What Is Scientific Progress?

by

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A dissertation submitted to the Graduate Faculty in Philosophy in partial fulfillment of
the requirements for the degree of Doctor of Philosophy,

The City University of New York

2010

This manuscript has been read and accepted for the
Graduate Faculty in Philosophy in satisfaction of the
dissertation requirement for the degree of Doctor of Philosophy.

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Abstract

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Better to discover how science is in fact developed and learned than to fabricate a fictitious structure to a similar effect.

-W. V. O. Quine

As Philip Kitcher observes, it seems that almost everybody agrees that science constitutes the richest and most extensive body of human knowledge. Among philosophers of science, however, there is curiously very little explicit discussion of scientific knowledge. As a result, the question “What is scientific progress?” almost never gets an answer in terms of the accumulation of scientific knowledge, even though this answer seems to be the most natural one. Indeed, this is how scientists themselves—from Early Modern natural philosophers to contemporary practitioners—conceive of scientific progress. For scientists, scientific progress occurs when there is an accumulation of scientific knowledge. A scientific episode is progressive when, by the end of such a period of scientific change, we know more than we did at the beginning.

I show that this is how scientists conceive of progress by examining some major episodes from the history of the life sciences, such as Harvey’s discovery of the

circulation of the blood, as well as some key episodes from the history of the Nobel Prize, especially in physiology or medicine. The Nobel Prize is a setting in which scientists reward their peers for what they take to be important contributions to scientific knowledge. Examining this scientific practice of assessing progress reveals that scientists make judgments about progressive discoveries based on epistemic criteria. This practice also reveals that, for scientists, scientific knowledge is not merely theoretical (inferential) knowledge. They also consider progressive the accumulation of empirical (factual), practical, and methodological knowledge.

Given that scientists take progress to consist in the accumulation of scientific knowledge, I argue that naturalists should articulate an account of progress that does justice to this scientific practice. Taking a naturalistic stance on the question of scientific progress, we want an account of progress that meshes with the history of science and the actual practices of scientists. I propose the epistemic account of scientific progress as such an account. The epistemic account simply says that scientific progress consists in the accumulation of scientific knowledge.

Why is it that philosophers of science have largely ignored the epistemic account of progress? I think this has to do with skeptical arguments, particularly against theoretical knowledge, advanced by the likes of Thomas Kuhn and Larry Laudan. I argue that these arguments do not provide compelling reasons for skepticism and pessimism about the accumulation of scientific knowledge. In order to address these skeptical arguments, I propose to (a) focus on individual claims to knowledge, rather than whole theories, as the units of progress, and (b) give up the distinction between ‘knowing that’ and ‘knowing how’. If we take practical and methodological knowledge to be types of scientific knowledge, as scientists do, then there are good reasons to be optimistic, rather than pessimistic, about the growth of scientific knowledge.

Acknowledgments

I consider myself fortunate to have been given the opportunity to pursue my graduate studies at the CUNY Graduate Center. I would like to thank all the members of the Philosophy Program—faculty, students, and staff—for making my time at the Graduate Center a memorable one. I will especially cherish the meetings of the Early Modern Philosophy reading group, with James Snyder and Damien DuPont, among others.

Special thanks are due to my advisor, Professor Catherine Wilson, from whom I have learned a great deal throughout my graduate education at the CUNY Graduate Center. I had the great pleasure of taking a number of courses with her, which helped set the stage for this dissertation. I am especially grateful to her for kindly agreeing to supervise my dissertation despite leaving CUNY for a new position at the University of Aberdeen. She made writing the dissertation enjoyable and the fact that she was overseas was hardly an obstacle.

I would also like to acknowledge Professor Clare Carroll from the Renaissance Studies Certificate Program, in whose course on Research Techniques in Renaissance Studies this project started to take shape. Although it has changed in many ways since then, this project got off the ground thanks to Prof. Carroll's initial support.

I have also greatly benefited from Professor Joseph Dauben's course on the Scientific Revolution. Throughout this course, Prof. Dauben made extensive comments on papers that later found their way into this dissertation. I am also grateful to him for kindly agreeing to serve on my dissertation committee despite being on leave.

Another committee member I am deeply indebted to is Professor Alberto Cordero. His influence on my philosophical development cannot be exaggerated. He was supportive not only throughout my graduate education but also throughout my undergraduate years. I have known Prof. Cordero for several years now. I cannot thank him enough for his constant support and sound advice.

I would also like to express my sincere gratitude to Professor Jonathan Adler and Professor Emily Michael for very useful comments on an earlier version of my proposal. Thanks to them, this project has developed in new and interesting ways. I am also grateful to Prof. Michael for giving me my first job as an adjunct at Brooklyn College.

Finally, this dissertation is dedicated to my wife, Moran Goldfarb, and to my son, Eli Mizrahi. “I couldn’t have done it without you,” is often said on occasions like this one. But you know, as well as I, that, in our case, this statement is true in more ways than the hackneyed one. Coming to a foreign country as a first-generation, non-traditional student was not easy. But we have managed to do so thanks to the love and encouragement of our parents, brothers, and sisters back home. By doing so, I hope we have blazed a trail for our children to take, if they so choose.

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Chapter 1
Introduction

1.a. The Problem

What is scientific progress? The idea that science is making progress is rather familiar. For example, here is how an article in the science section of *The New York Times* describes the work of astronomers and astrophysicists on dark energy:

> Astronomers have developed a smorgasbord of other ways of tracking the effect of dark energy. They have learned how to map the growth of clusters of galaxies, by analyzing how their gravity distorts the light from galaxies far behind them. […] Last year a committee from the National Academy of Sciences recommended that a dark energy observatory be the next mission in an astrophysics program called Beyond Einstein. […] Also in the works […] is a European mission known as Euclid, which could fly in 2017, if it is approved by the European Space Agency. NASA and the Department of Energy, working together, expect to make a final selection for the dark energy mission—known colloquially as J-dem for Joint Dark Energy Mission—next spring and launch it in the middle of the next decade. *That sounds like progress...* (Overbye, 2008, my emphasis).

This passage seems to be an illustration of the pre-theoretical and commonsensical idea that science is making progress. But this idea is not limited to popular science and reports about science in the media. Scientists themselves talk about progress all the time. Consider the following examples:

> It is not in the nature of things for any one man to make a sudden violent discovery; *science goes step by step*, and every man depends on the work of his predecessors. When you hear of a sudden unexpected discovery—a bolt from the blue, as it were—you can always be sure that it has grown up by the influence of one man on another, and it is this mutual influence which makes the enormous possibility of scientific advance. 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* (Rutherford, 1947, p. 178, my emphasis).

> The *increase of scientific knowledge* lies not only in the occasional milestones of science, but in the efforts of the very large body of men who with love and devotion observe and study nature. No single achievement in science is possible

> without the painstaking work of the many hundreds who have built the foundation on which all new work is based (Kusch, 1955, my emphasis).[1]
>
> *The aim of the scientist is, or should be, to extend the limits of human knowledge*. However, the roads open to him are many and he can render service in his chosen field in various ways. By developing fertile theories or hypotheses he may blaze new trails for human thought; by discovering unknown facts he may enrich our knowledge, and by inventing new technical devices and new methods he may forge new weapons for the arsenal of science. This last way is not the least important (Söderbaum, 1912, my emphasis).[2]
>
> The questions of the investigator to Nature and the yearning which fires his desire are directed in the first instance to *the gaining of new and deeper knowledge* (Mörner, 1904, my emphasis).[3]
>
> *Scientific knowledge is cumulative*; each individual builds on the accomplishments of others (Nathans, 1978, my emphasis).[4]
>
> [Science] is *the accumulation of humanity's organized, objective knowledge*, the first medium devised to unite people everywhere in common understanding (Wilson, E. O., 1999, p. 269, my emphasis).

The way in which scientists talk about progress and the aims of science raises the following questions: How do they use the term 'progress'? What do they mean when they say that progress has been made? What are their criteria for assessing progress? What do they take the aims of science to be?

According to Hasok Chang (2007), "Scientific progress remains one of the most significant issues in the philosophy of science today" (p. 1). This is partly because it seems rather odd to deny that science is making progress, and yet it is difficult to articulate in what sense exactly science is making progress. Even skeptics like Larry Laudan and Bas C. van Fraassen claim that they would like to "speak with the vulgar." By using Berkeley's phrase, I suppose they mean that they do not deny the pre-theoretical

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[1] Available at <http://nobelprize.org/nobel_prizes/physics/laureates/1955/kusch-speech.html>.
[2] Available at <http://nobelprize.org/nobel_prizes/chemistry/laureates/1912/press.html>.
[3] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1904/press.html>.
[4] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1978/nathans-speech.html>.

idea that science is making progress. Nevertheless, they argue for alternative accounts of progress, accounts that do not appeal to truth. For example, Laudan argues that scientific progress should be construed in functional terms (problem-solving) rather than semantic terms (truth, truthlikeness, verisimilitude). As for van Fraassen, although he has not developed an explicit account of scientific progress in terms of constructive empiricism, he argues that the business of science is "empirically adequate" theories, not true or truthlike theories. A theory is empirically adequate when what it says about the "observable" is true. Such theories merit the attitude of "acceptance" rather than belief. For van Fraassen (1980), "Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate" (p. 12).[5]

The problem of scientific progress, then, is to specify what constitutes scientific progress. More explicitly, since to say that a move from point *A* to point *B* constitutes progress is to say that point *B* is an improvement upon point *A* in some way, the way in which point *B* is better than point *A* must be spelled out. In other words, what are the criteria by which one determines whether or not the move from *A* to *B* constitutes progress? Here is how Peter Godfrey-Smith (2007) puts it:

> Progress requires that there is some *epistemically relevant quantity accumulated* as the process continues, or perhaps an increasingly close approach to a goal. […] *We need to be going somewhere worth going or collecting something worth collecting* (p. 14, my emphasis).

To specify criteria for progress, then, is to specify the values, aims, and goals that can serve as the constitutive criteria for progress in science, or the epistemically worthy quantity that is being accumulated.

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[5] According to Muller (2008), "The epistemic aim of science is empirical truth, i.e., the construction of (logically and) empirically adequate theories" (p. 149). I will revisit constructive empiricism in Chapter 5.

1.b. Methodology

In “The Concept of Observation in Science and Philosophy” (1982), Dudley Shapere shows how the term ‘observation’ is used by philosophers and scientists in rather different senses. He describes how astrophysicists use the phrase ‘direct observation’ when they talk about the core region of the sun. To philosophers, this use of the phrase ‘direct observation’ may seem either naïve or misleading. But Shapere thinks that it might be illuminating to analyze the astrophysicists’ use of the term. As Shapere (1982) writes:

> At the very least, it would be worthwhile to try to determine what does lie behind the astrophysicist’s way of putting his point about neutrinos, especially since philosophers of science have made such a point in recent years about the necessity of *understanding the way science actually proceeds* (p. 490, my emphasis).[6]

In this dissertation, I wish to examine the concept of scientific progress along similar, naturalistic lines. Let me say a bit more about what I mean by ‘naturalism’ here. Taking a naturalistic stance means articulating philosophical accounts about science that are historically informed. This idea is expressed by Imre Lakatos’ (1970) paraphrase of a Kantian thesis: “Philosophy of science without history of science is empty; history of science without philosophy of science is blind” (p. 91). More than simply being historically informed, however, naturalistic accounts of science must also do justice to the history of science and mesh with the actual practices of scientists. As Alexander Bird (2008) puts it:

> even if one thinks of history of science as descriptive and philosophy of science as normative, the former can be relevant to the latter in that, given certain assumptions about science in fact satisfying the norms (e.g., science is largely

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[6] According to Shapere (1982), “the question should be, not *whether* science is objective and rational, but rather in what, precisely, its objectivity and rationality consist. It is at the resolution of these issues, and therefore at an understanding of the nature of the scientific enterprise and its achievements, that the present analysis is ultimately directed” (p. 490).

