peirce's framework in place, realist and constructivist arguments about the success of science can be placed in the abductive mold. First, consider Boyd's version of the no-miracles argument. In this case, the C (surprising observed fact) at issue is the instrumental success of science, or, more precisely, "that a methodology so theory-laden as ours should prove reliable in navigating through the world." Boyd's proposed explanation is that the theories which are constitutive of this methodology reflect the real structure of the world. Thus, we have:

> (**BP1**) It is surprising that such a theory-laden methodology proves so successful in navigating through the world;
> (**BP2**) But if the theories which function in this methodology reflect the real <u>nature of the world, such success would be a matter of course,</u>
> (**BC**) Therefore, we have good reason to think that our scientific theories reflect the real nature of the world.

How does this differ from the more straightforward no-miracles argument? Without Boyd's focus on methods and procedures, as opposed to specific claims and theories, the premises would be:

> (**RP1**) It is surprising that scientific theories prove to be reliable indicators of

future phenomena;
(**RP2**) But if these theories reflect the structure of the world, such reliability would be expected.

with the conclusion unchanged. Using our criteria of explanation, we can illuminate Boyd's claim that this argument is faulty for not addressing the epistemic thrust of anti-realist arguments based on the evidential indistinguishability thesis (the claim that, since infinitely many theories could have identical observational consequences, no amount of evidence could justify us choosing one over another). It was noted that better explanations tend to utilize all relevant information, and the fact that an infinite variety of theories might be equally well-supported by observational evidence certainly *seems* relevant to the issue of scientific success. By ignoring this fact, this no-miracles explanation proves inadequate. But Boyd's explanation provides good reasons to think that the EIT is not a relevant fact at all; that, indeed, it is not a *fact* at all. According to his arguments, a theory which emerges from our actual theoretical tradition is better supported *evidentially* than one which does not, since this tradition is comprised of methodologies which employ approximately true descriptions of the world.

What, then, of Kuhn-style constructivist arguments? In his enormously influential *The Structure of Scientific Revolutions*, Thomas Kuhn provides a distinctly anti-realist picture of the way science works, a picture in which the nature of the world science describes is inextricably linked to the way in which it is described. Consider the following quotes which, though admittedly presented with little context, still provide a sense of the important epistemological threads of Kuhn's work:

> ...[D]uring revolutions scientists see new and different things when looking with familiar instruments in places they have looked before. It is rather as if the professional community had been suddenly transported to another planet where familiar objects are seen in a different light and are joined by unfamiliar ones as well....In so far as their only recourse to the world is through what they see and do, we may want to say that after a revolution

> scientists are responding to a different world.[29]

And in a discussion of Dalton's chemistry:

> But it is hard to make nature fit a paradigm....Chemists could not, therefore, simply accept Dalton's theory on the evidence, for much of that was still negative. Instead, even after accepting they theory, they had still to beat nature into line, a process which, in the event, took almost another generation. When it was done, even the percentage composition of well-known compounds was different. The data themselves had changed. That is [a sense] in which we may want to say that after a revolution scientists work in a different world.[30]

On a view such as Kuhn's, the mind (and the conceptual frameworks employed by minds) are the driving force in the constitution of the external world: scientists working under different paradigms inhabit "different worlds." Could an argument for an explanation of the success of science based on such constructivism fare better than Boyd's? The first premise of such an argument would be the same as Boyd's, but with a different explanation in the second premise:

> (**KP1**) It is surprising that such a theory-laden methodology proves so successful in navigating through the world;
> (**KP2**) But if the theories utilized in this methodology were constitutive of the <u>world, such success would be expected,</u>
> (**KC**) Therefore, we have good reason to think that our theories are constitutive of the world.

The 'fit' component of (**C2**) is violated by this constructivist explanation. A vast set of beliefs, both scientific and commonsensical, points to the existence of a world which exists independently of our theorizing. While this in itself is not sufficient reason to reject the constructivist explanation (as trade-offs are possible), the existence of an equally powerful explanation (namely, Boyd's realist explanation) which does not require us to discount this set of beliefs compels us to prefer it to the constructivist one.

However, powerful as it may be, Boyd's realism will not suffice as an

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[29] Kuhn, T. *The Structure of Scientific Revolutions*, University of Chicago Press: 1970, p. 111
[30] Ibid., p. 135

explanation of scientific success. As arguments in the last chapter have shown, our scientific picture of the world, insofar as it is one of particular things (with empirical properties) that interact causally in space and time, is one shot through with mind-involving conceptions. The nature of concepts like 'particular thing,' 'space,' and 'time' is such that mental functionings must be referred to in explicating their meanings. As it is axiomatic to the current argument that the external world does not possess mentalistic abilities, properties which invoke such abilities cannot be held to apply to externality. Thus, the world-in-itself, which Boyd holds is approximately described by scientific theorizing, is one not of particular things, but perhaps one of generic 'stuff.' Nor is it one of causal relations, but perhaps it is one containing abstract patterns of regularity. The external world might possess properties, but they are necessarily non-empirical. In short, the world-in-itself is quite unlike the picture we have of it.

