Erkenntnis (2005) 63:167–204
DOI 10.1007/s10670-005-3225-8

TIMOTHY D. LYONS

TOWARD A PURELY AXIOLOGICAL SCIENTIFIC REALISM

ABSTRACT. The axiological tenet of scientific realism, "science seeks true theories," is generally taken to rest on a corollary epistemological tenet, "we can justifiably believe that our successful theories achieve (or approximate) that aim." While important debates have centered on, and have led to the refinement of, the epistemological tenet, the axiological tenet has suffered from neglect. I offer what I consider to be needed refinements to the axiological postulate. After showing an intimate relation between the refined postulate and ten theoretical desiderata, I argue that the axiological postulate does not depend on its epistemological counterpart; epistemic humility can accompany us in the quest for truth. Upon contrasting my axiological postulate against the two dominant non-realist alternatives and the standard realist postulate, I contend that its explanatory and justificatory virtues render it, among the axiologies considered, the richest account of the scientific enterprise.

1. INTRODUCTION

Contemporary scientific realism consists of two primary tenets, one axiological and the other epistemological. According to the axiological tenet, science seeks true theories and is justified in doing so. According to the epistemological tenet, we can justifiably believe that our successful scientific theories have achieved (or approximated) this goal. While these tenets constitute the focus of the contemporary debate on scientific realism, that debate takes place upon a set of foundational theses. One of these, we can call the *rationality/progressiveness thesis*: in general, theory change in science has been rational and progressive. Another is the *progress determination thesis*: progress in science is determined by the extent to which its primary aim is achieved (or the degree to which that aim is approximated). Such foundational theses tend to be accepted by the most prominent opponents of realism, for instance, Bas van Fraassen and Larry Laudan.[1] And generally, both advocates and critics of scientific realism take the axiological and epistemological tenets to be inextricably connected: they see the justification of axiological scientific realism as contingent on the justification of epistemic scientific realism. In fact, so long as science is taken to be successful, *epistemic* *scientific realism* is entailed by the conjunction of *the rationality/progressiveness thesis the progress determination thesis*, and *axiological scientific realism*. Taking the tenets of realism to be related in this way has naturally led those who reject the epistemological tenet to reject the axiological tenet as well. Upon denying our ability to justifiably believe we have achieved the truth, non-realists deny the rationality of endeavoring to achieve it. Accepting nonetheless *the rationality/progressiveness thesis* and *the progress determination thesis*, they deny that truth is the aim of science.

Larry Laudan is explicitly critical of axiological realism. His chapter in *Science and Values* on the historical argument against realism is titled, "Reticulational Critique of Realist Axiology and Methodology." There he presents a list of past successful theories that cannot be approximately true. (For a detailed extension and defense of this argument, as well as my account of how it should be understood, see my (2002).) He takes his historical points to refute axiological realism no less than epistemic realism. Laudan places "the goal of building up a body of true theories" (1984, p. 53) in the class of "epistemically utopian" goals. Upon doing so, he infers that the pursuit of truth is an irrational enterprise. Maintaining that science is a rational enterprise, he denies, altogether, that science pursues the truth.

Bas van Fraassen also emphatically rejects axiological realism. While he has lately sought to distance himself from the epistemic arena of the realism debate, it is there that the bulk of that debate has transpired. And I think most realists have taken his rejection of axiological realism to stem from epistemic concerns. In any case, as van Fraassen construes both realism and his own position, belief plays a central role in the acceptance of a theory. And, he writes, "If you accept a theory, you must at least be saying that it reaches its aim, i.e. meets the criterion of success in science (whatever that is)" (1983, p. 327). Van Fraassen posits an aim for science that is epistemically less demanding than truth, empirical adequacy, which does not require truth about the unobservable. And "the acceptance of a theory involves as belief only that it is empirically adequate" (1980, p. 12), i.e., only that the theory's claims about what is observable are true.

I've argued elsewhere that epistemic realism has yet to make its case (2001, 2002, 2003). In fact, I'd suggest that we have good reasons to at least *treat* theoretical truth as epistemically utopian. I do not however join the non-realist in discarding axiological realism. Rather, following a suggestion made by Nicholas Rescher, I seek to defend the claim that science pursues the truth and is rational in doing so, irrespective of whether we are justified in believing that it has been achieved. Rescher, like Laudan, takes truth to be a transcendent ideal. He writes, "we clearly have no prospect of substantiating the claim that our scientific claims are drawing nearer to the 'real truth'" (1984, p. 63). He insists however that the utopian, "pie in the sky" (1992, p. 215), status of truth does not preclude it from constituting the paramount aim of science. Truth for Rescher "represents the ultimate *telos* of inquiry" (1984, p. 152).[2] While Rescher denies a *realism of achievement*, he advocates a *realism of intent*, i.e., axiological realism.[3]

In the same spirit, I will attempt to develop and defend an axiological realism that is independent of epistemic realism. Central to my approach will be a refinement of the realist's naive axiological postulate, "science aims at truth". Such a refinement, I'd suggest, is encouraged, if not required, by a number of additional concerns. For instance, we appear to be without grounds to say that the realist's naive postulate is relevant to, or even compatible with, the pursuit of specific theoretical desiderata, whose pursuit in science is non-contentiously accepted in the debate. (This is so even if one evades the need, or is successful in the attempt, to liberate axiological realism from the burden of the epistemological tenet.) A common example of such a desideratum is the preference in theory choice for simple theories. Worry about the relation between simplicity and truth often arises in debates about epistemic realism, most notably those pertaining to the underdetermination of theories by data. Van Fraassen contends that it is only by invoking metaphysical or theological assumptions that we can assert that the world, thus the set of true claims describing it, is simple (1980).[4] This threatens the claim that we are justified in believing that a simpler theory is more likely to be true than its equally successful rivals. Due to the epistemological focus of the realism debate, a similar and no less pressing threat to axiological realism tends to be neglected: positing truth as science's primary aim in theory choice, without grounds to claim a relation between simplicity and truth, the axiological realist appears entirely unable to account for the common scientific practice of choosing the simpler theory. The antirealist, by contrast, touts her ability to account for that practice on pragmatic grounds. While I will identify further problems with standard axiological realism, I mention the problem of simplicity here to indicate the need for a careful look at axiological realism.

Since axiological realism is (like its epistemological counterpart) a theory about science, it is useful to take stock of some of the basic data to be accounted for by such a theory. I anticipate that most philosophers of science would agree that any theory of science must allow for the following key insights of last century's philosophy of science.

Insight 1, pertaining to theory attainment: There is not, nor need there be, a rigorous algorithm by way of which theories must be formulated/attained in science. While some theories may come about by narrow inductive arguments from specific facts, others may come about by broad overarching conjectures (be they informed or uniformed), by dreams, etc. What is crucial is what we can do with, and can get out of, the theory. (Popper 1959; Hempel 1966.)[5]

Insight 2, pertaining to theoretical holism (with perhaps some limits): (a) Large scale scientific theories make predictions about the world only when connected to a significant quantity of auxiliary hypotheses; a full set of statements is involved in the derivation and testing of specific empirical predictions. And (b) when a prediction derived from the system conflicts with empirical tests/observations, one cannot automatically attribute the falsity to the central theory; the auxiliaries to which it is connected may well be at fault. (Duhem 1906; Quine 1975.)

Insight 3, pertaining to the preservation of large scale theories despite initial "falsifications": (a) Many (if not all) scientific theories are "falsified" in their original formulations and in the context of the systems that initially surround them; but they are and can legitimately be reconciled with the data by the modification of auxiliary hypotheses. (Kuhn 1970; Lakatos 1970; Feyerabend 1993). And (b) modifications that do not bring about immediately testable empirical claims are not altogether prohibited.[6] However, *ceteris paribus*, preference is given to those auxiliary modifications which, though non-isolable, contribute to the empirical consequences of the system.

Insight 4, pertaining to the comparative nature of theory acceptance and a preference for the better explanation: (a) The assessment of a scientific theory is not a straightforward comparison of the theory against the empirical data, but a comparison of the theory against alternatives, and their respective relations to empirical data; And (b) the set of criteria by which theories are compared goes beyond mere empirical considerations; it includes explanatory criteria as well: the theory that stands as the better explanation of phenomena is preferred.[7]

Insight 5, pertaining to the possibility of a demarcation criterion: We find ourselves without any obvious and straightforward criterion that clearly distinguishes science from non-science. The difference between forms of intellectual inquiry may often be a matter of degree rather than kind.[8]

Given this last insight, the disciplines we have called, and continue to call, "philosophy" and "science" are not, and need not be so, distinct. Accepting this (and the rationality/progressiveness thesis above) many philosophers have embraced a naturalistic approach to theorizing about science; essentially, for them, a philosophical theory about science that is empirically testable, and which succeeds in its empirical tests, is preferable to one that cannot be empirically tested or fails empirically.[9] It is argued thus that theories about science should ultimately be treated as part of science and tested empirically against the data to which they pertain, e.g., the history of science. And we should prefer the theories about science that best meet those criteria applied in the comparison of scientific theories.[10]

Many realists embrace this naturalistic approach. Hence, epistemic realism becomes the following thesis: we can justifiably believe the hypothesis that successful theories are (approximately) true. While I have argued that epistemic realism does not fare well empirically,[11] I consider naturalism to be conducive to and supportive of axiological realism, i.e., the hypothesis that science seeks true theories. In fact, invoking naturalism, insights in philosophy of science such as those listed above can be treated as more than mere data *to be accounted for* by our theories of science. We can look to these insights for *guidance* and *license* in our theorizing about science; they can be turned into principles by which we direct our own attempts to justify and even formulate our philosophical theories.[12] For instance, embracing Insight 1, we are liberated from the demand to build our theories upon premises from which they "follow". Rather our theories can be postulates that attain justification by being shown to meet certain criteria – e.g., entailing certain data, accounting for data not accounted for by competing theories, etc. – irrespective of the foundations on which we might *build* them.[13] Treating the realist's axiological postulate as an extension of scientific theory, I embrace and take license from Insight I here. I do think it is helpful to specify, in the next section, a few of the concerns that motivate my postulate, and I do see my task as simply explicating a set of intuitions – intuitions that drive scientific activity and are implicit in our understanding of that activity. Nonetheless, those motivations for my postulate and any intuitiveness my postulate may posses are not meant to suffice as justification for that postulate. The task of justification will be postponed to Sections 6–9, after I have fully formulated the postulate and its consequences.

