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Note to readers: the final published version will likely differ from the present version in several respects. The final version may omit some material in the present version and will contain additional references to other papers in the edited volume.

**Sketch of some themes for a pragmatist philosophy of science[[1]](#footnote-1)**

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1. **Introduction**

This paper sketches, in a very partial and preliminary way, an approach to philosophy of science that I believe has some important affinities with philosophical positions that are often regarded as versions of “pragmatism”. However, pragmatism in both its classical and more modern forms has taken on many different commitments. I will be endorsing some of these and rejecting others—in fact, I will suggest that some elements prominent in some recent formulations of pragmatism are quite contrary in spirit to a genuine pragmatism. Among the elements I will retain from many if not all varieties of pragmatism are an emphasis on what is useful, where this is understood in a means/ends framework, a rejection of spectator theories of knowledge and skepticism about certain ways of thinking about “representation” in science. Also skepticism about ambitious forms of metaphysics. Elements found in some previous versions of pragmatism that I will reject include proposals to understand (or replace) truth with some notion of community assent and skepticism about causal and physical modality. It is possible that the particular collection of ideas I will advocate is sufficiently idiosyncratic that it is held only by me and no one else. But in the end, I don’t much care whether the views I will be describing count as an instance of “pragmatism” or something else; what I do claim is that they have some internal coherence and some motivation in ideas found in writers like Peirce, James, Dewey and more recently, Hitchcock, Mitchell, Price, and Wilson among others.

**2. Orienting Remarks**

First, some orienting remarks. As I see it, philosophy of science – and in particular what is sometimes called general philosophy of science-- is under pressure as a discipline. On the one hand, the interests of many philosophers of science have shifted strongly to philosophy of the specific sciences and there is general acceptance of the idea that to do philosophy of science credibly one has to know a substantial amount about one or more particular scientific discipline. Learning such science can be a great deal of fun but there are dangers. One is that the more specialized philosophy of science becomes, the more fragmented it becomes. In consequence, it becomes more and more difficult for these different specialists to talk to one another, not to mention interact meaningfully with the rest of philosophy. And like it or not, philosophy is going to continue to be the discipline in which most philosophers of science will get jobs for the foreseeable future[[2]](#footnote-2).

Another danger with an exclusive focus on philosophy of the particular sciences is that in practice this sometimes leads to products that look like “mere science reporting” or “science journalism”—virtually all of the philosopher’s energy goes into mastering some cool body of science and describing it to philosophical audiences. The distinctive contribution of the philosopher—the intellectual “value added”—looks rather minimal, even if we agree that there is some value to knowledge transfer and intellectual arbitrage. When I speak of “the distinctive contribution of the philosopher” I do not mean that this needs to look like philosophy, traditionally understood. I mean only that it would be desirable for the philosopher to add something that is intellectually valuable in addition to the science that is reported. This might be something distinctively philosophical but if not that, it might, for example, consist of an analytical framework that allows one to see the science in a new light or to methodological issues surrounding it.

However, if we look to so-called general philosophy of science (as currently practiced) as one possible source of tools or ideas that might be employed in providing this value added, the results seem, with some conspicuous exceptions, underwhelming. The grand narratives associated with Popper, Kuhn and Lakatos seem stale and played out and in any case were largely pitched at far too high a level of abstractness and generality (and assumed far too much uniformity in the practice of science) to be useful in the present context. The analytical tradition associated with writers like Hempel is also in many respects unsatisfying, in part because of its lack of engagement, at least in many respects, with the content of real science— there is too much first-order logic, theory T, and “All ravens are black” and similar stuff. (I acknowledge that I’m caricaturing—for example Hempel himself had interesting things to say about the philosophy of the social sciences and psychiatry. However, caricatures can convey truths.)

Enter the siren song of metaphysics: Perhaps philosophers of science should do the metaphysics of science (or they should replace what they are doing with the metaphysics of science or what they were doing all along was metaphysics of science, often badly, but they can do it better by adopting the ideas of specialists in metaphysics). An apparent attraction of this suggestion is that it directly addresses issues about how what philosophers of science are doing differs from science reporting (philosophers of science are doing some variety of metaphysics) and it provides an avenue for restoring connections with portions of the rest of philosophy. But what should philosophers of science do if they want to preserve some of the concerns that animate general philosophy of science, but also don’t want to do metaphysics of science, at least as this as currently practiced by many analytic metaphysicians[[3]](#footnote-3)?

**3. Minimal vs. Ambitious Metaphysics**.

At this point some clarifications/distinctions are needed. “Metaphysics” as currently practiced is a very heterogeneous activity. At an extremely abstract level, one might think of metaphysics (or ontology) as having to do with what is “out there” or what “exists”, Understood in this way, the claim that there are electrons is (I suppose) a metaphysical claim. If one holds, as I do, that causal relations hold in nature, independently of our projective activities, this is again a metaphysical claim. By the same standard, the denial of this claim, according to which causal relations are not out there but rather something that we project onto nature is also metaphysics. Of course, this understanding of the subject effectively makes it impossible to avoid doing metaphysics. In any case, claims about the reliability of various inference procedures which I take to be one central concern of a pragmatist philosophy of science (see below), rest on claims about what is out there in nature; such procedures are reliable to the extent that they discover *truths* (or near truths) about the world. Similar commitments are present when one attempts to *explain* why various procedures and patterns of thinking are reliable since this inevitably involves appeal to the presence of worldly structures that support such procedures and patterns. However, according to the version of pragmatism that I favor, these facts about what the world is like are ordinary, contingent empirical facts that are discoverable by science. One might argue about whether claims about such facts are usefully described as “metaphysics”, but assuming one decides to so characterize them, I suggest that we use the phrase “minimal metaphysics”.

I contrast minimal metaphysics with metaphysics of a more ambitious or expansive variety. I won‘t try to provide necessary and sufficient conditions for the latter but here are some diagnostic features: 1) Appeals to special entities and relationships that seem to play no role in science or in ordinary empirical discourse such as “relations of necessitation between universals” claimed to underlie and explain facts about the obtaining of empirical regularities (Armstrong, 1983 , 2) Claims that in addition to ordinary scientific explanations, “metaphysical explanations” are also required to “ground” various scientific or ordinary empirical claims or practices, as with Loewer’s (2012) claim that we need a metaphysical explanation of laws of nature, where this is explicitly contrasted with ordinary scientific explanation. 3) “Reconstruction” projects that have no obvious motivation internal to science but are instead motivated by extra-scientific concerns. A prominent example is Lewis’ (1986) attempts to reduce laws of nature to facts about patterns in a Humean mosaic consisting of facts about the instantiation of intrinsic, perfectly natural properties at spacetime points. (I claim there is no obvious scientific reason for attempting to do this.) 4) Worries about what “really” exists, where the modifier “really” imports some non-ordinary standard for existence, as when it is claimed that tables do not really exist, and all that really exists are atoms arranged table-wise[[4]](#footnote-4).

My point in introducing this contrast is not to argue (at least here) against expansive metaphysics but rather to urge that it is different from minimal metaphysics and that a pragmatic philosophy of science needs only the latter and not the former. My aim in what follows (and I apologize that this sounds so grandiose and pretentious) is to sketch, in a very preliminary way, some fragments of a picture of what philosophy of science might be or what philosophers of science might do, that relies only on minimal and not ambitious metaphysics but also that does not return us to the bad old days of theory T and “All ravens are black”.

As already suggested, my organizing idea is that this will take the form of a *pragmatist* philosophy of science. This formulation is intended to highlight the twin projects of (i) *description/interpretation* and (ii) *methodology* as a normative enterprise that is central to philosophy of science. Insofar as these were always concerns of general philosophy of science, it attempts to retain some continuity with and to revivify this enterprise. As suggested below one way in which such a connection with general philosophy of science might be retained is through a focus on common methodological problems and issues that occur across different scientific disciplines. For example, there are common methodological problems concerning causal inference from observational data that recur across disciplines as different as economics and neurobiology. These are natural foci for generalist philosophers of science. Investigating these problems requires a lot of attention to specific details – thinking in terms of deducing observation O from hypothesis H is an unhelpful level of abstraction. Nonetheless at least some of the relevant details are common across different disciplines. So in this respect we have “generality” (as in “general philosophy of science”) but not the kind of generality associated with theory T and HD models of confirmation. And we have a kind of engagement with details that (because it involves focusing on problems that recur across different areas of science) is different from the engagement that is present if one focuses only on the philosophy of some specific science such as biology[[5]](#footnote-5).

A second, related way in which some measure of generality might be retained is through a focus on developing analytical tools that allow for perspicuous description/ interpretation of specific strategies, structures and argument patterns common to different areas of science. For example, a common problem in many areas of science from physics to biology arises from the use of a plurality of models in these disciplines that capture features of systems of interest at different scales (of length, time, energy) but where the relations or connections of these models is often very unclear. Classic Nagelian models of reduction seem descriptively inadequate and unhelpful for understanding such relationships. Scientists working on problems of relating models at different scales use language like “getting models at different scales to talk to one another” or “to exchange information” but these are just metaphors—a worthwhile task for philosophers of science would be to develop crisper tools for describing model relationships in such cases. Once we have more appropriate descriptive tools and a better understanding of the variety of strategies employed in different areas of science to connect models at different scales, we also be in a better position to offer methodological advice, since, depending on empirical details, some connecting strategies are more likely to be successful than others[[6]](#footnote-6). Again, this involves much more than mere science reporting, but the results need not involve ambitious metaphysics[[7]](#footnote-7).

The remainder of this paper is organized as follows. Section 4 sets out some overall themes and commitments of a pragmatist philosophy of science. Section 5 discusses the contrast, developed by Huw Price, between subject and object naturalism and its relevance to philosophy of science. Sections 6-7 discuss and illustrate the role of means/ends reasoning in a pragmatist philosophy of science. Section 8 discusses representation and Section 9 realism versus instrumentalism from a pragmatist perspective. Sections 10 and 11 take up some additional issues having to do with the “ontological commitments” of scientific theories and the status of modal claims.

1. **Some Themes**.

**4.1) Usefulness, Utility and Success.** I take pragmatism to involve a focus, first of all, on usefulness and utility, on what “works” or does not work. Applied to science, this involves a commitment to the idea that at least sometimes, scientific investigation works *successfully* to deliver information of various sorts about nature. The pragmatic philosophy of science I favor seeks to understand how this is possible—what sort of information is so delivered, what the methods and strategies are that are involved in its production, and how it is that these methods are successful to the extent they are, which in turn requires understanding the structures in nature (again understood in terms of minimal metaphysics) that support such success. To adopt a common characterization, this involves an *instrumental* approach to science—instrumental in the sense that theories/ models and various scientific practices of reasoning and intervening are seen as instruments or tools for the achievement of various purposes and evaluated accordingly. However, as we shall see, this does *not* involve a commitment to *instrumentalism* in the sense in which philosophers understand this notion.

**4. 2) Description and Evaluation**. As I see it, pragmatism as applied to philosophy of science has both a descriptive or interpretive component – characterizing accurately relevant aspects of scientific practice and reasoning—and an evaluative or normative component: we want to understand why various methods and strategies are successful in delivering knowledge (why they “work”), to the extent that they do, but also to identify cases in present methods and strategies fail to deliver what they claim to deliver and how, in the light of this, these might be improved. Thus critical evaluation of methods and approaches currently employed in various areas of science (and not just description of these) is an important element in the normative part of this enterprise. I see these as falling under the general rubric of *methodology[[8]](#footnote-8),* which is central to pragmatist philosophy of science as I conceive it.

**4.3)** **Means and Ends**. The enterprise described under **4.1**) and **4.2**)—and particularly its normative component-- is naturally pursued within a means/ends framework: Investigators have certain ends or goals that are part of the scientific enterprise. A very partial list includes but is not limited to successful prediction, causal analysis and explanation, exploring the mathematical structure of various theories and models, description and classification for specific purposes, and building and making things[[9]](#footnote-9). (These goals may be further subdivided – e.g. there are a number of different possible predictive problems with different structures.) Investigators employ various means (methods, strategies etc.) for achieving those goals. These means can either be well-adapted to or conducive to these goals or not and both description and evaluation should be pursued in the light of this. In other words normative assessment proceeds by asking whether various possible means are effective in reaching our goals, with ineffective means being discarded in favor of ore effective ones. Means/ends analysis is discussed in more detail in Sections 6-7 but examples of this sort of approach can be found in much of classical (frequentist) statistics, in formal learning theory, as illustrated by work of Kelly (e.g., 2007) and Schulte (2017), in machine learning (for a causal inference application see Janzig et al., 2012) and (in connection with issues having to do with variable choice) in Hitchcock, 2012 and Woodward, 2016.

