Understanding the success of science[*]

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Abstract

This chapter sketches a new defense of scientific realism based on understanding the success of science and then considers what features understanding must have for this defense to succeed. It argues that if scientific realism involves knowledge of unobservables, then the relevant state of understanding some phenomenon must involve grasping that the phenomenon occurs independently of the scientist's actions or community. The chapter concludes by arguing that both Giere and Potochnik are unable to provide this type of defense of scientific realism.

I. Introduction

This chapter sketches a new defense of scientific realism based on understanding the success of science (sections I and II) and then considers what features understanding must have for this defense to succeed (sections III and IV). There are thus at least four contentious terms that are central to my discussion: scientific realism, its defense, scientific understanding, and scientific success. To make the discussion tractable, I will simply assume that someone is a scientific realist just in case they believe that they know of various unobservable entities and some of their characteristics, and that this knowledge is based on scientific investigations. In this chapter I illustrate the scientific realist position using an example from electrostatics. Some of the entities that are central to this example are objects with positive and negative electric charges. These objects cannot be observed, and yet the scientific realist claims to know of the

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existence of these charged objects, and also some aspects of their interactions. For example, like charges repel one another and opposite charges attract one another via a force that is determined by Coulomb's law: $F = k (q_1q_2)/r^2$. Here the q_i are the magnitudes of charges, r is the distance between them and k is a constant fixed by the units.

One way that scientific knowledge is different from ordinary knowledge is that scientific knowledge arises by the deliberate activities of reflective agents. In an ordinary case, I may come to know where I left my glasses through a haphazard process of searching. This process ends when I perceive, and thereby come to know, that I left my glasses in the kitchen. When pressed to defend my claim to know that I left my glasses in the kitchen, I may not have much to say. Scientific knowledge, by contrast, aims to be much more articulate. An agent plans and carefully conducts an experiment so that the results can be communicated to other agents. One goal of this activity is to make whatever knowledge that arises from this experiment for that agent into sharable knowledge that is available to others. In our case, we will sketch an experiment that provides the experimenter with knowledge of the existence and character of unobservable charged objects. When this experimenter communicates the results of this experiment, other agents may also have this knowledge. For this process to function, the experimenter must be able to defend their claim to know. This defense will involve much more than just asserting what they suppose to be the case. In addition, they must clarify how the experiment was conducted and how the experimental results bear on the existence and character of these unobservable objects. A defense of a claim to scientific knowledge requires responding to the sorts of doubts that other scientists are likely to raise. If such responses are not forthcoming, then the experimenter's claims to know will be rejected by their scientific peers.

In section II I will sketch a defense of scientific realism that is inspired by this typical scientific procedure. I adopt a broadly naturalistic methodology that insists that philosophical arguments should resemble our best available scientific arguments and that philosophical doubts should be based on recognizably scientific considerations. This methodology constrains how one should conceive of a philosophical defense of scientific realism. The correctness of scientific realism requires knowledge of unobservable entities, and so a defense of scientific realism will involve a defense of some knowledge of unobservable entities. This defense takes the form of an argument whose conclusion is that there is knowledge of unobservable entities (by scientific means). The premises of this argument will provide reasons to accept the conclusion. These reasons are not a guarantee that the conclusion is true, and the premises will themselves be uncertain. The defense of scientific realism will be a good one, though, if it passes the level of scrutiny that is appropriately imposed on a novel scientific claim. This requires that the defense of scientific realism be capable of persuading a neutral party, but not that the defense persuade a resolute skeptic or anti-realist.

The defense of scientific realism that I develop here involves scientific understanding. Scientific understanding is a distinctive kind of cognitive achievement. Here I will focus on so-called objectual understanding: when an agent has objectual understanding, they stand in a demanding kind of cognitive relation to some object. One kind of object that is central to this defense is phenomena. A phenomenon is a repeatable type of event, state or process (Bogen & Woodward 1988). Our experiment concerns a phenomenon known as electrostatic induction.

The phenomenon of electrostatic induction occurs whenever a charged body is placed next to a conductor, and this placement brings about a new electrostatic force of attraction between the charged body and the conductor. As I will explain in section II, the core of my defense of scientific realism is an inference from an objectual understanding of a phenomenon (such as electrostatic induction) to knowledge of the existence and character of some unobservable objects (such as charged particles). This inference is legitimate to the extent that an understanding of a phenomenon involves knowledge of the existence and character of some unobservable entities. A successful defense of scientific realism may then proceed by making it plausible to a neutral party that this sort of understanding exists.

It remains to sketch the kind of evidence that can be offered in favor of the claim that an agent has achieved this sort of scientific understanding. Scientific realists typically emphasize the success of science, but exactly what counts as a success adequate for realism is contentious. Here I will focus on control of the phenomenon of interest, e.g. electrostatic induction. In an experiment, an agent may be able to create and manipulate some phenomenon. When this creation and manipulation provides the proper kind of understanding of the phenomenon, then the agent may rightly claim to know of the existence and character of the unobservable entities involved in this creation and manipulation. The aim of section II is to spell out this process.