More specifically, I will begin with the naive hypothesis of axiological realism and, taking some license from Insights 2 and 3, will refine that hypothesis in light of a number of concerns. I will then articulate the key concept involved in my refinement and discern the consequences of employing that concept in the postulate. Subsequently I will contend that my axiological realism is not threatened by arguments put forward against the pursuit of transcendentally ideal goals, and I will identify a possible justification for the endeavor to achieve such a goal. I will argue further that the pursuit of truth *explains* and, crucially, that it *justifies* the pursuit of certain virtues appealed to in theory change. Drawing on Insight 4, I will contend that my axiological realism provides a richer, more coherent account of the scientific enterprise than the available realist and non-realist accounts. Science, I propose, aims at the truth and is justified in doing so, irrespective of whether we can justifiably believe we've achieved it.

2. REFINING COMMON AXIOLOGICAL REALISM

Consider the axiological postulate so commonly and casually put forward by realists.

Postulate 1: *Science seeks true theories*. As a springboard, allow me to note a seemingly trivial point against this postulate, one borrowed from Karl Popper (1959) and redirected. The claim "all planets move in ellipses or don't move in ellipses" must be true; it possesses an intrinsic probability of 1. Popper rather famously notes that, if science sought theories with high probability, science would prefer such tautologies to theories possessing the inverse property, high content. Yet this basic point also encourages clarification of the naive realist's postulate. For, if Postulate 1 is correct, why ought science not take what it seeks where it can best be delivered, in tautological theories? Trivial though the question may seem, it indicates that the naive axiological postulate is an inadequate account of empirical science's aims. Focus must be directed toward a particular type of truth. Specifically (drawing on Insights 2 and 3 for guidance in our theorizing), we are prompted to conjoin auxiliary qualifications to Postulate 1 that isolate, as the aim of science, a subset of the set of true claims. As a preliminary attempt, consider,

Postulate 2: *Science seeks theories that are not only true but have bearing on, or make a difference to, the world of experience*. While this step is progressive, the postulate pertains only to scientific *theories*. However, wanting to account for Insight 2a as data, we recognize that a full theory complex is generally required for a scientific theory to bear on the world of experience. Here the term "theory complex" is meant to denote a theoretical system consisting of the following:

- A set of theories
- All auxiliary hypotheses required to generate predictions from that set
- All auxiliary hypotheses required to test predictions
- All auxiliary hypotheses going into the reports of tests

Including a complex in our postulate brings us to

Postulate 3: *Science seeks a full complex that is true and has bearing on the world of experience*. While this introduces an important clarification, when positing a goal, we are wise to specify the situation under which, and the time period during which, we desire to achieve it. Postulate 3 is sufficiently vague to be taken as positing little more than a final goal for science, perhaps centuries (if not millennia) from any state in which science has yet found itself. To have any significance to the actual decisions of past and present science, the realist should not posit in her hypothesis a goal so potentially distant. Granted, the posit should be about what science endeavors to achieve *over an extended period of time*. And, granted, one would want to acknowledge that *an eye to the potential* of a theory complex will bear on replacement as it occurs over time. However, the focus of the posit, the goal specified, should have bearing on, thus at least pertain to, the decisions involved in theory change (which we can take to include the replacement of auxiliaries, etc.), rather than some idealized end-state of the scientific enterprise.

Postulate 4: *Science seeks, in theory change, full complexes that are true and have bearing on the world of experience*. Though offering hope for the problem just noted, Postulate 4 invites its own problem. Even taking theory change to be extended over a period of time, I'd suggest that it is unlikely, and recognized to be unlikely, that *each* of the many statements involved in any past selected, or even soon to be selected, complex is exactly true.[14] Though I do seek to justify the pursuit of epistemically utopian goals, the concern here is not merely epistemic. It pertains to attainability. And I nonetheless consider a potentially *attainable* goal preferable to one that may well be entirely unattainable.

Postulate 5: *In theory change, science seeks true theories that are contained in complexes that have bearing on the world of experience.* Since it is quite possible that particular isolable theories we may come to possess are true, the threat of unattainability is remedied. However, this postulate altogether neglects any reference to the desired status of the many auxiliaries conjoined to the theory.

Postulate 6: *In theory change, science seeks to increase the truths that are contained in complexes that have bearing on the world of experience.*

At this point, I think it should be clear that Postulate 1, the naive axiological postulate so commonly and casually touted by realists, is at least incomplete. Failing to articulate these largely non-contentious, if not obvious refinements, realists have failed to deal with the no less obvious concerns that prompt them. And I suggest that the above sophistications constitute positive steps. However, in my view, we have yet to capture what science is after. The task remains to identify the specific subset of true claims that science seeks to increase in complex modification, that subset being a key ingredient in an adequate axiological postulate. While I am not convinced that the postulate I will offer below cannot be refined further or better stated, I expect that it will mark an advance over even the sophistications thus far considered.

3. A NEW AXIOLOGICAL REALISM: "SCIENCE ENDEAVORS TO INCREASE THE MANIFEST TRUTH OF ITS COMPLEXES"

I suggest that science is interested, not in true statements, *per se*, but a certain type of true statements, those that are *manifested* as true. I will call such statements *MT statements*.[15] The term, "MT statement" does *not* designate a statement whose truth is manifested *to us*. Its truth is not epistemically manifested. Rather an MT statement is one in a theory complex whose truth is manifested in the consequences of the complex. More specifically, its truth has bearing on, makes a difference to, the world of experience and is transmitted to tested predictions derived from the complex.

To clarify the notion of MT statements, consider those statements that cannot be manifested as true, non-MT statements. Of course, one class of such statements is the class of all *false statements*. Another set of non-MT statements consists of those that have no connection to our theory complexes, so their truth-value bears neither on derivable predictions nor the world of experience. We can call these *detached statements*. Their non-MT status is contingent on the nature of, and their relation to, other statements in our complexes at a given time. Given our present day theory complexes, "The Absolute has angst," would, even if true, make no difference to the world of experience. This statement would, as Popper might put it (1959), prohibit nothing. However, were we to conjoin this statement to auxiliaries that delineate any number of testable and true implications of The Absolute's angst, the truth of the statement could be made manifest.

A third set of non-MT statements is the set of those we might call *vacuous statements*. These are statements that are intrinsically such that they preclude nothing in the world of experience. Irrespective of whether and how they are conjoined to a complex, their truth can make no difference to, cannot be made manifest in, the world of experience. Tautologies, such as that noted earlier would fall into this class. So would statements that intrinsically preclude any means for testing their truth-value, e.g., "there exists an undetectable unicorn." Even if such claims were true, because they can make no difference to the world of experience, their truth could not be made manifest.

Now the most important set of non-MT statements are true statements that fail to render true predictions because other statements in the complex render those predictions false. I will call these *obstructed statements*. (It is the possibility of such statements that is recognized in Insight 2b above.) A common example: say our complex contains a true theory about the motion of heavenly bodies, and we have predicted a certain planet's behavior. Say further, our complex contains an auxiliary hypothesis that falsely asserts the number of bodies bearing significant gravitational effects on the planet. Our theory's truth is obstructed by a false auxiliary; its truth is not made manifest in (is not transmitted to) the prediction. Despite its truth, the theory fails, in this case, to bear truthfully on the world of experience.

Having identified those statements that are not manifested as true, we can now clarify four conditions that must be met for a statement, S, to qualify as an MT statement.

- S must be true. (S cannot be false.)
- S must be connected to a theory complex. (S cannot be detached.)
- S must be conjoined to a complex in such a way that it makes a difference to the world of experience. (S cannot be vacuous.)

- S must be such that its truth is made manifest in tested statements about the world of experience. (S cannot be obstructed.)

We recognize further that MT statements are such that their truth can be manifested *to a greater or lesser degree*–depending on, for instance, their breadth and the range of available data statements. The greater the range of phenomena in which the statement's truth is made manifest, the greater is its degree of manifest truth.

Even if we cannot identify which statements are MT statements, we can sometimes discern when and roughly where we have a deficiency of MT statements: we can identify a lack of desired information or a lack of true information in a theory complex. Thus, what we can deem *an evident MT-deficiency*, can take two forms:

Type (a) evident MT-deficiency: It is evident that non-MT statements are present in the complex.
Type (b) evident MT-deficiency: It is evident that we possess data statements that have no matching prediction statements.

The core claim of our postulate is that science seeks to increase the manifest truth of its theory complexes. A few further qualifications to our postulate are needed. First, because, according to our postulate, it is specifically manifest truth – not simply truth *per se* – that science endeavors to achieve, we must add the clause that science endeavors to avoid an increase in non-MT statements.[16] The second qualification is that, without replacements in hand, non-vacuous and non-detached statements are not discarded in complex modifications. The third qualification is that complex modifications, or specifically, attempts at increasing the manifest truth in our theory complex, occur only when an evident MT-deficiency is present.[17] Given these three further qualifications, let our postulate now read as follows: Science endeavors to

(1) remedy evident MT-deficiencies by increasing the number, breadth, and/or precision of MT statements in its theory complexes;
(2) retain or increase the degree to which the truth of each MT statement in the complex is manifested;
(3) retain non-vacuous and non-detached statements that are not replaced in its complexes;
(4) and avoid increasing the non-MT statements in its complexes.

Spelling out the details more explicitly, our postulate is finally as follows: Science endeavors to

(1) remedy evident MT-deficiencies by increasing the number, breadth, and/or precision of MT statements in its theory complexes;

– for type (a) evident MT-deficiencies this means

(a(i)) add MT statements that turn obstructed statements into MT statements and/or
(a(ii)) replace false statements with MT statements that make predictions about at least as many known phenomena[18] as did the replaced statements;

– for type (b) evident MT-deficiencies this means

(b(i)) add MT statements to our complexes that are manifested as true in those data statements and/or
(b(ii)) replace MT statements with other (more universal or more precise) MT statements that are manifested as true in those data statements;

(2) retain or increase the degree to which the truth of each individual MT statement in a complex is manifested;
(3) retain non-vacuous and non-detached statements that are not replaced in its complexes;
(4) and avoid increasing the non-MT statements in its complexes.

