What is Scientific Progress? Lessons from Scientific Practice

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Abstract: Alexander Bird argues for an epistemic account of scientific progress, whereas Darrell Rowbottom argues for a semantic account. Both appeal to intuitions about hypothetical cases in support of their accounts. Since the methodological significance of such appeals to intuition is unclear, I think that a new approach might be fruitful at this stage in the debate. So I propose to abandon appeals to intuition and look at scientific practice instead. I discuss two cases that illustrate the way in which scientists make judgments about progress. As far as scientists are concerned, progress is made when scientific discoveries contribute to the increase of scientific knowledge of the following sorts: empirical, theoretical, practical, and methodological. I then propose to articulate an account of progress that does justice to this broad conception of progress employed by scientists. I discuss one way of doing so, namely, by expanding our notion of scientific knowledge to include both know-that and know-how.

Keywords: aim of science; Alexander Bird; Darrell Rowbottom; scientific knowledge; scientific practice; scientific progress

1. Introduction

According to Chang (2007, p. 1), "Scientific progress remains one of the most significant issues in the philosophy of science today." This is partly because it seems rather odd to deny that science is making progress, but it is difficult to articulate in what sense exactly science is making progress. Recently, Bird has argued for an account of progress that has a rich tradition. According to Bird (2007, p. 64):

> Science (or some particular scientific field or theory) makes progress precisely when it shows the accumulation of scientific knowledge; an episode in science is progressive when at the end of the episode there is more knowledge than at the beginning.

As Bird admits, this conception of scientific progress is not new.[1] However, as Bird notes, it seems that philosophers of science have ignored this conception of scientific progress, at least since the publication of Kuhn's *The Structure of Scientific Revolutions*. Similarly, Kitcher (2002, p. 385) observes that:

> Almost everybody seems to agree that the sciences constitute the richest and most extensive body of human knowledge, and scientists routinely talk of "what we now know." Within the philosophy of science, however, there is little explicit discussion of scientific knowledge.

[1] The idea of the growth of knowledge looms large in the history of science, at least from the early modern period. It may be traced back to Francis Bacon, whose *Instauratio magna* (1620) frontispiece declares: "*Multi pertransibunt et augebitur scientia*." Sarton (1927, I, pp. 3-4) expressed a similar idea as follows: "*The acquisition and systematization of positive knowledge is the only human activity which is truly cumulative and progressive*."

Bird (2008, p. 279) distinguishes between epistemic and semantic accounts of scientific progress as follows:

(E) An episode constitutes scientific progress precisely when it shows the accumulation of scientific knowledge.[2]

(S) An episode constitutes scientific progress precisely when it either (a) shows the accumulation of true scientific belief, or (b) shows increasing approximation to true scientific belief.[3]

By considering hypothetical cases, Bird (2007, p. 65) argues that "our intuitions about whether there is progress show that progress matches changes in knowledge, but not changes in truth." According to Bird (2007, p. 65), (S) "yields a verdict about progress in certain kinds of case [i.e., cases of beliefs with insufficient epistemic support] that is at odds with our intuitions." These intuitions, according to Bird (2007, p. 65), "imply that epistemic characteristics are essential to progress."[4]

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[2] It is worth noting that, according to Bird (2007, p. 84) contributions to scientific knowledge can be more or less significant. Bird supplements (E) with the notion of significance because of cases of pointless investigation, such as investigating grains of sand. See also Kitcher (1993, p. 117) on significant truths.

[3] Proponents of (S) include Popper (1979) and Niiniluoto (1987). As is well known, explicating the notion of approximate truth is notoriously difficult. Popper's (1972) attempt to formalize the notion of verisimilitude was shown to be problematic (see Miller 1974 and Tichý 1974). Other formal approaches, such as the similarity approach (see, e.g., Niiniluoto 1984, 1987) and the type hierarchy approach (see, e.g., Aronson, Harré, and Way 1994), also suffer from technical problems (see, e.g., Aronson 1990 and Psillos 1999). For these reasons, realists have tried to explicate approximate truth in non-formal, qualitative terms (see, e.g., Leplin 1981, Boyd 1990, Weston 1992, and Smith 1998). For example, it has been suggested that T_2 is more approximately true than its predecessor T_1 if T_1 can be described as a "limiting case" of T_2 (see, e.g., Post 1971; French and Kamminga 1993). But there are problems with these informal approaches as well (Chakravartty 2010).

[4] It is important to note that Bird does not take knowledge to be justified true belief. Rather, he thinks that his arguments support Williamson's (1997; 2000) view that knowledge is a foundational concept in epistemology and

Contrary to Bird, Rowbottom (2008) argues against (E) and in support of (S). Rowbottom also uses thought experiments and appeals to intuitions in support of (S). But he doesn't share Bird's intuitions about putative cases of progress. According to Rowbottom (2010), scientific progress is possible without justification, and since knowledge entails justification, scientific progress does not consist in the accumulation of knowledge.

