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The Fundamental Naturalistic Impulse: Extending the Reach of Methodological Naturalism

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THE FUNDAMENTAL NATURALISTIC IMPULSE: EXTENDING THE REACH OF METHODOLOGICAL NATURALISM

by

JAMES BRADLEY SUMMERS

Under the Direction of Jessica Berry

ABSTRACT

While naturalistic theories have come to dominate the philosophical landscape, there is still little consensus on what “naturalism” means. I trace the origins of contemporary naturalism to a view, called the “fundamental naturalistic impulse,” that originates in Quine’s turn against Carnap and which I take to be necessary for naturalism. In light of this impulse, some “substantively naturalistic” theories are examined: a weak version of non-supernaturalism, Railton’s a posteriori reduction of moral terms, and “Canberra plan” conceptual analyses of moral property terms. I suggest that if we take the fundamental naturalistic impulse seriously, then there is no need to differentiate substantive versions of naturalism over and above methodological versions. Substantive thesis in ontology or semantics can be had

on account of one's methodological commitments. This not only cuts against the distinction between methodological and substantive naturalisms, but also demonstrates just how far method can reach.

INDEX WORDS: Naturalism, W.V. Quine, Rudolf Carnap, Peter Railton, Conceptual analysis, Frank Jackson, Meta ethics, Philosophical methodology

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JAMES BRADLEY SUMMERS

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1 INTRODUCTION

In closing “Naturalism and the A Priori” (2000), Penelope Maddy notes that “[a]ny discussion of naturalism these days is—overtly or covertly—an attempt to define the term” (114). The current paper is not an attempt at a precise definition, for I will not propose a set of individually necessary and jointly sufficient conditions for naturalism. Instead, a glance back at the beginnings of an important source of contemporary naturalism in the work of W. V. Quine will serve as a case study in which we can fix upon what I take to be an indispensable feature of naturalism: a demand for continuity between philosophy and the sciences. This approach takes a *one-level view*; philosophical and scientific inquiry takes place on a single level, namely the level occupied by the empirical sciences. As such, there is a marked skepticism about calls to some extra-scientific level of inquiry. Consider the following characterization from Maddy:

The naturalistic philosopher is the Neurathian sailor, working within science to understand, clarify, and improve science; she will treat philosophical questions on a par with other scientific questions, insofar as this is possible; faced with first philosophical demands—that is, questions and solutions that require extra-scientific methods—she will respond with befuddlement, for she knows no such methods. (2000: 108)

Clearly, methodological concerns are central to naturalistic philosophy, but method is not exhaustive of the naturalist’s efforts. Like the working scientist, the naturalist confronts the world with her methods in hand and seeks to refine and improve them. But the naturalist is like the working scientist in another respect: she also brings an inherited picture of the world to her inquiries as well. As Quine remarks:

The naturalistic philosopher begins his reasoning with the inherited world theory as a going concern. He tentatively believes all of it, but believes also that some unidentified portions are wrong. He tries to improve, clarify and understand the system from within. He is the busy sailor adrift on Neurath’s boat. (1975: 72)

Quine’s message here is that the naturalist begins her inquiry with a whole host of inherited beliefs; it is then her goal to test and refine her beliefs by applying the methods of the empirical sciences, the most

successful methods she knows. The figure of Neurath's boat suggests that, counter to some traditional philosophical views, we cannot start from scratch. Rather, our encounter with the world begins with a number of tentatively accepted positions: some methodological, some substantive.

Both substantive and methodological positions are planks in Neurath's boat; both are pieces of our inherited, best-going theory of the world. If our only successful methods of refining or replacing our beliefs are scientific methods, then so-called "metaphysical" beliefs—about what there really is, about word-world relations, about truth, etc.—will be, indeed must turn out to be, amenable to the methods and practice of the natural sciences.¹ This realization is the beginning of eroding the distinction between the methodological and the substantive. The naturalist is born into a particular world-view, replete with substantive positions; a pure methodological naturalism can only be had in the cloister.

In arguing for this conclusion—that naturalism is an approach to doing philosophy that contains both methodological and substantive commitments—it is first necessary to take a look at the history of naturalism in hopes of narrowing down the term a bit. The *locus classicus* of contemporary naturalism is found in the work of Quine, especially in his reaction to the views of his teacher and mentor Rudolph Carnap. The first section of this essay will recount their debate and note that Quine's naturalistic turn against Carnap is fundamentally characterized by the repudiation of any other level of inquiry apart from the level of the empirical sciences. Following Maddy (2001), I will call this view the *fundamental naturalistic impulse*.

Having fixed a core feature of naturalism, in section II I will turn to a popular contemporary taxonomy of philosophical naturalisms prominently represented by Peter Railton and Brian Leiter. According to this view, methodological and substantive naturalistic positions are distinct: one can be a metho-

¹ I do not intend this remark to be positivistic in character, such that purported "metaphysical" questions like "Is everything physical?" are translatable into a sense-data vocabulary. The foregoing remark is meant instead to highlight the one-level view of the naturalist. Purported metaphysical questions are *internal* to the sciences; they do not fall outside of the grasp of scientific methodology. For instance, we will see in section III that the naturalist under consideration treats ontological theses as high-level empirical hypotheses, thus they are adjudicated by appeal to theoretical criteria.

dological naturalist while abstaining from any substantive views, or one can be a substantive naturalist while rejecting naturalistic methodology. In section III I show, *contra* Railton and Leiter, that acceptance of methodological naturalism in fact entails the acceptance of some substantive positions: first with respect to ontological naturalism (concerning what kind of entities exist) and then with respect to semantic naturalism (which concerns the relationships between vocabularies—here, between descriptive and ethical vocabularies). The general strategy in these sections is to show that, given the fundamental naturalistic impulse, ontological and semantic inquiries are carried out at the same level and by the same methods as empirical inquiries in the sciences. Therefore, no intelligible distinction can be drawn between methodological and substantive varieties of naturalism.

This conclusion is important for two reasons. First, it shows that methodology goes a long way. Fixing some methodological beliefs is quite often enough to fix some substantive beliefs as well. Second, it shows that calls by metaphysicians to an extra-empirical domain of inquiry are superfluous. Our Quinean naturalist does not need “serious metaphysics,” to sort out her substantive commitments.

2 CARNAP & QUINE: The Roots of Naturalism and the Fundamental Naturalistic Impulse

In Quine’s eyes², Carnap sought to construct the empirical world from sense data, and in so doing, to set science on its proper foundation. Thus, Quine sees Carnap as an heir to Russell: “Russell had talked of deriving the world from experience by logical construction. Carnap in his *Aufbau*, undertook the task in earnest” (Quine 1970: 41). It is through this lens that Quine saw Carnap engaged in a project of *radical reduction* of scientific theories, i.e., the project of translating terms or statements from ordinary scientific language as employed by working scientists to “sense-data language.” For Carnap this sense-data language consists only of logical notations, including higher-order set theory, and ordered

² For the purposes of this explication of Carnap’s work, I will limit myself mainly to Carnap-as-Quine-sees-him. The aim of this section is to fix a critical feature of Quine’s naturalistic turn away from Carnap, so any misunderstandings inherited from Quine will do no harm.

quadruples specifying point-instances of space-time ($x; y; z; t$) wherein certain qualities reside (Quine 1951a: 40). The project of radical reduction is then to show how, by translation, all of our meaningful empirical discourse can be reduced to statements of the sense-datum language.

Radical reduction is foundationalist in two distinct senses, and we can see Quine's critique of Carnap as a response to his foundationalism.³ In the first place, Carnap's *Aufbau* project is foundationalist in the sense that he sought to provide a foundation for the superstructure of empirical knowledge on the basis of immediate sensory experience. Certain beliefs are basic, such as "red here, now"; from these basic beliefs Carnap hopes to derive more familiar ones like "there is an apple in front of me." This aspect of radical reduction's foundationalism runs parallel to the foundationalism of the logicians' attempted reduction of mathematical knowledge to logical knowledge: for the logicians, logical truths were to form the basis on which the superstructure of mathematical knowledge could be erected; similarly, in Carnap's radical reduction, statements of the sense-data language form the foundation upon which the superstructure of empirical knowledge is built. Secondly, radical reduction is foundationalist because it adopts the stance that epistemology, as some extra-scientific theory of knowledge, is needed to establish the legitimacy of empirical science.⁴ Philosophy is therefore in the business of seeking some extra-scientific truths, of a different kind or special epistemological status that would ground the practice of the sciences.⁵ From this perspective, empirical science cannot justify its knowledge claims by its

³ See Haack (1993: 338). We might also speak of Carnap's foundationalism as a two-level view: that the empirical level of the sciences needs justification from some other domain of inquiry.

⁴ Carnap's radical reduction to a pure sense-data language evinces his commitment to a verificationist theory of meaning; see Quine (1954) & (1951). The diagnostic utility of such a theory is twofold: first, metaphysical debate in philosophy was decried as meaningless, for the propositions under discussion could not be shown to be subject to any possible verification; second, and more germane to the present discussion, verificationism and radical reduction provided both a vindicative and critical function to the sciences by showing that scientific discourse was meaningful or meaningless, respectively. For Carnap, then, an extra-scientific domain of inquiry (here the *Aufbau*) is needed to secure the path of the sciences, to keep its discourse meaningful and free of metaphysical deviation.

