Forthcoming in *Reflections on Naturalism*, J.S. Galparsoro & A. Cordero (eds.)

Naturalism and Scientific Realism*

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1. Introduction

Little consensus exists in the literature about what either naturalism or scientific realism amounts to. The naturalist perspective considered in this paper takes philosophy and empirical science as continuous intellectual endeavors, united by mutual integration rather than reduction. To the extent that contemporary natural science takes all claims about the world to be synthetic, naturalism denies properly a priori knowledge. I agree with Sklar (2010) that fundamental branches of science typically comprise interpretive programs that are distinctly "philosophical." Hence my contrast camp to naturalism cannot be philosophy as such but radically anti-empiricist philosophy (e.g. rationalism, transcendentalism and mysticism). Naturalist positions vary depending on the degree of scientific empiricism they involve. Views that emphasize radical empiricist moves in science reject scientific realism, whereas those that emphasize moderate empiricist interpretations often favor realism.

Also in need of clarification is the term "realism" in connection with scientific hypotheses ("scientific realism"). Here I take as central van Fraassen's (1980)'s contrast between claims about observable and about unobservable aspects of the world. To antirealists, acceptance of a theory involves only the belief that it accurately describes phenomena within the reach of unaided human perception. By contrast, contemporary realists claim both that there is a world external to and independent of the mind, and that through experience, reason, imagination, and criticism, it is possible to obtain knowledge of that world, including aspects beyond the reach of unaided human perception[1]. I will the much reviled term "noumenon" to refer to entities and processes not accessed through the unaided senses (e.g. microbes, protein-folding, atoms, nuclear reactions, quarks, tunnel effects). This conception of the noumenon disregards idealist gobbledygook (e.g. "how things appear when they do not appear"). Realism simply does not grant epistemic primacy to "appearance."

The term "naturalist realism" will apply to any realist position inductively argued for from history and current knowledge about the character and cognitive achievements of science. Naturalist realist projects take empirical success as a marker of truth, the latter's assertability limited according to the degree of empiricism embraced. These

* Research support for this investigation was made possible in part by PSC-CUNY Research Award TRADA-43-748.

[1] For a good articulation of the claims of realism see e.g. Bunge (2006, Ch. 10.2).

projects argue the way scientific hypotheses are argued for in the natural sciences, i.e. especially in terms of agreement with data, consistency, and risky predictions. As we shall see in the following sections, naturalist and realist proposals present various difficulties. Vicious circularity reportedly mars attempts to justify naturalism, while realist proposals face objections concerning the limits of realist interpretation, the notion of "approximate truth," and the supposed advantages of realism over more modest interpretations of scientific success.

Scientific realism makes an attractive option for those who find global (metaphysical) skepticism excessive and agree that science exhibits instrumental progress that cries for explanation. Some naturalists, notably Michael Devitt (1999: 96), think realism becomes an irresistible option if one starts with an empirical metaphysics. Not all naturalists agree, however. Naturalist realism is a vibrant but difficult project; to see why it will be useful to begin with some history concerning the contending options. That is the subject of sections 2 through 5. Sections 6 through 11 discuss ongoing naturalist projects to make realism "irresistible." I start the historical part with William Whewell's realism, a position full of an optimism that ended with the breakdown of Newtonian mechanics in the early 1900s.

2. Some Historical Background

Empirical science, argued Whewell, progresses epistemically because the theories it offers pass stringent tests before scientists take them seriously. Whewell's emphasis was on predictive power, consilience, and coherence. Successful predictions of unknown facts, he stressed, provide greater confirmatory value than explanations of already-known facts (1858, Aphorism XII). No less significant was consilience, which occurs when an induction from the colligation of one class of facts also colligates facts belonging to another class (e.g. Newton's multiple inductions pointing to the existence of an inverse-square attractive force as the cause of different classes of phenomena). Cases of consilience "impress us with a conviction that the truth of our hypothesis is certain," Whewell thought (1858/1968: 87–8). What he termed "coherence" occurs when extending a hypothesis to a new class of phenomena can proceed without introducing *ad hoc* modifications, as when Newton extended his theory of gravitation to "tidal activity." These lines of testing did the required job, Whewell declared:

> "No example can be pointed out, in the whole history of science, so far as I am aware, in which this Consilience of Inductions has given testimony in favour of an hypothesis afterwards discovered to be false. … [W]hen the hypothesis, of itself and without adjustment for the purpose, gives us the rule and reason of a class of facts not contemplated in its construction, we have a criterion of reality, which has never yet been produced in favour of falsehood." (1847: 67-68)

In the 19th century scientists overwhelmingly agreed, not least Charles Darwin, who labored to pass at least the tests of consilience and coherence[2]. As the century entered its last quarter, Newtonian mechanics and gravitational theory, classical electromagnetism (including Maxwell's theory of the ether), and Lavoisier's chemistry, all passed Whewell's criteria of success with flying colors and were considered beyond reasonable doubt. Nothing, it seemed, could cast doubt on the scientific picture they

[2] See Ruse (1979, chapter 7). Darwinian arguments did not satisfy the requirement on prediction until late in the following century (Cordero 2011a).

yielded. But this confidence in the epistemic reach of science, along with Whewell's view of progress, were about to be shattered by the profound conceptual revisions that took place in physics in the early decades of the 20th century. Newtonian mechanics, it turned out, erred dramatically about the nature of mass, gravitation, matter, causality, separability, identity, and more. Confidence in the power to infer noumenal truth from empirical success plummeted, the scientific imagination now viewed as too fertile for realism to remain a plausible project. Reactions involving strong empiricist purges became widespread in theoretical physics. They prompted a group of thinkers to articulate "Logical Empiricism," a radical philosophy that privileged information from the unaided senses and distrusted abductive inference and theoretical speculation generally. To these thinkers, empirical success marked instrumental progress but not any advance of theoretical knowledge.

To their credit, the logical empiricists exercised a remarkably critical attitude towards their own claims. By the late 1950s, most had come to acknowledge that their image of science was both a logical impossibility and a historical falsehood. It seemed impossible to have completely theory-independent observation reports, on pain of their not being relevant to any theory. The positivist rationale for restricting ampliative inference to just observable levels collapsed. The end of the positivist program is well known[3].

Seizing on underdetermination and the problematic character of observation, Thomas S. Kuhn's *The Structure of Scientific Revolutions* (1962) extended to observation the distrust that radical empiricists had reserved for theory. Kuhn presented observation as something so theory-dependent as to claim that scientists adhering to different general theories live "in different worlds" (1962: 116-8). In his view, theories and their successors lack common neutral perspective from which to compare their respective merits (they are "incommensurable"). Kuhn's critique of traditional objectivity encompasses a holistic view according to which, if Einstein's mechanics is true then Newtonian (speed-independent) mass does not exist and Newtonian mass represents *nothing*. From this perspective, scientific descriptions refer to mere fictions and, therefore, the "real world" cannot (and should not) be the concern of science. Kuhn came to distance himself from this extreme relativist version of his views[4], admitting for example that scientific theory choice follows shared, informal criteria centered on accuracy, consistency, scope, simplicity, and fruitfulness[5]. Kuhn, however, never endorsed scientific realism, which he identified with the notion of theory-convergence on the truth. In the second edition of *The Structure*, he explicitly dismissed realist hopes as incoherent (1970: 206).