> rational), *it had better be that the historian's description meshes with the philosopher's prescription* (p. 73, my emphasis).

What this means, at a minimum, is that if we want to answer questions, such as "What is science?" or "What is scientific progress?", then we have to look at the history of science and the actual practices of scientists.

These remarks point to the methodological aspect of naturalism that I am going to adopt in this dissertation. I find Leplin to be illuminating in this regard. According to Leplin (1997):

> increasingly in epistemology, and especially in the philosophy of science, theories have been redirected toward methods of inquiry that can be empirically assessed for their utility in advancing cognitive goals without prejudice as to whether they are believed by practitioners to advance these goals, or whether they fit popular preconceptions as to what knowledge is or how it is acquired. This development abandons philosophy's traditional claim to a priori status in favor of an essential continuity of philosophy with empirical science (p. 99).

By taking a naturalistic stance, then, we abandon first philosophy; philosophy thus becomes continuous with science. In that respect, another notable naturalist is Michael Devitt who acknowledges a debt to W. V. O. Quine's (1969) program of naturalized epistemology. According to Devitt (1996), the quest for certainty, solid foundations, and ultimate justification should be abandoned (p. 76).

As far as the methodology of this dissertation is concerned, taking a naturalistic stance means that an account of scientific progress is itself a scientific hypothesis. As Leplin (1997) puts it:

> The view that philosophical theories of science are to be judged against scientific practice, much as science is judged against natural phenomena, is a form of naturalism in epistemology. […] The idea is that epistemological theories actually are scientific theories, albeit of a highly abstract and general kind. *Just as scientific theories are responsible to the workings of the natural world, a theory about the assessment of scientific theories is a theory about evaluative practice in science, and is responsible to this practice*. According to naturalism, there is

> nothing else for it to be about, for there is no source of facts about how theories ought to be evaluated apart from scientific experience (p. 102, my emphasis).

Similarly, a theory about the assessment of scientific progress is a theory about evaluative practice in science, and should be judged relative to this practice.

Accordingly, I am interested in finding out the sense in which scientists use the term 'progress' and how they apply the concept in their assessments of scientific progress. To this end, I will examine the terms in which scientists describe progressive episodes in science. Since the notion of progress is goal-relative insofar as one's progress can be measured relative to the attainment (or facilitating the attainment) of one's aims and goals, I would have to examine what scientists take the aims of science to be and how they evaluate the attainment of their goals.[7] To make this investigation more manageable, however, I will focus on the life sciences. I will discuss a few major episodes of scientific change in the history of the biosciences, and then I will examine the judgments made by scientists themselves as to the progressive nature of these episodes. I wish to focus on the life sciences, rather than the physical sciences, because I think that they have been largely neglected by philosophers of science.[8] Philosophers have been drawing their examples and case histories from physics for the most part. So it might be worthwhile to look at other scientific disciplines as well. Perhaps it would give us a more complete, and perhaps accurate, picture of science.

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[7] Perhaps this project may be better conceived as a project in "experimental philosophy" (x-phi). Although I have not designed and carried out any experiments, I am mostly drawing on what scientists actually say about the aims of science and scientific progress in much the same way that experimental philosophers draw on what lay folk say about certain philosophical questions and thought-experiments (e.g., the trolley problem). Nevertheless, I do try to subject what scientists say about progress to critical scrutiny and to see whether what they say can be maintained as a plausible view about scientific progress. Karola Stotz had organized a symposium on the connections between x-phi and philosophy of science. Some of the papers presented at this symposium were published recently. See Stotz (2009).

[8] Of course, I am not talking about philosophers of biology, who are grappling with conceptual issues within biology, such as what is a species.

After I figure out how scientists conceive of scientific progress and what they take the aims of science to be, I will examine three types of philosophical accounts that purport to be accounts of scientific progress. I will argue that only one of them is successful, when judged against actual scientific practices, but it has to be amended in light of the historical investigation. I will try to revise this account of progress in such a way that it will do justice to the way scientists conceive of progress. The kind of progress I am interested in is what philosophers of science usually call "cognitive progress." Philosophers of science commonly distinguish between practical and cognitive progress. According to Philip Kitcher (1993), for example, practical progress has to do with the idea that

> science ought to contribute to 'the relief of man's estate,' it should enable us to control nature—or perhaps, where we cannot control, to predict, and so adjust our behavior to an uncooperative world—it should supply the means for improving the quality and duration of human lives, and so forth (p. 92).

According to Kitcher, practical progress has to do with pragmatic goals, such as manipulation and intervention for the benefit of mankind, whereas cognitive progress has to do with epistemic goals, primarily with truth. As Kitcher (1993) writes:

> The most obvious pure epistemic goal is truth. Indeed, talk of truth—or approximation to truth—has dominated philosophical discussions of scientific progress (pp. 93-94).

Kitcher goes on to distinguish between conceptual and explanatory progress and to give his account of progress in terms of significant truth. I will discuss Kitcher's account of progress in Chapter 3. For now, the important point is that the sort of progress I am concerned with in this dissertation is what Kitcher calls "cognitive progress."

Chapter 2
The Concept of Progress in Science

In Chapter 1, I said that taking a naturalistic stance with respect to the question of scientific progress means articulating an account of progress that would mesh with the history of science and do justice to actual scientific practices. The next task, then, is to look at the history of science and scientific practices. In what follows, I will discuss a few examples that illustrate the ways in which scientists talk about scientific progress and the aims of science, and their criteria for evaluating episodes of scientific change as progressive. I will discuss examples from Early Modern science and draw on sources such as the protocols and records of scientific societies of the period, primarily the Royal Society of London, and the writings and letters of Early Modern natural philosophers. Focusing on the life sciences, I will also discuss some work done by Nobel Laureates in physiology, serology, biochemistry, bacteriology, and immunology. I will suggest that these examples reveal what may be called the "scientific practice of assessing progress" as it is institutionalized by the Nobel Prize. By examining this scientific practice, I suggest, we can gain insight into the problem of scientific progress.

2.a. Early Modern Science

The importance of the biomedical sciences for Early Modern science was emphasized by Donald Bates. According to Bates:

> Medicine is historically central to the development of science, and it is only through its history that the question of the uniqueness of scientific knowledge can be answered.[9]

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[9] Quoted in Wilson, C. (2009), p. 85. Bates' manuscript, "Medicine and the Soul of Science," from which this quotation is taken, was not published.

Catherine Wilson elaborates on Bates' thesis and defends it against the standard view of the "Scientific Revolution" as dominated by mechanics and physics. According to Wilson (2009), those who held a scientific view during Early Modern times subscribed to these theses:

> *Emergence*: The manifest image present to the senses is the effect of an underlying material reality. Precise correlations can be established between the properties and dispositions of macroscopic substances and the latent, atomic reality underlying them.
>
> *Determinateness*: Macroscopic phenomena are only semi-orderly and are not fully predictable. Yet the motions of body as such can be precisely described, as can the interactions of certain classes of bodies.
>
> *Accessibility*: The hidden order, though invisible, can be visualized and represented by means of experimental techniques and instruments, and can be manipulated by humans.
>
> *Permissibility and Utility*: These manipulations are permissible; i.e., they are sanctioned by God and are not demonic. They can contribute to human welfare, reducing toil, pain, and suffering (p. 97).

Wilson then suggests that the significance of the biomedical sciences of the seventeenth century may have been underestimated as a result of their apparent failure to exemplify the determinateness and utility expected from science.

Moreover, in her book, *The Invisible World* (1995), Wilson argues for the significance of the microscope as an instrument of discovery in Early Modern science. As Wilson (1995) writes:

> I offer an 'inverted' picture, in which it is not the luminous and remote objects of celestial mechanics but scrapings, bodily fluids, bits of earth, and fragments of tissue that are located at the juncture of descriptive and speculative natural history, or protoscience, and modern science, and the juncture of occult and scientific mentalities. I have concentrated on some aspects of medicine and of what we now know as biology that have sometimes been regarded as laggards relative to the physical sciences, or even argued out of existence for the period in question (preface).

For present purposes, it is of no concern to us whether the life sciences were more or less important than the physical sciences and whether or not they contributed more or less than the physical sciences to the so-called “Scientific Revolution.” What matters is that the life sciences are acknowledged as a valuable source of insight into scientific progress. This is important to emphasize because it seems that philosophers of science neglect the life sciences to some extent and focus too heavily on the physical sciences, especially physics. I would like to suggest that it might be worthwhile to give some attention to the life sciences as well. Perhaps we might gain new insights into scientific progress.

Granted that the biomedical sciences were significant for the development of Early Modern science, I now turn to an examination of the aims of Early Modern science, as natural philosophers understood them. In what follows, I will examine two main sources where the prospects of finding scientists expressing their views about progress and the aims of science seem promising: the protocols and records of the Royal Society of London, and the writings and letters of natural philosophers of the period.

2.a.i The Royal Society of London

Beginning with Early Modern science, the first source of insight into scientific progress is the protocols and records of scientific societies of the period. To make this historical investigation more manageable, I will mainly focus on “The Royal Society of London for promoting Natural Knowledge,” whose First Charter (1662), granted by King Charles the Second, reads as follows:

> We have long and fully resolved with Ourself to extend not only the boundaries of the Empire, but also the very arts and sciences. Therefore we look with favour upon all forms of learning, but with particular grace we encourage philosophical studies, especially those which by actual experiments attempt either to shape out a new philosophy or to perfect the old. In order, therefore, that such studies, which have not hitherto been sufficiently brilliant in any part of the world, may shine

> conspicuously amongst our people, and that at length the whole world of letters may always recognize us not only as the Defender of the Faith, but also as the universal lover and patron of every kind of truth.[10]

According to Robert Hooke (1635-1703), 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—(not meddling with Divinity, Metaphysics, Morals, Politicks, Grammar, Rhetoric, or Logic)" (Hunter, 1981, p. 146). And Henry Oldenburg (c. 1619-1677), the first Secretary of the Royal Society, 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 & Hall, 1965-1986, IV, p. 168).

According to Hunter (1981), the Royal Society espoused a "Baconian programme" of experiment and record, as outlined by Oldenburg (p. 37). As Oldenburg wrote in a letter to van Dam (January 23, 1667):

> It is our business, in the first place, to scrutinize the whole of Nature and to investigate its activity and powers by means of observations and experiments; and then in course of time to hammer out a more solid philosophy and more ample amenities of civilization (Hall & Hall, 1965-1986, II, pp. 13-14).

