CHAPTER 11

EPISTEMOLOGY AND PHILOSOPHY OF SCIENCE

OTÁVIO BUENO

1 Introduction

It is a sad fact of contemporary epistemology and philosophy of science that there is very little substantial interaction between the two fields. Most epistemological theories are developed largely independently of any significant reflection about science, and several philosophical interpretations of science are articulated largely independently of work done in epistemology. There are occasional exceptions, of course. But the general point stands.

This is a missed opportunity. Closer interactions between the two fields would be beneficial to both. Epistemology would gain from a closer contact with the variety of mechanisms of knowledge generation that are produced in scientific research, with attention to sources of bias, the challenges associated with securing truly representative samples, and elaborate collective mechanisms to secure the objectivity of scientific knowledge. It would also benefit from close consideration of the variety of methods, procedures, and devices of knowledge acquisition that shape scientific research. Epistemological theories are less idealized and more sensitive to the pluralism and complexities involved in securing knowledge of various features of the world. Thus, philosophy of science would benefit, in turn, from a closer interaction with epistemology, given sophisticated conceptual frameworks elaborated to refine and characterize our understanding of knowledge, justification, evidence, and the structure of reasons, among other key epistemological notions.

In this chapter, I argue for a closer involvement between epistemology and philosophy of science by examining two areas in which a close interaction would be beneficial to both: approaches to knowledge in traditional epistemology and in philosophy of science and the roles played by instruments in the production of scientific knowledge (considering, in particular, how these roles can be illuminated by certain forms of epistemological theorizing, such as internalism). I consider each of them in turn.

2 Approaches to Knowledge: From Epistemology to Philosophy of Science and Back

Epistemologists and philosophers of science have been concerned with the proper understanding of knowledge. In the hands of epistemologists, knowledge is approached primarily from the perspective of a conceptual analysis—as emerging from the attempt to introduce individually necessary and collectively sufficient conditions to characterize knowledge. The traditional tripartite characterization of knowledge in terms of justified true belief, which can be traced back all the way to Plato, received a significant blow with Ed Gettier's (1963) counterexamples, which questioned the sufficiency of the tripartite account by providing clear cases in which an agent satisfied the three conditions for knowledge—thus having a justified true belief—but failed to have knowledge. In response to the counterexamples, a variety of different accounts have been proposed, from causal accounts (Goldman 1967) through accounts that do not allow for unjustified false beliefs (Littlejohn 2012) all the way to accounts that resist the need to provide an analysis of knowledge (Williamson 2000). It seems safe to think, after decades of work by epistemologists on this issue, that the Gettier problem is unlikely to go away (Zagzebski 1994).

Although searching for a proper conceptual analysis of knowledge may help to clarify the concept, philosophers of science approach the issue differently. They tend not to provide an account of knowledge, but to examine ways of producing and assessing knowledge, however such knowledge is ultimately established. Different scientific fields invoke distinct resources to achieve the goal of advancing knowledge in the relevant field. There is very little in common between particle physics and clinical research, and, not surprisingly, techniques and procedures used in one are largely irrelevant to the other: particle accelerators are crucial to implement the research in the case of the former, whereas randomized controlled trials are central to the latter. Philosophers of science tend to examine critically the ways in which these techniques and procedures are invoked to secure knowledge, how they work, their limitations, and the possibilities they open. Epistemic resources are explored in the particularities of the fields under study. In order to do that, it is not required to have a conceptual analysis of knowledge; it is enough to have an understanding of what is involved in securing, obtaining, and assessing the relevant information in a given domain.

Is a conceptual analysis of knowledge necessary to be able to implement this evaluation? It is not. One can assess how information is obtained and assessed in a particular field independently of taking any stand on the success or failure of the project of conceptually analyzing knowledge. We often use concepts whose conceptual analyses are not available and, in many cases, cannot be available given their indefinability. Consider, for instance, the concept of *identity*. It cannot be defined since any attempt to define this concept ultimately presupposes identity, at least in the meta-language. After all, the sentence expressing identity requires the identity of the variables used to properly characterize the notion. This can be easily noted in the expression of Leibniz identity laws (in second-order logic), $x = y$ if, and only if, $\forall P\,(Px \leftrightarrow Py)$, which requires the *same* variables on both sides of the equivalence connectives (for a discussion and references to the relevant literature, see Bueno 2014 and 2015). Despite this fact, it would be a mistake to suppose that the concept of *identity* is somehow poorly understood and cannot be used in a variety of contexts. There is typically no difficulty in determining whether two objects are the same—unless their identity conditions are not properly specified. But this is not a problem for the concept of identity; instead, it is a difficulty that emerges from the lack of identity conditions for the objects in question.

Similarly, despite not having a conceptual analysis of knowledge, it is possible to determine the conditions under which, in a given field, knowledge is obtained and assessed. There are, of course, proposals in epistemology that argue that one should take the concept of knowledge as primitive rather than try to analyze it in more basic terms. These are knowledge-first approaches (Williamson 2000). In Timothy Williamson's approach, knowledge is then formulated, although not defined, as the most general factive mental state. This account provides an interesting alternative to attempts to define knowledge and it redirects some of the epistemological work away from the goal of identifying more basic concepts in terms of which knowledge should be characterized to examining the epistemological landscape by starting with the concept of knowledge in the first place. Epistemologically, it is a suggestive approach.

It is unclear, however, what implications the approach would have to the philosophical understanding of the sciences. First, it is not obvious that the concept of knowledge is, in fact, presupposed in scientific practice. Whether or not knowledge is presupposed to make assertions, it is safe to maintain that, to the extent that scientific practice is involved at all with knowledge, this involvement is one in which knowledge needs to be established rather than being assumed from the start.