On the other hand, Kuhn made some naturalist moves. From page one of *The Structure*, he gives center stage to history and psychology, much as naturalists recommend. He presents scientific thought as a process sustained by relations of analogy and perceived similarity. This he supplements with an antirealist interpretation of scientific progress that Kuhn links to an argument focused on Darwin's realization that a species becoming more successful attests to local improvement, without any implication

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[3] See, for example, Hesse (1974, Chapter 1); Suppe (1977, Introduction).
[4] Kuhn (1977, 1992); see also Bird (2011).
[5] "Objectivity, Value Judgment, and Theory Choice," in Kuhn (1977).

that the species in question is evolving towards some ideal form (1962: 170-173). Likewise, Kuhn proposed, theories change and improve as scientists encounter recalcitrant facts and conceptual conundrums, but this progress has nothing to do with how closer a current proposal is to an ideal true theory, only with how well scientists solve the pressing problems at hand. Kuhn agreed that theories improve in predictive power, scope and fertility, but in this he saw no sign that they get objectively more truthful, because—he stressed—at each stage the relevant assessments are internal to the theory and thus rendered merely local by incommensurability. A few years later we find Willard V.O. Quine (1969) promoting the naturalist view that there is no higher tribunal for truth than natural science or a better method than the scientific method for assessing the claims of science. Nor, he warned, is there any need for abstract metaphysics or epistemology ("first philosophy" in his jargon) to justify science. Furthermore, he urged, the success of naturalism in science convinces one that scientific methods should also be used in philosophy. However, his critique of Logical Empiricism emphasized the corrosive import of empirical underdetermination and holistic theory-dependence on scientific observation. Quine's advocated naturalism was thus too mortgaged to strong empiricism to help realism.

Objectivist reactions to the above developments were strong. At their center were many of the moves associated with naturalism in contemporary philosophy of science. They are the subject of the next three sections.

3. Two Realist Reactions

By the mid-1960s, nearly everybody agreed that observation and scientific method depend on theories, perspectives, and historical and cognitive circumstances. However, few accepted Kuhn's radical relativist proposals. To many critics, philosophical analysis, history and science teach that observation is theory-laden, but they equally teach that experience and observation are underdetermined by theory—as attested by how often experience disappoints theories.

On the realist camp, Dudley Shapere (1964) traced Kuhn's relativism to a naive logical-deductive view of scientific rationality compounded by hasty sociologism. In this and subsequent works, Shapere grants central importance to the study of history, but his project (unlike Kuhn's) leans towards "reason-based" naturalist realism. One of Shapere's goals is to reveal how reasons develop in science during "revolutions" and how scientific objectivity is compatible with deep theory-change. Shapere (1980) connects Kuhn's relativism to certain philosophical prejudices common in the study of science, particularly the project of understanding epistemic gains in metaphysical terms separate from the ongoing epistemology of science. Instead, he recommends, the explication of concepts should do justice to their actual use in scientific practice, a recommendation Shapere has applied in detail to the study of how observation functions in science. For example, Shapere (1982) explicates astrophysicists' claims that they observe the Sun, spelling out how, in this and other cases, observation reports rest on background information and reasoning based on prior learning about the world. To Shapere, objectivity and rationality are inextricably bound with background beliefs licensed by science itself, but he argues that the quest for either is not thereby automatically vitiated (1984: 639). In science, he insists, metaphysics is not separate from epistemology—truth is tentatively attained when a scientific claim shows success by current standards and, for a reasonably long period, it remains free of specific reasons for doubting it (scientific, as opposed to "metaphysical" doubts based on mere logical possibility).

According to Shapere, looking at science in this naturalistic way enables us to understand what scientists count as their body of background truths on which current considerations reasons rest; and how reasoning enables science to progress epistemically. From this perspective, the objectivity and rationality of today's science are rooted, not in the "pure given" imagined by strong empiricists, but inextricably bound with background beliefs, drawn increasingly from modern science, a mode of knowing that, in Shapere's view, has epistemically upgraded forms of learning received from evolution and pre-scientific life. For Shapere's approach to support realism, however, some hurdles must be dealt with first. One is that the most empirically successful contemporary theories, far from being "free of specific doubts," display significant conceptual tensions and mutual incompatibilities.

Realists who try to keep metaphysics and epistemology separate follow a different path. Typically, they explain empirical success in terms of the approximate (non-epistemologized) truth of successful theories. Boyd (1984) follows this approach and presents realism as an inference to the best explanation, according to which empirical theories grow in success because they become more and more approximately true. This is a naturalist perspective in which epistemological and methodological hypotheses stand as empirical theories and must be evaluated accordingly. In Boyd's view, all the alternatives to realism fare worse. Empiricism and social constructivism, he argues, fail to explain the predictive success achieved by scientific theories. Critics, however, ask how truth can be the best explanation of scientific success—and even if it were, why we should expect this explanation to be true just because it is the best available or foreseeable explanation of science's success. These worries have complementary presentations in projects led by Bas van Fraassen and Larry Laudan, to which we must turn.

4. Van Fraassen's Objectivism

Van Fraassen's Constructive Empiricism too rejects Kuhn's radical anti-progressive view, but from an antirealist stance wary of noumenal explanations and committed to epistemological modesty. Theoretical explanations guide inquiry, van Fraassen admits, but he thinks they lack reliability. He recalls Newton's theory, in which the overall Universe has absolute velocity even though, by the theory's own lights, that feature is not an empirically accessible quantity. Constructive Empiricism hinges on arguments from underdetermination, logical modesty, and analogy.

Central to van Fraassen's constructivism are concerns about empirical underdetermination along two lines. One, promoted by Quine (1951), says that evidence in favor of a theory equally well supports indefinitely many other quite different theories. This general thesis, which many think poses a threat to realism and even the rationality of science, has never received cogent argument, except for trivial cases. Naturalists largely disallow it (e.g. Laudan and Leplin, 1991). Importantly, however, van Fraassen's leading exemplifications do not draw from Quine (1975)'s general line but from a second set of considerations, focused on *contingent* underdetermination, defeasible in principle. This second kind of underdetermination often gives way to rankings in terms of compatibility with current knowledge and scientific methodology; also, changes in findings about the relevant domain and technology may resolve a current state of underdetermination. These considerations mitigate the predicament, but they leave untouched some significant issues. Scientific criteria for theory choice and technological advances reduce the number of offers on the table at a given time, but rarely to just one.

