**Sketch of some themes for a pragmatic philosophy of science**

James Woodward

HPS, Pittsburgh

Very rough draft 5/28/15

Note to reader: this draft tries to sketch some ideas about what a “pragmatic” approach to philosophy of science might look like. My apologies in advance for the disorganized and somewhat stream of consciousness character of this draft and also for its abstractness. Also for the many typos it likely contains. My goal is to get us talking and provoke reactions. The draft should be taken in that spirit.

**I. 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 “pragmatic” or as versions of “pragmatism”. However, pragmatism in both its classical and more modern has taken on many different commitments and I will be endorsing some of these and rejecting others—indeed, I believe that some elements prominent in contemporary formulations of pragmatism are quite contrary in spirit to a genuine pragmatism. Moreover, I will be including elements and ideas that may not be found in any of the classical pragmatists. In fact, I think it entirely possible that the particular collection of ideas I will be describing 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, and Price, among others.

1. **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 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 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.

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”—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 or other that is intellectually valuable to the science that is reported. This might be something distinctively philosophical but if not that, it should at least be something analytical or that allows one to see the science in a new light or that contributes in some way to the scientific topic itself or to methodological issues surrounding it.

 However, if we look to so-called general philosophy of science 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. But we don’t really have replacements for these. 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. (Again, I’m caricaturing—for example Hempel himself had interesting things to say about the philosophy of the social sciences and psychiatry).

 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 the rest of philosophy. But what should philosophers of science do if, for whatever reason, they don’t want to climb on the metaphysics bandwagon?

 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 an alternative picture of what philosophy of science might be or what philosophers of science might do, a picture that does not put metaphysics at the center of things but also does not return us to the bad old days of theory T. The organizing idea is that we need a genuinely *pragmatic* philosophy of science—“pragmatic” in a sense I will try to describe. This idea highlights the twin projects of (i) description/interpretation and (ii) methodology as central to philosophy of science. Insofar as these were always concerns of general philosophy of science, it attempts to retain some continuity with this enterprise and to revivify some aspects of general philosophy of science. As will become apparent, one way in which (I will suggest) 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 that are natural foci for 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, but 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.

 A second 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 both descriptively inadequate and unhelpful even as (realistic) aspirations 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. Again, this will require more than mere science reporting, but the results will not be metaphysics either, at least as I understand metaphysics.

The remainder of this paper is organized as follows. Section 3 sets out some overall themes and commitments of a pragmatist philosophy of science, Section 4 discusses the contrast between subject and object naturalism and its relevance to philosophy of science, Section 5 discusses some examples of means/ends justification (and its absence.) Section 6 concludes with a discussion of representation in science.

1. **Some overall themes**.

1. **Usefulness and Utility.** 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. 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* as an account of models and theories, as philosophers of science understand instrumentalism.
2. **Description and Evaluation**. This enterprise has both a descriptive or interpretive component – characterizing accurately relevant aspects of scientific practice and reasoning—and, I would argue, an evaluative or normative component: we want to understand why various methods and strategies are successful in delivering knowledge, to the extent that they, and 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. A pragmatic philosophy of science thus makes *methodology* a central focus.
3. **Means and Ends**. The enterprise described under 1 and 2 is naturally pursued within a means/ends framework: Investigators have certain ends or goals. A very partial list includes but is not limited to successful prediction, causal analysis and explanation, description and classification for specific purposes, and building and making things. 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.

I believe that many features of scientific inquiry can be understood within this general framework, as I will try to illustrate below: at least some 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 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 representationally, in the sense of mirroring or picturing nature. So the idea is to apply the means/ends framework at, so to speak, many different levels and to many different problems in philosophy of science. The picture is thus, in a sense, pragmatic twice over—science itself is thought of as a pragmatic, means/ends enterprise and we use this pragmatic 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.

1. **An Analogy.** As I have been emphasizing on this approach to science, description and evaluation go 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 has and then tries to understand the means by which the system achieves these goals (to the extent that it does). 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.