According to Hunter, there was an expectation in scientific circles at the time that the link between science and economic life could be strengthened, so that national prosperity and useful, scientific knowledge would advance together. As a result, Oldenburg wrote to Rycaut (January 30, 1668), it would be feasible "to render England the Glory of the Western World, by making it the Seat of the best knowledge, as well as it may be the Seat of the greatest Trade" (Hall & Hall, 1965-1986, IV, p. 133). In addition to merchants

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[10] Available at <http://royalsociety.org/Charters-of-the-Royal-Society/>.

assisting intellectuals, another hope was that the findings of the new science would improve techniques in agriculture and industry (Hunter, 1981, p. 4).

The Royal Society was founded in the year of Charles II's Restoration and the inauguration of this scientific institution distinguishes natural philosophy after 1660 from its Interregnum precursors (Hunter, 1981, p. 32). According to Hunter (1981):

> The early Royal Society was undeniably significant in what might be called its 'definitional' capacity, which mattered as much in its own time as since. It was specifically devoted to the study of natural philosophy—to what was emerging as 'science' in a recognisably modern sense—centering on natural and mechanical problems but extending through the life sciences towards medicine and through chemistry and applied mathematics towards technology. Hence both hostile and favourable contemporaries used the Society's name as shorthand for the hopes and achievements of the new science as a whole (p. 32).

And according to Hunter and Wood (1986):

> The Royal Society was founded to promote a program of intellectual reform by collaborative endeavor along the lines suggested earlier in the seventeenth century by Francis Bacon. To this end, it was intended that a systematic programme of information collecting and empirical enquiry should be implemented. The idea was that the Society should itself be the forum for the production as well as the discussion of scientific findings, through experiments actually carried out at meetings or by the efforts of experimenters employed on the Society's behalf (p. 51).

Furthermore, as evident in Thomas Birch's (1705-1766) *History of the Royal Society* (1756-1757):

> no aspect of the Royal Society in its early years is more striking than the formality of its aspirations and activities, part of its ambition to be a real national institution focusing scientific enterprise rather than a mere intellectual social club. Most important was the granting of a royal charter in 1662 which was replaced by a second the following year: this made the body far more permanent and formal, for it now joined the ranks of the chartered corporations, with a constitution which had to be observed and which conferred rights and privileges (Hunter, 1981, pp. 35-36).

As stated in the Second Charter (1663), the activities of the Royal Society and its "studies are to be applied to further promoting by the authority of experiments the sciences of natural things and of useful arts, to the glory of God the Creator, and the advantage of the human race."[11]

Another account of its history, namely, Thomas Sprat's (1635-1713) *The History of the Institution, Design and Progress of the Royal Society of London for the Advancement of Experimental Philosophy* (1667), reveals the Society's commitment to "experimental philosophy." Its founders regarded the Society as a place for experiments, and they attributed great importance to the consensus to be achieved in evaluating their results with as many witnesses as possible. According to Hunter (1981), "One request for money to the landed classes of Ireland survives, which characteristically stresses the utility of the Society's design not only in improving natural knowledge by checking things hitherto taken on trust 'as also for perfecting all usefull mechanicall uses & practices'" (p. 38). Sprat's *History of the Royal Society* is marked with enthusiasm for improvements on the national level through trade and industry, which is his vision for the progress of science. This anticipation for the close connection between science, commerce, technology, and national prosperity, was shared by Samuel Hartlib (c. 1600-1662), most notably, whose circle formed one of the foundations of the Royal Society.[12]

In 1665, Henry Oldenburg inaugurated the *Philosophical Transactions* of the Royal Society.[13] Natural philosophers recognized its value and the journal became a success. As John Wallis (1616-1703) wrote to Oldenburg (March 29, 1677), it was valuable as "a proper place for communicating new discoveries" and as "a convenient *Register*, for the Bringing in, and Preserving many *Experiments*, which, not enough for a Book, would else be lost; and [they] have proved a very good Ferment for the setting

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[11] Available at <http://royalsociety.org/Charters-of-the-Royal-Society/>.

[12] On the Hartlib Circle, see Jacob (1975) and Webster (1975), pp. 446-465.

[13] On the *Philosophical Transactions*, see Kronick (1976) and Andrade (1965).

Men of Uncommon Thoughts in all parts a work" (Hunter, 1981, p. 52). Some rather esoteric articles on mathematics in Latin seem to indicate that many articles in the *Philosophical Transactions* were meant for practitioners in adjacent fields. However, it seems that the journal was also designed to reach a wider audience. As Oldenburg (April 2, 1669) explains to René François de Sluse (1622-1685), "these philosophical commonplace books are published in the English tongue because they are intended to be for the benefit of such Englishmen as are drawn to curious things, yet perhaps do not know Latin" (Hall & Hall, 1965-1986, V, pp. 469-470).[14] For Oldenburg, the goal of the *Philosophical Transactions* was not merely to record information "but also, and chiefly, to sollicite in all parts mutual Ayds and Collegiate endeavours for the farther advancement thereof" (Hunter, 1981, p. 53).[15] According to Hunter (1981), "Oldenburg consistently and fully expounded scientific aims and methods and, since these tracts bore the Society's name at their head and Oldenburg wrote letters on the Society's behalf, they naturally seemed to reflect its orthodox opinions" (p. 54). Oldenburg advocated a Baconian notion of the proper role of natural philosophy insofar as he valued the accumulation of natural histories while, at the same time, he did not underestimate the role of speculation and hypothesis. He considered the achievements of Robert Boyle, "that Noble Benefactor to Experimental Philosophy," as the embodiment of this notion of natural philosophy (Hunter, 1981, p. 54).[16]

Other scientific societies of the period, such as the *Accademia dei lincei*, the *Accademia del cimento*, the *Deutsche Akademie der Naturforscher Leopoldina*, and the *Academie royale des sciences*, prescribed similar aims. According to Zilsel (1945):

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[14] See also Hoboken to Oldenburg, July 29, 1672, in Hall & Hall (1965-1986), IX, p. 197.

[15] See the *Philosophical Transactions*, vol. I (1666).

[16] See also Kargon (1966).

> The progress of science through cooperation was the aim of these organizations. It is explained with particular clearness in the introduction to the first issue of the *Philosophical Transactions* (1666), edited by the *Royal Society*. Here Oldenburg, the secretary of the Society, states that the new periodical is being published “to the end that such Productions being clearly and truly communicated, desires after solid and useful knowledge may be further entertained … and those, addicted to … such matters, may be invited and encouraged to search, try, and find out new things, impart their knowledge to one another, and contribute what they can to the Grand design of improving Natural knowledge…. All for the Glory of God, the Honour and Advantage of these Kingdoms, and the Universal Good of Mankind” (p. 348).

In the case of the Royal Society, in particular, it seems that its early Fellows had Francis Bacon’s (1561-1626) Solomon’s House in mind as their model for a scientific society (Hunter & Wood, 1986). According to Zilsel (1945), “Francis Bacon proclaimed the advancement of knowledge for the benefit of mankind as the goal of the scientists” (p. 333).

According to Hunter (1981), Bacon’s importance to the early Royal Society was symbolized when he was enshrined as “Artium Instaurator” in the frontispiece of the Society’s first history (p. 14).[17] In the *New Atlantis*, Bacon outlines his ideal of a publicly financed research institution, “Solomon’s House,” for the cooperative study of nature. The aim of this institution is “knowledge of Causes, and secret motions of things; and the enlarging of the bounds of Human Empire, to the effecting of all things possible” (Hunter, 1981, p. 13).[18] According to Hunter (1981), following Bacon’s vision, “Restoration scientists were obsessed by the usefulness of their studies” (p. 87). In his *History of the Royal Society* (1667), Sprat expresses this concern for utility by contrasting the new science with the sterile scholastic philosophy: “While the Old could only bestow on us some barren Terms and Notions, the New shall impart to us the uses of all the

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[17] See also Sprat (1667).
[18] See also Spedding, et al. (1857-1874), III, p. 156.

Creatures, and shall inrich us with all the Benefits of *Fruitfulness* and *Plenty*" (Hunter, 1981, p. 87).[19] In the Restoration, according to Hunter, many shared Bacon's conviction that the advancement of learning had suffered in the past from the dissociation of "speculative men" and "men of experience." As Sprat writes, "it were to be wished (as that which would make Learning indeed solid & fruitfull) that *active* men would or could become writers', so that natural philosophy might directly ameliorate human life" (Hunter, 1981, p. 87).[20]

As Hunter points out, Joseph Glanvill's (1636-1680) *Plus Ultra: or, The Progress and Advancement of Knowledge Since the Days of Aristotle, In an Account of some of the most Remarkable Late Improvements of Practical, Useful Learning* (1668) is a useful illustration of how Glanvill and others at the time thought of the aims of the new science· In *Plus Ultra*, Glanvill (1668/1958) calls the kind of learning he is recording "practical" and "useful." Like others at the time, he emphasized above all its potential for increasing the "*conveniences of Life*" as well as "for the *advancement of Knowledge*" (p. 64). According to Hunter (1981), "this hope for the amelioration of life is intrinsic to the science of the time and almost all scientists considered technology germane to their interests, recognizing the intellectual value of pursuits formerly considered rather menial" (p. 9). Glanvill says that those whose achievements he describes aspired to what he calls "solid knowledge." This knowledge, which is the result of the rapid and genuine advances in natural philosophy during the previous century, is contrasted by Glanvill, as by Sprat, with the slow progress of knowledge that he associates with the former

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[19] See also Hunter (1975), p. 95.
[20] See also Hunter (1975), p. 95.

dominance of the philosophy of the medieval schoolmen that was based on the works of Aristotle (Hunter, 1981, p. 9).

Glanvill's aim was "to encourage the *freer* and *better* disposed Spirits, to *vigour* and *endeavour* in the *pursuits* of *Knowledge*; and to raise the *capable* and *ingenious*, from a *dull* and *drowsie acquiescence* in the *Discoveries* of former Times; by representing the great *Encouragements* we have to proceed, from *modern Helps* and *Advancements*" (Hunter, 1981, p. 10). Glanvill describes improvements of recent centuries, such as the printing press and the mariner's compass. He mentions such techniques as the invention of logarithms and innovative instruments that facilitated observations, such as the microscope, the telescope, the thermometer, the barometer, and the air-pump, which were developed around the early and mid seventeenth century and were becoming increasingly widespread in Glanvill's time. He discusses work in chemistry, "by which *Nature* is *unwound*, and *resolv'd* into the *minute Rudiments* of its *Composition*," alluding to the work of Robert Boyle (1627-1691) as well as work on statics and pneumatics (Hunter, 1981, p. 11). He also mentions advancements in "natural history," in the collection of accurate information, which he considers to be essential for progress. He considers Robert Hooke's *Micrographia* (1665), in which the details of the structures of bodies are revealed by the microscope, to be the most impressive example of this kind of progress. According to Hunter (1981), then, Glanvill's *Plus Ultra* "gives an idea of what scientific contemporaries saw as the major recent improvements of natural knowledge" (p. 11).

It is important to note, as Hunter points out, that there was a certain tension within the Royal Society between what might be called "serious" natural philosophers and the "amateur" virtuosi. On the one hand, there was a Baconian inclination to instructiveness and utility. On the other hand, there was an inclination to inconclusive and frivolous curiosity some Fellows condemned, stressing the need for judgment and practical knowledge.[21] According to Hunter (1981), it "has long been known that the Society's connections with the establishment gave it a social éclat which attracted many to its tanks for frivolous reasons, and membership is not necessarily proof of an active commitment to science" (p. 70).[22] For present purposes, however, what is important is that the Fellows of the Royal Society considered natural knowledge as their aim. That is, they considered natural and useful knowledge as the goal of scientific inquiry. And those who evaluated their achievements, such as Glanvill, Sprat, and Birch, did so on the basis of these epistemic criteria.