Boyd's explanation of the instrumental success of science thus violates condition (**C2**) of explanatory adequacy. We wish to explain how it is that *these* concepts, meaning what they mean, prove so instrumentally reliable when utilized in scientific theories, so the nature of these concepts must be taken as a given. Since the logical consequences of these concepts are incompatible with Boyd's realism, an alternative explanation must be brought to light. With the resources of conceptual idealism in hand, such an explanation can be formulated.

3.3 The Conceptual Idealist Explanation

Essentially, a conceptual idealist explanation of the instrumental success of science involves recognition of the contributions to scientific success made by both the external world and mental functionings. The above examination of the nature of the world-in-itself (as opposed to the world-as-we-know-it) reveals that it is too sparse and barren to account for science's success--a world of particular things

interacting causally in space and time is only possible when mental functionings play a role, yet we have astounding success in predicting how *this* particular thing will cause *that* particular thing to alter its place in space and time. Clearly, more must be at work than the external world alone. The world we know, and the world in which scientific success is evident, is, as Rescher terms it, a 'chemical' combination of the world-in-itself and the active contributions of minds and the conceptual schemes they deploy.[31] And just as an adequate explanation of the result of a more commonplace chemical reaction (e.g., sodium and chlorine combining to form salt) cannot rely on features of just one element of the reaction, so too must an adequate explanation of why science is successful in navigating us through this world call upon both factors operative in making our world *this* world.

Within the framework of abductive inference, and in the spirit of Rescher's conceptual idealism, I thus propose the following as an explanation for the success of science; that is, as justification, above and beyond the mere fact of past success, for expecting continued instrumental reliability from scientific theories:

> (**CP1**) It is surprising that a methdology so theory-laden as ours proves reliable in navigating through the world,
> (**CP2**) But if it were the case that (a) the world possesses a structure described approximately at a very abstract (e.g., purely mathematical) level by scientific theories and (b) human minds shared a generic ability to enrich this abstract picture in a consistent way, both in theorizing and in experience, then such <u>success would be a matter of course;</u>
> (**CC**) We therefore have good reason to believe that the conditions of (**P2**) are indeed the case.

This explanation thus points to both of the operative factors that transform the world-in-itself to the world-as-we-know-it: the passive role of externality and the active role of mind. Beyond mere analogy with chemical reactions, do we have good reason to employ both factors, when, as condition (**C3**) tells us, a simpler explanation (i.e., one employing only one factor) would be preferable? Let us

[31] cf., *Conceptual Idealism*, p. 158

examine each component in turn to see if either is dispensable.

First, this explanation relies partly on purported features of externality; can this be eliminated, leaving only the active role of mind in shaping the world we know? To do so would contradict the assumptions of metaphysical realism that have been in place throughout this work (i.e., that there is an external world, and that its functionings are independent of any mental functionings), and so violates (**C2**). This may be little more than 'assuming away' a possible difficulty, but I am prepared to swallow that objection, reminding the reader that there is good motivation for making these conditions of metaphysical realism axiomatic.

Can the active, transforming role of mind then be eliminated, leaving the relationship between theory and world as the locus of explanation? Such an explanation would be tantamount to a robust, Boyd-style realism, and we have seen why such a realism will not do. The arguments for conceptual idealism explored in chapter two show that a rich correspondence between theory and world cannot obtain, and thus cannot be called upon to explain the success of science. Any correspondence between the world we know and the world-in-itself must obtain at a level where talk of such mind-invoking conceptions is absent; i.e., at the abstract levels operative in (**CP2**). Moreover, what the conceptual idealist arguments point to is an inability of the external world to ground the sort of success that science has had--such as success in predicting *future* states of the *empirical properties* of *particular things* after *causal* interactions, where all emphasized terms have essential mind-referential components.

But does this explanation meet the basic requirement of (**C1**): does it actually explain what is to be explained? Boyd's realist explanation gave us reason beyond the fact of past success to expect continued success from scientific theories; can this conceptual idealist view do likewise? To see if it can, we should return to the stronghold of Boyd's view: his illustrations of the instrumental success of theory-

laden methodologies. If the current proposed explanation can explain, in these specific instances of instrumental success, why such success should be expected (without resorting to a full-scale 'mirroring' between theory and world), then we will have arrived at a distinct and workable account of what makes science successful.