Both Bird and Rowbottom, then, appeal to intuitions elicited by hypothetical cases in support of their accounts of progress. Accordingly, the debate has traded heavily on intuitions whose methodological significance is unclear.[5] Perhaps it might be fruitful to try a new approach at this stage in the debate. If so, then I propose to approach the question of scientific progress from a historically-informed and practically-engaged perspective.[6] That is to say, I think that an examination of actual scientific practices, as opposed to hypothetical cases and intuitions, will prove fruitful in terms of revealing the complexity of scientific progress. Such an examination of actual scientific practices reveals that there is more to scientific progress than the accumulation of true propositions.[7]

My overall argument, then, is an argument from actual cases. I discuss two cases which show that scientists employ a conception of progress that is broader than what either (S) or (E) suggest. The evidence for this broader conception of progress comes from what scientists say

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that it does not have an analysis. Henceforth, I will take knowledge to be an unanalyzable primitive in the same way that Williamson and Bird do.

[5] Both Bird and Rowbottom basically argue as follows: "Upon considering hypothetical case *C*, it seems to me that *p*; therefore, *p*." However, the problem is that, while it seems to Bird that *p*, it seems to Rowbottom that not-*p*. That is why the methodological significance of such appeals to intuition is unclear (see Mizrahi 2012 and 2013).

[6] Cf. Lakatos (1970, p. 91); Bird (2008, p. 73); Leplin (1997, p. 99 and p. 102).

[7] It is worth noting that scientific practices are quite diverse and vary across scientific disciplines and historical periods. See, e.g., Boon 2011 and the essays collected in Pickering 1992.

and write about progress as well as the ways in which they acknowledge and reward seminal work (e.g., by awarding the Nobel Prize). For the sake of brevity, I discuss just two cases. But these cases are by no means exceptional or atypical. Other cases confirm the same general conclusions about the broader conception of scientific progress employed by scientists.

2. Case Study: Landsteiner's Discovery of Blood Groups

Karl Landsteiner (1868-1943) was awarded the Nobel Prize in Physiology or Medicine in 1930 for his discovery of human blood groups. The Presentation Speech for the 1930 Nobel Prize in Physiology or Medicine was delivered by G. Hedrén, chairman of the Nobel committee for physiology or medicine of the Karolinska Institutet. In his speech, Hedrén (1930) recounted the reasons why Landsteiner's discovery advanced the biomedical sciences:

> Landsteiner's discovery of the blood groups […] has *opened up new avenues of research in several branches of science* and has brought with it *important advances in the purely practical field* (my emphasis).[8]

According to Hedrén, Landsteiner's discovery yielded practical applications in medicine as well as new methods that proved useful in other fields. For example, the purification techniques Landsteiner used in his own research, i.e., techniques of purifying antibodies by dissociating them from antigens, were later used (and are still used today for some applications) in immunology (Landsteiner and Miller 1925). In addition (Hedrén 1930):

[8] Available at http://nobelprize.org/nobel_prizes/medicine/laureates/1930/press.html.

> In the field of genetics the discovery of the blood groups has also proved to be *of importance from the point of view of methodology* in the study of the hereditary transmission of other characteristics. [It] also prompted research on the question—important for the study of constitution—whether other body cells in addition to erythrocytes, and in particular the germinal cells, can be differentiated according to specific groups (my emphasis).

According to Hedrén, in addition to the medical applications of his discovery, Landsteiner was awarded the Nobel Prize because his discovery also opened up new avenues of research in several branches of science and it proved to be important methodologically in the study of the hereditary transmission of other characteristics.

In his Nobel Lecture, Landsteiner explained how new methods led to new discoveries. As Landsteiner (1930) said, "it was not the usual chemical methods but the use of serological reagents which led to an important general result in protein chemistry, namely to the knowledge that the proteins in individual animal and plant species differ and are characteristic of each species."[9] This general result proved useful to Landsteiner in his experiments on the phenomenon of agglutination (i.e., when mixing blood from two individuals can lead to blood clumping and the clumped red cells can crack and cause toxic reactions). Before Landsteiner, it was commonly believed that agglutination is a pathology. But Landsteiner showed that it was due to the unique nature of the individual's blood. Blood clumping is an immunological reaction that occurs when the receiver of a blood transfusion has antibodies against the donor blood cells. Landsteiner then grouped blood types into three groups, which he designated as A, B, and C

[9] Available at http://nobelprize.org/nobel_prizes/medicine/laureates/1930/landsteiner-lecture.pdf.

(which later became O and the AB blood group was subsequently added by others). Because of Landsteiner's work, the first successful blood transfusion took place in Mt. Sinai Hospital in New York in 1907.