II. Recasting the No Miracles Argument

The most well-known defense of scientific realism is the no miracles argument. Putnam is often credited with its most compact formulation: “The positive argument for realism is that it is the only philosophy that does not make the success of science a miracle” (given at Psillos

1999, 71). Putnam's formulation suggests a single, global argument whose conclusion is a claim about our best, current science as a whole. But more recently scientific realists have formulated a more generic argument that is apt to be applied in a series of limited, local cases. I take Psillos' case for selective scientific realism to be the most compelling (Psillos 1999). Psillos restricts his focus to cases where some theory-laden investigation is able to generate a novel, predictive success. In some of these cases, the very theories that informed that investigation also offer potential explanations for why that prediction occurs by appeal to mechanisms involving unobservable entities. If those potential explanations make essential use of these unobservable entities, then that is often sufficient evidence to conclude that those entities exist and have the characteristics needed for that explanation to go through: "The best explanation of the instrumental reliability of scientific methodology is that: the theoretical statements which assert the specific causal connections or mechanisms by virtue of which scientific methods yield successful predictions are approximately true" (Psillos 1999, 78). The selective scientific realist will deploy this same generic argument when the right kind of predictive success is achieved, and thereby make the case for a limited range of knowledge of unobservable entities.

As Psillos has sometimes emphasized, one inspiration for this sort of defense of scientific realism is what Feigl called "the Copernican turn" (Feigl 1950, 41, given at Psillos 2011, 308). The Copernican turn requires a scientist to take whatever evidence they have assembled in favor of some theoretical claims, and to use their theories to explain that evidence and the scientist's knowledge of that evidence. Feigl illustrates this aim with an example from electrostatics: "The divergence of the goldleaves of the electroscope which epistemically serves as an indicator of the presence of charges is immediately deducible from the theoretical assumptions of electrostatics, i.e. primarily from the Coulomb law of attraction and repulsion" (Feigl 1950, 40). Feigl's discussion makes clear that the point of this deduction of the evidence is to explain the evidence along with the scientist's knowledge of the evidence: "If knowledge (as behavior) is not to remain an utter mystery or miracle, it is clear that the knowing organism itself must find a place in the world it knows. Whatever object can be reached by empirical knowledge must, no matter how indirectly, be related, (yes, causally related) with the processes in the knowing organism" (1950, 41). What is initially simply a report on one's observations is then treated, as it were, "cosmologically" by embedding what is observed in a causally integrated nexus that includes not only what is responsible for the objects observed, but also the scientists themselves and their actions. Through this embedding, indexical terms like "I", "here" and "now" are eliminated using terms that place the observations in an objective time and place: "The epistemic uniqueness of the base corresponds only to an objective specificity and focal character of a spatio-temporal region in the cosmological account" (1950, 41). For example, in the electrostatics case, the behavior of the electroscope is traced to the presence or absence of charged particles via Coulomb's law. In addition, the role of the scientist in building and manipulating this experimental setup is used to explain how the scientist knows what they do by indicating the agent-independent causal relationships.

Feigl's case of the experimental creation and manipulation of electrostatic induction fits perfectly into Psillos' generic argument for selective scientific realism. The experimenter builds their experimental apparatus using their electromagnetic theory, drawing in particular on Coulomb's law for electrostatic forces. The agent models the metals in this apparatus as conductors, with charged particles that are free to move on the surface of these metals in response to charged bodies being placed near the apparatus.[1] When the gold leaves move as the agent expects using their theory, the agent acquires evidence that Coulomb's law is correct, and that the hypothesized charged particles really do exist, even though they are too small to be observed. So, in such circumstances, the best explanation of the experimental manipulation of the gold leaves is that these theoretical claims are true, and that the mechanisms in question really are operating to produce the experimental effects.

Recall that my goal is to reformulate this kind of defense of scientific realism in terms of understanding. Through adopting their electrodynamic theory, the agent acquires an understanding of electrostatic induction. More specifically, the agent understands electrostatic induction as that phenomenon is being created and manipulated in their experiment. For this understanding to inform a defense of scientific realism, it must have implications for knowledge. What about the agent's understanding of electrostatic induction indicates that the agent also knows of the existence and character of unobservable charged particles? As I have reconstructed the case, the agent believes their theory, and deploys their belief in these charged particles when they build and manipulate their experimental apparatus. The resulting success of these manipulations is the evidence that these beliefs are true. So in carrying out the experiment, and coming to understand this phenomenon, the agent also acquires evidence that some of their beliefs are true. This means that the experiment can afford knowledge of such unobservable entities.

[1] See, e.g., Griffiths 2013, sections 2.5.2, 3.2.2.