There are, of course, those within epistemology who balk at the claim that science is in the business of producing knowledge. As part of the development of his contextualist account of knowledge, David Lewis notes:

> The serious business of science has to do not with knowledge per se; but rather, with the elimination of possibilities through the evidence of perception, memory, etc., and with the changes that one's belief system would (or might or should) undergo under the impact of such eliminations. (Lewis 1996, p. 234)

This remark seems right (although somewhat one-sided). The elimination of possibilities is indeed an integral part of scientific practice, particularly in the context of obtaining and assessing evidence. In contrast, knowledge per se plays no comparable role. Despite being crucial to epistemological views, knowledge is not what, ultimately, scientific practice is involved with. (Although Lewis doesn't note this, the sciences also open venues for epistemic possibilities—highlighting, in some cases, possibilities that we can only be aware of given the theoretical and experimental practices they enable.)

Despite that, certain philosophical conceptions about the sciences do assign a key role to knowledge. In certain philosophical views (particularly realist ones), knowledge plays a key role. Knowledge (in particular, objective knowledge) may be taken as something science aims to reach in the long run by searching for the truth (Popper 1972). Alternatively, knowledge can be inherently invoked in the characterization of scientific progress (Bird 2007).

Both proposals, however, face difficulties. The notion of truth approximation in the way conceptualized by Popper cannot be implemented because false theories are equally far away from the truth (for a critical discussion, see Miller 1994). As a result, because, on the standard conception, truth is part of objective knowledge, one has no epistemic indication as to how close or far away one is from having such knowledge.

The characterization of progress in terms of knowledge is similarly problematic for it presupposes that knowledge has already been obtained. But how can it be determined that such knowledge has indeed been established in the first place? One could argue that we know more now than we knew centuries ago, and thus, given the intervening scientific progress, not only do we have knowledge, we have increasingly more of it. But this, once again, presupposes that we have the relevant knowledge to begin with, and from an epistemological viewpoint it is questionable whether one is entitled to start with this assumption. There are many possibilities of mistakes and mismatches that can undermine presumed knowledge claims, and unless one is in a position to rule out the relevant ones, one cannot properly have knowledge. As a result, to start by assuming knowledge is more troublesome than it may initially seem.

Note that the concern here is not that skepticism is prima facie correct (although, of course, it may well be). The issue is not that we don't have any knowledge of the world, but rather that particular knowledge claims may be contested because they can be undermined. Some local, restricted form of skepticism may play an important role in scientific practice. But an open-ended, global, Cartesian skepticism, according to which we don't know anything, is not part of that practice. Global skeptical arguments, such as brain-in-a-vat arguments (Nozick 1981), have no purchase in scientific activity and are properly ignored. According to these arguments:

(P_1) If I know *P* (i.e., some perceptually salient fact about the world), I know that I'm not a brain in a vat.
(P_2) I don't know that I'm not a brain in a vat.
Therefore, I don't know *P*.

(P_1) is motivated from the fact that my presumed knowledge of some perceptually salient fact about the world, *P*, undermines my being a brain in a vat because if I were a brain in a vat I wouldn't have the knowledge in question. Thus, if I know *P*, I know that I'm not a brain in a vat. (P_2) is motivated from the fact that my perceptual experiences would be the same even if I were a brain in a vat. As a result, it is unclear how I could have the knowledge that I'm not a brain in a vat. These considerations challenge that I may know anything at all about the world based on my perceptual experiences. However, there is a serious concern about the coherence of the situation entertained in this scenario. After all, the perceptual experiences one has are presumably the result of one's interactions with the world, which are undermined by the brain-in-a-vat scenario.

Rather than engaging with this sort of global skeptical challenge, scientific practice takes for granted that there is a world to be studied, although the details and properties of that world are precisely the kind of issue scientific research aims to elucidate. We will examine, in the next section, some conditions developed in scientific practice to this effect.

In response, Lewis provides an account of knowledge that emphasizes the importance of eliminating possibilities while still granting the difficulty of expressing the proposal. After all, the moment we note that we are properly ignoring certain possibilities, we are *ipso facto* attending—and hence, no longer ignoring—such possibilities. In his unique style, Lewis makes the point in this way:

> *S* knows that *P* iff *P* holds in every possibility left uneliminated by *S*'s evidence—Psst!—except for those possibilities that we are properly ignoring. (Lewis 1996, p. 232)

For Lewis, in providing an account of knowledge, two constraints need to be met. First, knowledge should be *infallible* on pain of not really being knowledge. Second, we should be in a position to assert that we have lots of knowledge; that is, skepticism should turn out to be *false*. In providing his form of contextualism, Lewis is trying to walk the thin line between infallibilism and skepticism. His starting point is Peter Unger's work, which takes infallibilism to be a requirement for knowledge. However, Unger ends up in skepticism, given that infallible knowledge is not possible (see Unger 1975). Lewis shares Unger's infallibility requirement, but tries to avoid skepticism. As Lewis insists:

> If you are a contented fallibilist, I implore you to be honest, be naive, hear it afresh. "He knows, yet he has not eliminated all possibilities of error." Even if you've numbed your ears, doesn't this overt, explicit fallibilism still sound wrong? (Lewis 1996, p. 221)

Given that, on Lewis's account, if you know *P*, then *P* holds in every possibility left uneliminated by your evidence (except for the possibilities you're properly ignoring), then your knowledge is indeed infallible. After all, you have eliminated all possibilities of error, and those possibilities that you haven't eliminated, you can properly ignore.

