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Naturalism and Scientific Practices: A Concluding Scientific Postscript

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Naturalism is the dominant philosophical stance in North American philosophy today. Many philosophers explicitly identify themselves as naturalists, whose philosophical work is closely aligned with the natural sciences. Yet the best evidence for the dominance of naturalism comes from those philosophers who profess to be its opponents. These critics often make very significant concessions to naturalism. They hasten to accept a broadly scientific understanding of the world. They also often disavow any philosophical constraints upon scientific inquiry. Many even express these sentiments by accepting 'naturalism' as a label for their own views: they oppose "radical" versions of naturalism, but do so by defending a more tolerant, inclusive version of naturalism.

My response to this apparent triumph of philosophical naturalism echoes the Danish philosopher Søren Kierkegaard's *Concluding Unscientific Postscript* to the triumph of Christianity in 19th Century Europe.

> Nowadays we all of us indeed are Christians. But with this, what have we all become, I wonder; and what has Christianity become by the fact that we all of us as a matter of course are Christians of a sort? This work has made it difficult to become a Christian, so difficult that among people of culture in Christendom the number of Christians will not be very great. (1941, 519–20)

My "concluding scientific postscript" advances a similar claim about philosophical naturalism. It has become too easy nowadays to profess to be a naturalist; we need to recognize how difficult it is to fulfill the philosophical demands of naturalism. I say this not to criticize naturalism, but to invite you to accept a more demanding conception of what naturalism commits us to. I then argue that a conception of naturalism which gives central place to scientific practices rather than scientific knowledge best satisfies these demands.

The paper has two main parts. The first part of the paper identifies some tensions and conflicting demands that arise within naturalist philosophy today. The second argues that shifting primary philosophical attention from scientific knowledge to scientific practices provides a constructive response to these tensions.

Part I: Making It Harder to be a Naturalist

The tensions and conflicts within naturalism stand out especially clearly in the context of the historical development of naturalism, so I begin the first part of the paper with some brief historical remarks. After a more extensive discussion of the resulting internal tensions, the first part of the paper concludes with a more detailed discussion of one prominent locus for these tensions, namely whether to think of a scientific understanding of nature in terms of natural *laws*, or whether to do so in terms of *causal* interactions and theoretical models. This issue will then play a prominent role in the second part of the paper.

Ia: Some Historical Remarks About Naturalism

Western philosophical naturalism arose in the long struggle to free science or natural philosophy from its origin within a religious understanding of the world. Most early modern natural philosophers (such as Isaac Newton in the 17th Century) placed the scientific interpretation of nature within the larger project of understanding God's creation. Reading the Christian Bible and "reading" God's creation were two sides of a single activity. During the 18th and 19th Centuries, scientific research gradually freed itself from subordination to theological concerns. Nature became a distinct domain of understanding, accountable solely to its own standards and norms. Natural science was thereby freed from the authority of church or scripture. The French mathematician Laplace famously expressed the spirit of this initial stage of naturalism. In response to the Emperor Napoleon's question about the place of God in Laplace's celestial mechanics, Laplace said, "I have no need for that hypothesis." His view expressed the "enlightened" aspiration to free our understanding of nature and ourselves from appeals to divine intervention or other supernatural elements.

When natural science had been partly freed from subordination to religion, however, naturalism took on a new dimension. New scientific disciplines in psychology and the life sciences emerging in the late 19th Century offered a more expansive naturalist vision: natural science might also replace philosophical accounts of thought and rationality. In Germany, this project had a practical import, since there were then no university chairs in psychology. If experimental study of the mind was to find a place in the university, it would have to replace logic and epistemology in university chairs previously devoted to philosophy.

For much of the 20th Century, most philosophers rejected the effort to give empirical scientific answers to traditional philosophical questions. The great anti-naturalist philosophers of the early 20th Century such as Frege, Husserl, or Carnap argued that science could not do without philosophical guidance as easily as it had forsaken religion. They agreed that only empirical science can describe the contingencies of nature, society, and human psychology.

Philosophy nevertheless had a different and supposedly vital task. Only philosophy could clarify and justify the meaning and validity of scientific understanding. Science therefore needed philosophy to fulfill its own mission of understanding nature and human society. The competing philosophical programs of logical analysis, phenomenology, and neo-Kantian philosophy agreed that some form of philosophically comprehensible necessity must provide the norms for genuine scientific understanding. The primary topic of debates over naturalism had then changed, however, from a metaphysics of nature that rejects anything "supernatural," to the semantics and epistemology of science.

How did naturalism become more acceptable to philosophers later in the 20th Century? What changes in science or philosophy allowed naturalism to achieve its current philosophical prominence? Internal criticism of the anti-naturalist philosophical programs was important, but that is less relevant to my argument. I will instead highlight three developments that made naturalism a more attractive philosophical position. I will then discuss some different conceptions of philosophical naturalism that emerged from these developments, and the difficulties of integrating these conceptions.

Sophisticated new scientific research programs in cognitive science, evolutionary biology, and neuroscience were the first development that made naturalism more attractive to philosophers. Early 20th Century psychological theories and research programs were vulnerable to philosophical criticism. Frege, Husserl, or much later, Noam Chomsky, could readily show why these research programs were unable to explain the content of thought and language or the norms of epistemic justification. In contrast, the new research programs in psychology and biology did propose a defensible basis for understanding content and justification, in terms of the cognitive or evolutionary functions of mental states. These scientific fields were also much better empirically grounded than their predecessors. Above all, however, they introduced a new relationship between natural science and philosophy. Early 20th Century naturalists sought to *replace* philosophy with empirical research in psychology. Contemporary naturalists instead typically propose *philosophical* theories for which cognitive psychology, evolutionary biology or neuroscience provide important resources. Naturalists now want to use science to do important *philosophical* work that empirical science alone could not replace. Science provides philosophers with new empirical and conceptual tools to help understand thought, language and knowledge philosophically. Moreover, these scientific resources enhance the legitimacy of naturalist philosophy. Naturalists can now plausibly claim to replace mere philosophical speculation with empirically grounded philosophical research.

A second major development occurred within the philosophy of science. Earlier opponents of naturalism argued that only philosophy could account for norms of scientific justification and understanding. Historical and contemporary studies of scientific research showed, however, that the sciences often did not fit philosophical claims about how science ought to be done. The failure of logical empiricist philosophy of science was the most widely discussed example, but phenomenological accounts of science encountered similar problems. The eventual response of most philosophers to conflicts between philosophical norms and scientific practice was to accept the self-sufficiency of the sciences. If scientific work conflicts with philosophical accounts of science, we should revise our philosophy. Philosophy of science has no standing to legislate norms governing science. Some philosophers still do offer general philosophical accounts of science, but their accounts now typically defer to how science is done. For example, recent disputes among scientific realists, instrumentalists, or social constructivists do not concern which standards *ought* to govern knowledge of nature. Proponents of these views instead claim to make best sense of science as it *is*.