One aspect of this process is crucial, although it is somewhat in the background of Psillos' discussion of his argument. However, this aspect seems to be the motivation for Feigl's talk of a "Copernican turn". Copernicus succeeded in understanding the trajectories of the planets by recognizing that the earth was a planet. This led him to embed his earth-based observations in a more cosmological coordinate system centered on the sun. The earth's own motion was thus a factor in the measurements that Copernicus had available, but this factor could be eliminated, and a more objective representation of the situation was thereby achieved. The same "cosmological" aim is essential to any instance of this sort of defense of selective scientific realism. The phenomenon in question and one's evidence that one is manipulating it successfully needs to be characterized in objective terms that dispense with indexical terms or other contextual restrictions. In putting forward a claim to know of the existence and character of these unobservable charged particles, the agent is claiming that this phenomenon is due to these charges quite generally. The claim to know is not qualified or restricted: everywhere and always, electrostatic induction arises due to the causal factors set out in the electrodynamic theory. The agent is not claiming just that this is what is going on in this experimental context or for this scientific community.

For this kind of knowledge of the objective world to result from an experimental success, the evidence acquired through the experiment must be made correspondingly objective or cosmological. To start, we can imagine an agent who conducts their experiment and records their personal observations in their "protocols". These observations only tell them what has happened here and now to them. But their theoretical understanding of their actions and the experiment indicates to the agent how these personal observations can be transformed into evidence of the character of some objective phenomenon. Their understanding of the phenomenon, then, must incorporate a grasp of the independence of that phenomenon from the particular circumstances of its creation in that experiment. By grasping that independence, the agent is thereby in a position to carry out the kind of objective rendering of what they have found. If this independence is not grasped, then the agent is not a position to know that some unobservable entities exist and are responsible for the phenomenon they have manipulated.

Here, then, is my proposed generic argument for a defense of selective scientific realism based on the special sort of scientific understanding just surveyed:

(1) An agent conducts an experiment that successfully creates and manipulates an instance of some phenomenon, drawing in part on their theoretical beliefs concerning this phenomenon.

(2) The agent grasps that the phenomenon in question obtains *independently* of their actions and scientific community.

(3) The agent grasps that the observable features of this phenomenon *depend on* the existence and character of some unobservable entities that are posited by their theoretical beliefs.

(4) The agent has a good understanding of the phenomenon.

(5) Therefore, the agent knows of the existence and character of some unobservable entities.

Premise (1) is the basis for premise (2), and both premise (1) and premise (2) are the basis for premise (3). The good understanding required for premise (4) must incorporate at least the grasp of the propositions given in premises (2) and (3). When all these conditions are met, it is highly likely that (5) obtains.

One recent account of objectual understanding can inform this defense of scientific realism. In "Beyond Explanation: Understanding as Dependency Modelling" Dellsén defends what he calls a "dependency modelling account of understanding" according to which

> DMA: S understands a phenomenon, P, if and only if S grasps a sufficiently accurate and comprehensive dependency model of P (or its contextually relevant parts); S's degree of understanding of P is proportional to the accuracy and comprehensiveness of the dependency model (or its contextually relevant parts) (Dellsén 2020, 1268).

Premises (2) and (3) of my argument spell out two of the essential elements of the understanding required for the defense of scientific realism. In Dellsén's terms, we are to imagine that the agent has a model of their experiment that includes not just what occurred, but also how what occurred was dependent on and independent of various salient factors. Premise (2) mandates that this model represent the experiment's results as obtaining for any situation, no matter how the electrostatic induction is created. Premise (3) says that this model also represents what the results do depend on, namely the movements of these charges. Premise (1) adds that this dependency model of the experiment is built using the theoretical resources that the agent has provisionally adopted.

It is worth noting that Dellsén himself makes it very easy for an agent to understand a phenomenon. As the title of an earlier paper "Understanding Without Justification or Belief" (Dellsén 2017) suggests, Dellsén maintains that an agent may grasp the right kind of dependency model for some phenomenon, and thereby understand that phenomenon, without any justification at all, and also without anything like a belief in the claims that their model makes concerning that phenomenon. However, this possibility does not undermine the argument that I am proposing. I am considering a case when an agent *does* believe the theory in question, and deploys some of those beliefs in their experiments. When these beliefs and the experimental results are responsible for their grasp of the propositions given in premises (2) and (3), then, I am claiming, the resulting state of understanding will make it highly likely that they know of the existence and character of those unobservable entities. That is the contentious core of this understanding-based defense of scientific realism. Nothing about Dellsén's account of understanding is incompatible with this defense, although of course he may not wish to endorse it.[2]

If Dellsén is right about objectual understanding, then it is easy to argue that the truth of premises (1), (2) and (3) makes it highly likely that (4) obtains. The contentious step will then be from the truth of these premises to the truth of the conclusion (5). Other accounts of scientific understanding make understanding more demanding. For example, in different ways, Kelp (2017) and Khalifa (2017) require that a good state of understanding just be a special kind of knowledge. If this is right, then the inference from the truth of premises (1)-(4) to (5) is easy. But there will be many cases where premises (1), (2) and (3) obtain but (4) does not. Grasping a proposition is not knowing it, and even for an agent who knows the right propositions, the knowledge may not be integrated in the way that either Kelp or Khalifa requires.