⁵ Radical reduction, as I take it, is primarily interested in purging the sciences of non-sense debates. Some sentences, even within scientific discourse, are devoid of content if they resist empirical verification in principle. Philosophy, on this view, is therefore needed to show that scientific enterprise is legitimate because the concepts it employs are contentful. Furthermore, reduction of scientific language to sense data language precisely specifies what sorts of experiences would count as evidence, and failing any specification of this kind, discourse that fails to specify how its conjectures might be verified should be abandoned.

own methods. Carnapian radical reduction is supposed to give to science what it cannot give to itself: a demonstration that its claims, so long as they are translatable into the sense-data language, are indeed justified.

Quine's critique of radical reduction is twofold. In the first place he faults Carnap for not adequately carrying out the task he set before himself. "*Der logische Aufbau der Welt*," Quine tells us,

does not give translational reduction.... The crucial point comes where Carnap is explaining how to assign sense qualities to positions in physical space and time. These assignments are to be made in such a way as to fulfill, as well as possible, certain desiderata which he states, and with growth of experience the assignments are to be revised to suit. This plan, however illuminating, does not offer any key to *translating* the sentences of science into terms of observation, logic, and set theory. (1969a: 76-7)

Carnap has failed to convince Quine that sentences containing physical concepts are adequately translatable into equivalent sentences comprised of only phenomenalistic vocabulary, that is, vocabulary that references immediate sensory experience. Showing how to assign sense-qualities to point-instances of space-time does not at the same time show how to reduce physical sentences to their counterparts in the sense-data language. In radical reduction "we want to establish the essential innocence of physical concepts, by showing them to be theoretically dispensable" (*ibid.*). By failing to provide adequate translation rules, Carnap has failed in this task.

But there is a deeper concern. The above criticism has shown merely that radical reduction had not been adequately performed (we might even suspect that, given Quine's admiration for Carnap, he may have felt that if the *Aufbau* had not managed to pull the project off, then likely nothing else would), but this criticism stops short of pointing out an in-principle difficulty with reduction. Famously, Quine gives us this kind of objection in "Two Dogmas of Empiricism" (1951a). The problem with reduction, we are told, is that it relies on the intelligibility of the distinction between statements that are analytic, or true in virtue of language, and statements that are synthetic, or true in virtue of the way the world is.

The central and innovative insight in Quine's "Two Dogmas" is supposed to be that both dogmas—analyticity and reduction—are the same. "The two dogmas" Quine tells us, "are, indeed, at root identic-

al" (1951a: 41): if the dogma of reduction holds that a sentence containing theoretical terms can be translated into a synonymous sentence of the sense-data language, then analyticity is simply a limiting case in which a translated sentence has no observable consequences whatsoever. In other words, analytic sentences are confirmed under any circumstances, they hold true "come what may" (ibid.). The contrasting synthetic sentences are confirmable only in the presence of certain kinds of experiences given by the sense-data translation. That is, synthetic sentences are confirmed by actual instances of their empirical content.

For Quine, the problem with this account of the analytic/synthetic distinction is that it makes no sense to speak of a sentence's "empirical content," and further that it is foolish to try to seek a boundary between sentences confirmable under any empirical circumstances and sentences confirmable by the presence of their empirical content.⁶ In the final section of "Two Dogmas" Quine forwards a picture of knowledge as "man-made fabric which impinges on experience only along the edges," in which "no particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium, affecting the field as a whole." An important consequence of this view is that *any* sentence can be confirmed under *any* empirical circumstances:

Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle? (1951a: 43)

⁶ The first four sections of "Two Dogmas" are principally concerned with showing that any plausible notion of analyticity is hopelessly circular; for example, analyticity might be defined in terms of synonymy, which is explicated with the help of analyticity (see Quine (1951a) pp. 20-32). It is interesting and perhaps worth noting that Quine does not give us a reason to think that circularity is in-itself pernicious. Indeed, some, like David Lewis, do not see circularity as the sticking point of Quine's critique: "Quine is right that there is a problem about analyticity, but the problem is not the circle" (2009: 219n4). However, even if Quine's appeals to circularity are not convincing, the arguments of the final two sections, recounted above, still go through.

If any kind of sentence is revisable, even paradigmatically analytic sentences like logical laws, then drawing a distinction between the analytic and the synthetic based on a sentence's immunity to empirical revision is untenable. Purported analytic sentences, Quine contends, cannot be different in kind from synthetic ones.

Quine's repudiation of the analytic-synthetic distinction is repudiation *a fortiori* of Carnap's radical reduction. Recall that that radical reduction suggests a way to specify, by translation, the empirical circumstances in which a sentence might be confirmed; but Quine's retort is that *any* sentence can be confirmed under *any* empirical circumstances. Therefore, it does not make sense to speak of a sentence's empirical content or to undertake a reduction project to uncover that empirical content.

Interestingly, we note that by 1951 (the year "Two Dogmas" was published) Carnap had long since abandoned the *Aufbau* project and had instead devoted himself to what he called the "logic of science." The significance of Quine's critique of the analytic-synthetic distinction not only cuts against reduction, but also against another project Carnap had "undertook in earnest"—*rational reconstruction*.

We saw earlier that logicism influenced Carnap in his project of radical reduction. We will now look at two other influences on this later project of rational reconstruction.⁷ First, the axiomatization of geometry by Hilbert provided a model in which any axiom set is true only in virtue of its consistency. For example, in non-Euclidean geometries, variations of the "parallel postulate" relate to different spaces with different properties (either hyperbolic or elliptical) that satisfy the axioms. The axiomatization of geometry demonstrated to Carnap that axioms themselves do not pick out any specific subject matter, rather their subject matter is simply whatever set of objects make them come out true. Axioms are *con-*

⁷ For an account of the influences on rational reconstruction, I am following Demopoulos (2007).

stitutive of their subject matter. Thus, we can know that some truths (e.g., geometrical truths) can be known for certain a priori.⁸

Secondly, Einstein's theory of special relativity demonstrated to Carnap that certain principles that appear in physical theories are not descriptive, as they might seem to be. Rather, some *prescriptive* principles are needed for the empirical interpretation of the language of physics. For example, Einstein postulated that "any ray of light moves in the stationary system of co-ordinates with the determined velocity c , whether the ray be emitted by a stationary or by a moving body" (this is the second numbered principle in §2 of "On the Electrodynamics of Moving Bodies" (1905)). Even if this postulate is in fact empirically confirmed, it nevertheless occupies a special status for Carnap because the postulate is crucial to the interpretation of various other physical phenomena like simultaneity or time dilation and length-contraction and very high velocities.

With this background in mind, rational reconstruction is viewed as a project that seeks to differentiate clearly the conventional and factual components of language, particularly the language of scientific theories.⁹ For Carnap, rational reconstruction is carried out as follows:¹⁰

Sentences of a language (say of physics) are sorted into *theoretical* and *observational* sentences (T- and O-sentences for short). Each kind of sentence is identifiable according to its primitive vocabulary, i.e., whether the predicates involved are about observable events or about unobservable events. A sentence containing only observational predicates is an O-sentence; a sentence containing only theoretical predicates is a T-sentence. Sentences containing both O- and T-predicates are called *correspon-*

⁸ We here see an indication that the Carnap-as-an-heir-to-Russell reading starts to break down. The Carnap of rational reconstruction of scientific language appears more like Kant. Both are engaged in projects to isolate the constitutive elements of knowledge. See Maddy (2007 I.5).

⁹ This does *not* mean that rational reconstruction settles so-called *external question*, i.e., questions that are posed independently of a language framework. Instead, radical reconstruction is a method for distinguishing factual and conventional components, *given a language framework*.

¹⁰ The following account of rational reconstruction owes much to Demopoulos (2007).

dence rules (or C-rules). These sentences serve as bridging principles linking O- and T-sentences together.¹¹

A theory is then written as a list of the T-sentences and C-Rules (**TC**):

$$\mathbf{TC}(O_1, \dots, O_n; T_1, \dots, T_n)$$

where O_1, \dots, O_n and T_1, \dots, T_n list the observational and theoretical predicates used in **TC**.¹²

Next, we can replace the theoretical predicates in **TC** by existentially quantified variables, written as

$$\exists X_1, \dots, \exists X_n \mathbf{TC}(O_1, \dots, O_n; X_1, \dots, X_n)$$

This is known as a *Ramsey sentence* **TC** and says that there is an n-tuple that plays the same role in the T-sentences and C-rules as the theoretical predicates. Importantly, Ramsey sentences eliminate the theoretical terms of a theory while implying the same observational consequences; i.e., all the same observational consequences will result whether or not all the theoretical terms in the list **TC** have been substituted by variables.¹³

The final phase of rational reconstruction specifies the related *Carnap sentence*, which has the Ramsey sentence of **TC** as its antecedent and the theoretical postulates and C-rules as its consequent. It is written as:

$$\exists X_1, \dots, \exists X_n \mathbf{TC}(O_1, \dots, O_n; X_1, \dots, X_n) \supset \mathbf{TC}(O_1, \dots, O_n; T_1, \dots, T_n)$$

The Carnap sentences says that if *any* predicates X_1, \dots, X_n satisfy **TC** , then the predicates specified by the theory (T_1, \dots, T_m) do.

¹¹ For example take the language of physics and the Rutherford experiment. Some statement concern only observable events such as “The zinc sulfide sheet was illuminated;” other sentences concern unobservable events like “The alpha particle collided with the nucleus;” further, some sentences contain both vocabularies: “When the zinc sulfide sheet is illuminated and such-and-such a location, the alpha particle collided with the nucleus.” These are examples of O-sentences, T-sentences, and C-rules, respectively.