3. Case Study: Pavlov's Work on the Physiology of Digestion

In 1904, Ivan Petrovich Pavlov (1849-1936) received the Nobel Prize in Physiology or Medicine "in recognition of his work on the physiology of digestion, through which knowledge on vital aspects of the subject has been transformed and enlarged." K. A. H. Mörner (1904), who delivered the Presentation Speech, opened his speech by saying that "*The aim of science is the acquisition of knowledge*" (my emphasis) and that assessments of progress must be made relative to this aim. According to Mörner (1904), then, in order to appreciate the significance of Pavlov's work, we must look at the state of knowledge in the field of physiology before Pavlov's work and then see how Pavlov's work improved upon the state of knowledge in the field. According to Mörner (1904):

> Before Pavlov, *knowledge in this field was in many respects very imperfect*. Pavlov has corrected earlier *erroneous opinions* which were held even with regard to the main points within this part of physiology. He has further *enriched it with significant data* (my emphasis).

Accordingly, in addition to contributions in terms of new methods and techniques of investigation (one, in particular, became known as the "chronic experiment," involving intact animals), Pavlov corrected erroneous beliefs and added new factual knowledge about the physiology of digestion. Pavlov also added to the already existing, yet incomplete, knowledge about the vagus nerve.

Accordingly, Pavlov's work on digestion was deemed a major contribution to the progress of physiology by his colleagues. In Pavlov's case, the reasons include revising imperfect knowledge, correcting erroneous opinions, and devising new methods and techniques for the study of the physiology of digestion. These epistemic reasons, in terms of the rejection of false beliefs, correction of erroneous opinions, and addition of new knowledge, are the grounds for the judgment that "Pavlov's work on digestion has been found to be of great importance for the study of disease, and undoubtedly the progress made in physiological knowledge in this case as well as in others will lead to a transformation of the concepts of diseases and their treatments" (Mörner 1904). For these reasons, Pavlov was awarded the 1904 Nobel Prize in Physiology or Medicine.

In his Nobel Lecture on the physiology of digestion, Pavlov (1904) identified the following aim for physiology:

> Precise *knowledge* of what happens to the food entering the organism must be the subject of ideal physiology, the physiology of the future. Present-day physiology can but engage in the *continuous accumulation of material for the achievement of this distant aim* (my emphasis).[10]

[10] Available at http://nobelprize.org/nobel_prizes/medicine/laureates/1904/pavlov-lecture.html.

After he surveyed the state of knowledge in the field before his contributions and explained how his contributions added to or corrected existing knowledge, Pavlov judged that the science of physiology "is making huge progress every day" (Pavlov 1904).

4. Lessons from Scientific Practice

From the cases of Landsteiner and Pavlov, and others like them,[11] I think that we can discern the following pattern, which seems to be the way scientists make judgments about progress:

1. Survey the body of knowledge *B* in field *F* at time *t* prior to discovery *D*.
2. Estimate what was known (*B*) in *F* at *t*.
3. Identify a lacuna, imprecision or error in *B* at *t*.
4. Spell out how *D* improved on *B* by adding new knowledge, correcting imprecision or exposing errors and correcting them.

In Landsteiner's case, a survey of the body of knowledge before his work reveals (a) a lacuna in terms of knowledge about the function of different proteins in different species, and (b) erroneous beliefs about the phenomenon of agglutination. Landsteiner corrected these erroneous beliefs and added new knowledge about the nature and function of blood clumping.

[11] See, e.g., the 1907 Nobel Prize in Physiology or Medicine awarded to Laveran "in recognition of his work on the role played by protozoa in causing diseases" (http://www.nobelprize.org/nobel_prizes/medicine/laureates/1907/laveran.html), the 1908 Nobel Prize in Physiology or Medicine awarded to Mechnikov and Ehrlich "in recognition of their work on immunity" (http://www.nobelprize.org/nobel_prizes/medicine/laureates/1908/), and the 1953 Nobel Prize in Physiology or Medicine awarded to Krebs "for his discovery of the citric acid cycle" (http://www.nobelprize.org/nobel_prizes/medicine/laureates/1953/#). See also Zuckerman (1996).

In Pavlov's case, a survey of the body of knowledge on the physiology of digestion before Pavlov's work reveals (a) a lacuna in terms of knowledge about the function of the digestive canal, the salivary glands, the intestinal canal, gallbladder, and the role of the nervous system, and (b) erroneous beliefs about the function of the vagus nerve and gastric secretion and how the gastric mucous membrane is excited by mechanical contact. Pavlov corrected these erroneous beliefs and added new knowledge about the stimulation of gastric secretion by the vagus nerve and the sympathetic nervous system, the differential excitability of the gastric mucous membrane relative to various substances and a similar one in the case of the mucous membrane lining the digestive tract. For example, as a result of Pavlov's work, it is known that "stimulation of gastric secretion of acid and pepsin and stimulation of pancreatic secretion of digestive enzymes starts with the anticipation of the ingestion of a desirable meal and is mediated by input to the stomach and pancreas from efferent nerves of the vagus" (Wood 2004, p. 327).