[2] See also Dellsén 2016. My discussion has been influenced by Rice's "understanding realism", but I do not think he would endorse my defense of scientific realism. See Rice 2021.

The substance of an understanding-based defense of realism must thus clarify how to connect understanding to knowledge, and then respond to concerns that such a connection is implausible or unlikely. This is not my aim here. Instead, my goal is to consider what accounts of scientific understanding would block any development of this sort of defense. I first identify a necessary condition for this defense in section III and then argue in section IV that Potochnik's recent proposal would undermine this necessary condition.

III. Scientific Understanding and Knowledge

The argument sketched in the last section assumes that the knowledge of unobservable entities that arises through the understanding of the phenomenon proceeds in part through the grasp of the independence of that phenomenon from the experimenter and their scientific community. For example, in the case of electrostatic induction, the experimenter came to know of the existence of unobservable charged particles only because they grasped that the phenomenon of electrostatic induction depends on those particles in an objective way that is valid quite generally. While that instance of the phenomenon is due to the experimenter's actions, the phenomenon has an independent and objective character that is described, at least in part, by the electrodynamic theory that includes Coulomb's law.

I will call this assumption an *independence condition* on understanding. An agent's understanding of some phenomenon X satisfies the independence condition just in case the agent grasps that X may obtain with the very same characteristics independently of the agent's actions or the operations of their scientific community. In this section I consider the effects of denying that scientific understanding can meet this independence condition. I claim that if someone denies that understanding can meet the independence condition, then that person cannot defend scientific realism.

Suppose, then, that an agent conducts an experiment that involves the creation and manipulation of electrostatic induction. According to whatever proposed account of scientific understanding is being considered, let us suppose further that the agent acquires an understanding of electrostatic induction. However, we can stipulate that according to this account of scientific understanding there is some barrier to the agent grasping that the phenomenon occurs independently of their actions and their scientific community. If this independence condition is not met, the agent may still grasp that the phenomenon depends on the existence and character of some unobservable objects. But if they are unable to grasp that the phenomenon occurs independently of their actions and their scientific community, then they are not able to defend their claim to know of the existence and character of these unobservable objects. For a natural and ordinary question for another agent to ask is "how objective are these unobservable entities that you are proposing?" If the agent cannot answer this question, then their claim to know will be rejected by other scientists.

To see how this exchange can develop in an ordinary scientific case, consider an experimental drug trial that is conducted without a control group. In the general population, 90% of people with a given disease recover in 7 days. When 500 people with the disease are treated with the new drug, suppose that 95% of these people recover in 7 days. If a scientist puts this result forward as evidence for the effectiveness of the drug through some unobservable mechanism, the first question they would be asked is how they had controlled for the so-called "placebo effect". This is the well-known phenomenon that sick people tend to recover more often when they believe that they are being treated, even if the treatment is an ineffective placebo. Doubts tied to the placebo effect are an instance of a legitimate scientific worry about our independence condition: to suggest that the higher rate of recovery is merely due to the placebo effect is one way of saying that the higher rate of recovery is dependent on the experimenter's actions. The experimenter's actions are responsible for the change, and so nothing about the effectiveness of the new drug can be inferred. The obvious way to remedy this kind of defect is to conduct the experiment with a control group whose treatment resembles the treatment of the drug trial group as much as possible.[3] This would allow an experimenter to assemble evidence that the independence condition is met, and so a necessary condition on their claim to know is satisfied.

I conclude that if an account of scientific understanding places principled restrictions on understanding that preclude satisfying our independence condition, then that account is unable to deliver a defense of scientific realism. There are two preliminary objections to this conclusion that are important to discuss right away. First, one might argue for the existence of genuine knowledge that cannot be defended in the way that I have required. As noted in section I, I may come to know that I left my glasses in the kitchen through perception, and be unable to respond to doubts about this knowledge. Other forms of basic knowledge have a similarly inarticulate character: my memory provides knowledge of where I was born, but a claim to know where I was born based only on my memory is hard to defend. Inspired by these cases, one might suppose that there are cases of scientific knowledge where an agent genuinely does know about some unobservable entities, but is unable to respond to legitimate doubts about

[3] Woodward 2003, section 3.1 motivates his interventionist account of causes using this kind of case.

their knowledge. The scientific community may not accept such claims to know, but that does not show that the agent lacks this kind of basic scientific knowledge.

My reply to this objection is that knowledge of the existence and character of unobservable entities is not basic knowledge, and it should not be approached on the model of basic kinds of knowledge like perception or memory. There may of course be basic knowledge that is involved in a defense of scientific realism. For example, any report on an experiment will rely on perception and memory. Scientific doubts about these elements can be addressed in ordinary ways such as by replicating the experiment. Philosophical doubts about perception and memory are not at issue in this defense of scientific realism, as I have assumed that only concerns that are recognizably scientific are salient to this defense.