¹² The typographical convention used here is intended to make the terms used in a theory perspicuous. “**TC**” represents a list of sentences; the parenthetical material lists the predicates used in **TC**.

¹³ Again, as with radical reduction, Carnap is trying to “establish the essential innocence of physical concepts, by showing them to be theoretically dispensable” (Quine 1969a: 76).

The point of this whole exercise is to differentiate clearly the conventional and factual components of a theory. The factual component of a theory is represented by its Ramsey sentences, since the Ramsey sentences of a theory imply the same observations as the theory itself. The Carnap sentences are supposed to represent the conventional or analytic portions of the theory, since they are observationally uninformative (i.e., all of the O-sentences it implies have the status of logical truths). The analytic components of a theory are akin to Hilbertian axioms or Einsteinian prescriptive principles because they are constitutive of the subject matter of the theory. Because the analytic truths are responsible for the interpretation of other sentences of a language, there are at least some truths that can be known a priori.

For Carnap, the distinction between the conventional and factual components of a theory is central to resolving disputes—or rather, for eliminating pseudo-questions—in science and philosophy. If we encounter disagreement concerning the conventional, analytic sentences of a language, for instance if there is disagreement over the proposition “space-time is a continuous manifold,” we do not find ourselves engaged in serious philosophical or scientific debate, rather we find two parties speaking entirely different languages at cross-purposes. This kind of debate attempts to settle *external questions*, apart from any linguistic framework. Without the requisite interpretive apparatus—i.e., the conventional aspects of a language that are provided *internal* to a theory—such questions are not questions of empirical fact as they purport to be, but are instead merely questions of convention. As Carnap puts it:

If someone wishes to speak in his language about a new kind of entities, he has to introduce a system of new ways of speaking, subject to new rules; we shall call this procedure the construction of a *framework* for the new entities in question. And now we must distinguish two kinds of existence questions: first questions of the existence of certain entities of the new kind *within the framework*; we call them internal questions; and second, questions concerning the existence or the reality of *the framework itself*, called *external questions*....Those who raise the question of the reality of the [framework] itself have perhaps in mind not a theoretical question as their formulation seems to suggest, but rather a practical question, a matter of practical decision concerning the structure of our language. We have to make the choice whether or not to accept and use the forms of expression for the framework in question. (1950: 242)

The situation is like Euclidean and non-Euclidean geometers debating the parallel postulate—they are not seriously considering any substantive positions but are instead championing their respective conventional, pragmatically chosen axioms against each other. They are, literally, speaking different languages. Similarly, debates over the continuity of space-time, perhaps between relativistic and quantum physicists, are not as they appear. What is actually up for discussion is the pragmatic decision to accept or deny a new language; what masquerades as a change of theory is in fact a change of linguistic framework. Rational reconstruction in the “logic of science” is intended to diagnose this kind of pseudo-debate, where pragmatic questions for which there is no matter of fact are unfortunately mistaken for questions of fact, squandering the efforts of scientist and philosopher alike.

Thus, for Carnap the “logic of science” tells us which questions are decided conventionally, external to any particular linguistic framework (e.g., the language of special relativity). I noted earlier that Quine’s repudiation of the analytic/synthetic distinction cuts equally against Carnap’s convention/fact distinction. For a view to this, we turn to Quine himself:

No more than the distinction between *analytic* and *synthetic* is needed in support of Carnap’s doctrine that the statements commonly thought of as ontological, viz., statements such as ‘There are physical objects’, ‘There are classes’, ‘There are numbers’, are analytic or contradictory given the language. No more than the distinction between analytic and synthetic is needed in support of his doctrine that the statements commonly thought of as ontological are proper matters of contention only in the form of linguistic proposals. (1951b: 210)

Conventional components of language are identifiable for Carnap because they are analytic, i.e., they imply no observational consequences. Such sentences are therefore true or contradictory no matter how the world may be; they are true *come what may*. Quine denies that there are any sentences of this kind. Every sentence, even purported logical truths are open to revision based on some (however remote) confrontation with experience. For example, even the law of the excluded middle has come under fire as the results of quantum experiments have proven recalcitrant to our usual conceptual schemes. Denial of the analytic/synthetic distinction is simultaneously a denial of a sharp conven-

tion/fact dichotomy and a refusal to recognize the goals of rational reconstruction and the logic of science.

[I]f there is no proper distinction between analytic and synthetic, then no basis at all remains for the contrast which Carnap urges between ontological statements and empirical statements of existence. Ontological questions then end up on par with questions of natural science. (1951b: 211)

Rational reconstruction thus represented a distinct level of inquiry apart from the empirical sciences. From this vantage point, one could keep a watchful eye on the sciences, occasionally stepping in to point out that its methods are insufficient. Here the work of the philosopher (the logician of science) is necessary to keep science tethered to its proper task of deciding questions by experiment, and to not get caught up in matters of linguistic dispute. The trouble is not that scientists ought not engage in the pragmatic practice of adopting new frameworks (they surely ought to), but that science errs when it makes the mistake of treating linguistic conventions as empirical facts.

Quine denies that this separate level of inquiry is necessary or even intelligible. So-called 'external' questions, Quine thinks, are decided by exactly the same methods as so called 'internal' questions:

Our acceptance of an ontology is...similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged. Our ontology is determined once we have fixed upon the overall conceptual scheme which is to accommodate science in the broadest sense; and the considerations which determine a reasonable construction of any part of that conceptual scheme, for example, the biological or the physical part, are not different in kind from the considerations which determine a reasonable construction of the whole. (Quine 1948: 16-7)¹⁴

In other words, Quine thinks that the considerations that decide questions at Carnap's linguistic level are exactly those considerations that decide questions at the empirical, scientific level. Where Carnap sought a new level of enquiry distinct from natural science, Quine showed us that this level is really of a

¹⁴ See also, Maddy 2001: 42ff.

piece with routine scientific practice. Rational reconstruction is therefore completely dispensable; we need only one level, the level of inquiry of natural science:

But why all this creative reconstruction, all this make-believe? The stimulation of his sensory receptors is all the evidence anybody has had to go on, ultimately, in arriving at his picture of the world. Why not just see how this construction really proceeds? Why not settle for psychology? (Quine 1969a: 75)

Quine is here urging the abandonment of a two-level view. If we want to know how our mature sciences have delivered us a complicated and detailed picture of the world, what their standards of evidential support are, how they go about constructing and updating hypotheses, how they resolve disputes that seem to outstrip available evidence, how higher-level norms of explanation are introduced and developed, etc., we need not appeal to another level to find out; the sciences themselves furnish us with the tools for their understanding. This exhortation embodies the *fundamental naturalistic impulse*: “a resolute skepticism in the face of any ‘higher level’ of inquiry that purports to stand above the level of ordinary science,” as Penelope Maddy aptly puts it (2001: 39). It is this impulse that I take to be absolutely fundamental to naturalism. I intend the fundamental naturalistic impulse to constrain a definition of naturalism. We will see that, even though, this impulse is overtly methodological, its methodological component is not exhaustive; some substantive positions are had as a consequence of method.

3 THE RAILTON-LEITER VIEW OF NATURALISM

Some recent proponents of naturalism have asserted that it comes in two varieties: there are *methodological* and *substantive* naturalistic doctrines. Representatives of this view include Brian Leiter (1998) and (2002), and Peter Railton (1989).¹⁵ In its methodological mode, naturalism represents a demand that philosophical inquiry be continuous with empirical inquiries carried out by the sciences. To the methodological naturalist, there are not philosophical methods distinct in kind from scientific me-

¹⁵ For other examples of this distinction see Forrest (2000), Goldman (1999), and Papineau (2007). What Leiter calls “substantive naturalism” is referred to by others as “philosophical naturalism,” “metaphysical naturalism,” or “ontological naturalism.” However, the basic distinction is the same: we may differentiate between a purely methodological doctrine of naturalism and some other doctrines that outstrip these bare methodological concerns.

thods; while philosophy might differ from the sciences in the nature of its questions (say, by asking “fundamental” or “general” questions), it does not differ in its ways of going about answering those questions or in the kinds of answers it gives. On this view, more traditional philosophy falls into error in conducting itself as an armchair exercise in conceptual analysis,¹⁶ or in supposing that philosophical theories of knowledge must first be in place before we are entitled to regard empirical knowledge as justified. On this picture, philosophy is continuous with the empirical sciences.

Leiter, in *Nietzsche and Morality*, fixes the notion of “continuity” by appeal to the following conditions:

Results Continuity...requires that philosophical theories—e.g., theories of morality or knowledge—be supported or justified by the results of the sciences. (2002: 4)

Methods Continuity...demands only that philosophical theories emulate the “methods” of inquiry of successful sciences. (2002: 4)

Results continuity names the demand that naturalized philosophy must be tethered to the delivered results of the sciences. “[P]hilosophical theories that do not enjoy the support of our best science,” Leiter writes, “are simply *bad* theories” (2002: 4). For instance, a philosophical theory of mental phenomena in terms of spontaneously arising energies would violate the widely accepted conservation law of kinetic plus potential energy, a principle that has been employed to good effect in many areas of science. A naturalist bound by Results Continuity would have to reject this theory on the grounds that it stands in contradiction to well established scientific results.