 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 and Patricia Cheng, among others.

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

**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. What I have in mind is different in several respects. First, the sort of methodological inquiry I recommend involves hypothetical rather than categorical imperatives. They take the form: if you want to achieve goal G, you should employ means X and not means Y. They are not unconditional apriori requirements. Such hypothetical imperatives are typically established from such sources as mathematical demonstration, heuristic argument, simulation, and empirical investigation (see below). Second, as an empirical matter, methodological criticism and advice seems more appropriate in some areas of science than others and philosophers should focus on the 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. Pragmatic 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.

1. **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) . 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 the “merely” in front of pragmatic and also resist sharp distinctions between “pure” and “applied” science—more on this below.
2. **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, and with physical possibility and necessity. 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 (or can) 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. We don’t just infer, we *do*.

I believe that spectator approaches to modality tend to encourage either (7.1) a tendency toward misplaced reification or, alternatively (7.2) skepticism or various forms of subjectivism about modal notions. A paradigm of (7.1) is Lewis’s treatment of modality in terms of “possible worlds. Illustrations of (7.2) include the view that modal claims are unclear or not part of legitimate science (e.g., Quine) or that they are rooted grounded in facts about our projective or epistemic organizing activities. I believe that both alternatives (7.1) and (7.2) 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 7.1 or 7.2?

1. **Spectator Orientation as rejected by some but not all self-identified pragmatists**. I said that a distaste for spectator theories of knowledge is far from universal among pragmatists. I agree with Peter Godfrey-Smith 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 or alter or interact with nature. The “action” component of pragmatism is 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.
2. **Focus on practice, not just verbal behavior and belief.** Reinforcing this passive observer picture is another focus that is characteristic of a number of contemporary pragmatists – again Quine and Rorty come to mind-- and that is suggested by last observation 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 or as representational vehicles of some other sort) 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 and belief formation and many non-verbal forms of behavior.
3. **Subject versus Object Naturalism**. Another way of describing the interpretive/evaluative emphasis in #1) and #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 connecting with Huw Price’s notion of *subject naturalism* and the contrast he draws between this and what he calls *object naturalism.* Huw describes this contrast primarily in terms of two different ways of approaching *discourse* but, as already explained, I would favor broadening the focus so as to include forms of practice that are not discursive, as well as those that are. But 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* (aka human beings) 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) 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 correspond to or grounding these practices. Pragmatists, I assume, will want to adopt some form of naturalism but as Huw’s discussion reminds us, there are different ways of being a naturalist about some body of discourse or practice. Often the most promising strategy for naturalists is to try to understand what subjects are doing within a naturalistic framework, instead of (or in addition to) only trying to “naturalize” the products (discourse, representations etc.) they produce.

 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, 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 subject naturalizing accounts. On the contrary, as emphasized by Mark Wilson 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. 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. However, these roles for the world are not very well captured by philosophical treatments which make use of problematic ideas about correspondence and isomorphic representation in trying to understand science—a subject to which I now turn.

**10) Skepticism about (or rejection of) REPRESENTATIONALISM**. 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 works, at least when representation is understood, as it often has been, in terms of notions like mirroring, picturing, correspondence and the like. This attitude continues in the work of more contemporary pragmatists like Rorty and Huw Price. I think that philosophers of science should adopt this skeptical attitude, at least toward many of the structures and models employed in science.

 In what follows in the main body of this paper, my focus will not be on representation in general (I leave open the issue of whether representationalism is OK for sentences of “the cats is on the mat” variety—perhaps it is, and the problem is with assimilating other sorts of claims and representations to this paradigm), but rather on certain very popular claims about 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 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. partial isomorphism etc.) between a successful theory or model and the system modeled. (One might describe this as big R representationalism: REPRESENTATIONALISM). As already suggested, 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) REPRESENTATION*. In other words in rejecting representationalism conceived in terms of picturing or isomorphism , we needn’t embrace some form of idealism, linguistic or otherwise. Nature or the world remain as central parts of the story; it is just that they don’t figure in it in the way that REPRESENTATIONALISM suggests.