An occasion of theory change that exactly meets the conditions 1–4 I will call an *increase in manifest truth* or an *IncMT*. My postulate is that complex modifications in science constitute the endeavor to achieve such a state. For the moment, I offer it only as a postulate in order to make clear its implications. Again, MT statements are not manifested as true *to us* but in the consequences derived from the theory and in the world of our experience.

I will now relate an IncMT to a number of theoretical virtues. We can understand the degree of a theory complex's *empirical accuracy* (E) to be a reference to the number, breadth, and precision of prediction statements that match data statements in the complex.[19] (This is distinct from van Fraassen' s empirical adequacy: it denotes a syntactic rather than semantic relationship; it does not pertain to all observables, etc.) At the outset, we can recognize that, at least roughly, where predictions and data statements do not match, a true statement leading to those predictions cannot be manifested as true. But I will demonstrate more thoroughly that an IncMT will require an increase in E.

Conditions (3) and (4) capture what is unchanged in the achievement of an IncMT, and (2) denotes a state that may, but need not, be changed as a result of that achievement. An IncMT does require a change of state from among the possibilities captured in (1). Consider then each of the sub-states of (1) to determine their effect on a complex's degree of E, taking first the shifts brought about by a *type (a) evident MT-deficiency* (i.e., the noted presence of non-MT statements in a complex). In condition (a(i)) the addition of MT statements renders manifest the truth of formerly obstructed non-MT statements. Upon the achievement of that condition an increase in E will result: if the predictions and data statements do not match where the true statement had been recognized to be blocked, the true statement remains obstructed and cannot be manifested as true. The achievement of (a(ii)) constitutes the replacement of false statements with MT statements that cover at least as much known phenomena as the replaced statements. When this condition obtains, predictions derived from the new statement will have to match some of those data statements with which the replaced false statement conflicted (in which the MT-deficiency was evident before the modification); for the new statement cannot be manifested as true if its predictions are blocked by those data statements. So an increase in E will result when condition (a(ii)) is achieved.

Consider an IncMT in response to a *type (b) evident MT-deficiency* (i.e., the recognized presence of data statements that have no corollary prediction statements). An IncMT can obtain by (b(i)), that is, by adding MT statements to the complex that are manifested as true in those data statements. Or it could occur by (b(ii)), that is, by replacing MT statements with more universal or precise MT statements whose truth is manifested in those data statements. In either change of state, an increase in E will result; for, again, to be manifested as true the prediction and data statements must match, and those data statements that the new predictions from the MT statement have come to match had matched none of the pre-modified complex's predictions. It is clear then that each of the specific changes by which an IncMT can occur will necessitate an increase in empirical accuracy.

Consider now consistency. We can understand an *increase in consistency* as denoting an increase in a theory complex of the number and/or breadth of possible statement-pairings where no contradiction results.[20] Since MT statements cannot be contradicted on their way to the predictions, the new statement must be able to be compatibly conjoined to other statements in the complex. When (a(i)) obtains, the unobstructed statement will be compatible with, not only the added MT statement that ensures that its truth is manifest, but also statements (e.g., data statements) that contradicted it when obstructed. Upon the achievement of (a(ii)), the replacement MT statement will be compatible with those statements that had contradicted the false statement it replaced. In a (b(i)) IncMT, the new statement will be compatible with the set of previously unaccounted for data statements and numerous others, as no statement can contradict it on its way to the conclusion. In (b(ii)) modifications, the replacement statement will likewise be compatible with the previously unaccounted for data statements. In the securing of each of these IncMT states, an increase in consistency will be a consequence.

There are agreed to be additional theoretical or explanatory virtues sought in theory complexes. Let us consider testability, breadth of scope, and simplicity. While *increases* in these additional desiderata will not be ensured by an IncMT, their *retention*, at least, will be. We can understand the degree of testability (TST) to be a reference to the number, breadth, and precision of (relevant) *prediction types* that can be derived from the complex.[21] Just as the achievement of (b(i)) and (b(ii)) will increase E and C, they will increase TST; for in both states, the prediction types derivable from the theory will be increased. However, an IncMT via (a(i)) and/or (a(ii)) need not increase the prediction types. Given the *type (a) evident MT-deficiency* of the pre-modified complex, some predictions derived will have failed to match data statements, but that *match* is irrelevant to TST. A set of prediction types would still have been derivable from the obstructed or false statements, and this set could have been of equal measure to the new complex's set of prediction types. Nonetheless, since an IncMT requires conditions (3) and (4), and since the new or replacement MT statements must bear on the world of experience, the IncMT will not reduce those prediction types. The testability of a complex, then, must at least be retained, if not increased, when an IncMT obtains.

A fourth theoretical virtue related to an IncMT is breadth of scope (BOS). We can understand (BOS) to be a reference to the *range of phenomena* to which available prediction and data statements pertain. The complex contains information (makes predictions) about objects and events in the world of experience (which are referred to in data statements) – though that information need not be correct (e.g., contradictions can obtain between the prediction and data statements). The achievement of an IncMT via (b(i)) and/or (b(ii)) will increase BOS: the range of phenomena in regard to which the prediction and data statements pertain will broaden. However, as with testability, an increase in BOS need not come about upon the achievement of (a(i)) and/or (a(ii)). Remedying a *type (a) evident MT-deficiency* with MT statements need not extend the prediction and data statements over a greater range. Nor however will it reduce them to a narrower range. So the BOS will be at least retained, if not increased, when an IncMT is secured.

Turning to another set of theoretical desiderata, consider three forms of simplicity. The first is similar to that mandated by Ockham's razor: *do not posit anything beyond necessity*. To achieve an IncMT, the conditions in (4) must be achieved: non-MT statements, including vacuous and detached statements, cannot have been added. Vacuous statements, no matter the circumstances, can put no restrictions on the world of experience. Detached statements, floating unconnected to any complex, have bearing neither on the conclusions drawn from the complex nor on the world of experience. Neither can, as it stands, be manifested as true. Both of these subclasses of non-MT statements will be precluded from an IncMT. Thus, an IncMT will retain a form of simplicity captured in Ockham's Razor, which we can denote as *simplicity-1*.

Unfolding a second form of simplicity, note that we can concoct indefinitely many empirically distinct theories as rivals to any theory we might prefer. Nicholas Maxwell provides examples of such "aberrant" competitors: each rival to Newton's theory, N, states that "everything occurs as [N] asserts" but then goes on to say something in respect to which we have neither corresponding nor conflicting data statements: e.g., "except for the case of any two solid gold spheres, each having a mass of 1000 tons, moving in otherwise empty space up to a mile apart, in which case the spheres attract each other by means of an inverse cube law of gravitation" (1998, p. 140). As Maxwell points out, while individual aberrant theories could, in principle, be eliminated by empirical tests, since indefinitely many such theories can be generated, we could never eliminate all or even most of them empirically. Instead, that property appealed to in the rejection of such "aberrant" competitors appears to be another form of simplicity, which we can call *simplicity-2*. As with simplicity-1, the retention of this form will obtain when an IncMT is achieved. Assume our complex contains N and we are comparing it to many other complexes, each of which contains an aberrant variation of the sort above. While those competing complexes will enjoy the same empirical success as our own, the switch from our own to any of the others could not, given the added clauses, constitute an IncMT. Even if one of those aberrant clauses among the set of complexes is true, it is not manifested as such. By contrast, if the universal claims in our N complex are both true and unobstructed, they are manifested as true. The portion of N that goes beyond what has been made manifest at a given time is still part of a statement whose truth has been made manifest in many instances; no part of any of the aberrant clauses has such instances. Thus the retention of a second form of simplicity (*simplicity-2*) is required of an IncMT as well.

Yet a third form of simplicity, *simplicity-3*, will come about upon the achievement of an IncMT. Say N is true and manifested as such. Say also that the specific part of N that is manifested as true could be expressed in a number of less encompassing, non-unified statements, n1, n2, n3, ... , which do not exhaust but are entailed in N. These would also be true and manifested as such. However, attaining an IncMT requires that condition (2) obtains, which requires the retention or increase of the degree to which the truth of each individual MT statement in the complex is made manifest. Though all the statements at hand are true, the truth of N in its complex would be manifested as true to a greater degree than the narrower claims, n1, n2, or n3. To achieve (2), the non-simple complex cannot be chosen over the simpler complex containing N. Such a choice could not constitute an IncMT.[22] An IncMT will bring about an increase in, or at least the retention, of this third form of simplicity. (This form of simplicity is limited to situations in which the MT statements in one complex entail less encompassing MT statements in another. I will introduce a related, fourth form of simplicity in the next section, which comes into play in cases of non-entailment between statements in the respective complexes.)

Philosophers on both sides of the scientific realism debate tend to agree that the realist's appeal to simplicity requires the assumption that the world, thus the truth about the world, is simple. The axiological realism here proposed makes no such assumption. Given our postulate that science seeks an increase in *manifest* truth, whether the truth "as a whole" is simple or not, that subset of truth sought in science is simple, in at least the three senses outlined here. (Similarly, the truth itself need not be testable. However, that subset of truth sought in science, which I have called manifest truth, is testable.) Notably, this relationship between simplicity and truth is discerned, not empirically, but by working through the consequences of achieving an IncMT. For this reason, we are not faced with the problems that come from an empirical attempt to establish such a relationship (for these problems, see my (2001, Chapter 3)).

We have now identified a set of seven desiderata without which an increase in manifest truth cannot obtain. Obtaining the full set is a consequence of achieving an IncMT. I will refer to the whole as the "consequent-set" (CS)

(CS) the achievement of an increase in empirical accuracy and consistency, and the retention of, or an increase in, testability, breadth of scope, simplicity-1, simplicity-2, and simplicity-3.

One might suspect there to be a circularity here, that an increase in manifest truth ultimately reduces to being no more than CS. This is not the case, however: in addition to the seven virtues specified in CS, an IncMT must entail supraempirical or ontological accuracy. Whether an MT statement pertains to the observable, the unobservable, or both, it must be true. The attainment of CS does not imply the attainment of an IncMT. For the modification that brings about CS may very well not be ontologically accurate. Since CS is a necessary but not sufficient condition for an IncMT, the latter cannot be reduced to the former, and no charge of circularity can be directed here. Moreover, let it be emphasized, I will not be resting my argument for axiological realism on any claim that we can discern when increases in the manifest truth of our theory complexes have occurred. As mentioned above, such increases are *not* being said to be *epistemically* manifest. The notion of attaining an IncMT is being put forward as an ideal, in the sense that it cannot necessarily be recognized as being attained, and is being treated as nothing more (and nothing less) than an ideal.