It is also important not to overestimate the emphasis on utility and useful knowledge in the period. As Hunter (1981) points out, "some contemporaries reacted against the overvaluation of utilitarian priorities in intellectual life that has been called 'vulgar Baconianism'" (p. 89). For example, Thomas Hobbes (1588-1679) complains that "not every one that brings from beyond seas a new gin, or other jaunty device, is therefore a philosopher" (Hunter, 1981, p. 89).[23] However, it is also noteworthy that advocates of the natural and useful knowledge of the Royal Society, such as Glanvill in *Plus Ultra*, tried to tout the merits of improved knowledge of the natural world just as much as its utilitarian applications. The first section of Robert Boyle's *Some Considerations touching the Usefulness of Experimental Naturall Philosophy* (1663-71)

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[21] See, e.g., De Beer (1955), III-IV and Nicolson (1965), chap. 1. See also Hooke on the virtuosi in Robinson & Adams (1935), p. 235. Cf. Houghton (1942).

[22] See also Hunter (1976), pp. 32-42.

[23] See also Purver (1967), xv and Molesworth (1839/1966), IV, p. 437. Cf. Shapin & Schaffer (1989).

deals with the value of knowledge about the natural world for its own sake as well as for religious enlightenment. Another contemporary praised the work of the Royal Society as being “to the Glory of God, the promoting of true & reall knowledge & both the Profit & reputation of our Kingdoms” (Hunter, 1981, p. 90).[24]

Despite the efforts of enthusiasts, such as Sprat and Birch, the Royal Society and the new science were criticized as trivial and insignificant. To mention one example, in Thomas Shadwell’s (c. 1642-1692) satire *The Virtuoso* (1676), Sir Nicholas Gimcrack tries to learn how to swim by lying on a table in his laboratory and imitating the motions of a frog that he placed in a bowl of water. He says that he would not dare practice swimming in water because he dislikes getting wet. “I content myself with the speculative part of swimming,” Gimcrack says, “I care not for the practical. I seldom bring anything to use; ‘tis not my way. Knowledge is my ultimate end” (Nicolson & Rodes, 1966, p. 47).[25] Robert Boyle’s studies on luminescence are also parodied when the virtuoso attempts to read the Geneva Bible by the light emitted by a rotting leg of pork. Robert Hooke is not spared by Shadwell as well when Gimcrack spends too much money on his microscopes while being questioned as to the significance of studying spiders and ants. To the question, “What does it concern a man to know the nature of an ant,” the reply is that “it concerns a virtuoso mightily; so it be knowledge, ‘tis not matter of what” (Nicolson & Rodes, 1966, p. 69).[26]

Such satires notwithstanding, Early Modern natural philosophers thought that it was within their power, using the sort of knowledge they were searching for, to improve human welfare. According to Hunter, Fellows of the Royal Society maintained that

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[24] See also Hunter & Davis (1999), II, pp. 5-63.
[25] Another example is Jonathan Swift’s *Gulliver’s Travels*; see Greenberg & Piper (1973).
[26] Cf. Gilde (1970).

understanding natural phenomena and a concern for utility are inseparable for the natural philosopher. As Boyle writes: “I shall not dare to think my self a true naturalist till my skill can make my garden yield better herbs and flowers, or my orchard better fruit, or my field better corn, or my dairy better cheese, than theirs that are strangers to physiology” (Hunter, 1981, p. 91).[27] The project for a great collaborative *History of Trades*, which was promoted by the Royal Society and which adopted Bacon’s technological counterpart of his projected natural history, is a testament to this pursuit of natural knowledge. According to Hunter, this project involved the collection of information about technical processes for its value in its own right and as a potential source of data for hypotheses. The expectation was that, by means of collaboration, improvements in one field could bring about advancements in others. As Sprat writes: “By this help the worst *Artificers* will be well instructed, by considering the *Methods*, and *Tools* of the best” (Sprat & Cowley, 1734, p. 310).[28] Boyle also developed such a program in *Some Considerations*, while William Petty (1623-1687), John Evelyn (1620-1706), and Oldenburg listed trades suitable for such a study (Hunter & Davis, 1999, III, pp. 442-456).[29]

In the case of Evelyn and silviculture, for instance, his *Sylva, or a Discourse of Forest-Trees and the Propagation of Timber in His Majesties Dominions* (1664) may be seen as a relatively successful collaboration. It was the result of work done by several Fellows of the Royal Society as a response to a request from the Commissioners of the Navy to improve the supply of timber. Evelyn’s task was to supervise the work and to use his eloquence as a writer to present it in a suitable form (Hunter, 1981, p. 93). This was

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[27] See also Hunter & Davis (1999), II, p. 64.

[28] See also Houghton (1941).

[29] See also Lansdowne (1927), I, pp. 205-207 and Sieveking (1923-4).

not unique. According to Hunter (1981), the *History of Trades* was successful in stimulating careful descriptions of industrial practices: "The archives of the Royal Society are full of detailed accounts of all sorts of techniques—from the making of candles and parchment to that of cheese and pitch—though certain themes tended to be stressed, such as dyeing, tanning, salt-making, the production of alum, the brewing of cider, mining, farming and agriculture" (pp. 91-92).[30]

The aspirations of Early Modern natural philosophers may be reasonably summed up with Hooke's urge to draw conclusions not only "for the finding out the operations of Nature" but also for finding out "how this art may be varyed or improvd either as to the materiall, on which they work or as to the instruments & manner of their working, or both" (Hunter, 1981, p. 103).[31] We should not forget, says Hunter, that, as admirable as their intentions were, most of the projects of the early Royal Society discussed above were quite disappointing relative to the aspirations of the Fellows (Hunter, 1981, p. 106). However, for present purposes, the important things to keep in mind are that a "commitment to technology continued throughout the Restoration period" (Hunter, 1981, p. 96), and that "Curiosity about technology characterized science throughout the period" (Hunter, 1981, p. 98). More importantly, the Fellows of the Royal Society "agreed that to achieve its avowed aim of improving natural knowledge, the Society needed to perform experiments, collect observations, maintain a correspondence network, and collate the writings of naturalists both ancient and modern" (Hunter & Wood, 1986, p. 65). Apparently, there was a tension between the "vulgar Baconians," such as William Neile (1637-1670), who thought that the task of the natural philosopher is to be confined to the

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[30] See also Birch (1756-1757), I, pp. 99-102.

[31] See also Tenison to Oldenburg, November 7, 1671 in Hall & Hall (1965-1986), VIII, p. 348.

cooperative collection of natural histories, and the "moderate Baconians," such as Hooke, who accepted cooperative and utilitarian goals, but insisted that inquiry into the causes of natural phenomena is also required. Nevertheless, they all agreed that "one of the chiefe ends of the society is to advance knowledge."[32]

2.a.ii. Harvey and the Circulation of the Blood

As we have seen, the Fellows of the Royal Society considered Bacon's Solomon's House as their model of a scientific society whose aim is "the knowledge of Causes, and secret motions of things; and the enlarging of the bounds of Human Empire, to the effecting of all things possible" (Spedding, et al., 1857-1874, III, p. 156). For Bacon, "Human knowledge and human power meet in one" (Spedding, et al., 1857-1874, IV, p. 47). Like others in the Early Modern period, Bacon was impressed by new discoveries and inventions, such as the compass, printing press, and gunpowder, which had beneficial effects for society. Accordingly, Bacon sought to find and employ new methods for finding this kind of useful knowledge, so that humanity would be endowed with more discoveries and powers (Spedding, et al., 1857-1874, IV, p. 79).[33]

Whether or not his methods were successfully followed by natural philosophers in the period, the Early Modern preoccupation with methodology was not limited to the works of Bacon. René Descartes (1596-1650), of course, is another familiar example. In the *Discourse on Method* (1637), Descartes writes:

> Because I planned to spend my entire life in the pursuit of this very essential knowledge, and because I had found a path which seems to me such that by following it one must infallibly discover this knowledge, were one not prevented from doing so either by the brevity of life or by lack of experiments, I judged that there was no better remedy against those two impediments than to communicate

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[32] William Neile, Royal Society, Domestic Manuscripts 5.11 (first item), *Proposalls humbly offered to better consideration*, quoted in Hunter & Wood (1986), p. 79.

[33] See also Sargent (2002).

> faithfully to the public the little that I had discovered, and to urge good minds to try to go further by contributing, each according to his inclination and ability, to the experiments which would have to be performed, and also by communicating all the things they learned to the public (Olscamp, 2001, p. 51).

By applying his method, Descartes says,

> it is possible to arrive at *knowledge which is very useful in this life*, and that instead of that speculative philosophy taught in the schools, we can discover a practical one, through which, knowing the force and action of fire, water, air, the stars, the heavens, and all the other bodies which surround us, as distinctly as we know the different skills of our artisans, we can use them in the same way for all the purposes to which they are suited, and to make ourselves the masters and possessors, as it were, of nature (Olscamp, 2001, p. 50, my emphasis).

Again, whether or not his method is successful is not important for present purposes. What is important is that Descartes conceives of the aims of inquiry in terms of the attainment of knowledge that is not only intellectually satisfying but also practical and useful. Advancing knowledge, Descartes realizes, would take the distinct contributions of numerous individuals over a long period of time.

Since Descartes' writings on method were studied in great detail, I would like to discuss another example that has received less attention in this regard. In *Exercitatio Anatomica de Motu Cordis et Sanguinis in Animalibus* (1628), William Harvey (1578-1657) writes in his dedication to the President of the Royal College of Physicians:

> I profess both to learn and to teach anatomy, not from books but from dissections; not from the positions of philosophers but from the fabric of nature; and then because I do not think it right or proper to strive to take from the ancients any honour that is their due, nor yet to dispute with the moderns, and enter into controversy with those who have excelled in anatomy and been my teachers. I would not charge with willful falsehood anyone who was sincerely anxious for truth, nor lay it to anyone's door as a crime that he had fallen into error. I avow myself the partisan of truth alone; and I can indeed say that I have used all my endeavours, bestowed all my pains on an attempt to produce something that should be agreeable to the good, profitable to the learned, and useful to letters (Bowie, 1889, pp. 6-7).

This passage seems to capture some of methodological themes associated with the so-called "Scientific Revolution":

(SR1) The dissatisfaction with absolute reliance on the authority of the ancients;

(SR2) The emphasis on studying the book of nature rather than the books of the ancients;

(SR3) The need for a method to distinguish truth from falsehood and to eliminate error;

(SR4) The realization that the pursuit of knowledge is not easy and that it may lead to dead-ends occasionally;

(SR5) The insight that setting modest epistemic goals that can actually be achieved is a reason for epistemic optimism rather than skepticism.[34]

Whether or not this period may be correctly characterized as a "Scientific Revolution" is of no importance for present purposes. It is important to recognize, however, that the Early Modern period was a period of (proto) scientific change. As Wilson (1995) writes:

> In the early mid-seventeenth century, Western Europe was the site of theoretical and technological achievements that determined its later economic, military, and cultural history. Scientific societies were founded and journals established; new mathematical techniques, notably analysis and algebraic geometry, were invented and applied to physical problems. Forces and masses were quantified, orbital motion analyzed to determine its components, and free fall brought under the rule of law. The Copernican system won widespread acceptance, and the circulation of the blood was established. Instruments for the observation of very distant or very small objects were put into use, as were newly constructed apparatuses for hydrostatic and barometric experiments. A self-consciousness about the study of nature and its benefits, humanitarian and theological, emerged. New discursive forms, the experimental narrative and the apologia for scientific activity, made their appearance (p. 3).