In addition to showing that scientists make judgments about progress that are based on epistemic criteria, rather than criteria that have to do with truth alone, the cases of Landsteiner and Pavlov also show that scientists employ a conception of progress that is broader than what either (S) or (E) suggest. This broad conception of progress includes knowledge of the following sorts:

(EK) *Empirical Knowledge*: Empirical knowledge usually comes in the form of experimental and observational results.[12]

[12] Admittedly, I am painting EK with a rather broad brush. Finer distinctions can be made, for example, between "raw data," i.e., the sort of data we get from immediate observation, and "models of data" (Suppes 1962, pp. 252-

(TK) *Theoretical Knowledge*: Theoretical knowledge usually comes in the form of well-confirmed hypotheses.

(PK) *Practical Knowledge*: Practical knowledge usually comes in the form of both immediate and long-term practical applications.

(MK) *Methodological Knowledge*: Methodological knowledge usually comes in the form of methods and techniques of learning about nature.

In the case of Landsteiner's discovery of blood groups, for example, we have advancements in terms of the following types of knowledge:

(EK) *knowing that* mixing blood from two human beings can lead to blood clumping or agglutination; *knowing that* the clumped red cells can crack and cause toxic reactions.

(TK) *knowing that* blood clumping is an immunological reaction (not a pathology, as erroneously thought) that occurs when the recipient of a blood transfusion has antibodies against that donor blood cells; *knowing that* there are distinct blood groups that can be classified according to the ABO and Rh systems (among others).

(PK) *knowing how to* determine blood groups and type human blood; knowing how to carry out safe blood transfusions; *knowing how to* type dried blood stains in the investigation of crimes where blood stains are left at the scene (though, nowadays,

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261). Other distinctions can be drawn between types of data processing, such as data assessment and data reduction (Hacking 1992, pp. 29-64). For present purposes, however, the important point is that advancements in terms of EK count as scientific progress.

there are other methods of typing, in addition to forensic serology, involving other body fluids and DNA).

(MK) *knowing how to* purify antibodies to study immunological responses and allergic reactions (since antigens as diverse as bacteria, pollen grains, and foreign red blood cells trigger the synthesis of antibodies in the lymphoid tissue).

As we have seen, Landsteiner was deemed worthy of the Nobel Prize because his discovery led to an increase not only in TK but also in PK and MK. In other words, scientists judged that medical science advanced, due to Landsteiner's work, not only because of the theoretical knowledge of blood types, but also because of the practical knowledge of blood transfusions and forensic serology, and the methodological knowledge of antibody purification.

In Pavlov's case, his work on the physiology of digestion was judged to have been progressive in terms of EK and TK:

(EK) *knowing that* stimulation of gastric secretion of acid and pepsin and stimulation of pancreatic secretion of digestive enzymes starts with the anticipation of the ingestion of desirable food.

(TK) *knowing that* stimulation of pancreatic secretion of digestive enzymes is mediated by input to the stomach and pancreas from efferent nerves (i.e., nerves that carry impulses away from the brain or spinal cord) of the vagus; *knowing that* the stimulation of secretion induced by connecting environmental stimuli with appearance of tasty food is a conditioned reflex.

In addition, and equally important, Pavlov's work was judged to have been progressive in terms of PK and MK as well:

(PK) *knowing how to* treat peptic and duodenal ulcer disease with selective vagotomy (in selective vagotomy, the branches of the vagus nerve to the gall bladder and pancreas are left intact; usually performed to reduce secretion of acid and pepsin by the stomach to cure a peptic ulcer); *knowing how to* treat gastric acid-related disorders with selective muscarinic receptor antagonists.

(MK) *knowing how to* study the anatomy of conscious animals by using surgical techniques, such as the Pavlov gastric pouch.

According to Wood (2004, p. 326), Pavlov believed that "chronic studies in surgically prepared conscious animals were most likely to yield new insights into the integrated physiology of organ systems in general and the digestive system in particular." Before Pavlov, experimental physiologists worked mostly with anesthetized animals. Pavlov showed that "sequentially repetitive studies in surgically prepared conscious animals are most likely to advance knowledge basic to humans" (Wood 2004, p. 326). Since Pavlov, it has been a standard methodological principle in physiology that "we must understand the normal functioning of an organ in the alert animal, as well as its anatomy, histology, and cellular biology, to know disease" (Wood 2004, p. 326). Wood (2004, p. 327) also notes that the Pavlov gastric pouch was crucial for "the discovery of the cephalic phase of secretion and its role in the aciticipatory preparation of the upper digestive tract for the ingestion of a meal."

