

Understanding Perspectivism

Scientific Challenges and
Methodological Prospects

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7 Safe-and-Substantive Perspectivism

David Danks

1 Bases for Perspectival Models

Recent years have witnessed a renaissance in “other-than-realist” approaches in the philosophy of science, whether pragmatist, instrumentalist, or one of many other -ists. Many of these approaches can be broadly understood as advocating some kind of perspectivism: roughly, the idea that scientific theories, models, knowledge, and claims are from a perspective, rather than necessarily expressing objective, universal truths. The idea that scientific claims are perspectival does not automatically imply any kind of strong relativism about science, but it does imply that there are problems with naïve realism. Of course, this high-level characterization subsumes an enormous diversity of accounts, depending on the nature of a “perspective,” the reasons why science employs perspectives, the implications for scientific practice, and more. For example, a pragmatist philosophy of science might emphasize the necessary role of practical goals in our scientific theories, while an instrumentalist philosophy of science might focus on the role of measurement methods and instruments. Nonetheless, both agree that science necessarily instantiates various perspectives, rather than consisting of universal truths. Overall, much of this other-than-realist philosophy of science has emphasized either the nature of these perspectives in science or the methodological implications of such perspectives (though not always using the language of “perspectives”).

In this chapter, I aim to provide a detailed account of two reasons for perspectives in science. In general, perspectivist approaches in the philosophy of science face a significant challenge, as these scientific perspectives must arguably be grounded in sources that are neither specific to the individual nor high-level banalities. If the relevant perspectives are individual specific—that is, the relevant aspects of some scientific perspective are based on particular features of particular scientists or particular research groups—then we have an “unsafe” perspectivism. Such a view implies that science itself is dependent on local, contingent properties of specific people, and so we have as many sciences as we have scientists. Hyperlocal perspectivism means that science does not provide us with a shared view of the world but rather personalized, individualized accounts. One

of the hallmarks of science, however, is supposed to be exactly its ability to produce objective accounts of the world (or, at least, more objective than ordinary cognition). At the same time, if we instead ground the perspectival nature of science in high-level claims about humans and our practices, then we risk having an “insubstantial” perspectivism. While it is true that science is done by bounded humans, this observation provides no insight or guidance into the nature of scientific perspectives. If we want our philosophical frameworks to be helpful in some way, then we should insist that any such account, including perspectivist ones, should consist of more than obvious truisms.

In contrast with both of these extremes, I argue here that the influences of scientific concepts and scientific goals imply that science is necessarily, but also unproblematically, perspectival. My arguments will focus on the causes of perspectivism, and thus will not provide a definition of exactly what constitutes a perspective. Instead, this type of analysis aims to reveal some (though obviously not all) key perspectival features of science, which jointly imply that standard realist views cannot be correct. The resulting perspectivism provides specificity about the nature and impact of these factors and thereby provides substantive constraints and methodological implications for scientific practice. At the same time, this approach blurs the lines between scientific and everyday perspectives, thereby implying that perspectivism in the philosophy of science is no more problematic than perspectivism about everyday perception. More precisely, these sources of perspectivism are not unique to scientific theories, knowledge, and beliefs but rather apply to their everyday counterparts. That is, there is nothing special (with respect to these arguments) about science, and so the resulting perspectivism about science does not threaten a collapse into complete relativism (or at least, poses no more threat than we face about *all* of our beliefs and knowledge).

I begin by examining these two sources of (scientific) perspectives in more detail: concepts in section 2 and goals in section 3. For both types of influences, I focus on the ways in which particular scientists’ concepts and goals impact their scientific theories, models, and knowledge. That is, my approach here employs (mostly) methodological individualism, as I largely focus on the influences of concepts and goals of particular scientists rather than the concepts and goals of scientific communities. In particular, notions such as paradigms, research programs, or similar group-level frameworks enter into this analysis through the cognition and activities of particular scientists rather than through some independent social existence.¹ Section 4 then takes up a more general discussion of the resulting scientific perspectivism, both characterizing it and showing that it blurs smoothly into more everyday, prosaic perspectivism. Thus, there is nothing to fear from (this kind of) scientific perspectivism: science does not provide an objective, universal mirror of the world, but its distortions are no more problematic than those of our ordinary, everyday perception

and cognition about the world. Yes, science is perspectival, but in a safe-and-substantive way.