For Lewis, however, infallibilism doesn't lead to skepticism. Because skepticism is ultimately *false*—well, at least when we are not doing epistemology! In epistemological contexts, when we cannot properly ignore "uneliminated possibilities of error everywhere"

(Lewis 1996, pp. 230–231), ascriptions of knowledge are hardly ever true. That's how epistemology destroys knowledge (Lewis 1996, p. 231). But this is only temporarily:

> The pastime of epistemology does not plunge us forevermore into its special context. We can still do a lot of proper ignoring, a lot of knowing, a lot of true ascribing of knowledge to ourselves and others, the rest of the time. (Lewis 1996, p. 231)

In other words, in nonepistemological contexts, where we can properly ignore various possibilities of error, we do end up knowing a lot of things. In these contexts, it would be a mistake to claim that we lack knowledge. After all, as long as what we claim to know holds in every possibility that is not eliminated by the available evidence (except for the possibilities that we are properly ignoring), the proposed characterization of knowledge is satisfied. As a result, Lewis is entitled to claim that we know; in fact, we know lots of things. In these contexts, skepticism—conceived as the claim that we don't have knowledge—is therefore false, and Lewis achieves his goal.

Does this strategy successfully deal with the skeptic? I don't think it does. In this context, sensitivity to details of scientific practice would have been helpful. In epistemological contexts, as Lewis acknowledges, the game is handed over to the skeptic because we cannot rule out all possibilities of error, and, as a result, we cannot claim to have knowledge on Lewis' account. Similarly, in nonepistemological contexts, the skeptic can still raise the issue as to whether the ignored possibilities are indeed *properly* ignored. And in all the cases in which such possibilities are *not* properly ignored, we still don't have knowledge. But when are possibilities properly ignored?

Lewis introduces three main rules for how possibilities can be properly ignored:

(a) The *Rule of Reliability*: "Consider processes whereby information is transmitted to us: perception, memory, and testimony. These processes are fairly reliable. Within limits, we are entitled to take them for granted. We may properly presuppose that they work without a glitch in the case under consideration. Defeasibly—*very* defeasibly—a possibility in which they fail may properly be ignored" (Lewis 1996, p. 229).

(b) The *Rules of Method*: "We are entitled to presuppose—again, very defeasibly—that a sample is representative; and that the best explanation of our evidence is the true explanation" (Lewis 1996, pp. 292–230).

(c) The *Rule of Conservatism*: "Suppose that those around us normally do ignore certain possibilities, and it is common knowledge that they do. . . . Then—again, very defeasibly!—these generally ignored possibilities may properly be ignored" (Lewis 1996, p. 230).

But these rules are problematic, particularly if they are used in response to skepticism. Employed in this way, they end up begging the question against the skeptic. (a′) With regard to the rule of reliability, perception, memory, and testimony are reliable processes for the most part. However, this doesn't entitle us to suppose that we can ignore the possibilities in which they fail, even defeasibly. The skeptic would insist that, despite the fair reliability of these processes, the rule of reliability doesn't entitle us to ignore the cases in which these processes can be mistaken (i.e., mislead us in significant ways). After all, in all of the cases in which the processes are mistaken, we will fail to have knowledge. Clearly, cases such as these cannot be properly ignored, not even defeasibly.

Part of the difficulty here is to determine under what conditions we can properly ignore the cases in which these information transmission processes fail. The rule of reliability recommends that we uniformly ignore all such cases (although defeasibly so). But this uniform rejection ends up including those cases in which the skeptic is right in insisting that we would be mistaken in trusting these processes of transmission of information. After all, perception, memory, and testimony are not always trustworthy. To ignore the problematic, unsuccessful cases given that, for the most part, these processes work well is clearly an invalid inference. Moreover, the move ends up begging the question against the skeptic since it assumes, without argument, that perception, memory, and testimony are in fact reliable. Whether and under what conditions these processes can be so taken is what the skeptic is challenging. Thus, invoked as a response to skepticism, the rule of reliability is clearly inadequate.

The contextualist strategy is to insist that we can ignore the cases in which perception, memory, and testimony fail. However, to do that, the contextualist simply ignores along the way the challenge raised by the skeptic. Because it just ignores the skeptic, the strategy can't provide an adequate response to skepticism.

(b′) With regard to the rules of method, two points should be made: (i) The skeptic would challenge that one is entitled to presuppose that a sample is representative. In fact, if anything, samples tend to be *un*representative in all sorts of ways. Convenient ways of getting a sample typically are incompatible with a random selection, and the sample size is often not large enough to be representative. Moreover, usual randomization devices, such as a roulette wheel, are not truly random because, strictly speaking, we would need an infinite list of outputs to generate a truly random process. In practice, we end up working with samples that are not randomized and so are not strictly speaking representative. And even if a sample is obtained via some almost randomized method, the way in which we get to particular units in the sample is often not done randomly at all, but rather by invoking some convenient type of method. Suppose that Joe Smith is randomly selected to be part of the sample of a population we are interviewing for a polling study. But when we get to Joe Smith's house, he has just moved out, and the current owner of the place has no idea of Joe's whereabouts. Since we are already there, we just interview the owner instead of Joe. That's convenient, no doubt; but at that point, a truly random sample is left behind. Cases such as this are, of course, ubiquitous, and they bring in additional sources of bias.In other words, as opposed to what the reliability rule states, samples tend *not* to be representative. If you simply get a sample, more likely than not, it will turn out to be unrepresentative. As a result, one needs to work extraordinarily hard to obtain a representative sample. To assume that samples are, for the most part, reliable is to ignore, without argument, the huge difficulties of actually obtaining a sample that is in fact representative. As a result, to use the reliability rule as part of an argument against skepticism is entirely ineffective. The skeptic is entitled to complain that the outcome of the reliability rule (that samples are representative and thus can be trusted) is something that needs to be earned rather than simply assumed.