Features of scientific inquiry that can be profitably understood within a general means/ends framework include: procedures for hypothesis-testing and inferring from evidence to different sorts of hypotheses, issues having to do with choice of variables or vocabulary for framing theories, many issues concerning the structure of scientific explanation and causal analysis, and many aspects of modeling practices, including strategies for “backgrounding” or “neglecting” certain features of systems in order to better understand others, as well as the idea that models can often function so as to provide information about nature, while not always functioning so as to mirror or picture nature.

Thus a central part of the pragmatist program is to apply the means/ends framework at many different levels and to many different problems in philosophy of science. The picture is thus pragmatist twice over—science itself is thought of as a pragmatist, means/ends enterprise and we use this pragmatist framework to understand and evaluate it. What results is not a single grand narrative or single overarching treatment of science in the manner of Popper, Kuhn or Lakatos. Rather, various particular and specific practices of inference, modeling and so on are understood with a means/ends framework. For this reason, it is hard to fully convey what this approach involves in a brief summary; rather whatever value or insight the approach provides requires illustration on a more piece-meal, case by case basis—in connection with the evaluation of particular strategies and practices pursued in specific empirical contexts.

**4.4) Description and Evaluation Together: An Analogy.** As I have been emphasizing, on this approach to science description and evaluation should work together. As a partial illustration/analogy for how this might work consider theories of human vision as developed by vision scientists. (This analogy is due to Alison Gopnik.) Human beings form beliefs about aspects of their environment on the basis of visual input and vision scientists wish to understand the processes and mechanisms that underlie this. But human beings do not just form beliefs on the basis of visual inputs, they are often pretty good at forming reliable beliefs on this basis—that is to say, the visual system is fairly successful (within certain limits) in providing accurate information about the visual environment (which is not to say that the visual system is in the business of constructing an internal representation which is isomorphic to every feature of the environment, a point that should be kept in mind when think about how scientific models incorporate information about nature) . Part of what vision scientists want to explain is this success—what is consists in, how it is possible. Doing so has often taken the form of formulating “ideal observer” or other sorts of normative theories of visual processing—theories that look to understand how (that is, in virtue of what input information and subsequent processing) it is possible to extract information that in at least some respects is fairly reliable about the visual environment. One tries to formulate both high level and much more specific goals that the visual system or components of it have and then tries to understand the means by which the system achieves these goals (to the extent that it does)—for example, the computations that the visual system employs and the algorithms that realize these computations. The result is both a descriptive account of the operation of the visual system and a kind of evaluation in the sense that it provides material for assessing how well the system does in achieving these goals. The normative theory or theories furnish a framework or benchmark for understanding what is going on descriptively. They also can provide an explanation for why the visual system succeeds to the extent that it does. Note that the source of normativity here (as with the other examples I have mentioned) has to do with assumptions about ends or goals; the end of providing accurate enough information about the visual environment provides the basis for assessing the computations that the visual system employs in achieving this end.

In recent years some psychologists have adopted a similar sort of approach to understanding more cognitive activities including causal cognition. Given certain goals associated with causal cognition, one can formulate “rational learner” accounts which explore which strategies of learning from evidence and of reasoning will conduce or best conduce to those goals. One can then investigate empirically whether and to what extent various kinds of subjects—human adults, small children, non-human animals—follow those strategies and procedures and to what extent they do not. The result is both a descriptive account of aspects of causal cognition and an account that has an evaluative or normative component. This sort of work has been carried out by psychologists like Gopnik, (e.g., Schulz et al., 2007) Patricia Cheng (Carroll and Cheng, 2010) and Josh Tenenbaum, among others[[10]](#footnote-10).

What I am recommending is that a pragmatic approach to philosophy of science should follow a similar path.

**4. 5) More on Evaluation**: My emphasis on methodology and the normative element in a pragmatic philosophy of science may be disquieting in the sense that it recalls another feature of the bad old days—the willingness of some philosophers of science to dismiss large swaths of scientific theory on the grounds that these violate favored *apriori* methodological principles. (Recall, for example, Popper’s dismissal, at least at one point, of evolutionary theory as unscientific because unfalsifiable, Hempel’s insistence that explanations in biology, the social sciences and history should ideally appeal to “laws of nature” -- a requirement rarely if ever met in these disciplines-- and claims that theories that appeal to “unobservables” are unscientific or at least should be replaceable in principle by theories that do not have this feature.) What I have in mind is different in several respects. First, the sort of methodological inquiry I have gestured at above involves hypothetical rather than categorical imperatives. They take the form: if you want to achieve goal G in circumstances C, you should employ means X and not means Y, where this leaves open the possibility that some different means should be employed in circumstances different from C. (If the empirical circumstances characterizing biological investigations are different from those characterizing investigations in high energy physics, as they clearly are, different methods may be appropriate.) By contrast *apriori* methodological requirements are usually understood as a categorical and (if any attempt to is made to justify them) they are often taken to follow from the nature of “rationality” or to be requirements of “logic” in some broad sense. Alternatively they are taken to be vindicated by some form of rational intuition[[11]](#footnote-11). As a result the requirements are understood as holding independently of empirical circumstances and as not subject to ordinary means/ends assessment.

The hypothetical imperatives relevant to methodology can be established in a variety of ways including mathematical demonstration, heuristic argument, simulation, and empirical investigation (see below, Sections **6-7**). For example, in some cases one can prove, mathematically, that if one wants to achieve certain goals, certain means will lead to those goals and others will not—this is illustrated by the choice of an estimating procedure with good error characteristics discussed in Section 7 .

As an empirical matter, methodological criticism and advice seems more appropriate (and needed) in some areas of science than others. Pragmatist philosophers should thus focus on the most appropriate areas. Perhaps (I’m not in a good position to say) physicists have their house in order, methodologically speaking. But in areas of science in which I am most interested—e.g., problems of causal inference and reasoning in the social, behavioral and bio-medical sciences – there are uncontroversial arguments (some of them of a mathematical nature, some of them quasi- empirical) that sub-optimal or fallacious methods are employed with some frequency[[12]](#footnote-12). Pragmatist philosophers of science should not be afraid to draw attention to this—indeed this (along with suggestions for improvement) is one of the primary ways they can be useful.

**4.6.) The centrality of action and interaction with the world in science and in knowledge acquisition more generally**. A common although far from universal theme among many pragmatists, emphasized particularly by Dewey, is a distaste for “spectator” theories of knowledge. We don’t just passively observe the world, we often can *act* on it, changing and manipulating it. It is not just that the world causally affects us—e.g., through perception; we causally affect the world. This matters a great deal for the practice of science. In the context of philosophy of science, pragmatists will thus attach a great deal of importance to the role of experimentation and to activities of making or constructing or building (making includes everything from the construction of instruments to synthesis of new chemical compounds or in the case of social science, the creation of new institutions.) These activities are legitimately part of science and are not to be dismissed as of “merely pragmatic” significance or as unimportant because they are only part of “applied” (as opposed to “pure”) science. Experimentation, construction, and applications of science should be thought of sources of information about the world that are incorporated into and help to structure our models and theories—the results of such manipulative activities tell us something about what the world is like. Thus, as good pragmatists, we should resist putting “merely” in front of pragmatic and also resist sharp distinctions between “pure” and “applied” science—more on this below[[13]](#footnote-13).

**4.7) Modal knowledge**. The role of action and associated notions having to do with manipulation, control, planning and so on is particularly important in gaining an adequate understanding of knowledge involving physical modality in science—knowledge having to do with causal relationships, with physical possibility and necessity, and with counterfactuals. There is a tendency among some philosophers sympathetic to pragmatism (e.g., Sellars, perhaps Brandom) to attempt to understand notions connected to physical modality in terms of a spectator picture, so that, for example, laws and causal generalizations are understood as “inference tickets” or as “licenses” for predicting the value of one variable for another. Laws and causal generalizations do play this role but they are also centrally bound up with concerns having to do with manipulation and control and their role in science cannot be understood apart from these. Relations that support inference and prediction but not manipulation (such as the relationship between barometer readings and storms) should be distinguished from relationships that support manipulation and control. Physical modality has to do with the latter.

I believe that spectator approaches to modality tend to encourage either (i) a tendency toward misplaced reification (with modal claims taken to refer to special sorts of objects or properties—see Section 11 below) or, alternatively (ii) skepticism or various forms of subjectivism about modal notions. A paradigm of (i) is Lewis’s treatment of modality in terms of “possible worlds”, each as real as our own. Illustrations of (ii) include the view that modal claims are unclear or not part of legitimate science (a position adopted by, for example, Quine) or that they are rooted in facts about our projective or epistemic organizing activities, rather than how matters stand in the world. I believe that both alternatives (i) and (ii) become much harder to maintain when one thinks of modal knowledge as rooted in activities like experimentation. How, for example, are we to understand the characteristic concerns of experiment design if the sort of modality involved in causal claims is understood along the lines of (i) or (ii)? When we do a randomized experiment designed to assess the efficacy of a drug, and reach a conclusion about the causal role of the drug in promoting recovery, it does not seem plausible that we should understand this either as an inquiry into a possible world distinct from the actual world or as an inquiry into our own projective proclivities.

**4. 8) Spectator Orientation as rejected by some but not all self-identified pragmatists**. I said that a distaste for spectator theories of knowledge is common but far from universal among pragmatists. I agree with Peter Godfrey-Smith (2015) that at least two recent philosophers who are often regarded as pragmatists—Quine and Rorty— seem to be in the grip of this spectator conception. Quine, for example, talks of sense experience “impinging” on our web of belief, leading to various adjustments in the latter, according to some principle of “minimal mutilation”. The picture is very much one of a passive observer who responds to nature by engaging in belief adjustment but does not act on or change nature. The “action” component of pragmatism goes missing here—or rather the only kind of action contemplated is verbal behavior—“lo, a rabbit” as one bounds by. A satisfactory philosophy of science should reinstate the action element, which immediately suggests that we interact with the world causally, and do not just form beliefs about it.

**4.9)** **Focus on practice, not just verbal behavior and belief.** Reinforcing this passive observer picture is another focus that is characteristic of a number of pragmatists – again Quine and Rorty come to mind-- and that is suggested by the observation under **4.8**) above. This is an almost exclusive focus on language use and verbal behavior and (what are often taken to be their inner analogs) beliefs. (Among philosophers of science, this may be broadened to include “theories” construed as systems of beliefs.) But while language, the making of true statements, or more broadly the exhibition of models, theories etc. that are intended to convey information about nature are certainly important in science, a pragmatic philosophy of science should bear in mind, that science consists in a lot more than this—there are, again, activities that involve building, manipulating, synthesizing, and so on. A core notion for pragmatist philosophers of science should thus be the notion of a scientific *practice*, understood to encompass both verbal behavior, belief formation and the construction of representations but also many other forms of behavior linked to choice and action.

**4.10) Subject versus Object Naturalism**. Another way of describing the interpretive/evaluative emphasis in **4.1**) and **4.2**) above (what works and so on) is in terms of a focus on what practitioners are *doing* when they engage in various forms scientific practice, on what the *point* or *function* (or goal) of the practice is or the role or roles it is playing. I see this focus as closely connected to Huw Price’s notion of *subject naturalism* and the contrast he draws between this and what he calls *object naturalism* (e.g. Price 2011).

Roughly speaking, the object naturalist tries to understand some body of discourse in terms of the objects the discourse is about or which serve to “ground” the discourse (and where these objects are construed in a way that is acceptable to naturalists—that is, as part of the world that science reveals) while subject naturalists instead focus on understanding what subjects do with the discourse, again in a naturalistic vein (so that disciplines relevant to such understanding may include, for example, anthropology and psychology). This frames the contrast in terms of two different ways of approaching *discourse* but, as already explained, I would favor broadening the focus (particularly in the case of subject naturalism) to include forms of practice that are not discursive, as well as those that are. In any case, the central feature of subject naturalism, as I understand it, is its emphasis on the importance the role of the *subject* or the *user* (typically a human being) who engages in various practices. Human beings have particular features that are relevant to this subject role: they have specific goals and interests, and various computational and manipulative abilities and limitations and these inform the science they produce. What we want to understand is what such subjects are aiming at accomplishing when they engage in various scientific practices, what the point or goal of those practices is and the extent to which they are successful. (This is an idea that I have particularly tried to emphasize in my own work on causation—see, e.g., Woodward, 2014 and Forthcoming) At least in many cases, these are very different concerns than the object naturalist concern with getting the ontology right or identifying objects in the world allegedly corresponding to or grounding these practices. As explained below, I see an impulse toward object naturalism in inappropriate domains as one source of ambitious metaphysics.