Consider, for example, the multiple ontological interpretations that classical mechanics came to comprise (Jones, 1991). In one ontology, point-particles act upon each other at a distance; in another, action takes place only by contact; in a third ontology, the motion of bodies is determined by total trajectories between spatial points; in yet another, post-Einsteinian picture, space-time is both put at the center of the ontology and endowed with causal efficacy. What, then, might a realist about classical mechanics be realist *about*? Jones' paper invites a pessimistic general conclusion beyond the case of classical mechanics: ontologies, he suggests, fall prey to multiple empirically equivalent fundamental ontologies.

Jones' conclusion is a blow to traditional realist intuitions, but arguably not a fatal one. In the case of classical mechanics (and arguably all cases of interest), the multiple ontological frameworks on view converge dramatically at intermediate levels of descriptive depth, leaving no underdetermination regarding vast portions of the theoretical picture. Such seems to be the situation of classical mechanics at some regimes of energy, speed and accuracy, regarding for example noumenal descriptions of ensembles of particles, the phases of matter, the laws and "nature" of heat, fluids, pressure waves, and so forth; also of celestial orbits under regimes of low energy and gravity and their "laws;" and numerous sorts of histories too. Seemingly this comment generalizes well to other scientific domains[6]. The realist point is, then, that the effective underdetermination found in actual theories leaves a theoretical core suitable for realist interpretation. Scientists may have little prospect of articulating the "ultimate ontology of the world," but they already have a detailed, far reaching, highly textured—and credible—noumenal description of the natural world at various "intermediate" levels, well beyond those of ordinary, pre-theoretical descriptions, not to mention perceptual phenomena. Still, effective empirical underdetermination does compromise levels of theoretical description traditionally dear to realists—e.g. current physics suggests that we may never manage to decide whether physical reality is deterministic or indeterministic, or whether quantum "worlds" actually develop into worlds, among other questions.

Van Fraassen's empiricism is radical, however, advocating an agnostic stance towards *everything* a theory says about unobservables. A sensible aim for science, he thinks, is to produce theories that accurately describe the observable phenomena (empirical adequacy). This restriction seems bizarre and artificial to realists. Why should anyone believe that successful theories stop telling the truth at aspects of the world accessible to human perception? Again, however, while the empiricist boundary van Fraassen proposes seems unjustified, drawing a boundary is not, given the state of science. "Effective" underdetermination, while in place, sets limits to what naturalist realists can convincingly support. Also, more radical underdetermination may arguably constrain realism at some fundamental levels—for example concerning identity and individuality in quantum mechanics[7].

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[6] Elsewhere I consider the cases pertaining to quantum mechanics and the theories of light (Cordero 2001, 2011b, respectively).

[7] See Steven French and Decio Krause (2006). French's contribution to this volume proposes a radical ontic-structuralist response to this level of underdetermination.

Two other pillars of Constructive Empiricism are logical modesty and analogy. I'll comment on van Fraassen's appeal to analogy in section 7. On modesty, he repeatedly states that it is safer to believe less; but without a reasonable criterion this maxim lacks guidance at best—at worst its equilibrium point is skepticism, an option Van Fraassen has considered (Ladyman et al. 1997). Emphasis on empirical adequacy at the expense of realism might make sense if some vast credibility gap separated perception reports and inductions from science's most successful claims about noumenal posits. But no such vast gap seems "in sight." Constructive empiricists hope to argue that successful ampliative inferences invoking unobservables are systematically less reliable than ampliative inferences involving only observables. However, both varieties of inference are fallible, and many involving unobservables are among the most credible we seemingly have. Scientists, in particular, do not understand empiricist calls for caution about claims they regard as well established as anything—e.g. that ice is made of angular molecules, with hydrogen atoms at the tips and an oxygen atom at the vertex, forming an angle of 104.45 degrees. On the other hand, the grass-roots realism scientists generally advocate is often overoptimistic.

5. Naturalist Antirealism

To one influential naturalist, Larry Laudan, the reliability attained by the sciences is no indication that theories unveil what there is beyond the observable level (Laudan 1981, 1984a). The historical record, he notes, is littered with explanations that, having gained empirical warrant, subsequently got discarded in favor of dramatically different stories about the furniture of the world. Even theories with strong predictive power have had this fate, he stresses. Laudan's examples include the theories of caloric, phlogiston, and ether-based light, all of which—he notes—failed to refer the way truthful theories supposedly do. We thus get Laudan's "confutation" of the presumed realist link between empirical success and truth. His argument seizes on the epistemic instability of "deep" theoretical descriptions in science, which according to Laudan, shows that realism is both underdetermined by data and refuted by the history of science. In his view, science delivers growing instrumental and methodological reliability, but truth is not needed for realizing that feat. We have no reason to trust what contemporary theories say about deep reality, he warns.

Not everything in Laudan's work creates trouble for realism, however. One side of significance to naturalist realists is his defense of the objectivity of scientific values, norms and methods. Laudan turns scientific findings into epistemological considerations:

> "Although we appraise methodological rules by asking whether they conduce to cognitive ends (suggesting movement up the justificatory hierarchy) the factors that settle the question are often drawn from a lower level in the hierarchy, specifically from the level of factual inquiry. Factual information comes to play a role in the assessment of methodological claims precisely because we are continuously learning new things about the world and ourselves as observers of the world." (1984b: 38)

Scientists thus put forward methodological principles because they believe that following them promotes certain ends. According to Laudan, such principles should not be construed as categorical imperatives, but as hypothetical ones, i.e. imperatives whose antecedent clause is a statement about goals, and whose consequent describes some recommended action: "If one's goal is Y, then one ought to do X." That is, we make methodology out of empirical claims about the world, specifically claims about something instrumental in accomplishing identified goals. As such, methodological principles are about the empirical world, and appraising them is no different from testing empirical claims and theories. A methodological conditional found wanting invites revision the way a theory lacking empirical support invites revision. As with theories, scientists consider methodologies and choose between rival ones. Most importantly, there is no need of any meta-methodology in Laudan's approach. If a methodological rule amounts to a postulated assertion of covariance, then evidence for such covariance provides warrant for accepting the rule, while negative evidence provides warrant for rejecting it. If so, in methodological appraisals what matters is evidence regarding the ends-means connections embodied in rules. The provider of such evidence is history. To Laudan the goals of science are just those scientists embrace, which are open to change, variation across disciplines and even among individual scientists[8]. Goals that seem downright unavailable cannot be considered reasonable.

None of this softens Laudan's antirealism, however. In his works the clear cognitive gain of science is manipulation and control of natural systems. Why the acknowledged improvement in instrumental reliability cannot be related to gains with respect to truth, Laudan does not make clear. His antirealism connects with peculiar ideas about false theories, language, and approximate truth, views that realists regard with considerable suspicion.