2 Sources of Perspectives: Concepts

Our scientific and everyday cognition is thoroughly conceptualized: our understanding of the world is almost entirely in terms of concepts² rather than some kind of unconceptualized, direct access to the world. There are, of course, numerous debates about whether *some* aspect of (early) perception is perhaps nonconceptual (e.g., Dretske 1981; Evans 1982; Crane 1992; Peacocke 1992, and many other papers in subsequent years), but there is no debate about whether our thinking inevitably involves concepts and conceptualized content at some point. We acquire concepts from a very young age, and those concepts and conceptual frameworks are essentially ubiquitous in our cognition. These observations are no less true for scientific cognition, though scientific concepts are often more clearly articulated and more widely shared (in some sense) within the scientific community.

A common belief about concepts, at least those of the everyday sort, is that they simply provide a compact encoding of information about the world. On this view, concepts enable us to efficiently and quickly encode relevant information about the state and structure of our environment. For example, much of the psychological literature on concept acquisition emphasizes the tight connection between environmental statistics and learned concepts for those environments. This line of research emphasizes the ways in which concepts encode environmental regularities and thereby help to identify what is relevant, anomalous, and so forth. The underlying intuition is that one major function of concepts is to convert a messy, complex external world into cleaner, relatively more tractable cognitive representations.

This way of thinking about concepts suggests that they largely play a filtering role. In general, more compact representations will almost always involve a loss of information relative to the original, but the talk of statistical encoding (among other features) suggests that the loss might involve only irrelevant information. If that were correct, then concepts could be understood as a non-distorting information filter that provide a “mirror” of the world (at least, for all of the information that made it through the encoding process). Unfortunately, this line of thinking is mistaken: concepts do not simply filter information about the world but rather actively influence and transform that information. That is, concepts distort the world (when compared to a mirror) and so constitute a substantive element of a perspective. Although there are many phenomena to which one could point, I focus here on only three.

First, consider the phenomenon of categorical perception (Harnad 1987; Goldstone and Hendrickson 2010): at a high level, instances that

are close to a category boundary are perceived (in conscious cognition) as further from that boundary than they actually are. More precisely, when some instance *X* is understood as falling under the concept *C*, then the perception of *X* is shifted toward the centroid (in the relevant feature space) of *C*. Perhaps the best-known instance of categorical perception arises in phonemic discrimination. Many pairs of phonemes in a language will differ on only one acoustic dimension; for example, the phonemes /*r*/ and /*l*/ form such a pair. Individuals who learn at a sufficiently young age (Eimas, Siqueland, Jusczyk, and Vigorito 1971) a language in which these are distinct phonemes lose the ability to “hear” sounds that are intermediate between these phonemes. Instead, they hear intermediate sounds as something much closer to the central phoneme sound. Moreover, these unconscious discriminations are resistant (though not immune) to alteration through training (Strange and Dittmann 1984). More importantly for our present purposes, “hearing” a sound as a particular phoneme involves a distortion of the world: the experienced sound is simply not a mirror of the acoustic properties of (that part of) the world. Rather, categorical perception involves changes to the closeness relations of various stimuli and, more generally, a shift in the perceived “location” in perceptual space (Goldstone 1994; Livingston, Andrews, and Harnad 1998). This type of categorical perception is not limited to phonemic discrimination but rather arises for a very wide range of concepts, arguably every concept that has a perceptual component. Our concepts and categories have been shown to (directly) influence our experience of the world in perceptual modalities such as visual perception (Livingston et al. 1998), as well as more complex, not purely perceptual concepts (Etcoff and Magee 1992).

This focus on “distortions” due to primarily perceptual concepts might seem irrelevant to most of our scientific cognition. As the second example shows, however, expertise can play a significant role in the concepts we form and therefore the ways that the world appears to us. As just one example, Medin, Lynch, Coley, and Atran (1997) showed that the plant-related concepts of park maintenance workers are quite different from the plant-related concepts of taxonomists. That is, people whose job required a focus on the ecological niche of park trees had significantly different concepts than people whose job required a focus on genetic or biological relationships. Moreover, those different concepts made a behavioral difference in reasoning, inference, and descriptions, and are not intertranslatable in any straightforward way; they carve up the world in different terms. Of course, while concepts do more than just represent summary statistics, they also do have that representational function. Thus, as someone gains more experience and expertise in a domain, her concepts can significantly shift as she learns more about the relevant summary statistics. This conceptual change would not necessarily be an issue, except that those same concepts influence both basic perceptions (see the

previous paragraph) and more complex, conscious cognitions (Cohen, Dennett, and Kanwisher 2016). Thus, conceptual change can have quite wide-ranging and hard-to-predict impacts on other concepts. Of course, all of these observations do not mean that concepts are somehow non-veridical (Cohen 2015), but rather that the content of the concepts—both perceptual and cognitive content—is not what we might expect. In particular, that content does not provide a simple mirror of the world in the way assumed by simple realist models, whether of perception, cognition, or even science.