Pragmatists, I assume, will want to adopt some form of naturalism but as Price’s discussion reminds us, there are different ways of being a naturalist about some body of discourse or practice. Often an important part of the most promising strategy for naturalists will be to try to understand what subjects are doing within a naturalistic framework, rather than just trying to “naturalize” the products (discourse, representations etc.) they produce by providing an account of the “objects” they are about. For example, we should try to understand what subjects are attempting to do when they engage in causal reasoning (what their goals are) rather than just focusing on questions about what causation “is”. Because of its focus on subjects and what they are doing, a subject naturalist treatment of science yields a picture according to which science, even physics, is not “the view from nowhere” (there is no such view) but always the view of concrete, situated inquirers. Thus, for example, limitations in what it is possible for humans to compute or calculate or manipulate should not be dismissed as irrelevant to the theories of science we construct or the concepts that we use to understand science. Indeed, a sensible methodology of science (which after all, within a pragmatist framework, is one that should be useful to scientists) is not possible without taking account of these limitations.

Finally (and again to anticipate) let me emphasize that this focus on the subject is (of course) not at all meant to imply that the world or nature should play no role in the account of science we seek to provide. On the contrary, as emphasized by Mark Wilson (2017) among others, subjects make use of and exploit various features of the natural world when they engage in scientific practices and predict and manipulate and so on. The world needs to cooperate (to provide support or enabling conditions) if these practices are to be successful. It is thus perfectly in order (and a very good question) to ask, for example: what is it about the world that enables us to fruitfully employ causal reasoning or particular kinds of causal analysis[[14]](#footnote-14). Relatedly, we can ask what information about the world is conveyed or captured by such claims. Similarly, if a certain computational procedure returns reliable results we can ask what features of the world support this procedure[[15]](#footnote-15). However, these roles for the world are not very well captured by philosophical treatments which make use of ideas about correspondence and isomorphic representation in trying to understand science—a subject to which I now turn.

**4.11) Skepticism about (or Rejection of) Representation**. Historically, many pragmatists have been skeptical about (or hostile to) “representation” as a useful concept for understanding how language and other sorts of activities such as scientific theorizing work. This attitude continues in the work of more contemporary pragmatists like Rorty and Price. My own view is that the appropriate target for this sort of skepticism is not representation per se but rather certain conceptions of what representation—particularly representation via models and theories in science-- involves. These are conceptions that understand representation in terms of notions like mirroring, picturing, correspondence, isomorphism and the like. I will call this conception *literalism*.

In what follows my focus will not be on representation in general (I leave open the issue of how sentences of “the cats is on the mat” variety should be understood) but rather on certain very popular claims about how to understand the representational role played by (i) structures like theories and models in science and (ii) along with this, the role of modal concepts like “cause” and “physical possibility”. And as I have already signaled, I think that what we should reject is not the idea that successful models and theories represent but rather a particular way of thinking about representation in science—that this is always or even usually best understood in terms of metaphors like picturing, correspondence or (to use the currently fashionable term) the idea that a successful theory requires the existence of an isomorphism (or some notion linked to isomorphism—e.g. a partial isomorphism) between a successful theory or model and the system modeled. I think one can reject this idea, without rejecting the idea that theories, models and other structures convey or allow us to extract *information* about what the world is like. So our guiding slogan should be something like: *information without (necessarily) isomorphism* or *truths about nature without literalism[[16]](#footnote-16).* In other words in rejecting representationalism, conceived in terms of picturing or isomorphism, we needn’t embrace some form of idealism, anti-realism or views that regard scientific theories or models as mere “fictions”. Nature and the world remain as central parts of the story and theories and models tell us about these ; it is just that the relations between the two need not be what picturing metaphors suggest.

**4.12) Against Eschatological or End-of -Days -Science**. A common way of approaching many issues in philosophy of science, especially among philosophers inclined to ambitious metaphysics, begins with the idea of a final or fully completed science—a theory of everything with nothing left out. (Presumably this will be largely or entirely a theory in which physics plays a fundamental role.) One then tries to understand features of contemporary incomplete science, including the “special” sciences, by reference to this theory. For example, Michael Strevens (2008) begins with the idea of a complete description of Causal Reality (as he calls it) of a sort that presumably would be described by some final physical theory (or perhaps the starting point is Causal Reality as it is in itself, apart from ay description of it) and then attempts to understand current theorizing, in both physics and the special sciences, as the result of a process of “abstraction” (by which he basically means dropping details) from this ur-description. David Lewis begins with the notion of the full Humean mosaic-- a record of what happens everywhere and at all times throughout the entire history of the universe -- and then attempts to understand laws of nature in terms of the systemization of this information.

A pragmatist philosopher of science of the sort I am envisioning has a very different starting point: the goal is to understand “actually existing science” as carried out by human beings as they are actually are, given what they can know, calculate, and manipulate. Humans do not have access to all of Causal Reality, the full Humean mosaic, and similar sorts of information and yet somehow manage to do science, sometimes successfully. The goal of the pragmatist philosopher of science should be to understand how that is possible—and in the nature of the case, science as it will exist at the end of history, the full description of causal reality and so on cannot be part of this story[[17]](#footnote-17). Saying that current science reflects the processing limitations of human beings when dealing with the full Humean mosaic or that it approximates God’s summary for beings with our limitations does not provide the right story about how our limitations come into the picture. There is no God to provide the summary and we do not “deal with”—either causally or otherwise-- the full Humean mosaic which we then theorize about in ways that reflect our processing limitations.

Adoption of a pragmatic approach to philosophy of science suggests a major shift of focus away from “this end of days” picture. Philosophers of science instead should attempt to understand how scientists learn even in the presence of very partial information and limited computational possibilities—for example, how strategies that involve what Mark Wilson (2017) calls “physics avoidance” work, and how strategies like randomization work to enable the discovery of causal relationships even when a great deal about the detailed behavior of some system of interest is not known. Rather than asking what science would look like (what our concept of law of nature etc. would be) if we knew everything, we should take seriously the idea that we don’t know everything and consider how this affects the content of our science.

**4.13) More on Metaphysics**. I distinguished earlier between minimal and more ambitious metaphysics and suggested that pragmatist philosophy of science requires only the former[[18]](#footnote-18). Part of my motivation for this claim is that it appears that even if one were to have definitive answers to the fundamental questions addressed by ambitious metaphysics these would tell us very little about the methodological/interpretive issues that are the province of the pragmatic philosopher of science. Indeed, ambitious metaphysics, at least as currently practiced, seems largely uninterested in these issues: To take just one example, to the best of my knowledge neither metaphysicians who claim that causal claims are grounded in laws of nature conceived in accordance with the best systems analysis nor the metaphysicians who favor non-Humean treatments of causation make any serious attempt to connect their views with the methodological issues about causal reasoning and inference (issues of the sort that are discussed in e. g. Pearl, 2001, Spirtes, Glymour and Scheines, 2000, Morgan and Winship, 2015, Woodward, 2014) that are natural topics for pragmatist philosophers of science. Moreover, there are systematic reasons for this lack of connection: as I have noted the willingness of most metaphysicians to abstract away from what they regard as merely practical barriers to what we can know or calculate and other human limitations virtually guarantees that many of their inquiries will be of limited relevance to the descriptive and methodological questions that interest the pragmatic philosophers of science.

For these reasons pragmatist philosophers of science should feel free to insist that, given their goals, they don’t have to do ambitious metaphysics and that the projects they wish to pursue are important and valuable in their own right. When criticized by metaphysicians for failing to articulate proper metaphysical underpinnings for science, pragmatists should push back by demanding to be shown why, given their goals, such underpinnings are necessary.

**4.14). Reductionism**. A number of philosophers of science, again especially those who are inclined to ambitious metaphysics, are attracted to one or another variety of reductionism about various concepts employed in science[[19]](#footnote-19). A common focus of such reductive programs is causal and modal notions, with so-called Humean reductivist projects being very prominent among these. The questions a pragmatist philosopher of science should ask about such projects are these: (i) First, given the goals of the pragmatic philosopher of science as described above, how if at all would successful completion of the reductivist project contribute to these goals? If, as I suspect, carrying out the interpretive/evaluative projects described above often does not require carrying out a reduction, pragmatist philosophers should feel free not to provide it. (ii) More generally, the pragmatic philosopher should ask the ambitious metaphysician just what the point or purpose of the reductions she wishes to provide are. What does a reduction give us (beyond the reduction itself and the satisfaction of a metaphysical itch to reduce)? In the past, it was common to argue that in the absence of a reduction, we did not really understand causal and modal locutions or that we lacked a story about how we could acquire knowledge about them. But these semantic and epistemological claims are no longer plausible. Is there some other reason why reductions are valuable? Or to put the question a bit differently: suppose we have the pragmatist goals described above in connection with causal reasoning—understanding the use of causal notions, how we reason with them, infer to causal claims and so on. What reason is there to think that failure to carry out a reduction would impede those goals?

**5. More on Subject and Object Naturalism**

Having set out these themes, I turn now to an attempt to develop and motivate them in a bit more detail, beginning with the distinction, due to Price and alluded to earlier, between object and subject naturalism. Price characterizes object naturalism, at one point, as the view that “all there *is* is the world studied by science” and, as an epistemological doctrine, as “the view that all genuine knowledge is scientific knowledge” (2011, p. 5). He contrasts this with subject naturalism according to which “philosophy needs to begin with what science tells us *about ourselves*” (p. 5) As Price explains, when applied to some bit of discourse, the subject naturalists asks what we humans are *doing* with the discourse, what its goal or point or function is, and tries to understand this (and the subject engaged in this activity) naturalistically.

Since I’m not sure that I understand either the claim that what is studied by science is all there is or its denial, I’m going to adopt a slightly different characterization of object naturalism from the one quoted above, but one that I think remains in the spirit of Price’s usage. This construal takes object naturalism to involve a thesis about (or project motivated by assumptions about) *representation*  (that it is to be understood in terms of notions like mirroring or picturing or correspondence or literal truth ) and an accompanying thesis about what is required for a representation to be true or fully accurate: roughly speaking, the representation claims that certain *objects* exist – the objects that the representation would mirror or picture if it were construed literally-- and the representation is true or accurate to the extent that this is so[[20]](#footnote-20). In other words, the objects that are required to exist can be read off in a straightforward literal way from the representation itself. (I will understand the notion of “object” here very broadly so that it includes properties or relations, but one of the rules of the game is that these are understood as having thing-like characteristics, so that one can raise questions about their “ontological” status.) For example, claims about what is possible are understood as claims about the existence of “possibilities” or “possible worlds” and so on, the ontological status of which then become matters of concern. If one can find such objects for all of the claims in the discourse and these objects are naturalistically acceptable, then these claims are taken to be true (legitimate etc.) One the other hand, suppose the attempt to provide such a construal runs into difficulties, as one might think that it does with respect to, say, moral claims or claims involving physical modality, where one faces puzzles about the objects such claims are about or the things that serve as “truth-makers” for them. Then one faces what Price calls a *placement* problem, which might be resolved in a number of different ways—e.g., by expanding one’s view of what is a naturalistically acceptable object or, alternatively, by postulating objects that are acknowledged not to be naturalistically acceptable in order to secure the truth-aptness of the claims in question. Yet another alternative is to reconstrue the claims as not about objects at all and hence (on the view of representation we are considering) as not (at least literally) the sorts of things that can be true or false. (Perhaps the claims should instead be understood “expressively” or as “useful fictions”.) In any case, the focus is on *existence* claims and on what objects in the world correspond to or make true the claims or representation in question[[21]](#footnote-21). In other words, in contrast to subject naturalism, the focus is ontological, even if the conclusion is that the appropriate objects to make the representation true do not exist.

This suggests how a kind of literalism about representation combined with the kinds of concerns that underlie object naturalism tend to lead to ambitious forms of metaphysics when applied to discourse and practices whose corresponding objects are not obvious. Either one is led to postulate objects whose existence seems puzzling or controversial to serve as such correspondents and hence to worries about the ontological status of these or one is led to controversial claims about the discourse in question being fully reducible to claims about objects that are naturalistically acceptable. Alternatively one reinterprets the discourse as not truth-apt at all.