The second objection to consider is that there may be a defense of scientific realism that does not involve scientific understanding. If so, then one's account of scientific understanding would have no implications for one's capacity to defend scientific realism. It is difficult to prove that any defense of scientific realism must go through claims about understanding, and I do not propose to defend that strong claim here. If a defender of scientific realism has a proposal that gets around the problems I consider here, then it would constitute a valuable contribution to the debate about scientific realism. Most accounts of scientific realism either give up the defense of knowledge or else defend their claim to know through an appeal to explanation or understanding. Explanationist defenses of scientific realism have immediate implications for understanding, at least if we assume that grasping an explanation can afford objectual understanding. Versions of scientific realism that do not require knowledge are certainly worth considering, but I have set them aside due to reasons of space.

To conclude this section, I would like to illustrate how one clear way of denying the independence condition goes along with an inability to defend scientific realism. Here I focus on Giere's "perspectival realism". This proposal renders the achievements of science essentially contextual. That is, there is no way to detach that achievement from the context that enabled it, and present it in an unqualified or absolute way. As Giere describes the view,

> For a perspectival realist, the strongest claims a scientist can legitimately make are of a qualified, conditional form: "According to this highly confirmed theory (or reliable instrument), the world seems to be roughly such and such." There is no way legitimately to take the further objectivist step and declare unconditionally: "This theory (or instrument) provides us with a complete and literally correct picture of the world itself" (Giere 2006, 5-6).

Giere's objectivist target here is actually poorly framed. The scientific realism that results from the defense sketched in the last section does not purport to offer "a complete and literally correct picture" of the phenomena under investigation (let alone the whole world). But the scientific realist does aspire to detach a claim to know from the context that enables it. This aim is encapsulated in premise (2) and our independence condition. In our electrostatics case, some preliminary conclusions could be presented in a way that makes reference to a given experimental or theoretical context: "According to our highly confirmed theory of electrostatics, the movement of the gold leaves is due to the changing distribution of electric charges." The realist goes further, though, and insists that agents can know more than this. A scientist can know, in an unqualified sense, that the movement of the gold leaves is due to the changing distribution of electric charges. What is known here is a genuine feature of the world. It is not qualified or conditioned by the context that enabled its discovery.

In his critical discussion of perspectivism Chakravartty has helpfully identified this core issue: for the perspectivist, “Knowledge of scientific ontology is bound within specific contexts because our epistemic abilities do not extend as far as perspective-transcendent knowledge” (Chakravartty 2017, 177). Chakravartty examines the arguments offered for perspectivism and convincingly shows how they fail. Here I emphasize a different point: adopting perspectivism blocks any defense of scientific realism. This is perhaps not a very surprising result for Giere’s perspectivism: the position is flatly incompatible with a claim to know outright any features of reality. However, it is still instructive to see how Giere’s perspectivism blocks any defense of scientific realism because this will provide a roadmap for how to proceed in more contentious cases.

A perspectivist like Giere offers principled reasons to reject our independence condition. That is, no matter how well an agent understands some phenomenon like electrostatic induction, the agent is not able to grasp how the phenomenon can occur independently of the agent’s actions or the operations of their scientific community. That is, as far as they can tell, the features that they find the phenomenon to exhibit in their experiments could very well be dependent on the agent’s actions or some of the operations of their scientific community.

Rejecting the independence condition leads a perspectivist to a highly qualified conception of scientific knowledge as essentially contextual or perspectival. There is thus a huge difference between the knowledge that the scientific realist ascribes to the agent and the knowledge that the perspectivist ascribes to the agent. The scientific realist moves from the understanding of some phenomenon to knowledge of the existence and character of some objects that that phenomenon depends on. In the case of electrostatic induction the realist supposes that an agent who understands this phenomenon knows something about unobservable charges. The perspectivist cannot endorse this reasoning. Instead, they attribute knowledge that is about something else: what is occurring in that context, which is jointly produced by the agent's actions along with the target phenomenon. Knowledge of what is occurring in that context has no clear implications for what will occur in any other context. The knowledge that the perspectivalist opts for is like the knowledge of the results of the drug trial without a control group. In the drug trial case, the results include the effects of the drug (if any) along with the placebo effect, and there was no way initially to separate these two contributions to the experimental results. The perspectivalist supposes that this problem is a permanent one as we can never detach what we find from its experimental or theoretical context. And so even if the agent can know, with respect to some context, that Coulomb's law obtains, they cannot draw any inferences about what will happen in any other context. By contrast, the realist forms legitimate expectations for what would happen in a wide variety of new experimental contexts, from picking up bits of cloth to bending streams of tap water. That is the "cash value" of knowing outright rather than knowing in a way that is confined to some context.

IV. A Challenge to Potochnik

Giere's perspectivism is motivated by a broad worry about the possibility of detaching scientific knowledge from its context of origin. By contrast, Potochnik has defended a more nuanced account of understanding that is more narrowly focused on the prevalence of idealization in scientific practice (2017). Still, it seems that Potochnik is committed to denying our independence condition. In a recent discussion of her account of scientific understanding, for example, she notes that "I do not think that science generates a unified understanding or explanatory store but rather different, crosscutting varieties of understanding, even of a given phenomenon" (2020, 940). These varieties arise based on the goals of the investigators. The worry is, then, that an agent's understanding of some phenomenon will be based on the goals of the agent's research community. If these goals partly constitute the understanding, then our independence condition will be violated, and the proposed defense of scientific realism will fail.

