

from **The Pragmatist Challenge: Pragmatist Metaphysics for Philosophy of Science**

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Oxford University Press

[2023]

3: Trueing

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Abstract: Even in areas of philosophy of science that don't involve formal treatments of truth, one's background view of truth still centrally shapes views on other issues. I offer an informal way to think about truth as trueing, like trueing a bicycle wheel. This holist approach to truth provides a way to discuss knowledge products like models in terms of how well-trueed they are to their target. Trueing emphasizes: the process by which models are brought into true; how the idealizations in models are not false but rather like spokes in appropriate tension to achieve a better-trueed fit to target; and that this process is not accomplished once and done forever, but instead requires upkeep and ongoing fine-tuning. I conclude by emphasizing the social importance of being a pragmatist about truth in order to accurately answer questions about science such as, "but do we really *know* that..."

Keywords: truth, idealization, explanation, models, modeling

1. Introduction

What is it, exactly, that makes it worth declaring oneself a pragmatist, as a practicing philosopher of science? What does pragmatism offer that could be worthwhile if one is (like many philosophers of science, and some pragmatists as well) averse to declaring oneself using -isms? The goal of this chapter is to clarify one way of being a pragmatist about truth even if one does not work on anything particularly related to truth. Our background views on what truth is like still shape our other views about, for instance, idealizations, explanation, and understanding. Pragmatism offers a way of un-entrenching some entrenched (and, thus, less fruitful and more frustrating) debates, and re-orienting them in ways that improve the possibility of making genuine progress. This means my arguments here are more synoptic and sweeping than thorough and precise, with the aim of convincing that there is something to this pragmatist approach rather than spelling out every detail.

Part of my aim is to highlight the inextricability of questions that count as part of metaphysics and questions that count as part of epistemology. While this volume is directed towards pragmatism at the intersection of metaphysics and philosophy of science, this chapter supplements this with some pragmatist epistemology as it fits into these pragmatist metaphysical views. Addressing metaphysical issues without considering the deeply connected epistemological issues will always result in a partial or distorted view. Similarly, questions about epistemological cannot be neatly hived off from metaphysical presuppositions and implications. Substantive answers to metaphysical and epistemological questions need to occur in an integrated and mutually supportive way to be illuminating of knowledge practices and products in the sciences.

Even when one is working in areas of philosophy of science that don't involve formal treatments, one's background view of what truth is, and how it behaves, still centrally shapes one's other views on issues. Everyone, not just those doing formal work, needs to have at least a moderately clear and appropriate understanding of truth in order to effectively do a wide range a work. There are pragmatist ways of thinking about truth that can be much more useful in thinking about how models and larger theoretical units of commitments, including instrumentation, fit onto the world. We need not endorse a sharp distinction between truth and justification, or an atomistic approach to assigning truth values or credences to scientific results, or a distortion of holistically connected concepts and claims, to nevertheless have our views shaped by the background implicit view we hold about the nature of truth. Epistemological norms continue to play foundational roles even in views that purport to reject truth as a central aim of scientific work. I claim that we are better off being explicit about how we think about truth, and improving that view, rather than rejecting it and reinventing the wheel from scratch with epistemic norms under different names. James and other early pragmatists rejected a correspondence theory of truth: in contemporary discussions, many views offering understanding as some replacement for truth or for explanation also reveal that in fact, what is being rejected is a *particular* view of truth, not truth *tout court*. We could instead reject that implicit but guiding view of truth, which is a correspondence theory applied atomistically, in favour of a more apt way of thinking about epistemic norms in the sciences.

Thinking of truth as trueing, like trueing¹ a bicycle wheel, facilitates thinking of models and products like diagrams or maps in terms of how well-trueed they are to their target. It captures the Duhemian holism that is also central to pragmatism. It emphasizes the *process* by which models are brought into true with their target systems, and the fact that this process is not accomplished once and then is done forever, but instead requires upkeep and ongoing fine-tuning even in the best of cases. True need not be thought of as part of a binary state ascribed to propositions corresponding to individual bits of the world, true forever or never. True should instead be thought of in terms of being *in true*. It is a directed relation between a knowledge product and some feature(s) of the world. It is not ascribed only to individual propositions, or even to well-defined collections of propositions. It admits of degrees and can be applied to a wide variety of formats, including maps, diagrams, highly idealized models, and more. There is no single Right Way to true a wheel, though there are clearly norms by which we can judge if a wheel is in true. This is not so much of a new, water-tight pragmatist account of truth. It is a Jamesian metaphor applicable in scientific contexts where formal approaches are not at the forefront, to make truth-apt norms clearer and more format-appropriate for application in different sciences.

A characteristically pragmatist emphasis is on the *connection* between belief and action: beliefs are for guiding actions, and actions are guided by beliefs (inter alia, Misak 2013). These two must fit, and knowledge processes and products (beliefs, methods of justification, factual knowledge derived through application of these processes, etc.) are normatively evaluated by this fit. Furthermore, the beliefs in question are not merely individualistic. The beliefs and actions in question are themselves social – no single individual’s beliefs are at issue, but instead the shared belief-like structures of effective working models and other knowledge products like this. Even when philosophers of science reject ‘truth’ in their accounts of various aspects of scientific work, including instrumentation or explanation, there is ongoing use of characteristically epistemological norms in a social enterprise where scientists communicate and coordinate with one another. Knowledge in the sciences is intrinsically collective: we inherit much of what we know without having contributed to it, nor having even checked the evidence; we generate and refine it using socially structured methods that thereby improve our resulting knowledge. This means also that a pragmatist approach to questions

¹ Ordinarily, this term should be spelled ‘truing’. However, I am retaining the ‘e’ to emphasize the connection with truth in this philosophical re-use of the original term.

in philosophy of science recognizes the hopelessness of a strict segregation between various kinds of values and other aspects of inquiry. Our actions, past and future, *unavoidably* guide inquiry, not just for ourselves but for communities of knowers and actors. Committing to pragmatism can then be seen as a commitment to accept this explicitly and do a better job in providing that guidance.

In the last section, I make the stakes of accepting a pragmatist way of thinking about truth clearer. Many of us are concerned with a characteristic anti-scientific attitude, one which situates the activities of non-specialists reading misleading pop articles as ‘research’, one which rejects scientific results with challenges of the form, “But do we really *know* that?”. This challenge is one we lose when we cede ground by acknowledging that we do not *know*, for instance, that human-caused climate change is driving increasing ecological disasters and climate instability. The current strategy is often to say, No, we don’t *know* that, but... and then add caveats such as: science can’t really know anything; here is our extensive justification for this claim; here is how knowledge generation in science works; etc. I think these are all wrong, from the start. We *do* know these things, full stop, on a pragmatist account of truth and knowledge. Hedging because we have concerns about the correspondence theory of truth, or about a speciously precise Bayesianism, means that we end up inadvertently misleading audiences. We don’t have to choose between our philosophical scruples and our social ones. As a pragmatist about truth as trueing, and knowledge in the sciences involving socially held commitments rather than merely individualized personal beliefs, we can simply state, of many such questions, that yes, indeed, we *do know* this. Importantly as well, it leaves open space to say of some scientific work that no, at this stage, we do not know this, and have it be a more constructive act of communication precisely because it contrasts with other claims that we do already know. Pragmatism aptly navigates the space between a knee-jerk scientism and anti-science, anti-expertise trends. This is not merely something we *can* do with a reclamation of truth; it is something we *ought* to do, for reasons that are scientifically, metaphysically, epistemically, and socially normative.

Briefly: in section 2, I describe the “no answers at the back of the book” problem for many contemporary discussions in epistemology that preclude usefulness for philosophy of science. Section 3 offers a short overview of highlights from William James on truth and clarification of the pragmatist challenge, “what difference does it make to experience?”. Section 4 describes trueing as a holist, process-oriented way of thinking about truth for knowledge products like models, etc. Section

5 shows how several recent rejections of truth as a central aim in science are actually rejections of a particular view of truth, and instead of rejecting truth tout court, we could reject that correspondence view and replace it with a pragmatist view of truth. Section 6 situates truing in terms of means-end metaphysics and epistemology, and Chang's work on the inextricability of conceptual change and instrumentation development. Section 7 makes the case that philosophers of science have an obligation to be pragmatists about truth.