¹⁶ This does *not* mean that analysis has no place in any naturalistic philosophy, but only that the information uncovered by such a method is trivial. As we will see, some versions of the “Canberra Plan” of analysis are acceptable to naturalists. Let’s say some contested theoretical term may be analyzed only in accepted terms of the vocabulary of mature sciences, and such an analysis is intended to set the stage for further philosophical enquiry. For instance, quoting from David Brandon-Mitchell, “in the classic case of philosophy of mind we might be able to specify the belief role in a Ramsey sentence [e.g., “that there is a unique kind of entity with such-and-such features”]; or alternatively, as a corresponding Carnap sentence: “if there is a unique kind of entity with such-and-such features, then it is belief], and then the only allowed move would be to see if there is something in our scientific ontology that plays that role (or near enough), in which case we have vindicated beliefs, or else we discover there is no such thing, in which case we have discovered that eliminativism about belief is true” (2009: 26). This kind of analysis would supposedly jibe with many naturalists because the definitive work on whether or not to accept or reject eliminativism about belief is perfectly consistent with empirical inquiry, even if the inquiry is framed by the so-called Ramsey sentence (for a view against the analyticity of Ramsey sentences, see Papineau (2007)).

Just as Results Continuity gives philosophical importance to the results of the sciences, Methods Continuity validates the judgments of scientists regarding what they take to be good methods of explanation, rather than calling for an independent philosophical account of explanation. Leiter remarks that “methods” ought to be broadly construed, “to encompass not only, say, the experimental method (e.g., the method of testing progressively refined claims against experience), but also the styles of explanation and understanding employed in the sciences” (2002: 4). For example, to at least some naturalists, like Hume and Nietzsche, a commitment to causal explanation is characteristic of the empirical sciences.¹⁷ In this case, Methods Continuity demands causal explanations in accounting for typically “philosophical” phenomena, such as belief, knowledge, morality, action, etc. For the methodological naturalist, “[j]ust as we often understand events in the inanimate world by identifying the natural causes that determined them, so, too, we understand human beliefs, values, and actions by locating their causal determinates in various features of human nature” (2002: 5).

Another feature of the methods of the empirical sciences that is transmitted to philosophy via the methods continuity condition, and one that will play a special role for us in determining the relationship between so-called methodological and substantive naturalism, is a criterion of indirect evidential support. Part of the scientific method (broadly construed) is that hypotheses can garner support indirectly, by enjoying a number of theoretical benefits. Consequently, rational choices between two theories that are equally supported by all known evidence can be made by appeal to how much indirect support is conferred.

¹⁷ To be clear, Leiter in no way intends this characterization to speak to the so-called “Demarcation Problem” between genuine science and pseudo-science—astrology for instance offers causal explanations. It is enough for him that “there are enough *clear* cases of science on which to draw for methodological guidance” (2002: 6n3). We should also keep in mind that contemporary naturalists might reject the idea that natural phenomena have determinate causes, in keeping with scientific developments in some interpretations of quantum theory. The point is not that Methods Continuity requires a commitment to determinate causal explanations full stop (although causal theories of scientific explanation seem best supported), but rather that philosophical methods are and should be adaptable to ever-evolving scientific styles of explanation and understanding.

In contrast to methodological naturalism, according to the Leiter-Railton view, naturalism can also be substantive. According to Railton,

[substantive naturalism]...is not in the first instance a view about philosophical methods, but about philosophical conclusions. A substantive naturalist advances a philosophical account of some domain of human language or practice that provides an interpretation of its central concepts in terms amenable to empirical inquiry. (1989: 156)

This speaks to what Leiter might call “semantic substantive naturalism.” He also mentions an ontological form of substantive naturalism: “the (ontological) view that the only things that exist are *natural* (or perhaps simply *physical*) things” (2002: 5). Historically, ontological naturalism has been the thesis that there are no supernatural entities, i.e., entities that somehow stand outside the natural world and cannot be understood or explained in the same way we understand events in nature. However, contemporary naturalists are often drawn to another, stronger version of ontological naturalism known as physicalism. Physicalism has been a notoriously under-defined term in contemporary philosophy. Some, like Crane and Mellnor (1990), suggest that there is no plausible formulation of the doctrine; others, like Pettit (1993) think physicalism is perfectly definable. For our purposes, we will just stick with Leiter’s characterization: physicalism is “the doctrine that only those properties picked out by the laws of the physical sciences are real” (2002: 6 & 1998: 84).

There is another feature to the Leiter-Railton view of naturalism besides the sharp division between methodological and substantive naturalism. In Leiter’s words, “it is important to notice that a commitment to [methodological] naturalism does *not* entail [substantive naturalism]...methodologically, it is an open question whether the best philosophical account of morality or mind or knowledge must be in substantively naturalistic terms” (2002: 6). We might call this thesis the “Non-Entailment Thesis.” This view states that there is no necessary relationship between methodological naturalism and its substantive variants.

4 METHODOLOGICAL NATURALISM AND SUBSTANTIVE ONTOLOGICAL NATURALISM

I think that the Non-Entailment Thesis is false. I believe that methodological naturalism plus an inherited world theory does in fact entail a number of substantive conclusions. In this section, we will see how the acceptance of widely-held views in the sciences concerning indirect evidential support invariably leads the methodological naturalist to embrace a substantive ontological position—namely, that there are no supernatural entities.

As we have seen, the fundamental naturalistic impulse gives expression to a number of related theses, including Quinean gradualism (the idea that there is no difference in kind between philosophical and scientific enquiry) and the related results and methods continuity conditions spelled out by Leiter. I have been urging that this idea is central to naturalism, and is a position that a Leiter-Railton methodological naturalist would surely accept (under the banner of continuity). Given that philosophical methods just are scientific methods, a naturalist seems rationally required to accept ontological naturalism if our current best-going scientific theories lend their support to the idea that there are no super-natural entities or that everything is ultimately physical. That is, if our best science gives reasons to prefer ontological naturalism, then the naturalist, *as a consequence of her methodological stance*, should accept ontological naturalism.

In Quine's debate with Carnap we saw that ontological questions are decidable in exactly the same way as scientific ones. Once we have fixed the conceptual scheme of our best-going, total picture of the world, we have thereby fixed our determinations as to what there is. The relevant passage is worth quoting again:

Our acceptance of an ontology is ... similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged. Our ontology is determined once we have fixed upon the overall conceptual scheme which is to accommodate science in the broadest sense; and the considerations which determine a reasonable construction of any part of that conceptual scheme, for example, the biological or the physical part, are not different in kind

from the considerations which determine a reasonable construction of the whole.
(Quine 1948: 16-7)

In this passage, we find an apt expression of the fundamental naturalistic impulse: this time in regard to questions that are typically thought of as extra-scientific *par excellence*. Ontological investigations, traditionally conceived, are carried out prior to the workings of the sciences themselves, as a piece of first philosophy, to shape and ground scientific practice. Quine, and the naturalists who follow him (including the Railton-Leiter methodological naturalists), see the traditional questions of metaphysics recast as scientific ones. What I think this means is that a commitment to naturalistic methodology *does* settle ontological questions, insofar as these commitments fix our over-all conceptual scheme.

The fundamental naturalistic impulse, and the methodological naturalism consistent with it, wipes out the distinction between philosophical methods and the empirical scientific methods. This has important consequences for the determination of our overall conceptual scheme. In the first place, this means that philosophers use the same standards of explanation, evidential support, and theory choice exercised in the empirical sciences. Important to our current consideration are the well-worn scientific ideas that (1) hypotheses can garner support *indirectly* through coherence with some set of theoretical virtues, and that (2) two hypotheses that are equally consistent with all available evidence can be differently supported by that evidence. (1) speaks to the existence of kinds of evidence other than direct observation, namely coherence with some set of theoretical virtues; (2) speaks to an appeal to those virtues to adjudicate decisions between hypotheses that accord equally well with observation. Since the fundamental naturalistic impulse places ontological questions on a plane with empirical ones, it seems that a methodological naturalist should accept ontological naturalism if there is evidence for doing so.

Quine, in "Posits and Reality," notes that molecular theory is supported by a convergence of *indirect* evidence (1955: 247ff.); we might expect the same kind of theoretical benefits to count in favor of ontological naturalism. Molecular theory enjoys a number of theoretical benefits that its competitors lack: Quine lists simplicity, familiarity of principle, scope, fecundity, and successful testable conse-

quences. It is on the basis of these virtues that physicists find support for the theory, even if it is, *ipso facto*, beyond all direct, observable confirmation.¹⁸ The theoretical benefits conferred on the molecular doctrine allowed for its acceptance in the case where more traditional, direct forms of evidence are unavailable. Ontological naturalism is similar in that it is a position somewhat sheltered from confrontation with experience, in the interior of the web of belief (to use a Quinean figure). It is a kind of high-level belief that, like other kinds of high-level hypotheses in the sciences, assimilates and organizes a whole body of seemingly unrelated and discordant phenomena. Thus the evidence for ontological naturalism will be very much like the evidence for the molecular theory—it will come only indirectly by virtue of its theoretical utility. Because the methodological naturalist recognizes the sanction of this kind of evidence, she will accept ontological naturalism too.

It will be useful to compare ontological naturalism with another universally accepted doctrine: the law of conservation of energy. The law of conservation of energy states that in all possible physical interactions the total quantity of kinetic plus potential energy remains constant. It is well confirmed that energy is conserved in certain cases. For instance, the total amount of energy is maintained in mechanical interactions between bodies; Joule's apparatus clearly demonstrated that the total amount of energy is conserved when gravitational potential energy is converted into kinetic energy. However, the conservation law is fully general: kinetic plus potential energy is conserved in *any possible* type of physical interaction.