As for PK, Wood (2004, p. 327) says that "the invention of the now-obsolete selective vagotomy for treatment of peptic and duodenal ulcer disease in humans in the 1930s and the successful pharmacological development of selective muscarinic receptor antagonists as therapy for gastric acid-related disorders emanate from Pavlov's discovery." In addition, "modern knowledge of the action of histamine as a powerful gastric acid secretagogue evolved directly from Pavlov's physiology factory" (Wood 2004, p. 327). Using Pavlov's techniques of surgical preparation of gastric fistulas in dogs, Pavlov's student, L. Popielski, "reported in 1916 that injection of histamine stimulated copious secretion of gastric acid in dogs with surgically prepared gastric fistulas" (Wood 2004, p. 327).

The cases of Landsteiner and Pavlov also show that advancements in EK and TK can occur when previously held beliefs are shown to be erroneous, as when Landsteiner showed that agglutination is not a pathology but rather an immunological reaction, and when new facts are discovered, as when Pavlov showed that the vagus nerve is the secretory nerve that controls the gastric glands. Furthermore, the cases of Landsteiner and Pavlov show how advancements in EK and TK can occur as a result of advancements in MK, as when Landsteiner's purification techniques were used to study allergic reactions and Pavlov's surgical techniques were used to study the anatomy of conscious animals.

This broad conception of progress employed by scientists in the way they acknowledge and reward seminal work is further confirmed by what scientists say even beyond the setting of the Nobel Prize. For example, according to Rutherford (1947, p. 178):

> Scientists are not dependent on the ideas of a single man, but on the combined wisdom of thousands of men, all thinking of the same problem, and each doing his little bit to add to the great structure of knowledge which is gradually being erected.[13]

I think that any account of scientific progress should be able to do justice to this broad conception of progress, as it is employed by scientists when they acknowledge and reward seminal work, and to the ways in which scientists talk about progress and the aim of science. I think that both (S) and (E), at least as formulated by Bird, fall short of doing that. That is to say, the semantic account of progress in terms of truth and the epistemic account of progress in terms of knowledge are both too narrow to accommodate the broad conception of progress employed by scientists. Here is why.

On (S), we get the following picture of science. The scientific enterprise is a truth-aiming enterprise. That is, the aim of scientific inquiry is truth. Scientists are making progress by approaching this goal. More explicitly, they make progress either by accumulating more true beliefs about nature or by getting increasingly closer to the truth. On (S), we are going somewhere worth going, i.e., truth. We are also collecting something worth collecting, i.e., true beliefs (Godfrey-Smith 2007).

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[13] Cf. Feyerabend (1987). As an anonymous referee pointed out, historians of science are becoming increasingly hostile to the "Great Men" style of historiography. This increasing hostility is partly due to the recognition that these "Great Men" stood on the shoulders of others, including lab technicians and assistants, data collectors, experimenters, inventors, and the like. See also Fissell and Cooter (2003, p. 156).

There are several objections to this picture of science. For present purposes, however, I simply wish to see how (S) fares when judged relative to actual scientific practices. As we have seen, scientists assess scientific progress on the basis of criteria that are literally epistemic, i.e., that have to do with knowledge rather than truth alone. More explicitly, scientists consider what was known before a certain discovery, and what is known as a result of that discovery, and if there is an increase in knowledge, then progress has been made.

Truth is indeed epistemically worthy. Since knowledge entails truth, there can be no scientific knowledge without truth. In other words, if *S* knows that *p*, then *p* is true (although this does not mean that knowledge can be analyzed in terms of truth plus something else). However, the Landsteiner and Pavlov cases show that, as far as scientists are concerned, justification and methodology are as important as truth. In the case of Landsteiner, the methodological know-how of antibody purification was deemed as important as the theoretical knowledge of blood groups. And, in the case of Pavlov, the surgical techniques he developed, such as the chronic experiment, were deemed as important as the theoretical knowledge of conditioned reflexes.

Even if (S) is the correct account of progress, and truth is the aim of science, there are still important lessons to be learned from an examination of actual scientific practices. That is to say, the Landsteiner and Pavlov cases teach us that, even if theoretical truth is the ultimate aim of science, there are many "sub aims" that are prerequisites for the "grand aim" of truth. As the Landsteiner and Pavlov cases show, those "sub aims" include error correction, data collection, methodological refinement, and the like.

On (E), we get the following picture of science. Science is a knowledge-seeking enterprise. Scientific progress consists in the accumulation of scientific knowledge. (E) takes truth to be necessary (since knowledge is factive), but not sufficient, for scientific progress. In addition to truth, justification and reliable methods are also necessary for scientific progress.[14] In that respect, unlike (S), (E) can begin to account for actual scientific practices. Scientists not only conceive of the aims of science in epistemic terms but also assess scientific progress on the basis of epistemic criteria.