Are the elements of CS such that we can justifiably believe that we've achieved them? Note first that CS does not require the attainment of the elements in their perfected states – perfect empirical accuracy or consistency, etc. It requires only their increase and/or retention. Further, note that each of the elements of the consequent-set signifies a syntactic relationship. Even with empirical accuracy, we are only considering whether prediction statements match data statements. In discerning whether CS has been achieved, we need at most compare the relations between statements in one complex to the relations between statements in another; we need not justifiably believe what our complexes tell us about the world. Because such comparison between complexes can be performed by analysis, it is possible to justifiably believe that we've achieved the elements of CS, irrespective of whether we can justifiably believe that we've achieved an IncMT.

4. THE ENDEAVOR TO ACHIEVE AN INCREASE IN MANIFEST TRUTH

The present claim: the actual achievement of an IncMT entails the achievement of CS. Directing this toward an axiological realism, I seek to take this further: *endeavoring* to achieve an IncMT entails *endeavoring* to achieve CS. One might object that this modification brings the entailment into question. If, in fact, CS is a necessary condition for an IncMT and if (by what is presently our postulate) the scientific enterprise endeavors to achieve an IncMT, it does not follow that the scientific enterprise endeavors to achieve CS. The crucial information regarding the relationship between the two may not be included in the information available to, and being acted upon by, the scientific enterprise.

While the above articulation of the relation between the postulate and the consequent-set was largely in the abstract, addressing this objection requires a brief step into the descriptive regarding what occurs in science. I contend that the claim, "CS is a necessary condition for increasing the sort of truth science seeks (what I am calling manifest truth)", is recognized and in fact promoted within and acted upon by the scientific enterprise. What first are the scientific enterprise's vehicles of promotion? They are the exchanges that occur under the rubric of science, those found in textbooks and lecture halls; in lab, departmental, and team meetings; at conferences; in journals; between supervisors and graduate students, etc. In short, they are the critical and supportive exchanges of both written and verbal form between colleagues themselves and their students. Also among the vehicles of promotion are the prototypical activities of the scientific enterprise, those that are put forward and appealed to in these various arenas as being examples of good science (essentially, Kuhn's "exemplars" (1970, p. 187)). I submit here that, in these activities and many varied forms of exchange, the scientific enterprise demonstrates its recognition of, and promotes, the tenet that CS is a necessary condition for an increase in the sort of truth sought in science, what I have deemed manifest truth. I consider the entailment relationship of concern to be an intuition that is endorsed very strongly within the scientific enterprise – an intuition I have attempted to make coherent in the above explication of what it is to increase manifest truth.

Consider the promotion of the increased elements of the consequent-set (empirical accuracy (E) and consistency (C)) as necessary conditions for the sort of truth sought in science. First, via the vehicles of the scientific enterprise at nearly all levels of inquiry, there will be found statements expressing doubts about the correctness of a theory or hypothesis due to an empirical anomaly and/or lack of consistency: e.g. assertions that Bohr contradicted Maxwell's electromagnetic theory and that this called for later work; assertions that Carnot's theory conflicted with Joule's work and required synthesis; claims that the null result of the Michelson–Morley experiment showed the falsity of ether theory, and that this called for some sort of resolution; claims that quantum mechanics and relativity "are known to be inconsistent with each other – they cannot both be correct" (Hawking, 1988, p. 12). I suggest that the demand for resolving these concerns stems from the view that increasing E and C are necessary for increasing the sort of truth sought in science.

Second, there will be actions in which the recognition of the tenet of concern is evident. I suggest that (what Kuhn calls) puzzle solving and the attempt to account for anomalies come directly from the recognition of the tenet that the achievement of manifest truth requires the achievement of E and C. Such action is prompted by the recognition that the theory complex taken as a whole cannot be correct. And what generally leads to that conclusion is the recognition of a theory complex's failure to attain E and/or C in some specified domain.

Let us turn to the second part of CS, those desiderata whose *retention* (though not necessarily increase) is necessary for attaining an IncMT. I suggest that the historical cases of complex revision appealed to as exemplary are those in which the retention of, or an increase in, testability (TST), breadth of scope (BOS), and the three forms of simplicity (S) can be recognized as having occurred. One case of complex revision that exemplifies the meeting of these necessary conditions would be the change from the assumption that space is Euclidian to the postulate that space is curved, as this involved the retention, at least, of TST, BOS, and S (e.g., space is curved was not a detached statement; non-simple, aberrant conjuncts were not added, etc.). (This case of theory change appears to have actually marked an *increase* in TST and BOS.) Moreover, cases where the conditions are *not* met are often ignored in historical appeals to good science – for instance, Leibniz's theory of monads. I submit here that what drives the appeal to cases in which these conditions *are* met in a complex is the *recognition* that these conditions are required for an increase in the specific sort of truth desired in science, what I am calling manifest truth.

The strongest evidence for the recognition and promotion within the scientific enterprise of the idea that manifest truth requires T, BOS, and S are historical occasions in which those who hold (or believe) certain metaphysical programs to be true seek, sometimes desperately, to connect them to the complexes with the hope of meeting those conditions. Einstein held some sort of hidden variable thesis to be true; Newton held that space is absolute; Dalton, Einstein et al. held the general statement of atomism to be true. Each of these scientists held the respective (at one time) detached thesis to be true. And, while avoiding a sacrifice in scope, while avoiding the addition of bizarre, non-simple conjuncts, etc., they endeavored to make that thesis manifest in predictions about the world. Such endeavors, I submit, are motivated by the recognition that T, BOS, and S are necessary for increases in the specific type of truth that is sought in science, manifest truth. (In Section 6, a few important clarifications will be made regarding the above descriptive points.)

So my response to the concern that the entailment relation is lost by introducing the terms "the endeavor to achieve" takes its place in (3) of the following summary:

(1) If we have an IncMT, then we attain CS;
(2) A necessary condition for achieving an IncMT is achieving CS (given 1);
(3) *Statement 2 is a tenet promoted within and acted upon by the scientific enterprise*.[23]
(4) Therefore, in endeavoring to achieve an IncMT, the scientific enterprise endeavors to achieve CS.

Note further that the endeavor to achieve an IncMT entails the endeavor to achieve *each of the seven elements* of CS, not simply one or two at the exclusion of the others.

I add that the *endeavor* to achieve an IncMT *promotes* a number of significant virtues beyond those contained in CS. Simplicity-3, discussed above dictates that between two equally successful complexes we opt for one containing, for instance, N over one containing nl, n2, n3 .... The retention of this form of simplicity is required for the *achievement* of an IncMT, but this requirement has bearing only when our complex entails a set of narrowed claims found in a competing complex that fits the same data. However, we can identify a related form of simplicity, *simplicity-4*, whose appeal is mandated by *the endeavor to* achieve an IncMT and comes into play beyond situations marked by such basic entailment relations. Among the key questions in the scientific realism debate is that of why we should prefer a simple complex to another contradictory complex containing many intricate *ad hoc* constituents, when both complexes can fit the data. The key to our answer comes from the posit that we are not merely after truth; we are after manifest truth. More specifically, the second postulate of our axiological realism mandates that we retain or increase the degree to which the truth of *individual MT statements* is made manifest. The force of this postulate becomes especially significant when we consider what it demands *in practice*. Because in comparing the two complexes we cannot be sure which statements in the respective complexes as true, we are prompted to *treat* all statements in the competing complexes as true. We are led to prefer the complex whose *individual statements* would, *if true*, be manifested as true to a greater degree. The complex whose individual statements would, if true, be manifested as true to a lesser degree, would not meet condition (2), i.e., would not be what we're after in our quest for an IncMT. Consider two contradictory complexes, both enjoying equal empirical success. C1 contains broad ranging encompassing statements while C2 contains many *ad hoc* narrow statements that nonetheless accommodate the data. The many individual statements in C2, even if true, would not be made manifest to any degree close to the degree that the fewer, broad ranging individual statements in C1 would, if true, be made manifest. By way of condition (2) in our axiological postulate, then, we are prompted to eliminate C2 in favor of C1. Thus, while it is not *required* by the *achievement* of an IncMT, a fourth type of simplicity is *promoted* by the *endeavor* to achieve an IncMT.[24]

Additionally, the sort of testability discussed above *may* include, but is not limited to, the prediction and testing of *temporally novel predictions*. However, the derivation and testing of temporally novel predictions, in particular, is strongly encouraged by our quest for further increases in manifest truth. If our theories are true, a confirmed novel prediction will constitute an IncMT. And a failed test of a novel prediction will serve to indicate where truth, if present, has not been made manifest. Such an MT-deficiency will prompt and guide us toward modifying our complex. Similarly, the pursuit of IncMT promotes *explanatory depth*. It encourages us to postulate deeper explanatory statements so long as those statements, if true, can be made manifest as such – that is, so long as the addition of those statements to our complex brings about an increase in empirical accuracy, so long as they would, if true, be manifested as true in a wide range of phenomena, etc. I conjecture that further analysis of the endeavor to achieve an increase in manifest truth would show that it encourages many more actual desiderata of science. However, I will stop here. Collectively, I will refer to these promoted virtues – simplicity-4, temporally novel predictions, explanatory depth – as *the secondary-set* (SS).

5. EPISTEMIC UTOPIA

Having articulated my axiological postulate and its intimate relation to the ten theoretical desiderata found in CS and SS, I now turn to indicate some of the virtues of my axiological realism. What has been said thus far is compatible with treating an increase in manifest truth (IncMT) as a transcendent ideal, or more specifically, an epistemically utopian goal, one we cannot justifiably believe we've achieved. I argue here that, even if an IncMT does fall into this category, construing science as pursuing such an end need not render that enterprise irrational. (The positive justification for my postulate will follow, in Sections 6–9.)