More importantly, it is important to recognize that Early Modern natural philosophers were concerned with matters of methodology. Arguably, one of the insights of the Early

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[34] Cf. Wilson (2008).

Modern natural philosophers seems to have been the realization that setting epistemic goals that are impossible to achieve (e.g., grandiose projects of universal science) are a recipe for despair. Being modest about one's epistemic goals, however, and attaining them in a piecemeal fashion, is a reason to be optimistic about the pursuit of knowledge and its progressive growth. The work of Harvey seems to embody some of these methodological themes.

According to Debus (1954), "the discovery of the circulation of the blood may be easily cast in terms of the progressive growth of knowledge" (p. 72). However, this characterization would be somewhat inaccurate, according to Debus, because "one is faced with the seeming paradox that one of the most impressive achievements of the Scientific Revolution was accomplished by a professed Aristotelian and that his work appealed first to mystical Hermiticists" (p. 73).[35] It is true that Harvey may be characterized as an Aristotelian insofar as his discovery is in accordance with Aristotle's doctrine of the supremacy of the heart. This fact in itself, however, does not seem to undermine Harvey's contributions to the progressive growth of knowledge in the life sciences. By the same token, one might argue that Harvey was a Galenist, for his work seems to be situated in the Galenic tradition to the extent that it addresses questions derived from Galen's work.

Early Modern physiologists and anatomists faced the following questions, which were an inheritance from Galen (c. 129-200 C.E.): (a) How does blood flow from the

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[35] As an example of mystical claims about the importance of the heart, Debus cites the following passage from Harvey's dedication to Prince Charles: "The animal's heart is the basis of its life, its chief member, the sun of its microcosm; on the heart all its activity depends, from the heart all its liveliness and strength arise" [Franklin (1963)]. Whether this is mystical or simply metaphorical, as M. B. Hall (1962) points out, these claims might lead one to attribute mystical significance to Harvey's work, or to interpret Harvey's discovery as motivated by mysticism, only if one fails to realize that "the mysticism is an enthusiasm which led Harvey to investigate the function of the heart in an eminently non-mystic fashion" (p. 285). Cf. Westfall (1971), pp. 90-92.

right ventricle to the left? (b) Does the venous artery contain blood or air? According to Galen, the veins and arteries are two distinct systems. The only point of communication is the permeable septum in the heart. The problem was that Galen's conviction that the septum is permeable is based more on speculation than empirical evidence. Apparently, Andreas Vesalius (1514-1564) was aware of this problem. In his *De Humani corporis fabrica* (1543), he writes:

> The septum of the ventricles, therefore, is […] made out of the thickest substances of the heart and on both sides is plentifully supplied with small pits which occasion its presenting an uneven surface towards the ventricles. Of these pits not one (at least in so far as is perceptible to the senses) penetrates from the right ventricle to the left, so that we are greatly forced to wonder at the skill of the Artificer of all things by which the blood sweats through passages that are invisible to sight from the right ventricle to the left (O'Malley, 1965).

And in the 1555 edition, Vesalius writes:

> Nevertheless, howsoever conspicuous these pits may be, not one of them, in so far as is perceptible to the senses, penetrates through the ventricular septum from the right to the left ventricle. Indeed, I have never come upon even the most obscure passages by which the septum of the heart is traversed, albeit that these passages are recounted in detail by professors of anatomy seeing that they are utterly convinced that the blood is received into the left ventricle from the right. And so it is (how and why I will advise you more plainly elsewhere), that I am not a little in two minds about the office of the heart in this respect (O'Malley, 1965).

For Galen, much of the blood in the left ventricle comes from the right ventricle through these pores or pits in the septum. As Vesalius points out, however, no such pores or pits could be detected in the septum.

Following Vesalius, other anatomists and physiologists became increasingly dissatisfied with the Galenist account of the existence of such pits. To name one of them, Realdus Columbus (c. 1516-1559) describes the pulmonary route of the blood from the right ventricle to the left in *De re anatomica libri xv* (1559):

> Between these ventricles there is placed the septum through which almost all authors think there is a way open from the right to the left ventricle; and according to them the blood is in transit rendered thin by the generation of the vital spirits in order that the passage may take place more easily. But these authors make a great mistake; for the blood is carried by the artery-like vein to the lungs and being there made thin is brought back thence together with air by the vein-like artery to the left ventricle of the heart (Hall, 1962, p. 273).

Although Juan Valverde da Hamusco (c. 1525-1588) was the first to publish experimental work on the minor circulation of the blood, the work of Columbus was widely read (Hall, 1962, p. 274).

By the time Harvey began his work on the circulation of the blood, the problems, if not the rudiments of a solution, were already set for him by his predecessors. After all, in addition to Valverde and Columbus' work on the minor circulation, Fabricius' (1537-1619) *De venarum ostiolis* (1603) was known to Harvey. As a student of Fabricius at the University of Padua, Harvey was likely influenced by Fabricius' work, even though Fabricius did not publish his work until 1603 (Debus, 1954, pp. 63-65). For Fabricius, these *ostiola* are little doors that obstruct the flow of blood, so that the veins are not ruptured. Indeed, Harvey acknowledges a debt to his predecessors. In the introduction to the *De motu*, Harvey writes:

> It profits one who is pondering on the movement, pulsation, performance, function, and services of the heart and arteries to read what his predecessors have written, and to note the general trend of opinion handed on by them. For by so doing he can confirm their correct statements, and through anatomical dissection, manifold experiments, and persistent careful observation emend their wrong ones (Franklin, 1963, p. 7).

So, according to Harvey, one should not assume that the claims of one's predecessors must be wrong merely by virtue of being ancient. Rather, one should examine them carefully, by meticulous observations and repeated experiments, in order to confirm those that are true and reject those that are false.

Harvey emphasizes this methodological principle again in his *Anatomical Disquisitions on the Circulation of the Blood* (1648). In this work, Harvey responds to objections made by the French physician, Jean Riolan the Younger (1577-1657). As Harvey writes:

> …there is no science which does not spring from pre-existing knowledge, and no certain and definite idea which has not derived its origin from the senses (Bowie, 1889, p. 93).

This passage seems to reveal not only Harvey's empirical approach but also his commitment to a piecemeal approach to the growth of knowledge. As for the growth of knowledge, the emendation of previous work, such as Galen's, is a modest epistemic goal. According to Harvey, it is easier to make progress by building on the work of one's predecessors than starting from scratch.

As for the empirical approach, it seems that Harvey is careful in his speculations and modest in his claims about the unobserved "capillaries." Gillispie (1960) argues that Harvey postulates the existence of capillaries, even though he could not observe them (p. 72). But, as Elkana and Goodfield (1968) show, "Harvey was aware that the circulation was completed *by some means or other*; but throughout his life he was in considerable doubt as to how this was actually accomplished" (p. 62, original emphasis). Harvey's reluctance to posit the existence of unobserved capillaries is evident in his replies to Riolan's objections. As Harvey writes:

> …neither our learned author himself [i.e., Riolan], nor Galen, nor any experience, has ever succeeded in making such anastomoses as he imagines, sensible to the eye (Bowie, 1889, p. 107).

Harvey continues:

> I can therefore boldly affirm, that there is neither any anastomosis of the vena portae with the cava, of the arteries with the veins, or of the capillary

> ramifications of the biliary ducts, which can be traced through the entire liver, with the veins (Bowie, 1889, p. 108).

So, according to his own accounts of the discovery of the circulation of the blood, it seems that Harvey was looking for such anastomoses as Galen’s theory required. In doing so, he was clearly working within the framework of questions set for him by his predecessors. However, unlike Riolan, he was not dogmatic in his dependence on the authority of Galen, and he was willing to emend Galen’s theory in light of empirical evidence. As Harvey writes:

> True philosophers, who are only eager for truth and knowledge, never regard themselves as already so thoroughly informed, [so that they do not] welcome information from whomsoever and from wheresoever it may come; nor are they so narrow-minded as to imagine any of the arts or sciences transmitted to us by the ancients, in such a state of forwardness or completeness that nothing is left for the ingenuity or industry of others. On the contrary, very many maintain that all we know is still infinitely less than all that remains unknown. [Nor] do philosophers pin their faith to others’ precepts in such [ways] as they lose their liberty, and cease to give credence to the conclusions of their proper senses. Neither do they swear such fealty to their mistress Antiquity, that they openly, and in sight of all, deny and desert their friend, Truth (Schultz, 2002, p. 179).

After Harvey’s death, Marcello Malpighi (1628-1694), studying the lungs of frogs using a microscope, showed that veins and arteries are joined by anastomoses (Elkana & Goodfield, 1968).[36] Here is what Oldenburg wrote to Malpighi about the latter’s work (March 25, 1669):

> Our company of philosophers thinks that you are treading the real paths leading to a true knowledge of nature’s secret places when you put aside the empty arguments of the schools’ theory for pursuing almost nothing but generalities, and devote your mind and hands to observing accurately and eviscerating minutely the things themselves (Wilson, 1995, p. 35).

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[36] See also Allchin (2005).

For Early Modern natural philosophers, then, the aim of their inquiry into the workings of nature is what they call “natural knowledge,” by which they meant knowledge that is not only valuable for its own sake but also useful.

2.b. Modern Science

In addition to the history of Early Modern science, a different, more contemporary source of insight into scientific progress is the institution of the Nobel Prize. The Nobel Lectures contain several references to scientific progress and the aims of science. The institution of the Nobel Prize seems to be particularly revealing in this respect because it is a setting in which scientists reward their colleagues for what they take to be important contributions to science. It would seem to be illuminating, then, to look at the grounds on which they reward their peers and the criteria based on which they judge certain contributions as progressive.

There are many interesting discoveries documented in the Nobel Lectures that one might wish to examine. However, in keeping with my focus on the biosciences, I would like to examine the reasons why the following Nobel Laureates were awarded the Nobel Prize in Physiology or Medicine: Charles Luis Alphonse Laveran (1845-1922), Ilya Metchnikoff (1845-1916), Paul Ehrlich (1854-1915), Karl Landsteiner (1868-1943), Ivan Petrovich Pavlov (1849-1936), and Hans Adolf Krebs (1900-1981). On what grounds were their discoveries judged as advances in their fields? I think that these cases might shed some light on the question of scientific progress, at least as far as the biosciences are concerned, for they are instances in which the work of a few scientists was evaluated by their colleagues as progressive. It would be interesting to find out, I suggest, on what grounds these evaluative judgments were made.