As a third demonstration of the perspectival nature of concepts, consider our episodic memories of particular experiences, such as my memory of eating breakfast this morning. A common view of episodic memories (at least, within people who do not study memory) is that they involve relatively direct recall of the earlier events. Of course, that recall is subject to many types of noise and error, and so our memories need not be particularly accurate. However, this noise is (on the common view) largely independent of the content of the memories, though it can depend on the circumstances in which the memory is initially encoded (e.g., emotionally laden events are often thought to be more likely to be correctly encoded and recalled). However, there is now substantial research demonstrating that episodic memories involve a process of reconstruction, not simply one of recall. For example, our memory of an event at some past time is “recalled” using the concepts that we have now (Schacter, Norman, and Koutstaal 1998; Conway and Pleydell-Pearce 2000). Hence, if our concepts change between the time of the events and the recall time, then the memory will shift along with the conceptual change. Alternately, if we are asked to recall whether we have previously seen particular images, then we will make more errors on previously unseen images that are close to actual prior images when both are not too far from the concept centroid (Koutstaal and Schacter 1997). The distortions in our episodic memories are not ubiquitous or uniform but rather depend on relatively fine-grained details of our concepts at that later moment in time.

One might object that these roles of concepts fall under ordinary, garden-variety theory-ladenness of observation. Philosophers of science long ago became used to the idea that our theories, including our concepts, influence our observations (Hanson 1958; Kuhn 1962). For example, we look at a needle deflection and instead “see” an atom undergoing radioactive decay, or we look through a microscope at some squiggles and “see” a cancer cell. The observations that we qua scientists record and use in our scientific practices are themselves conceptualized by the scientific concepts in our theories. Hence, this section might appear to be much ado about nothing. In response, we should first note that at least one conception of theory-ladenness of observation does not fit with the phenomena described in this section: namely, scientific concepts (and the theories from which they are built) cannot simply act as a “filter” that identifies certain features or

properties as bundled together in a concept and thereby ignores the others. In the examples above, the act of “seeing” a cancer cell is not simply a categorization judgment; rather, as with other kinds of concepts, we should expect that this act will also distort (relative to more objective measures) the perceptions of the squiggles in a top-down way.

Observations are not merely theory-laden but rather are theory-*shaped* or theory-*distorted*. Our understanding of human concepts implies that our scientific observations should be pulled toward the centroid of the relevant concepts; shaped by the functions for which we use those concepts; and potentially, even unknowingly, revised over time as the scientific concepts shift. More generally, the role of concepts that I have outlined in this section is significantly more active than one often finds in discussions of the theory-ladenness of observations. At the same time, I grant that everything I have written to this point is consistent with a philosophical account of theory-ladenness that is based on the fact that we humans perceive the world in ways that are distorted (depending on our concepts), and so scientific perception is distorted. However, such philosophical accounts are often used to argue for a broader type of relativism or incommensurability (Kuhn 1962; Feyerabend 1975; Longino 1990) and so contrast with the larger, non-relativist view that I develop here (see section 4).

3 Sources of Perspectives: Goals

A second set of causes of the perspectival nature of science—again not constitutive of those perspectives—is the goals and intended functions or tasks of scientists. That is, I contend in this section that our cognition about the world is deeply shaped, and arguably distorted in key ways, by the goals that we have or the tasks that we believe we will need to perform in the future. One might immediately object that this proposal cannot be right, as goals should only enter into our cognition (whether scientific or not) when we are engaged in reasoning and decision-making. This “standard view” holds that our learning and conceptualization of the world aim solely to reflect the structure, both causal and statistical, of the learning environments. Of course, as we saw in the previous section, concept learning can lead to perceptions that are distorted in various ways, but the standard view holds that those distortions are not driven by goals. That is, the core content of our concepts should, on this view, be goal-free. Many standard cognitive models of learning embody this standard view: Bayesian learning algorithms, neural networks, and most computational models of learning all mirror environmental statistics without regard to goals.³ On this view, goals enter into cognition only after we have learned concepts that roughly mirror the world.