Larry Laudan is perhaps the most vocal opponent of axiological realism and of pursuing transcendent ideals in general. Laudan claims that construing science as aiming at the truth ''leads to the view that science represents a utopian, and therefore irrational activity'' (qtd. in Rescher 1982, p. 227). Laudan writes, ''to adopt a goal . . . whose realization we could not recognize even if we had achieved it, is surely a mark of unreasonableness and irrationality'' (1984, p. 51).[25] He emphasizes that if a transcendent ideal is posited as the aim of science, we have no means at our disposal to discern that science is progressive. Progress is a relative notion: it is relative to some end. Showing that science is progressive requires the ability to identify (i.e., justifiably believe) that it has advanced toward its goals. However, truth, according to Laudan, is a transcendent ideal, ''closed to epistemic access'' (1981a, p. 145). Thus, upon the proposal that science aims at the truth, ''science emerges as non-progressive since we evidently have no way of ascertaining whether our theories are more truth-like ... than they formerly were'' (1981a, p. 145). While the *immediate* claim pertains to progress, its bearing on rationality comes from the intuitively plausible notion that, if science is rational, it must at least make progress.

Does postulating an ideal as the aim of science eliminate the possibility of deeming science progressive? Against Laudan, Nicholas Rescher (1982, p. 220, p. 229) points out that the need for discerning progress does not prohibit a transcendent ideal from being employed as *an* aim of science. It only precludes that ideal from being employed as *the specific means by which we measure our* *progress*. We need not commit ourselves to the *progress determination thesis* – i.e., the thesis that progress in science is determined by the extent to which its primary aim is achieved (or the degree to which that aim is approximated). Drawing on Rescher's point for our own system, consider the entailment relation I've delineated above: an IncMT requires the attainment of the consequent-set (CS), an increase in, or the retention of, each of the seven virtues. And we can identify when CS has been achieved in a complex modification. Assuming that science does pursue an IncMT, we can then measure the progress of science, and show that science does progress, by assessing its ability to attain the specific conditions that must be attained to achieve an IncMT. While this measure of progress is not identical to the achievement (or approximation) of our primary goal, it is inextricably and directly relevant to that goal. In short, we can affirm that science makes progress in attaining the necessary conditions of its primary goal. Even if an IncMT is epistemically utopian, construing science as pursuing that end need neither render science non-progressive nor irrational.

One might discern a second argument against the admissibility of ideal goals in the context of Laudan's normative naturalism, specifically his meta-methodology. Laudan's meta-methodology – his means for determining the acceptability of methods – involves linking methods with aims via hypothetical imperatives: "If one's goal is y, then one ought to do x" (1987, p. 203). Such statements rest on claims of the following sort, "Doing [x] is more likely than its alternatives to produce [y]" (p. 205), which can be tested against historical data. This provides a means for testing the efficacy of methods toward various goals – the intended product being a collection of empirically supported, normative methodological rules. Now Laudan's meta-methodology is not developed as a criterion for goals. However, because it links action to goals, were we to espouse that meta-methodology, we would need a suitable axiology. Laudan points out that, were our axiology to allow transcendent aims, "we would no longer be able to say that a methodological rule asserts connections between detectable or observable properties" (1987, p. 205, footnote 18). His worry is this: if we invoke a goal we cannot justifiably believe we have achieved, we cannot empirically test whether an action (method) has led to that goal. Thus we cannot assert that the particular action *should* be part of our methodology. For this reason, it may seem, an axiology that permits utopian goals would be incompatible with the demand for a meta-methodology of the sort he proposes.

Assume again that our postulate is correct: science seeks an IncMT. In answer to the demand for a meta-methodology, let us look again to the relationship between an IncMT and the seven virtues of CS. We can test the efficacy of our methods in bringing about the latter goals, which are necessary conditions of our transcendent aim. Since methods that do not lead to these ends do not lead to our ideal goal, we have reason to discard those methods. Therefore, we *can* invoke a meta-methodology, and it is one that has significant bearing on our ideal goal. While some transcendent goals may be rendered irrational by Laudan's arguments, we find ourselves without reasons to exclude *our* transcendent goal as irrational.

Let us consider potential positive grounds for pursuing transcendent ideals in general and then see if these grounds would be applicable to the pursuit of an IncMT. Rescher argues that the pursuit of transcendent goals can be justified by their utility. Citing Kant, Rescher points out that, while we may never be able to recognize the attainment of moral perfection, we are justified in striving for moral perfection if, in our doing so, we elevate our morality (1984, p. 151; 1992, p. 93). While a craftsperson may never know when she has perfected her craft, if, in striving for perfection, her technique and product improve, the pursuit of such an ideal is rendered rational (1987, p. 29). While the goal of perfect health (1987, p. 29) may be unrecognizable, if, in striving for that state one feels better, is more active, is happier, lives longer, etc., the pursuit of that ideal may be rational. Despite our inability to know when we've attained a utopian aim, its pursuit can be legitimated if it is effective in bringing about other accessible and valuable goals. On Rescher's utility picture of goal justification, we see, a goal need not be among the consequences of the actions it produces, so long as those consequences include other desired ends. The transcendent aim itself takes the role of a methodological device. Not only do we attribute utility to the action in bringing about those ends, we may also credit the goal that prompted that action. As noted earlier, while Rescher takes truth to be a transcendent ideal, he defends a realism of intent, i.e., an axiological realism. In fact, he attempts to justify the pursuit of truth using his utility model of goal justification. I have argued elsewhere (2001, Appendix 3) that Rescher has not successfully followed through on the project he has proposed. Nonetheless, he has given us a framework for dealing with transcendent ideals as goals.

I suggest that this utility model for justifying transcendent ideals could be fruitfully applied toward a justification of the pursuit of an IncMT. The endeavor to bring about an increase in the manifest truth of our complexes *prompts us* to do what is required to bring about that goal. Specifically, given that an IncMT requires CS, we are prompted to invoke a meta-methodology, i.e., to test for methods that will be efficacious toward CS.[26] If our actions or methods do not serve us by bringing about CS, we know they cannot be serving our attempt to bring about an IncMT; and we are prompted by our primary/leading goal, to revise those actions. Upon identifying methods that are efficacious toward achieving CS, the endeavor to achieve an IncMT prompts us to *employ* those methods. Given our possession of a suitable meta-methodology and given the fact that CS is identifiably achievable, pursuing an IncMT guides us toward or is effective in bringing about CS, thus the individual elements included therein.

Since the utility model requires our transcendent goal to be effective in bringing about *valuable* ends, at least some of the elements of CS must posses *value in themselves*, i.e., value beyond that which they receive via their inclusion in the set of necessary conditions for an IncMT. I take it as non-contentious that at least two of the elements of CS do possess intrinsic value: the achievement of an increase in empirical accuracy (E) offers predictive reliability in the range of phenomena to which it applies; and the retention of breadth of scope (BOS) preserves our ability to make predictions about a broad range of phenomena. (While other elements of CS may also have their own intrinsic value, I focus here on E and BOS.) E and BOS are therefore desired ends. Since the endeavor to achieve an IncMT requires E and BOS, pursuing the former prompts us toward action, toward employing those methods that serve to bring about E and BOS. The pursuit of an IncMT is in this way efficacious toward the achievement of these desired theory characteristics. Just as we may be justified in pursuing the goal of perfect health – were it to lead us toward feeling better and living longer, etc. – I am proposing that the endeavor to bring about an IncMT could be justified by its utility toward these subordinate but nonetheless desired virtues. And this justification could stand, regardless of whether or not we can discern when an IncMT has been achieved.

6. THE EXPLANATORY AND JUSTIFICATORY VIRTUES OF AXIOLOGICAL REALISM

As has been suggested, I do accept the descriptive claim that these virtues have been and are sought in science. While I cannot provide a full defense of that claim here, a few clarifications of the commitments to which the axiological postulate leads are in order.[27] While there have been many cases in which theories do not posses certain criteria from the moment of their inception, these problems are not simply put to the side: often, the theories are nonetheless *pursued* (though not necessarily accepted) with hope that these failings will be eliminated by further inquiry. Complexes are chosen for pursuit with an eye to their *capacity* or potential for meeting the conditions. As theory choice is a long term, gradual process, made after long deliberation, articulation, and comparison, our postulate pertains to theory choice as it is made over a span of time. And as has been emphasized, empirical accuracy, consistency, and the other elements of the consequent-set are *not* being promoted here as sufficient for an IncMT; they are only asserted to be necessary. Thus no claim is being made that the history of science constitutes a convergence on truth.[28] While most realists struggle to downplay the dramatic nature of large-scale theory change by emphasizing or exaggerating the retention of specific theoretical constituents, we have no need.

It is also important to recognize various points in respect to the particular virtues of CS. For instance, while significant overlap in phenomena will be required to constitute a genuine case of complex modification, the claim that *breadth of scope* is retained requires only that a new/revised complex account for at least *as much* phenomena as its predecessor. It need not be exactly the same phenomena. (That is, the phenomenon often deemed "Kuhn-loss" poses no threat) Similar points hold for *testability*; so long as a new/revised complex can be tested as thoroughly as its predecessor, variations in some of the *specific* phenomena by which the complexes can be tested are not precluded. Additionally, the claim that *simplicity-1* is sought does not require denying that disconnected theses are entertained on the fringes of complexes and afforded adequate time to be connected; detached statements might be seen to show promise or to offer heuristic advantages. Nonetheless, the axiological postulate asserts that a complex is only accepted once these conditions, attachment and testability, are recognizably met. Similar points hold for the virtues of SS. For instance, while there is debate about how important the confirmation of *temporally novel predictions* is for scientists, I do not consider it contentious that scientists at least endeavor to *derive and test* such predictions. And I submit that *greater explanatory depth* is preferred on the condition that the modification brings about additional confirmed empirical predictions. Given these clarifications, I think it must be granted that science seeks CS and prefers SS.

We've seen that seeking an increase in manifest truth (IncMT) entails, thus requires, the achievement of *each of the seven elements* of the consequent-set (CS), not simply one or two at the exclusion of the others. I propose that, accepting the descriptive claim that CS *is* sought in science, the pursuit of each and all of the elements of CS can be *understood* by way of our axiological postulate: if CS has not been attained in theory change, the primary goal, an IncMT, has not been achieved.[29] We've seen further that the quest for an IncMT encourages the three elements of SS. In short, I am proposing that the following facets of theory choice in science can be *explained* by the posit that the scientific enterprise endeavors to achieve an increase in manifest truth: the endeavor to achieve an increase in empirical accuracy and consistency; the retention of testability, breadth of scope, simplicity-1, simplicity-2, and simplicity-3; and the preference for retaining simplicity-4, for testing novel predictions, and for seeking greater explanatory breadth when new empirical predictions are confirmed.