To develop this worry in more detail, I will briefly review the key elements of Potochnik's approach to understanding. For Potochnik, when an agent investigates a phenomenon, they may come to understand that phenomenon, but only in a qualified way. The investigation is invariably directed by an "epistemic aim" (2020, 937) or "research questions" (2020, 940), which in turn help to determine if the resulting cognitive state constitutes genuine understanding. This allows an idealization to be an essential part of many states of understanding. In her apt formulation, "Idealizations are assumptions made without regard for whether they are true and often with full knowledge they are false" (2020, 934). These assumptions will be part of the state of understanding when they enable an agent to understand a feature of the phenomenon that is of interest to the agent.

Potochnik has developed a special vocabulary to characterize the features of a phenomenon that are salient to understanding that phenomenon. They are what she calls *causal patterns* that are *embodied* in the phenomenon. More fully, "scientific understanding of some phenomenon requires (a) grasping a causal pattern (b) that is embodied in the phenomenon and (c) focal to the cognizer(s)" (2020, 936). I will assume that a phenomenon is a repeatable type of state, process or event. A causal pattern is a regularity of a causal kind that may be "limited in scope and that may permit exceptions" (2020, 935). Crucially, our representation of a causal pattern may not make these limitations or exceptions explicit. One of Potochnik's examples is the ideal gas law. It is a representation that "ignores molecular size and intermolecular forces" (2020, 935). In this respect, the representation is arrived at through assumptions that are believed to be false of actual gases. At the same time, such "idealizations can be used to facilitate representation of simple patterns to generate scientific understanding" (2020, 935). In this case, representing an actual gas with the ideal gas law can constitute genuine understanding, but only if the aims of the researcher focus their attention on specific aspects of the gas. These include "how temperature increasing in a sealed container of gas with a fixed volume increases the pressure" (2020, 935). This is an example of a causal pattern that is embodied in the phenomenon of some gas in a sealed container.

Although an agent's representation of the embodied pattern may require an idealization, whether or not the pattern is embodied by the phenomenon is fixed independently of the agent's representation. At the same time, an agent's understanding of the phenomenon is irredeemably representational, and so the conditions on that representation are essential to the state of understanding. This is the reason that Potochnik concludes that understanding is nonfactive: "an account that is less accurate of a phenomenon (i.e., more idealized) can generate better understanding of that phenomenon when it depicts the causal pattern focal to those who seek understanding" (2020, 941). By contrast, "knowledge is factive" (2020, 941). So even if the ideal gas law is central to the state of understanding some gas in a sealed container, an agent cannot know that the ideal gas law governs the gas for the simple reason that this law is not true of any real gas. For Potochnik, an agent can know "the pattern described by the ideal gas law is embodied in this system" (2020, 941). This knowledge is consistent with the phenomenon embodying other causal patterns, including patterns that would frustrate the pattern picked out by the ideal gas law.

Consider a case where an agent has an understanding of some phenomenon P. According to Potochnik's account of understanding that agent does not grasp that the phenomenon obtains independently of their actions and scientific community. This is because the agent's understanding is constituted in part by their research interests. The research interests do not merely enable a grasp of some characteristic of the phenomenon that is detachable from those interests. Instead, the research interests permeate their understanding to such an extent that they cannot suppose that what they understand of the phenomenon obtains independently of those research interests. This has immediate implications for what the agent can claim to know about that phenomenon. As Potochnik recognizes, "the object of our scientific knowledge is not technically the phenomena scientists investigate but the causal patterns those phenomena embody. Science generates understanding of phenomena, and it does so via knowledge of the causal patterns they embody. Knowledge and understanding go hand in hand, but there is a gap between their objects" (2020, 942). The agent may come to know that a given causal pattern is embodied in their phenomena, but this knowledge is insufficient to defend scientific realism. The understanding achieved from within one research community involves knowledge that is restricted by the aims of that community. So there is no way for an agent in one community to draw any conclusions about what would occur if their research interests changed or if they aimed to study the phenomenon in a fully objective fashion.

Potochnik could reply that an agent can combine a series of qualified knowledge claims that some causal pattern is embodied in some phenomena (for some research program) in order to know a conclusion about how the phenomenon is (independently of any research program). For example, a common representation that incorporates Coulomb's law proves apt to generate understanding of the electroscope phenomenon along with others, such as picking up pieces of cloth, having balloons stick to walls, and bending thin streams of tap water. Perhaps this repeated use of Coulomb's law could afford knowledge that Coulomb's law holds absolutely or in a way that is independent of the research interests of scientists. It is unclear to me, though, how Potochnik can validate this inference. For it looks to be an instance of the argument for scientific realism that I considered back in section II. I have argued that this argument must proceed through a state of understanding that involves a grasp of a dependency model. Crucially, an agent needs to grasp that the phenomenon in question obtains independently of the agent's community, including the interests of the agents doing the experiments. Even if we have a series of phenomena that are known to embody a causal pattern, knowledge of this embodiment does nothing to establish what will happen for other phenomena. So this approach to understanding and knowledge is unable to validate the core defense of scientific realism developed here. Unless some other defense is available, Potochnik's account of scientific understanding undercuts a defense of scientific realism.