2. No answers at the back of the book

This chapter is not a rejection of traditional epistemology, nor a positioning of philosophy of science as somehow superior at the task of assessing knowledge. However, there are some real reasons about why traditional epistemology has left philosophy of science cold. Some issues to which traditional epistemology has paid attention rarely apply in philosophy of science, and some of the key simplifications made in service of discussions in epistemology seem like trading in the very things philosophers of sciences are interested in. The key point I will focus on is the emphasis on variations of the justified true belief (JTB) structure. Even when questioning or denying that justified true belief is knowledge (inter alia, Gettier 1963, Zagzebski 1994; see Ichikawa and Steup 2018 for more), the focus is still on these three elements as distinguishable. What accounts involving a JTB structure have in common is a separation between the J and T such that one could have all the justification in hand for a given theoretical claim, from all different available evidential avenues, and still be able to coherently ask, "It's well-justified, but is it *true*?" That gap between truth and justification is where one imagines that some extremely well-justified theory might in fact be false. That question doesn't simply miss, but actively undermines, the processes and dissemination of scientific work.

It may seem obvious that there is a difference between truth and justification. My point is not to deny that there are many excellent examples that illustrate this difference, but instead to offer an existence claim: there also exist cases where there does *not* exist a clean distinction between truth and justification, and these are left out of or distorted by any account that insists on there always being such a distinction. In a scientific context, requiring the T and the J to be always distinguishable reveals a naive expectation I will call the "answers at the back of the book". When learning physics,

as an example, one learns from a textbook in which various problems are given at the end of a chapter, and the student works these problems out on their own using the material in the chapter. It can be quite time-intensive to solve even one or two of these problems, and more immediate feedback that one is on the right track is useful before doing a whole set of problems the wrong way. A solution to this is to have a section at the back of the textbook in which the answers to selected problems are printed. Students can simply check and see if they got the correct answer. When one finishes a particularly elaborate calculation and finds that one has reached exactly the right answer, there is a distinctive elation to that success.

Textbooks don't lay out the steps in those answers back there, though. The whole point is to figure out how to solve it yourself. Because of the role it serves, the answer at the back of the book is often just a single number. If you did every step of the solution correctly, this should be the number you got. Yet it is completely context-less: finding out that the number is different than the one you got doesn't give you much to go by, in terms of what you did wrong. Any mistake, at any stage, is enough to end up with the wrong answer. It can be laborious and frustrating to work through where one might have gone wrong, and how wrong one might have gone. But in solving the problems, there is always the faith that there is *one* right answer, and there is *some* means of solving the given problem with the existing resources, specifically the resources in that very chapter, to yield the right answer. Leaving out the steps heightens the clarity of the resulting answer: you either nailed it, or you did not. There is no approximately correct here.

The T in JTB presupposes that the world has an equivalent of the One Right Answer at the back of the book. Once we cash in all our justification tokens, there is still a further question about whether we got it right. If we could just check the answers at the back, lift the veil for a moment, there would be a determinate and unique Yes, This is True, or No, This is False. This presupposition, the expectation of there being a single right answer we either got or failed to get, is ill-fitting in the context where science is genuinely expanding the boundaries of human knowledge. In these cases, what can be called the liminal cases of research, there just is no equivalent to the answer at the back of the book. There is no chance to check our answers against the 'right' one to see we are solving problems correctly. And importantly, this is more fundamental than there simply being a lack of access to the answers, determinate though the answers are. It is *not* merely that such answers exist but we can't get to read them. They simply don't exist. There isn't an equivalent of the instructor's

manual with all the right answers in it. There will never be a point where we get to peek behind the veil of the universe and see if we got it “right”. There is no guarantee of a path to a guaranteed, unique solution via the recommended resources for the problems that face us. There is no designated set of tools to solve it that the chapter points us towards. And even as we progress in physics, these answers don't appear: in principle, they simply are not there. All the justification in the world is, bluntly, all there is: there is no further "and this is Right" that can get tacked on in these cases.

Discussions about how to analyze the J, T, and B in JTB accounts share the assumption that there is a meaningful distinction in *all* cases between truth and justification. Part of what makes the sciences distinctive from other contexts in which we say that epistemic agents know something is the liminal character of what is known. Contrast this with ordinary contexts, where we might say things like, “I know that so-and-so is at the coffeeshop.” We can question how well our evidence lends justification to the knowledge claim. We might discover that our evidence is misleading. But in these ordinary contexts, it does make sense to distinguish the truth of the matter, whether so-and-so *really* is at the coffeeshop or not, from that evidence and the justificatory support it lends the claim.

In cases to which JTB applies more straightforwardly, there are three important features. First, as noted already, there is a clean distinction between truth and justification for the proposition. Second, no other propositions, and no communities of researchers, are ineliminably involved in adjudicating the meaning of the claim involved. Third, there is determinacy to such a claim being true or false. There aren't cases where, having resolved all the possible facts and how they related to the claim at hand, we might not really be sure, after all this, if we should call it true or false. There is no community of other researchers whose consensus, or lack thereof, could impact our willingness to say it is true.

Each of these three features fails to obtain where science is adding to our collective knowledge. When researchers evaluate their current best theories, models, instrumentation, or other knowledge product, what they *do not do* is add, to all the justification and conceptual refinement, an additional claim, “and this is true.” They are neither leaving it off, nor adding it. It is superfluous. If pressed to answer questions like, “This is extremely well-justified, but is it true?”, there will often be genuine

confusion (even annoyance) at what more the questioner is looking for. Responses to these kinds of questions often involve researchers show how to use some part of a model, how the result was reached, explaining more about how the important equations behave, how they should be applied to some prototypical cases, etc. The knowledge product is shown to work well; it is put into action, and the results are taken to speak for themselves. This response cannot be separated into the justification bits and the truth bits. It illustrates knowing as a process and as action. It is not a list of propositions that get an extra column in which to put checkmarks for “is it true?” Or its close cousin, “do we really know that?” There is no certification stamp to the effect, “we looked in the back of the book and indeed this is the correct answer.”

Insofar as these liminal cases where science is developing genuinely new knowledge exist, they fall outside the purview of any account that requires a JTB structure, no matter how the J and the T are construed. There exist instances of knowledge where there is no meaningful difference between justification and truth. This is not simply a matter of pragmatic encroachment, where circumstantial factors shift the standards for what counts as sufficient justification. It is in-principle: there are cases where, no matter how one construes the threshold for evidence, there fails to be a clean distinction among the J, T, and B.

I'll briefly mention three specific examples to show how plays out in rendering contemporary epistemological discussions less helpful or worthwhile for philosophy of science: the knowledge norm of assertion; the KK Principle; and lower- versus higher-order evidence. These are not singled out because they are especially bad, nor am I claiming that there is nothing worthwhile happening in those discussions. Rather, these examples illustrate how this background assumption about JTB still infuses these discussions and makes the price of entrance too steep for philosophy of science.

According to the knowledge norm of assertion (Williamson 2000), one is only justified in asserting something one knows. In order to have genuine justification, one first has to have knowledge. Something that turns out to be untrue, then, could not have been justified at any time of assertion. Consider, though, how this would work in ascertaining whether physicists were justified in asserting the discovery of the Higgs boson. In one way of understanding justification, they were arguably more justified in this than in any other assertion in the history of science, with a fantastically high threshold of confidence of five sigmas. But one could still say, if one is a knowledge-firster, “They

were only justified in asserting the Higgs boson had been discovered if they *knew* that the Higgs boson had been discovered.” This is a way of saying that they were only justified if indeed the Higgs boson is a One Right Answer at the back of the metaphorical book of the world.

If it turns out that we replace the Standard Model with something else in a decade, then we must modify our assessment of this episode retroactively. Physicists will turn out not to have been justified at that time, because one can’t know something that wasn’t true, and thus they could not have been justified in asserting it, no matter the actual evidential situation. It will not matter how much further development was based on that discovery, or how much successful prediction or manipulation of high energy particles was involved. Yet from the perspective of the physicists making the discovery, there is no space between knowing that it had been discovered and the justification they had for asserting that it had been discovered. If that is not enough to count as knowing, then they don’t know that they have discovered it and will likely never know. It becomes unknowable. If they don’t know that they have found the Higgs boson, though, then what are they doing? Because they very much take themselves to be asserting with a very high degree of confidence (an unprecedentedly high degree of confidence) that they know it. The Standard Model is a good example because a very large number of physicists expect it to be replaced, and nevertheless need to use it in the meantime. No one *knows* any particle physics, from a Knowledge-first perspective, despite its impressive track record as one of the most confirmed theories ever, and its fruitfulness for discovery. Requiring that assertions in science are only justified if they are knowledge requires scientists to also know that what was reported is not ever going to change; this is an unattainable, and unreasonable, standard. No scientific results would ever count as genuine knowledge (or if they did, we would never know that we know those results; see below). As reassuring as it might be to a certain kind of epistemic existential anxiety, there will never be a guarantee that now we know something finally, and are done, and this result will never need to be modified, refined, reformulated, or replaced.