6. Realists Strike Back

Jarrett Leplin, a sometime collaborator but also a critic of Laudan's work, thinks that only realist views account for the ability of theories to explain and predict successfully phenomena outside the scope of the empirical laws they were designed to cover. History, Leplin (1984) notes, is not opposed to realism any more than our experience of ordinary objects is unambiguously veridical. He agrees that whether there is conceptual continuity through revolutions remains a contentious matter, but draws attention to a valuable historical induction that favors realism:

> "…one historical pattern that has remained stable throughout scientific change is the tenacity of preferential judgments about theories. Although a theory that replaces another is in turn replaced, its superiority over its predecessor continues to be recognized. As much as history records sustained judgments of the ultimate unacceptability of theories, it records sustained judgments of their relative merits. […] Such judgments are not restricted to the pragmatic dimensions of predictive success, but include explanatory comparisons. Newton provided a better explanation of free fall than did Galileo although both explanations have been superseded. If we retain such judgments beyond the tenure of the theories themselves, we must regard one theory as having got more of the relevant facts right or as having described those facts more accurately, even if both theories are false. If the explanations proposed by both theories were rejected as utterly devoid of truth, such comparisons would be impossible." (1984: 214)

On this view, there are as good inductive grounds for concluding that scientific theories increase in truth as for concluding that all theories are false (in the sense of being not completely true). Critics, however, may object that even if this reply may succeed

[8] Laudan is vague about this particular, but his emphasis on instrumental success is clear. See Laudan (1984b, 1987).

against Laudan it does not respond to van Fraassen's objection, who can turn Leplin's induction into a more modest (and credible) claim about the tenacity of preferential judgments regarding the *empirical adequacy* of theories, a topic I consider in the next section. Leplin convincingly argues that Laudan's ideas distort the way scientists understand language and truth. Scientists point to significant bodies of theoretical claims in discarded theories that remain accepted either as true or "nearly so," Leplin notes. Maximally accurate description is rarely a goal in actual science, he stresses. Leplin thinks realist articulation of the notion of "nearly so" would benefit from shifting to a multi-valued logic closer to scientific practice. Most realists do not favor that sort of semantic revision, however. Still, realism needs an articulation that does justice to the scientific practice of presenting as true moderately abstract versions of many intermediate-level assertions from theories rich in novel predictions that then proved wrong as whole theoretical proposals. Approaches in that direction are the subject of sections 8 through 11.

Kuhnians, of course, deny coherence to all such realist moves. To most philosophers of science today, however, Kuhnian meaning holism constitutes a suspect stance, particularly when conjoined with the presumption that theoretical claims are epistemologically and semantically exhausted by their observable implications. For one thing, most theoretical terms enter science already endowed with meaning and knowledge (Shapere 1984, Devitt 1991)—i.e. science builds on and improves pre-scientific knowledge. Prior information and understanding are not "lost," Kuhnian style. As Alexander Rosenberg (2000) says, antirealists need to account for the apparent agreement about kinds and regularities characteristic of pre-scientific peoples, and the speed with which they shift to the exotic kinds and regularities proposed by modern science. Naturalist realists thus challenge Laudan's presumptions regarding language and truth, charging that linguistic holism makes no sense of human cognitive and linguistic practices, has no proper evidence on its behalf, its sole basis being metascientific prejudice about language and meaning.

Dismissing holistic objections paves the way for other realist resources, particularly concerning the identification of as yet unrefuted parts in discarded theories endowed with rich records of empirical success. Still, to constructive empiricists, moves to save theoretical progress and approximate truth lack *relevance*, given van Fraassen's persistent claim that theoretical explanations are not credible.

7. The Allure of Modesty and Analogy

To strong empiricists, confining the descriptive goal of science to empirical adequacy is both epistemologically safer than realism and sufficient for making sense of science. Critics disagree, however, pointing that warrant for explanatory inference naturally floods over van Fraassen's theory/observation dike, often reaching deeply into noumenal discourse. One favored constructivists response is that, since scientific explanations have to stop somewhere, they might just be stopped at the observable/unobservable boundary. This hardly convinces, however. That explanations must stop somewhere does not mean that they should stop just anywhere, let alone where strong empiricist prejudice dictates. If an epistemic ditch separates the claims of science, let science suggest where it lies[9]—

[9] Naturalists do not fully agree regarding the boundary's location. Shapere emphasizes success and freedom from specific doubt. Most entity-realists, settle for a generalized version of empirical adequacy, extended to

this alternative to raw modesty suggests a realist stance about numerous noumenal entities and structures (molecules and atoms, elementary particles all the way to at least quarks, microbiological structures, remote histories)—all beyond the range of unaided human observation, but with strong scientific warrant nonetheless.

Van Fraassen, however, points to fruitful moves away from noumenal explanations in modern theorizing. This brings us to an analogy he takes from biology. Like Kuhn before, Van Fraassen (1980) notices that, in Darwinian theory, natural selection operates on just phenotypical characteristics, a level he associates with the observable level for theories:

> "… the success of current scientific theories is no miracle. It is not even surprising to the scientific (Darwinian) mind. For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which in fact latched on to actual regularities in nature." (1980: 40)

Contrary to hasty critics, here van Fraassen is not "invoking theory" but just showing how scientific theorizing can fruitfully move away from noumenal accounts. Functionality in the living invites explaining organisms as products of intelligent design, but there is no need for such explanation, and even less truth in it—random variation and natural selection suffice. Likewise, suggests Van Fraassen, the instrumental reliability of science improves by ruthless theory selection in favor of empirical adequacy. However, realists cannot accept this.

Realists insightfully complain that van Fraassen is just explaining why we humans hold successful theories, not why those particular theories are successful (Lipton 1991: 170-172; Devitt 1991: 116). Furthermore the metaphor at hand is seriously faulty. As James R. Brown (1985) points out, successful new theories yield many more different correct predictions than simple guessing would allow. By contrast, Darwinian change is random and succeeds in only one or few respects at a time (the analogue of a new prediction). Philip Kitcher (1993) goes further and exposes the analogy as disingenuous at best. In his view, explaining the survival of theories by their predictive power is like explaining survival by appeal to fitness without then trying to explain fitness in any detail. According to Kitcher, any properly Darwinian account aims to include, at least (1) a list of current and past species that have endured; (2) specification of comparative fitness levels on the basis of endurance for each relevant species; (3) an explanation of attributed fitness by identifying fitness-making characteristics; and (4) a story about how organisms achieve these characteristics. Kitcher faults van Fraassen for arbitrarily stopping at the second stage. Explaining the instrumental reliability of scientific theories, Kitcher urges, requires looking for current and past theories, then determining which theories have been most accepted because of their explanatory and predictive success, followed by finding out what characteristics give these theories explanatory and predictive power, and then telling something about specific ways in which theories achieve these characteristics.