Changing philosophical attitudes toward causality and necessity are the third and final historical development I consider. Under the influence of David Hume, empiricist philosophy of science was long suspicious of the notion of causal connection. Hume's followers argued that observation could only recognize empirical regularities, not causal connections. Moreover, most early 20th Century philosophers thought that all empirical truths were contingent. "Necessity" could only mean logical, rational or transcendental necessity, recognizable by reason alone. Causality and natural necessity have now become more philosophically respectable concepts, however, for several reasons. First, philosophers of science now recognize that scientific understanding and inductive confirmation seem to require some concept of necessity or causal connection; strictly empiricist descriptions of nature cannot suffice for science. Second, important technical work in modal logic has allayed earlier philosophical suspicions about the coherence of modal concepts and inferences. Finally, the empiricist philosophy that supported suspicions about causality or nomological necessity now looks like an unwarranted philosophical imposition upon the sciences. As a result of these changes, naturalists today have much more powerful *philosophical* concepts and inferences for understanding thought and knowledge than were available to their predecessors. Naturalists can now use a scientific understanding of nature that goes beyond merely contingent facts to grasp causal relations or natural laws.

Ib: Some Conflicts and Tensions Within Philosophical Naturalism

These philosophical and scientific developments have now given naturalism a central place in North American philosophy. This apparent triumph of naturalism nevertheless masks some important disagreements over what naturalism is, and what difference naturalism would make in philosophy or science. In this part of the paper, I will describe some of these tensions, and argue that they are genuine conflicts that naturalists must resolve.

The first issue concerns the argumentative force of naturalism. What difference would accepting philosophical naturalism make to how we live and think? Is naturalism a *radical* position that requires its proponents to reject many otherwise attractive beliefs or practices? Or is naturalism a *tolerant* stance that accommodates most of what people already believe and do? The first stage of naturalism, which gradually freed science from religion, was understood to be radical at that time. An autonomous natural science may leave no place in the world for God, supernatural powers, or direct revelations of knowledge. The early 20th Century attempts to replace philosophy with empirical science also adopted a radical stance. These naturalists sought to *abolish* armchair philosophical speculation about reason or cognition. They would replace all traditional philosophy with resolutely empirical methods and an unsentimental conception of human thought undisciplined by scientific methods.

Such radical orientations are still an influential aspect of philosophical naturalism today. Opposition to religious conceptions of nature is admittedly no longer a radical stance among Western intellectuals. Some radical naturalists do argue, however, that a scientific worldview has no place for a "folk" psychology of beliefs and desires, for consciousness, for reliable self-awareness, or for binding moral norms. Radical naturalists believe or hope that the progress of neuroscience, cognitive science, evolutionary biology or physics will eventually replace some or all of these aspects of human self-understanding. Indeed, some believe that current scientific theories already show that these familiar philosophical conceptions and vocabularies are otiose.

Radical naturalists are now frequently challenged by advocates of a more gentle and tolerant naturalism, however. More tolerant naturalists have an inclusive vision of a scientific understanding of the natural world.[1] They argue that the self-sufficiency of natural science within its own domain does not support a scientific imperialism that reconstructs other disciplines in natural scientific terms. The sciences are not a unified domain, and do not require the reduction or elimination of all concepts apart from a single austere scientific vocabulary. If appeals to folk psychology, conscious awareness, or rational insight offer improvements in prediction or explanation, and do not openly conflict with established scientific results, they are compatible with a naturalistic stance in philosophy. Perhaps a tolerant naturalism could even accommodate an appropriately modest theology and religious life.

Finally, there are what might be called reactionary strains of naturalism. I am thinking of philosophers such as Bernard Williams, John Searle, Thomas Nagel, or Charles Taylor. These philosophers *oppose* naturalistic approaches to

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1 De Caro and MacArthur (2004) bring together some eloquent and influential approaches to a more tolerant philosophical naturalism.

mind, knowledge, or ethics.[2] Yet they do so on the basis of a philosophical understanding of science that early 20th Century philosophers would have regarded as naturalist. They agree that the natural sciences are self-sufficient in their own domain, with no need for philosophical justification. Moreover, they endorse a relatively naive conception of science: natural science secures an "absolute conception of nature" (Williams 1985), or a straightforward grasp of natural kinds, natural causality, or natural laws. They offer no elaborate philosophical defense of their views of science, which they assume to be the common heritage of all sensible participants in a scientific culture. These ostensible opponents of naturalism thus indicate how thoroughly naturalism dominates contemporary North American philosophy. Even many critics of naturalism are now "naturalists of a sort."

The differences between radical, tolerant, and reactionary naturalist attitudes only concern how deeply naturalism challenges familiar conceptions of thought and agency. A more substantial difference in the very idea of philosophical naturalism accompanies these differences in attitude. What does it mean to align philosophy with the natural sciences, as naturalists propose? For many philosophers, naturalism is a commitment to understand mind, knowledge or morality as part of scientifically-comprehended *nature*. I call this approach "metaphysical naturalism." A different conception of naturalism is widespread in philosophy of science, however. Here, naturalism concerns how to do philosophy rather than how to understand mind, knowledge or morality within nature. I call this second conception "scientific naturalism." Scientific naturalism demands that philosophy answer to *science* rather than to nature. Many scientific naturalists give up the aspiration to a general philosophical conception of science or nature, and simply engage with ongoing work in a specific scientific field. Others do develop more general philosophical views about explanation, experimentation, or scientific theory, but hold those views directly accountable to how science is done in different fields.

Metaphysical naturalism and scientific naturalism have different histories and invoke different standards of philosophical adequacy. Metaphysical naturalism inherits the Enlightenment's attempt to eliminate God and the supernatural from our understanding of the world and ourselves. Human beings are natural entities. If semantic, epistemic, or moral norms have content, authority or force, it comes

2 Searle (1983) presents his view as naturalist. Yet Searle's "naturalism" turns out to involve a philosophical account of intentionality developed without significant appeal to empirical science, coupled with an expression of faith or hope that subsequent work in the biological sciences will show how intentionality so construed can be shown to be a higher-level feature of some biological systems. I regard this view as closer to those of anti-naturalists about the mind, because it effectively gives primacy to philosophical analysis over empirical research, which is presumed to follow in philosophy's wake.

from their role in our *natural* lives. There is no normative authority apart from the natural world. This stance has important consequences for how we do philosophy. Philosophy must be empirically grounded; appeals to rational insight or philosophical intuition are no more acceptable in philosophy than are appeals to divine authority.

Scientific naturalism has a different history. It arose primarily from the failure of logical empiricism and other philosophical programs that claimed authority to legislate for science. Scientific naturalism emphasizes that science need not accord with philosophically-imposed limits upon what methods, evidence, or ontology is scientifically acceptable. Scientific naturalists make philosophy continuous with scientific work, and permit no impositions upon science that serve philosophical but not scientific ends. For example, if a scientific discipline finds it useful to refer to unobservable entities, then a prior commitment to empiricism gives philosophers no grounds to object to these references.