3.4 The Adequacy of the Conceptual Idealist Explanation: Boyd Revisited

3.4.1 Boyd's Question of Measurement

Let us begin with the second of Boyd's illustrations, a puzzling instance of measurement. Boyd claims that only with a realistic reading of scientific theories can the fact of two disparate theories giving rise to the same measurement of the value of a theoretical term *t* be explained. He apparently has in mind here something like the measurement of Avogadro's number, whose value was predicted accurately by widely varied theories. But is the realist explanation the only one available? Operating with an assumption not of realism, but of the sort of conceptual idealism espoused here, wherein knowledge of the external world, though possible, is not realized by our theories (insofar as they involve causality, particularity, etc.), such convergence can still be explained. For as widely varied as the $T_1(t)$ and $T_2(t)$ of Boyd's example are, they are not *fundamentally* different views of the world; indeed, as it is the *same t* which is to be measured by the methodologies suggested by each theory, there must be substantial commonalities between the theories (that is, each theory postulates a world of which the sort of entity referred to by *t* is a part). So what is to be explained is not the considerably mysterious case of:

(**M1**) Every theory which we employ to learn about the world converges on a certain kind of entity (referred to by *t*) having a certain value.

but rather the less puzzling:

(**M2**) Every theory which postulates a certain kind of entity (referred to by*t*) arrives at approximately the same value of *t*.

(**M1**) is a case of absolute necessity: no matter how we try to figure out the world, we end up with a certain entity having a certain measurement; (**M2**), on the other hand, is an instance of hypothetical necessity: *if* we consider the world as having the kind of entity referred to by *t*, *then* we always seem to arrive at the same value for t. And this case, (**M2**), does not require a full-blown realist explanation--that is, it does not require that 'measuring *t*' with the theory in hand is really *measuring t*. It is readily explicable using the strategy outlined in (**CP2**) above, since the measurement of a particular is a paradigm instance of utilizing a mind-referential conceptual scheme. The explanation for this convergence of measurement, then, is not that the world really does contain an entity referred to by *t* which has a certain measurement, but that to conceptualize the world as having things of type *t* is to conceptualize it in a mind-invoking way, so arriving at the same value of *t* with disparate theories must be due not only to the features of the world to which our conceptual schemes are brought to bear, but also to the way in which the conceptual schemes utilizing *t consistently transform* this world from one without particular things to one in which things of type *t* have a stable, enduring existence as the things they are.

3.4.2 Boyd's Questions of Simplicity and Experimental Design

The first and third of Boyd's illustrations are quite similar, in that they both highlight how influential our accepted theories are in future scientific endeavors. Our current theories are in the driver's seat when it comes to designing experiments and considering new theories. Having one small set of theories, out of an infinite number of possible (i.e., observationally equivalent) theories, being so influential in the progress of science would seem to hamper science's hopes for continued

instrumental reliability, yet the opposite appears true. Only if these current theories are approximately true of the world, Boyd argues, can this phenomenon be explained; only if these theories are reasonably close to the way the world actually works can we expect the relativization of new theories and experiments to these theories to be consistently successful.

But, bearing in mind how conceptual idealism has shown the need for acknowledging the active role of mind in such scientific pursuits, another, scientifically plausible explanation for the instrumental reliability of these practices can be arrived at. The world we know, it has been repeatedly stressed, is not a passive reflection of the world itself, but rather a chemical combination of the world itself and the active influence of conceptual schemes. Starting with Boyd's third illustration, then, it becomes apparent that what constitutes an anomaly is not merely a certain kind of event in the external world. An anomaly is an occurence *conceptualized in a certain way*. For instance, it was shown that empirical properties are not part of externality proper; when the litmus paper turns blue instead of the expected red, then, it must be a result not only of the happenings of externality, but also of the way in which minds, armed with certain conceptual schemes, *transform* these happenings into empirical-property-laden happenings. When designing experiments (procedures designed to provoke anomalies, if any are to be found), it therefore seems entirely sensible to do so with our current conceptual schemes at the front of our minds, even though these schemes (*contra* Boyd) are not descriptive of the world.

Boyd's first illustration of theory-laden methodological principles can be handled in a similar fashion. The usual anti-realist response to such questions of simplicity judgments is, as Boyd points out, largely pragmatic: it is just easier for us to test relatively simple modifications of existing theories before moving on to more esoteric theories. But, Boyd goes on to say, a crucial question remains unanswered:

why do the relatively simple modifications so often work? Why are the more esoteric theories never called in? Rather than calling on the approximate truth of the pre-modified theories, the instrumental reliability of this practice can be explained by the conceptual idealist. The way we conceptualize the world is relevant to the consideration of new theories because it is relevant to the very *constitution* of the world we inhabit (that is, the world-as-we-know-it). When we need to bring in a new theory, one which is a relatively simple modification of an existing theory is most likely to preserve the features of the world we know, features which have proven to be useful in navigating our way through the world. This is not to say that a more esoteric theory *couldn't* do just as well instrumentally, just that a simple modification of an existing theory is *likely* to do well instrumentally, is considerably easier to arrive at, and requires less of an overhaul in the way we view the world (although, as per (**CP2**(a)), there may be certain non-mind-referential features of a theory which, unlike much of our scientific picture of the world, actually do latch on to the structure of reality; such features would be retained, at least approximately, by succeeding theories for the reasons Boyd gives). Indeed, the only reason a full-blown realist explanation seems to be called for in this case is because the realist picture of mind-passively-reflecting-the-world is so often assumed; when the transformational component of mind is realized, requiring successor theories to resemble their predecessors is seen as an easy and safe route to continued instrumental success.

3.4.3 Summary

The success of theory-laden scientific methodologies in each of Boyd's three illustrations can therefore be explained. What's more, Boyd's distinctly realist assertion that there is "no scientifically plausible explanation of the instrumental reliability of actual scientific methods ... which does not portray those methods as

reliable for the acquisition of theoretical knowledge as well,"[32] has been shown false. In each of these cases of instrumental success, an explanation was arrived at which did not grant these processes reliable for knowledge-acquisition, if 'knowledge' is taken to mean (as, I presume, Boyd would want it to be) an approximately accurate description of the external world. The current position allows that *some* knowledge of the world can be arrived at and built upon by successive theories; but it denies that the whole of our scientific picture of the world is descriptive of the world. Knowledge of the external world must be, by assumption, absent of mind-referential components. The world may indeed have something like, say, a quantum-mechanical mathematical structure, but it is misleading to say that such a mathematical structure represents real causal relations in nature, for to attribute causality to a process is to endow it with a mind-involving conception and thus to remove it from the realm of what is 'objectively' real (in a mind-precluding, realist sense of 'objective').

It seems, then, that these two features of the conceptual idealist position can be called upon to underwrite competing realist and anti-realist intuitions. The realist picture of science as successive approximations to the truth rests comfortably with the accumulation of (mostly abstract) knowledge of externality. But such knowledge does not exhaust what is termed 'scientific knowledge'; the remainder enjoys the status of knowledge not just because of what the nature of externality is, but also because of the ways in which reality is interpreted and organized by human minds. The radical changes in the history of science--Kuhn's 'scientific revolutions'--so often referred to in anti-realist arguments (e.g., Laudan's 'pessimistic induction'[33]) may therefore be seen as radical changes in the interpretation of such abstract, largely mathematical knowledge of the world. Such changes are deep and

[32] "Scientific Realism and Naturalistic Epistemology," p. 617
[33] cf., "A Confutation of Convergent Realism," in *Scientific Realism*

jarring, but, *contra* Kuhn, they stop short of the bone: any successor to a successful theory must operate within (approximately) the same structure postulated by the preceding theory--hence, the recurring phenomenon of one theory's equations being retained, as special or limiting cases, in the equations of a new theory.

3.5 Structural Realism

This picture of science is not unlike the one offered by John Worrall in his "Structural Realism: The Best of Both Worlds?"[34] An examination of his position, with its purported shortcomings, will both clarify the current position and show its desirability as a view of scientific practice. Worrall is also motivated by the competing intuitions outlined above; how can science be so empirically successful when the rejection of past theories gives us good reason to think that our current theories will also, someday, be proven wrong? Both the empirical success of science and the periodic revolutions science experiences can be explained, Worrall argues, if there were accumulation of knowledge at a deep level with more superficial claims being abandoned in the overhaul that accompanies adoption of a new theory. And this is the thrust of structural realism: *relations* in the external world, in the form of mathematical equations involving primitive terms, are discoverable through scientific practice, while the *nature* of the external world (the proper *interpretation* of the terms in said equations) is not so discoverable. As Worrall says:

> [The structural realist] insists that it is a mistake to think that we can ever "understand" the *nature* of the basic furniture of the universe....[She] simply asserts [that], in the view of [quantum mechanics'] enormous empirical success, the structure of the universe is (probably) something like quantum mechanical. It is a mistake to think that we need to understand the nature of the quantum state at all....[35]

This is indeed much like the position here endorsed, but with a rigid distinction

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[34] Worrall, J., "Structural Realism: The Best of Both Worlds?" in *Dialectica*, Vol. 43, No. 1-2 (1989(

[35] Ibid., p. 122-3.

between what is knowable (mathematical relations) and what is not (the nature of the *relata*). This distinction proves troublesome, though, as Stathis Psillos points out in his "Is Structural Realism the Best of Both Worlds?"[36]