2.b.i. Laveran on the Role Played by Protozoa in Causing Disease

In 1907, Charles Louis Alphonse Laveran (1845-1922) was awarded the Nobel Prize in Physiology or Medicine "in recognition of his work on the role played by protozoa in causing diseases." The Presentation Speech for the 1907 Nobel Prize in Physiology or Medicine was supposed to be delivered by C. Sundberg, member of the staff of professors at the Karolinska Institutet. Although Sundberg (1907) did not deliver his speech, since the presentation ceremony was canceled because of the death of King Oscar II, he wrote the following:

> To appreciate properly the importance of Laveran's investigations into the protozoan causes of disease, one must remember *the state of this branch of science at the time* of Laveran's earliest work, i.e. about 1880. The *body of knowledge* relating to the causes of infectious diseases was making rapid *progress* at that time in the field of bacteriology. Pasteur's "Theory of Germs" had provided the key to the riddle of fermentation processes, and its relevance to infectious diseases had been grasped. So several pathogenic bacteria had been discovered by 1880: those of anthrax and relapsing fever; other germs, such as those causing tuberculosis, glanders, pneumonia, typhoid fever, diphtheria, tetanus, Asiatic cholera, traumatic fevers, etc. were discovered one after another during the years 1880-90. All these germs were found to belong to the last category of the plant kingdom, the bacteria (my emphasis).[37]

According to Sundberg, then, in order to appreciate the significance of a scientific discovery, one must compare the "body of knowledge" in the relevant field prior to the discovery to the one after it. What was known before Laveran's work?

The germ theory of disease has a long history. During the seventeenth century, the discovery of microorganisms was made possible as a result of the development of the microscope. In 1675, Antonie van Leeuwenhoek (1632-1723) wrote to the Royal Society about his observations of "little animals." As Leeuwenhoek writes:

> I discovered new living creatures in rain, which had stood but a few days in a new tub, that was painted blue within. This observation provoked me to investigate

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[37] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1907/press.html>.

> this water more narrowly; and especially because these little animals were, to my eye, more than ten thousand times smaller than the animalcule which Swammerdam has portrayed and called by the same of Water-flea or Water-louse, which you can see alive and moving in water with the bare eye (Wilson, 1995, pp. 88-89).[38]

Leeuwenhoek was apparently inspired by Robert Hooke's *Micrographia* (1665). Whereas Hooke was using a compound microscope in his observations, Leeuwenhoek was using single lens microscopes. Nevertheless, it seems that his lens grinding skills and acute eyesight compensated for his use of less sophisticated instruments (Wilson, 1995, p. 93). On September 17, 1683, Leeuwenhoek wrote to the Royal Society about his observations on the plaque between his teeth, "a little white matter, which is as thick as if it 'twere batter." He repeated these observations with two ladies and two old men who had never cleaned their teeth. As Leeuwenhoek reports: "I then most always saw, with great wonder, that in the said matter there were many very little living animalcules, very prettily a-moving. The biggest sort […] had a very strong and swift motion, and shot through the water (or spittle) like a pike does through the water. The second sort […] oft-times spun round like a top […] and these were far more in number." In the sample taken from the mouth of one of the old men, Leeuwenhoek found "an unbelievably great company of living animalcules, a-swimming more nimbly than any I had ever seen up to this time. The biggest sort […] bent their body into curves in going forwards. […] Moreover, the other animalcules were in such enormous numbers, that all the water […] seemed to be alive" (Fred, 1932, p. 10).[39]

Leeuwenhoek wrote about his work in his letters. For example, in this letter from June 12, 1716, he writes about his pursuit of knowledge:

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[38] See Leewenhoeck (1677-1678), pp. 821-831. The spelling of Leeuwenhoek's name was changed in this publication.

[39] See also Ford (1991).

> my work, which I've done for a long time, was not pursued in order to gain the praise I now enjoy, but chiefly from a *craving after knowledge*, which I notice resides in me more than in most other men. And therewithal, whenever I found out anything remarkable, I have thought it my duty to put down my discovery on paper, so that all ingenious people might be informed thereof (Palm, 1999, my emphasis).

And in a letter from March 21, 1698, he writes about the aim of this pursuit:

> what I have written is only *for the improvement of medical knowledge*, and as an incentive to the laborious enquirers after new discovers in science, the field of which, is indeed indefinite (Hoole, 1800, p. 160, my emphasis).[40]

This "craving after knowledge" also led Leeuwenhoek in 1698 to demonstrate the circulation of the blood in the capillaries of an eel.

Subsequently, in 1835, Agostino Maria Bassi (1773-1856) showed that a minute fungus caused muscardine disease in silkworms. He argued that "All infections, of whatever type, with no exceptions, are products of parasitic beings; that is, by living organisms that enter in other living organisms, in which they find nourishment, that is, food that suits them, here they hatch, grow and reproduce themselves" (Mazzarello, 1999, p. 19). And Casimir Joseph Davaine (1812-1882) reported observing rod-shaped structures, which he named "bacteridia," in the blood of organisms that had died as a result of anthrax.

In 1840, Friedrich Gustav Jacob Henle (1809-1885) laid out the theoretical framework for the germ theory of disease by articulating four criteria that usually suffice to confirm the causal relation of an agent or pathogenic organism to an infectious disease. These criteria are the following: (a) the agent must be present in each case of the disease; (b) the agent is not present in other diseases; (c) after isolation in culture, the agent must be able to produce the disease in experimental animals. Heinrich Hermann Robert Koch

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[40] The translator notes that this is taken from a letter written to Leeuwenhoek by G. Bidloo on March 21, 1698.

(1843-1910) later adopted these criteria and added a fourth one, namely, that the agent can be recovered from the experimental animal. These four criteria came to be known as “Koch’s Postulates.”

In 1864, Louis Pasteur (1822-1895) showed that putrefaction and fermentation were caused by microorganisms. He thus rejected erroneous beliefs about spontaneous generation by demonstrating the absence of microbial growth after heat inactivation. His realization that the processes of putrefaction and fermentation by microorganisms occur not only in organic material but also in the living body provided the foundation for the germ theory of disease. Koch identified the causative organism of anthrax (1876), and later those of tuberculosis and cholera, while Pasteur found in 1879 that pathogenic microbes could be rendered less virulent by changing their environment (e.g., by exposing them to oxygen) for several generations of growth. Pasteur tried injecting weakened strains into animals in an attempt to produce immunity to the virulent strain. In doing so, he developed vaccines for chicken cholera (1880), anthrax (1881), and rabies (1885). By 1900, the bacterial causes of several diseases had been discovered. By that time, it was commonly accepted that every disease has its causal bacterium.

Against this background, Laveran’s contribution begins with his work on malaria. While he was serving as an army doctor in Algeria, he used to perform autopsies on malaria victims. He noticed that they had numerous pigmented bodies in their blood. More importantly, he noticed that some of these bodies were in the red blood cells, whereas others moved freely with the help of movable filaments or flagella. The rapid and varied movements of these flagella, which emerged from a clear spherical body, suggested to Laveran that these were parasites. He named these parasites *Oscillaria* *malariae*. Out of 192 blood specimens from malaria patients he had examined, these parasites were present in 148 specimens. At first, Laveran's discovery was met with resistance. In 1882, he visited Rome and showed his findings to Marchiafava, Celli, and Corrado Tomassi-Crudeli. They were not impressed, however, and thought that the spherical bodies identified by Laveran were degenerative changes in the red blood cells. But later, when they examined fresh preparations in which the movements of the parasites could be observed, they accepted that these were indeed parasites. They did not credit Laveran, however, because they thought that they were observing parasites that did not resemble those identified by Laveran, and so they named them *Plasmodium malariae*. By 1885, others were able to record the development of these parasites within the red blood cells and the accumulation of pigment using more powerful microscopes with oil immersion lenses (Sherman, 1998, pp. 5-6).

Laveran gives his account of the role played by protozoa in causing disease in his *Treatise on Marsh Fevers* (1884). In his Nobel Lecture (1907), he said the following:

> The *knowledge* of these new pathogenic agents has thrown a strong light on a large number of formerly obscure questions. The *progress* attained shows once more how just is the celebrated axiom formulated by Bacon: "Bene est scire, per causas scire" (my emphasis).[41]

K. A. H. Mörner, Rector of the Karolinska Institutet, made a similar point in his Presentation Speech when Robert Koch was awarded the Nobel Prize in Physiology or Medicine in 1905 for his work on tuberculosis. As Mörner (1905) said:

> To make Koch's significance in the development of bacteriology clear, one must look at the situation with which Koch was confronted when he made his appearance. Pasteur had indeed already published by then his epoch-making work, which laid the foundations of bacteriology, and medical art had already gathered in one very beneficial fruit which stemmed from this work, namely the antiseptic method of treating wounds proposed by Lister. However, the trail was

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[41] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1907/laveran-lecture.html>.

> yet to be blazed, which bacteriological research had followed with such success during recent decades, *to discover the causes of individual diseases and to look for the means of combating them*. Koch was a pioneer in this (my emphasis).[42]

Mörner (1905) continued:

> It is true that there were good grounds for *supposing* that certain other diseases were caused by micro-organisms. But *detailed knowledge* concerning this was lacking, and experimental findings were very divergent (my emphasis).

In other words, the evidence in support of the claim that other diseases were caused by microorganisms, just as anthrax and typhus are, was not compelling at the time before Koch's work. When Koch came along, however, he "not only set himself the task of examining the problem of whether diseases were caused by bacteria," according to Mörner (1905), "but also endeavored to discover the special micro-organisms of the particular diseases and to get *to know more* about them" (my emphasis).

According to Mörner, then, after it was known that infectious diseases are the result of harmful microorganisms that invade the body, the next step was to find the causative agents of specific diseases. Koch, for instance, managed to do that with anthrax, tuberculosis, and cholera. Laveran made further progress by identifying the causative agent of malaria. More specifically, he showed that malaria is an infectious disease that is caused by parasitic protozoa of the genus *Plasmodium* (or *Oscillaria malariae*, as he called them) within the red blood cells.

2.b.ii. Metchnikoff and Ehrlich's Work on Immunity

In 1908, Elie Metchnikoff (also known as Ilya Mechnikov) (1845-1916) and Paul Ehrlich (1854-1915) were awarded the Nobel Prize in Physiology or Medicine for their work on immunity. The Presentation Speech was delivered by K. A. H. Mörner. According to Mörner (1908):

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[42] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1905/press.html>.

> the possibility of obtaining protection against certain diseases has long been *known*, for the observation has been made that in many cases organisms which have gone through an infectious disease will acquire protection against being attacked by the same disease again. We then say that the organism has acquired immunity against the disease in question (my emphasis).[43]

But this knowledge was not sufficient, according to Mörner. What was missing?

> In scientific development, however, there is a very big step between this observation and a *real knowledge* of the changes which have taken place in the organism through immunization, and it is also a very big step from the same observation to the ability, consciously and without the danger attendant upon the course of the illness, to give the organism such powers of resistance. It was, therefore, rightly considered to be an epoch-making and blessed moment in the history of medicine when Edward Jenner introduced, more than a hundred years ago, protective vaccination with cow-pox substance which can give immunity against a disease, namely smallpox, the ravages of which the present generation can hardly imagine. Great though the practical importance of Jenner's discovery was, it did not advance the development of the study of immunity in respect of other diseases or permit of any deep penetration into the problem of immunity generally. The prerequisites for a successful scientific elaboration of the study of immunity were still missing. The first and most important condition for making the problem of immunity the subject of real scientific research was namely to establish the cause of disease. It was the revolutionary work of Pasteur and Koch which was done during approximately three quarters of a century after Jenner's discovery which laid the necessary foundation for the present important development of the study of immunity (Mörner, 1908, my emphasis).