**11) Against Eschatological or End-of -Days -Science**. A common way of approaching many issues in philosophy of science, especially among metaphysically inclined philosophers, 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 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 CR 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 conceptualizes matters very differently: 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, manipulate etc. These subjects 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, the final theory, the full description of causal reality and so on cannot be part of this story. (Saying that current science reflects the processing limitations of human beings when dealing with the full Humean mosaic etc. 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.) Pragmatist philosophers of science should accordingly 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 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.

**12. Metaphysics**. The relation between pragmatism and metaphysics is obviously central to this conference. One possibility is to try to develop a distinctively pragmatist metaphysics, perhaps organized around notions of process, as John Dupre suggests. Whatever the value of this project, I’m inclined to take a different tack[[1]](#footnote-1). As I’ve already suggested, I think that many of the issues about science described above and below are issues that can be pursued without doing much, if any, metaphysics, at least of the analytic sort that flourishes in contemporary philosophy. I thus suggest that there is much to be said in favor of what Huw calls quietism about metaphysics—the pragmatist philosopher of science should just decline to do metaphysics or make ontological claims or take a stand on the issues about grounding, truth conditions, and so on that dominate contemporary metaphysical discussion. Of course this requires that it be possible to do this. This is possible because, as nearly as I can tell, a pragmatic philosophy of science of the sort described above can be pursued independently of heavy duty metaphysics. Indeed, as already intimated, even if one were to have definitive answers to the fundamental questions of metaphysics, I believe that these would tell us very little about the methodological/interpretive issues that are the province of the pragmatic philosopher of science. Part of my reason for thinking this is that metaphysical discussion, as currently conducted, seems uninterested in making contact with these issues: To take just one example, 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 that are natural topics for pragmatic 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.

Although, as I say, there is much to be said for quietism about metaphysics if one is pragmatic philosophers of science, I think this quietism itself should be vigorously defended. That is, pragmatic philosophers of science should insist that, given their goals, they don’t have to do metaphysics and that the projects they wish to pursue are important and valuable in their own right. When criticized by metaphysicians for the failure to articulate proper metaphysical underpinnings for science (“grounds” etc.) , pragmatists should push back by demanding to be shown why, given their goals, such underpinnings are necessary.

**13. Finally, Reductionism**. Many philosophers of science, again especially those who are metaphysically inclined, are attracted to one or another variety of reductionism, especially about causal and modal notions, with so-called Humean reductivist projects being very prominent among these. The questions a pragmatic philosopher of science should ask about such projects are these: 13.1) 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 does not require carrying out a reduction, pragmatic philosophers should feel free not to provide it. 13. 2) More generally, the pragmatic philosopher should ask the 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 pragmatic goals described above—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?

**4. 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. Huw 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” (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 Huw 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 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 a characterization that I think is fully consistent with Huw’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) and an accompanying thesis about what is required for a representation to be literally 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 accurate or true. In other words, the truth or accuracy of the representation requires that certain objects exist, where this is often accompanied by the assumption that these can be read off in a rather straightforward way from the representation itself. (We understand the notion of “object” here broadly so that it includes properties or relations, but one of the rules of the game is that these are understood in a thing-like manner—they are entities of some kind, so that one can raise questions about their “ontological” status.) In other words, one attempts to interpret the discourse “objectively “in the very literal sense that one takes it to be about the existence of *objects a*nd their properties—it is only if one can find such things that the claims in the discourse can be taken as true. If one can find the appropriate objects and if they are uncontroversally naturalistically acceptable, then one has given an acceptable naturalistic construal of the representation. If 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, then one faces what Huw 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 as not (at least literally) the sorts of things that can be true or false. 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. 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. REPRESENTATIONALISM and the kinds of concerns that underlie object naturalism tend to lead to metaphysics when applied to discourse and practices whose corresponding objects are not obvious—one either 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.