How do these differences in the force and content of philosophical naturalism matter to my project to make it harder to be naturalist? It is obviously more challenging to uphold the more radical naturalist positions. Radical naturalists need to give arguments that are sufficient to rule out beliefs or concepts that philosophers might otherwise accept. The more tolerant strains of naturalism hold their own dangers, however. While they seem to make naturalism easier to accept, that easy acceptance may lose its content and significance. If a wide range of opposing philosophical positions is all consistent with naturalism, what difference does naturalism make to philosophy, science, or the conduct of one's life?

A more substantial problem arises, however, if we consider the relation between metaphysical and scientific naturalism. These two views are often defended by different philosophers working in different sub-fields of philosophy. Metaphysical naturalism is more widely espoused in philosophy of mind, epistemology, and ethics; scientific naturalism is more common within the philosophy of science. Yet a coherent philosophical naturalism must answer to the concerns of both views. The rightful legacy of metaphysical naturalism is that philosophical understanding cannot appeal to anything supernatural, that is, to what is not part of a scientific understanding of nature. Scientific naturalism also constrains any aspiring naturalist, however. Naturalists should accept no *philosophical* restrictions upon science. No philosophical conception of how science *ought* to be done can block well-motivated developments within a science. The problem is that these two concerns are difficult to satisfy jointly.[3]

We must first ask why naturalists must satisfy both of these philosophical commitments. Consider metaphysical naturalism. We can easily see why

3 For a more extensive discussion of the internal difficulties within philosophical naturalism, see Rouse 2002.

metaphysical naturalists should also accept the central commitment of scientific naturalism. Metaphysical naturalists want to understand human thought and action as part of *nature*: we are natural beings, governed by physical and chemical laws, and shaped by natural selection. But why should we believe *that*? We believe that because we think we have learned this conception of nature and ourselves from the natural sciences. If we have not correctly acquired this conception from the sciences, but instead have imposed it upon the sciences for philosophical reasons, then the primary rationale for metaphysical naturalism dissolves. Metaphysical naturalism would then need a very different, philosophical justification that did not claim the authority of science.

The first problem confronting metaphysical naturalism is the need to justify the scientific authority of its preferred metaphysics of nature. This problem arises because metaphysical naturalism no longer seeks to *replace* philosophical arguments with scientific research. Naturalists instead offer their own philosophical theories about how scientific work enables them to answer philosophical questions about mind, knowledge or morality. In their efforts to establish the scientific credentials of their preferred metaphysics of nature, many metaphysical naturalists appeal to the laws of physics, Darwinian natural selection, or recent cognitive science to defend their philosophical views about mind, knowledge, or morality. But naturalists cannot just take a plausible story about the metaphysics of nature from current scientific theories. They must defend the much stronger claim that scientific understanding *requires* their preferred metaphysics of nature. Otherwise they risk imposing upon the sciences a philosophical theory that science itself neither needs nor wants. They would also risk falsely claiming the authority of science for a philosophical view that science itself need not endorse.

A second problem for metaphysical naturalists comes from their inability to settle upon a single philosophical account of a scientific understanding of nature. For example, some metaphysical naturalists understand "nature" to mean actual objects and their causal interactions. Others understand nature as the domain of natural *laws*, which apply not only to the actual world, but to all mutually accessible possible worlds. Still other metaphysical naturalists think that the *biological* history of natural selection that shaped our cognitive capacities is what matters to philosophy, not physical causes or laws. Alongside these alternative conceptions of nature, metaphysical naturalists also have different conceptions of *how* to "naturalize" rational agency, consciousness, or normative authority. Some philosophers still think that understanding mind, knowledge or morality as naturalists requires eliminating traditional philosophical vocabulary in favor of scientific language. Others argue that familiar philosophical concepts are reducible to or supervene upon a scientific account. The more tolerant conceptions of naturalizing mind and knowledge allow any philosophical account that has predictive utility, or does not overtly contradict current scientific theories.

Perhaps we should not worry about such differences among metaphysical naturalists' conceptions of nature and the place of mind and knowledge in nature. We might have compelling reasons to understand mind and knowledge in scientific terms, even though science has not yet settled exactly which terms we should use for that purpose. That response will not do, however, for it presumes that science *should* eventually settle this philosophical question for us. Yet many philosophers of science now challenge the presumption that the sciences need to resolve philosophical disputes about the proper metaphysical interpretation of scientific achievements. Thomas Kuhn's rejection of this presumption was an important and relatively uncontroversial part of his account of "normal" science. Kuhn insisted that "scientists can agree in their *identification* of a paradigm [for subsequent research] without agreeing on, or even attempting to produce, a full *interpretation* or *rationalization* of it. Lack of a standard interpretation … will not prevent a paradigm from successfully guiding research" (1970, p. 44). More recently, Arthur Fine has argued that philosophers should accept and respect this diversity of scientific opinion. Fine urged philosophers to "try to take science on its own terms, and try not to read things into science.… [We should] tolerate all the differences of opinion and all the varieties of doubt and skepticism that science tolerates, [and] not tolerate the prescriptions of empiricism and other doctrines that externally limit the commitments of science" (1986, 149–50). If Kuhn, Fine, and others are correct, the success of the sciences accommodates a wide range of metaphysical views among scientists, including metaphysical agnosticism. Philosophers' attempts to claim scientific authority for a specific metaphysics of nature may therefore be an unjustified philosophical imposition upon science. Metaphysical naturalists cannot just defend the philosophical advantages of their preferred conception of nature; they must also show its entitlement to the authority of empirical science.

These difficulties confronting metaphysical naturalism may encourage us to emphasize scientific naturalism instead. Scientific naturalism seems to be consistent with a tolerant attitude toward the metaphysics of nature. No naturalist could make philosophical appeals to intuition or divine revelation, but one might accept and engage with the best current scientific work without demanding a specific metaphysics of causality, law, or biological adaptation. If so, it might be easy to be a philosophical naturalist after all.

I am sympathetic to this approach, which is very close to my own. Unfortunately, it makes it look *too* easy to be a philosophical naturalist. To see why, we need to consider more carefully what happens when we combine scientific naturalism with an inclusive and tolerant metaphysical naturalism. Two claims are being defended together:

1) The natural sciences are well-ordered practices on their own; philosophy has no independent authority to prescribe how science should be done;

2) Acceptance of the natural sciences does not require that philosophy use a single favored scientific vocabulary, or explain thought, knowledge, or action within a favored metaphysics of nature.

Philosophy and science would then seem to go together easily. Science does not need philosophical justification, yet a scientific understanding of nature imposes only minimal constraints upon a philosophical understanding of mind or knowledge.