Using one of Price’s running examples, consider moral claims and discourse as an illustration of these possibilities. Presented with the contention that, stealing is wrong, a philosopher with an orientation toward object naturalism would consider, first, whether there is some property or thing corresponding to “wrongness” that is instantiated by various acts of stealing. Candidates for such a property that might be argued to be naturalistically acceptable may include, say, “not conducive to maximal preference satisfaction or welfare maximization”[[22]](#footnote-22). Alternatively, the investigator might want to consider giving up on naturalism and postulating some non-natural property of “wrongness” to serve as a truth maker for claims about the wrongfulness of stealing. If one thinks that plausible candidates for such properties do not exist, another alternative is to construe the claim as not a candidate for literal truth of falsity at all, and to re-interpret it as, say, (merely) an expression of disapproval regarding stealing.

The subject naturalist takes a very different approach: rather than focusing (just) on what the objects and properties might be that are the worldly correspondents to moral claims, the subject naturalist focuses instead on what speakers are doing when they make such claims, or when they engage in practices associated with moral assessment more generally. The subject naturalist asks questions like “what is the point (or points) or goals or function of these practices?”. “How (if at all) does labeling actions as wrong contribute to these?” In the case of moral claims, for example, such goals might have to do with coordination with others about rules governing social interaction in order to advance certain commonly held social goals, the advancement of proposals that others adopt and act on certain rules (where this might involve arguments that the rules are feasible and motivating for others) and so on[[23]](#footnote-23). On a descriptive level this is an inquiry that can and is pursued by anthropologists, sociologists, economists and others. And of course there are evaluative projects along these lines that might also be pursued: one formulates various social goals and then asks whether various moral practices or judgments are conducive or not to realizing them.

As already suggested, as I will understand subject naturalism, it *need not* involve the view that the claims (or representations or practices or whatever) in question are the sorts of things that are not true or false or that fail to have import for how matters stand in the world. That is, one does not have to be some kind of subjectivist or relativist about the practices in question to engage in a subject naturalist approach to them. What is distinctive about subject naturalism is that the *focus* is different from the focus of object naturalism, in the ways that I have tried to characterize—the focus is, as Price says at one point, “anthropological”[[24]](#footnote-24) (to which I would add “or, alternatively, evaluative”) rather than “ontological” . However, it is consistent with this focus that the best way of understanding some practices (including practices of scientific theorizing) is to understand them as in the business of providing information about how matters stand in the world.

While a subject naturalist treatment of a practice or discourse thus does not require construing it as non-truth apt, subject naturalist approaches can open up, in a way that I will illustrate, the possibility that literalist construals in terms of mirroring and correspondence may not be the best way to understand those practices. So a subject naturalist understanding of some practice may help to undermine certain literalist understandings of the practice (and accompanying claims about the objects to which the practice is committed), although in doing so it need not also undermine interpretations of the practice as providing information about nature. This is because a better understanding of what subjects are doing when they engage in some practice can lead us to rethink what, so to speak, the worldly correlates of the practice are and the structures in nature that account for the success of the practice[[25]](#footnote-25).

It is striking (and further evidence for the importance of the distinction between subject and object naturalism) that the accounts provided by object naturalists are often rather unilluminating regarding the issues that interest subject naturalists—they just don’t address the subject naturalist’s concerns. Relatedly, it seems entirely possible for subject naturalists to pursue their concerns without committing themselves on many of the issues on which object naturalists focus. This is illustrated by the example of morality. Subject naturalist investigations of this topic flourish in many different disciplines but for the most part these successfully avoid taking up issues having to do with the ontological status of “wrongfulness” and the like. Conversely, if we were told by some oracle that, say, objective wrongfulness exists as a non-natural property or, alternatively, that it can be identified with some complicated natural property, this would tell us very little about what subjects do when they engage in various moral practices, make moral judgments and so on. Nor would it help with moral evaluation. Indeed, should they be forthcoming about such matters, it seems to me that most of those interested in the ontology or metaphysics of morals will agree that they are not interested in such descriptive or evaluative questions, and that they instead regard these as mainly the concern of other disciplines such as psychology and anthropology or perhaps, in the case of evaluation, social policy analysis.

Returning now to a topic of more direct interest to philosophers of science, I think that a similar point holds for investigations into causation and causal reasoning. Here too I see two quite distinct foci of interest, corresponding to object and subject naturalist approaches. The object naturalist wants to know what in the world corresponds to the causal relation (or to the variety of such relations, but often for the object naturalist the assumption is that there is just one). What is the “nature” of causation or the “grounds” or “truth makers” for causal claims or the correct ontology/ ambitious metaphysics for causation? By contrast, subject naturalists have the usual mixture of descriptive and evaluative concerns—at the descriptive level, they want to know how various subjects reason about causal relationships, how and on the basis of what sorts of evidence subjects infer to causal relationships, what sorts of goals inform causal thinking (why we care about discovering causal relationships rather than resting content with “mere correlations”) , what distinctions subjects make among different sorts of causal relationships and so on. These are issues that are addressed by psychologists interested in causal cognition but also by a number of philosophers—for example, by Danks, Glymour, Hitchcock and Wilson, among others. On the evaluative level we have a vast literature on the methodology of causal inference and reasoning. Once again, I think there is a disconnect between these concerns and more purely ontological ones. Even if some oracle were to tell us that the correct ontology for causation is such and such, it is not clear that this would provide much by way of answers to the sorts of descriptive/ methodological questions described above. Conversely, it would seem that researches outside of philosophy who pursue such questions do so without much attention to object naturalist/ metaphysical questions about causation and it is not at all obvious that they are wrong to do so. (Cf. Woodward, 2014)

I assume that you can see where this is going. I want to recommend the subject naturalist attitude or approach not just in connection with understanding the role of causation in science but also more generally in philosophy of science (although again not to the exclusion of minimal metaphysics) . An approach in which subject naturalism figures prominently fits naturally with and supports both the descriptive/ interpretative and the evaluative concerns of philosophers of science that are a natural expression of a broadly pragmatist approach to science

**6. Means/ Ends Reasoning**

Let me next turn to some remarks about goals and means ends reasoning. There are very influential strands of pragmatism, associated with Quine and Rorty among others, that aim at collapsing or undermining distinctions. Where others see differences, these pragmatists see continuity, sameness and seamless wholes: theories confront evidence holistically in a way that prevents our regarding different components of those theories as less or more well supported by evidence, science itself is claimed to be continuous with and not to differ in principle from pure mathematics and perhaps even from metaphysics. A common device among these pragmatists for motivating such views is to invoke the idea that it is appropriate to think of all elements of science as animated by a single overriding undifferentiated goal— that of finding what “works” or “is useful” or “fruitful”. The assumption is that anything that contributes to this generic goal has a similar status and that further distinctions are unnecessary and even invidious . Thus, for example, just as we allegedly ”posit” the existence of electrons because it is “useful” to do so, so also we posit the existence of sets and natural numbers because this too is useful.

Notice that this idea that there is one overriding goal of usefulness is distinct from the characterization of the pragmatic approach adopted in Section 4 above. Section 4 says that science is concerned with such useful goals as prediction and control and that various aspects of scientific practice are to be understood and evaluated in terms of their conduciveness to these goals. Nothing is said about there being a single undifferentiated goal of usefulness.

In contrast to this notion of a single goal of usefulness, the sort of pragmatism that I favor takes seriously the idea that human beings have a number of different goals, both within scientific investigation and elsewhere These goals carry with them different standards of adequacy and success. For example, even at a very general level, prediction is a different goal than causal analysis and it is unhelpful to subsume both of them under the single goal of discovering relationships that are “useful” or that “work” , even though it is of course true that both prediction and causal analysis can be useful in their different ways. Pragmatists should be sensitive to these differences rather than collapsing them into a single ur-goal. In addition, on the approach I favor, a goal like prediction is usually understood as a matter of making *true or true enough or accurate* predictions—there is no suggestion that notions like truth or accuracy themselves can be understood in terms of some undifferentiated notion of usefulness. There is also no suggestion that we are entitled to “posit” claims as true just because doing so would be useful to us in some general way—I take this to be an endorsement of wishful thinking and antithetical to science

Thus the version of pragmatism I favor presupposes the availability of some notion of truth. (I take this to include some notion of being nearly true or close enough to true as to make no difference, as when one says that Newtonian gravitational theory is true or valid for large objects in weak gravitational fields). It does not attempt to analyze truth in terms of usefulness or warranted belief or anything similar. Instead, the assumption is that we need a notion of truth to specify in a non-trivial way which means in fact lead to goals, which reasoning strategies lead to correct conclusions and so on.

I have already mentioned, as among the goals associated with science, prediction, explanation, and causal analysis, classification, building and making. These goals can in turn be individuated more finely in many different ways.; for example, there are many different kinds of control-related questions to which investigators seek answers, and many different kinds of predictions, distinguished both by their targets and the information on which they rely[[26]](#footnote-26). Moreover, investigators have choices (i.e., they may have different goals) about which behaviors of which systems they wish to predict, explain and so on. And of course, realization of these goals will require realization of more specific subgoals—e.g., successful prediction may require accurate measurement of some quantity of interest, construction of instruments for doing this and so on. Nonetheless the goals themselves are distinct and, as I said above, can have different success conditions. For example, models or choices of variables that are useful for certain predictive purposes may not be optimal for purposes of explanation and causal analysis, as recent work by Danks 2015 and numerous other examples show.

Even when we are interested in explaining aspects of the behavior of a single system, different models and strategies will often be appropriate, depending on which aspect of the system or the level or scale of its behavior we are aiming to explain. For example, a circuit-level model like the Hodgkin-Huxley model of a neuron, which seeks to explain the generation of an action potential, has a different target explanandum (or set of such explananda) than a molecular level model that aims at the explanation of the opening and closing of the ion channels that are responsible for the ionic currents that are at work when the action potential is generated (Herz et al, 2006). Pragmatists will find it reasonable to look for an understanding of explanation according to which one can explain one of these explananda (satisfy one goal) without explaining the other—see Section 10.

One corollary of this is that, corresponding to this plurality of goals, pragmatists should expect to find in science a plurality of theories or models, even in connection with the same kind of system, depending on what goals inquirers have in connection with that system— what scale of behavior they want to predict or explain and so on. At the same time, investigator’s goals will often require establishing connections or relationships among different models. For example, for certain predictive purposes it may be adequate to model a material as a homogeneous continuum. But for other predictive purposes (e.g. in modeling cracks and fractures) it may be best to continue to use a continuum like model but feed it information from lower scales— as it sometimes put, finding ways of allowing lower scale models to “talk to” more upper scale models and vice-versa[[27]](#footnote-27). So we should expect to find, not just a plurality of models but attempt to integrate and connect these— a picture that fits very well with Sandra Mitchell’s integrative pluralism (e.g., 2002)—see Section 10 for additional discussion.

Let me also add in this connection that my view is that it is only if we acknowledge a plurality of goals, individuated in a somewhat fine-grained way, that we can carry out the evaluative and interpretive tasks described above in a way that is interesting and illuminating. If we think in terms of specific goals like estimating the value of some parameter from a body of statistical data or inferring causal relationships from observational data meeting certain conditions, we can describe and evaluate specific procedures for achieving these goals and in this way provide useful guidelines for the conduct of inquiry. If we insist on framing discussion around highly generic and imprecise goals holding for all inquiry, whether this be “usefulness” or “accommodating observational evidence with minimal changes in existing theory” or something similar this is unlikely to generate helpful guidelines[[28]](#footnote-28) .

In my list of possible goals above, there are conspicuous omissions: For example, I did not include the goal or goals of conveying the full and literal truth about nature “at the fundamental level”, or of “providing a single completely accurate representation of what nature is like in every respect”. I have several justifications for this omission. First, I am skeptical that, as an empirical matter, this goal plays a central role in science. Second, I doubt that this and related goals are attainable or even fully intelligible. Indeed, I think that such alleged goals are part and parcel of the literalist idea that scientific theories and models aspire to some sort of mirroring or isomorphism relation with nature and, as explained above, I favor rejecting this whole package. Third, even if the goal of full and literal truth is among the goals of science, I think it obvious that it is not the only such goal. And more importantly, as we shall see, this literalist goal is often in tension with the other goals described above. For example, it is often not true that the model or theory which is most representationally detailed or accurate provides the most adequate explanations. So even if we think isomorphic representation and full and literal truth are goals, these are often overridden by other goals. Note also that to the extent that the project of reading off the correct metaphysics from our best scientific theories depends on the idea that the only goal of scientific inquiry is something like isomorphic representation of the fundamental nature of reality, that project will be problematic if science reflects other goals as well that are in tension with the isomorphism goal.