Similar circularities befall the KK principle. According to this principle, if one knows some proposition P, then one also knows that one knows it. Put into use in the Higgs boson example, P in this case could be, “The Higgs boson has been discovered.” But pressing physicists on this issue would reveal that they don’t meet the KK principle. Sure, they know that they found it. But do they know that they know? Physics has already given the best answer it has – what they know. Do they

know more about what they know, such as the fact that they also know it? No: the totality of what they know was already included. The KK Principle asks, “Ok, you found the boson, with five sigmas of confidence. But did you *really* find it?” And any answer to this has to be something between a shrug, or a “I just said that we found it,” or even a “I don’t know what more you are asking for here.” Posing this question to such a discovery raises the suspicions that what the inquirer is asking for is something that could not, in principle, be given with respect to any scientific result. Applying the KK Principle in science is akin to asking, Are you sure that this is it, that you will never change your mind? And a positive response to this query is not something that science can assert.

As a final example, consider recent discussions about lower order and higher order evidence (e.g. Kelly 2010, Christensen 2010). The idea is that some evidence bears directly on whether a proposition is true, and other evidence bears on *that* evidence as true or not, but does not bear on the proposition in question directly. One might think one has good evidence for a belief, but then further evidence comes along to the effect that some of the original evidence might not be reliable. The distinction involves a commitment to some evidence bearing directly on a proposition as true or false (lower-order evidence), and other evidence bearing instead on the lower-order evidence (higher-order evidence). On this distinction, the higher-order evidence somehow does not bear on the proposition in question directly, but does so only filtered through the other evidence on which it bears.

The initial response from philosophers of science to the claim that there is evidence that bears on evidence might be something like, Yes, we’ve read Duhem. That there are further statements implicated in assessing the evidential support for some given claim is familiar. Claims in the sciences cannot be completely extricated from a web of other claims required to support it, a feature of evidential connection that is highlighted when something goes wrong. There are supplementary hypotheses that are not directly related to the one being tested, but are required to set up the experiment in the first place. Finding that something did not work indicates that one of an indefinitely long list of auxiliary hypotheses may have to be revised. None of these are higher-order, though: they are just part of the regular evidence base. There is no neat or consistent way to divide up the evidence into direct versus indirect. The degree of directness will shift depending on how thoroughly one renders inferential steps explicit.

The Duhemian initial reaction combines with the point above about having no answers at the back to the book. Holding that there is a consistent distinction to be drawn between evidence for some claim and evidence that bears on that evidence is just to forget how intertwined all evidence already is. This distinction also implicitly relies on there being some Right Answer at the back of the book, if we could just sort it out properly, such that only the evidence that goes into the Rights Steps to the solution yielding that One Right Answer should count as lower order evidence. The best we can do in the sciences, as seen in cases like the Standard Model of Particles, may just be a really well-justified theory where, if pressed on its truth, there just is no further, clearer answer that could be given, even in principle. There is no answer at the back of book, hidden from us; it simply isn't there to do the work of ensuring that there is One Right Answer. And there is just a lot of very interconnected evidence to be weighed and sifted.

There are other excellent critiques of the various aspects of the JTB approach as it fails to help much with scientific knowledge. Dang and Bright (2021) offer compelling reasons to think that scientists need not have any of those three (see also Fleisher 2018). Brown (2015) takes up a similar challenge based on the involvement of non-epistemic values. I'm adding to those some further reasons that philosophers of science are dissatisfied with the tools coming out of contemporary epistemology in recent decades for analyzing what goes on in the sciences. In particular, accounts of knowledge that presuppose a firm distinction between truth and justification that holds even in the liminal cases will inevitably fail to capture what is epistemically distinctive about the sciences. We should not accept that these cases fail to be knowledge. Moreover, philosophers of science use epistemically normative terms frequently in contexts where explicit formal treatments of truth are not involved. Normative claims about knowledge, justification, and evidence suffuse our work. This means there is a space for a view of truth and of knowledge that serves a broader guiding role rather than merely serving as a formal apparatus into which we plug propositions and chug out truth values on occasion.

3. James and truth as making a difference to experience

James famously offered what arguably counts as one of the most mis-understood and quoted-out-of-context dictums in philosophy: what difference does it make to experience? This is the pragmatist challenge. A good deployment of a pragmatist challenge will be revealing; it is not an in-principle

challenge that stops inquiry, but an open question which offers a path forward to resolution. This path might be a dead end: it is a useful outcome of issuing a pragmatist challenge that clarity on pseudo-questions can also be gained. But sometimes it simply clarifies what is at stake in a question or claim in a helpful, empirically substantive, way. James offered pragmatism centrally as a way of construing truth, rather than the more Peircean way of offering pragmatism as primarily about meaning (Misak and Talisse 2019). My focus here will be on truth, rather than on meaning and reference.

I want to lay out the features of experience and making a difference to experience that can be used to put together a way of thinking about truth that is not a version of a correspondence theory of truth. For reasons of space, I am developing the pragmatist alternative to correspondence views of truth, but not directly criticizing correspondence theories. There are excellent critiques of the naïve correspondence theory by expressivists such as Huw Price (e.g., 2011, 2013) on which I will draw. Price's critique is apt, and also fits neatly with the point in the previous section about there not being any answers at the back of the book. In the correspondence view, as Price describes it, the meaning of words is a matter of matching a linguistic bit to the corresponding shape in the world, rather like the kid sticker matching books. In these books, there are shadows or outlines, and in the back, there are pages of stickers. The matching game is to find the right sticker for the right outline and stick it in its proper place. There is only one sticker per shadow, and one shadow per sticker. There is a real tendency, among philosophy of science, to assume that truth works similarly. Much of the discussion around realism, for example, has assumed that theories are real when they correctly refer, and they correctly refer by having little sticker terms that correspond, uniquely, to some shadow-shape in the world, and it is the job of science to match the sticker to the right bit of world. Price offers a pragmatist alternative for word meaning, namely, that of expressionism, where word meaning is instead given by the full nexus of usages in which that word figures. I direct readers interested in more detail on this to Price's work and to Chang (2022).

James' challenge makes it useful for contemporary application, and his pragmatism both overlaps with and differs from more familiar views like early Carnap. If you like logical empiricism despite its shortcomings, you might love pragmatism. Misak and Talisse (2019) note that James was dissatisfied with the narrower version of the 'pragmatic maxim' that Peirce offered, and "proposed a broader rendition according to which the point of pragmatism is not to dispel metaphysical nonsense, as

Peirce had alleged, but rather to settle metaphysical disputes” (3). Carnap’s popular earlier works are vehemently anti-metaphysical, in line with Peirce. But James’ work shifts the rejection of metaphysics into something more useful and less intellectually dismissive, in a way more in line with later work by Carnap and especially with contemporary discussions of Carnap. James offers a formulation of the challenge that allows for it to clarify and illuminate, rather than merely eliminate or reject. He offers an unusual instrumentalist view of truth: truth is *for* something, assessable by how effectively it allows us to navigate experience. Truth is not sterile; it ties metaphysical and epistemological inquiry inextricably together.

I’ll highlight several key features of this new pragmatist challenge, as clarifications and updates to James’ original version. First, it involves a more expansive, holist, and active understanding of what is comprised by experience. Second, there is an expanded construal of what counts as making a difference. Thirds, this challenge is answerable, and can be used in practice to transform apparently in-principle or untestable metaphysical commitments into ones with genuine empirical consequences. Fourth and finally, because the body of experience which requires navigation is growing and will continue to grow, truth is processual, in need of ongoing maintenance and updating, not an Absolute that, once attained, must remain timeless unchanging forever lest it have always been false all along.

First, this challenge invokes a much broader notion of *experience* as something to which such a difference can be made. James’ views of experience can be found in *Principles of Psychology* (1890), and in his later writings such as *The Meaning of Truth* (1975/1909). James took experience to be a holist process, a continuous stream without naturally occurring breaks or internal divisions. He rejected the idea that it came pre-marked into discrete content or time units. Experience is not composed out of smaller bits; the smaller bits can be identified and discussed by identifying them as part of the whole. Furthermore, experience is not simply passive; it doesn’t just happen to us. James thought experience was active in several senses, including the fact that the actual content of moment to moment experience is actively directed by volitional control that we have over attention. *We do* things, like attending to something, that direct and shape experience. Experience also involves our actions and their consequences, and how we respond flexibly to continue directing our actions towards ends or goals. There is no neat way to separate out a passive component, in which

experience ‘comes in’ all on its own, from an active component ‘going out’ over which we have control.