Below, I will consider other possible explanations of these phenomena and compare them against our own. Before doing so, allow me to extol another closely related and crucial virtue of our axiological realism: not only does the quest for an increase in manifest truth *explain* the pursuit of the virtues of CS and SS, it also *provides a rationale* for the fact that the CS and SS are sought. In other words, we can redisplay the entailment relationship between an IncMT and CS in terms of justification. In quest of an explanation we ask, Why *do* we seek CS and SS? In quest of justification we ask, Why is it *appropriate* to seek CS and SS? What reasons can we give for seeking CS? Given that we are pursuing an IncMT, we are justified in pursuing CS; for achieving the latter is a necessary condition for achieving the former. If CS is not attained in changes made in a complex, those changes cannot constitute an IncMT. Our pursuit of CS is, therefore, justified by the pursuit of an IncMT. And since the quest for the virtues of SS is *promoted* by the endeavor to achieve an IncMT, our quest for SS can likewise be justified by that endeavor.[30]

7. COMPARISON WITH LAUDAN'S NON-REALIST ACCOUNT OF SCIENCE

I am claiming that the postulate that science seeks an increase in manifest truth explains the pursuit of the consequent-set (and the secondary-set) and that it affords a justification for the pursuit of these collective ends. Are there rival systems that will do the same?

Let us consider the system put forward by the leading critic of axiological realism, Larry Laudan, and ask how it fares against our own. Focusing for the moment on empirical accuracy (E) and consistency (C), we note that, Laudan agrees that both are central goals of the scientific enterprise. In addition to the demand that empirical problems be solved, he emphasizes the "correlative demand that theories must minimize conceptual difficulties" (1996, p. 81). In his system, a theory poses a conceptual problem when it is "internally inconsistent" and/or makes "assumptions about the world that run counter to other theories" (79). He writes, "a broad range of both empirical and conceptual checks are of equal importance in theory testing." His system, he says, "explicitly acknowledges that both concerns are co-present" (80). He insists that any theory of science wishing to explain how science works must not ignore these correlated aims.

Quite significantly, Laudan emphatically contends that the *pursuit* of theoretical desiderata (such as E and C) must be justified by other goals. For Laudan, all reasons for doing something (including pursuing certain desiderata) are reasons put forward in terms of a goal being sought. "Good reasons are instrumental reasons; there is no other sort" (178). This is especially explicit in the context of his meta-methodology. Laudan provides a list of what he calls "methods" (131–132). (He is not necessarily advocating these "methods," only listing them.) Imposing any strict understanding of "methods," the only true methods in his list of ten are double blind techniques and controlled experiments. The eight other so-called "methods" are not, in any strict sense, methods at all. They are really goals (or subgoals) regarding the sorts of theories we might wish to attain. Like the mandate that we seek the elements of CS, they are mandates that we seek theories that have such and such characteristics. To paraphrase his list (131–132), he includes seeking theories that

are falsifiable;
are non-*ad hoc*;
render temporally novel predictions;
can "exhibit an analogy with successful theories in other domains";
do not invoke unobservables;
are consistent;
are simple;
explain "the successes of [their] predecessors".

These goals (in addition to the two methods noted above) complete his list.

Now, for Laudan the justification of these goals requires that they be linked to another goal. He writes that the examples above "are propounded for a *particular reason*", (my emphasis) and that by "formulating" them "without reference to the axiological context which gives them their bite . . . one is systematically disguising the route to their warrant" (132). Imposing the more accurate terminology of "subgoals" on his statements (in place of his word, "methods"), he tells us these *subgoals* "should be construed not...as if they were categorical imperatives, but rather as *hypothetical imperatives*...whose antecedent is a statement about aims or goals, and whose consequent is the elliptical expression of" the *subgoal*, or as he puts it, "the mandated action" (132). Laudan himself is insisting that the rules, "reject inconsistent theories," "prefer simple theories," and "propound only falsifiable theories" – all of which are mandates for theoretical desiderata – must be justified by a goal.

We see then that, according to Laudan, not only are E and C sought in science, they must be connected to goals for their justification.[31] To what goal can he relate them? Unfortunately, Laudan does not take pains to provide *examples* of the sorts of goals to which his subgoals (or, again, as he calls them, "methods") are supposed to be connected.[32] He does however offer an overarching goal of science: he claims the broad, general aim of science is problem solving: "the aim of science is to secure theories with a high problem-solving effectiveness" (78). Overarching though this goal may nonetheless be, I'd suggest its pursuit can neither explain nor justify our quest for E and C. For, given that the lack of E and the lack of C constitute problems in Laudan's system, the question of why we are pursuing E and C is simply pushed back. It becomes, "Why do we seek to solve such problems?" Another goal is needed beyond problem solving. The crucial point is this: *a state of affairs is only a problem in respect to something we are seeking*. A problem is only a problem in respect to some goal. What is the goal in respect to which the "problems" we seek to solve are problematic, i.e., the goal whose pursuit is being hindered by the empirical and conceptual problems? We appear to be lacking an account of why problems are problems and why we should solve them.

One might suspect Laudan could reply that problems are problems in respect to the aim of maximizing "the *progress* of scientific research traditions" [my italics] (1977, p. 124). But this response would again only push the question back further; for in Laudan's system "science *progresses just in case successive theories solve more* [empirical and conceptual] *problems than their predecessors*" (124). The question remains: Why do we desire progress in the form of E and C in our research traditions? With Laudan's system, it appears that problem solving is just something we do, and we are given no reason for doing it. Despite his demand that subgoals receive their warrant from the goal they are linked to, he has not provided any explanation of, or justificatory merit for, problem solving, for seeking E and C in our complexes. Axiological realism by contrast tells us why a lack of E and/or C is a problem: it is evidence that truth is not being manifest. An increase in the manifest truth of our complexes cannot occur without an increase in E and C.

Now I cannot see that a theory of science could be adequate while denying that science also favors the retained elements of CS – testability, breadth of scope, simplicity-1, simplicity-2, and simplicity-3.[33] And Laudan does note that "even the most ardent empiricists grant that considerations of simplicity, economy and coherence play a role in theory appraisal" (1996, p. 52). Were Laudan to grant that these (sub)goals are pursued in science – as I think he must – the threat is compounded. For we find ourselves asking, why are these seven different goals of CS pursued? Given Laudan's system, the mandate for each of the seven goals will take the form of a categorical imperative rather than a hypothetical imperative. And, as we've seen, he insists that such mandates be formulated in the hypothetical form. The consequence: in Laudan's system, we are left with no justification for pursuing these seven separate and ostensibly unrelated ends. By contrast, according to our axiological realism these virtues are elements of CS, and CS is required of an IncMT: if we have not achieved these ends, we have not achieved our primary aim.

Additionally, we've seen that the pursuit of an IncMT strongly *encourages* the testing of novel predictions (as well as simplicity-4 and explanatory depth). From the context of Laudan's system, however, one wonders why testing a temporally novel prediction would be of value. It is not obvious that confirming predictions of previously unknown phenomena would solve any "problems". And testing a prediction that turns out to fail would only create a problem. Without a broader goal, why would we want to create any problems? From an axiological realist standpoint, either result is valuable: if the novel prediction is confirmed, it may constitute an IncMT. If not, it will make evident an MT-deficiency; it will reveal a point in our complex where we are lacking MT statements. Thus it will push us to modify our complex in quest of an IncMT. Our axiological realism appears to possess far greater explanatory and justificatory value than Laudan' s non-realist system.

8. COMPARISON WITH VAN FRAASSEN'S NON-REALIST ACCOUNT OF SCIENCE

What about Bas van Fraassen's non-realist position? The aim he has posited for science is empirical adequacy – a state in which everything a theory asserts about observable things is true. First we recognize that, despite the modest term, "adequacy", this really amounts to empirical perfection and may itself be epistemically utopian. (For instance, it suffers historically under what I call the *pessimistic meta-modus tollens*, see my (2001, Chapter 5), and my (2003)). Thus, van Fraassen may well be faced with addressing Laudan's objections to transcendent ideals (against which I've defended axiological realism in Section 5). Second, I suggest that a justification for the practice of appealing to simplicity-2 poses a significant threat to van Fraassen's axiology. It has been noted in Section 3 that, for any theory we might accept, indefinitely many empirically *distinct*, yet equally successful, competitors can be generated at will. Despite their empirical success, however, in scientific practice, the entire set of competitors is rejected without reflection. Van Fraassen holds that "empirical adequacy is the pre-eminent virtue" (1980, p. 92) and the aim of science (12). However, because each member of that set would, at any given time, enjoy the same empirical success as any favored theory, and because that set is indefinitely large (to reiterate Nicholas Maxwell's (1998) point noted above), we can obtain no *empirical* justification for choosing our favored theory over the indefinitely many competitors in that set. With no empirical grounds for our choice, the quest for empirical adequacy provides no guidance.

It appears instead that an appeal to what I've been calling simplicity-2 is required to eliminate such competitors. It may be noted in defense of van Fraassen that, for van Fraassen, *explanation* is pragmatic and that, according to him, the appeal to simplicity in an explanatory context is pragmatically valuable. It is on pragmatic grounds that we should select, for instance, that theory among empirically equivalent theories that posits fewer explanatory mechanisms. However, the practice I am pointing to is that of choosing the simpler theory among the class of *empirically distinct* but equally successful theories. This context in which simplicity is employed *is not* distinctly *explanatory*, especially for van Fraassen: the divergence lies not in the explanatory mechanisms postulated by the theories but in the theories' claims about the phenomena themselves.[34] Van Fraassen himself explicitly denies that simplicity *indicates* empirical adequacy:

"the virtue, or patchwork of virtues, indicated by the term ["simplicity"] is a factor in theory appraisal, but does not indicate *special features* that make a theory more likely to be true (*or empirically adequate*)" [the second italics are mine] (90.) Nor can simplicity-2 be said to be a necessary condition of empirical adequacy: only one theory among the set of equally successful yet empirically distinct theories can be empirically adequate; and the empirical data affords no grounds to assume that the empirically adequate theory is not one of the non-simple competitors. Here, with an emphasis on empirically distinct rather than equivalent competitors, and on a specific type of simplicity, we see one of van Fraassen's most potent points against realism turned against him, the problem of simplicity. The possibility of redirecting this threat also bears significantly on the epistemic dimension of the scientific realism debate (e.g., believing that the theory is empirically adequate – which, for van Fraassen, is central to theory acceptance – appears to require an epistemic justification for simplicity-2). However, my focus here is axiology and my contention is that the quest for empirical adequacy can neither explain nor, provide justification for, the ubiquitous practice of choosing the simpler theory over the indefinitely many non-simple, empirically distinct, alternatives.