¹⁸ Modern quantum tunneling microscopes do have resolution down to the atomic level, but it is important to note that molecular theory gained the upper hand *long before* molecules were directly observable. It is an interesting question for historians of science to try to identify the tipping point; however, it is a matter of historical fact that direct observation is not a necessary condition for accepting a theory. See Maddy (1997: 135-43) for an interesting discussion of the eventual acceptance of atomic theory in the early 20th century.

Also, note that I am not here arguing in support of Quinean theoretical virtues or a Quinean theory of confirmation. The point, I think, is uncontroversial to philosophers of science: theoretical benefits, short of direct observation, do count as evidence in favor of a theory.

Like molecular theory, the law of conservation of energy garners overwhelming support from its theoretical utility; and like molecular theory, the conservation law, due to its generality, is not amenable to direct confirmation. Seizing on this latter point, Émile Meyerson wrote:

What really would be a valid experimental demonstration of the conservation of energy? We should need a considerable series of experiments showing that through all kinds of change, under the most varied conditions, different forms of energy transform themselves into one another according to equivalents remaining constant within the limits of error of measuring instruments.... It is not even certain that if we were able to measure very exactly all the energy known to us, as present in any phenomenon, we should find it really constant, and this for the quite simple reason that we are in no way sure of knowing all the forms of energy. (quoted in Elkana 1974: 162-3)

The crucial point here is not that we cannot test to see if energy is conserved in all possible *known* mechanical interactions; this is at least in principle possible, even if it is a daunting task. The idea is that we could never, in principle, come up with a sufficient demonstration of the law of conservation of energy because we cannot know if we know all the different forms of energy.

Yet the law has become a mainstay of modern science; its utility has been proven in many areas of research, from astronomy to thermodynamics to relativity. The law holds quite generally, though a consequence of the Heisenberg's uncertainty principle allows that the law may be violated provided that that period of time in which the total energy of a quantum system increases is extremely short.¹⁹ But this only speaks to the anomalous character of the quantum; we would surely rather, at least for the time being, make alterations in other parts of our total theory to account for this possibility than give up a long-standing and theoretically rich law.

I have suggested that the weaker form of ontological naturalism is preferable to the doctrine that there are supernatural or non-natural entities or properties on account of its theoretical benefits. If we take up the aforementioned Quinean virtues, we will see its benefits: the view is simple in that only one kind of entity or property is posited; it is familiar because the entities and properties mentioned

¹⁹ Energy-time uncertainty says that uncertainty in energy times uncertainty in time is greater than some very small number (Planck's constant), or: $\Delta E \Delta t > h$. This has the consequence that systems may gain energy so long as the surplus energy is remains in the system for a time no greater than h/E .

play in our best-going scientific picture of the world; it implies a wide array of testable consequences (e.g., demonic possession has a natural explanation); the view is fecund for new extensions (e.g., there have been attempts to naturalize nearly everything, from epistemology to metaphysics to ethics to legal theory); lastly, that testable consequences have turned out well (e.g., supposed supernatural phenomena have found a naturalistic explanation).

The above considerations of the theoretical benefits of ontological naturalism only speak ostensibly in favor of anti-supernaturalism. However, this is enough to undermine the Leiter-Railton view that methodological naturalism leaves open the question of ontological naturalism. Given that the naturalist's methods counsel her to accept what is best supported by the evidence, and that there are discernable kinds of indirect support, those methods also counsel a preference towards some form of ontological naturalism.

Suppose though that a methodological naturalist wished to remain neutral on whether or not there were supernatural entities. To her, there is not enough *clear* evidence either way, and in situations that lack clear evidence, she opts to suspend judgment. Surely, this is within her rational rights as there is no logical entailment between methodological and ontological naturalism. But to say that there is a lack of clear evidence suggests that there is a lack of direct, observational evidence. If it is her maxim to suspend judgment in all cases in which no direct evidence obtains, then she should likewise suspend judgment about the existence of atoms (or sub-atomic particles), for that supporting evidence is also indirect. Yet we cannot sympathize with this reasoning. The methodological naturalist is persuaded by the methods of the sciences precisely because of their effectiveness—surely the adoption of the atomic theory has been responsible for a wide array of rich discovery in the sciences. Rational adoption of such high-level hypotheses surely counts in *favor* of scientific methods; indeed, the ability of the scientific method to adjudicate hypotheses in terms of convergence of indirect evidence—here, theoretical virtue—seems to be a boon of the scientific method itself. Rejecting the rational acceptability of

these kinds of decisions seems to go against scientific methods, and *a fortiori* against methodological naturalism.

Still it might seem that the question of ontological naturalism is different in kind from molecular theory or the law of conservation of energy. One might object that the latter are empirical hypotheses, while ontological naturalism is a *metaphysical* thesis. But again, such an objection would not jibe with a methodological naturalist who has, consistent with the fundamental naturalistic impulse, eschewed any method of inquiry other than the methods of the empirical sciences. Methodological naturalism *is* the denial that there are questions of a different kind or character from empirical ones. “Metaphysical” questions, whether ontological or semantic, are naturalized by recasting them as empirical questions. Where formerly philosophers sought answers to such questions from the armchair, the naturalist will reframe such questions as empirical hypotheses, the merits of which are to be weighed in the same way as other scientific hypotheses are adjudicated.

5 METHODOLOGICAL NATURALISM AND SEMANTIC SUBSTANTIVE NATURALISM

For the Leiter/Railton view, naturalists who arrive at particular conclusions about ontology or semantics are *substantive naturalists*. Substantive naturalists “go beyond” methodological naturalism (Leiter 2002: 6). I will here argue that embracing a ‘substantively naturalistic’ conclusion is not distinct in any important way from applying the naturalistic approach when investigating the world in which we live. For Leiter, “it is an open question whether the best philosophical account of morality or mind or knowledge must be in substantively naturalistic terms” (2002: 6), and with this I do not disagree. There is no sense in which it is an analytic, conceptual truth that *methodological naturalism* entails *substantive naturalism*. Indeed, a methodological naturalist might live in a cloister, perhaps never coming to any conclusions, as their method does not countenance the effectiveness of armchair philosophizing. However, any active inquirer, with naturalistic methods in hand, *is* lead to some substantive conclusions

about the world. In this section, we will look at two proposed semantic naturalisms: first, Railton's a posteriori reduction of moral terms, and second, the Canberra Plan conceptual analysis of moral propriety terms.²⁰ For both positions, we will see that they are either starkly non-naturalistic, as they are at odds with the fundamental naturalistic impulse, or else the "substantive" label is redundant, as it doesn't capture anything over and above methodological naturalism (which, as a matter of course, contains some substantive positions).

I would like to note that critics of naturalistic ethics should not dismiss either of the following views by way of the naturalistic fallacy. The naturalistic fallacy claims that it is a mistake to infer from descriptive facts to ethical prescriptions. For instance, from the fact that exercise promotes well-being, one cannot infer that exercise is good or ought to be done—this inference, and inferences like these, are said to commit the fallacy. Closely related to the naturalistic fallacy is what has come to be known as the "open question argument," which says that there is no necessary connection between the descriptively given way things are and the ethically given way things are. Suppose one is given all the descriptive information about exercise, from there it is still an "open question" whether or not exercise is good or ought to be done. This means that one might reasonably enquire into the rightness of an action, even after having attained a complete descriptive account of it. The open question argument provides a reason for thinking that the naturalistic fallacy is a bad inference: the lack of a necessary connection between descriptive and ethical accounts of the way things are underwrites the naturalistic fallacy.

What Railton and Jackson need to do to fend off the naturalistic fallacy, it would seem, is to show why the open question argument does not cut against their view. In the discussion of each view below, after characterizing each view, I will briefly point out why they think the open question argument does not pose a threat.

²⁰ This view is also sometimes called "Analytic Moral Functionalism" (see, e.g., Miller 2003: 232ff.).

5.1 Railton's *A Posteriori* Reduction of Moral Terms

Railton suggests that moral predicates, such as “right” and “good,” are semantically reducible to non-moral terms, and that these non-moral terms are amenable to empirical inquiry. This naturalistic reduction strategy employs the use of *reforming definitions*. Reforming definitions revise the content of moral judgments—e.g., “X is right” is reformulated to “X is *N*,” where *N* is some complex natural property. A *natural property* is one that figures in scientific explanations; they are “terms of properties or relations that ‘pull their weight’ within empirical science” (1993: 315). It is important to note that Railton does not have an a priori notion of natural property ready to hand, nor does he claim that “right” or other moral predicates are synonymous with some complex of natural properties; rather, in order to identify “right” with *N* it is enough that the initially stipulative identification turns out, a posteriori, to play the same explanatory and motivational role as “right.” Thus his reduction stands in stark contrast to another kind of analytic reduction we will soon discuss, typified by Canberra Plan-style analyses of moral terms. That Railton’s reforming definitions of moral terms ultimately face the tribunal of experience highlights his commitment to the fundamental naturalistic impulse: Railton, like Quine and the abstract naturalistic inquirer under discussion, is skeptical that any level of inquiry apart from the natural sciences themselves can settle philosophical questions (here questions about moral concepts).²¹

Railton views his project of reducing moral terms to natural terms in the same spirit as the scientific reduction of “water” to “H₂O.” In the case of “water,” Railton claims, no suitable a priori analysis could show that “water” and “H₂O” denote the same stuff; likewise, no suitable a priori analysis of “right” could show that it denotes the same property as *N*, where *N* is a complex natural property. In both cases, the identifying reduction is something that must be found out and, once the reduction is in place, must be tested against experience. That is, the identification of “water” and “H₂O” must show itself to be explanatorily useful, indeed it must aid in our understanding of what water is. Similarly, the reduc-

²¹ We will see later if Railton is able to make good on his commitment to the fundamental naturalistic impulse.

tion of “right” to *N* is also an a posteriori matter: in order for us to accept the reduction, the identification must prove itself explanatorily useful and must aid in our understanding of the concept of “right.”