V. Conclusion

Recent refinements of scientific realism emphasize its limited, piecemeal character. For such a realist, there is no simple or monolithic way to understand the success of science. Instead, there is only a series of successes, including novel, accurate predictions and technological applications, where each should be understood in their own, local terms. I have sketched a defense of this selective form of scientific realism that relies on scientific understanding and that incorporates the central aspects of Feigl's Copernican turn. This sort of understanding requires appreciating the objective character of the phenomenon. Any approach to scientific understanding that allows for this can make the right connections between understanding a phenomenon and knowing unqualified propositions concerning that phenomenon. This analysis should help the realist to see how best to deploy recent work on understanding in the service of their defense of realism. It also presents a dilemma for some accounts of understanding: either amend the account of understanding or give up on the prospects for a defense of scientific realism.

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Reply to Potochnik

Christopher Pincock

Marxists often argue about how best to achieve a communist revolution. They agree on the ultimate aim, but not on the means to achieve that aim. I take my debate with Angela Potochnik to have a similar structure: we both agree that science provides knowledge, and that this feature of science should be maintained against doubts motivated by the practice or history of science. But we disagree about the most effective means of maintaining scientific realism. In my chapter I began my examination of scientific realism by supposing that it is subject to a certain kind of defense. This contributes to the somewhat schematic character of my contribution, where I argue that we can defend a realist's claims to know in a special type of situation. But my contribution does not engage with the complicated practice of actual science. By contrast, Potochnik draws on a rich account of how science is done by humans, with all of their various aims and cognitive limitations. This informs her account of what scientific knowledge amounts to. But this focus on practice leaves open how defensible these claims to knowledge will turn out to be.

In this brief reply I want to examine in more detail what is involved in Potochnik's causal pattern realism and consider again how the defense I sketched in my contribution can be adapted to this form of realism. It might seem that any sort of selective scientific realism can be defended using the kind of argument that I sketched. In my contribution I argued that this is not the case. Any form of selective scientific realism that involves contextual limitations on knowledge, explanation or understanding is not defensible. This argument needs to be fleshed out in more detail if it is to convince Potochnik (and others) to avoid these sorts of contextual limitations. I doubt I can persuade her to make these changes, but I believe the exchange can still be fruitful as it clarifies how various forms of selective scientific realism fit with different styles of defense.

In her contribution Potochnik makes clear that she is a causal pattern realist. Scientists investigate phenomena, which are repeatable types of events, states or processes. Some phenomena involve unobservable entities such as molecules or charged particles. When these investigations go well, scientists come to know that a causal pattern is embodied in such a phenomenon. Causal patterns exist and are embodied by some phenomenon in a completely objective and mind-independent way. And so if scientists succeed in knowing of the existence and embodiment of causal patterns, it is perfectly appropriate to say that they have acquired knowledge of a feature of the objective world.

In my contribution I supposed that a scientific realist must insist on knowledge of the existence and character of unobservable entities like molecules or charged particles. From Potochnik's discussion it seems that a scientist may know that a causal pattern is embodied in a phenomenon without knowing of the existence and character of any unobservable entities. For example, if all the causal patterns that a scientist knows about are observable, then this is knowledge that the traditional anti-realist about science would be happy to accept. So I think it is fair to consider a case where there is something unobservable about the embodiment of a causal pattern in a given phenomenon. In my electrostatics case, the theory claims that there are charged particles that come in one of two kinds. Like charges repel one another and unlike charges attract one another based on their distance. What sort of causal pattern is thereby claimed to be embodied in an electrostatic phenomenon? I suppose that such patterns involve the right kind of counterfactual relationship between variables, or what Potochnik calls "factors". These in turn can be treated as families of properties. On this reading, one salient causal pattern is how varying the distance between like charges varies the force of repulsion between them. In an experimental manipulation of electrostatic induction, the experimenter decreases the distance between large numbers of like charges and thereby brings about the divergence of the gold leaves on their electroscope. The observable causal pattern that relates their movements to the divergence of the gold leaves is said to be due to the unobservable causal pattern that relates their movements to the movements of unobservable like charges, which in turn are related to the divergence of the gold leaves.