(ii) Clearly, a skeptic would also challenge that the best explanation of our evidence is the true explanation. There are several arguments as to why inference to the best explanation is not generally reliable. Consider, for instance, the bad lot argument (van Fraassen 1989). Suppose we have various explanations of the same phenomena to choose from. Inference to the best explanation would lead us to conclude that the best explanation is the true one. However, unbeknownst to us, the lot of explanations we were considering was a very bad one, and none of the available explanations, including the best one, was even remotely close to the truth. Clearly, inference to the best explanation is not reliable in this context.

The trouble is that we typically don't know whether, among the lot of available explanations, the true explanation is included. And so the situation we are typically in is one to which the bad lot argument clearly applies. Unless we can rule out the bad lot possibility whenever we infer the truth of the best explanation, it is not clear how such an inference could be reliable.

In response, one could suppose that the true explanation is included in the lot of available explanations. This move is an attempt to ensure the reliability of the inference to the best explanation rule. After all, if the true explanation is in the lot of admissible explanations, by inferring the best explanation one minimizes the chance of inferring something false from true premises.

However, this move still doesn't work. To guarantee reliability, it is not enough to include the true explanation among the lot of available explanations. One needs also to assume that the best explanation is the true one. Suppose, for example, that the true explanation is too complicated, computationally intractable, and extraordinarily difficult to understand. It is unlikely that anything having these features would count as the best explanation, even if it were true. Moreover, if we assume that the true explanation is in the relevant lot of explanations, we end up trivializing the rule. If we already know that a certain lot includes the true explanation, we might just as well infer the true explanation directly!

(c′) With regard to the rule of conservatism, clearly, a skeptic would challenge the idea that one can properly ignore the possibilities that those around us normally do ignore. It is precisely by systematically *violating* this rule that scientific change takes place. Consider the case of splitting the atom in the 1930s. Consider also the case of the moving earth in the context of the formulation of the Copernican theory.

In response, the Lewisian can always note that Lewis was very careful in formulating each rule with a built-in defeasibility condition: "We are entitled to presuppose—again, *very defeasibly*—that a sample is representative . . .." But note that this is, first, a significant concession to the fallibilist! Second, this eventually grants the point to the skeptic. If these possibilities cannot be properly ignored after all, one cannot still claim to know.

As these considerations illustrate, attention to details of scientific practice would have made the difficulties involved in this defense of contextualism in epistemology more transparent.

3 Instruments and Scientific Knowledge

Associated with changes in particular contexts of knowledge assessment we find a phenomenon that has challenged various realist views about the development of science: the incommensurability problem. [1] There are many ways of understanding this problem (see, e.g., Feyerabend 1981; Hoyningen-Huene 1993; Kuhn 1970; Sankey 1994; Siegel 1980 and the references quoted in these works). For the purposes of the present work, I take it to be the problem to the effect that there is no common standard of evaluation to determine the reference of certain theoretical terms. Depending on the context one considers (such as particular domains of application of a theory), there are different ways of determining the reference of such terms. And if that reference is opaque, it is unclear how one could provide realist interpretations of the theories that use these theoretical terms. If there is no fact of the matter as to whether, say, "mass" refers to Newtonian mass or to relativistic mass, it is unclear how one could claim that we should read such theories realistically. It seems that a commitment either way would be just arbitrary. As a result, the incommensurability phenomenon raises problems for different forms of realism about science.

It may be thought that a contextualist approach to knowledge would simply reinforce a relativist view regarding scientific practice, according to which anything goes. But this is not the case. In fact, the contextualist conception will grant that incommensurability will inevitably emerge, but it will resist any relativist consequence. Central to this move, I argue, is a particular understanding of the functions that instruments play in scientific practice and the epistemic features they share with observation.

But can instruments be used to overcome the problems that emerge from incommensurability? In principle, it might be thought that the answer should be negative. After all, the argument goes, the use of instruments relies on various theoretical assumptions, and the latter will just carry over the incommensurability found at the level of theories.

[1] In this section, I rework some parts of Bueno (2012). But here I examine, in particular, the significance of epistemological considerations for the proper understanding of scientific instruments.

In other words, since theories are routinely invoked in the construction and use of scientific instruments, it would not be possible to use the latter to resist the effects of incommensurability. After all, these effects will emerge just as vividly in the theories that underlie the instruments as in the theories that inform the relevant parts of scientific practice. Bringing in the instruments will be of no help.

This line of argument, however, can be resisted in multiple ways. First, it is quite controversial to claim that theories need to be adopted in the construction and use of scientific instruments. In several instances, some theories are indeed used in the *construction* of instruments, but these theories turn out to be false—and the instruments work just as adequately. This is the case, for instance, in the use of Kepler's optics in the construction of the first microscopes in the seventeenth century. Despite the falsity of Kepler's theory, the telescope ended up becoming a major tool for the study of astronomical phenomena (for a provocative discussion, see Feyerabend 1974). With the falsity of the theories used in the construction of the instruments, one cannot claim that the problem of incommensurability seriously threatens these theories. After all, the fact that the theories in question weren't even (taken to be) true indicates a less than full commitment to them. In the presence of better alternatives, one may just simply rule out these theories.

Second, it is quite controversial to claim that theories need to be invoked in the *use* of instruments (see Hacking 1983). Although some theories may need to be employed in the case of sophisticated use of certain instruments such as electron microscopes, this need not be the case for every instrument in every context. There's a relative autonomy of the instruments with respect to theories (see Hacking 1983, again, as well as Azzouni 2000; Baird 2004; Humphreys 2004). And some optical microscopes clearly fit into this category.

It is more difficult, however, to make this case for complex tools such as electron microscopes. After all, to obtain particularly sophisticated results in electron microscopy, not only does one need to know in great detail how such instruments work, but one also needs to master a great deal about the quantum mechanics that goes into the instrument's construction. In these cases, can we say that the incommensurability issues that emerge in the context of quantum mechanics will also carry over to the use of an electron microscope? Yes, we can. But the incommensurability won't pose a problem here—at least not for the empiricist. After all, the use of these instruments is ultimately based on a robust group of observational practices, and these practices are invariant under theory transformations. What are these practices? (Observational practices are linked to Azzouni's gross regularities [Azzouni 2000]. However, they are importantly different from the latter because observational practices are restricted to observations in a way that gross regularities need not be.)