Far from an elimination of moral terminology, Railton thinks of his reforming definitions as a *vindication* of common sense moral language (1989: 161). The scientific reduction of “water” to “H₂O” is again his pet example: that “water” was identified with “H₂O” shows us that our concept of water was indeed useful all along. In his words, “The successful reduction of water to H₂O reinforces, rather than impugns, our sense that there really is water” (1989: 161). Railton hopes that a successful reduction of moral terms to naturalistic terms will be vindicative in the same way. First, that facts about the naturalistic reduction base (*N*) can play a role in an explanation of our experience of morality; secondly, that the reduction base are still properties that can motivate people in way familiar to moral properties (1993*b*: 317).

Let me return briefly to the open question argument. Recall that the open question argument is supposed to demonstrate that no analysis of “right” or any other ethical property in purely descriptive terms is adequate. For even after a descriptive account of something is exhausted, it will always be an open question whether that this is good. To Railton, the open question argument no longer applies. Railton’s claim that “right” is identical to some natural complex “*N*” speaks of a *a posteriori* identity. Because the reforming definition does not represent an analytical connection between “right” and “*N*,” there is no presumption that the reforming definition is meant to conceptually close the question of whether right is *N*. In the face of further empirical evidence we might have to alter or drop the reforming definition. The open question argument does not cut against Railton’s strain of ethical naturalism simply because it does not seek to conceptually close questions about the relationship between descriptive and ethical accounts (conceptually, if something is *N*, it *is* an open question whether it is right); therefore, the open question argument cannot be used against it (Railton 1989: 155-6).

So much for our explication of Railton's a posteriori reduction of moral terms. What is of interest to our discussion is whether and in what sense this project qualifies as substantively naturalistic in the Leiter-Railton taxonomy of naturalistic positions in philosophy. Recall that substantive naturalism is supposed to "go beyond" its methodological counterpart. If this is true, then we should be able to identify some part of Railton's reduction project that outstrips the higher-level explanatory norms of working sciences. If, on the other hand, we find no reason to believe that semantic reduction (vindicative or otherwise) represents an alien method to natural science, then we should find no reason to label Railton's project "substantive."

Railton's exemplary use of the reduction of "water" to "H₂O" brings me to my point: does it make sense to call a methodological naturalist, taking the methods and results of the empirical sciences as her guide, who accepts the reduction of "water" to "H₂O" a *substantive naturalist*? What is gained by affixing this label? Can we make sense of a methodological naturalists who, living in the twenty-first century, did not accept the reduction of "water" to "H₂O" ? Wouldn't this person strike us as anti-science and therefore anti-naturalistic? It seems then, that one can be a pure methodological naturalist only if one is cloistered away, or else prepared to deny long-standing scientific discoveries. Such abject denial of commonplace scientific knowledge would be more indicative of a thoroughgoing *anti-naturalism* than a pure methodological naturalism.

Let us consider an abstract naturalistic inquirer. Our naturalist deeply feels the fundamental naturalistic impulse that there are not any more worthwhile methods of investigating our experience of the world than the methods of the natural sciences. To quote Maddy once more:

[Our naturalist] will ask traditionally philosophical questions about what there is and how we know it...she will take perception as a mostly reliable guide to the existence of medium-sized physical objects, she will consult her astronomical observation and theories to weigh the existence of black holes, and she will treat questions of knowledge as involving the relations between world—as she understands it in her physics, chemistry, optics, geology, and so on—and human beings—as she understands them in her physiology, cognitive science, neuroscience, linguistics, and so on. (2007: 18)

Now, suppose our inquirer is asked whether or not she believes that water is H_2O . No doubt she will believe that to be true. When asked why she believes this identification to be true, she may call attention to Cavendish's synthesis of water by burning pure hydrogen in 1784, to Nicholson and Carlisle's dissolution of water into hydrogen and oxygen in 1800, to the success of Dalton's atomic theory, to the measure of atomic weights and proportions by Berzelius in 1826.²² All of these experiments and hypotheses give credence to the idea that hydrogen and oxygen combine in a 2:1 ratio to form water-like fluid, and that the resultant fluid does indeed contain all the properties—potability, density, boiling and freezing points, conductivity, etc.—associated with what is normally called “water.” She will agree with Railton that the reduction of water to H_2O serves an invaluable explanatory role. Understanding of its chemical composition leads to an enriched understanding of a number of its properties such as surface tension, solvency, capillary action, etc. Considering all the evidence at hand, she agrees that water really is H_2O . Note that her acceptance of the identification of water with H_2O does not depend on any higher, extra-scientific semantic principle, nor does it depend on the universal validity of any principle of molecular analyzability; though she might accept the latter as an explanatory maxim, her decision to accept that water is H_2O does not *depend* on the veracity of this principle, the relevant experimental results and gained explanatory virtue are enough.

Reasons for her cheerful acceptance of a reduction of “water” to “ H_2O ” are, however, not realized in the case of reducing “right” to “*N*”. In the first place she is not presented with a sizable history of discourse about *N* as she is with a history of discourse about H_2O . Second, she knows of no crucial experiments that have established that “right” really is just “*N*,” though she would surely welcome such news. She, like Railton, finds encouragement that “claims about the world which have, historically, been deemed by philosophers to be *a priori* true...have, with distressing regularity, been revised or abandoned in the face of emerging scientific theories” (1989: 156). She would certainly be thrilled to see

²² See Boynton (1948), pp. 258-79.

that what are all-too-often deemed non-natural, moral properties are in fact just complexes of natural properties or that so-called analyses of “right” attain their air of quasi-necessity in virtue of a posteriori identification. However, for her to accept such a definitive conclusion, her modesty would have it that enough evidence has accrued and that some track record of discourse about “right” being identical to “*N*” has materialized. As of now, she is certainly excited about the prospects of such a reduction, and welcomes advances in empirical moral psychology, neuroscience, cognitive science, and the like; however, an embrace of an overarching semantic principle with regard to the reducibility of moral property terms to complexes of natural terms, as a philosophical conclusion, is far too enthusiastic. After all, her methods demand that there are no distinctly ‘philosophical’ conclusions.

Accumulations of a posteriori identification of vernacular terms (e.g., water) with technical terms (e.g., H₂O) give inductive support to the idea that all terms referring to substances reduce to terms referring to chemical composition. Thus, our naturalists may well accept reductionism in this domain as a matter of principle, though this principle represents a high-level norm with respect to explanations of substances. Reducing water to H₂O has proved beneficial to our understanding of water; we should therefore seek out reductions of other terms in order to better understand the world in which we live. However, it is important to note that the acceptance of this principle is contingent on whether reduction projects turn out well. The acceptance of an overarching semantic principle with respect to some domain of language antecedent to any successful reduction cases would be inconsistent with the methods of sciences.

Back to Railton’s reduction of moral terms to natural properties: we do not seem to have the same kinds of successful reduction cases to appeal to. It therefore seems that the acceptance of a general principle—that moral predicates are identifiable with non-moral terms amenable to empirical inquiry—happens on a level distinct from the empirical sciences. This cuts against the fundamental naturalistic impulse to deny the reliability of any other level. Therefore, this project seems non-naturalistic

on my account. It is a drawback to define naturalism in terms of the methodological/substantive distinction, because it sets the bar for naturalism too low. A mere inclination towards explanations that use only natural terms will not do, for one might arrive at such a position by divination, or astrology, or coin-flipping; surely, one would not entertain promoters of those methods as naturalists. However, if a commitment to the fundamental naturalistic impulse is necessary for naturalism, then *any* methods other than scientific methods are akin to chances tosses of a coin. So Railton's semantic naturalism will win acceptance because successful cases of reduction count in its favor—in which case it is like the reduction of "water" to "H₂O;" or else the matter is settled on some other level apart from the natural sciences—in which case, to the methodological naturalist, it might have just as well been settled by flipping a coin. So Railton's naturalism, under my suggested, more narrow account, is either of a piece with methodological naturalism or else not naturalistic at all. If there is no real methodological difference between "water is H₂O" and "right is *N*," as Railton claims (in both cases the identification is discovered a posteriori, representing a tolerable revision of "water" and "right," respectively), then there is likewise no real difference in slapping routine scientific reductions (of water, of heat, of lightening, etc.) with the 'substantive' label—and certainly *many* beliefs of our naturalist would bear that label.

5.2 "Canberra Plan" Conceptual Analysis of Moral Terms

I have just laid out my reasons for thinking that Railton's reduction of moral terms to natural terms is either anti-naturalist or substantively naturalist in only a Pickwickian sense—methodologically naturalistic inquiry comes to substantive conclusions as a consequence of its methodological commitments. Calling the acceptance of the reducibility of one domain of language to another "substantive" is redundant, considering an adherence to the fundamental naturalistic impulse. We will now look at another candidate substantive ethical naturalism from the Canberra Plan of conceptual analysis by Frank Jackson, inspired by David Lewis' method for defining theoretical terms. We will see that such a view is either decidedly non-naturalistic, because it contravenes the fundamental naturalistic impulse (which I

have been urging is central to a focused understanding of “naturalism”), or else it is of a piece with methodological naturalism.