3.6 Psillos' Objections

In his discussion of structural realism, Psillos aims to rescue methodological insights from Worrall's position while denying it as a coherent view of science. Attempting to reconstruct a structural realist version of the 'no miracles' argument (i.e., a structural realist explanation of the success of science), Psillos arrives at the following:

> Predictive success is cumulative; that is, subsequent theories predict at least as much as preceding theories predicted. But mathematical structure is also cumulative; that is, subsequent theories incorporate the mathematical structure of preceding theories. Therefore, there is a correlation between the accumulation of mathematical structure and the accumulation of predictions. Successful predictions suggest the theory is on the right track. Then, the claim that the 'carried over' mathematical structure of the theory correctly represents the structure of the world explains this predictive success.[37]

This seems to be a fair argument to attribute to the structural realist, but Psillos has several cogent objections to this line of reasoning. In exploring these, I hope to show how the conceptual idealist position can avoid the pitfalls of structural realism while still providing comparable explanatory power.

3.6.1 Psillos' First Objection

Psillos' first objection is that, for the structural realist to utilize this argument, "she must argue that the mathematical structure of a theory is somehow exclusively

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[36] Psillos, S., "Is Structural Realism the Best of Both Worlds?" in *Dialectica*, vol. 49, No. 1 (1995)

[37] Ibid., p. 27-8. The claim that predictive success is cumulative is not entirely uncontroversial, as the move to Newtonian mechanics from Aristotelian physics was a move away from a well-confirmed body of physics to a bare mathematical sketch of motion. But we can only imagine that, had Newtonian mechanics not lived up to its expectations of eventually providing at least as much predictive success as its predecessor, it would have been abandoned. Thus, predictive success can be seen as generally cumulative, if not step-wise cumulative.

responsible for the predictive success of the theory."[38] But, he goes on, it is a mistake to think that mathematical equations alone can provide any predictions whatsoever. *Some* physical content must be bestowed upon the variables of the equation, and numerous assumptions and auxiliaries must be in place, in order for any predictive success to be achieved. Thus, the structural realist's claim that it is the mathematical structure of a theory that entails empirical success, and that is therefore retained in theory change, is false.

But this objection fails to damage the conceptual idealist position for several reasons. For one, although mathematical structure was suggested as a possible avenue for a mind-independent description of the world, this is by no means the only route available. For instance, it is hard to see how quantum mechanics is a theory about discrete 'things' at all, and how Newtonian mechanics is anything but. So, bearing in mind the mind-involvement of the concept of particularity, it seems that the move from Newtonian physics to quantum physics holds much promise as providing a more accurate picture of the world: to describe a world of things is, *prima facie*, not to describe the world-in-itself. Furthermore, given the success of quantum mechanics, it is likely that any successor theory will also not be thing-based. This feature of the world as described by science (i.e., that the world is not constituted, at its most fundamental level, by discrete things, in the ordinary sense of the word) is non-mathematical, yet can be called upon by the conceptual idealist in explaining the predictive success of physics. The similarity between this position and structural realism does not lie in a sharing of the mathematical structure / physical nature dichotomy, but rather the emphasis on the parts of a theory that 'get it right' about the world being rather deep and abstract parts.

Secondly, Worrall's position falls prey to this objection in a way that

[38] Ibid., p. 28.

conceptual idealism does not because of the distinctly realist flavor of structural realism; i.e., the way in which empirical success is taken to arise from objective features of reality alone (in this case, the mathematical nature of reality). But the conceptual idealist explains scientific success not only through an examination of what is 'out there,' but also through the abilities of the conceptual schemes through which we learn about what is 'out there.' Consider, then, a theory which, it is alleged, accurately reflects only the mathematical structure of the world (for the sake of argument, let us say that Newtonian mechanics is such a theory). Psillos would argue that at least *some* of the physical content of Newtonian mechanics, and not just its mathematical structure, is approximately true of the world. The conceptual idealist, though, argues that the entities and properties of Newtonian mechanics are not actually existent, but that our conceptualization of the world is such that we consistently interpret the world as having such properties. That is, Newtonian mechanics suggests that the world is comprised of discrete things which have certain (mathematically described) relations to each other; the conceptual idealist takes from this not that the world is therefore composed of discrete things, but that *given* that we so consistently view the world as being thing-based, Newton's equations provide an approximately accurate picture how things work (recall the conceptual idealist answer to Boyd's puzzle of measurement).