So, according to Mörner, Jenner made progress when he discovered that cowpox confers immunity to smallpox, and then Koch and Pasteur improved on Jenner by realizing that infectious diseases are caused by microorganisms. How, then, did Metchnikoff improve on Koch and Pasteur?

> Elie Metchnikoff was the first to take up consciously and purposefully, by means of experiments, the study of the question so fundamental to the question of immunity; by what means does the organism vanquish the disease-bearing microbes attacking the organism in which they have succeeded in establishing themselves and developing? At first his experiments were restricted to the lower animals. This was the case in his important work concerning a kind of infection in certain microscopic aquatic animals, the so-called water fleas. If the guiding principles behind these investigations were not known they could appear to be remote from any medical interest. They were, however, the first links in a chain of

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[43] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1908/press.html>.

> investigations leading to phenomena of immunity, also in mammals and in humans. These investigations opened the way for Metchnikoff's theory of phagocytosis. According to this theory, the microorganisms are destroyed by the activity of cells in the organism. Certain kinds of cells in the bodies of humans and animals, namely, are supposed to have, in addition to other functions, the task of catching and destroying disease-producing microbes which have succeeded in penetrating the organism, and also of rendering certain bacterial poisons harmless (Mörner, 1908).

According to Mörner, Metchnikoff improved on Koch and Pasteur by uncovering some of the mechanisms by which living organisms fight off virulent microbes. Thanks to Jenner, it was known that cowpox confers immunity to smallpox, but since no one knew how it works, this useful bit of knowledge could not be replicated in the case of other diseases. Thanks to Koch and Pasteur, however, the underlying mechanisms of disease causation were partially uncovered, and thus other vaccines were developed using this knowledge. Now, due to Metchnikoff, we know not only about the causes of diseases but also how living bodies respond and fight off disease-producing microbes. In his Nobel Lecture, Metchnikoff (1908) said, "I take heart from the thought that it was the intention of the generous founder Alfred Nobel to reward men of learning who give their lives to knowledge without deriving any benefit from its practical applications."[44]

Metchnikoff shared the Nobel Prize in Physiology or Medicine with Paul Ehrlich. "A man," according to Mörner (1908), "who has been responsible for important scientific progress as organizer and leader in this field deserves to be mentioned among the first of those who have dedicated themselves to a study of immunity." How did Ehrlich improve on Pasteur, Koch, and others? Ehrlich was awarded the Nobel Prize for his work on serum therapy for diphtheria and antibodies. Ehrlich discovered that poisons that are generated by bacteria occasionally produce certain elements in the organism that have an

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[44] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1908/mechnikov-lecture.html>.

antagonistic effect, one that is directed against the substance that caused the production of the elements. This process is known as the formation of antibodies. After immunity has been achieved, such antibodies are found in the organism. These antibodies provide protection not only against disease-producing microorganisms but also against the toxic products of these microorganisms (Ehrlich, 1908).[45]

According to Mörner and Ehrlich, the latter improved on Koch and Pasteur by adding to our knowledge about immunity. In particular, we know that there are two kinds of immunological responses in living organisms. One consists in the ability to destroy microbes or to inhibit their development. The other consists in immunity against the poisons that microbes produce and distribute in a living body. Metchnikoff's work contributed to our knowledge of the former, i.e., about bacteria-destroying immunity. Ehrlich's work contributed to our knowledge about the latter, i.e., about poison immunity. Thanks to Ehrlich, we know that cells have receptors (what Ehrlich called "the side-chains") that normally function to capture nutrients. The receptors could be blocked by toxin, which would cause the cell to repair itself by producing an excess of new side-chains, which would be shed into the serum and formed serum antitoxin. Toxin and antitoxin would bind irreversibly on contact. According to Ehrlich, receptors for every possible antigen were already present in the normal animal. This aspect of his side-chain theory, however, was met with some resistance. Critics objected that the theory required too many specific substances. An alternative was proposed by Karl Landsteiner (1868-1943). According to Landsteiner, specificity is approximate rather than absolute, i.e., it is a matter of the closeness of fit, which is determined by the charge outline of antigens. The antigen-antibody reaction is reversible. On this view, antibody might be formed by

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[45] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1908/ehrlich-lecture.pdf>.

assembling protein molecules on antigen as template, as Felix Haurowitz (1896-1987) suggested in 1928. Contrary to Ehrlich’s theory, Landsteiner’s view required no preformed antibody. Despite these disagreements, however, Ehrlich’s terminology and his diagrams of antibodies remain remarkably close to the present view of the formation of antibodies.

It is noteworthy that, although they shared the Nobel Prize in Physiology or Medicine in 1908, Metchnikoff and Ehrlich did not work together, and their theories were initially thought to be at odds. The disputes between the “humoral school” and the “cellular school” were notoriously heated, though they never degenerated into fights. In fact, that was the nature of the rivalry between Ehrlich and Metchnikoff, strictly professional and never personal. According to the humoral school, to which Ehrlich belonged, immunity is due to a substance—a humoral factor—in the blood. According to the cellular school, to which Metchnikoff belonged, cells are responsible for protective immunity, in particular phagocytes. As it turned out, both schools are correct. It is now known that immunity comprises both humoral and cellular components (Kaufmann, 2008).

2.b.iii. 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. An examination of the Nobel Lectures might reveal why Landsteiner’s colleagues considered his contribution important. On what grounds did they evaluate it as progressive?

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*. However, it is only recently that the scientific importance of Landsteiner’s discovery has been fully realized (my emphasis).[46]

According to Hedrén, Landsteiner’s discovery yielded significant practical applications in medicine as well as new methods that proved useful in other fields. For example, the purification techniques he 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.[47] But that is not all. In addition:

> 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 (Hedrén, 1930, my emphasis).

So, according to Hedrén, in addition to the important medical applications that his discovery brought about, 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.

Indeed, 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

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

[47] See Landsteiner & Miller (1925).

and are characteristic of each species."[48] This general result helped Landsteiner in his experiments regarding the phenomena 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. He grouped blood types into three groups, which he designated as A, B, and C (which later became O and the AB blood group was subsequently added by others). Thanks to Landsteiner's work, the first successful blood transfusion took place in Mt. Sinai Hospital in New York in 1907.

2.b.iv. 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." Here is what K. A. H. Mörner (1904), who delivered the Presentation Speech, had to say about progress:

> The medical sciences are mutually interdependent. *Progress in one field is often closely associated with development in others*. The rise in one branch of science can often have its origin in a recently made analysis within another sphere, and yet it may appear at the first glance that the former is of outstanding importance while the latter is apparently of second value. *It is not always such progress, as immediately are useful and of benefit, which should be considered as especially important; this character can also be attributed to those which are themselves less spectacular but form the basis for the others which are then only a further development of it* (my emphasis).[49]

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

[49] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1904/press.html>.

According to Mörner, then, scientific discoveries are not sudden acts of creativity by isolated geniuses whose task is then simply to convince the scientific community that they have stumbled upon the truth.[50] But what is the aim of science relative to which we make judgments about progress? According to Mörner (1904):

> *The aim of science is the acquisition of knowledge*, the value of which should not be measured by the ease with which it can be brought immediately into practical usefulness (my emphasis).

So the aim of science, according to Mörner, is the attainment of knowledge. Assessments of progress, then, must be made relative to this aim. As Mörner (1904) said:

> The importance of the activities of these men [i.e., Vesalius and Harvey] for the whole of Medical Science *must be estimated from the contributions they gave to the advancement of knowledge* (my emphasis).

How was this aim achieved in the case of Pavlov? According to Mörner (1904):

> In the early days opinions on the course of digestion were speculations as to what was termed as ‘cooking’ or ‘grinding’ in the stomach, etc. So long as the digestive processes could not be observed or investigated directly in the stomach no *real knowledge* could be obtained (my emphasis).

How did Pavlov help improve upon the state of knowledge in the field? According to Mörner (1904):

> An accident turned physiological research in this field in a direction which has later become very important. In the 1820’s a young man sustained a gunshot wound in the stomach and developed a gastric fistula which to some extent permitted the gastric processes to be studied. Observations were carried out on this man by the American physician W. Beaumont. This accidental path of investigation, allowing actual observation of processes taking place in the digestive tract, was later followed by many workers using animals. *Technique is an important factor in such experiments and has been perfected in a masterly way by Pavlov*, whose animals remain in good health, without any injury to the function of their digestive tract, permitting observation and systematic investigation over an almost unlimited period (my emphasis).

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[50] See Zuckerman (1996). See also the quotation from Rutherford in Chapter 1.

According to Mörner, then, Pavlov made contributions not only in terms of factual knowledge about digestion but also in terms of methods and techniques of investigation. As Mörner (1904) put it:

> These methods for the study of the physiology of digestion established by Pavlov have been taken up by various physiological institutions, but above all much important work was performed in his own laboratory. From this has followed far-reaching *transformation of our knowledge in this field which has also been enriched by new fundamental facts* (my emphasis).

Why Pavlov was awarded the Nobel Prize in Physiology or Medicine? 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).

In addition to contributions in terms of new methods and techniques of study (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. What additional knowledge did Pavlov add to the field?

> Before Pavlov's work it was the general opinion that the gastric secretion was not influenced via the nerves connecting the stomach and the central nervous system. This conception has, however, been shown to be incorrect. Pavlov has demonstrated that the vagus nerve linking the brain with various thoracic and abdominal organs contains fibers which during their activity stimulate gastric secretion and others which have an exactly opposite effect. In this way the secretion of gastric juice is controlled by the central nervous system and can be influenced from different parts of the body (Mörner, 1904).

So Pavlov exposed erroneous opinions and corrected them. Furthermore:

> *Pavlov has enriched our knowledge* of the significance and functions of this important nerve in other respects also. The vagus nerve paths are not, it appears, the only simulators of gastric secretion. Pavlov has shown that it may also be influenced through the sympathetic nervous system (Mörner, 1904, my emphasis).

Pavlov also added to the already existing, yet incomplete, knowledge about the vagus nerve. In addition:

> Pavlov has also demonstrated another aspect of the functional association between the gastric mucous and the nervous system, in the specific excitability of the mucous membrane itself. Before the work of Pavlov it was supposed that it could be brought into activity by almost anything within the stomach. Purely mechanical contact was considered to have this effect. *Pavlov demonstrated, however, that this generally accepted view was incorrect*. The reverse is true, the gastric mucous membrane having a sharply differentiated excitability for special substances with which it comes into contact. Thus there are relationships reminiscent of the specific excitability of the sense organs (Mörner, 1904, my emphasis).

In addition to showing that previously held beliefs were false and correcting them, Pavlov also contributed to physiology by adding new knowledge. According to Mörner (1904):

> It is now *known*, thanks to Pavlov, that a similar specific excitability is present in the mucous membrane lining the digestive tract, even though the subject is not conscious of it, and it acts by influencing the processes of secretion and of motility of the canal (my emphasis).

Furthermore:

> As another aspect of this specific sensitivity of the gastric mucous membrane one must consider the remarkable fact, demonstrated by Pavlov, that the amount of gastric secretion and its digestive strength show certain typical variations dependent upon the quality of the food taken (Mörner, 1904).