Conjoining these two claims may nevertheless be more difficult than it seems at first. A tolerant naturalism may seem to leave wide scope for philosophical accounts of mind, language, or knowledge. Yet such accounts must also apply to *scientific* thought, scientific language use, and scientific knowledge. In seeking a philosophical theory that applies to scientific work, philosophers once again risk imposing philosophical constraints upon science. Philosophical theories are usually normative: they do not simply report what people say, do, or believe, but propose norms for what they ought to say, do or believe. If we take our philosophical theories of knowledge, thought or language seriously, and accept that they apply to science, why wouldn't we cautiously reclaim philosophical authority over the sciences? We should also ask whether our philosophical conceptions of science implicitly ascribe to us supernatural cognitive capacities, either in fixing the content of scientific claims, or in assessing their justification.

Most naturalistically-inclined philosophers minimize or ignore these concerns. They are confident that their careful, philosophically- and scientifically-sophisticated theories about knowledge, language, or mind will readily account for science, and that scientific understanding can accommodate their theories. Yet the history of logical empiricist, post-empiricist, and other philosophical theories of scientific knowledge should be a cautionary tale. The logical empiricists were equally confident that scientific knowledge was governed by the formal structures and strict empirical accountability that were central to their semantics and epistemology. Careful study of scientific practice later shattered that confidence. Part of what still makes it hard to be a naturalist today is the need to pay closer attention to the relations between philosophical theories of mind, knowledge or language and the history and current practice of science.

Ic: From Nomological Necessity to Causal Interaction

I conclude the first part of the paper by considering briefly how philosophers of science now think about one important aspect of scientific work that bears on the metaphysics of nature. This issue, concerning causality and nomological necessity, nicely illustrates the danger of taking for granted a philosophical conception of science and a scientific understanding of nature. In my earlier historical remarks about the rise of naturalism, I noted the importance of changing attitudes toward causality and necessity. These changes have profoundly affected metaphysical naturalism. Many naturalistic philosophers of mind, language and knowledge have made extensive use of the concept of a natural law, and related concepts such as "natural kinds." When they talk about 'causes', they often use the term interchangeably with talk about causal laws. These are powerful, far-reaching concepts. Many prominent naturalistic theories of mind, language or knowledge employ them extensively and effectively. Indeed, philosophers outside of the philosophy of science often simply identify scientifically-understood nature with events governed by natural laws.

Such appeals to natural laws, or to theories as systems of laws, were encouraged by work in the philosophy of science in the 1970's and 1980's. In responding to the failures of logical empiricism, philosophers of science initially emphasized the unifying role of theoretical laws in explaining diverse events. They also thought that scientific concepts are developed primarily through their systematic role in broadly explanatory theories. Many philosophers of science now think differently about laws and causes, however. I will therefore briefly consider how and why concepts such as 'law' or 'natural kind' have become less central to philosophy of science, and which concepts have begun to replace them.

Post-empiricist philosophers of science were attracted to the concept of scientific laws as empirically-discovered necessary truths for two reasons. Laws seemed important for explanation, by unifying diverse events within a more general pattern. Laws also seemed important in inductive reasoning: new predictions from prior observations are reliable when the concepts they use belong to general laws. Neither explanation nor inductive reasoning seemed to work unless the connections they found were *necessary* connections; yet the failures of logical empiricism suggested that the relevant necessity could not be merely logical necessity.

What changes nevertheless led philosophers of science to give less emphasis to laws or natural necessity? I will emphasize three reasons why philosophers of science are now less attracted to understanding laws as necessary truths: closer attention to biology; a reconception of scientific theory in terms of models rather than laws; and the separation of causes and mechanisms from general laws. Consider biology first. For much of the 20th Century, the philosophy of science was primarily a philosophy of physics and chemistry. If biology was considered at all, it was treated as an immature science because it lacked well-established theories and laws. The growing success of the biological sciences throughout the 20th Century made disregard or disrespect for biology untenable, however. More important, philosophers of biology argued that biologists' success came through modes of conceptualization and explanation that did not employ laws. There are no laws of genetics or biological development, for example. Central concepts in biology, such as 'gene' or 'species' do not fit the standard philosophical conceptions of natural kinds. Some common features of biological systems explain why. Evolution makes biology historically contingent. Biological systems often depend upon complex organization of their components, and their behavior may be sensitive to their environment. These aspects of biological systems seem to require different modes of analysis and understanding that are not conceived in terms of necessary natural laws.

Natural laws and natural necessity have also encountered hard times in the physical sciences, however. Philosophers such as Ronald Giere, Nancy Cartwright, Margaret Morrison and Mary Morgan have argued that the primary work of explanation and conceptual articulation in science is done by families of models rather than laws. Giere (1988) has shown, for example, that classical mechanics is not usually understood and used by physicists directly through a general law expressed by F = ma. Physicists instead learn standard models for various abstractly characterized systems. They analyze real systems by adding corrections and approximations to the models. Often scientists use mutually inconsistent models. Mark Wilson points out that classical physics textbooks

> usually provide accounts that work approximately well in a limited range of cases, coupled with a footnote of the "for more details, see …" type. … [Yet] the specialist texts [referred to] do not simply "add more details," … but commonly overturn the underpinnings of the older treatments altogether. (Wilson 2005, 180–81)

In more complicated settings, scientists will sometimes employ models drawn from logically inconsistent theories to capture different aspects of the same phenomenon. For situations on the borders between classical chaos and quantum mechanical effects, for example, physicists shift back and forth between classical, semi-classical, and quantum mechanical models. In other circumstances, they use different, inconsistent models for different aspects of a more complicated phenomenon.

The models used in these situations are not merely derived from more general theories, either. Consider how Margaret Morrison and Mary Morgan summarize their influential book of philosophical papers on *Models as Mediators*:

> Autonomy is an important feature of models. … Viewing models strictly in terms of their relationship to theory draws our attention away from the processes of constructing models and manipulating them. Both [processes] are crucial in gaining information about the world, theories, and the model itself. (Morgan and Morrison 1999, p. 8)

Recognizing the central role of models in scientific understanding provides a less unified and more haphazard conception of the world than that suggested by the traditional hierarchy of increasingly general laws.

This disunity is strengthened by an increasing emphasis upon causation and causal mechanisms within philosophy of science. Not so long ago, many philosophers treated causation and law as virtually interchangeable concepts. Now philosophers of science often talk about causal relations, causal structures, or mechanisms without presuming that they instantiate general laws. Nancy

Cartwright points out that philosophers of science now "describe a variety of different kinds of singular causal relations … such as hasteners, delayers, sustainers, contributors [with] different counterfactual tests for each of the different kinds of relationship" (Cartwright 2004, p. 242). One reason for the plurality of causal relationships is that causes typically have definite effects only within a larger causal structure. How a cause works depends upon where it is placed. Moreover, models of a causal mechanism mostly help us understand what happens when that mechanism operates normally. Under other conditions, the normal outcome will not occur. Understanding the mechanism will help us understand how its normal operation can be disrupted, but will not always indicate what happens instead when disruptions occur.