Observational practices are certain procedures that are ultimately grounded on observation. Observation, of course, is not a static, passive process of simply opening one's eyes (Hacking 1983). It is rather a much more active process of interaction with different, observable aspects of the world, one that relies on all our sense modalities and explores the triangulation provided by the interconnection between them. Processes of this kind involve the following epistemic features (which are freely based on and adapted from Azzouni 2004 and Lewis 1980):

(i) *Counterfactual dependence*: These are the two key conditions from which the remaining three listed below follow. (a) Had the scene before our eyes been different, the image it yields would have been correspondingly different (within the sensitivity range of our senses). (b) Had the scene before our eyes been the same, the resulting image would have been correspondingly the same (again, within our senses' sensitivity range). In other words, the images that we form are sensitive to the scenes before us. The particular form this sensitivity takes is expressed in terms of the robustness, refinement, and tracking conditions that are entailed by counterfactual dependence.

(ii) *Robustness*: The scene before our eyes produces an image independently of us. There are two related senses of "independence" here: (a) Given the sensory faculties we have, the image that will be produced is not of our own making. Unless we are hallucinating, we won't see a pink elephant in front of us if none is there (counterfactual dependence condition (i)(a)). (b) What we see doesn't depend on our beliefs regarding the scene before us (the two counterfactual dependence conditions apply independently of our beliefs about the objects we experience). Our beliefs may change our *interpretation* (our understanding) of what we see, though. Someone who doesn't have the concept of a *soccer ball* won't see a ball *as* a soccer ball, even though the person will see the round object. It is the latter sense that is relevant here.

(iii) *Refinement*: We can refine the image of what we see, say, by getting closer for a better look. As we approach the target object, we compare the resulting images of the object with the previous ones (obtained when the object was seen further away). The counterfactual dependence conditions remain invariant as we move closer to the object, thus enabling the comparison. This improvement in the image allows us to discriminate better what is seen—even though there are well-known physical limits to how far the discrimination can go.

(iv) *Tracking*: Two forms of tracking are enabled by the counterfactual dependence condition: given the sensitivity of our perceptual apparatus to the environment around us, we can track objects in *space* and in *time*. We can move around an object to detect differences in it (spatial tracking). We can also stay put and observe changes in a given object over time (temporal tracking). In both cases, the differences in the resulting images, allowed for by the invariance in the counterfactual dependence conditions, are contrasted and compared. As a result, we are able to track the scene before our eyes in space and time.

Based on these four conditions (or on conditions of this sort), observational practices are elaborated. These practices explore the significant epistemic features of observation.

With *counterfactual dependence*, we have the sort of sensitivity to the scene before our eyes that we would expect in the case of good detection systems. Changes in the scene correspond to changes in our observations. In this way, what we observe is sensitive and responsive to the environment.

With *robustness*, we obtain the sort of independence that is significant for the objectivity of observation. It is not up to us what we will observe when we open our eyes even though our observations depend on concepts that we invoke to interpret, categorize, and understand what we see.

With *refinement*, we can improve our observations. From an epistemic point of view, this increases our trust in what we observe. This doesn't mean, of course, that observations are always reliable. They aren't, and we learn when to trust the results of observation and when not to. Since this is accomplished by using our own senses (what else could we use?), refinement clearly plays an important role in this process. After all, refinement allows us to determine in more fine-grained ways how the images we form depend on our position with respect to the scene before our eyes.

Tracking finesses the counterfactual dependence condition, and it spells out the particular type of correlation that is established between the scene before our eyes and what we observe. The sensitivity that tracking establishes is to both the presence of certain details in the scene before our eyes and to the absence of other details there.

Observational practices are grounded on these four epistemic conditions, and they explore the latter to guarantee reliable results. These practices ground theoretical and experimental activities in scientific practice. The central idea, briefly, is that the observational features of our experience are grounded on observational practices, and these, in turn, are invariant under theoretical reinterpretations. As a result, the presence of incommensurable theories won't affect them. This is because, although observation is undeniably theory laden, it has certain aspects that are preserved even when theories change. These robust features of observation emerge from the four epistemic features discussed earlier. And reliable instruments, it is not difficult to see, exemplify these features. After all, the same support that the four epistemic conditions offer to observation extends very naturally to certain instruments and the stability of the results the latter offer.

Now, the presence of incommensurability challenges the realist understanding of scientific results because it calls into question the existence of common standards to assess the adequacy of rival theories. That is, with incommensurability in place, it is not clear that there are standards that do not favor one theory in contrast with another. This questions the realist's ability to choose between rival theories without assuming the standards that favor one of them (e.g., by implicitly assuming the framework of one of the theories). Since the realist needs to decide if one of theses theories (if any) is true (or approximately true), he or she needs to be in a position to provide noncircular grounds to prefer one theory over the other.

But are we inevitably led to embrace relativism if we accept the incommensurability thesis? As will become clear shortly, I don't think so. The proposal advanced here supports the incommensurability thesis but resists the move to relativism. There may not be common standards in theory evaluation, but we can't conclude that anything goes—that any theory is just as good as any other. Observational regularities, even those based on some instruments, would be preserved—they are quasi-true and thus preserved through theory change. And we can choose among rival theories based on their consistency with these observational regularities. Although this criterion will not uniquely single out one theory (which is not a problem, of course, for the anti-realist), it rules out some theories. As a result, we have theoretical pluralism without relativism.