**7. Means/Ends Methodology Illustrated[[29]](#footnote-29)**.

Let me turn next to some additional explication of the means/ends picture of science and its methodology gestured at earlier. Recall the basic picture: scientists have certain ends or goals and many aspects of science—particular choices of methods and choices about modeling and theorizing should be understood as means to attaining those goals and evaluated accordingly. The category of “means” is understood very broadly – it includes strategies for experimental design, procedures for causal inference from various sorts of data, choices of variables in model construction, testing procedures of various kinds and much more. A simple illustration is provided by the treatments of estimators in classical (frequentist) statistics. Suppose we wish to estimate the value of some quantity *m* on the basis of noisy measurements resulting in statistical data *d* bearing appropriately on *m*. Such an estimator will be a random variable *m\** that is a function of the data *d*. One proceeds by adopting criteria for what it is for an estimator to be “good”. For example one such (widely used) criterion is that the estimator be unbiased in the sense that its expectation value *E (m\*)* is equal to *m* – i.e., to the true value of the quantity being estimated. Another criterion is that the estimator should be chosen so that, among such unbiased estimators, its variance is as small as possible. Given these criteria and certain other assumptions, such as the assumption that the estimator must satisfy a linearity requirement , one can then prove mathematically that the best estimator for *m* must take a certain specific form.

What I want to draw attention to here is not the details of this idea but rather the general form that this justification takes: one specifies a goal -- a good estimate for *m*, subsidiary goals that specify what counts as a good estimate in this context —and then shows that a certain choice of an estimating function is an effective means to this goal, thereby providing a means/end justification for this estimating procedure. This is exactly the sort of justification that a pragmatist should like and indeed in my view it is no accident that one of the founders of philosophical pragmatism, Peirce, was an early source for the use of such frequentist or error-characteristic justifications in statistics.

**7.1. Causal Claims**. In the example just described the chosen goal is rather local and the demonstration that the means are optimal is purely a matter of mathematics. But, as already intimated, I think that the general idea can be applied much more widely and used to illuminate and evaluate many other concepts and reasoning strategies used in science. (As noted above, in the more general case, means/end analysis may involve some mixture of empirical evidence, heuristic argument, strategies for calibration and simulation as well.) For example, consider an interventionist treatment of causation according to which causal claims are understood about as claims about what the results of hypothetical experiments would be. Take the discovery of the truth about such claims as the goal at which we are aiming. One can then raise questions about the kinds of strategies, evidence and methods that are likely to be effective in realizing this goal. One thing that is immediately suggested is that when presented with a causal claim, it pays to try to clarify or disambiguate it by specifying as exactly possible just which hypothetical experiment is intended (as capturing the content of the causal claim), since what looks like the “same” causal claim can often be associated with different possible hypothetical experiments. This is common methodological advice in at least some portions of the literature on causal reasoning and inference.

Having clarified which hypothetical experiment is intended, one can then assess the extent to which different bodies of evidence and inference procedures are effective means to this goal. Obviously one very simple procedure is to actually *do* the relevant hypothetical experiment, since this will tell you what would happen if you were to do it. But even if one can’t (or doesn’t) perform the relevant hypothetical experiment conceptualizing causal claims in the manner described can be very useful: it suggests a program of evaluating inference methods in terms of what they can tell us about the outcomes of hypothetical experiments without actually doing the experiments. And in fact, certain procedures such as the use of instrumental variables and regression discontinuity designs can, under the right conditions, be demonstrated to furnish reliable answers to such questions—indeed this is a standard way of justifying the use of such procedures. In the case of instrumental variables, for example, one looks for relationships among the observed or measured variables that mimic the effects of interventions, so that one can use this information to identify what the result of an intervention on the candidate cause variable would be without actually performing the intervention in question[[30]](#footnote-30). Again, this is means/ ends reasoning: if you want to predict the outcome of a hypothetical experiment without actually doing the experiment, you should proceed in such and such a way. That is, if you follow the appropriate procedures and the relevant assumptions are satisfied, it follows, as a matter of mathematics, that you will correctly predict the results of a hypothetical experiment.

**7.2. Invariance**. As another illustration, also in the context of causal reasoning, consider the role played by notions of invariance. A relatively invariant causal relationship is one that is relatively stable or insensitive in the sense that it continues to hold across changes in other variables. It should be obvious why finding relatively invariant relationships is worthwhile goal— among other considerations, such relationships are more generalizable to new circumstance and afford wider opportunities for manipulation and control. Given this goal, various strategies and practices can then be understood in terms of their conduciveness to this goal. As an illustration consider our interest in discovering “mechanisms”. Given an overall relation of dependence between some input *I* and output variable *0*, the individual relationships between mediating or intermediate variables between *I* and *O* will often be more invariant than the overall *I🡪O* relationship. Guided by an interest in finding relevantly invariant relationships we look for such links; we decompose the black box *I🡪O* relationship into intervening steps or each of which, taken individually involves relationships that are more invariant that the overall *I🡪O* relationship. These comprise the mechanism linking *I* to *O*. Notice the form that this takes: we don’t just posit an interest in finding mechanisms as primitive or invoke it because, as a descriptive matter, scientists seem to be interested in looking for mechanisms. Instead we try to explain why this is interest is reasonable or intelligible as a means to other more general goals such as the goal of finding invariant relationships which is in turn seen as contributing to the even more general goals described above. One advantage of such an approach is that it also illuminates why, in some circumstance or with respect to systems with certain structures, the search for the specific sorts of invariant relationships associated with mechanisms may not be such a good strategy—it may not serve more general goals of the sort just described very well. (See Woodward, 2013.)

Yet another illustration of the same basic framework is provided by recent work by Patricia Cheng and collaborators (e.g., Carroll and Cheng, (2010). She shows that in some circumstances (different from those discussed immediately above) a concern with finding invariant relationships will justify or motivate the introduction of latent or unobserved variables, while in other ostensibly similar circumstances, it will not. This claim about when it is appropriate to introduce latent variables is a normative claim, again in the means/end hypothetical imperative sense described above, but Cheng also shows that subjects in her psychological experiments behave in accordance with these norms. It and similar investigations have the potential to cast light on the issue of when investigators, whether lay or scientific, introduce terms or entities that go beyond what is observed and when it is rational or justified to do so.

**7.3. Variable Choice**. Next consider issues having to do with vocabulary or variable choice in science. When discussed by philosophers of science (or metaphysicians) the guiding thought often is that certain terms or predicates or properties are “natural” and others are not and that good theories are (and should be) formulated in terms of the former rather than the latter. The natural predicates in turn are identified on an intuitive, example-driven basis, often relying on syntactic or else broadly metaphysical considerations—“grue” and “overly disjunctive” predicates are non-natural and bad, “purely qualitative” predicates are more natural. A striking feature of most of this discussion is that the good and bad predicates are not evaluated as such with respect to the goals or purposes they are being used to achieve – presumably because the “naturalness” of such predicates is thought to depend on metaphysical considerations that are independent of human goals and purposes

An illustration of this is provided in Sider (2011), who imagines a universe consisting entirely of fluid that is divided by a plane into a half that is uniformly red and a half that is uniformly blue. He supposes that observers encountering this universe divide it instead by means of predicates that cross-cut this color division. Sider contends that in doing so the observers are making a “mistake” and that they are carving up the world incorrectly. In contrast to Sider, pragmatists will think that the question of which classification or choice of variables is “correct” cannot be answered independently of what the goal or purpose is for which those variables are to be used and how in turn these goals interact with the empirical circumstances in which the variables are used. In Sider’s example the role of these considerations is suppressed because the imagined universe has no additional differentiating properties besides those described-- it is left completely unclear what the observers might be trying to *do* with the distinction they introduce[[31]](#footnote-31). Consider instead a much more realistic example in which one’s goal is to formulate a distinction or classificatory variable that enables prediction of the values of certain other variables. It is entirely possible that, for this purpose, the “unnatural” distinction described by Sider that cross-cuts a classification based on color may be better (in the sense of yielding more accurate predictions) than the allegedly more natural distinction that he favors[[32]](#footnote-32).

As I have suggested, a pragmatist philosopher of science wil approach the problem of variable or vocabulary choice in a very different way from Sider, asking instead which such choices will best further various ends of inquiry, so that the choices are evaluated in a goal-relative way. Thus rather than assuming at the outset on the basis of syntactical or other considerations that disjunctive predicates are defective, pragmatists will ask whether there is any means/end argument that shows that such variables or predicates contribute less well to our goals than alternatives. The introduction of new terms and variables, however intuitively unnatural or non-simple, will be regarded as justified when they allow for the formulation of theories etc. that best advance goals of inquiry. Moreover, pragmatists will recognize that because of the plurality of goals in science, it is entirely possible that the variables or vocabulary that work best for purposes of one set of goals (e.g., prediction) may not work best for other goals (e.g. causal explanation) And of course different variables may be appropriate for causal explanation even in connection with the same kind of system depending on what one is interested in explaining

A very nice example of this general strategy in connection with variable choice is provided by Hitchcock (2012). Hitchcock takes the “metaphysical” question of whether the time at which an event occurs is “essential” to it and transposes this into a question about variable choice. For purposes of causal analysis should we represent events of the same kind occurring at different times (e.g., a fire occurring in May or in June) as different possible values of the same variable or as values of different variables? As Hitchcock shows, the first choice leads to a causal analysis that is obviously defective in the sense that it does not adequately capture the causal possibilities and is a misrepresentation of causal structure, assuming that one takes causal claims to have to do with the outcomes of interventions — for example, a causal cycle is represented as present in a situation in which it is not. More generally, the resulting structure does not accurately represent the full range of manipulability relations or interventionist counterfactuals which we assume to be true in the situation as described. The second choice has none of these defects and thus better serves our purposes.

In a recent paper (2016), I have attempted to further explore how the goal of discovering causal relationships in the interventionist sense, where these relationships also have such additional desirable features such as relative invariance, can guide variable choice. My idea is that some choices of variables make possible the formulation of relationships that are more invariant, and have other features that are desirable from the point of view of causal inference than others. Again this project is both descriptive/ interpretive and normative/evaluative: the idea is both to explain features of the variable choices that scientists make in terms of the goals they are trying to achieve *and* to assess whether and to what extent these choices are well adapted to these goals.

**7. 4. Simplicity**. Many of the examples I have discussed so far have to do with causal inference and explanation but , as I said above, it is possible to adopt a similar approach to other aspects of scientific practice. Consider the role of “simplicity” in science. One possible approach is to observe that scientists sometimes appeal to simplicity considerations in theory choice and to take this as primitive fact, perhaps making a preference of simplicity constitutive of good science (or constitutive of some important concept in science, such as that of a law of nature, as in the Best Systems Analysis of this concept), but without trying to be more precise either about what simplicity involves or why it is important or valuable. An alternative approach, which is “pragmatist” in the spirit I have been trying to articulate, instead asks whether and how simplicity conceived in this or that specific way might be conducive or not to various additional goals—in other words, it tries to justify appeals to simplicity in a means/ends framework. In order to carry out this project, one has to be precise about what is meant by “simplicity” and about the goal to which it is taken to contribute.

One paradigm of this sort of approach is the Akaike framework (e.g., Akaike, 1974) for assessing predictive accuracy. Within this framework, one particular way of thinking about simplicity, connected (roughly) to trading off the number of free parameters in one’s model or hypothesis against a measure of goodness of fit on already observed data can be shown to be an effective means of maximizing predictive accuracy on new data, provided certain additional (and rather restrictive) assumptions are satisfied. In effect, a particular conception of simplicity connected to number of free parameters is understood as a corrective to over-fitting known data. Again, my point here is not to argue in favor of this particular set of ideas (which to repeat, apply only in a rather restricted framework) but rather to provide another illustration of “pragmatic” means/end justification and analysis.