Not surprisingly, these three challenges to a conception of nature as law-governed often function together. Scientists can understand complex events in the world by constructing models of simpler causal mechanisms. These models link together causal capacities whose outcome depends upon the overall structure. Models of mechanisms are especially prominent in many areas of biology. Yet scientific understanding of physical systems also often works similarly at all scales, from structured arrangements of molecules, to global climate, to collisions between galaxies. These philosophical and scientific developments challenge traditional philosophical conceptions of nature as organized by laws governing its constituent natural kinds. Nancy Cartwright offered a provocative summary of this challenge:

> [Advocates of law-like natural order] yearn for a better, cleaner, more orderly world than the one that, to all appearances, we inhabit. But it will not do to base our methods on our wishes. (Cartwright, 1999, 12–13).

To base our methods on the world we live in, not the world we wish for, is of course the only acceptable strategy for naturalists.

Part II: Scientific Practices

Philosophical attention to scientific practices is a relatively new and unfamiliar approach to the philosophy of science. Discussions of scientific knowledge are much more familiar. Much work on scientific practice is now being done, however. Philosophers of science in Europe, North America, and Australasia have formed a Society for the Philosophy of Science in Practice that will hold its first meeting in 2007. Philosophers are also relative latecomers to the topic. An emphasis upon scientific practices rather than scientific knowledge has been common among historians, anthropologists, and sociologists of science for some time.

I begin this part of the paper with some brief remarks about practices, because the concept may not be familiar. The central sections of this part then consider three important aspects of scientific practices: causal interaction with the world to create phenomena; conceptual articulation; and the role of laws in scientific practice. The final section returns to the themes of Part I by asking how a philosophy of scientific practices contributes to naturalism.

IIa: The Very Idea of a Scientific Practice

What are scientific "practices"? The concept of a practice has been widely used in philosophy, social theory, and science studies, albeit with divergent interpretations of the core notion of a "practice."[4] The following initial remarks about the concept are thus intended to clarify my own use of the term, and especially to *block* some familiar uses of the term whose implications are at odds with my own account:

1. Practices are activities. They are what people do, rather than their beliefs, or the results of what they do.
2. Practices are interactions with the world around us. We can have false beliefs, or beliefs about things that do not exist. Our practices, by contrast, cannot lose contact with their worldly surroundings. That is because practices *incorporate* those aspects of the world with which we interact. That is true even when participants in the practice misunderstand what they interact with.
3. Many people participate in a practice. People who participate in the same practice need not perform the same activities, or hold the same beliefs, however. Practices can be complicated patterns of activity; different participants may contribute to a practice in different ways. People can also participate in the same practice despite holding different conceptions of that practice. They can disagree about how its constituent performances belong together, about the aims of the practice, or about the stakes in its success or failure.
4. The activities that belong together in a single practice may have no common features. Practices are held together instead by the interactions among those activities. These linked interactions extend in space and time. A practice can therefore change over time, and be done differently in different places.
5. Language use is normally integral to practices, especially to scientific practices. The ability to use words that are repeatable and recombinable in new judgments is crucial to conceptually articulated practices. We understand a concept by understanding how to use it or respond to it appropriately, in the right circumstances. Our responsiveness to conceptual differences nevertheless goes beyond our ability to express those concepts in words.

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4 For an extensive, critical review of the practice literature in philosophy, social theory, and social science, see Rouse 2006.

6. Philosophical attention to practices is an alternative to a philosophical focus upon knowledge. A philosophy of scientific practices can nevertheless accept and use the concept of knowledge. The sciences obviously allow us to know much about the world. Yet we still might understand science better by looking at scientific practices rather at relations between knowers and what is known.
7. Scientific practices include more than just scientists' activities. Many people participate in scientific practices, most of whom are not scientists. Talking about scientific practices also does not require a sharp distinction between scientific practices and other practices. Practices overlap and interact with one another. The term 'scientific practices' only assumes loose historical connections between sciences at different times, and different sciences at any one time.

These initial remarks about the concept of practices have been brief and abstract. These abstract descriptions are intended only to provide some initial guidance and to avoid some simple misunderstandings. The concept will become clearer in the following sections, which consider several aspects of scientific practices specifically.

IIb: Scientific Practices as Causal Interactions

I begin by discussing scientific practices as patterns of *causal* interaction with the world. A salient feature of natural science is the extensive work done in laboratories, observatories, medical clinics, and carefully prepared field sites. For simplicity, I will use the word 'laboratories' to refer to any site of scientific work, even though their differences are important in other contexts. Most philosophers recognize that laboratory work is at least an indispensable means to acquiring scientific knowledge. When considering scientific practices, however, laboratories and experimentation are integral to science, and not merely a means to something else. Attention to practices can even reverse the means/end relationship. The knowledge achieved at one stage of an ongoing research program is often mostly important as a means to further experimental research.

Laboratory work is integral to science, because the world normally does not show itself intelligibly. Scientists must instead interact with things and rearrange them. When that interaction is successful, the world shows itself intelligibly in new respects. This way of talking about scientific work reverses the more familiar empiricist idiom. What matters is not what we can observe in nature, but what the phenomena can show us.

> What is a "phenomenon"? Ian Hacking once said that Old science on every continent [began] with the stars, because only the skies afford some phenomena on display, with many more

> [obtainable] by careful observation … Only the planets and more distant bodies have the right combination of complex regularity against a background. (1983, 227)

Science began with observable phenomena in the night sky, but obviously did not stop there. In most scientific domains, however, very few phenomena in nature already display a clear pattern against a background. Where scientists do not find phenomena in nature, they work hard to create them. When Hacking or I say that scientists create phenomena, we do not mean that such work is dubious or unreliable. Creating significant and revealing patterns in the world is careful, skillful work. It requires extensive understanding of one's instruments, materials, and circumstances. These items are integral components of the phenomenon, and experimenters' skills must use and respond to their causal capacities.

Experimental work involves causal interaction with the world. Scientists are causally effective agents who are part of those interactions. Intelligible phenomena only occur when the causal capacities of their components are properly organized. To this extent, experimental phenomena are mechanisms. Like biological or technological mechanisms, phenomena invite normative assessment. We say an experiment runs "properly" and produces the "normal" or "correct" result, or that there were "mistakes" or "malfunctions." The most common and basic failures result in noise or confusion, rather than clear errors. A clear but misleading pattern is still a phenomenon.

We often describe phenomena briefly and abstractly. We talk about the melting point of a substance, the activation of a gene, or a synthetic pathway for a chemical. Such descriptions usefully highlight the scientific significance of the phenomenon. Yet they also abstract from crucial components of the phenomena they describe. A phenomenon includes all of the causally relevant components of a very complex, regulated interaction. We should remember just how many components must come together properly to produce a revealing phenomenon. The components of a laboratory phenomena typically include properly prepared and contained *materials*; controlled *circumstances* (e.g., temperature, air pressure, or magnetic fields); *signifying* elements (e.g., radioactive labels, biological stains, induced emissions of radiation, or antibiotic resistance); *detectors* for those signifying elements; standardized *measures* (of mass, electrical resistance, time intervals, or work); *instruments* calibrated to those measures; proper *sequencing* of events; skillfully performed or properly automated *techniques*; and above all, extensive *shielding* of these components and events from possible interference.