This expansive and active character of experience contrasts with the spare, atomistic view of experience from influential empiricist movements in the early 20th century, like logical positivism or Russell and Whitehead. A Jamesian pragmatist can draw on this much richer understanding of experience, shifting the significance of saying that all meaningful claims, even those that have no directly observable component, nevertheless can make a difference to experience. A difference can be made by making a conceptual structure simpler to apply, or easier to explain, or more understandable. Applying, understanding, explaining are parts of a broadened view of experience, to which differences can be made.

Second, consider what it means to *make a difference* to that broadened understanding of experience. The pragmatist challenge originated in puzzles like that of the squirrel on a tree, encountered by James and his companions on a hike (1875/1908). The squirrel managed to keep on the opposite side of the tree as some hikers circled the tree. So, did they “go around” the squirrel, or no? James, apparently fed up with the tedious argument, tells the reader that if by “go around,” one means, moved in a circle in which the squirrel is contained, then yes, they did go around the squirrel; and if one instead means, went in a circle that allowed one to view the squirrel from all sides.

What difference does a terminological dispute make to experience? A common answer is that it is *merely* terminological, and, thereby, not empirical or not substantive (this includes philosophy of science, and also metaphysics; see e.g. Sider 2013). A Jamesian approach to truth allows for differences in precise usage of language to be genuinely meaningful.² Even if disagreements about the precise boundaries of term usage do not have clear and immediate empirical consequences, they have diffuse empirical consequences that also matter for experience construed broadly. All conceptual usage in the sciences connects with other conceptual usage; no usage floats so free of other meanings that it can be changed arbitrarily with no impact. Such terminological disputes are often quite important in formulating new versions of a theory. They clarify how to apply concepts in

² This pragmatist stance on terminological disagreements still being significant is also in line with Chang’s (2004) analyses of conceptual extension and revision.

practice; they are often important to improve understanding. All of these count as making a difference on the broader view of experience. This also highlights how collective the endeavor is: it is not the Quinean web of one individual changing terms to fit their idiosyncratic preferences. As Putnam emphasized, the language of science is a tool like a ship, not a hammer, one that requires many people to coordinate to be used.

Third, the pragmatist challenge is emphatically *not* a skeptical challenge or rhetorical move. It offers opportunities to be answered definitively. It is not the philosophical equivalent of throwing one's hands in the air and declaring, "How can you think that?!", or pressing the skeptical challenge of how it could all be an illusion or mistake. Applying the pragmatist challenge does not stymie further discussion; instead it opens it up for a clarification of what, exactly, the original claim, term, or idea is supposed to be. It can be answered by carefully examining the role a contentious concept might be playing in other reasoning, how it connects to other, more settled, terms; it invokes pragmatist, or expressionist, accounts of meaning as the totality of usages, including inferential connections, in which a term is involved. The pragmatist challenge is intended to move discussion forward. To be well-posed, it must be in-principle answerable.

To illustrate this third point, consider the example of Andersen's (2018) deployment regarding Lange's (2018) account of laws versus meta-laws. Meta-laws, such as conservation laws, might explain why there are the phenomenological laws that there are, because they must be thus as necessitated by the meta-laws; or, meta-laws might be coincidences that hold because of the phenomenological laws that hold, where the phenomenological laws determine or explain why the meta-laws happen to hold. Lange claims that we should acknowledge this distinction as an open question, not settled by current evidence and open to revision pending future developments from science. Andersen (2018) uses the pragmatist challenge on this distinction: what difference could it make to experience, if a law is a meta-law constraining phenomenological laws, or holding coincidentally based on phenomenological laws? The resolution of the challenge reveals that accepting this as a meaningful distinction is incompatible with holding most versions of a Humean account of laws. Treating the world as a Humean mosaic requires commitment to the non-existence of certain kinds of laws, namely, those that constrain the ways the mosaic could be. Even insisting on the possibility that laws might constrain rather than merely describe rules out Humean mosaic views: the possibility of constraint laws just is the possibility that the Humean mosaic description is

incomplete. By asking, what difference does it make to experience to allow this distinction as an open question?, we don't even need to have an answer to the specific question of whether such laws do in fact exist to see that this open-mindedness already rules out Humeanism; committing to Humeanism just is to commit to this distinction being meaningless or inaccurate, to commit to it *not* being an open question on which science might yield additional insight. This makes it an indirect yet still empirical issue. The challenge does not tell us what to do but it makes clearer the stakes of the distinction.³

A fourth point, which will be central in later sections, is that when James rejected metaphysics, it involved a fairly specific understanding of what metaphysics was such that it ought to be rejected. He was not rejecting any possible approach to metaphysics; he was rejecting the Hegelian idealism that was the then-current form of metaphysics. Indeed, he was much more amenable to the radically different 'metaphysic' of someone like Shadworth Hodgson. James' pragmatism was an alternative to what he thought was a sterile idealism; he does not specifically invoke the term metaphysics, but pragmatism is a direct alternative to the specific metaphysical views of that time as a way of philosophizing about the same topics. James' pragmatism involves a deep inextricability of what we would now call metaphysical inquiry with epistemological questions. His rejection is of metaphysics shorn of its connections to other questions about knowledge and experience, shorn of its situation as part of natural philosophy. Insofar as truth is what helps us get around experience better, and metaphysics proceeds via questions like, what difference does it make to experience?, what it means for something to be true just is another way of talking about the difference that something makes to experience. The pragmatist challenge, what difference does it make to experience?, is the question of how to *use* metaphysics, done in a pragmatist vein, to make inferences, connect concepts, integrate different arenas of scientific work into a coherent single view of the world. On a pragmatist approach, neither metaphysics nor epistemology can be effectively done in isolation from one another.⁴

³ There are many other papers that offer what I would count as a pragmatist challenge to make progress on a debate, though I am not presuming all such authors would also count their projects as of this sort or label themselves as pragmatists; for example, Jacquart (2021).

⁴ See also Chakravartty (2017) on the inextricability of metaphysics and epistemology.

As part of this inextricability of metaphysics and epistemology, any answer to the question, what difference does it make to experience?, is going to be holist to some degree. The truth of claims cannot be assessed without the inferential connections between those meanings that mutually constrain them in a coherent web or network. Attempting to atomistically isolate and evaluate one claim by itself would involve having to pin the meaning of that claim down in such a way that it would ineliminably involve a host of other claims and commitments.

In addition to being holist, knowledge is for James is active: it requires updating or maintenance as the body of experience on which it is based and which it is supposed to help us 'get around' grows over time. Truth will not be timeless and unchanging; even the vocabulary used to express claims will necessarily shift over time. Jamesian truth, conceived of as improving our ability to get around in experience, and the holistic and collective aspects of that experience, already carries with it the double commitment that something which makes no difference to experience could not be true; and that truth is *for* guiding actions, including the actions of belief formation and refinement.⁵

4. Trueing

This section develops a pragmatist way to think about truth in the sciences for philosophers of science who don't usually think about truth. It is different than what many or most philosophers of science conceive of truth, or even think of as what an account of it could look like. I will not give a formal definition, or necessary and sufficient conditions; it will not look like a standard epistemological definition of truth. It actively precludes easy use of spurious mathematicisms. Instead, it is a background way of thinking about truth to facilitate more apt discussion on topics where truth itself does not play a central or formalized role, almost like a conceptual metaphor. Any time we invoke epistemic norms, *some* notion of truth serves as the foundation for those norms. We ought to make sure it is a good one. I'll argue it should be a pragmatist version, instead of the

⁵ There is a similarity here in the Jamesian approach when compared to, e.g., Legg's (2014) exegesis of Peirce's view of truth as a tool for the ongoing process of inquiry. As she points out, James has a more explicit focus on truth while Peirce's focus is on the process of inquiry. My claims about James in this section renders his view regarding the process more similar to that of Peirce's, yet without the focus on the end of inquiry. James leaves open the possibility of ongoing inquiry in a way that contrasts with Legg (2014).

correspondence theory many philosophers of science implicitly think of. We ought to make sure our background view of truth is one we considered and chose, rather than one we ended up with and haven't re-examined.