 Using one of Huw’s running examples, consider moral claims and discourse as an illustration of these possibilities. Presented with the contention that, say, 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”[[2]](#footnote-2). Alternatively, the investigator might want to consider some 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, an alternative is to construe the claim as not a candidate for literal truth of falsity at all, and to re-interpret it as, say, (just) 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. What is the point (or points) or goals or function of these practices? 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 commonly held social goals, the advancement of proposals that others adopt and act on certain rules rather than others (where this might involve arguments that the rules are feasible and motivating for others and so on. 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 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 not the sorts of things that can be true or false or 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 Huw says at one point, “anthropological” (to which I would also add “evaluative”) rather than “ontological” .

But while a subject naturalist treatment does not require construing a kind of discourse or practice as non-truth apt, subject naturalist approaches can open up, in away that I will illustrate, the possibility that the simplest and most straightforward REPRESENTATIONALIST construals forms of discourse and other structures that are truth apt (or at least convey information about how matters stand in the world) may not the best way to understand these. So a subject naturalist understanding of some practice may help to undermine REPRESENTATIONALIST understandings of the practice, although it may or may 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 objective correlates of those components are[[3]](#footnote-3).

 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 naturalists 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 wrongness and the like. Conversely, if we were told by some oracle that, say, objective wrongness 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 claims and so on. Indeed, if they are honest, it seems to me that most of those interested in the ontology of morals should agree that they are not interested in such questions, which they regard as mainly the concern of other disciplines.

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 usually for the object naturalist there is just one). What is the “nature” of causation or the “grounds” or “truth makers” for causal claims or the correct ontology 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 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, this by itself would provide little 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 questions about causation and it is not at all obvious that they are wrong to do so.

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. It is the sort of approach that fits most naturally with and best supports both the descriptive/ interpretative and the evaluative concerns of philosophers of science and also the natural expression of a broadly pragmatic approach to science

 **5. 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 metaphysics. A common device among these pragmatists for motivating this view is to invoke the idea that it is appropriate to think of all elements of science as animated by a single over riding 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 fruitful to do so, so also we posit the existence of sets and natural numbers.

 (Notice that this idea that there is one overriding goal of fruitfulness is distinct from the characterization of the pragmatic approach that I adopted in Section 3 above. 3 says that science is concerned with such useful goals as prediction etc. and that various aspects of scientific practice are to be understood and evaluated in terms of their conduciveness to this goal. Nothing is said about there being a single undifferentiated goal of usefulness. )

In contrast to this, 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. (Recall the remarks about means/ends justification gestured at earlier.) Prediction, for example, 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”, 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 of course understood as a matter of making *true 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 regard claims as true or to “posit “ them as true just because doing so would be useful to us in some general way—that is an endorsement of wishful thinking and antithetical to science[[4]](#footnote-4).

 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 one thing, 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 etc. It is also true of course that these goals will be inter-related in the sense that information relevant to satisfying one of them may be relevant to satisfying others. 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 (and numerous 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. Pragmatist 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.

 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—whether they want to predict or explain, what scale of behavior they want to 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. 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 Sandy Mitchells’ integrative pluralism.

 To anticipate a bit, 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 specific certain conditions, we can describe and evaluate specific procedures for achieving these goals and in this way provide useful guidelines for the conduct 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” this is unlikely to generate helpful guidelines[[5]](#footnote-5) .

 Having said this, I should acknowledge that I have provided no principled basis for what should be included on my list of goals for science. To some considerable extent it is an empirical matter which goals are most widely accepted in different areas of science. I also assume that it is somehow possible to distinguish the goals that are of interest for philosophers and methodologists of science from goals that scientists may have that are more personal or adventitious such as career advancement,

 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 most 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 big R REPRESENTATIONALIST idea that 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 REPRESENTATIONALIST goal 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 Representationalist 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.