Recognizing the special circumstances of laboratories suggests a problem for the significance of scientific practices. The sciences seek to understand what happens in the messy, complex world around us. At their best, however, they seem to produce something else instead: a clear, precise grasp of phenomena in isolated, regulated laboratory settings. This problem has no *general* solution. The proper response is to consider what inferences can be drawn from a laboratory phenomenon to other circumstances; understanding when such inferences are good is important to scientific practice.

One further point about inferences beyond the laboratory concludes this part of my discussion. The creation of laboratory phenomena involves scientists in causal interaction with the world. These causal interactions are not confined to the laboratory, however. Inferences from laboratory phenomena to other settings are now more extensive and reliable precisely because scientific practices extend far beyond the laboratory. The sciences guide a massive, continuing effort to engineer the world partly in the image of the laboratory. We are surrounded by laboratory artifacts and procedures in our everyday lives. Our mundane surroundings include purified or synthesized substances; insulated wires in electrical circuits; complex machines and other mechanisms; standard measures and calibrated instruments; carefully timed and sequenced events; and shielding from other causal influences. The world is now more intelligible and predictable, because these extended scientific practices make it so. The scientific practices that make the world intelligible, however, are themselves causal interactions within the world.

IIc: Conceptual Articulation in Scientific Practices

We usually think of scientific progress primarily as the replacement of false beliefs with true beliefs, or beliefs that are more adequately justified. Such a conception of progress reflects a familiar philosophical emphasis upon scientific knowledge. It expresses an important achievement. Most educated people in modern scientific cultures no longer believe, for example, that sick people are possessed by demons, that fire gives off phlogiston, or that the earth is flat and a few thousand years old. Yet scientific practice also enables a more basic comparison to our predecessors. In most domains of science we can now *say* things that people before us could not say. On these subjects, people previously had no beliefs at all, rather than false beliefs. Being able to say what others cannot say is not just learning new words; it requires being able to "*tell*" what you are talking about. As philosopher John Haugeland noted,

> Telling [what something is, telling things apart, or telling the differences between them] can often be expressed in words, but is not in itself essentially verbal. … People can tell things for which they have no words, including things that are hard to tell. (Haugeland 1998, 313)

Science allows us to talk about very many things, by enabling some of us to "tell" about them. Here are some examples. People can now tell and can therefore talk about mitochondria, the pre-Cambrian Era, subatomic particles, tectonic plates, retroviruses, spiral galaxies, and amino acid sequences. Not long ago, people were in silence rather than error on these and many other scientific topics.

How was that silence broken? To hold beliefs and to talk about something, we need concepts that can express those beliefs. Having a concept is not just knowing a word, but being able to "tell" something. W.V.O. Quine used metaphors to express one familiar account of how conceptual articulation occurs in science:

> [Scientific theory] is a human-made fabric which impinges upon experience only along the edges, or a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. (Quine 1953, 42)

For Quine and many other philosophers, concepts are parts of a systematic theory, and are developed or changed by internal adjustments in that theory. I call this view traditional because it regards knowledge as a relation between a system of verbal representation, and something unconceptualized (experience, nature, or "the world"). The world impinges upon us from *outside*, compelling us to adjust the *internal* relations among our sentences or thoughts.

On this view, having a theory allows us to be "articulate," to express thoughts in words. The English word "articulate" now primarily describes a verbal capacity, but it has a more fundamental meaning. Something is "articulated" when it has joints, like the human skeleton. Verbal articulation is simply our most powerful and fine-grained way of finding or (as I prefer) *telling* "joints" or boundaries in the world. Familiar philosophical views of science regard the primary work of articulation as verbal. On one version of this claim, the world is already articulated into kinds of things and properties. Verbal articulation just tries to match words to those kinds. On another version, the world comes more-or-less adaptable to different verbal articulations. I think understanding scientific practices requires rejecting either version. More fundamentally, we must reject the distinction between how the world already is, and how we represent it in words. Science *articulates* the world, allowing us to *tell* about it, by developing new patterns of interaction and new ways of talking, together.

This claim is rather abstract, but some examples may help explicate it. As a first example, consider the rich vocabulary for the internal components of cells and their functions now available in cell biology. Modern cell biology began, many historians would argue, when Albert Claude spun pulverized chicken sarcoma cells in the ultracentrifuge at 28,000 revolutions per minute.[5]

Different materials gradually precipitated; after a week, several layers of cellular debris lay beneath a liquid. At first, Claude could only describe the layers as "small particles" or "large granules," and note when each precipitated. By itself, that is like trying to understand how an automobile works by blowing it up, and sorting the pieces by size or where they land. That does not help you say much about the automobile. Cell biology did better by interconnecting multiple interactions with cell components. Claude analyzed the different fragments biochemically. Later, he and his successors identified layers in the ultracentrifuge with

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5 For more extensive discussion of this example, see Bechtel 2006, Rheinberger 1995.

features visible through light microscopes, and then with electron microscopes. Connecting these experimental, structural, and biochemical interactions with cell components helped locate biological functions like respiration or protein synthesis. Cell biology then had a very good start. Empiricists might think the key work came earlier, when some cell structures became visible in light microscopes. That is a mistake. Microscopes alone cannot indicate whether or how the boundaries they make visible are biologically meaningful. The visually identifiable elements need to be robustly connected to other interactions with cells. If not, they could be mere artifacts. The stains and instruments that make them visible, or the prejudices that equate visibility to us with importance, might lead science astray.

Consider another deceptively simple case that I adapt from Hasok Chang (2004). What does it mean for one thing to be hotter than another? We distinguish temperature from quantity of heat, but what is temperature? People noticed that hotter and colder correlate with expansion and contraction, and constructed thermometers. Is temperature simply whatever a thermometer measures? No. I ignore the real difficulties of defining some fixed points, such as the freezing and boiling points of water. With these points fixed, however, suppose we mark 100 equal lengths between them on a thermometer. Here are some different measures of the same temperatures with mercury, alcohol, and water thermometers:

Mercury	0°	25°	50°	75°	100°
Alcohol	0°	22°	44°	70°	100°
Water	0°	5°	26°	57°	100°

(from Chang, 2004, 58)

How should we understand these differences? Is there one concept of temperature, or many, or none? Or is temperature a purely conventional concept? Perhaps scientists could just agree to use mercury or alcohol as a standard measure. Yet we also want to understand temperature when these thermometers would melt, or their contents freeze. What is a "degree," and what is it a degree *of*, at 1000º, 1,000,000º or −250º?

I have three points to make about this example. First, scientists were able to define a unified scale of temperature *experimentally*, and use it to assess the accuracy of various thermometers. This achievement did not primarily involve internal adjustment within a theory. Second, defining the concept of temperature more precisely required connection to another experimental and practical domain, in which steam engines allowed correlations of heat with a capacity for mechanical work. Third, however, the resulting unity and coherence of the concept of temperature is highly unusual among empirical concepts. Consider by contrast the question of what it is for one solid material to be harder than another, for example. Mark Wilson (2005) reminds us of many partly conflicting empirical measures of hardness. Is hardness best displayed by resistance to denting, to scratching, to cutting, or to friction? Here there is no prospect of unifying different measures into a single scale. Materials can be harder or softer in many ways, which only partly overlap. Yet like temperature, articulating the concept of hardness involved creating phenomena more than redefining words or formulating theories.