Another sort of means/ends justification of appeals to simplicity is provided by formal learning theory, as developed in a series of papers by Kevin Kelly and co-authors (e.g., Kelly, 2007). These papers show, roughly, that a learning procedure that conjectures the simplest hypothesis (in a specified sense) will converge to the truth (on a hypothesis space that contains the true hypothesis) and, considering all possible data streams, will in the worst case converge to the truth with the minimal number of retractions. Convergence to the truth with the least number of retractions in the worst case is thus the goal in terms of which adoption of this particular understanding of simplicity is justified.

There are of course other ways of thinking about simplicity and its role in science in addition to the notions and justifications just described. One alternative conceptualization of simplicity is in terms of ease or economy of use, as when it is said that use of a representation in spherical rather than Cartesian co-ordinates makes solution of a certain class of problems easier. There is an obvious means/ ends justification for a concern with this sort of simplicity but we need to be clear about just what such a justification can deliver. In particular, choosing a representation on the basis of this sort of justification does not address the problem that the Akiake framework attempts to address. On this ease of use conception of simplicity, the same hypothesis can have both a simple and a more complex formulation, both of which (since they represent the same hypothesis) will be equally reliable or non-reliable in predicting new data. In general, showing that a hypothesis is simple in this ease of use sense seems to no implications for whether it possesses such other desirable features as truth or evidential support.

A frustrating feature of many appeals to “simplicity” in current philosophy of science (and metaphysics) is that little attempt is made to distinguish among these different notions of simplicity and the alternative justifications that go along with them. Consider, for the example, the role of simplicity in best-systems accounts of laws of nature. Is this to be thought of just as a matter of ease or convenience of use, along the model of spherical versus Cartesian coordinates? If so, axiomatizations of the Humean mosaic which differ in “simplicity” will apparently just amount to alternative representations of the same set of facts—representations that don’t really differ in what they claim about the world, but only ease of use, in the way that representations of the same curve in terms of Cartesian and spherical co-ordinates do. If so, it seems to follow, within this framework, that different claims about what the laws are will differ only in the way that representations of the same hypothesis that are more or less easy to use differ. Alternatively, if some more substantive conception of simplicity is intended by advocates of the BSA, this conception needs to be spelled out and some account needs to be provided for why it leads to the successful identification of laws of nature.

**7.5. Physics Avoidance**. As a final illustration, consider Mark Wilson’s notion of “physics avoidance” (e.g., Wilson, 2017) . In many case of physical modeling, it is a good strategy to efface or avoid detailed representation of some features of the system being modeled in order to highlight other features that receive more detailed attention. Thus a great deal of complex physics may be summarized in a small number of macroscopic parameters, whose values are measured experimentally rather than derived, and this information then used in more detailed modeling elsewhere. Or certain gross features of complicated physics may be captured via a particular choice of boundary conditions, with these chosen so that they will fit with (allow for the solution of) a particular set of differential equations that model what is going on elsewhere in the system such as its interior, with the latter being modeled in much more detail. Again this is a strategy with a means/ends justification that deserves more attention from philosophers of science.

**8. Representation**

I noted above that pragmatists have often been skeptical of the notion of representation when understood in terms of concepts like mirroring and correspondence—what I have called literalism. Sometimes this skepticism is global, as with Price’s global expressivism. I take no stand here on this global issue but do want to argue that when one focuses on theories and models in science, rejection of expectations associated mirroring, correspondence, and isomorphism has much to recommend it.

To motivate this line of thought, let me begin with some examples. The basic claim that I want to make is that a model or theory or structure can provide information about how matters are in the world (so that we can understand the model as making claims that are *true* of the world[[33]](#footnote-33)) but that we need not think of this in terms of the model mirroring or being isomorphic to what is in the world. The model tells us about the world but what it tells us need not be something we can read off by interpreting the model as a kind of mirror or isomorph of nature. Often a “literal” interpretation of a model is not what the model tells us about the world.

**8.1. Wave function realism**. Non-relativistic quantum mechanics models multi-particle systems with many degrees of freedom by means of a state function defined on a high dimensional configuration space that evolves in accord with the Schrodinger equation. Take this representation literally and you have what has come to be called wave function realism: reality itself is a high dimensional configuration space inhabited by some wavelike thing, as advocated by Ney and Albert (2013) and others. This is construing quantum mechanics in accordance with what I have called literalism—everything in the theory has the job of referring in a very direct way to elements of reality.

There are technical problems with wave-function realism but even before getting to these, a natural reaction, which I hope at least some of you will share, is that taking QM seriously does not require this sort of literalist construal of what it tells us. QM certainly tells us something about what nature is like, including so-called unobservable aspects of nature, but for this to be true it is not required that everything in the Hilbert space (or some alternative) representation has a direct counterpart in nature or stands in some relation of isomorphism to the world’s constituents. It isn’t even required that there be a no-trivial partial isomorphism of some kind between the theory and some subset of the world’s constituents.

**8.2. Continuua**: Many aspects of the behavior of gases or systems consisting of gases, liquids and solids are well modeled by treating the these as continuua with an infinite number of degrees of freedom. Indeed, it appears that an adequate treatment of a number of phenomena, including phase transitions, requires such a treatment (taking the continuum limit and so on). If we approach such models with literalist expectations, it is hard not to be puzzled by this. On the one hand, of course gases etc. are not literally continuua but rather are made up of molecules. We might say that the continuum models are strictly speaking false are but “useful fictions” or “idealizations” or “approximations” but in my view these are just labels that give us little insight into what is going on. On my view, often a better strategy is to think of the continuum models as telling us things that are genuinely true of (or informative about) the systems modeled, but not as telling us what we might naively take them to be saying on a straightforwardly literalist construal. That is, the best way to understand these models is not as telling us that matter literally is continuous (everyone knows this to be false) but rather as functioning so as to convey other sorts of information. For example, one thing we learn from the use and success of such models is that the bulk behavior of many macroscopic systems is surprisingly independent of the details of the behavior of their molecular constituents—so independent that even a continuous fluid will exhibit the same macroscopic behavior as its atomic counterparts as long as it satisfies certain generic constraints. Another related point is that phenomena like phase transitions (as well as many other macroscopic properties) are scale-dependent in the sense that their existence is tied to certain size and energy scales and we can think of continuum limit is a way of bringing this out.

Once again, my concern is not with the details of this construal but rather with the more general strategy of interpretation: the continuum models are telling us something true about the world (providing worldly information) but what they are telling us may not be best understood in terms the assumption that there is an isomorphism between such models and nature. Or at least the appropriate conclusion is that we can’t just read off just from the surface semantic features of these models what it is that they are telling us about how matters stand in the world. They inform us about the world but in a more subtle and indirect way than is suggested by the isomorphism picture[[34]](#footnote-34).

To make a connection with earlier portions of this paper, one of the things that this sort of literalism leaves out is the role of the subject or user of the model. It is the user of QM who knows how to use the Hilbert space formalism to extract or capture information about the behavior of quantum mechanical systems, predict their future behavior and so on. Similarly for the user of continuum models. Such users know how to employ these models for various purposes without necessarily construing them according to literalist expectations; users know what parts of a model should be taken literally in certain applications and which should not. Pragmatic philosophers of science should accordingly focus on how such models are used and resist the temptation to automatically construe them in literalist ways. Or to put matters differently, rather than trying to understand the use of models in terms of a relationship of correspondence or isomorphism between model and world, think instead of the models as a devices or tools that subjects use to extract or capture information about nature.

**9. Realism vs Instrumentalism**.

Ideas of the sort just described are frequently denigrated as “instrumentalist” in contrast to realism, which is thought to be good. However, as I see matters, these ideas do not amount to traditional instrumentalism as that doctrine is understood by philosophers of science. Instrumentalism as traditionally conceived involves commitments like the following: (i) a sharp distinction between observational and theoretical claims and the idea that theories are just devices for calculating observational consequences, with theoretical claims not being truth apt at all and merely playing the role of facilitating calculation of relations between observational claims. Along with this many versions of traditional instrumentalism are committed to (ii) the idea that theories and models merely codify or summarize relations involving “regularities” among observational claims and do not explain (they merely describe) and do not provide genuine causal or modal information, in any sense that goes beyond the description of regularities. The anti-literalist view of theories and models sketched above not does endorse either of these commitments. Theories and models are seen as embodying modal information and as explaining and much of the information they convey has to do with matters that go beyond straightforward observation. For example, on the construal suggested above, when continuum models are used to capture information about independence from lower level molecular details this information both has modal import and is not purely “observational” in the sense favored by classical instrumentalists[[35]](#footnote-35).

Thinking of matters this way, we should be able to see that the traditional dichotomy between scientific realism (which is often formulated in a way that it involves a commitment to something like a picturing or isomorphism conception of theories or models) and instrumentalism, when construed as committed to (i) and (ii), is far from exhaustive—there are other possibilities, including the one I have tried to describe. I believe that much of the appeal of strong forms of realism understood in terms of correspondence derives from the thought that the only possible alternative to it is traditional instrumentalism. However, t as I see it there is no reason why a pragmatist philosopher of science should think that if she wishes to reject forms of realism that are committed to picturing and isomorphism, she needs to take on commitments like (i) and (ii). Pragmatists should not allow themselves to be browbeaten by vague accusations of instrumentalism and they should be suspicious of formulations of scientific realism that depend on strong versions of correspondence and isomorphism.

**10. Dependency Relations, Ontology and Model Pluralism**

In thinking about particular formulations of scientific theories (and models) we can often make a rough and ready distinction between (1) what the theory tells us about dependency relations (how certain magnitudes or properties depend on others) and (2) the entities or objects that the theory seems to tells us exist (the “ontology” or ontologies associated with theory), at least on a literal interpretation[[36]](#footnote-36). Roughly speaking dependency relations are encoded in the laws or causal generalizations of a theory. As an example of (1) Newtonian gravitational theory tells us about how the motions of bodies depends on their masses and the distances between them. As an example of (2) Newtonian gravitational theory can be understood as claiming that there exists a force, gravity, that acts instantaneously at a distance. It is an interesting fact that theories that are the same or equivalent in what they say about (1) can be very different in what they say about (2). For example, in Newton-Cartan version of Newtonian gravitational theory, the role of the Newtonian gravitational force is re-interpreted in terms of spacetime geometry. The ontology changes dramatically but the dependency relations are the same. Another interesting fact is that at least in many cases there is far more continuity and preservation of content between successive theories at the level of (1) than at the level of (2). General Relativity, a successor theory to Newtonian gravitational theory, allows for recovery of the laws of the latter theory in the weak/slowly changing gravitational field/ low velocity limit. But the apparent ontology of GR is very different from the ontology of Newtonian gravitational theory. In turn GR is widely expected to break down at sufficiently small length scales (the Planck length) and presumably the ontology of a full quantum mechanical theory of gravity will be very different from that of GR. Nonetheless one expects that the dependency relations postulated in GR will continue to be correct in other, more “macroscopic” domains of application.

I take these observations and others made above to suggest (I don’t claim that they conclusively establish) that insofar as (1) and (2) can be separated it is often claims about (1) that are the more secure and important forms of scientific knowledge, at least insofar as we are concerned with physical theories that have a relatively fundamental status[[37]](#footnote-37). It is clear how information about dependency relations contribute to predictive success. And insofar as explanation has to do with correctly answering what-if-things-had -been-different questions (w-questions), as argued in Woodward, 2003, it is again (1) rather than (2) that contributes to explanatory success[[38]](#footnote-38).

The gravitational examples immediately above involve fundamental physical theories but I also think that at least one important feature of the examples fits a great deal of science outside of physics. This feature has to do with the domain or scale dependence of successful theorizing in many areas of science. That is, in many cases we have collections of different theories or models of what in some sense are the same systems, each with their own domains or regimes or proprietary explananda. These theories/models work well (in terms of getting the dependency relations right or almost right) in their proper domains of application but either break down in the sense of yielding false predictions outside of their domains or else are unable to make predictions at all for reasons having to do with computational and other sorts of epistemic limitations. They are thus “effective” theories rather than theories of everything. Often not just in physics but elsewhere, the domains in question are distinguished by means of scale-based considerations—either spatial, temporal or energetic. For example, Herz et al (2006) describes a number of models, at different (spatial) scales or levels, of neuronal behavior. Fine-grained multi-compartment models divide the neuron into a number of different “compartments” and attempt to understand how the details of its spatial structure contribute to certain features of its behavior. Computational and analytic impossibilities make it impossible to use such models to predict or explain qualitative features of the behavior of whole neurons such as the generation of the action potential. Instead a very different single compartment model such as the Hodgkin- Huxley model may be used for this purpose. Herz et al, also describe models at additional levels (a total of 5) both “above” and “below” that of the Hodgkin-Huxley model[[39]](#footnote-39).