However, the conception of progress offered by (E) is still too narrow to be able to accommodate the broad conception of progress employed by scientists. So I propose to take seriously the lessons of the Landsteiner and Pavlov cases. The lessons are that (a) scientists take scientific knowledge to be the aim of scientific inquiry; (b) scientists make judgments about scientific progress based on epistemic criteria; (c) scientists take scientific knowledge to include not only TK but also EK, PK, and MK. Each contribution to scientific knowledge in terms of one of these types of knowledge counts as progress. Scientists themselves seem to recognize that science advances owing to not only the grand scale theoretical frameworks of theoreticians but also the work of experimenters and data collectors, lab technicians, field workers, and the like.

In the next section, then, I discuss one way of expanding our notion of scientific knowledge, and so our notion of scientific progress as well, so as to accommodate the broad conception of scientific progress employed by scientists. If we grant that 'knowing how' (PK and MK) and 'knowing that' (EK and TK) are both types of scientific knowledge, then we could give

[14] By necessary and sufficient conditions here, I do not mean "individually necessary and jointly sufficient" as in conceptual analysis, but rather collectively sufficient for scientific progress, or better yet, constitutive criteria for progress.

an account of progress that might be broad enough to accommodate the broad conception of progress employed by scientists. Granted that PK and MK both count as scientific knowledge, and hence that their accumulation counts as scientific progress, my proposed account of scientific progress is the view that scientific progress is constituted by the accumulation of scientific knowledge, where scientific knowledge consists of each the following: EK, TK, PK, and MK. Each of these counts as scientific knowledge; the accumulation of each advances science.

5. Varieties of Scientific Knowledge

It seems to me that Baird and Faust (1990, p. 147) correctly point to the problem of focusing almost exclusively on theoretical truth in philosophy of science when they write:

> According to most philosophers, experiments are run in order to promote theory […]. Thus, according to most philosophers, improved theories account for the progress of scientific knowledge. […] this asymmetry is a mistake. Technicians, engineers and experimenters […] are able to make devices work with reliability and subtlety when they can say very little true, or approximately true, about how their devices work. Only blind bias would say that such scientists do not *know anything* about nature. Their knowledge consists in the ability to *do* things with nature, not *say* things about nature (original emphasis).[15]

[15] Baird and Faust suggest that philosophers of science talk about knowledge, but when they do, it is theoretical knowledge exclusively. On the other hand, Kitcher (2002, p. 385) says that, within the philosophy of science, "there is little explicit discussion of scientific knowledge."

Using the example of the cyclotron, Baird and Faust show that experimental work in science sometimes proceeds largely without being illuminated or guided by theory because sometimes part of the reason for doing experimental work is to provide data that is the material of theoretical work. More importantly, in those instances where theory is not available for whatever reason, scientists can still make progress with experiments and instruments. If this is correct, then (S), given its emphasis on theoretical truth, leaves out important aspects of science (i.e., experimentation, methodology, and practical applications) that scientists seem to take seriously when they evaluate progress.

The Landsteiner and Pavlov cases teach us that the conception of scientific knowledge employed by scientists is much broader than what either (E) or (S) suggest, and that conception includes knowledge of the following sorts: EK, TK, PK, and MK. For the most part, philosophers of science have focused on TK, while underestimating the importance of EK. As the Landsteiner and Pavlov cases suggest, however, EK plays an important role when it comes to evaluating discoveries in science. If a certain scientist is credited with having discovered something that was previously unknown, then that counts as progress, even if the discovery doesn't involve a new elaborate theoretical framework like the ones philosophers of science often focus on (e.g., quantum mechanics).

In addition to EK, two other types of knowledge that are of interest to scientists but that are either largely ignored by philosophers of science or subordinated to TK are MK and PK. This concern with "useful knowledge" can be traced back to the early modern roots of science. For instance, according to Robert Hooke, the business of the Royal Society is to "improve the knowledge of natural things, and all useful Arts, Manufactures, Mechanic, practices, Engines and Inventions by Experiments" (Hunter 1981, p. 146). Similarly, Henry Oldenburg wrote in a letter to Norwood (February 10, 1667) that the Royal Society "aims at the improvement of all useful sciences and arts, not by mere speculations but by exact and faithful observations and experiments" (Hall and Hall, 1965-1986, IV, p. 168). A similar concern for "useful knowledge" also motivated the members of the American Philosophical Society, which was founded in 1743 following the publication of Benjamin Franklin's *A Proposal for Promoting Useful Knowledge among the British Plantations in America*.[16]

Devitt (1984, p. 163) says that "not only are scientists learning more and more about the world, but also […] they are learning more and more about how to find out about the world" (cf. Kitcher 1993, p. 140). Likewise, Kitcher acknowledges practical progress as a goal of science. But then he sets it aside and goes on to give an account of cognitive progress. The problem, for Kitcher (1993, p. 92), is that "the notion of practical progress proves far more difficult than we might have thought." However, I think that we can begin to make sense of PK and MK, in the larger context of scientific progress, by taking seriously the lessons of the Landsteiner and Pavlov cases. Since both PK and MK are types of know-how, i.e., 'knowing how to', rather than 'knowing that', one natural way of expanding our conception of scientific knowledge is to grant that 'knowing that' and 'knowing how to' are both types of scientific knowledge.