A defender of familiar accounts of concept articulation might now object. Not all conceptual developments in the sciences are as straightforwardly empirical as temperature, hardness, or cell structure. Internal adjustments within theories might still be the main form of conceptual articulation in science. I accept the premise of this objection, but reject the conclusion. The role of theories in conceptual development does not imply that conceptual articulation is primarily verbal, because theories are not primarily verbal either. Recall my earlier discussion philosophical work on models. Theories do not just connect a general verbal representation to a situation in the world. Scientific theorizing is better understood as a *practice* of modeling various actual or possible circumstances. Although scientists sometimes formulate general theories, from classical mechanics to thermodynamics, we learn what those theories and equations say by developing and using families of models. How do we recognize the relevant forces, masses and accelerations in $F = ma$ in classical mechanics, for example? We model specific situations, such as free fall, harmonic oscillators, or planetary orbits. We then understand more complex situations by comparison to the models, with appropriate corrections or complications. Models are even more important in sciences that describe complex interactions. When we think about the workings of a cell on a small scale, or the dynamics of global climate on a larger scale, models become stand-ins for the actual systems we seek to understand. We cannot comprehend such complex interactions except with more simplified models.

Understanding theoretical modeling helps us recognize that models and experimental phenomena play similar roles in articulating concepts. Theoretical models and experimental phenomena each establish simplified, idealized settings in which conceptual relations can be clearly displayed as telling differences. First, differences among various components or factors in a situation can be identified and highlighted within a model or phenomenon. Second, their dominant modes of interaction stand out more clearly in the simplified or idealized circumstances of the model. Understanding how the model works provides an indication of where to look and how to intervene in “real” circumstances for scientific or practical purposes.

This treatment of conceptual articulation has been long and detailed, so I conclude this section with a brief summary of its contribution to understanding the sciences as practices. Familiar images of science focus upon comparisons between knowledge claims and the world. These comparisons would stand on their own if we could independently determine what these claims say, and whether they are true. Our most familiar conceptions of knowledge do treat them independently: understanding what knowledge claims mean is mostly verbal and internal to theory; assessing truth requires looking at the world. There is no clear separation between these tasks, however. We only understand scientific concepts by interacting with the world in appropriate ways. Understanding what these concepts say, and understanding the world in those terms, go hand in hand. Moreover, by creating and using experimental systems and theoretical models, we transform the world to let it show itself intelligibly to us. My discussions of experimental practice and conceptual articulation thus make the same point from different angles. Scientific practices are disciplined patterns of causal interaction that transform the world to articulate it conceptually.

IId: Laws in Scientific Practice

As I noted in the first part of the paper, philosophers of science now discuss diverse kinds of causal interaction. We are less inclined to talk about laws of nature. Many metaphysical naturalists, however, assume that nature is understood scientifically as the domain of laws. The distinction between necessary laws and merely contingent truths seems to provide useful resources for philosophical theories of mind and knowledge. These differences mark one of the tensions that make it harder to be a philosophical naturalist. Metaphysical naturalists are not entitled to understand laws as necessary truths if this conception is not part of a scientific conception of nature. Scientific naturalists, on the other hand, should not abandon the concept of law if the sciences do find it useful. In that case, a conception of "science without laws" would be an unwarranted imposition upon science.

To resolve this tension, consider how scientists understand "laws" in scientific practice. Marc Lange (2000) argues that laws work differently in scientific practice than philosophers usually recognize. Most philosophical discussions of laws treat them as a special kind of truth, such as "nomologically necessary" truth. Lange argues instead that laws have a special use in scientific practice. A scientist who regards a hypothesis as a law undertakes a strategy of inductive reasoning. Laws would support inferences from a small number of examined cases to predictions about unexamined cases. If the examined cases are instances of a law, we have good reason to expect later instances to behave in the same way. Such inference strategies are not always reliable, and must sometimes be revised in light of later evidence. Yet without committing to some inductive strategies, and the laws that express them, scientific research could not proceed. If the events we study in the laboratory or elsewhere did not tell us about events elsewhere, scientific research would be pointless.

The familiar problem, however, is that many inference strategies are consistent with any given data. Lange argues that scientists consider which possible inference strategy is *salient* in context. A salient inference strategy suggests an appropriate scope. For example, inferring from experiments with copper wires in Taipei that "all copper objects *in Taipei* conduct electricity" would be too narrow; "all solid objects conduct electricity" would be too broad. Geography is not a salient aspect of these experiments, but copper is. A salient strategy also makes no unmotivated changes in later applications. Suppose we do some elementary experiments with the pressure, volume and temperature of gases. We will likely discover that P, V, and T vary in a fairly constant relationship, expressed by PV = kT (where k is a constant). To infer that the product PV will *increase* if we raise the pressure substantially would introduce an unmotivated change. I will return to this example of Boyle's Law shortly.

> I first need to say more about "salience." Lange rightly insists that [the salience of an inference strategy] is not something psychological, concerning the way our minds work. … [Rather] it possesses a certain kind of justificatory status: [like] observation reports, this status requires that there be widespread agreement, among qualified observers who are shown the data, on what would count as an unexamined [case] being relevantly the same as the [cases] already examined. (Lange 2000, 194)

Comparing inference strategies to observation reports is instructive. The salient patterns of experimental phenomena, and the salience of an inductive strategy expressed in laws, play related roles in scientific understanding. Experimental phenomena, and the inductive strategies that extend them beyond the laboratory, work together to articulate the world conceptually. Moreover, the salience of each pattern has a normative status within scientific practice. Such patterns are defeasible, but they offer default justification. They should be accepted unless there are good reasons not to do so.

We can now return briefly to Boyle's Law of gases as an example. Boyle's Law has a problem common to many familiar empirical laws. Strictly interpreted, this "law," PV = kT, is false. The problem dissolves, however, when we think about scientific practices. Practices are patterns of interaction with the world. Because we are part of those interactions, their norms refer to us. In this case, the inference strategy expressed by this law is sufficiently accurate for some scientific purposes. For these purposes, Boyle's Law is both salient and reliable.

Which laws are salient also depends upon us in other ways. Different background assumptions can change which inferences are appropriate. Boyle's Law is inductively salient from how gases behave at "ordinary" pressures; predicting that the product PV rises at higher pressures would be unwarranted. Some plausible background assumptions can change our inferences, however. Suppose that gas molecules occupy part of the volume of their container, and attract one another at very close distances. A different strategy then suggests itself. On these assumptions, the van der Waals law becomes salient, and *Boyle's* Law suggests an unwarranted change in our inductions at higher pressures. Under other assumptions, even the van der Waals law becomes unreliable. Yet Lange rightly argues that both should be recognized as laws, because stricter assumptions would permit no general gas law at all. To overlook the salience of Boyle's and van der Waals' laws for certain purposes would impose unreasonable restrictions on scientific reasoning and explanation.