What we need is a way of thinking about truth that is itself useful: the view itself should be directly applicable. It should guide analyses of knowledge products like models across disciplines, and also guide actions that require knowledge derived from those products, such as policies. It should emphasize the fact that knowledge products in the sciences are *used* – they serve functions and are put to use by a wide range of people, for a wide range of tasks. It should accommodate change in what counts as ‘true’ without trivializing it: it should not be the case that anything accepted at some time counts as true at that time, but it also should not be the case that we only reach truth in science when we reach a final, never-changing and final ‘end of science’. An effective view of truth should be explanatorily illuminating of the specifics of how what counts as true have changed through specific historical episodes.⁶

Briefly, we should think about truth in science not as a static property of individual propositions, but instead as a state into which knowledge products can be brought, and out of which they can fall, where truth is a process-oriented activity. It is aptly called trueing, and knowledge products like models can be in true or out of true. Trueing fills the lacuna identified in earlier sections: it does not require a clear and consistent distinction between truth and justification, and it precludes application of truth to isolated statements taken out of context. Knowledge products like models have directions of fit, and norms governing what counts as fit give us a handle on what it means for something like a model — not individual statements or implications drawn one at a time from a model, but the whole package — to be true.

An extended metaphor illuminates what trueing is and how direction of fit provides epistemic and action-guiding norms for the process of trueing and for using trued products. Consider bicycle wheels. A bike wheel that is in true spins around the wheel center with rotation only in one plane. It doesn't wobble (rotate side to side) nor hop (go up and down) while rotating. Most wheels are kept in true using a series of spokes. These spokes radiate from the center to the outer rim, and are in

⁶ This does bear some similarity to the discussion offered in Teller (2012).

tension: they pull the outer and center rims. Crucial to their performance, spokes are not aligned in the plane in which the wheel should rotate. They cross the plane in which the wheel needs to rotate, starting on one side at the outer rim and meeting the inner rim on the other side, alternating. It is the arrangement of spokes around the wheel and the tension in them, plus the intrinsic springiness of the materials, that keeps the wheel in shape so that it rides the way that two-wheeled bicycles should. If you consider just one spoke alone, it definitely is not aligned along the plane where the bike should rotate, and it is often aligned off that plane in an asymmetric way. In this way of understanding true, it is not opposed to false. It is instead a directed relation, a fit towards which something is guided or at which something can be aimed. In true is a state we aim at, which can be accomplished in degrees, not a binary of in-true or false. The wheel aims at that alignment, and even where it falls out of alignment, it is evaluated according to that direction of fit, of wheel onto plane of rotation.

When a wheel is out of true, it can, depending on how far out of true it is, probably still be ridden (hopefully to the shop to get it fixed). To true a wheel, it is taken off the bike (out of the context of immediate application) and mounted on a system that lets it spin so that a technician (who, for the sake of this metaphor and with some presumption, we'll call a scientist) can assess how out of true it is and where. Then, and this is also an important part of the pragmatist metaphor here, the wheel is put back into true by adjusting the spokes. It is vital to note that being in or out of true is not a dichotomous, yes-or-no situation. The same wheel can be more or less in true: barely out of true; noticeable but still rideable if you're careful; so out of true it would be unsafe to ride; so out of true that it can't even rotate within the forks of the frame. There isn't a fixed threshold such that above it, a wheel is in true, and below it, it is out of true. One wants to have a wheel that is more in true, other things being equal; and one can only get the true so good before it realistically cannot be improved on.

It is physically impossible to have a perfectly in true wheel, even in the best-case scenario. There is a mathematical limit towards which the wheel is aiming, in the sense that a technician working on the wheel has a completely unproblematic limiting case towards which the wheel is being trued. It is just not possible, with physical materials of any kind no matter how carefully engineered, to achieve a mathematically exact trueing of a wheel. This has nothing particular to say about any given wheel,

but rather about how mathematical limits (the idealization that is the limit of perfectly in-true) fit onto the physical world.

It is meaningless to ask if any individual spoke is in true. The *wheel* is in true, because of the positions and tensions of the spokes. A single spoke cannot be in or out of true. It could be broken, it could be at a weird angle, it could be quite tight or quite loose. But it cannot be either in or out of true, by itself. For the wheel to be effectively in true, those spokes must, in fact, individually align in ways that are not directly related to the overall orientation of the wheel in the plane of motion. It is *necessary* for such a wheel to be in true that the off-plane orientation and spoke tensions support each other so as to achieve the correct overall shape. It does not mean any given spoke (or, an idealization taken out of a model and evaluated on its own as not literally accurate) is out of true (or, 'false'). There is not a unique 'right' setting for any given spoke; each could be set to genuinely different tensions that would all result in a true wheel, given a different arrangement of tensions in the other spokes. One spoke in isolation is neither in true nor out of true.

Fixing a bike wheel that is out of true is a process that involves a lot of craft, experience, and handiness. It can be taught, but not simply by reading some guides to it. It must be learned by practitioners in a context where they actually have to engage in the task, and where they have clear conditions for success even though there are non-unique paths to achieve that success. There is no formula for truing any given wheel, no recipe that will give the right result every time. It may involve hacks or shortcuts that work, even though no one is entirely sure why they work.⁷ There isn't One Right Way to fix the spoke tension to get the wheel back into true. Wheel mechanics is a pluralist endeavor.

However, there are some very important limits on pluralism here. One can be a pluralist about the ways in which this particular bike wheel should be gotten back into true. But one can't just say that anything is as good as anything else. There really is a difference between a wheel that is in true and wheel that wobbles. The wobble is 'wrong' in the sense of being out of true: the wheel should aim at

⁷ I have it on trustworthy authority that sometimes one might even just take a particularly bent wheel and whack it a few times, hard, on a counter. This is generally done out of view of the customer, and often quite effective, though in another sense it is a very bad idea with much to recommend against it. This also makes it a very apt for the analogy with science.

being a certain shape, even if it doesn't quite manage that shape; and the further away it is from that shape, the worse it is. That towards which wheels are trued is not an arbitrary invention. There are stakes to getting it trued, and there are norms for judging whether something is in true, true enough, more or less trued compared to another wheel, and so on. Even two wheels that are both in true may differ, because one has spokes with overly high tension compared to the other.

Trueing the wheel is a requisite part of getting the whole bike to function as it should, and getting the bike to work effectively is a key part of getting where we want to go on it, safely and efficiently. Truth as trueing highlights that knowledge is not generated and then left in pristine, never-changing condition. Just as the most in-true bike wheel will need some ongoing maintenance, trueing our theories and models in the sciences involves ongoing maintenance and refinement. It is not all a process of getting it right once, and then being done. Trueing is a process that must be refreshed, actively maintained. Even if no major changes or advancements have occurred for one particular model, it is still tied, in very Duhemian ways, to other areas via the very meaning of the terms involved or the instruments used to measure the relevant quantities. This means that changes elsewhere can still shift a model out of true.

This makes sense in light of points from James, such as the claim that truth can change over time, and that it is relative to experience, so that what is true for one time may not be at a future time with new experience in the meanwhile. The pragmatist view of trueing provides for knowledge to change over time. A model that had been true a decade ago may have drifted out of true, and benefit from tightening up, getting better fitted onto the parts of world where it is applied. There is very good reason to reject any account of knowledge that requires us to not know anything till we reach some (probably mythical) state of final Perfect Science where nothing will ever change. According to pragmatism, such an End of Science would require the actual termination of accumulation of new experience. When cashed out this way, the End of Science is not an ideal towards which we could (or should⁸) even aim.

⁸ On James' way of thinking, the End of Science would be literally apocalyptic; it would just be whatever we had gotten to, before there was zero new experience accumulating, which could only happen where there simply were no more people to experience. This sounds dramatic and science-fictional, but it is a useful perspective: what we believe at the end of science is used in contemporary metaphysical discussions as a kind of orientation point, such as providing a reference magnets for current terms (e.g. Sider 2013). It is a substantive presupposition that the end of science is some

This also lets us make more nuanced claims about truth in the sciences than the scientific “Everything science says is true” approach. Using the notion of trueing, it should be quite clear that some results, like whether we know that we found the Higgs boson, should count as true, while others, such as nutritional information regarding the health consequences of chemicals like caffeine, are still in the trueing process.