One might be concerned that, even if axiological realism does provide the *best explanation* for the phenomena emphasized, this point carries little if any weight against the likes of Laudan and van Fraassen: non-realists need not, one might claim, share a preference for the best available explanation. In response, I submit, first, that any account of science is an attempt to best explain it. Peter Lipton makes this point in respect to Kuhn's account of science (1993, pp. 57–58). I add that it holds no less for the proposals of Laudan and van Fraassen. According to Van Fraassen, a central part of his "positive argument for constructive empiricism" is that "it makes better sense of science, and of scientific activity, than realism does" (1980, p. 73). We are all attempting to put forward a theory of science that best explains that enterprise. Second, my argument goes beyond asserting that axiological realism is the best available explanation: I am crucially pointing out that axiological realism provides a *justification* for the virtues of CS and SS, while these alternative axiologies do not. Finally, as I will note below, I am not claiming that the explanatory strength of my axiological realism licenses belief in it. For these reasons, I submit that my argument for axiological realism is not threatened by my appeal to its explanatory superiority.

9. CONCLUDING NOTES

We have not exhausted the possible axiologies.[35] And given the non-verifiability of nonexistence statements, I cannot show that no other non-realist goal exists by which the pursuit of these virtues can be explained and justified. (So I will not follow epistemic realists who invoke a no-miracles argument.) However, as the systems of the two prominent non-realists, Laudan and van Fraassen, stand, such a goal is left wanting, and I suggest that their non-realist systems are thereby rendered inadequate in comparison with the one I've been articulating.

As has been touched on in Sections 1 and 2, the standard axiological tenet of realism fares no better. The problems are not limited to supraempirical virtues such as simplicity. In regard to epistemic realism, I've argued in my (2003) that the mere stipulation that "T is true" – taken in itself, with no stipulation whatsoever regarding auxiliaries, etc. – does not even render likely the empirical success of T (likewise for "T is approximately true"). Nonetheless, the epistemic realist points to empirical success as that which only the (approximate) truth of the theory can explain. Implications extend to the standard axiological claim that "science seeks true theories": quite surprisingly, that naïve postulate may have trouble providing an explanation or justification for even the most basic demand of theory choice, empirical accuracy. (Similar threats apply in respect to the other virtues as well.) The realist axiology, as it is usually construed, may well fare worse than those of the non-realists.

A final possibility to be considered is that science has no deeper goal at all: the ten virtues of CS and SS are simply pursued. Without a deeper goal, however, those ten virtues would stand as seemingly unrelated and disparate ends, whose individual and collective pursuit we cannot justify. We would be unable to say why scientists *should* revise their complexes so as to attain greater empirical accuracy, consistency, etc. And we could give no reason for preferring this set of virtues to any other randomly selected set. While such a position may be possible, it takes us well outside of the scientific realism debate, a debate in which the pursuit of these desiderata is taken to be non-arbitrary and justified. By contrast, the claim that we seek an IncMT extends well beyond seeking only CS and SS. Manifest truth requires not only the collective virtues but also supraempirical accuracy. As has been emphasized, we are afforded a rationale, a justification for problem/puzzle solving, for modifying a complex when falsity becomes apparent, etc. We can specify a reason for engaging in such activities. For, in the light of axiological realism, we want our modifications to constitute an IncMT. And we cannot achieve what we want unless we attain CS. We can here discern the value of having a leading, overarching, and unifying goal in general. Such a goal can provide not only an explanation, but also good reasons, for the pursuit of its subgoals – and the ability to offer these provides for a far more coherent, robust, and, I think, rational system.[36] Embracing Insight 4 (Section 1) regarding the comparative nature of theory choice, I advocate axiological realism as the best contender among those considered.

While the points made in Sections 6–9 stand as part of the justification for my postulate that the scientific enterprise endeavors to increase the manifest truth of its theory complexes, I do refrain, as noted, from claiming we are justified in believing that postulate. Perhaps this strikes the reader as odd. However, I readily advertise that any evidential support my axiological realism may obtain will pale beside the evidential support enjoyed by successful scientific theories. And if, as I have argued elsewhere, we have yet to show we are justified in believing the latter, we can hardly extend more confidence to a philosophical theory about science. Instead, I put forward my postulate as one to be conjoined to our theory complex about the nature of science. Upon so conjoining it, we can compare it to its competitors and test whether the postulate itself meets the identifiable necessary conditions of bringing about an increase in manifest truth. That complex is a starting point that invites refinement. Though I resist asserting that my axiological realism, itself, constitutes an increase in manifest truth, my hope, of course, is that it does.

ACKNOWLEDGEMENTS

For conversations and/or written comments regarding the topics of this paper, I am deeply indebted to Howard Sankey, Neil Thomason, Brian Ellis, John Worrall, Alan Musgrave, David Papineau, Peter Lipton, Stephen Ames, Kristian Camilleri, Jason Grossman, Herman de Regt, Martin Carrier, Gerald Doppelt, Cornelis de Waal, André De Tienne, and Michael Burke.

NOTES

[1] For a full list of these foundational posits see (Lyons and Clarke, 2002).

[2] Rescher equates truth with *perfected science*: "the truth = the truth as ideal (perfected) science purports it to be" (1982, p. 224). He writes, "Only at the idealized level of perfected science could we count on securing the real truth about the world that 'corresponds to reality,' as the traditional formula has it" (1992, p. 298). See also (1982, p. 224), (1984, p. 152) (1987, p. 32). In accord with this, to avoid confusion, I am taking the liberty of using the word "truth" in place of "perfected science" while discussing Rescher.

[3] While I will draw on some of Rescher's insights below, I reject his particular axiological realism in my (2001).

[4] In my (2001, Chapter 3), I argue against the common view that the connection is or can be empirically secured.

[5] The distinction here is often articulated as the problem of discovery versus that of justification. (Of course, Insight 1 does not in itself deny that internal heuristics can guide modifications within large-scale systems, etc.)

[6] For instance, Wolfgang Pauli's neutrino hypothesis saved the law of conservation of energy from the threat posed to it by beta-decay experiments. Yet, neutrino detectors were not developed for three decades.

[7] Although the realist is more inclined to emphasize the practice of favoring the better explanation, non-realists accept (or at least endeavor to allow) that such a preference is part of scientific practice. To accept this is not, of course, to grant license for inferring that the better or best explanation is true.

[8] Even so, it is often held, we can at least comparatively evaluate the methods (and thereby the products) of intellectual inquiry/science (Laudan).

[9] There are, of course, variants of naturalism. But the basic tenet noted here is rather non-demanding and commonly accepted.

[10] This is not to say philosophical theories should, like scientific theories be universal, or that they must make future predictions, etc. There is, for instance, nothing to prevent the activity of science from becoming something other than what it has been.

[11] It is, I believe, falsifiable. In fact, I contend that Laudan (1981) has shown that, in its simple form, it is falsified. Of course, taking a lesson from Insight 3a, we reject naive falsificationism and invite modifications to the hypothesis, one of the most sophisticated modifications being what I call deployment realism. Though I consider this a noble effort at salvaging epistemic realism, I criticize it in my (2002).

[12] However, I'd suggest that, aside from Insight 5 – which feeds the naturalist program in philosophy – few of these insights have been significantly appealed to as providing guidance or license.

[13] An exemplary case from science: the conjunction of the two postulates of special relativity is not accepted due to the *prima facie* intuitive plausibility of that conjunction. In fact, in respect to common and immediate experience the consequences of that conjunction are counterintuitive. Only when it is shown that certain criteria are met by that conjunction does special relativity attain its support. We see here an interesting consequence of the naturalistic move: although some criteria by which we measure our philosophy become more stringent, others can be less stringent.

[14] Although I expect (and hope) that the epistemic realist would grant that this is unlikely, the *acceptance* of my postulate does not depend on that – or, for that

matter, on the plausibility of any of the other points I'm noting in this section. Again, I am simply spelling out some (of my) motivations for the postulate.

[15] This specification is not, of course, meant to *exhaust* the subset of truth sought. The specific questions asked within a particular complex will determine *just which* MT statements are candidates. While the questions can in some sense arise from within the complex itself, we need not deny that psychological, social, economic, and/or political interests will delimit the questions further. Allowing that *questions of interest* restrict just which MT statements are sought poses no problem for the axiological postulate, which says only that *the desired answers*, etc., remain MT statements.

[16] To avoid increasing the non-MT statements in our complexes is to

(a) avoid conjoining additional false statements
(b) and avoid replacing statements so as to increase falsity, i.e., avoid replacing

– true (whether manifested as true or obstructed) statements with false statements
– and false statements with more universal false statements, etc.

(c) avoid conjoining vacuous and detached statements.

[17] According to this view, there is simply no reason to modify the complex otherwise.

[18] That is, MT statements whose predictions can be compared against at least as many available data *statements* as could the predictions from the false statements.

[19] Of course such matches will often occur only via "bridges," e.g., auxiliary statements regarding the margin of error, etc.

[20] The set of possible statement-parings of a theory complex consists of subsets containing each statement in the complex paired with each other statement in the complex: (S1, S2) (S1, S3) (S1, S4) (S1, S5) . . . (S2, S3) (S2, S4), etc. If we have ten statements in a complex we have forty-five statement-pairings. If we add one statement we have ten new statement-pairings.

[21] Relevant according to the complex itself, or according to "background information". With this qualification, many predictions derived from contra-positives would not qualify as prediction types of the complex.