 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. A simple illustration of this idea 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—unbiasedness and so on—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.

In this case, 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 will 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 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 don’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, under the right conditions, can 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 finds 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. 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.

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. For example, 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🡪0* 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 mechanisms, each of which, taken individually involves relationships that are more invariant that the overall *I🡪O* relationship. Notice the form that the argument 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. 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 mechanisms may not be such a good strategy—it may not serve goals of the sort just described very well. (See Dupre, Woodward.)

Yet another illustration of the same basic framework is provided by recent work by Patricia Cheng and collaborators. 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 is a normative claim, again in the means/end hypothetical imperative sense describe 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.

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 and allegedly simple (e.g. linear) relations are more natural. A striking feature of most of the philosophical 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[[6]](#footnote-6). Consider instead a much more realistic example in which one’s goal is to formulate a distinction or classificatory variable that enables prediction 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.

 As I have suggested, a pragmatist philosopher of science will 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, is justifiable 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 Chris Hitchcock’s paper, “Events and Their Times: A Case Study in Means/Ends Metaphysics”. Chris 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? 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 leads to a misrepresentation of causal structure assuming that one takes causal claims to have to do with the outcomes of interventions —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. The second choice has none of these defects.

 In a recent paper (“The problem of variable choice”) , 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 stable, specific and that 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.

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 “pragmatic” 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.

A paradigm of this sort of approach is the Akaike framework 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 in a way that reduces predictive accuracy on new 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.

There are of course other ways of thinking about simplicity and its role in science. 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, at least in any obvious way, warrant, for example, belief in the representation being predictively reliable on new data, in the way that the Akiake framework sometimes can or belief that the hypothesis has various other desirable features in addition to ease of use . On this 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.

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, axiomizations 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 content, but only ease of use, in the way that representations of the same curve in terms of Cartesian and Spherical co-ordinates do. Thus on this analysis, different claims about what the laws are will apparently 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 is justifiable to appeal to it for the purpose of identifying laws of nature.

As a final illustration, I mention Mark Wilson’s notion of “physics avoidance”. 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[[7]](#footnote-7).

**6. 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. Sometimes this skepticism is global, as with Huw’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 or on the use of causal and modal concepts in science, some form of anti-representationism (where again what is rejected is invocations of notions like mirroring, correspondence, and isomorphism) has much to recommend it.

To motivate this line of thought, let me begin with some examples. The basic conclusion that I want to illustrate it that a model or theory or structure can convey or capture information about how matters are in the world, but it can do so in an indirect way, so that what is captured is not what one would think of if one construes these as mirrors or isomorphs to what is in the world.

**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 Albert, Ney and others. This is construing the theory as a REPRESENTATION with a capital R—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 REPRESENTATIONALIST 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 have a direct counterpart in nature or stand in some relation of isomorphism to the world’s constituents. It isn’t even required that there be an isomorphism with some subset of the world’s constituents.

**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 REPRESENTATIONALIST 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 atoms. We might say that the continuum models are literally false 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, 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 REPRESENTATIONALIST construal. Thus 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.

**Third Example: Minimal Models (To be added.) These can provide predictions and explanations but (arguably) not in virtue of REPRESENTING**

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 of notions like REPRESENTATION and isomorphism—or at least 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.

(To make a connection with earlier portions of this paper, one of the things that the focus on REPRESENTATION 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 REPRESENTATIONALIST expectations. Pragmatic philosophers of science should accordingly focus on how such models are used and resist the temptation to automatically construe them in representationalist 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.

Ideas of the sort just described are frequently denigrated as “instrumentalist” and it is true that the sort of pragmatism I have described stresses the idea that theories and models are instruments or tools that can be used for various purposes. On the other hand, as I see it, this does not amount to traditional instrumentalism as that doctrine is understood by philosophers of science. Instrumentalism as traditionally conceived involves commitments like the following: 6.1) 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 6.2) 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. The anti-representationalist 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.)