Reflecting on these reactions, Rosenberg (2000) suggests that constructive empiricists should simply refuse to answer the question of why science has succeeded, on

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encompass whatever entities become empirically accessible through scientifically-aided observation and manipulation, which now includes subatomic structures. Another alternative locates the boundary using as guide the most demanding conditions of acceptability found in scientific practice (Cordero 2012a).

the ground that no answer to it would improve the empirical adequacy of science. This does not seem right. Explaining success as a product of the truth of at least part of the story provided by the theory does seem to help empirical adequacy. While successful theories that proved wrong litter history, looking for noumenal truth in their accounts has helped scientists figure out the actual domain over which discarded theories are empirically adequate. And seeking to explain the scope and limits of the empirical adequacy of a successful theory in terms of deeper characteristics and relationships responsible for the encountered adequacy has ostensibly advanced science—from the study of gases, to chemical composition, to Mendelian genetics, and countless other cases.

I suggest there is a further complaint realists can advance against the noted use of natural selection. In biological evolution change meets the challenges of the environment by means of genomic incorporation of progressively deeper levels of information about the external world—from the shape and behavior of entities and processes relevant to survival to abstract structures concerning space, time, dynamical trajectories, light, chemical reactions, even rudiments of Euclidean geometry. Detailed research supports this claim[10], whose analogue in theorizing occurs in fields where theory-change involves manifest growth of retained descriptive schemes at increasingly deeper theoretical levels.

Considerations such as the above make realists regard Constructive Empiricism as an unpromising project of epistemic modesty. Less radical challenges to traditional realism are not so easy to dismiss, however—particularly the challenges posed by effective underdetermination and by the improbable truth of "whole theories," both of which force a request to clarify what realists can be realist *about*. In response, from the late1980s naturalist realists have shifted the link between success and truth to theory-parts (as opposed to whole theories), a strategy labeled "Divide and Conquer Realism" (DAC)[11]. DAC projects argue that empirically successful theories make epistemic gains in at least three ways: (1) they get right some significant clusters of theoretical descriptions of their intended objects; (2) what theories get right provides a substantial (if limited) account of relevant parts of the domains at hand; (3) the descriptive parts in question seem overwhelmingly likely to survive as putative truths in successor theories. To DAC realists, in short, discarded successful theories leave behind substantial noumenal descriptions that deserve to be called "true" as much as anything in science and ordinary life.

What counts as a "theory-part"? DAC literature says rather little about this, so here is my own naturalist line. There being no strong scientific case for freeing theories

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[10] A host of controlled experiments strongly suggest that animals make abstract representations of space. For example, the indigo bunting bird orients itself during migration using trigonometric relationships between stars (Emlen, 1969). Clark nutcracker birds store food throughout large areas during the fall season (in thousands of sites); experiments show that these birds manage to retrieve them by figuring out the mid-point between two landmarks (Kamil and Jones 1999). A neurological basis has been found for these abilities. In rats, the firing rate of individual cells in the hippocampus changes as the animal moves from one region of its environment to another. (O'Keefe and Dostrovsky 1971, Save et al. 1998, Lever et al. 2002); these "place cells" form a cognitive map. Experiments such as these indicate that place cells in the hippocampus of rodents and other animals, even blind ones (Save et al. 1998), give them an abstract level of geometrical representation of whatever environment they find themselves in. For a discussion of these developments see De Cruz (2007).

[11] Label introduced by Stathis Psillos (1999: 108).

of claims about “unobservables,” realists can turn strong empiricism on it head and exploit non-radical versions of its purgative strategies on behalf of DAC. In particular, instead of limiting epistemic commitment to Ramsey formulations of empirical theories—e.g. Sklar (2010)’s “thinning”— realists can (should) expand commitment along the lines of a generalized, more liberal version, now with topic-neutrality reaching into noumenal posits and structures sanctioned by the stringent tests of current methodology. Holists, of course, deny coherence to this realist move, but they do so from a highly suspect perspective, as noted in Section 6.

The remainder of this paper is devoted to the ups and downs of DAC moves within naturalism and realism. I begin with two influential perspectival articulations of the DAC approach.

8. Perspectival Realisms

At the heart of one group of DAC projects are the partiality and abstract character of scientific representation and description. This includes Ronald Giere’s approach, according to which scientific theories, like maps, are “perspectival” rather than absolutely “objective”—interests and conventions mediate the representations theories make, and as such they can be neither complete nor wholly accurate (Giere 1997, 2006a). They capture selected aspects of the world brought to prominence by prior knowledge, social circumstances, and biological evolution. Successful theories get many things wrong and their fundamental terms sometimes fail to refer, Giere admits, but he rejects Laudan’s antirealist argument, which, in his view,

> “…rests on the unstated assumption that approximation is always just a matter of degrees. If the ether does not exist, claims involving the ether cannot just be a little bit off—they encompass radical error. The argument collapses however if we abandon talk of approximate truth in favor of similarity between the model and the world which allows approximation to include respects as well as degrees of similarity […] Whether the ether exists or not, there are many respects in which electromagnetic radiation is like a disturbance in an ether. Ether theories are thus, in this sense, approximations.” (Giere 1988: 107)

To Giere, then, discovering that there is no ether justifies the rejection of ether models, but not the rejection of all realistically understood claims about similarities between ether models and the world. Ancient maps based on flat conceptions of the Earth still got a great deal right. In Giere’s view, there is room for a modest yet robust scientific realism that has scientists succeeding at least to some extent in their attempts to represent the world. His position ties in with field studies he has made of the ontological commitments of experimental nuclear physicists and their practices. To him, “the only remotely plausible, generally scientific account of what is going on at [cyclotron facilities is … that] nuclear physicists are producing protons with desired characteristics […] and then using them, together with other particles, to investigate the properties of various nuclei.” (1988: 125)

In Giere’s approach, scientific knowledge focuses on just some (out of indefinitely many possible) aspects of any given domain of interest. Like Mary B. Hesse (1974) before him, Giere tries to minimize concessions to constructivism by focusing representation on finite lists of respects of similarity invoked in scientific observation, language and scientific theorizing. He emphasizes “partial similarity” as the key relation that models bear to real systems. In his work the approximation relation has two dimensions: approximation in respects and approximation in degrees. Fresnel's descriptions are utterly wrong about the existence and structure of the ether, but—Giere adds—the theory is correct to a high degree about the transversal character of light waves, their reflection, refraction and polarization. Antirealists, however, have old objections to this move.