[22] This point makes salient an important relationship between the conditions. While condition (1) includes an increase in the number of MT statements as a means by which a complex can qualify as an IncMT, condition (2) restrains that condition; requiring breadth of the statements, it limits the number of total statements in the complex.

[23] Of course, much in addition to premise 2 is promoted, including claims regarding other such relationships: e.g. perhaps, "empirical accuracy and consistency are not only necessary but also sufficient for the truth." etc. Whether this is so, however, does not threaten the point of immediate concern, that this particular tenet about the relationship between an IncMT and CS is promoted in science.

[24] The *endeavor* to achieve an IncMT also gives *extra* impetus to the demand for testability. It is only by testing our theories against the world that we can hope to discover falsity (or more carefully, what we've called above, Type A evident MT-deficiencies); i.e., it is via testability that we can best note where truth has not been made manifest. Thus, testability is a quality demanded for *further* increases in manifest truth.

[25] Notably, epistemic realists express similar sentiments. Newton-Smith writes, "It is, if not downright irrational, certainly pretty unpalatable to play a game which you have reason to believe cannot be won" (1981, p. 183).
[26] As I will note below, Laudan tends to view a meta-methodology as a means for discerning which theory characteristics should be preferred or avoided (e.g., "avoid ad hoc theories"). I would emphasize instead the specific actions themselves: among the candidates would be "test our theory in different areas," "try to falsify our theories," "employ inductive generalizations," "use abduction," etc.
[27] A more adequate defense of the descriptive claim can be found in my (2001).
[28] Arguments I have presented elsewhere strongly suggest that we avoid such a commitment (2001, 2002, 2003).
[29] I am inviting the reader to understand the history of science as being driven by the recognition of the tenet that CS is a necessary condition for the sort of truth sought in science, what I am calling an IncMT. According to this view, complex modifications, such as that activity Kuhn calls puzzle solving, are intended to clear the "clogged" logical channel between (what are hoped to be) true statements and the predictions drawn from those statements. The intention is *not merely to "immunize" the large-scale theories* (Kuhn) or to *protect the hard core* (Lakatos) of the complex in light of anomalous results; it is rather to make manifest (what is hoped to be) the *truth* of these theories.
[30] Considering this argument in light of earlier points, the concern that there may be a certain circularity involved may arise. Above I suggested, (1) *The pursuit of an IncMT could be justified by the fact that it brings about (pushes us toward a methodology that leads to) E and B (which are included in CS). And E and B are valuable.* Here I am asserting, (2) *The pursuit of CS (which includes E and B) is justified by the fact that it is required for an IncMT. And (I've postulated that) science seeks an IncMT.* It should be clear that, if there is a circle here, it is not entirely "symmetrical." For instance, in (1), the justification comes because pursuing an IncMT brings about CS. In (2), the justification comes about because CS (including E and B) is *required for* an IncMT. Further, in (1), the pursuit of IncMT receives its justification from the intrinsic desirability of only *some* of the elements of CS; in (2), it is the pursuit of elements of CS as *a set* that receives its justification from the pursuit of IncMT. If there is a circularity here it is "asymmetrical," to use an awkward metaphor. I'd also suggest it is minimal, if not innocuous. Finally, I contend (in my 2001) that, if we do demand that each goal in an axiology be justified – a demand that I contend in the next section Laudan makes – at least a minimal circularity of this sort will be required of any axiology.
[31] Whether the principle that all goals must be justified by other goals can or should be fully applied is discussed in my (2001).
[32] One example he does gives is "if one wants to develop theories which are very risky, then one ought to avoid *ad hoc* hypotheses" (1996, p. 133). Seeking risky theories is the justification for seeking *non-ad hoc* theories. Here again, seeking a goal receives its "warrant" from a broader goal.
[33] Though, as noted above, my defense of the claim that they are sought lies beyond this paper (see my 2001).
[34] Further, an appeal here to the *ease of* applying the simpler theory would conflate van Fraassen's aim and criterion for acceptance with the quite different instrumentalist aim and criterion invoked by, say, the engineer. Contemporary engineers can and do *accept* Newton's theory, N, (for use) because it is easy to apply and they take

it to be appropriate to a certain range of application. For van Fraassen, however, acceptance in science explicitly involves the *belief* that *all* the claims the theory makes about observables are true; and science seeks theories that make only true claims about observables (12). Since contemporary science denies, not only the truth, but also the empirical perfection of N, in contrast with the engineer, van Fraassen's contemporary scientists cannot *accept* N. The two criteria are altogether distinct.

[35] Among these might be the idea that we can provide an evolutionary justification for pursuing the desiderata of science, an idea I have challenged in my (2001).

[36] Importantly, my earlier assertion that *some* of the elements of CS may, as *individual* characteristics, have their own intrinsic value does not conflict with the concern that certain other systems fail to justify pursuing the *collective set* of the elements of CS. First, any particular intrinsic value possessed by an individual element of CS need not, in itself, be sufficient to justify the pursuit of that element: for instance (setting aside its relationship with the sort of truth I am claiming science seeks), the *intrinsic* value of a given form of simplicity may be, at best, that it is psychologically desirable (likewise, perhaps with consistency). While I would not appeal to this sort of value in making my case for the utility of pursuing IncMT, one might wish to say that psychological desirability does lend a theory-characteristic, such as simplicity, *some* intrinsic value. In any case, that value in itself would not, I suggest, be sufficient to justify the pursuit of that element in science. (For instance, our psychological preference for theories that accord with commonsense would not be sufficient to justify rejecting those theories that clash with commonsense, e.g., quantum mechanics, etc.) Second, even assuming that each element's intrinsic value were sufficient to justify the pursuit of that element, the collection of (what would probably be disparate) intrinsic values need not be adequate to justify the *mutual and balanced pursuit* of the elements. (For instance, *psychological benefit* would likely have to be made subordinate to *practical value*, etc., thus throwing off the balance of CS). Finally, even if somehow a piecemeal axiology of this sort, one that appeals to intrinsic value, could justify the collective pursuit of the elements, that pursuit would not be justified in so coherent and unifying a manner as it is by its relation to an IncMT. On these grounds, I submit that my earlier claim that some of the elements of CS possess intrinsic value does not conflict with my claim that these other systems cannot explain and justify the pursuit of CS.

REFERENCES

Duhem, P.: 1906, *The Aim and Structure of Physical Theory*, P. Wiener (trans), Princeton University Press, Princeton, 1954.

Feyerabend, P.: 1993, *Against Method, 3rd ed.*, Verso, London.

Hawking, S: 1988, *A Brief History of Time*, Bantam, New York.

Hempel, C. G.: 1966, *Philosophy and Natural Science*, Prentice Hall, Upper Saddle River, NJ.

Kuhn, T.: 1970, *The Structure of Scientific Revolution, 2nd ed.*, University of Chicago Press, Chicago.

Lakatos, I.: 1970, 'Methodology of Scientific Research Programmes', in I. Lakatos and A Musgrave (eds.), *Criticism and The Growth of Knowledge*, Cambridge University Press, Cambridge.

Laudan, L.: 1977, *Progress and Its Problems*, Oxford University Press, Oxford.
Laudan, L.: 1981, 'A Confutation of Convergent Realism', *Philosophy of Science*, **48**, 19–49, reprinted in D. Papineau (ed.), *Philosophy of Science*. Oxford University Press, Oxford, 1996.
Laudan, L.: 1981a, 'A Problem Solving Approach to Scientific Progress', in I. Hacking (ed.), *Scientific Revolutions*, Oxford University Press, Oxford.
Laudan, L.: 1984, *Science and Values*, California University Press, Los Angeles.
Laudan, L.: 1987, 'Progress or Rationality: The Prospects for Normative Naturalism', in D. Papineau (ed.), *The Philosophy of Science*, Oxford University Press, Oxford.
Laudan, L.: 1996, *Beyond Positivism and Relativism: Theory, Method, and Evidence*, Westview, Boulder.
Lipton, P.: 1993, *Inference to the Best Explanation*, Routledge, London.
Lyons, T. D.: 2001, The Epistemological and Axiological Tenets of Scientific Realism, Ph.D. Dissertation, Department of History and Philosophy of Science, University of Melbourne, Australia.
Lyons, T. D.: 2002, 'Scientific Realism and the Pessimistic Meta-Modus Tollens', in S. Clarke and T. Lyons (eds.), *Recent Themes in the Philosophy of Science: Scientific Realism and Commonsense*, Kluwer, Dordrecht 63–90.
Lyons, T. D.: 2003, 'Explaining the Success of a Scientific Theory', *Philosophy of Science*', **70**, 891–901.
Lyons, T. D. and S Clarke: 2002, 'Scientific Realism and Commonsense', in S. Clarke and T. Lyons (eds.), *Recent Themes in the Philosophy of Science: Scientific Realism and Commonsense*, Kluwer, Dordrecht, ix–xxiii.
Maxwell, N: 1998, *The Comprehensibility of the Universe: A New Conception of Science*, Oxford University Press, Oxford.
Newton-Smith, W: 1981, *The Rationality of Science*, Routledge, London.
Popper, K. R.: 1959, *The Logic of Scientific Discovery*, Routledge, London (1980).
Quine, W. V. O.: 1975, 'On Empirically Equivalent Systems of the World', *Erkenntnis* **9**, 313–328.
Rescher, N.: 1982, *Empirical Inquiry*, Athlone Press, London.
Rescher, N.: 1984, *Limits of Science*, California U P, Los Angeles.
Rescher, N.: 1987, *Scientific Realism: A Critical Reappraisal*, D. Reidel, Dordrecht.
Rescher, N.: 1992, *A System of Pragmatic Idealism: Human Knowledge in Idealistic Perspective, Vol 1.*, Princeton University Press, Princeton.
Van Fraassen, B. C.: 1980, *The Scientific Image*, Oxford University Press, Oxford.
Van Fraassen, B. C.: 1983, 'Theory Confirmation: Tension and Conflict', Seventh International Wittgenstein Symposium, Vienna: Hoedler, Pichler, Tempsky.

Department of Philosophy
Indiana University-Purdue University Indianapolis (IUPUI)
IN 46202-5140
U.S.A.
E-mail: tdlyons@iupui.edu