The aims of scientific disciplines also affect which laws express salient strategies of induction. Consider biology. Earlier, I noted that many philosophers of science now think laws play little part in biology. Biological systems evolve contingently and variably, so there are no necessary truths of biology. Lange argues that laws *are* important in biology, however, if we understand laws as related primarily to norms of scientific inference. In biological disciplines like molecular genetics, developmental biology, physiology, or medicine, scientists do draw inferences from data about one organism to others of the same species. When scientists sequence the genome of model organisms like yeast or *Drosophila*, for example, the sequence functions as a genetic law for all members of that species. For the purposes of molecular genetics, the widespread genetic variation among individual organisms is rightly ignored. In evolutionary biology and population genetics, however, that variation is important. In those disciplines, different laws express salient strategies of inference.

These references to scientists' purposes and assumptions do not compromise the objectivity of science. Scientific practices are not activities that we impose upon nature from "outside." They are instead patterns of causal interaction with nature. How a scientific practice develops is determined neither by us alone, nor by how the world is apart from us. A practice is instead shaped by ongoing interaction between scientists and the world. The "purposes" of the practice emerge from within that interaction. To show how this happens, let's return briefly to Albert Claude's experiments with the ultracentrifuge.

I mentioned earlier that Claude's experiments were the beginnings of the modern discipline of cell biology. That is not how Claude conceived his own work at first, however. Claude worked in a medical school, trying to understand cancer. He put chicken sarcoma cells in the ultracentrifuge to discover how cancer cells differ from normal cells. When he compared the debris from the two kinds of cell, he found no differences between them. This result was not a failure, however. The differences among the layers of cellular debris were instead a striking and salient result. This result suggested new lines of inquiry and new inferential strategies that eventually came together in a new discipline, with new goals. When Claude pursued those new directions, however, he continued to use his familiar cancer cells. When he started, these cells were treated as pathologically abnormal. For the new purposes emerging from his research practice, cancer cells were cells like any other. The incipient "laws" of cellular structure and function were simply more salient than the cancer pathologies that first motivated the research. Their salience was neither an objective feature of cells, nor a subjective imposition by scientists, but an emergent feature of their interaction in scientific practice.

IIe: Scientific Practices and Philosophical Naturalism

We can now, at long last, return to the topic of naturalism. In the first part of the paper, I claimed that it is more difficult to be a naturalist than many philosophers recognize. There are many varieties of naturalism in philosophy. Both scientific naturalism and metaphysical naturalism make legitimate demands upon naturalists. Scientific naturalism demands that we not impose philosophical constraints upon science. Metaphysical naturalism demands that we understand mind, knowledge, and language as part of scientifically-understood nature. The difficulty is that these demands are often in tension with one another.

In this part of the paper, I have introduced a less familiar way to think about the sciences philosophically. Instead of focusing upon scientific knowledge, I invited you to think about the sciences as practices. I discussed three aspects of scientific practice. First, the sciences involve causal interaction with the world. Such interactions allow scientists to create phenomena. Phenomena are arrangements of instruments and materials, often novel arrangements, that allow the world to show itself in revealing ways. Second, by creating phenomena, the sciences also articulate the world conceptually. We can now talk about many previously inconceivable aspects of the world. We can do so, because new scientific concepts express telling differences that show up clearly in the phenomena. Systematic interconnections among these concepts give them content and enhance their reliability. Third, scientists extend these concepts to apply beyond the immediate experimental context through strategies of inductive inference. These strategies point toward a different conception of scientific laws. Laws are not necessary truths. In their scientific uses, laws express inference strategies that apply concepts within scientific practices. The scope and content of the laws reflect salient strategies of ongoing interaction with the world.

How does this conception of scientific practices contribute to naturalism in philosophy? A philosophy of scientific practices is first and foremost a version of *scientific* naturalism. It aims to avoid unwarranted philosophical impositions upon science, by attending more closely to what scientists say and do. My discussion of scientific practices today highlighted aspects of scientific work that many philosophical discussions of science often overlook. One under-emphasized aspect of science is the causal interactions that create phenomena. Scientific practice is part of the causally interactive world that science discloses. I also call attention to the future-orientation of scientific research that articulates the world conceptually and extends those concepts inferentially. Philosophers more commonly focus upon the retrospective justification and systematization of scientific knowledge.

A philosophy of scientific practices also adopts what initially seems to be a tolerant metaphysical naturalism. Along with many philosophers of science now, this approach takes causal interactions at face value. Human beings understand causal relations through our own causal involvement in the world as embodied agents. We do not need natural laws to explain the difference between genuinely causal interaction and merely accidental correlation. Yet causal interactions take many forms, and function together in more complex causal patterns. Causality is not a univocal concept, and there is not one privileged mode of causal relation that science discovers. Recognizing the diversity of causal relations, we will not be tempted to try to impose an austere and restrictive vocabulary upon philosophical understanding. Nor will we think that such metaphysical views can claim the authority of the natural sciences. In metaphysics and philosophy of science, a philosophy of scientific practices joins Arthur Fine in "tolerating all the differences of opinion and all the varieties of doubt and skepticism that science tolerates" (Fine 1986, 150).

Yet a naturalist philosophy of scientific practices also takes a radical stance in other respects, ruling out some familiar philosophical positions. I conclude this section of the paper by briefly suggesting just how radical this form of scientific naturalism may be. Giere (1999, ch. 5) and others have noted that the concept of natural laws as necessary truths arose within a theological understanding of nature as God's creation. God was the legislator who laid down the laws, and whatever happens must obey them. Giere criticized this conception of natural law, arguing that naturalists should not ascribe universality and necessity to scientific understanding. These features of a metaphysics of natural law belong to an earlier theological conception, which naturalists should avoid.

While I endorse such criticisms of a metaphysics of natural law, I also think a more basic trace of a theological conception remains in many philosophical accounts of science and nature. A theological conception of God as creator places God outside of nature. God's *understanding* of nature is also external to the world. Such a God could understand his language and his thoughts about the world, apart from any interaction with the world. Naturalists long ago removed God from scientific conceptions of the world. Yet many naturalists still implicitly understand science as aiming to take God's *place*. They interpret science as trying to represent nature from a standpoint outside of nature. The language in which science represents the world could then be understood apart from the causal interactions it articulates. A philosophy of scientific practices denies that such an otherworldly understanding of nature is possible. Scientific concepts and scientific understanding are situated in the midst of ongoing causal interaction with the world. That is why I talk about conceptual articulation in science rather than theoretical representation. We understand scientific concepts only by understanding the phenomena they articulate. We find ourselves in the midst of the world, and cannot understand it except from within. That is the radical vision of a naturalistic philosophy of science expressed in my "concluding scientific postscript."

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