5. Trueing models

In this section, I will consider contemporary discussions that are, I’ll argue, already working with a more or less pragmatist view of knowledge, but doing so by apparently rejecting ‘truth’ as a goal or epistemic norm in the sciences. Discussions of idealizations in philosophy of science almost universally proceed by presupposing the falsity of idealizations. I will take issue with this: only on a correspondence theory of truth, parceled out atomistically, are idealizations straightforwardly false. We don’t need to reject truth and replace that function with scientific understanding. Doing so still smuggles truth in, in implicit forms. We are better off just dealing explicitly with truth, and responding to shortcomings in how truth norms are applied in scientific contexts by improving, not eliminating, our view of truth. Instead of accepting a problematic view of truth on which central activities of science turn out to produce ‘falsehoods’, we should reject the starting assumption that idealizations are actually false. We should remodel our view of truth, rejecting one that does not work (a naïve correspondence theory) with one that does (a pragmatist view). Then idealizations make a great deal more sense, analogous to spokes on a trued wheel even though none of the individual spokes are aligned in the plane of trueing.

The correspondence theory of truth in this context is, roughly, that individual propositions should be carved out of models and then evaluated in terms of how well each of their individual terms correspond to specific bits of the world. Concerns about idealizations as false share this assumption, often made implicitly, that idealizations retain their meaning, without change, when isolated from

utopian state of knowledge; it is as or more likely to be dystopian, and not at all illuminating of what we, as historically situated humans doing our imperfect best now, ought to strive towards.

their modeling contexts, and that they should be literally true descriptions of the world in order to count as true. This is a view held even by those who want to defend idealizations and their importance to scientific practice: the issue is framed as one where we must explain away the falsity, or explain their success *despite* the falsity.

This shared background assumption is what I will isolate and reject here: idealizations are not straightforwardly false. They don't need to be made more true, or accounted for as false yet useful. They are only false on a naive correspondence view of truth applied atomistically to artificially isolated sentences. These idealizations make sense, and the models in which they must be interpreted are true, on the trueing view. Situated in the context of models trueed onto the relevant systems in the world to which they are applied, these idealizations are just like other statements that allow us to get around experience, construed broadly, in a more efficient way. What would make them look more 'true' on the atomistic correspondence view would actually make them less true on the pragmatist view of trueing. They do not require explanation as to their ubiquity and usefulness *despite* being false, because they are simply ubiquitous and useful: they are not incompatible with or in tension with truth-seeking as an aim of science. It is equally viable to reject the view of truth that makes idealizations false and avoid having to develop a new set of epistemic norms developed just to account for them.

I'll make this case by focusing on recent work by Potochnik (2018) and Elgin (2017) as examples of the assumption that idealizations are false, and that as a result, truth should be rejected in favour of understanding as an epistemic aim of science.⁹ These authors differ in the details, yet share the commitment that idealizations are straightforwardly false, that idealizations persist despite falsity because they have some other attribute that renders them more useful than they would be if they were made true, and that they require analysis or explanation in virtue of this tension where idealizations and truth are at odds. All of this, I'll show, makes more sense when we instead think of idealizations as akin to spokes: not in true themselves, but also not evaluatable as true on their own.

⁹ These criticisms of idealizations are similar to ones made by Rice (2021), and his book offers a much more detailed and nuanced discussion of the issue of idealizations in models, their role in explanation even when those idealizations are distorting the representational target, and other ways to address this than treating them as simply false. His account differs from the one I offer here, but I take his view to be compatible with this general metaphor of trueing.

We should instead think of idealization as analogs of orientation and tension that makes them look 'false' on a correspondence theory but which, on a pragmatist approach, is how they successfully make a whole model in true.

Elgin (2017) offers a very clear example of what it looks like to accept the presupposition that truth requires an atomistic oversimplification of complex scientific claims. She holds that epistemic normativity, in which she includes truth and truth-conduciveness, is not a part of what science does, as evidenced by the extensive reliance on idealizations and other claims that she labels as obviously false (see, *inter alia*, p. 2). I begin with her account both because it is the most extreme example, and because she specifically identifies the target of her criticisms as a Jamesian view of truth.

Following William James (1951), many veritists hold that our overriding cognitive objective is to believe as many truths as possible and disbelieve as many falsehoods as possible (e.g., Alston, 1988, 258; Goldman, 1999, 5; Lehrer, 1986, 6; Bonjour, 1985, 9). This is the objective with respect to each and every individual contention. It is, they say, never legitimate to accept an acknowledged falsehood, even if so doing would in the long run result in the acquisition of more or a higher proportion of true beliefs than the repudiation of that falsehood would (Firth, 1981). As stated, Jamesian veritism is remarkably tolerant. It does not discriminate among true (or false) beliefs. Insofar as our goals are purely cognitive, it says, we should amass as many truths and as few falsehoods as possible. (2017, 10-11)

There are several things that go wrong here. One is that, put bluntly, James never endorsed such a view of truth, and would have spurned it completely. The view she calls Jamesian is precisely the sort of view James rejected as sterile and making no difference to experience. She claims that compounding propositions with logical operators like 'or' results in a new truth, and that one can thus multiply vacuous truths ad nauseum (10-11). James explicitly denied this; this proliferation of vacuity is precisely what he wants to rule out by imposing the requirement about making a difference to experience. Elgin's target is a view of truth, however, that is common in recent decades in discussions in traditional epistemology. In that regard, I concur with some of her criticisms of that view of truth. It simply is not (emphatically) a *Jamesian* view. Authors who take this approach to truth, such as Gettier and work on the Gettier problem, reject James and pragmatism. The tradition

she identifies as a 'Jamesian' target is the tradition that a Jamesian would reject, and a tradition that rejects James.

Elgin offers an entire account of 'felicitous falsehoods' that is predicated on the idea that "science unabashedly relies on models, idealizations, and thought experiments that are known not to be true" (ibid., 1). She wants to 'dethrone' truth as an epistemic norm for science. The replacement is felicitous falsehoods and 'objectual understanding' (e.g. Kvanvig 2003), which is understanding of a topic or area. The key parts she takes issue with here seem to be the atomistic character of many accounts of truth (see e.g. 10, 13, 115). The move to objectual understanding is at least partly intended to correct this atomism with more holism.

What is a pragmatist response to Elgin's claims? Truth should make a difference to experience, and that alone is reason to reject those views of truth in which it does not; one does not need to also reject the entirety of truth-seeking in science. We can excise with a scalpel rather than an axe. It is more philosophically precise to reject a wrong-headed view of truth, and replace it with a better way of thinking about truth, than to purport to reject epistemic normativity altogether yet continue to rely on implicit versions of it in other forms, such as objectual understanding, or the felicity of felicitous falsehoods. Unless anyone's objectual understanding counts as good as anyone's else's, then epistemic norms are playing an under-the-table role. And what the objections are about is not any possible view of truth, but just one strain; that strain can be rejected without giving up entirely on usage of truth.

Potochnik (2018) follows Elgin and argues that we don't use, or need, truth in the sciences, and offers a range of alternative features of scientific practice in its stead, in particular that of understanding. What she offers is really quite close to the pragmatist understanding of truth, though she offers it as a rejection of truth. What she presumes and then rejects is not any view of truth, but one that strongly resembles a naïve correspondence theory of truth attributed atomistically to propositions. She targets idealizations specifically, not idealized models per se: it is individual elements of models that count as idealizations, not the whole model containing such components. Idealizations are taken to be statements that are not literally accurate as descriptions of the world, and sometimes is extended to include statements that are abstract in their description, such that adding more detail about concrete particulars would thereby make the statement less idealized

(chapter 1, chapter 4; see especially 93). Idealizations are “assumptions made without regard for whether they are true and often with full knowledge that they are false” (42).

She frames her view in response to the tension between two claims: first, that idealizations are false; and second, that they are extremely useful and ubiquitous.

In my view, idealizations are both rampant and unchecked in science. By *rampant*, I mean that idealizations are found throughout our best scientific products, and they stand in for even crucial causal influences. By *unchecked*, I mean that little effort is put toward eliminating or even controlling these idealizations. (41-42)

Elimination or control of idealizations would involve adding details that make for a much more complicated description. In her second chapter, she situates the rampant character of idealizations as serving a simplifying function: because we are modelling complex parts of the world, we need simplifications to render that complexity more tractable. Thus, she explains the “rampant and unchecked” reliance on idealizations as a necessary strategy for scientists in such a complex world.

Note that the very definition of idealizations here stipulates their falsity. Idealizations are not merely simplifying assumptions or elements of models: they are defined as the simplifying assumptions that are *known to be false*. On her approach, there is no neutral stance for idealizations to possibly be true, or play a truth-apt epistemic role, because they have already been defined as false. She sometimes interchangeably calls them inaccuracies (e.g. 43), and wants to explain the use of idealizations under the assumption that such use *is* the introduction of inaccuracies. Terms like “rampant and unchecked” are laden with normative judgments. As with the definition of idealizations where they are false, this is somewhat question-begging. But it is central to her framing that idealizations be false. Otherwise, their centrality in scientific practice is unsurprising, and requires no move to understanding.