For starters, similarity is not easy to take seriously as a relation between models and the empirical world, because in some respect or other everything is similar to (also different from) everything else. Secondly, one needs a criterion to compare approximation in respects and approximation in degrees. Giere's answer is, again, strongly naturalistic (1988, 2006a, 2006b): Without the benefit of social conventions, he notes, animals discriminate exceedingly well among objects relevant to survival in their environment, a capacity humans share. Thus, for most perceptual judgments, widespread agreement does not require social explanation—the explanations of biology suffice. Complementarily, biological evolution has enabled animals to make and preserve internal maps of their surrounding empirical world, ones that bear useful similarities with that world (1988: 110; 2006b, Chapter 4). In humans, experience and reasoning allow us to generalize and revise received maps, thus far with considerable success in numerous areas. So, like many naturalists, Giere defuses radical constructivism by stressing the benefits of having a robust head-start on objectivity and truth, courtesy of natural selection.

There is a hindrance, however. As Rosenberg (2000: 16) objects, at most Darwinian biology makes it probable that the respects of similarity we have been selected to focus on had local survival value. Such gains may not increase overall similarity to reality, let alone absolute similarity. Just as local survival value led the blind mole to lose its sight, in science the winning theory may not be better in some respects of similarity to the external world. I think Giere can grant this objection by simply stressing that "mole maps" represent reliably the world *relevant to moles*. Applied to science, the realist expectation is not that a theory should represent reliably everything in the world but just a portion (significant for current purposes) of the theory's intended domain. The realist point is that science extends human epistemic reach beyond the biologically relevant, not that it does so "fully."

Giere's realism builds on a primeval quest for "perspectival" models. But, how such models arise, and how are they checked for "truthfulness"? Cognitive scientists discern several relevant evolutionary stages in organisms. To Daniel Dennett (1995), for example, slugs and other invertebrates learn by testing randomly generated actions in the external environment: favorable actions are reinforced and tend to be repeated, much as B.F. Skinner thought humans do. More in line with Karl Popper, mammals, birds, reptiles and fish (arguably also some invertebrates) have inner ("mental") environments that allow them to preview and then select among possible actions by filtering out the silliest options before risking them in their unforgiving world. As hominids developed, their inner environment became informed by tools and other designed portions of the outer environment (including word-like tools), possibly along the lines proposed by information theorist Richard Gregory (1984). These are just examples of the proposals naturalists make about the rise of modeling. Critics demand more, unsurprisingly. They want a convincing supplement about the processes used to select models and assert cognitive progress. On this too the preferred naturalist approach sticks to evolutionary theory.

9. Justification Matters

Reflecting on objectivity, truth, and justification, Philip Kitcher, also a perspectival realist and strong supporter of the map analogy, emphasizes the importance of the biological head-start. In his words, "perception contributes a theory-independent basis, inter-subjectively available" (1993: 66-67). From a public body of empirical information theories arise that unify and project that information, their justification resting on the elimination of proposals. However, for elimination to work, criteria for theory acceptance and rejection must be commonly shared across the theories involved. This is disputed by Kuhnians, who insist that fundamental theories encompass evaluation criteria that vary from one theory to another. As noted earlier, in the 1970s anti-relativists (notably Shapere) responded by showing that supposed paradigm-shifts in the history of science actually proceeded on the basis of shared reasons and evidence. Kitcher (1993) focuses on commonalities grounded in biology, particularly at the level of categorization and model-selection. We, he notes, have a primeval propensity, rooted in our genome and early human environment, to theorize under categorizations favored by diverse episodes of natural selection. This has resulted in mammals spontaneously focusing on certain kinds of things and relations and generalizing from few single cases henceforth. To Kitcher, "this primitive apparatus works tolerably well in confronting the problems our hominid ancestors encountered; it is relatively well designed for enabling primates with certain capacities and limitations to cope with savannah environment and with the complexities of primate society" (p. 241). It is with this apparatus in place, he maintains, that a deeply rooted eliminativist propensity we have gets activated. Other brain developments open the use of this propensity to modification of practice, revision of primitive categorizations and views of dependence, making it possible to figure out alternative views and subject them to eliminative induction. However, what justifies Kitcher's conviction that this eliminativist strategy advances theoretical knowledge?

Winning over just a finite number of competitors is not enough. The most such elimination can yield is confidence that a certain theory is the right one, *unless* some other proposal makes more sense. But realism requires that the theory selected advance theoretical knowledge in a stronger sense. Reasons must be provided other than the hypothesis' place in the current order of confirmational merit. The ether hypothesis long sustained the status of best confirmed hypothesis, yet—antirealists emphasize—it turned out to be quite false (there is no light ether). As Rosenberg (2000) warns, if eliminative induction is all there is, then realism commits science to a wasteful search for more and more alternative hypotheses beyond the needs of empirical adequacy. For only once all the possible hypotheses have been specified and compared with each other would the winner be "well-confirmed"—an impossible accomplishment, since the number of possible hypotheses is indefinitely large.

Elimination by itself fails to convince, but realists have available other resources to discern truth. A marker less mortgaged to the limitations of the human imagination is novel empirical success, which many DAC-realists take as their choice indicator (instead of explanatory virtue, e.g. unification). The DAC thesis, to repeat, is that, as theories change, the successful ones leave behind truthful descriptions that add to previous maps of the noumenal world. This harmonizes well with Leplin's already mentioned historical induction about preferential judgment (when a theory is replaced, its superiority over its predecessor continues to be recognized). However, which of the noumenal descriptions introduced by a successful theory are true? Theories, however successful, can be quite wrong about things. Retention across theory-change alone is not a good marker of truth, judging by the array of long lingering ideas that have turned out badly (caloric, fire/phlogiston, teleological holism in biology, the ether of light, the Euclidean conception of space and time through Lorentz). Specifying which parts a successful theory gets approximately right is trickier than might seem at first. Realists have several ways to go. In the remaining two sections of this article I discuss some ongoing moves, including my own.

10. Theory-Parts Suitable for Realism

One prominent DAC approach, variously developed by Kitcher, Leplin, and Psillos[12], focuses on theory-parts that scientists can identify at the time of a theory's success. On this view, components really off the mark are generally not implicated in the historical predictions of a theory, and one can tell whether a part is "not implicated" simply by checking the theoretical options available at the time ("synchronic" DAC). Fresnel—defenders of this approach claim—could have derived his famous predictions using available Lagrangian derivations that bypassed the ether of light. This, they argue, shows that the ether posit was "idle," "dispensable" or worse.