We have already seen the broad strokes of Lewis’ plan for defining natural terms in our earlier treatment of Carnapian rational reconstruction. Lewis’ proposal to define theoretical terms is essentially a revamping of Carnapian rational reconstruction (e.g., he rejects Carnap’s division of language into its theoretical and observational portions (1983: 79, 82). Jackson in turn revamps the Lewisian proposal in hopes of defining moral terms like “right” and “good” in a descriptive, naturalistic vocabulary.²³ We can see the parallels with Carnapian rational reconstruction in that Canberra Plan analyses likewise dispense with problematic vocabulary and prescribe a method for defining what we consider indispensable, but nonetheless problematic, terms in a favored, descriptive, and natural vocabulary.

Jackson’s proposal to identify ethical and descriptive properties goes as follows (1998: 140-1): Let **M** be a mature folk morality, written as a long conjunction of moral platitudes, with predicates in property name form (e.g., “Killing is morally wrong” is rewritten as “Killing has the property of being morally wrong”). Each conjunct of **M** is a platitude that a competent user of moral terms would have knowledge of a priori. Analogously, we might image a mature folk color theory **C**, that would contain a long conjunction of platitudinous sentences that a competent user of color terms would know in virtue of having mastered the relevant color concepts (e.g., “The property of being red is what causes objects to appear red to in visual experience under standard conditions,” “Red has the property of being more similar to orange than to blue”, etc.). In **C**, the platitudinous color statements constitute a folk color theory. Likewise, **M** lists the statements that are constitutive of morality. We can write **M** as

$$\mathbf{M}(m_1, \dots, m_n)$$

where (m_1, \dots, m_n) makes explicit the predicates used in **M**.

²³ The particular innovation here is to extend Lewis’ proposal for defining theoretical terms by analysis of their causal-functional role to terms, in this case moral property terms, that are generally held to have no genuine causal role. See Menzies and Price 2009: 183.

Next, we may rewrite **M** as the *Ramsey sentence of M* by removing moral property terms and replacing them with existentially bound variables:

$$\exists x_1, \dots, \exists x_n \mathbf{M}(x_1, \dots, x_n)$$

The Ramsey sentence of **M** says that there are some other things that function in the same way as moral terms do in the moral platitudes. For instance, there is some property that plays the role same role as “right” in our moral platitudes.²⁴

What this account alleges is that there is some necessary conceptual connection between the moral property terms routinely employed and some purely descriptive, natural properties. In this sense, we may draw a comparison with Railton’s reduction: Railton wanted to revise the surface content of moral judgments by means of reforming definitions. When we say, for instance, “that X is right” what we really mean is that “X is N” (where N denotes some complex natural property) because “right” is identical, *a posteriori*, to N. Here, the identity relation between “right” and N was something to be found out, not a consequence of any analytic connection between moral predicates and natural ones. On the other hand Jackson’s reductionism *does* claim an analytic connection between moral property terms and natural terms. This connection is revealed through an analysis of the roles played by moral property terms in **M**, and showing that some descriptive properties play exactly the same roles.

For example, consider the following analysis that says what it means for some action A to be right:

$$(R) \quad A \text{ is right iff } (\exists x_1) \dots (A \text{ has } x_r \ \& \ (y_1) \dots (\mathbf{M}(y_1, \dots) \text{ iff } x_1 = y_1 \ \& \dots))$$

where ‘ x_r ’ replaced ‘being right’ in **M** (Jackson 1998: 140-1). We now have the conditions under which A is right:

²⁴ Jackson modifies the above Ramsey sentence to say that there is a unique realization of **M**: $\exists x_1, \dots, \exists y_1 \dots \mathbf{M}(y_1, \dots)$ iff $x_1 = y_1 \ \& \ x_2 = y_2 \dots$. For our purposes we can disregard whether or not **M** is multiply realized, for our interest is an assay of this brand of conceptual analysis for a naturalist bound to the fundamental naturalistic impulse.

[A] is right just if it has the property that plays the rightness role as specified by the right-hand side of (R), a property we can be confident is a purely descriptive one, given the unrestricted, global, a priori supervenience of the ethical on the descriptive. (ibid.)

Our naturalist, who seriously countenances the fundamental naturalistic impulse, will surely balk at the claimed a priori supervenience of the ethical on the descriptive—to her supervenience theses are akin to the higher-level norms, like the ontological positions discussed in the previous section. Like conservation laws, causal principles, mechanism, and physicalism, supervenience principles seem to our naturalist to be propositions deeply embedded within the web of belief, insulated from empirical confrontation, but not ultimately immune to revision. She sees no reason to think of supervenience as different in kind from any other explanatory norm, and so will see it in the same light as physicalism—namely, as a high-level empirical hypothesis.

What should our naturalist think of the claim that moral property terms like “right” are analytically identifiable with descriptive terms that play the same role? Skeptical against methods that suppose another level of inquiry over and above the empirical sciences, she would wonder whether adjudication of the modified Ramsey sentence (R), for instance, entangles us in any extra-scientific investigation. Jackson makes it quite clear that conceptual analysis is an indispensable method of inquiry quite apart from the methods of the sciences. Conceptual analysis finds its particular utility in setting the agenda for further empirical investigation. If we are interested in giving an account of ethics or mind or knowledge, then we had better be sure what we are looking for:

When bounty hunters go searching, they are searching for a person and not a handbill. But they will not get very far if they fail to attend to the representational properties of the handbill on the wanted person. These properties give them their target, or, if you like, define the subject of their search. Likewise, metaphysicians will not get very far ... in the absence of some conception [of their subject]. (Jackson 1998: 30-1)

Analysis is in the business of clarifying what we mean by our usual ways of speaking about ethics or mind or knowledge, as such it sets the agenda for further investigation, defining the subject. If we want to know what rightness is, we must first spell out what rightness involves, for which conceptual analysis

is perfectly suited. If conceptual analysis is in fact an indispensable supplement to the empirical sciences, then the modified Ramsey sentences must be epistemically distinct from the synthetic claims encountered in the sciences; indeed, they must be knowable, in some sense, a priori.

It is exactly this claim that the special status of conceptual analysis rides on. For Jackson, we could not be assured of reductive claims like “Water is H₂O” unless we were presented with a demonstration that H₂O plays the same role as water. Just what comprises the water-role is purportedly knowable a priori, since this role is just the list of reference-fixers for the term “water” and any proposition of the form “X is the F” is knowable a priori just in case “the F” fixes the referent of X (Jackson 2002: 161). Conceptual analysis is therefore in the business of mining definite descriptions of common vocabulary and then specifying the connection between terms of one kind to terms of some other kind. In particular, as our discussion of moral property terms has shown, conceptual analysis is interested in showing that folk-concepts are reducible to descriptive concepts. Analysis of the folk concept of morally right, under this view, makes explicit the rightness-role and then seeks to find a term in some other, preferred vocabulary that plays that same role. It is important to note here that, like Carnap, the conceptual analyst is proposing another level of inquiry apart from the level of empirical science. “Your methods,” he might say to the scientists, “are perfectly in order given your purposes. However, I want to make sure that our investigations don’t founder, that we aren’t looking for a handbill instead of a person, and that we can be confident, in fact certain, that water is H₂O or that descriptive properties terms play the role of moral property terms. For this we need serious metaphysics—we need conceptual analysis.”

Our naturalist will not feel the pull to this higher level of inquiry. She will agree that we know “water is H₂O” to be true because, *inter alia*, water and H₂O play the same role. However, she is not persuaded that conceptual analysis of the folk concept of water is necessary for determining the water-role. It is at least an open question for her whether or not reference fixing occurs by definite descrip-

tion, ostension and baptism or by causal-historical transmission. Indeed, she may wonder if we need an account of reference at all.²⁵

But let us grant the definite description of the rightness-role, and try to see why conceptual analysis is indispensable for determining the truth conditions of the moral property term “right.” Recall that these conditions are given by

(R) A is right iff $(\exists x_1) \dots (A \text{ has } x_r \ \& \ (y_1) \dots (\mathbf{M}(y_1, \dots) \text{ iff } x_1 = y_1 \ \& \dots))$.

This will come out as true if and only if A does in fact play the rightness role, meaning that the Ramsey sentence of \mathbf{M} with respect to “right” is also true. In other words, (R) will be true if and only if it is the case that there is something that plays the same role as “right.” This claim, to our naturalist, seems to say something about the world. That there is something, some complex of states for instance, that plays the role of right does not seem to her to be guaranteed by concepts alone. To generalize from the present case: the proposition that “There is a such-and-such that plays a distinctive role” strikes her as an empirical hypothesis, the truth or falsity of which is not decidable a priori, but rather is a matter of experiment. The suggestion from the Canberra Plan now sounds as if it is an exhortation to look to our preferred, scientific account of the world to see if we can uncover whether or not the property terms used there can cover other cases. When chemistry became a systematic science, it was then up to the chemists to demonstrate that their new way of talking about water as H_2O does not change the subject. Knowing that the water synthesized by burning hydrogen is the same (or near enough) as the water that we dip out of a well or as the water that falls from the sky is, our naturalists will insist, *pace* the analyst, a matter of empirical inquiry: we might smell, taste, boil, freeze, dissolve other substances in, run currents through each of the samples and conclude that they are in fact the same—that water really is H_2O . Similarly, if we want an account of ethics or mind or knowledge, we should look to our scientific account

²⁵ Debate over the need for a philosophical account of reference for scientific practice goes back to Tarski’s semantic theory of truth (1944), which, quite famously, dispensed with reference in assigning truth-values to propositions. Hartry Field (1972), also quite famously, disagreed with Tarski, claiming that an account of reference is required.

of what the world is really like. Advancements in the brain sciences invite a great deal of enthusiasm about the possibility of integrating moral, mental, and epistemological property terms into our fundamental scientific vocabulary, for our naturalist this will be a matter of experiment and not a matter of connecting folk concepts, analytically, to concepts of another vocabulary.