At this point, though, it might be objected that the floodgates have been opened; for what is to prevent one from extending this line of argument to hold that mathematical relations themselves are imposed by conceptual schemes? This is a well-taken objection, and points to an important feature of conceptual idealism: that this position makes the form / content distinction almost hopelessly fuzzy, since the form imposed by conceptual schemes is so decisively content-conditioning. Our only hope of separating features of reality from features of our minds is to attempt to unearth mind-referential components from such features; since the

assumptions of metaphysical realism entail that true features of the world make no reference to things outside the realm of mind-denuded actuality, unearthing such a mind-referential component removes that feature from consideration of what truly resides in externality. Thus, the success of a theory which utilizes the concept of thinghood does not license us to infer the existence of 'things' in the world, since particularity has essential mind-referential components. But, as there is no apparent mind-referential component to a mathematical structure, such a structure might well inhere in reality. When we have such a non-mind-invoking concept, our best attempt at deciding whether or not such a concept inheres in reality is to subject it to scientific scrutiny in accord with our ordinary methodologies--and these methodologies seem to endorse the idea that, say, the universe has something like a quantum-mechanical mathematical structure.

In summary, then, Psillos might object to the current position, as he does to structural realism, by arguing that mathematical structure alone is not sufficient to explain predictive success. But this objection fails when launched against the conceptual idealist for a couple of reasons. First, although mathematical structure (and the word 'structure' generally) has been used to illustrate what might be realistic about scientific theories, the current position does not rest (as Worrall's apparently does) on a mathematical structure / physical nature dichotomy, and so it is acceptable to the conceptual idealist that some physical content of a theory is also descriptive of the world. Second, the fact that some physical content is necessary to get predictive power from a mathematical equation does not mean that such content is necessarily descriptive of the world, as the (non-structural) realist would have it, for such content might be entirely imposed by the functionings of a conceptual framework.

3.6.2 Psillos' Second Objection

Psillos' second objection concerns the whether the structural realist is the one who can best utilize the above argument:

> [T]his version of the 'no miracles' argument cannot be available only to a structural realist. A scientific realist who claims that we can know the furniture of the world can argue along the same lines to the effect that some theoretical mechanisms--as well as substantial properties and law-like behavior ascribed to entities--are also retained while theories change...The only difference [between defending structural realism and full-blown scientific realism with a 'no miracles'-type of argument] is one of degree, relating to how much excess non-empirical content one is willing to justify belief in.[39]

I embrace this objection wholeheartedly, as it, too, does no damage to the current position. I readily admit that, all things being equal, a full-blown scientific realism is the most desirable explanation of scientific success. But all things are *not* equal (are they ever?): we have overriding reasons from Rescher to think that the scientific picture of a world of particular things, with empirical properties, interacting causally in space and time is *not* the picture of the world-in-itself. Thus, a realistic belief in those properties is not justified, and the question becomes, what *do* we know about the world-in-itself? Is it enough to explain why science works so well in the world? And I have argued here that what we know about the world partially explains the success of science, with the rest of the picture filled in by the ways in which our conceptual schemes transform what we perceive.

[39] Ibid., p. 29.

Chapter Four: An Assessment of Conceptual Idealism

In this chapter, I aim to assess the conceptual idealist explanation of the success of science. I will explore its apparent strengths and weaknesses, and see if it really is adequate as an explanation of why science is successful. First, I will review the content of the current position.

4.1 The Conceptual Idealist Explanation

Essentially, the conceptual idealist draws on both features of the external world and features of the workings of conceptual schemes to explain how it is that scientific theories prove to be so reliable in predicting observable phenomena. There is a strong realist intuition that underlies this position, but what separates this position from a full-blown realism is the claim that the nature of the concepts we deploy in learning about the world is not conducive to describing the world *as it is*. Rather, our concepts describe the world with a distinctive element of the mental added to the picture, especially the ways in which unrealized possibilities form the mentalistic background against which actual events are explained. Since the world-in-itself (a world emptied of mental involvement) does not seem sufficient to ground the success of a conceptual scheme that goes beyond the actual in significant ways, there must be more to the explanatory story.

The 'more' in question is the role of the conceptual schemes themselves. The ways in which we conceptualize the world-in-itself and turn it into the world-we-know must also have some part in the story of why science is so successful.

Explaining, for instance, why a theory was successful in predicting a future event by pointing to a correspondence between the theory and a world of which 'the future' is not properly a part (i.e., the external, mind-independent world) seems destined to fall short. But the external world *conceptualized in a certain way* is not so insufficient, for this conceptualization can add the element of 'the future' to the relatively sparse external world.

4.2 Strengths of This Position

The strongest feature of the conceptual idealist position is its ability to accomodate both realist and anti-realist intutions about scientific practice. The realist picture of science as an accumulation of knowledge can be recast on this position, as it is allowed that some knowledge of externality can indeed be arrived at and built upon by future theories. However, unlike most varieties of realism, this knowledge is not held to be of, for instance, causal relations among things, but at deeper and more abstract levels. Such knowledge, I hold, must be free of mind-involving conceptions, and so must not utilize concepts such as causality or particularity.