While this standard view is appealing in many ways, it is arguably not normatively justified. If a cognitive system, whether human or other, has

to interact with its world, then the ultimate measure of its learning will be whether the learned content enables the system to succeed. For example, if the system should select option *A* anytime the perceived object is between 0 cm and 2 cm long, then there is no extra value to encoding the precise length, rather than only the fact that the object falls into the relevant interval (Danks 2014; Wellen and Danks 2016). Moreover, if the system exhibits any noise in its decision-making processes, then there can actually be an incentive to “misperceive” the object as being further from the decision boundary than it actually occurs, as that misperception will increase the likelihood of the system answering correctly (Hoffman, Singh, and Prakash 2015; O’Connor 2014). For example, an object that is 1.9 cm ought to be perceived as closer to 1.5 cm if the decision boundary is set at 2 cm, though that same shift ought not occur (to a noticeable degree) if the decision boundary is set at 10 cm. More generally, there is a normative argument that cognitive systems ought to sometimes be indifferent to believing falsehoods and sometimes ought positively to believe falsehoods. For example, if some false belief fits more cleanly with our other knowledge (perhaps because of a shared structure or analogy) and that falsehood does not impair our ability to succeed at various goals, then we ought to go ahead and believe the falsehood. Of course, falsehoods or inaccuracies that impair our ability to achieve our goals (whatever those might be) ought to be rejected during learning. Nonetheless, the door is open for goals possibly having a significant impact on our *learning*, not solely our reasoning and decision-making.

In fact, the descriptive data reveal that people often do have these kinds of inaccurate or false beliefs, exactly when they do not impact our ability to achieve our goals. For example, if people are shown multiple sequences of numbers and asked to estimate which sequence has the largest (or, alternately, smallest) average value, then they learn relatively little about the sequences that are clearly goal-irrelevant (e.g., low-magnitude sequences when the goal is to learn which has the largest mean), to the point of failing to distinguish between sequences that are easily distinguishable when they are goal-relevant (Wellen and Danks 2014; Wellen 2015). In these studies the only variation between people is what goal they were provided in the experimental cover story, and so that is the only available explanatory factor for the significant differences in *learning*, not simply reasoning. Alternately, if people have the goal of “learn to control a dynamical system,” then they learn relatively little about the underlying causal structure governing the system, even though they have no trouble with that given the goal of “learn the causal structure” (Hagmayer, Meder, Osman, Mangold, and Lagnado 2010). Many more examples of this type can be found in the empirical literature (Ross 1997, 1999, 2000; Markman and Ross 2003).

Moreover, there are also cases of goal-determined learning of falsehoods, not just failures to learn. For example, Feltovich, Spiro, and Coulson

(1989) showed that many medical doctors (at that time) had incorrect beliefs about the causal direction between *heart size* and *heart strength* in congestive heart failure: the doctors believed that the causal connection was $size \rightarrow strength$, but the actual physiology is $size \leftarrow strength$. Moreover, the true causal direction was known at the time of Feltovich, Spiro, and Coulson's study; the relevant information was readily available to the medical doctors. However, the false belief had no practical impact given the medical technologies and interventions available to doctors at the time. And there were positive reasons for doctors to believe the falsehood, as it fit cleanly with their knowledge about other muscles in the human body. Hence, if doctors have the goals of diagnosis and treatment while minimizing or reducing cognitive effort (given the complexity of the domain), then they ought and do learn a falsehood. Alternately, if people are charged with manipulating the world to bring about an outcome, then they will often systematically mislearn the causal structure of the world, though in exactly the right way to minimize the probability of incorrect action (Nichols and Danks 2007). Again, we have a case in which the goals influence the learning in deep ways.

If all of this is correct, then we should expect our *scientific* goals to impact our *scientific* learning, whether to yield various inaccuracies (which are goal-irrelevant) or perhaps even justifiable falsehoods. One response would be to argue that scientists share a single goal—namely, to discover the truth—and so these observations about everyday learning are unproblematic: there will be no variation in what is learned (since we all have the same goal), and we ought not learn falsehoods (since that would fail to satisfy the goal). However, this single goal cannot actually be the guide to scientific learning, as we have no way of directly assessing whether we are moving closer or further from it; we have no Archimedean point from which to assess the truth or truth-aptness (or whatever concept one prefers) of our scientific theories (Kitcher 1993; Danks 2015). Of course, the scientific community could perhaps have a single goal guiding all of their inquiries, though that goal cannot be “discover the truth.” Once we rule out this overarching truth-centric goal, though, then it is hard to imagine what that single goal might be.