It is unlikely that Jackson would deny the naturalistic interpretation above that confirmation or information of Ramsey sentences is an empirical matter. It is important to note that Jackson is not in fundamental disagreement here, as acknowledgement of the *a posteriori* status of these sentences provides the way out of the open question argument. Recall again that the open question argument says that no matter how much descriptive information is given about an action, one might still reasonably enquire about the rightness of that action. But this is no objection to Jackson's account, for the identifications of ethical properties, like rightness, with descriptive properties are one and all *a posteriori*. Because Jackson makes no attempt to conceptually close the question of whether or not rightness is identical to some descriptive properties, future evidence may force his to modify or abandon this identification, the open question argument is cannot be an objection to his account. What Jackson *does* claim on behalf of conceptual analysis is that there is a necessary connection, which is also knowable *a priori*, between the descriptively given way things are and the ethically given way things are. What is at issue here is called "*a priori* passage." The inference from the ethically given way things are to the descriptive way things are *is a priori, given that the associated Ramsey sentence comes out true.*²⁶ Once it is known that some descriptive property plays the same role as some ethical property, there is a further demand from proponents of conceptual analysis that that is not enough. We still need a further account of the connections between vocabularies that takes place at the conceptual level. This is why conceptual analysis is a two-level view.

²⁶ See Jackson 1998: 80-3; 2002.

The above interpretation of Canberra Plan analyses of moral property terms held fast to a one-level view. The modified Ramsey sentences represented empirical hypotheses that are investigated by routine scientific methods. Analyses of folk concepts, from our naturalistic perspective, do not reveal anything interesting, guaranteed by concepts alone; they instead seem to suggest hypotheses about the roles that moral property terms might play, calling for a look into our current best-going theories to see if we might identify something that does in fact play that role. Once the demand for further analysis is rejected, Canberra Plan analyses seem to either be of a piece with methodological naturalism, which tells us to look to our preferred scientific account of the world when investigating typically philosophical problems, or else it is a call to a higher, extra-scientific domain of inquiry. We have already seen how the fundamental naturalistic impulse would shun appeal to any other methods other than those employed by the sciences. Given Quine's reaction against Carnap, it is especially clear that conceptual analysis is anathema to naturalism, as it depends heavily on the partition between sentences that are analytic and sentences that are synthetic.

6 CONSEQUENCES AND CONCLUSIONS

Debates about the meaning of "naturalism" are, I think, important, especially given the prevalence of the term. Not everyone agrees with this. David Papineau, for instance, thinks that the meaning of "naturalism" is "essentially a terminological matter" (1993: 1). For him, "the important question is which positions are right, not what to call them," and the proper course, given the nebulous meaning of the term "naturalism" is to "address the substantial philosophical issues first, and worry about terminology afterward. Once we have worked out which commitments *ought* to be upheld by philosophers who aspire to 'naturalism', then we can agree to use the term accordingly" (ibid.)

However, it seems clear to me that "naturalism" has become somewhat of an honorific label and I think that this result is detrimental. For we do ourselves and others a disservice if we fly the banner of

naturalism while not quite understanding the commitments involved. At the very least, we often unwittingly equivocate while discussing naturalism, what follows from adopting naturalistic views, and whether and how different positions that are often labeled as naturalistic hang together. To me, the work to be done in getting clearer on a definition of naturalism, or at least what is minimally required for naturalism, is part and parcel of work on the “substantial philosophical issues;” minimally, a more precise vocabulary facilitates fruitful discussion. We should do everything in our power to deflate notions that have become puffed-up, partisan indicators. Much recent work has gone into refining the definition of “physicalism,” and as a consequence, I think, the whole area of debate has been improved. I do not think this work indicates that getting clear on physicalism is “essentially a terminological matter,” apart from some other investigation into the veracity of our beliefs.

In section III, I aimed to demonstrate that ontology could be settled by appeal to the very same theoretical desiderata that are routinely employed to adjudicate between competing scientific theories. This methodological admonition to set “philosophical” concerns on the same level as scientific ones—*qua* the fundamental naturalistic impulse—is much more than a stipulative terminological matter. Far from showing merely that only those who cleave to the fundamental naturalistic impulse should be called naturalists, I have also shown that acceptance of a methodology fixes some substantive beliefs. Here, a question about what naturalism is—a supposedly terminological matter—has revealed the connection between methodological commitments and other beliefs, in this case ontological beliefs. Thus, getting clearer on the central features of naturalism, we have gained some insight about which views ought to be upheld: those who espouse philosophical continuity with the empirical sciences ought to deny the existence of super-natural entities *and they ought to do so for purely methodological reasons*.

Section IV gained a similar result. Methodological considerations are also able to fix further beliefs about semantic reduction. A look at Railton’s use of reforming definitions (IV (a)) showed that questions about the reduction of moral property terms to complexes of natural properties would have to be set-

tled in the much the same way as terms are reduced in scientific practice. For instance, reduction of “water” to “H₂O” grew out of an accumulated background molecular theory, replete with laws of proportional combination of elements, and became justified by a dramatic enlargement of our understanding of the properties of water when considered as a chemical combination. Macro properties of water, such as surface tension and conductivity, became explicable in light of its micro-chemical composition. We found the case of reduction of moral property terms like “right” to be of a different sort. In this case we are not presented with an entrenched background theory, nor has treatment of moral properties as natural properties lead to a greater understanding of morality in the same way as treating water as H₂O did. Naturalists committed to the fundamental naturalistic impulse, should therefore be cautious about acceptance of broad semantic doctrines concerning the reducibility of moral property terms to complexes of natural properties, for in this case it appears as if Railton and those who follow him have elected to make the semantic decision by some criteria quite apart from the ordinary goings-on of scientific practice. For in the “water” to “H₂O” case, much more was needed than reforming definitions, a coherence with well supported molecular theory and a continued demonstration the benefits of chemical composition were both important in vindicating a change in vocabulary. With Railton’s proposal, it seems that these desiderata have not been satisfied. Again, far from a mere stipulative suggestion about the meaning of “naturalism,” I have tried to show how far methodology reaches. Something as austere as the fundamental naturalistic impulse can go a long way in fixing our sundry beliefs—as we have already seen with respect to ontology and Railton’s semantic reduction of moral terms.

Finally, in section IV (b), I wanted to take a look at another kind of semantic reduction popularized by the Canberra Plan of analyzing moral property terms. Here, conceptual analysis of folk theories of morality was supposed to show that moral property terms like “right” are analytically reducible to what ever other terms play the same functional role. The Canberra Plan analysis urges that there is a prior investigation at the conceptual level to be carried out before having a look at our best going theory to

see if there in fact is such a thing that plays the rightness role. Conceptual analysis, so it is claimed, is necessary in order to set the agenda for scientific research to ensure that we are seeking a person and not a handbill. I have suggested that conceptual analysis is not crucial to scientific practice. When, for instance, “water” was identified with “H₂O” what was crucial was that chemists were able to demonstrate that they had not changed the subject and that our talk of water could be appropriated by all our talk of H₂O. This appropriation is justified, again, by general scientific considerations—that it coheres nicely within a working background theory and that it fosters greater explanatory power—not because of anything about some folk notion of water. This reading of the Canberra Plan deflates the claim about the special status of conceptual analysis; this reading of the Canberra Plan follows directly from the fundamental naturalistic impulse. Again, methodology fixes further beliefs about conceptual analysis.

I have not merely stipulated that naturalism means first and foremost a commitment to the fundamental naturalistic impulse. Instead, this commitment is part of the history of the term; it comes, so to speak, preinstalled. To understand a term, I think, is to understand the history of its usage. Quine gave powerful expression to the idea that all areas of philosophy are amenable to empirical adjudication, even those areas, like logic and metaphysics, that seem to be extra-empirical if anything is. Quine suggests that nothing really is. This is the simple but radical starting point of naturalistic philosophy in America, and I cannot understand any usage of term “naturalism” that would dispense with its roots in the fundamental naturalistic impulse. What this means is that recent usage of the term that has focused on only the “substantive” naturalistic positions—physicalism or semantic reduction—has found history obsolete. “Naturalism” originally and primary indicates a method of doing philosophy. It is cavalier to use “naturalism” indicate positions that might be gained in any old way, by divination or coin-flips. Again, this is not mere stipulation. To say that naturalism is indicative of a method of doing philosophy is to pay due homage to the way in which attempts to naturalize various domains of philosophy, like epistemology or metaphysics, was first a revolution of method, not of substance. However, this does

not mean that substantive positions do not follow from methodological considerations coupled with a going world theory—as the foregoing sections have shown.

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