Furthermore, and more importantly, an explanation of scientific success, which seemed to be the exclusive property of the realist, can be arrived at on this position. The real, actual events of the world form a part of this explanation, just as with realism, but the element of conceptualization is also seen as a necessary component of scientific success. The realist generally (if implicitly) holds that the role of minds and conceptualization is largely passive: that the mind receives a

more or less accurate picture of what is 'out there.' This claim is untenable in light of the considerations explored in the second chapter, as a relatively accurate picture of what is 'out there' would likely be far different than the picture we currently have, due to the involvement of the mental in the *meanings* of the concepts we deploy.

The anti-realist intuitions which underlie such claims as the evidential indistinguishability thesis (EIT)--the claim that evidence alone does not decide among an infinite number of possible theories--can also be illuminated under the light of this position. Since our picture of the world appears far richer than the 'best guess' we can make about the nature of the world itself (i.e., that it is thingless, amodal, etc.), it seems only reasonable to suppose that there are alternative ways of describing and experiencing the world. What makes *our* way of describing and experiencing the world a good one is linked to what 'good' means in this context: for us, a good way of conceptualizing the world is one which leads to predictive success. We know that conceptualizing the world in this way allows us to get around in it, even though we are not describing the true nature of the world. The situation we are in is rather like trying to navigate around a dark, unfamiliar room: we may never know *what* it is that we are running up against (as the light may never be turned on), but we can devise theories to help keep us from walking into things.

4.3 Weaknesses of This Position

However, there are doubts about whether or not this position can adequately serve as an improvement upon realist explanations of the success of science. There are, as I see it, two main classes of difficulty in proposing this explanation as an improvement upon realism: (1) whether this position truly amounts to something unique, above and beyond more familiar realisms, and (2) whether this position can actually explain, in a satisfactory way, what it claims to explain. I will evaluate each of these difficulties in turn.

4.3.1 Is This Just Realism?

Earlier, I stated how the conceptual idealist calls for both the nature of externality and the nature of conceptualization to be operative in explaining the success of science, and it is interesting to note that the realist, in a sense, agrees with this statement. The difference between the positions lies in what, exactly, is involved in 'the nature of conceptualization.' The realist holds that conceptualization is necessary for scientific success--more specifically, that we conceptualize the world *as it is*--while I argued that the way in which we conceptualize the world is such that it *transforms* the world into something substantially different. I agree that to know the world as it truly is, is possible, but that most of our concepts used to describe the world have an element of mind-referentiality that makes them unsuitable for this task.

But can this element of mind-referentialty be 'factored out'? Consider an analogy: viewing the world through rose-colored glasses will necessarily change the nature of the perception from the 'real' nature of the world to a rose-colored one. If,

though, we were able to discover truths about the glasses, truths about the way they alter the paths of light-waves, then the change effected by the glasses could be factored out in trying to determine what the world viewed through them is really like. We could come to realize that, though the sky *looks* pink, our glasses shift the wavelengths of light in certain ways, and that *that* shade of pink that we see in the sky really correpsonds to blue in externality.

The analogous question is, then, can the extent to which the mind-referentiality of our conceptual scheme affects what we perceive be understood, so that we might have an accurate picture of the world combined with an accurate picture of how we transform it? And there does not seem to be any overriding reason why this should not be possible; indeed, it is what I myself have done on several occassions throughout this paper in attempting to elaborate on the difference between the world we know and the world itself. After all, concepts are a mental creation through-and-through, so they should not prove to be inscrutable upon mental examination.

The effect this has on the issue of realism is this: if such a separation between the world and our transformation thereof can be understood so that, when we perceive X, we can know what the 'true' nature of X is and how we have transformed this nature into the X that we perceive, then the realist can take the transformational component as a given, and focus on the features of the real nature of X that are of interest. The realist assertion that "a successful theory about X approximates the real nature of X" can be understood as claiming that, minus the (now well-understood) component of conceptualization, what remains of our

theorizing about X corresponds to the real nature of X, and, furthermore, this is what we are interested in when exploring why this theory about X is successful. In my arguments, I have attempted to show why the transformational component of conceptualization is substantive enough to warrant inclusion in an explanation of scientific success; but explanations, for pragmatic reasons, must always focus on some factors at the exclusion of others, and the particularities of a discussion about what makes science successful may be such that only the real properties of the world, and how they relate to the science in question, are of interest.

4.3.2 Is the Success of Science Explained?

The other potential shortcoming of this position is whether it truly gives an adequate explanation of why scientific success is to be expected. Despite the discussions in the last chapter, it may still be held that the conceptual idealist explanation lacks the intuitive connection between theory and success given by both the realist and the constructivist. Indeed, it seems that in attempting to draw on insights from both of these positions, the clarity and force they possessed have given way to vagueness and indeterminacy. It is not really clear, for instance, what, specifically, we can know about the external world, and how it relates to success. I suggested mathematical structure as a possible candidate for such knowledge, but even mathematics may have ineliminable elements of mind-involvement. The only interpretation we seem to be able to attach to a mathematical equation is one involving particular things; this may well mean that mathematics, as we know it, is conceptually mind-involving.