There is a serious problem with this thesis, however: mature science introduces hardly any idle posits. The ether was not a dispensable posit, and could not have been so taken until at least the early 20th century (see the Introduction Chapter). This is due, among other reasons, to "metaphysical entrapment" (Cordero 2011a, 2011b): 19th century theoreticians could not give up the ether because, at the time, the whole of physics, including the Lagrangian approach, conceived of waves as *propagating perturbations* and thus as a *mode of being* in need of a substratum. Here is an illustration of the confidence the ether commanded:

> "You can imagine particles of something, the thing whose motion constitutes light. This thing we call the luminiferous ether. *That is the only substance we are confident of in dynamics*. One thing we are sure of, and that is the reality and substantiality of the luminiferous ether." [Lord Kelvin: "The Wave Theory of Light," Johns Hopkins Lectures, 1884. (My italics)]

How damaging this problem with synchronic approaches is to other versions of DAC realism depends on whether realists can produce a criterion that, while more purgative of metaphysical entrapment, manages to identify with high probability theory-parts that yield approximately correct noumenal descriptions of the intended domain, and does this without resorting to mere retrospective projection of current theory.

A prime place to go for naturalist hints is, again, scientific practice. Scientists use strategies that ostensibly enhance the credibility of theory-parts. Promising DAC moves on view include several strategies that take time to bear fruit ("diachronic"), four in particular (Cordero 2012a). Two are close to Whewell's recommendation (1858/1968:

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[12] Kitcher (1993, 2001), Leplin(1997), Psillos (1999). Psillos' DAC started partly in response to John Worrall's austere Structural Realism

109) of having theories "turned in all directions, examined on all sides; the strength and the weakness of the maxims which men apply to them are fully tested; the light of the brightest minds is diffused to other minds," only now applied to theory-parts in Giere's sense rather than full theories.

> (S1) Hostile Probing: This comprises moves that try to do without the central tenets of a theory, particularly by opponents reacting to a theory's initial success. For example, 19th century corpuscularians responded to the wave theory of light by laboring to show Fresnel's central tenets wrong, most famously in the episode that led to the experimental demonstration of the so-called "Poisson Spot." Poisson and other corpuscularians thought this prediction was to be the downfall of Fresnel's theory; to their surprise it crowned it.

> (S2) Checking Auxiliary Assumptions: Supporters of a theory follow this strategy when difficult cases come their way, as when, in 1801, the discovery of double-slit interference corpuscularians into convoluted auxiliary hypotheses to account for the phenomenon. Their efforts failed to satisfy, leading to the effective collapse of the particle camp.

As a theory plays out, the above strategies give salience to two kinds of components. At one extreme are theory-parts systematically implicated in the derivations of either failed predictions or conceptual conundrums. These are deemed dubious, as in the example for S2. At the opposite extreme are parts that seemingly cannot be removed without bringing the theory to stagnation, suggesting that they are indispensable and very probably truth-worthy, as in the example for S1.
A third DAC strategy comes on view when successful theories begin to wane:

> (S3) Efforts to Identify Adequacy Conditions for Successor Theories: These occur when a theory faces persistent difficulties and scientists start looking for alternatives. From the yields of S1 and S2 they select theory-parts found particularly trustworthy and place them as correspondence rules, limiting cases, and so forth. At the start of quantum optics, for example, Maxwell's laws functioned as adequacy conditions at certain levels of quantum mechanical representation.

A glance at other scientific episodes suggests that the above strategies generalize well. In numerous disciplines they have advanced explanatory coherence and broadened the evidence base of theory-parts over time. On the whole, the strategies have a good track record of picking theory-parts accepted as approximately correct to this day (e.g. geometric properties of light waves, kinetic theory of matter, classical chemical structures, conservation principles, to name a few). Nevertheless, the specter of metaphysical entrapment remains a worry. In some notorious cases (e.g. the ether case), the three strategies passed "chaff as wheat," strengthening and propagating received metaphysical entrapment into the next generation of theories. Application of the strategies did eventually expose the ether and other such posits as fiction, but stowaways can be hard to catch. Happily scientific practice has other resources, in particular two strategies that center on elucidation. One focuses on *external* explanatory support, specifically of theory-parts that gain explanatory elucidation from independent successful theories:

> (S4) External Explanatory Elucidation occurs when claims merely assumed in a theory T subsequently gain justification from another, initially unrelated theory T*. For example, since the 1950s numerous aspects of cell biology have gained elucidation from molecular biochemistry—e.g. posited neural mechanisms have been explained by noting that neurons consist of proteins and other molecules that are organized into functional sub-systems such as the nucleus, mitochondria, axons, dendrites, and synapses. In Paul Thagard's version of this strategy (2000, 2007), the emphasis is on *explanation*: if a theory not only maximizes explanatory coherence, but also broadens its evidence base over time and its assumptions are elucidated by explanations of why the theory's proposed mechanism works, then we can reasonably conclude that the theory is at least approximately true. Elucidation has accompanied much of the advance of modern theoretical science. But how good a marker of probable truth is it?

In an elucidation instance, the part that gets singled out is the derived structure along with whatever accompanying assumptions both theories clearly share, but nothing else. Thus, because in the 19^{th} century Lagrangian theory and mechanics agreed on the metaphysics of waves, Lagragian elucidations of Fresnel's predictions could not expose the ether as a "dispensable posit." By the same token, elucidations from cognitive psychology about unconscious levels of the mind do not boost Freud's theory holistically—just those structures and claims the two theories clearly share. In an elucidation instance, then, what typically gains epistemic weight is a level of description more abstract than T and T* provide for.

Since external explanatory elucidation springs from an independently supported theory T*, elucidation raises the credibility of the assumptions and narratives it casts light on—hence its relevance for realists. Moreover, elucidation's purgative power against metaphysical entrapment is greater than that of the previous strategies in cases where the initial remoteness of T* lowers the likelihood of shared metaphysical underpinnings. Although clearly a realist resource, elucidation seems neither necessary nor sufficient for realism, however. As the previous strategies suggest, ascriptions of probable truth can be made on the basis of success with novel predictions alone. On the other hand, unsavory counterexamples give pause to granting high probability to elucidated theory-parts. Here are two examples (Cordero 2012a). When Kepler looked for theoretical support for his 2nd Law, he derived it from the Aristotelian laws of motion and some principles of optimal action. Kepler elucidated the law, but the premises he invoked included some of the wrongest claims of Aristotelian physics. S4 can be improved by requiring the elucidating theory to be successful in terms of novel predictions, but this too fails to filter out some lamentable cases. In the 1940s and 1950s, Freudians allegedly grounded some of their principles (e.g. the "death instinct") in thermodynamics; they did not convince.

The final strategy I consider focuses on "post mortem" elucidation of the successes of superseded theories:

(S5) Retrospective Elucidation: These are efforts to explain why a discarded theory showed empirical success. The task here is to provide causal and/or structural justification for a theory's accomplishments, as in the account wave theorists provided for the success of corpuscularian optics regarding the phenomena of reflection, refraction and polarization.