How exactly do instruments get into the picture? First, they are devised in order to extract observational regularities from things that are unobservable. And by preserving the observational regularities, one can choose between different theories (those that accommodate such regularities), even though the results may have different interpretations in different theories.

Second, by invoking the four epistemic conditions, we can neutralize the familiar troubles posed by incommensurability. After all, the robustness of the access to the sample that is provided by the relevant instruments prevents conceptual changes from affecting that access. Even though theories may be needed to interpret certain results from microscopes, the practice with these instruments doesn't change with the change of theories. Priority is still given to the rich texture of data obtained from the instruments.

Note, however, that the results that the instruments generate don't change with changes in the relevant theories. As will become clear, this is the outcome of the four epistemic conditions mentioned earlier. The conditions establish a strong connection between the sample and the instruments. And this connection, as we'll see, is not theory-dependent—even though theories are involved at various points in the construction and use of the instruments in question. Hence, the type of the detection of the objects in the sample is invariant under transformations of the theories that are used.

But how can instruments provide a common ground—and a common standard—in theory evaluation? How exactly can instruments be used to avoid the relativistic consequences of the incommensurability thesis? The proposal here, I insist, is not to *undermine* the latter thesis. It is not even clear how exactly that could be done. After all, in theory evaluation, one does end up invoking some standard or other in order to judge the adequacy of the theories involved. And, typically, the standards that directly support the theory one already favors would be invoked in defense of that theory. But it doesn't follow from this that there are no objective standards of theory assessment. Different scientific theories are usually expected to conform to the evidence. And, even if such theories may interpret the evidence differently, the results conveyed by that evidence are stable and systematic enough that the theories couldn't simply ignore them. The results—typically produced by various kinds of scientific instruments—provide stable, theory-neutral grounds for theory assessment.

The results generated by scientific instruments, I noted, are relatively theory-neutral in the sense that relatively few theories are needed to interpret them. Usually, only those theories that were used to construct the instruments are invoked in interpreting the results obtained with these instruments. Additional theories are not required—practice with the images in question and with the instruments are central here, though. This means that instruments have a relative independence from theories, which means that the results that they offer can be used to adjudicate between rival theories without favoring any one of the theories in particular.

Let me elaborate on this point. Instruments can provide a common ground between rival theories in their evaluation process for the following reasons:

(1) As just noted, instruments are relatively theory-independent, and thus no theory is favorably supported by an instrument alone. This condition is sufficient to guarantee that instruments yield a common ground in theory assessment. As we saw, the reason why instruments are only relatively rather than absolutely theory-independent is because one needs theories to construct sophisticated instruments, and it is often crucial to invoke suitable theories to use the instruments to their maximum limit.

For instance, without knowledge of the tunneling effect in quantum mechanics, one cannot construct—let alone imagine the construction of—a scanning tunneling microscope (STM). This doesn't mean, of course, that knowledge of this effect is sufficient for the construction of that instrument. Clearly, it isn't: the tunneling effect was known since the 1920s, but the STM wasn't actually built until the 1980s. In addition to theoretical knowledge, a considerable dose of engineering and practical skills is required. Moreover, to use an STM effectively and obtain detailed results about the surface structure of the sample, it is crucial that one knows not only the way the instrument operates, but also the theories that are relevant in bringing about the phenomena under study—particularly, quantum mechanics. Once again, knowledge of the *relevant* theories is not sufficient to guarantee successful results given that it is central that one has the required skills to obtain the results. And that is certainly *not* simply a matter of having theoretical knowledge (see Baird 2004; Hacking 1983; Humphreys 2004).

But if theories are involved in the construction process and in the use of certain instruments, isn't the epistemic status of these instruments necessarily compromised? After all, it might be argued, because the instruments rely on these theories, the former can't yield results that conflict with the latter. I don't think this is correct, though. First, I mentioned earlier that knowledge of the *relevant* theories is involved in the successful use of certain instruments. Now, what are the relevant theories? This is obviously a context-dependent matter. Usually, the relevant theories are those that are explicitly invoked by those who have constructed and by those who regularly use the instruments in question. But reference to these theories won't settle the matter. After all, one can successfully construct an instrument using a false theory. This was actually the case with the construction of the first telescopes in the seventeenth century based on Kepler's optics. The use of that theory was clearly relevant. However, one can't claim that, from an epistemological point of view, the theory is responsible for the results of the instrument. In the end, what matters is how reliable, reproducible, and stable are the results generated by the instruments. The epistemological status of the theories used won't affect that.

Second, this feature also highlights another sense in which instruments are only relatively theory-independent: although in sophisticated cases theories are used in the construction of instruments, these theories do not support the instruments in question.

In fact, the theories sometimes are not even consistent—as the case of Kepler's optics illustrates.

(2) Instruments can provide a common ground in theory assessment for a second reason. Instruments navigate across theoretical boundaries without disturbing the theories involved. Consider, for instance, an electron microscope (whether a transmission or a scanning electron microscope). This type of tool is regularly used in a variety of fields, ranging from chemistry through molecular biology to physics, and so it needs to bridge the theoretical gap between quite different disciplines. In practice, the instrument is simply used in the way it needs to be used in each field. Not surprisingly, different practitioners use the instrument in different ways depending on their specific research needs. And that is accomplished independently of the quite dramatic differences between the various theories invoked in each case. Again, this doesn't mean that theories play no role in the practice with this instrument. It just means that different users will engage with different theories depending on the application at hand. As a result, also in this sense, instruments yield a common ground in theory evaluation.

(3) Moreover, the results obtained from the instruments are robust (in the sense discussed earlier with the strong epistemic conditions of access they provide to the sample). When working correctly, instruments systematically track the stuff in the sample. This will generate suitable, robust data to adjudicate between rival theories. (Of course, different theories may be used to interpret the data differently. But the body of data itself can be used to assess the theories in question.)