Science instead arguably proceeds through convergence, as we employ multiple methodologies in the hope that they will imply the same theory, the same concepts, or the same representations of the world. When our multiple methods seemingly lead to the same answer, then we conclude that we must be tracking *something* truthful about the structure of the world. Hence, our ability to “objectify” our measurements and conclusions might be taken as evidence that goals are not actually playing a significant role in our scientific learning. As noted earlier, of course, the empirical phenomena discussed in this section do not imply that we should *always* be learning falsehoods; sometimes, the best thing to learn might be the truth (at least, in experimental settings where we can talk sensibly about knowing the truth). The challenge is that we do not know a priori

whether we are in such a circumstance. Perhaps our goals either should or do instead lead us toward biased or distorted learning. Our mixture of scientific goals—prediction, explanation, discovery of unobservables, and so on—might be best satisfied by learning the truth (whatever that exactly means for the world), but we have no particular reason to expect that at the outset, nor do we have any way to test it. Moreover, the existence of a single best (scientific) theory is not informative in this regard: for any given goal or mixture of goals, there will typically be a unique theory that optimizes performance relative to that goal or goals (Danks 2015). We know in advance that there will be a best theory relative to our scientific goal(s); we just do not know whether it will be the correct (or true) one.

These considerations seem to point, though, toward a *reductio* against my conclusion: (1) scientists clearly exhibit a diversity of goals in terms of what they are trying to explain or predict, even within a scientific domain; thus, if (2) different goals imply different concepts and theories, then we should expect diversity of scientific concepts; but (3) we are able to communicate and debate with one another in scientific contexts, and so we must not have this kind of conceptual diversity (and hence, proposition (2) must be incorrect). However, when we look at scientific practice, we do sometimes see exactly the kinds of diversity that proposition (3) denies. For example, consider the goal of explaining how people perform certain kinds of key cognitive operations, whether concept learning, decision-making, various predictions, or other cognition. This goal is actually ambiguous between explanations that are grounded in rational justifications about the limited nature of human cognition—so-called rational process theories (Denison, Bonawitz, Gopnik, and Griffiths 2013; Vul, Goodman, Griffiths, and Tenenbaum 2014)—and those based on descriptive, empirical observations and constraints—the process models traditionally developed by psychologists to model the actual mechanisms of the mind. Crucially, scientists pursuing these two different subgoals have demonstrated exactly the predicted difficulties in communication, such as debates that seem to involve all parties talking past one another. Moreover, the core problem in the discussions between researchers with these two different goals is precisely that they do not agree about the standards for evaluating the proposals. Both sides are trying to answer questions about “how the mind actually does what it does,” but one side (rational process theorists) requires normative justification for the theory and the other (traditional process or mechanism modelers) requires precise empirical validation of the model. The different goals translate directly into different learnings and therefore into different understandings of the human mind.

4 Everyday Perspectivism

Given these observations, I propose that a perspective should include (though not necessarily be constituted by) the particular concepts, goals, and thus accompanying distortions. Importantly, this characterization

implies that every individual has a perspective, but perspectives are not relative to specific individuals. Two different people could have the same perspective, as long as they have the same (up to relevant noise, error, or change) concepts and goals. For example, we might plausibly think that members of a research group would likely share concepts and goals, as they work closely and presumably discuss what is meant by their terminology, and what standards or goals are relevant for their research. Since perspectives are individual-independent objects, they can be shared across many people; in fact, some measure of shared perspective is almost certainly required for certain types of debates. Moreover, perspectives can be judged against various standards, whether the goals that they contain or some other goal. If one thinks, for instance, that empirical prediction is a goal that should be part of every legitimate scientific perspective, then we can assess various putative perspectives according to that standard, even if the perspective is developed with emphases on other goals (e.g., explanatory power). In addition, this conception of a perspective implies that an individual's history, relevant sociocultural factors, measurement methods, and so forth should all be rendered irrelevant once we know their concepts and goals.⁴ Of course, an individual's history matters, but on this account only inasmuch as that history leads to the individual having a particular set of concepts or because of the goals that the individual had at some earlier point in time. In particular, multiple individuals might share relevant aspects of their histories and so share some concepts and goals.