There may still be a more 'primitive' mathematical structure to reality, a structure that is free from mind-involvement: for instance, a structure wherein statements like 'stuff and stuff is more stuff' are true. But this would require even more organizing work being done on the part of conceptual schemes, and it is unclear how they function on this account already. It has been maintained that our conceptualization transforms externality into the rich world we inhabit, but *how* this happens is far from clear. Could we conceptualize it in any way we choose--that is, what role does the external world play in constraining theory choice? Does the adopting of a certain purpose (e.g., prediction and control) for a conceptual scheme commit one to a certain kind of scheme? When does a manner of conceptualization *change* what we perceive, and when can we 'merely entertain' a certain conceptualization? These are all important, salient questions that need to be answered for this explanation to truly be complete, and, unfortunately, I have not been able to answer them here.

4.4 Concluding Remarks

In this thesis, I started with a seemingly simple question: why does science work so well? As this question lends itself to various formulations with various responses, I followed Boyd's lead in narrowing the focus to our largely theory-dependent scientific methodologies. Why should such theory-laden methodologies be so reliable in navigating us through the world? The asking of such a question provides powerful motivation for realism; as I showed in my recasting of Boyd's arguments, there seems to be no scientifically plausible answer to this question

other than scientific realism: the view that the properties, entities, and processes attributed to the world by our scientific theories are actually part of the world itself (with, of course, suitable qualifications of 'approximation' concerning the nature of these properties, entities, and processes). If a Boyd-style scientific realism is true, then the success of these methodologies is no longer surprising, but rather to be expected.

However, such a straightforward answer will not suffice. As my exploration of Nicholas Rescher's *Conceptual Idealism* in Chapter 2 showed, the nature of the concepts we happen to use in theorizing about the world, concepts such as 'particularity,' 'space,' 'time,' 'lawfulness,' and 'empirical properties,' make an ineliminable reference to the output of mental functionings--not merely in the fact *that* they are used in theorizing (which, it is trivial to say, obviously requires the workings of minds), but in their very *meanings*. What it means to be, say, a 'particular thing with certain empirical properties' is such that one could not even make sense of this phrase without referring, however tacitly, to purely mentalistic objects

This is unfortunate for the scientific realist, since the realist holds that the properties we use to describe the world are, approximately, properties of the world-in-itself. However, assuming that the world-in-itself is not endowed with mental functionings and that properties which make reference to mental functionings cannot consistently be said to hold of something (which lacks mental functionings) in itself, the properties which science compels us to attribute to the external world cannot truly reside there. Lacking recourse to realism, can the success of science still

be explained? As I attempted to show in Chapter 3, it can.

The general strategy for explaining the success of science involved recognition of both the (passive) role of the external world and the (active) role of mental functioning. With both of these factors in mind, the success of our theory-laden scientific methodologies can be explained in anti-realist terms: anti-realist because, despite Boyd's claim to the contrary, instrumental success is plausibly explained without granting the ability of science, as it currently exists, to reliably generate knowledge about the nature of the external world. This strategy for explaining the success of science is much like John Worrall's structural realism. However, as Stathis Psillos points out, Worrall's position, with its structure / nature dichotomy, is unable to explain predictive success at all, because mathematical structure alone is insufficient to generate predictions. This is where the conceptual idealist position shows its plausibility as a way of explaining the instrumental success of science, as the gap between highly abstract (including, but, unlike Worrall, not limited to, mathematical) knowledge of the external world and the full-scale predictions we make based on that knowledge is filled by the active, transforming role of minds and their conceptual schemes.

Sadly, this view of science ultimately falls short of providing an adequate explanation of why science is successful. Until it can be explained *what* features of reality go into making a theory succesful, *what* it is about conceptual schemes that allows them to transform what we perceive, and *how* it is that this happens, the story of how science came to be such a reliable method of generating predictions will necessarily be filled with holes in key places. However, this is not to say that this

project was completely unsuccessful, though it admittedly falls short of the standard it set for itself. Hopefully, I have at least made a case for the need to include more than just the relation between the real features of the world and the features of a theory in explaining why science is successful. Though the role of conceptual schemes is still not fully clear, it seems that an adequate account of scientific success must involve an exploration of this role. And though, in the end, I may not have replaced the realist explanation with a superior one, I have at least attempted to show why the realist account stands in need of greater elaboration and why the role of conceptual schemes is not the passive one implied by realism, but an active one whose nature must be better understood in attempting to understand science.

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