Contrary to first appearances, S5 does not involve "vicious post-hoc maneuvers." Explaining the success of a theory T0 from the vantage point of a successor T1 often contributes epistemic gains along two complementary lines. One is a greater awareness of divergences between T0 and T1, which typically leads to further novel predictions from T1, and thus to epistemic gains along S1and S2. Another bonus is the unveiling of regions within the logical space of T0 where noumenal descriptions licensed by T0 are approximately correct (from the vantage point of T1), which usually helps to grade the scope and accuracy of T0-parts relative toT1. Yet a third contribution has to do with the expectation (now widely shared by realists) that "whole theories" are generally false. By finding truth-content in a discarded theory S5 retrospective elucidations enhance the coherence of taking a realist stance.

The five strategies just outlined pick structures seemingly indispensable for a theory's success, including noumenal descriptions of various levels of abstraction. Their combined filtering does seem to raise to high levels the trustworthiness of numerous parts of the current scientific picture. Note, however, that none of the contributions associated with the strategies is either guaranteed or trivial. Their status is that of learned, empirical findings. Note also that, historically, preservation of noumenal posits and structures from early theories began in earnest only when novel predictive power gained recognition as an epistemic virtue. Transitions from one Ptolemaic theory to another displayed virtually no common theoretical parts (cycles, epicycles and such changing dramatically from one proposal to another), shared descriptions limited to the observable level. Even Descartes' Vortices hypothesis displays similarly poor theoretical convergence with Newton's gravitational theory.

By itself each of the strategies fails to satisfy. Together, however, they seem to do the required job, or so I suggest. The final section outlines my proposal of a naturalist criterion that pulls together the strengths of the diachronic strategies reviewed in this section.

11. A Proposal

The following suggestions draw from Cordero (2001, 2011a, 2011b, 2012a, 2012b). They focus on theories designed with one set of data in mind that then, unexpectedly and improbably (relative to prior information), predict some phenomenon unknown to the theory's author. As a theory T is applied to diverse situations and strategies S1 through S5 act on it, clusters of noumenal descriptions first licensed by T (theory-parts) gain salience as either (1) probably false or (2) probably true. Salience becomes noticeable as derivations of predictions (successful or failed) and paradoxes from T intersect at particular theory-components and auxiliary assumptions (Balashov 1994).

(R1) Refutational DAC: A given theory-part will reveal itself as "doubtful" if derivations of failed predictions in various areas intersect at that theory-part, and saving that part is consistently accompanied by degeneration of the whole system (as measured by current epistemological criteria).

At the other extreme, input from S1 through S5 will favor a part when it is either (a) implicated in the theory's novel predictive success to the point that removing or changing it leads to empirical degeneration; or (b) the part has gained elucidation from some independently well-established theory. The prolonged retention of posits like the ether warns against (a); against (b) stand counterexamples like those outlined for S4.Conjoining (a) and (b) thus seems a better option to try.

(R2) Corroborational DAC: A theory-part will reveal itself as "very probably approximately-true" if conditions (a) *and* (b) above obtain. Note that neither (a) nor (b) proceed by retrospective projection. Indeed both conditions can apply to parts while theories are still in full flight. Cases like those of the caloric and the 19th century ether did not meet this criterion. A perspectival DAC stance follows accordingly:

(R3) Naturalist Realist Thesis: Law-like structures and tokens of the noumenal types invoked by a theory-part that meets R2 obtain objectively, independently of any minds and (approximately) follow the laws that the part in question and other successful science spell out for them.

By the proposed criterion, in several significant respects, light is as Fresnel said, and atoms are as classical physics portrayed them; material transformations are to an significant extent as pre-quantum chemistry said; the evolution of many species is largely as Darwin's original proposal stated. In terms of R2, the epistemic yields of the last 250 years amount to arrays of thickly textured descriptions and narratives about numerous noumenal aspects of reality. The result is a world picture composed of "objects" that develop at various empirical levels and ranges, each of the objects displaying invariant property clusters and entering into law-like relations at their respective level[13]. Included in this picture are entities, origins and evolutionary histories, rich portions of Newtonian and gravitational theory, pre-quantum chemistry, early quantum mechanics, and Darwin's original theory, among other features.

The suggested DAC realist stance is a fallible, empirical conjecture, subject to scientific standards of acceptance and rejection. More work is needed on the matters involved, of course, but I think the above proposal shows promise. For one thing, while everything in the scientific picture remains defeasible, arguably (paraphrasing Whewell) *no example can be pointed out, in the whole history of science, in which filtering of the noted kinds together with explanatory elucidation has given testimony in favor of a theory-component afterwards discovered to be false*.

I end with some clarifications.

(A) This proposed version of diachronic DAC-realism lowers the risk of metaphysical entrapment by emphasizing external support and granting higher epistemic

[13] See David Bohm (1957, Ch. 5). Anjan Chakravartti (2007) offers a properties-oriented structuralist version.

value to corroborated novel prediction than to consistency and unity. It counteracts a widespread tendency to smuggle in prior information in theorizing; it does this by requiring that qualifying novel predictions be unknown to the theory's author(s), in the sense recommended by Worrall (1989).

(B) The sense in which truth applies to theory-parts piggy-backs on the ongoing use of truth in experimental science and ordinary life. It builds on explications of truth ascription to restricted, coarse-grained theoretical claims in scientific practice (Cordero, 2012b), but otherwise leaves deeper theorizing about "Truth" to others. A similar caveat applies to justification.

(C) Justification for the criteria and thesis advocated above is primarily scientific and rests on considerations of coherence, agreement with data, and risky predictions. As such, the proposal will fail if the predictions it makes fail to come out true almost without exception for theories that thrive in terms of corroborated novel predictions, in particular the following:

> (C1) After a successful empirical theory is replaced, its superiority over its predecessor will continue to be recognized (Leplin's Induction).
>
> (C2) For any theory-part that passes R2, restricted versions of some models original to it (a) will be embeddable in coarse-grained models of successor theories, and (b) the resulting restricted descriptions will be deemed true from the vantage point of those successor theories. An exemplar case is the embeddability of restricted Galilean models and associated descriptions of free fall on coarse-grained Newtonian models and descriptions.

Of course, even if the suggested proposal succeeds as a scientific claim, to some non-realists it will seem philosophically naïve and misguided from the start. For what, they will ask, justifies the scientific methods to which it appeals? I think Ernest Nagel's response to this line of complaint remains strong. The objection matters, he noted at the dawn of contemporary naturalism, only to those who "refuse to dignify anything as genuine knowledge unless it is demonstrated from self luminous and self evident premises" (1956: 15). But there is no such thing as complete justification for any claim, and so requiring complete warrant for naturalist proposals is an unreasonable request. The proper guideline for naturalist realists is thus clear: develop naturalism and realism using the methods of science; if this leads to a fruitful stance, then explicate and reassess. The resulting proposal will exhibit *virtuous circularity* if its explanatory feedback loop involves critical reassessment as the explanations it encompasses play out.

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