Pragmatist philosophers of science need not regard this (multiple models at different scales, each with their own proprietary explananda) as a troubling state of affairs. A theory or model can do a very good job of capturing dependency relations (and explaining, at least on a w-conception of explanation) within a given domain or a given scale, even if it is not successful at others. Theories/models of the same system but at different scales or designed to capture different features of those systems thus can viewed as complimentary to the extent that (as is typically the case) they make predictions about/provide explanations of different explananda, rather than conflicting predictions about the same explananda—the models of neuronal behavior at different scales mentioned above provide a good illustration of this[[40]](#footnote-40). Of course, as noted above, it is sometimes (but by no means always true) that to develop an empirically adequate theory of a given set of explananda at a particular scale one needs *some* information from other scales, so that getting theories originally designed for different explananda to talk to one another becomes an important problem. (“Some information” does not mean that all of the information provided by a model at another scale is needed or relevant—this is rarely the case.) Recognizing this is different from saying that a plurality of models is in itself objectionable or that we should always attempt to replace these with a single theory of everything.

So far I have focused on pluralism of theories/models in connection with dependency relations, where I suggest that matters often go relatively smoothly, in the sense that there is complementarity rather than conflict. The problematic issues seem to be the ones that arise in connection with (2)—the ontological commitments of the models, at least in important parts of physics. Here it is tempting to see conflict or outright contradiction rather than complementarity. If, say, we use the Navier-Stokes equations to model aspects of fluid flow, aren’t we assuming that the fluid is a continuous substance of some kind? How then can we consistently also model the fluid as composed of molecules? How can we model spacetime as flat with a gravitational force that acts at a distance but also curved with no such force?

One way of responding to such conflicts is to argue that in each case just one of the conflicting ontologies must be correct—typically this will be the ontology associated with the more fundamental theory. Thus one might conclude that continuum models of fluids and gases are “false” because they involve commitment to a false ontology and so on. This in turn generates the various puzzles, gestured at above, about how false models nonetheless seem to be illuminating (or perhaps leads to a denial that they are illuminating). Rather than wading yet again into these waters let me just reiterate the observation that the ontologies that go with many theories seem to be the least stable and secure part of those theories and arguably the parts that do the least work when it comes to prediction and explanation. Perhaps, then, there is something to be said for taking such ontological commitments less seriously or having a more relaxed and permissive attitude toward them. To the extent that one does this, conflicts in ontology will seem less alarming[[41]](#footnote-41).

**11.** **Modality Again[[42]](#footnote-42)**.

One of themes illustrated by the examples above is that we should not identify the issue of whether a claim or model conveys correct information about how matters stand in the world and whether it can be assessed accordingly (that is, as true or false, accurate or inaccurate and so on) with the issue of whether that claim or model stands in a mirroring or correspondence relation to nature. The assumption of correspondence leads one to look immediately for things or entities or structures that are the worldly relata of the correspondence relation—high dimensional Hilbert spaces as “real” and so on. The pragmatist thinks instead that the mathematics of Hilbert space can be used to say true things about quantum mechanical systems without its functioning in such a simple mirroring sort of way.

Let me conclude by floating the possibility that a similar point might hold for the use of modal concepts in science. (I readily acknowledge that what follows is more like what Richard Feynman once called an idea for an idea, rather than anything worked out). In thinking about the physical modalities, I have always been struck by two considerations that seem prima-facie to pull (or at least pull me) in opposite directions. On the one hand, it is very hard to deny that claims about physical possibility (and perhaps even more so, claims about physical and causal dependence) are the sorts of claims that are true or false and that they are in some sense answerable to the world. For example, in my view it is very hard to understand what could possibly be going on when one infers to causal conclusions on the basis of experiments if this were not the case and if the modality associated with causal claims was simply a “projection” of our inferential practices onto the world. Moreover, attempts to reduce modal claims to non-modal claims about regularities and so on seem uniformly unsuccessful. This may seem to point us in an anti-Humean direction. However, on the other hand, attempts to accommodate the observations just described through the postulation of “non-Humean stuff” (John Earman’s terminology)—powers, dispositions, relations of necessitation among universals and so on ) seem unilluminating. As suggested above, they don’t provide ordinary scientific explanations of anything and don’t provide guidance for how we should test or reason about causal claims. Moreover, at least in many cases, this postulation leads to all sorts of puzzles of a placement sort—about the “ontological status” of these non-Humean things and relations.

My suggestion is that perhaps there is a way to accommodate both of these reactions by analogy with the treatment of models and theories floated previous sections. I assume that there is nothing wrong with the idea that modal and causal claims are truth apt and that, when true, are informative about the world. Where we go wrong is when we try to understand the truth-aptness of modal claims in a literalist or object naturalist fashion – that is, by postulating special things or entities (or entity-like properties) to serve as correspondents for such claims. Thus while it indeed may be true that (i) it is possible that I might be now standing two feet to the left of where I am now standing, we should not try to interpret this claim in terms of the existence of some special entity such as a possible world in which I am two feet to the left. This misunderstands the way that (i) works, construing it on the model of something like “the cat is on the mat” (where the referents of “cat” and “mat” are unproblematic and even the “on” relation has a straightforward correspondent) and then inventing special new stuff to accommodate this construal.

Similarly, (ii) “aspirin causes headache relief” is unproblematically true, but I think that it is consistent with this to doubt that “causes” has the function of naming some special sort of entity—the “causal tie”-- which we might go on to isolate and investigate in the way that we might investigate, say, cats, electrons or the Higgs boson. Talking about causation in the context of a claim like (ii) is better viewed as (something like) a claim about what would happen if one were to ingest aspirin in certain circumstances (in cases in which other causes of headache relief are not present etc.). Of course there are conditions in the world that support this and other causal claims when they are true but these are often complex and distributed in ways that may not be immediately transparent. Moreover the conditions in question may be non-trivially different for different causal claims. This is why, although one finds plenty of discussion of causation, methodologies for causation inference and so on in science, one does not find investigations into the kind of stuff or thing causation is in the sense one finds such investigations for, say, electrons or DNA. Put differently, “causation” is a fit subject for methodology but not necessarily for ontology, at least if one has the usual expectations of what an ontology of causation would look like.

Let me conclude with another analogy which brings together some of themes about literalism as applied both to theories and modal claims. Consider a 6n phase space representation of an n-particle system conforming to the laws of classical mechanics, with the representation in terms of the three position and momentum variables for each particle . Those with an inclination toward literalism may interpret this representation (or at least the claim that it is used correctly to represent some physical system) as the claim that there “exists” an abstract space having the features of the phase space—it has 6n dimensions and so. (In using this representation we are “ontologically committed” to such a space.) One might then worry about how this space relates to the more familiar three dimensional space in which we apparently find ourselves or to other abstract spaces that might be constructed. The attitude I am recommending allows us to use the phase space representation and to think of it as conveying information about what the represented system is like (in particular its modal structure) without interpreting it in this way. The use of this representation will be appropriate if for example the system represented really does have 6n degrees of freedom, if the position and momentum variables for each of the particles can vary independently of each other and so on. It is claims like this that are implied by the use of the phase space representation, not the claim that space “really” has 6n dimensions. Note that in this way the phase space representation encodes information about physical modality and possibilities but it does not do so by postulating possibilia or the like.

**13. Conclusion**

I’ve tried to sketch some elements of a possible pragmatist philosophy of science. However, my overall goal is not so much to convince readers on particular points but rather to point to a path forward: there are many issues and problems that belong to general philosophy of science that can be approached in a way that does not involve ambitious metaphysics. I’d be very pleased if readers were convinced of that general point even if they disagree with the details of much of what I have said.

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1. This was written as a sort of position paper for the PragMAPSII conference at the University of Oslo in 2016. It was my attempt to engage with a number of the themes that were to be discussed at that conference. The idea was to get some positions on the table which could then be discussed. This helps to account for the somewhat freewheeling character of the paper (it was intended to provoke), as well as why I chose to refer to some ideas (discussed by other participants at this and other PragMAPS conferences) and not others that are equally worthy of discussion. In addition to PragMAPS participants, I would like to thank Bob Batterman, Porter Williams and Mark Wilson for comments on an earlier draft and Holly Andersen for very detailed and helpful comments on the present version. [↑](#footnote-ref-1)
2. Just to clarify: I do *not* mean by this remark to suggest that there is necessarily something wrong or unfortunate about philosophers of science being primarily located in philosophy departments. In fact, I think that philosophy of science as a discipline would benefit from more contact with the rest of philosophy and particularly from the influence of standards of argument and precision found in the best “non-science” philosophy. I just mean to draw attention to the tension between, on the one hand learning a lot of science and scientific methodology and, on the other hand, keeping up with developments in philosophy. I see the current interest in metaphysics of science as one response to this tension: the philosopher of science reconnects with philosophy by doing metaphysics. [↑](#footnote-ref-2)
3. Some may wonder: why try to do “general” philosophy of science at all, rather than philosophy of the more specific sciences? There are a number of possible responses in addition to those already gestured at (disciplinary fragmentation, connection with the rest of philosophy etc.): 1) issues often thought of as within the province of general philosophy of science-- having to do with e. g,., evidence and theory testing, causal reasoning, explanation, inter-theory relations theory structure and so on are increasingly discussed in interesting and sophisticated ways in other disciplines such as statistics, artificial intelligence and psychology. General philosophy of science can (and has) contributed to this discussion and doing so enables it to retain contact with these other disciplines. That the other disciplines are concerned with these problems suggests both that they are real and that it is possible to make progress on them, even if philosophers have not always succeeded in doing so. 2) (Here I am going to step on some toes) Philosophers of science should ask themselves where their comparative advantage lies— what special skills and background they have that best enables them to contribute to the general intellectual and scientific culture. I don’t deny that some philosophers of discipline X may be in as good as a position to contribute to X as researchers whose professional home is within X itself, but this does not seem to me to be common. In many cases the abilities and background of philosophers of science are better suited to general philosophy of science. [↑](#footnote-ref-3)
4. An inevitable response to this distinction will be that it is less clear and sharp than I suggest. I won’t try to fight this battle here. I do claim (i) that the distinction is analytically defensible and (ii) it is corresponds to something sociologically “real” . Papers in the *Physical Review* contain (at most) minimal metaphysics and are usually not written by philosophers. Papers in *Oxford Studies in Metaphysics* are written by philosophers, often contain ambitious metaphysics and likely not publishable in the *Physical Review.* There is a difference here that we can recognize, even if there are borderline cases.

   I also recognize that one way of attacking the minimal/ambitious distinction is to contend that when we try to “interpret” scientific theories we find that many make ambitious metaphysical claims and that these are supported by the empirical success of those theories—we can read off ambitious metaphysics from the science. My reasons for skepticism about such contentions can be found in Sections 5-9. [↑](#footnote-ref-4)
5. In other words, if one wants to engage with science in a detailed way, there are at least two different ways of doing this. One is to divide up the territory in terms of currently recognized scientific disciplines: philosophy of physics, biology etc. The other way focuses on specific problems that may be common to a number of different scientific disciplines. I recommend that some philosophical work engage with the latter. [↑](#footnote-ref-5)
6. For example, philosophical proposals regarding the best way to understand the relationships between psychology and neurobiology have typically considered only a very small range of possibilities—Nagelian type-type reductions, supervenience relations with token identities and so on. But there are a huge number of other possibilities regarding relations between theories and model at different “levels’, instantiated in other areas of science, that are arguably better models for the psychology/neurobiology relationship. I intend “better” here has a normative as well as a descriptive component—e.g., we are more likely to discover informative relationships between psychology and neurobiology if we do not look for type-type identities. [↑](#footnote-ref-6)
7. Examples of work of the sort of work I have in mind include Batterman, 2013 and Wilson, 2017. [↑](#footnote-ref-7)
8. It is sometimes claimed that there is no such thing as “the scientific method”. If the quoted phrase refers to a single method for all of science at the level of generality sought by philosophers like Popper and Lakatos, I agree. But this is compatible with their being many more specific methods employed in particular sciences. I see these as belonging to methodology—see below—and very much within the purview of philosophers of science. [↑](#footnote-ref-8)
9. I emphasize that, although, as will become apparent below, I regard the discovery of truths about nature as among the goals of the scientific enterprise, science has goals (e.g. building) that don’t reduce to the discovery of truths. [↑](#footnote-ref-9)
10. Work of this sort on causal cognition is discussed in Woodward, forthcoming. [↑](#footnote-ref-10)
11. Consider the ubiquitous references to “logic” in various influential programs in mid-twentieth century philosophy of science: the logic of confirmation, the logic of explanation and so on. Exactly what “logic” means in these contexts is not so clear, but one consequence of this form of framing is that means ends/analysis is not employed—criteria for hypothesis “confirmation” are not advocated on the means/ends basis that following them will lead to some desirable outcome like truth but rather have some other basis—perhaps “intuition” or ease of analysis with the tools of first-order logic. [↑](#footnote-ref-11)
12. A simple illustration is provided by the common practice of using regression equations as a general method for inferring causal relations from statistical data and “controlling” for everything the researcher can think of in estimating these equations. This demonstrably leads to incorrect results for many reasons including “collider bias”. See Pearl (2018) for a discussion of this example and many other illustrations of bad but common statistical practice. Contemporary economics is also fertile ground for informed philosophical criticism. [↑](#footnote-ref-12)
13. More generally of course it goes without saying that pragmatist philosophers of science will not think that labeling a consideration as “pragmatic” is not good grounds for dismissing it. [↑](#footnote-ref-13)
14. Again, one can think of such issues as part of metaphysics but if so, I claim that they only involve “minimal metaphysics” [↑](#footnote-ref-14)
15. In this respect I fully agree with Ruetsche (this volume) when she writes:

    I don’t take “what theories represent” and “what theories are for” to lie on opposite sides of a methodological fault line. Focus on one aspect needn’t preclude focus on the other. After all, a theory’s representational capacities could have *something* to do with its patterns of and aptness for use, and our aims in applying it could shape its representational capacities!