Thinking of matters this way, we should be able to see that the traditional dichotomy between scientific realism (which often does seem to involve a commitment to something like a picturing or isomorphism conception of theories or models) and instrumentalism, when construed as committed to 1) and 2) is far from exhaustive—there are other possibilities, including the one I have tried to describe. Much of the appeal of strong forms of realism understood in terms of correspondence derives I believe from the thought that the only possible alternative to it is traditional instrumentalism, but 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 1 and 2. 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.

 **Modality Again**. One of themes illustrated by the examples above is that we should not identify the issue of whether a claim or model embodies 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 of course 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 (this is more like what Richard Feynman once called an idea for an idea, rather than anything I have worked out) that a similar point might hold for the use of modal concepts in science. In thinking about these, 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 and when true incorporate information about it. 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 physical modality were 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) of one kind or another have always seemed to me unilluminating—it does not add anything to the observations I have just made. Moreover, at least in many cases, this postulation leads to all sorts of puzzles of a placement sort. By “non-Humean stuff” I mean modal objects or properties of various sorts, stories about laws and causation that appeal to dispositions or powers in some metaphysically weighty sense, relations of necessitation among universals and so on.

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 above. There is nothing wrong with the idea that modal and causal claims are truth apt and when true informative about the world. It is also true that reductivist programs seem to systematically fail. Where we go wrong is when we try to understand the truth-aptness or objectivity of modal claims in a representationalist fashion and in particular 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 (a) 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 or in terms of some property possessed by me or my counterpart or in terms of the existence of possibilia or possible worlds or the like. This misunderstands the way that (a) 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, (b) “aspirin causes headache relief” is unproblematically true, but I think one can sensibly 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 (b) is better viewed as, for example, a claim about what would happen if one were to ingest aspirin in certain circumstances ( for example in cases in which other causes of headache relief are not present etc.) or something similar that captures aspects of what would happen under appropriate episodes of aspirin relief. The conditions in the world that support such causal claims when they are true are often complex and distributed in ways that we may not expect. Moreover these conditions are often 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 of, say, electrons or DNA. (“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.) Similarly, physical theories are often presented in a state space format which, if the theory is correct, provides information about what the physical possibilities are—a claim that might be checked by some combination of observation, experiment, and interpolation. (Observing particles moving at 1 m/sec and 2 m/sec, we conclude, correctly, that intermediate velocities are also possible.) However, this does not mean that we should interpret the theory as “ontologically committed” to possibilia or possible worlds corresponding to this state space.

1. I have no objection, though, if someone wants to describe the sort of project I favor as pragmatic metaphysics or means/ends metaphysics . [↑](#footnote-ref-1)
2. 2 in fact, these are dubious candidates for *naturalistically* acceptable properties but let’s put this consideration aside. [↑](#footnote-ref-2)
3. As Huw remarks, there is a tendency among to suppose that (assuming one wants to be some kind of naturalist) object naturalist concerns are prior to and more fundamental than subject naturalist concerns. I agree with Huw, however, that matters are 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-3)
4. In general, the version of pragmatism I favor presupposes the availability of some notion of truth, rather than attempting to analyze truth in terms of usefulness or anything similar: our goals include the making of true predictions, the tracking of dependency relations that are true of correct in causal analysis and explanation and so on. [↑](#footnote-ref-4)
5. 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 the satisfaction of whatever other goals the inquirer has. 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-5)
6. It might be thought that this question has an obvious answer—the observers are just trying to “describe” the situation they see. But I would deny that this notion of pure description, independently of goals and purposes, has any application to science. [↑](#footnote-ref-6)
7. Many other examples of means/ends reasoning can be found in statistics, machine learning, and formal learning theory. Additional philosophical examples include Sober on black box inference, Glymour on inference from lesion data in cognitive neuroscience. [↑](#footnote-ref-7)