Ryle (1946; 1949) was perhaps the first to distinguish explicitly between 'knowing that *p*' (propositional knowledge) and 'knowing how to *A*' (knowledge of skills). Since then, it seems that philosophers have focused mostly on the former. As we have seen, however, PK and MK

[16] Available at http://nationalhumanitiescenter.org/pds/becomingamer/ideas/text4/amerphilsociety.pdf.

(‘knowing how to’) seem to be of equal importance in science when it comes to assessing progress. When Landsteiner’s discovery of blood groups was evaluated by his peers and the Nobel Committee, they didn’t seem to consider the medical applications of his discovery as less (or more) important than the theoretical knowledge of blood types. Landsteiner’s contributions—theoretical, empirical, practical, and methodological—were judged equally as contributions to scientific knowledge, and hence progressive.

According to Baird and Faust (1990), when most philosophers of science speak of scientific progress, they focus on theoretical truth and subordinate other aspects of science, such as experimentation, instrumentation, and methodology to theoretical truth. They offer their diagnosis for the source of this mistaken asymmetry (Baird and Faust 1990, p. 148):

> When approached from the classical analysis of knowledge [as justified true belief], it is hard to say exactly *what* is progressing, when speaking of the progress of *experimental* scientific knowledge (original emphasis).

They urge a conception of scientific progress “broad enough to include the production of new scientific instruments and instrumental techniques” (Baird and Faust 1990, p. 148). They argue that “scientific knowledge consists of, among other things, scientific instruments and instrumental techniques, and not simply some kind of justified true beliefs” (Baird and Faust 1990, p. 148). They also argue that scientific knowledge “consists in the ability to *do* things with nature [as well as] *say* things about nature” (Baird and Faust 1990, p. 147). In other words, there is scientific work (i.e., work that is done by some scientists) that is not just about representing nature but also about intervening in nature. As Hacking (1983, p. 149) writes:

> Philosophers of science constantly discuss theories and representation of reality, but say almost nothing about experiment, technology, or the use of knowledge to alter the world.[17]

We can accommodate these insights, I suggest, by granting that know-how counts as scientific knowledge. Scientific knowledge, as the Landsteiner and Pavlov cases suggest, consists in the cognitive abilities to do things with nature as well as say things about nature. This is how scientists conceive of scientific knowledge. Their conception of scientific knowledge, and hence of scientific progress, includes knowledge of the following sorts: EK, TK, PK, and MK. As far as scientists are concerned, contributions in terms of each of these types of knowledge count as progress.[18]

In that respect, it is worth noting that scientists don't necessarily employ their conceptions of knowledge and progress consistently in their everyday work. In other words, the fact that scientists' remarks about knowledge and progress are often made in the context of

[17] As an anonymous reviewer pointed out, one reason why philosophers of science focus on theories (the end result of science) rather than practices might be that most of them are not practicing scientists (although there are exceptions, such as Michael Weisberg). In that respect, it is also important to mention the Society for Philosophy of Science in Practice (SPSP) whose "aim [is] to change [the fact that concern with practice has always been somewhat outside the mainstream of English-language philosophy of science] through a conscious and organized programme of detailed and systematic study of scientific practice that does not dispense with concerns about truth and rationality" (http://www.philosophy-science-practice.org/en/mission-statement/).

[18] As an anonymous referee pointed out, in order to understand scientific progress, we might need to expand our notion of scientific knowledge even further—beyond know-that and know-how—to include something like Baird's (2004) notion of "thing knowledge." Roughly speaking, Baird's idea is that things, e.g., scientific instruments, bear knowledge. Doing justice to Baird's material epistemology is beyond the scope of this paper.

speeches, memoires, and the like, suggests that judgments about progress are often made *ex post facto*. For instance, in an interview, 2009 Nobel Laureate in Physiology or Medicine, Carol W. Greider, was asked the following question (Dreifus 2009):

> It's been said that you and Dr. Blackburn didn't receive the Nobel Prize earlier because it hadn't yet been proved that telomeres and telomerase would be valuable in understanding disease. Does the prize this year mean that there now is an acceptance of their value?[19]

In response, Greider said the following (Dreifus 2009):

> I certainly hope so. That's why Nobel Prizes are usually awarded long after the original discovery. It takes time for the medical implications to become clear. I think it's clear now that the basic science we did is important to understanding cancers, some human genetic diseases and the age associated degenerative diseases.[20]

As Bird (2007, p. 87) writes, "the most exciting contributions to progress are often recognizable as such only with the benefit of hindsight."