    More generally, attention to the role of the user of models and theories doesn’t mean that we should neglect the capacity of the theory to provide information about nature; instead the two are complimentary. [↑](#footnote-ref-15)
16. If this seems oxymoronic the idea is that an overly literal interpretation of what they theory says may not capture the truths it conveys about nature. As a simple illustration, a model in evolutionary biology may postulate a population with an infinite number of rabbits. It would be a mistake to interpret this a claim about the cardinality of the rabbit population (the literal interpretation) ; its use is rather a way of claiming that certain other things are true of the population—e.g., that drift is unimportant and that mating is random or nearly so. These last claims will be straightforwardly true or false and the model will be appropriate if they are true or nearly so. We don’t need to appeal to explanatory falsehoods or fictions to capture what is going on in such a case. [↑](#footnote-ref-16)
17. Ideas about “reference magnetism” might be understood as claiming otherwise but they come with metaphysical commitments that will not be attractive to pragmatists. [↑](#footnote-ref-17)
18. This proposal cross cuts some other possibilities for engaging with metaphysics. For example, John Dupre (2012) advocates the development of a distinctively pragmatist metaphysics, organized around notions of process, at least for biology. Depending on the details, this might be a kind of minimal metaphysics, centered on general empirical claims about the nature of biological phenomena and how these fit with practices of biological investigation and theorizing. Alternatively it might involve far more ambitious forms of metaphysics, as in the process philosophies of Whitehead and Hartshorne. I take Dupre to have the former in mind. [↑](#footnote-ref-18)
19. My target here is reduction of one set of concepts like cause, probability etc. to

    another, not the reduction of one scientific theory to another. [↑](#footnote-ref-19)
20. The metaphysics literature on laws of nature provides one illustration of this tendency to interpret scientific claims as claims about the existence of objects, even when this seems inappropriate. In a recent survey, Bhogal (2020) describes laws of nature as “things like the Schrodinger equation” and suggests that many anti-Humeans treat laws as “separate governing entities” . Bhogal’s sympathies are with Humeans but (apparently) not because he thinks there is anything wrong with thinking of laws as things or entities—he just thinks the things with which laws are to be identified are patterns or regularities in Humean mosaic. But even putting aside this elision between the Schrodinger equation, which is a mathematical structure and what in the world it is answerable to, it seems bizarre, whether or not one is a Humean, to think of laws as things or objects, with the accompanying thought (if one is a non-Humean) that these might stand in “governing” relations with more ordinary objects. Laws describe how the behavior of some objects depends on the states of others, but laws are not entities that make things happen in the way that electrons do. Pragmatists need another way of interpreting laws besides in this object oriented way.   
     [↑](#footnote-ref-20)
21. Discussions of whether numbers “exist” (where this involves asking what sorts of objects numbers are) and whether when mathematics is used in science, this requires an “ontological commitment” to the existence of numbers are an extreme example of the tendency to try to interpret a discourse in terms of the “objects” it is about. [↑](#footnote-ref-21)
22. 2 In fact, these are dubious candidates for *naturalistically* acceptable properties but let’s put this consideration aside. [↑](#footnote-ref-22)
23. At this level of generality such goals may seem anodyne and uninformative. They acquire more content when combined with assumptions about feasibility etc. [↑](#footnote-ref-23)
24. I take it that anthropological in this context means something like “descriptive/interpretive”. Historical or psychological investigations can thus fall under this rubric. [↑](#footnote-ref-24)
25. As Price remarks, there is a tendency to suppose (assuming one wants to be some kind of naturalist) that object naturalist concerns are prior to and more fundamental than subject naturalist concerns. I agree with Price, however, that matters are often the other way around, in part because without some (subject-naturalist) account of what one is doing with some bit of discourse (or theory or model), one does not know whether or in what respects an object naturalist construal is appropriate. [↑](#footnote-ref-25)
26. For example, in causal analysis, the notion of a total effect corresponds to a different manipulation- related question than the notion of an effect along a causal path—see Woodward, 2003. Finding a function that maximizes accurate predictions for random draws from an i.i.d distribution on the basis of previous draws from t he same distribution is a very different problem than predicting the future positions of the planets and so on. [↑](#footnote-ref-26)
27. See Batterman, 2013. [↑](#footnote-ref-27)
28. Lots of claims in contemporary philosophy of science are framed at a level of generality and vagueness that make any sort of serious means/ends analysis impossible—for example, claims about the virtues of “inference to the best explanation”. In standard treatments, the notion of a “best explanation” is left unspecified and there is also no analysis of why this “method” of inference should be expected to lead to truth or anything similar. In my view, pragmatist philosophers of science should avoid claims and concepts of this sort. A similar complaint holds for many appeals to “simplicity” in philosophy of science (see below). [↑](#footnote-ref-28)
29. Many other examples of means/ends reasoning in addition to those discussed below can be found in statistics, machine learning, and formal learning theory. Additional philosophical examples include Sober on black box inference, and Glymour (2001) on inference from lesion data in cognitive neuroscience. Let me add that although in this section I focus on means/ends reasoning in which the goal is truth or good error probabilities of truth and falsity, there can of course be other goals besides truth. [↑](#footnote-ref-29)
30. Suppose that the data generating process is represented by *Y=bX+ U*, with *X* and *Y* the candidates for cause and effect variables, and *U* an error term that is *correlated* with *X*. As is well known, when *X* and *U* are correlated, we cannot use the simplest possible estimator for *b*, involving ordinary least squares. We can, however, reliably estimate *b* if we can find an instrumental variable *Z* for *X* with respect to *Y.*  The are various ways of characterizing such an instrument. One is that *Z* is an instrumental variable that (i) is associated with *X,* (ii) is independent of *U,*  and (iii) is independent of *Y* given *X* and *U* . If *Z* meets the above conditions, then *b\*= cov (Y,Z)/cov (X,Z)* is an estimator for *b,* interpreted as the *causal effect* of *X* on *Y*. Within an interventionist framework this interpretation is justified because (or to the extent that) Z functions as an intervention on *X* with respect to *Y*. [↑](#footnote-ref-30)
31. It might be thought that this question has an obvious answer—the observers are just trying to “describe” the situation they see. Again, I deny that this notion of pure description, independently of more specific goals and purposes, has any application to science. [↑](#footnote-ref-31)
32. After all, as a matter of empirical fact classifications based on color are not very useful for many predictive and explanatory purposes, however “natural” they may seem [↑](#footnote-ref-32)
33. Recall that by true claims I’m including claims nearly true or true enough as to make no difference. [↑](#footnote-ref-33)
34. To be clear, I do not mean to claim that every feature of a useful model or theory can be construed in the way described—that is, as providing true information about nature although not necessarily the information that is suggested by a literalist construal. Some predictively useful models contain assumptions that likely can’t be reconstrued in this way—e.g., models of the nucleus that attribute a shell structure to it /(Morrison. 2015) My point is rather that many of the examples of model and theory assumptions that philosophers have regarded as “fictions” or falsehoods” need not be construed as such. Ironically, “models as fictions/falsehoods” views often tacitly adopt the very literalist expectations that they officially reject. [↑](#footnote-ref-34)
35. For further defense of this view, which I have called “instrumental realism” see Woodward, 2003 and for sympathetic recent discussion see Saatsi, 2019. I acknowledge that this label is not ideal, but can’t think of anything better. “Effective Dependency Relation Realism” does not exactly trip off the tongue. [↑](#footnote-ref-35)
36. Cf. Maudlin (2019) p. xi. “A physical theory should clearly and forthrightly address two questions: what there is, and what it does”. Of course in recommending that philosophers should focus just on the second question at least when it comes to understanding scientific progress, my view is very different from Maudlin’s. I recognize that a natural thought is that Maudlin’s two questions are not very readily separable but I don’t think this is right, at least in many cases. I might, for example, use non- relativistic quantum mechanics and the Born rule to show how the probability of a particle penetrating a potential barrier depends on the potential and on the kinetic energy of the particle, with this being non-zero when the energy is less than the potential, thus answering w-questions about the probability of penetration. Doing this counts as an explanation by my lights but it does not require answering questions about the ontology of the wave function. [↑](#footnote-ref-36)
37. I don’t mean that the ontology associated with a theory is unimportant in science. It may, for example, help to facilitate visualization and calculation as with various mechanical models of the ether. It may also help with extending the theory to new applications. But this does not require taking the ontology as a literal truth that will be preserved across theory change. [↑](#footnote-ref-37)
38. Thus on this picture, getting the ontology right (whatever that might mean) counts for less in successful explanation than getting the dependency relations right. Note also that on this view, “truth” or something in the neighborhood of truth matters for successful explanation. The question is truth about what? I suggest it is truth about the dependency relations holding in a particular domain, rather than truths about ontology.

    It is also worth noting in this connection that many prominent arguments (e.g., Laudan) for the “pessimistic induction” and similar conclusions focus on failures of preservation of the ontologies of successive attempts at fundamental theories: it is noted that particle theories of light are replaced by theories according to which light is a longitudinal wave, then by theories that take light to be a transverse wave, each accompanied by changing accounts of the mechanical ethers in which these “vibrations” occur. These ontology-related features of scientific change to which Laudan and others point are quite real (as noted above) but this is consistent with the observation that there is accumulation of increasing knowledge of dependency in optics, as noted by Saatsi, 2019. It is ironic that defenses of the existence of scientific progress and the cumulative character of scientific knowledge have very often been in framed in claims about terms of continuity of reference for “unobservable entities” – that is, as claims about preservation of ontology. If my argument above is correct, this is not where one should look for continuity and progress in science. [↑](#footnote-ref-38)
39. For more details, see Woodward, 2018b. [↑](#footnote-ref-39)
40. In other words, as I understand “conflict” it arises when two different models make inconsistent predictions about what will happen under the same conditions—model 1 says that under condition X, Y will result and model 2 says that under condition X, Z≠Y will result. The point that models at different scales are often complimentary is developed in detail in Morrison 2015. [↑](#footnote-ref-40)
41. To be clear: of course I don’t deny that there are “ontological” claims made by scientific theories that are secure and unproblematic: we are not going to change our minds about the existence of atoms and molecules or the chemical composition of DNA. The philosophical debates about “ontological status” typically arise for other sorts of claims, often but exclusively for theories that are taken to be “fundamental”, as in the examples discussed above. It is these cases where we don’t seem to find continuity at the level of ontology. Above the fundamental level, we seem to have “effective” or “scale-dependent” ontologies, that are often preserved across theory change [↑](#footnote-ref-41)
42. For a treatment of modality and causation in a somewhat similar spirit, that emphasizes the role of phase space in representing possibilities, see Andersen, 2017. [↑](#footnote-ref-42)