Extensive reliance on idealizations in modelling is only a problem to be accounted for if those idealizations are individually false and the goal of modelling is to produce individually truth propositions. Potochnik solves the tension by rejecting this second claim. I’m arguing that we should reject that first claim (though can also, of course, reject the second): idealizations are not

straightforwardly false. If they are not false, then they aren't rampant or unchecked, they are instead ubiquitous and central (see also Rice 2021). Idealizations are required to true models (similar to fitting-out models, e.g. Cartwright 1999). They are neither false nor true on their own. They should be assessed in terms of their role in bring the model into alignment, to move it in the direction of better fit.

Khalifa (2018) holds that there is necessary place for truth in explanation and understanding, contra Potochnik and Elgin, and clarifies understanding without thereby rejecting truth. Yet he also subscribes to the view that idealizations are false, and that we thereby must account for why false statements can still be so useful and widespread. His view would be even stronger if one instead thinks of truth as a pragmatist, thus eliminating the need to account for the presumed falsity of idealizations and adding explanatory resources to why such statements are both useful and widespread. A host of other participants in discussions on models and idealizations also subscribe to this idea that idealizations are false, fictional, or otherwise untrue (e.g. Bokulich 2012). Each of these accounts would be stronger, and better for the authors' purposes, by removing the presumption that idealizations are individually and literally false.

Note that accounts like Bokulich's and Khalifa's are not about truth per se. They are concerned with other aspects of science, and don't need a formal definition of truth to do this work. It is instead a vague form in the background that gives the impression that idealizations are atomistically false and, thus, in need of explanation. It is for this kind of task I am offering the account of truth as trueing: it can be a different mental image that subtly shapes discussions about other topics than truth itself, in a more fruitful way than the naïve correspondence theory has. One might think this is a merely terminological dispute, especially since neither Bokulich nor Khalifa reject truth-seeking as an aim in science. Indeed, Bokulich (2016) offers something quite close in spirit to the view developed here. But terminology also matters. This is not *merely* a terminological dispute, but a difference in the ways in which we can organize, adjudicate, and find our way around within the total scope of human knowledge. That is why it is pragmatist. Better maps for how to get around the same territory are not merely terminological differences. They have consequences for action. Thinking of truth as trueing can *facilitate* these other discussions, not merely rearrange terms.

I've discussed idealizations in terms of how they help true models onto the parts of the world for which they are used. This is subtle but deliberate terminology: I am not merely talking about representation. Some models may work by successfully representing. But some work without representing; for instance, views about models as tools (e.g. Bailer-Jones 1999, Sterrett 2006) are quite amenable to pragmatism (it is likely also that Weisberg 2013, especially chapter 6, is pragmatism friendly). Models, standing in as a central example of a knowledge product, are *for* something, putting active and action-guiding aspects of scientific practice more explicitly at the center of the discussion. It is suitability for that use that settles the questions about how in-true the model is and how it could be made more so.

This complements but also refines and extends the means-end epistemology of Schulte (1999), means-end metaphysics of Hitchcock (2012), and means-ends analysis offered by Woodward (this volume, 2021). Trueing helps identify the granularity at which means and ends are aptly picked out. Return to the bike wheel analogy. It is very normal for a technician to ask, for what will you be using the bike? Because the wheel needs to be in true not for some purely aesthetic Platonic ideal of perfect circles. It needs to be in true because you use it, by riding your bike to do things, and in order to do those things on your bike, the wheel has to be capable of filling certain roles effectively. But the technician is not looking for answers like: I will ride it to the store, and also to the library, and also to work. Those are not well-differentiated ends for this wheel to serve as a differentiated means by which to achieve them.

The wise technician listening to those answers will instead ask a few clarificatory questions, like: Are those all paved surfaces, or are any of the paths gravel? How smooth are the roads you are riding on? Are you usually on the sidewalk or a side road, or are you also riding in heavy traffic on busier roads? How fast are you riding? Are you the main person riding the bike, or is it shared? The more specific descriptions (to work, to the store) of the end cluster together (all on the same paved sideroad), because there is no difference in the performance of the wheel that matters to your performance of them. Instead, ends like the type of surface over which you ride, and the speed at which you ride, matter a great deal to how the wheel needs to perform. Means-end analysis runs the risk of fine-graining the ends too much compared to the different means available. This overly fine grain obscures the underlying unity of means required to reach those ends. Thinking of means-ends

analysis in conjunction with trueing for scientific practices and knowledge products guides the analysis to pick out ends, and thereby means, with the right granularity.

Trueing also connects to the work of Hasok Chang (see especially 2004, forthcoming). Chang emphasizes how instrumentation development goes hand in hand with conceptual refinement and extension, and model or theory development. He identifies the epistemic circularities endemic to any development process in the sciences where new instrumentation is used to measure quantities. These circularities look self-defeating, on a more traditional epistemological analysis. Yet these ‘puzzles’ don’t look so puzzling when we think in terms of trueing. Chang offers an account of epistemic iteration for the dynamic of how progress can occur despite epistemic circularity. The principle of respect offers a resistive force to change, counterbalanced by the imperative of progress. Small changes in iterative steps allow researchers to start from suppositions they may already know will eventually be improved. They iteratively incrementally improve their starting position, including of the very initial suppositions required to get the whole process moving. Those starting assumptions may eventually be entirely rejected in lieu of replacements, a rejection only enabled by presupposing them in the first place.

This is what the metaphor of wheel trueing picks out. One doesn’t simply fix a wheel, at once, with a single go. Trueing involves changing the tension on one spoke, moving to another, and testing the effects of altering a given spoke’s tension by spinning the wheel. It might be that the initial change made to a spoke eventually gets undone. Nevertheless, having started with that change was part of getting all the spokes into a tension alignment that eventuated in the wheel spinning evenly along. One need not jump to the conclusion that the original change to tension was ‘wrong’, even if it was eventually undone; nor need we jump to the conclusion that the original presuppositions were ‘false’, even when they are eventually modified or replaced. Instead, epistemic iteration and truth as trueing both push us towards a more dynamic and temporally evolving process of refinement and improvement. Epistemic iteration and trueing both emphasize the ineliminable diachronicity of truth-seeking processes in the sciences.

As terms shift in usage anywhere in a science, these shifts alter the meaning generally, not just in the localized area in which they occur. Change in some other areas can bring a given theory or model out of true over time. There is nothing wrong when that happens: we need not judge the original

version of a model to have been lacking because it now needs to be re-trued. Instead, we can see how it was as true as it could get under the circumstances where those were the available conceptual resources. With new concepts, more precisely bounded because of subsequent developments in nearby areas, we are able to true even closer than we were before. This is James' point about the growth of the body of experience that truth is required to help us get around. This does not have to mean the wheel was out of true at that earlier time. No wheel stays true forever.¹⁰

6. What's at stake: "But is it really true that..."

One might think, who cares? Why should we go back to talking about truth, even if we do a better job of it, when we can skip all that and simply talk directly about other things that matter, such as scientific understanding and explanation? In this section, I am climbing on a soapbox to tell philosophers of science that it matters a huge amount that we reclaim and make use of 'truth', that we be able to clearly, definitively state that some claims are true without extraneous caveats, and that we do so without the pangs of conscience that would come if we had some non-pragmatist view of truth, or no particular view at all.

There are two primary reasons on which I will focus (there are more; but I will just address these). The first is that we don't have sole claim to the term 'truth', and so cannot simply declare it to be worthless or not an aim of science, when many scientists themselves take themselves to be in the business of finding (what they want to call) truth. The second is that we do a disservice to non-scientists trying to make responsible use of scientific results when we declare that truth isn't important, or isn't what science produces, or isn't a central epistemic aim for science. Trying to substitute in something like understanding raises legitimate concerns that science is really about feelings of understanding, to be had as (or more) easily from watching videos on one's own as from struggling to grasp a complex scientific explanation. We owe people clear and effective communication, because they are largely trying to develop their own beliefs to guide their own actions in order to navigate the genuine lay of the land, not merely what they wish or feel. And we

¹⁰ This section could have responded to a much larger number of other connected and interesting projects that interact with trueing in various ways. Magnus (2012) on natural kinds is such an example, as is Massimi on perspectivism (e.g. 2012, 2018) and Ismael (2007).

can do that kind of effective communication, without compromising our philosophical scruples, by getting rid of a naive correspondence view of truth and replacing it with a more apt view instead. We owe it to others to be pragmatists about truth.¹¹

On the first point: as philosophers, we have some claim to be experts on truth, and as philosophers of science, are well-situated to make informed claims about the truth-aptness of scientific practices and results. But we do not have sole proprietary claim to usage of that term. Explication projects, such as this one and the views discussed in the previous section, have some flexibility in terms of settling the precise boundaries of term usage. This is already an under-the-table pragmatist way of thinking about settling what ‘truth’ means, with word meaning given by clarification of actual usage. Sometimes usage is ambiguous, and then explication projects work on settling it to facilitate clearer use. Terms like truth and understanding hang together inferentially; moving the boundaries of the meaning of one term implicates the meanings of other terms. Expanding ‘understanding’ or ‘explanation’ to take up much of the work that was previously done by ‘truth’, even if one refrains from explicitly talking about truth, is not neutral with respect to truth.