Now, since instruments provide such a common ground, can they also be used as a common standard to adjudicate between rival theories? I think they can. Being relatively theory-independent and producing robust results, instruments can be used to assess the adequacy of scientific theories. Moreover, instruments are often used to yield novel results that, in turn, will require explanation; in terms of the adequacy of the latter, one can choose among competing theories.

For these reasons, we can rule out relativism even acknowledging the presence of incommensurability. Instruments—at least good ones—provide strong epistemic access and are relatively theory-neutral. The fact that the results from instruments track the evidence (in particular, relevant features of the sample) provides grounds to choose a theory that entails that piece of evidence.

Someone may resist this move. After all, the argument goes, to use instruments and their results in the adjudication between rival theories, one has to adopt a methodological standard to the effect that if there is an inconsistency between the results generated by certain instruments and the consequences derived from relevant theories, we need to revise something in the resulting package. The problem here is that, for instruments to play such a role, their results need to be stable over radical theory change. It turns out, however, that the meaning of the notions that are used to characterize and describe the results of instruments changes with shifts in the theories one considers. As a result, incommensurability applies to the notions that are invoked in the description of the results of instruments. The result is therefore clear: relativism.

In response, we can simply block the last step in the argument. Even if we grant the presence of incommensurability in this context, as we should, this is not enough to entail relativism. The nature of the access provided by the instruments is central for that. Consider the *robustness* condition: the results we obtain from an instrument are not dependent on our beliefs about the sample under study. Whatever we believe about what might be going on in the sample is irrelevant to what is actually happening. The interaction between the sample and the instrument—once the latter is suitably calibrated and the former is properly prepared—is a process that depends on the instrument and the sample. It is in this sense that the robustness condition is an independence condition.

Now, when we *interpret* the results of an instrument, our beliefs about what might be happening in the sample are obviously relevant and so are the theories we may invoke in making sense of the data. But the process of interpretation of experimental results is a very different process from the one in which these results are obtained. This doesn't mean that no interpretation or no theorizing goes on when experimental results are being obtained and constructed. For example, the statistical manipulation of data is clearly in place, and it constitutes an extremely significant form of theorizing in the construction of experimental results. However, before any such statistical techniques can be invoked, it is crucial that raw data are generated. The robustness of each particular piece of datum is central.

And this robustness is a crucial feature of the strategy of resisting relativism. The robustness is another way of supporting the relative theory-independence of the instrument's results. Moreover, the theories involved in the actual construction of the data, being statistical in character, are not contentious in the context of assessing rival *physical* theories. After all, such statistical theories can be applied only if proper application conditions are satisfied. But as long as these theories are correctly applied, rival physical theories can be discriminated even if the relevant statistical theories may be different for the different physical theories. In this way, statistical theories also provide a common ground between rival physical hypotheses. Clearly, this sort of common ground is not strong enough to overcome the incommensurability between the theories in question because the standards are not general enough to effectively undo the effects of incommensurability. However, the presence of theories in the construction of experimental results doesn't undermine the robustness of such results, which can then be used to assess between rival theories. As a result, relativism doesn't emerge here.

Consider, also, the *counterfactual dependence* conditions: had the sample been different (within the sensitivity range of the instrument), the image produced would have been correspondingly different; had the sample been the same (within the instrument's sensitivity range), the image produced would have been correspondingly the same. This establishes a clear dependence between the sample and the images that are generated. Clearly, there might be a variety of different theories involved in the description of the particular form of interaction between the sample and the instrument. And, as an outcome of the presence of these theories, incommensurability may emerge. However, the counterfactual dependence between the sample and the instrument prevents us from getting relativistic consequences from the incommensurability. After all, the relation between the sample and the instrument holds independently of the theories involved. It is a matter of the relation between the two (sample and instrument) alone.

A similar point emerges when we consider the third epistemic feature of scientific instruments: the *refinement* condition. In a series of instruments, it is possible to improve the resolution of the images that are generated. This indicates that the access to the sample provided by each instrument, although robust, can yield further information about the sample in the end. Once again, in the process of improving the instruments, in addition to careful practice, theories can play a significant role. These theories can suggest new avenues to explore the interactions between the instrument and the sample. They can also suggest new mechanisms that might be active in the sample, which in turn may indicate new possible forms of intervening on the latter. Obviously, theories are not sufficient to achieve these results because systematic engineering techniques and, sometimes, better instruments are also needed.

Given the presence of various sorts of theories throughout the process of refinement, it might be thought that the incommensurability involved will prevent instruments from being used effectively in theory selection. But this is not the case. After all, the theories employed in the development of the instruments need not be involved in the actual implementation of the latter. Moreover, once the instruments are constructed, the results that are obtained are largely independent from the theories in question. And with the refinement of the instruments, more fine-grained results can be generated. In this way, even more detailed ways of testing theories and yielding novel effects can be produced. As a result, once again, incommensurability won't generate relativistic conclusions. Because the results are largely independent of the theories, the incommensurability won't raise a problem for the objectivity of the results.

Finally, consider the *tracking* condition, which, given the counterfactual dependence conditions, specifies the particular form of the dependence between the instrument and the sample: the instrument is sensitive to the sample and tracks its main features—spatially and, in some cases, temporally. Now the tracking relation between the instrument and the sample depends ultimately on what goes on between the two of them. Thus, the fact that some theories are invoked in the construction and use of certain instruments doesn't affect the outcome of the instrument. The images that the latter yield are sensitive to what is going on—to the best of our knowledge—in the sample. Of course, since we may not *know* (or may not be justified in believing) that the counterfactual (and, hence, the tracking) conditions are actually satisfied in the case of certain events, we cannot know for sure what is really going on in the sample. But *if* the conditions are met, then the instrument is indeed sensitive to the presence of the sample and the details it has. In other words, the fact that theories are involved doesn't affect the counterfactual dependence (or the tracking). As a result, due to the presence of various theories, we may well have incommensurability. But, once again, for the reasons just discussed, this won't yield relativism.