The causal pattern realist claims that these sorts of theory-informed experimental manipulations are apt to provide knowledge of some unobservable causal patterns. More specifically, the scientist can come to know that this unobservable causal pattern is embodied in that very phenomenon. This is knowledge that something has these properties, but the character of the property bearer does not seem relevant to the embodiment of the causal pattern. Potochnik's causal pattern realism thus has some similarities to what Woodward once called "instrumental realism": for successful theories, "what is worth taking most literally and realistically … [are] claims about relational structures and patterns and particularly their claims about how changing one quantity, property, or feature will change some other quantity" (Woodward 2003, 114). Causal pattern realism is thus a highly restrictive form of scientific realism. This should make it easier to defend. In what follows, I argue that causal pattern realism cannot be defended if contextual limitation are placed on knowledge, explanation or understanding.

Can Potochnik adapt my defense of selective scientific realism to her causal pattern realism? Let us see how the steps of that defense are changed when tailored to causal pattern realism. Here the changes are underlined:

(1) An agent conducts an experiment that successfully creates and manipulates an instance of some phenomenon, drawing in part on their theoretical beliefs concerning this phenomenon, <u>including a belief that this phenomenon embodies an unobservable causal pattern</u>.

(2) The agent grasps that <u>the embodiment of the unobservable causal pattern</u> in question obtains *independently* of their actions and scientific community.

(3) The agent grasps that the observable features of this phenomenon *depend on* the <u>embodiment of an unobservable causal pattern</u> that is posited by their theoretical beliefs.

(4) The agent has a good understanding of the phenomenon.

(5) Therefore, the agent knows of the <u>embodiment of an unobservable causal pattern</u>.

Here I have substituted "embodiment of an unobservable causal pattern" for "existence and character of some unobservable entities." In my contribution I argued that if explanation or understanding are constituted in part by an agent's research interests, this defense will break down at step (2). If the agent is unable to grasp that the causal pattern is embodied independently from their research interests, then whatever understanding they achieve in step (4) will be unlikely to go along with the knowledge claimed in step (5). This is because the knowledge claimed in step (5) requires a strong sort of independence, including independence from the agent's research interests. Potochnik and I agree that scientific knowledge is knowledge of an objective matter of fact. So if the understanding fails to include a grasp of this independence, then the agent will fail to know of this feature of the objective world.

What should Potochnik's attitude towards step (2) be? One option that she could take is that it is not necessary. On this reading, the argument goes through even without step (2) because step (2) is redundant. Taking this option involves arguing that meeting step (1) and step (3) is sufficient to obtain the right kind of good understanding described in step (4). And when step (4) is arrived at through this route, it is highly likely that the agent acquires knowledge of the embodiment of that unobservable causal pattern. The worry I have about this defense of causal pattern realism is that the state of understanding is partly constituted by the agent's research interests. In her contribution Potochnik emphasizes the contextual aspects of explanation. A genuine explanation "addresses the explanation-seekers cognitive needs" and the best explanation "best addresses the audience's cognitive needs" (p. 14). As I note in my original contribution, Potochnik has the same contextual account of scientific understanding. So it would be natural to insist that step (2) is irrelevant to the sort of understanding her account allows. But then it is hard to see how achieving such a state of understanding would make it likely that the agent knew, in an interest-independent way, of the embodiment of the pattern. Potochnik is here clear that she does not wish to make knowledge of patterns itself context-specific. For she says that "the status of qualifying as knowledge is not indexed to research context" (p. 12). So there is a gap between the context-specific understanding that her account requires for step (4) and the context-free knowledge in step (5).

Another option is that step (2) plays an important role in the argument, but that Potochnik can provide sufficient motivation for this step in the cases where knowledge of unobservable causal patterns is acquired. On this option, even if the understanding in step (4) is context-specific, when it is informed by a grasp of independence in step (2) it is highly likely to go along with context-free knowledge of the embodiment of that causal pattern. How, though, can an agent grasp that the pattern is embodied independently of their actions and research community? Here I would appeal to my discussion of the placebo effect in my original contribution. Notice that according to Potochnik every phenomenon embodies many causal patterns, including those that involve an experimenter's actions. In any situation where an agent is inferring the existence of a new causal pattern through their experimentation, they must be able to sort out when an observable feature genuinely depends on a new causal pattern and when an observable feature arises as an artifact of their experimentation. There is a common realist strategy to address this sort of worry: the best explanation of the observable features found in the experiment is that they depend on this objectively existing unobservable causal pattern. But this strategy is not available to Potochnik. As we have just seen, she makes cognitive needs central to the goodness of a proposed explanation. Elsewhere she emphasizes that our best explanations are permeated with idealized representations, including idealized representations of the very causal patterns at issue. So if Potochnik's accounts of explanation and understanding are correct, it is not clear how an agent can satisfy step (2) in this defense.

In conclusion, it is important to clarify what I think these considerations establish. They are meant to establish that this defense of scientific realism is not available to those who index knowledge, explanation or understanding to an agent-relative context such as a goal or community. I am not arguing, though, that Potochnik is wrong when she says that we have knowledge of the embodiment of unobservable causal patterns. There very well may be the kind of knowledge that she insists on. But if a defense of these claims to know is critical to maintaining one's realism in the face of doubts, then this kind of realism is in trouble precisely because it cannot be defended.

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