Our explication projects must still respect how the term ‘truth’ is used in settings outside of philosophy. Indeed, the accounts of idealization in the earlier section take it to be a specific point in advantage of their view, and a criterion for competing views, that adequate justice be done to actual scientific practice. Looking for true models, or truth about a system, is a way of speaking that is used widely and extensively outside of philosophy, and most relevantly for this chapter, in the sciences. Insofar as we take ourselves to also be accountable to provide apt and accurate construal of scientific practice, we must acknowledge that the idea of truth, and the goal of truth-seeking, is one that is widely endorsed by practicing scientists. It is definitely true that mostly, they are not seeking Truth, the absolute, never-changing, perfect and literally true version where there is a correspondence that neatly matches up bits of a description to the right bits of the world. But we can reject Truth as an apt characterization of the processes and results in science, while still having to recognize the ubiquity of truth, construed pragmatistically. One might be able to convince some

¹¹ de Melo-Martin and Intemann (2018) offer a much more detailed and pointed discussion on inappropriate dissent in science and the ways it can fracture trust. Elliott and Resnik (2014) offers an effective case for the way in which values inevitably influence science communication and policy and how transparency is a solution to this. See also Douglas (2016).

group of scientists that there is no truth but Truth, and that science does not seek Truth. But presenting an alternative, pragmatist, view of truth results in a very different reception. Many scientists (though not all, it should be noted) resist the idea that what they are doing is producing a timeless, unique, literal and perfect Truth about the world. But most of them very much endorse some version of truth with a small t, and would be unwilling to give that up as a central epistemic norm for research. Pragmatism offers truth with a small 't'.

The second point in favour of endorsing a pragmatist view of truth is that it matters for genuinely effective communication. As philosophers with this legitimate claim have expertise in 'truth', we need to be able to make clear and unequivocal statements about truth-apt claims from the sciences. And, we have to do this while satisfying two further considerations: that of not adding lots of hesitant or overly convoluted caveats; and that of not having to compromise our philosophical scruples and make such statements while also feeling like we are lying to or misleading our audience. This is entirely possible if we adopt a pragmatist view of truth. It is easy to mistake this point for being merely pragmatic. But pragmatic, and pragmatist, are not interchangeable. Pragmatic is something one can be with unattainable ideals of truth on which we give up because of our compromised epistemic positions. Being pragmatist involves rejecting those unattainable ideals of Truth in the first place. Pragmatism isn't giving up or compromising on our goal; it is having a different goal. We can be pragmatists about truth without having to be pragmatic about truth.

It is a social obligation that we have, in communication with practicing researchers and with others who are neither scientists nor philosophers of science, that we accurately and clearly communicate without unnecessary confusion. It is just true, full stop, that vaccines do not cause autism, and COVID is not a hoax but an incredibly serious disease, that human-induced warming of the global climate is an unfolding disaster we can yet alleviate. Even though science is not perfect or complete, it is true that science remains our best method for finding out about how to address challenges like public health, like effective policies on education, like appropriate sources of information on which to base decisions and for guiding our policies and actions to best achieve our goals. The most effective way to genuinely communicate this is to be a pragmatist about truth and knowledge.

Adding caveats like “well, we can't say it's *true* that humans are exacerbating climate change, but our best theories tell us that...”, or “we don't *know* that this series of heat waves is occurring because of

human-induced climate change, but it is highly probable ...”, needlessly undermines legitimate communicative goals and conveys a misleading impression instead of what we wanted to convey. Even if what we wanted to convey was precise and accurate, it is not *successfully* conveyed, and we have to take that success in communication into account. It does an injustice to those views: it falsely makes them weaker, in a way that understandably creates confusion in our audience. When we invoke more complex notions like justification or understanding as a substitute when answering questions that are about which claims are true or count as knowledge, we thereby mislead about the status of those claims. Philosophers might take themselves to be making classificatory points about preferred terminology. What is opaque to the audience about the philosophical reaction to first correct the terminology then answer the question is that someone doing this wouldn’t actually endorse *anything* as true, because they reject the very idea of truth. Insofar as they would be willing to endorse any assertion as true, this assertion under question would be among those endorsed. That is why this is an inappropriate context in which to be making those terminological corrections: instead of succeeding in correcting terminology, it misrepresents the epistemic status of the claims under discussion.

Furthermore, these caveats and terminological corrections open space for pernicious pseudoscience to flourish. When philosophers give ground by saying things like, “we don’t *know* X, but we have good justification for believing it,” it thereby endorses a generally incorrect view of truth, one we don’t actually have to buy into anyways, and then it uses that ill-fitting view of truth to draw the inference that we lack knowledge. This is backwards. We do in fact know some of these things; and we know it on a pragmatist view of knowledge and truth. We don’t have to explain all of that, every time; we can just be pragmatists, and make clear statements accordingly, and then answer further questions like “what do you mean by ‘true’” with details about pragmatism if such questions arise.

What I am urging here is how *essentially* misleading it is to an audience when we refrain from a full endorsement of some statements as true. Those caveats, “we don’t *know* it’s true...” would be appended to anything, but the audience to whom we are communicating when addressing questions like this are unaware of that, and do not have the opportunity to hear us respond to those other questions to calibrate their understanding of our hesitancy. As such, we are misleading them by hesitating – we are inducing false beliefs by inducing or growing false degrees of doubt about these particular statements, as if they were different than other statements that could have been, but

weren't, asked instead. Communication is not a solo act whereby we do our best using our preferred terminology in exactly the specialist fashion we want to, regardless of what other people think it means.

This may sound extreme, but it matters. We would do other members of our society an injustice if we answer questions like, "Is it true that this vaccine won't give my child autism?" with answers like, "Science doesn't seek truth, but we do have a good understanding of how this vaccine works..." This leaves the legitimate impression that science is only a collection of understandings, some beliefs and some justification, but no truth or knowledge. This encourages rather than discourages the idea that, if someone watches a video or reads a meme and gets the feeling of understanding, they are doing their own 'research', just like the scientists do., and who is say what is true or false. It can harm people. We should not overlook this: people outside of philosophy do, in fact, read and respond to what we say.

Philosophers of science don't get to stipulate how words are used in non-philosophical settings, and answering questions like this is generally done in a non-academic-philosophical setting. It is very prescriptivist, and somewhat condescending, to decide that truth is a notion that no one should use, or should use differently. It is explicated, clarified, etc. in philosophy, but does not belong to philosophers alone. Hedging on whether a claim is true or not because one has philosophical misgivings about 'truth' is a way of undermining the stance of anyone outside of philosophy to ask meaningful questions that we might be in a position to answer. It is a form of epistemic injustice by philosophers against non-philosophers, to decide that the idea of truth should simply be avoided and replaced with something else, and that we are only willing to answer in our own preferred terminology. Adopting a pragmatist stance towards truth lets us answer these kinds of questions, and make these kinds of assertions, in a way that is unhesitating, apt, and in alignment with our philosophical scruples.

Acknowledgements: This chapter has been in the works for years, and owes much to many great discussions and several seminars. Thanks to: Endre Begby, Eddie Cai, Hasok Chang, Mateo Ochoa Coloma, Iman Ferestade, Christopher Hitchcock, Yichen Luo, Matthew Maxwell, Milos Mihaljovic,

Sandra Mitchell, Nicolas Rescher, Aaron Richardson, Sherif Salem, Jingyi Wang, and participants in the three PragMaPS workshops.

I am deeply grateful for the opportunity to live and work on the traditional unceded territories of the Coast Salish people, especially the Squamish, Tseil Watuth, Musqueam, and Kwikwetlem First Nations.

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