Note that, at this point, being able to know whether the counterfactual conditions actually apply enters the considerations. An externalist about instruments—in analogy with an externalist in the theory of knowledge (BonJour and Sosa 2003)—insists that as long as the counterfactual and the tracking conditions are met, instruments will be reliable. There's no need to know (or be justified in believing) that these conditions are indeed satisfied. The reliability of scientific instruments depends only on their properties and the relations they bear to the environment. Our epistemic access to the relevant epistemic conditions is irrelevant. In contrast, an internalist about instruments—again, in analogy with the epistemological internalist—argues that the satisfaction of the counterfactual conditions is not enough: one also needs to know (or, at least, be justified in believing) that these conditions are indeed met. Otherwise, we will be unable to know whether the instrument in question is indeed reliable or not. After all, suppose we don't know (or have reason to believe) that the counterfactual conditions are indeed satisfied, and suppose that these conditions fail; we may then take an image produced by an instrument as depicting a feature that the sample actually lacks or may fail to identify a feature of the sample because no such a corresponding feature is depicted in the image. Admittedly, if the counterfactual conditions are met, we shouldn't be prone to these mistakes. But unless we are able to know (or be justified in believing) that the conditions are in place, we can't rule these errors out.

These considerations indicate the significance that internalist and externalist considerations play in the proper formulation of suitable epistemic conditions for scientific instruments. By engaging with this aspect of epistemological theorizing, philosophers of science can be provided a better account of the epistemology of instruments, which produce some of the most significant pieces of knowledge in scientific practice.

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References

Azzouni, J. (2000). *Knowledge and Reference in Empirical Science* (London: Routledge).
Azzouni, J. (2004). *Deflating Existential Consequence* (New York: Oxford University Press).
Baird, D. (2004). *Thing Knowledge: A Philosophy of Scientific Instruments* (Berkeley: University of California Press).
Bird, A. (2007). "What Is Scientific Progress?" *Noûs* 41: 92–117.
BonJour, L., and Sosa, E. (2003). *Epistemic Justification: Internalism vs. Externalism, Foundations vs. Virtues* (Oxford: Blackwell).

Bueno, O. (2012). “Inconmensurabilidad y Dominios de Aplicacíon.” In Lorenzano and Nudler (eds.) (2012), 27–65.

Bueno, O. (2014). “Why Identity Is Fundamental.” *American Philosophical Quarterly* 51: 325–332.

Bueno, O. (2015). “Can Identity Be Relativized?” In Koslow and Buchsbaum (eds.), 2015, 253–262.

DeRose, K., and Warfield, T. (eds.) (1999): *Skepticism: A Contemporary Reader* (New York: Oxford University Press).

Feyerabend, P. (1974). *Against Method* (London: New Left Books).

Feyerabend, P. (1981). *Realism, Rationalism and Scientific Method*. Philosophical Papers, Vol. 1. (Cambridge: Cambridge University Press).

Gettier, E. (1963). “Is Justified True Belief Knowledge?” *Analysis* 26: 144–146.

Goldman, A. (1967). “A Causal Theory of Knowing.” *Journal of Philosophy* 64: 355–372.

Hacking, I. (1983). *Representing and Intervening* (Cambridge: Cambridge University Press).

Hoyningen-Huene, P. (1993). *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science*. A. Levin (trans.) (Chicago: University of Chicago Press).

Humphreys, P. (2004). *Extending Ourselves: Computational Science, Empiricism, and Scientific Method* (New York: Oxford University Press).

Koslow, A., and Buchsbaum, A. (eds.). (2015). *The Road to Universal Logic*, Vol. II (Dordrecht: Birkhäuser).

Kuhn, T. (1970). *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press).

Lewis, D. (1980). “Veridical Hallucination and Prosthetic Vision.” *Australasian Journal of Philosophy* 58: 239–249. (Reprinted, with a postscript, in Lewis, 1986, 273–290.)

Lewis, D. (1986). *Philosophical Papers*, Vol. II (Oxford: Oxford University Press).

Lewis, D. (1996). “Elusive Knowledge.” *Australasian Journal of Philosophy* 74: 549–567. (Reprinted in DeRose and Warfield [eds.] 1999, 220–239.)

Littlejohn, C. (2012). *Justification and the Truth-Connection* (Cambridge: Cambridge University Press).

Lorenzano, P., and Nudler, O. (eds.). (2012). *El Camino desde Kuhn: La Inconmensurabilidad Hoy* (Madrid: Editorial Biblioteca Nueva).

Miller, D. (1994). *Critical Rationalism: A Restatement and Defence* (La Salle: Open Court).

Nozick, R. (1981). *Philosophical Explanations* (Cambridge: Harvard University Press).

Popper, K. (1972). *Objective Knowledge* (Oxford: Clarendon Press).

Sankey, H. (1994). *The Incommensurability Thesis* (Aldershot: Avebury).

Siegel, H. (1980). “Objectivity, Rationality, Incommensurability, and More.” *British Journal for the Philosophy of Science* 31: 359–384.

Unger, P. (1975). *Ignorance: A Case for Scepticism* (Oxford: Oxford University Press).

van Fraassen, B. (1989). *Laws and Symmetry* (Oxford: Clarendon Press).

Williamson, T. (2000). *Knowledge and Its Limits* (Oxford: Oxford University Press).

Zagzebski, L. (1994). “The Inescapability of Gettier Problems.” *Philosophical Quarterly* 44: 65–73.