The concept- and goal-based perspectivism that I have outlined here is thus "safe" in the sense that it does not automatically lead to a descent into hopeless relativism. For most interesting scientific domains and research challenges, the practices of scientific training (which arguably homogenize the community along the lines of concepts and goals) and also people's shared cognitive architectures (by virtue of being human beings) should lead to most scientists having, in practice, relatively similar perspectives. There is little reason to think that multiple scientists' concepts or goals are so different as to imply that there are substantively distinct perspectives. Moreover, the world "gets a say" in the perspectives, as there will typically be a normatively unique (or close to unique) set of concepts and theories for a set of goals in a scientific domain, though we might not, in practice, be able to determine that set. Relatedly, our concepts are not arbitrary or ungrounded in experience but rather are learned from experience. We cannot simply invent and use whatever perspective we might want. Rather, we are significantly constrained by the world in terms of the acceptable perspectives, at least once we have specified the relevant goals (and sometimes some auxiliary concepts).

At the same time, this type of perspectivism is substantive, as it is not simply the banality that "humans do science" (and so science is done from the "human" perspective). Rather, this perspectivism is grounded in

features of human cognitive processing and representations: the details of our shared cognitive architecture matter and can ground predictions about the types of scientific perspectives that we ought to have given our scientific goals and experiences. Moreover, as noted above, different scientific goals can lead to substantively different (normative) perspectives, along with the very real possibility of non-unifiability of the corresponding scientific theories. That is, this perspectivism can potentially lead to pluralism, though the details matter in terms of predicting whether and when pluralism might arise.⁵ More generally, this perspectivism implies that “mirror realism” should not necessarily be correct in many cases, but rather we should expect—particularly for sciences that are more focused on measuring and controlling rather than explaining—to find theories that turn out to have various (defensible) misconceptions or falsehoods. There are thus multiple ways in which this type of perspectivism makes substantive claims (that could potentially have turned out to be wrong).

The careful reader will have noticed that nothing I said in the preceding few paragraphs was actually specific to scientific learning and theorizing. Exactly the same points could be made about everyday learning and theorizing. The perspectivism that I defend here results naturally for almost any cognitive agent that must learn about its world and then reason to try to achieve particular goals. For example, our “theories” about the spatial environments in which we move ought, on this account, to be expected to be perspectival in various ways in light of the goals we typically have when navigating those environments (Maguire et al. 2000; Maguire, Woollett, and Spiers 2006). More generally, I contend that we should embrace the type of perspectivism that I defend here, partly because we are all already (or should be) perspectivists about our engagement with the everyday world. Our perspectivism about everyday experiences is (or should be) similarly safe-and-substantive: we are not forced into strong relativism or skepticism about the world, since the world “gets a say” in our perceptions; but we are also not left with vacuous claims about our “contributions” to our understanding of the world.

In this regard, this perspectivism fits closely with the type of view advanced by Chirimuuta (2016). She argues that advocates of scientific perspectivism should base their metaphors and analogies on haptic perception, or perception by touch, rather than visual perception. Haptic perception is clearly mediated by the particular sense organs, rather than purporting to give a “mirror” (perhaps with a subset filter) of the world. We are not under any illusions that our touch-based understanding of the world provides some kind of direct access. Moreover, haptic perception is clearly action-driven: our touch perception is intimately connected with our abilities to influence, move, and manipulate objects in our environment. That is, Chirimuuta’s (2016) argument depends on perspectives having exactly the same components that I have discussed here—concepts (so no mirroring) and goals (so actions). More generally, our arguments share

the high-level idea that scientific perspectivism is a special case of the perspectivism that arises in our everyday lives. And just like our everyday perspectivism, our scientific perspectivism is as safe-and-substantive as our views about people, penguins, and puppies.

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Notes

1. Where appropriate, I will note places that this methodological individualism is potentially limiting or distorting.
2. For the purposes of this chapter, I will not worry about the distinctions and relationships between concepts and categories.
3. Importantly, this generalization only holds for models that do not incorporate a decision-theoretic action component into the learning system.
4. Note that my assumption of methodological individualism is doing substantive work here. To the extent that we want to talk about the perspective of a community, then we plausibly have to include external factors of the sort that are often lumped together under terms like “paradigm” or “research program.”
5. It is also unclear whether this “non-unifiability” is problematic, at least if we adopt a thoroughgoing goal-based perspectivism about scientific theories. I have elsewhere (Danks forthcoming) argued that the pragmatic perspectivist will almost always have exactly as much unifiability as she wants or needs, even if that falls short of the realist’s demands.

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