6. Objections and Replies

I have argued that scientists employ a conception of scientific progress that is broader than what either (E) or (S) suggest. This broad conception of progress includes scientific knowledge of the

[19] Available at http://www.nytimes.com/2009/10/13/science/13conv.html?emc=eta1.
[20] Available at http://www.nytimes.com/2009/10/13/science/13conv.html?emc=eta1.

following sorts: EK, TK, PK, and MK. This can be multiply confirmed in what scientists say about knowledge and progress as well as in how they acknowledge and reward seminal (e.g., Nobel-worthy) work. Some might object to my argument by insisting that scientists are misguided in what they take to be progress. That is, scientists are not competent judges of the goals of science.

In reply, I think that there is no more reason to think that scientists are oblivious to the goals of their enterprise than footballers are oblivious to the goals of football, plumbers are oblivious to the goals of plumbing, or teachers are oblivious to the goals of teaching. The burden of proof, then, is on those who think that scientists are somehow different from practitioners of other crafts to show that scientists are indeed different from footballers, plumbers, teachers, etc., in some relevant way that bears on their ability to make judgments about the goals of science. Alternatively, those who put forth this objection to my argument would have to show that improvements in investigative methods, experimentation techniques, observation instruments, and the like should not count as progress or that Nobel Prizes are not appropriate indicators of scientific progress.

Others might object to my argument by insisting that the cases I have discussed in Sections 2 and 3 (i.e., Landsteiner and Pavlov), and those I have mentioned (Laveran, Mechnikov and Ehrlich, and Krebs[21]) are not representative of scientific practice, since they are cases from the life sciences (e.g., physiology, biology, chemistry). If we examine scientific

[21] As an anonymous referee pointed out, we might add to this list the 1939 Nobel Prize in Physics, awarded to E. O. Lawrence "for the invention and development of the cyclotron and for results obtained with it" (http://www.nobelprize.org/nobel_prizes/physics/laureates/1939/), apropos Baird and Faust's discussion of the cyclotron quoted in Section 5.

practices in the physical sciences (e.g., physics, cosmology, astronomy), so the objection goes, we might get different results.

In reply, I would like to take cosmology as a case study in order to show that a look at scientific practices in cosmology also confirms my claim about the broad conception of progress employed by scientists. As far as PK is concerned, one example of a potential practical application in cosmology is time travel. This may seem farfetched, but cosmologists and astrophysicists are considering the possibilities of time travel. (See, e.g., Davies 2003 and Gott 2002.). As far as MK is concerned, there is a noticeable emphasis on methodological advancements in cosmology. At the 1995 conference on "Key Problems in Astronomy and Astrophysics," Allan Sandage introduced the term "Practical Cosmology." It was an attempt to respond to the charge that cosmology was an "immature" scientific field with plenty of fascinating ideas but no substantial data to confirm them. To address this charge, Sandage and others thought that they must devote more attention to the methodological aspects of their work, specifically the ways in which astronomical observations are conducted and the ways in which data are gathered and analyzed. This was Sandage's vision of practical cosmology, which was discussed recently in the international conference "Problems of Practical Cosmology" (Baryshev 2008):

> *The advancement of cosmology is determined by the growth of observational data* and […] by the development of fundamental physical theories. Practical cosmology is the science which makes a link between observation and theory. *The major goal of practical cosmology is to develop strategies for uncovering and attacking cosmological problems*.

> Even with the wonderful advanced observational methods available, successful cosmological tests require that we *know how to* detect and handle different severe selection effects, which may be hidden both in data and, even seemingly secure, methods of data analysis (my emphasis).[22]

Some of these strategies and methods include the "classical cosmological tests," red-shift surveys, measurements of anisotropies of the cosmic background radiation by balloon and satellite (e.g., the Wilkinson Microwave Anisotropy Probe) experiments, and fractal techniques.[23]

7. Conclusion

In this paper, I have taken a historically-informed and practically-engaged approach to the question of scientific progress. I have argued that an examination of actual scientific practices reveals that there is more to scientific progress than the accumulation of true propositions, as proponents of (S) argue. I have also argued that scientists employ a conception of scientific knowledge, and hence of scientific progress, than is broader than what either (S) or (E) can accommodate. I have proposed one way of trying to account for this broad conception of progress as it is employed by scientists, namely, by expanding our notion of scientific knowledge to include both know-that and know-how.

[22] Available at http://ppc08.astro.spbu.ru/index.html.
[23] For more on the philosophy of space exploration, see Munevar (unpublished manuscript). Available at http://philosophyofspaceexploration.blogspot.com/.

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