This is just a sample of the contributions to the knowledge of the physiology of gastric secretion and other aspects of the physiology of the stomach made by Pavlov (Mörner, 1904). Being familiar with the full extent of his contributions, however, Pavlov’s colleagues could judge that:

> *Through Pavlov’s work we have obtained a much more extensive and clearer insight than our previous knowledge could give us*. We have now a fairly comprehensive view of the influence which the activity of one portion of the digestive apparatus can exert on another; in other words how the wheels of the digestive mechanism fit together for the efficient use and advantage of the body (Mörner, 1904, my emphasis).

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 are also epistemic and 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, my emphasis). 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).[51]

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).

2.b.v. Krebs’ Discovery of the Citric Acid Cycle

Hans Adolf Krebs (1900-1981) was awarded the Nobel Prize in Physiology or Medicine in 1953 for his discovery of the citric acid cycle. The Presentation Speech was delivered by E. Hammarsten, member of the staff of professors of the Karolinska

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

Institutet. In his speech, Hammarsten recounted how, before Krebs, no one knew how individual metabolic processes in the cell are related to each other. As in other presentation speeches, Hammarsten (1953) surveyed the state of knowledge in the field before the discovery:

> It has long been *known* that the main components of our foods (proteins, fats, and carbohydrates) are transformed by the living cell into compounds having much smaller molecules (my emphasis).[52]

And then Hammarsten (1953) said what was lacking:

> Much had been *known* about all this [i.e., metabolic processes of the cell] before the advent of Hans Adolf Krebs, but this *knowledge* was concerned only with details and partial processes here and there. *Nobody knew how these isolated reactions were related to each other*, and no one could present a uniform picture of a logical overall reaction mechanism (my emphasis).

So how did Krebs improve upon existing knowledge? He did so by producing a uniform and coherent picture of an overall reaction mechanism. As Hammarsten (1953) put it:

> It was Krebs who discovered how these individual reactions are linked to each other in a cyclic process. He brought us a *clear understanding* of the essential principle of how the released energy is used for the building up processes which take place within the cell (my emphasis).

Prior to Krebs' discovery, it was known that minced animal tissues contain substances that can transfer hydrogen atoms from specific intracellular organic acids, such as succinate, malate, and citrate, to methylene blue dye. Subsequently, it was discovered that minced tissue suspensions rapidly oxidize citrate, fumarate, malate, and succinate to carbon dioxide in the presence of oxygen. This discovery was made thanks to a method for studying the respiratory exchanges of isolated tissues by placing thin slices in a medium in which they could survive for several hours, which was developed by Otto Warburg (1883-1970). Warburg also designed a micromanometer that could measure the

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[52] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1953/press.html>.

oxygen the tissues absorbed and the carbon dioxide they emitted. Albert Szent-Gyorgyi (1893-1986) then discovered that adding a small amount of malate or oxaloacetate stimulates the reduction of more oxygen than is needed to completely oxidize the substance added. Another part of the sequence was discovered by Franz Knoop (1875-1946) and Carl Martius (1906-1993). In short, several distinct reactions were identified but the relationships between them remained unknown (Holmes, 1993). According to Hammarsten (1953), it took Krebs' discovery to make sense of this mishmash of reactions. As Hammarsten (1953) said in his speech, "Out of the chaos of isolated reactions Krebs succeeded in extracting the basic system for the essential pathway of oxidation process within the cell."

In his Nobel Lecture, Krebs also surveyed the state of knowledge in the field before his discovery and described how his discovery improved on it. According to Krebs (1953), "very little was *known* in the earlier 1930's about the intermediary stages through which sugar is oxidized in living cells" (my emphasis).[53] He also described how he worked out the cyclic nature of this assortment of reactions. He realized that certain organic acids are readily oxidized by muscle. He found that the oxidation of endogenous carbohydrate or pyruvate could be stimulated by a number of specific acids that are substrates of the tricarboxylic acid cycle enzymes. He also realized that the succinate to fumarate reaction is a crucial link in a chain of reactions involving all of the known catalytically active acids that can stimulate oxidation of pyruvate (Krebs, 1970).

2.c. The Scientific Practice of Assessing Progress

In the 1958 Nobel Lecture for the Nobel Prize in Physiology or Medicine, Edward Tatum (1909-1975) presented a case history in biomedical research that purported to

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[53] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1953/krebs-lecture.pdf>.

show that scientific progress depends “on knowledge and concepts provided by investigators of the past and present all over the world; on the free interchange of ideas within the international scientific community; on the hybrid vigor resulting from cross-fertilization between disciplines; and […] on chance, geographical proximity, and opportunity” (Tatum, 1958).[54] It is important to keep in mind that taking a naturalistic approach does not mean that we now accept at face value such pronouncements as made by scientists concerning the aims of science and scientific progress. Such pronouncements will have to be subjected to critical scrutiny. It may be that philosophical accounts of scientific progress (which will be discussed in Chapter 3) can accommodate them after all. Or it may turn out that another account of progress should be advanced. In any case, it seems that the scientists’ pronouncements cannot simply be ignored. As Leplin (1997) writes:

> *Naturalism is not obliged to place scientific practice above critical scrutiny, any more than scientific practice requires that observational data be taken at face value*. Science does not abandon objectivity in treating observations as variously interpretable and potentially misleading. That practice is ill conceived or unavailing to the goals of science are possible conclusions of naturalistic inquiry. *Naturalism only requires that such conclusions be empirically based, rather than preordained by an antecedent, a priori commitment to a particular theory of method* (p. 102, my emphasis).

Devitt (1997) also points out that naturalism does not prevent a reconsideration (p. 76); for Devitt, of scientific realism, for our purposes, of scientific progress.

What can we learn, then, from the historical investigation into Early Modern science and the episodes from the history of the Nobel Prize? As we have seen in Chapter 2, the Fellows of the Royal Society regarded “natural knowledge” as the aim of their inquiries into the natural world. For these natural philosophers, pursuing natural

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[54] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1958/tatum-lecture.html>.

knowledge was not simply pursuing knowledge for its own sake. Influenced by Bacon, they sought knowledge "for the glory of God and the benefit of mankind."[55] As for the episodes from the history of the Nobel Prize, they seem to reveal what may be called "the scientific practice of assessing progress." This practice is embodied in the institution of the Nobel Prize and is clearly couched in epistemic terms, i.e., in terms of knowledge. In light of these episodes, the following pattern, based on which scientists make their judgments about progress, can be discerned:

PROGRESS ASSESSMENT IN SCIENCE

(PA1) Survey the body of knowledge in field *F* at time *t* prior to discovery *D*.

(PA2) Estimate what was known (the body of knowledge) in *F* at *t*.

(PA3) Identify a lacuna, imprecision or error in the body of knowledge in *F* at *t*.

(PA4) Spell out how *D* improved on the body of knowledge in *F* by adding new knowledge, correcting imprecision or exposing errors and correcting them.

Take, for example, the case of Pavlov. 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 factual 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

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[55] See Warner (1994).

"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).

The scientific practice of assessing progress also reveals that scientists take progress to consist in the accumulation of knowledge of the following sorts:

(EK) Empirical knowledge: Empirical knowledge usually comes in the form of experimental results, observations, instrumental readings and measurements, and other sorts of "data."

(TK) Theoretical knowledge: Theoretical knowledge usually comes in the form of explanations and 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 domains in nature.

In the case of Landsteiner's discovery of blood groups, for example, we have the accumulation of knowledge of the following sorts:

(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 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 to help solve crimes in which blood stains are left at the scene (though, nowadays, 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 in Chapter 2, Landsteiner was deemed worthy of the Nobel Prize because his discovery led to an increase not only in theoretical knowledge but also in practical and methodological knowledge. In other words, 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.

The same reconstructions, it seems, can be made with respect to the other episodes discussed in Chapter 2. However, instead of doing so here, I will outline them in later chapters when making further clarifications and considering objections.

In the next chapter, I will examine three major types of philosophical accounts of scientific progress, namely, semantic accounts, functional-internalist accounts, and epistemic accounts. I will suggest that the first two are problematic from a naturalistic point of view. I think that naturalists should articulate an account of progress that would do justice to the way in which scientists make judgments about progress [i.e., (PA1)-(PA4)] and the types of knowledge they consider to be important contributions to the advancement of science [i.e., (EK), (TK), (PK), and (MK)]. After examining these types of philosophical accounts of progress, I will suggest that the epistemic account is the most promising insofar as it does justice to the scientific practice of assessing progress. However, the version of the epistemic account I will discuss in Chapter 3, due to Alexander Bird, will have to be amended in light of the historical investigation in order to accommodate the types of knowledge that scientists actually value.

Chapter 3
The Concept of Progress in Philosophy of Science

In Chapter 2, we have seen that scientists are no strangers to the concept of scientific progress. Early Modern natural philosophers, particularly the Fellows of the Royal Society, considered natural knowledge to be the aim of scientific inquiry. As for modern science, scientists are not only conversant with the concept of progress but also make evaluative judgments about contributions and discoveries that advance science. This evaluative practice is institutionalized by the Nobel Foundation and the institution of the Nobel Prize. As the Nobel Lectures reveal, evaluative judgments about progressive discoveries and important contributions to science are couched in epistemic terms, i.e., in terms of knowledge. The judgments are usually made by surveying the state of knowledge in the field in question before the discovery, stating what sort of knowledge was lacking or what erroneous beliefs were held, and then pointing out how the discovery improved upon existing knowledge, either by correcting false beliefs or adding new knowledge, and how the Nobel Laureates added to the body of knowledge in the field.

I propose that this evaluative practice in science cannot be ignored. Taking a naturalistic stance, I suggest that naturalists should articulate an account of scientific progress that would do justice to the scientific practice of assessing progress. Naturalism is the view that philosophical accounts of science must be judged against scientific practice in the same way that scientific theories are judged against natural phenomena. From the naturalistic viewpoint, epistemological theories are scientific theories. A philosophical account about the assessment of scientific theories is a theory about evaluative practice in science and is responsible to this practice. Similarly, a philosophical account about the assessment of scientific progress is a theory about evaluative practice in science and should be judged relative to this practice. It is important to bear in mind, however, that taking a naturalistic stance does not mean accepting whatever scientists say at face value. What they say about progress and the aim of science will be subjected to critical scrutiny and skeptical challenges will need to be addressed.[56]

In this chapter, I will discuss what I take to be three of the major types of cumulative accounts of scientific progress in the philosophy of science. Following Alexander Bird (2007), I will classify them as follows: (a) semantic accounts of progress take truth to be the aim of science and progress is made when truth is accumulated or approached (including approximate truth); (b) functional-internalist accounts of progress take problem-solving to be the aim of science and progress is made when puzzle-solutions are accumulated; (c) epistemic accounts of progress take scientific knowledge to be the aim of science and progress is made when scientific knowledge is accumulated. In this chapter, I will outline these accounts of progress. Then, in Chapter 4, I will see how these accounts fare when examined against the scientific practice of assessing progress discussed in Chapter 2.

3.a. Semantic Accounts of Progress

3.a.i. Verisimilitude and Truthlikeness

According to Karl Popper, scientists are interested in theories with high empirical contents because such theories have great predictive power and are thus testable. For Popper, scientific theories are not inductively inferred from observations of natural phenomena and scientific experimentation is not carried out in order to verify the truth of such theories. Instead, Popper argues, scientific knowledge is conjectural and

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[56] I will do so in Chapter 5.

hypothetical. Scientists can only corroborate or falsify their theories. Since there is a potentially infinite number of theories that can explain a set of natural phenomena in question, scientists can only eliminate the theories that are false. Then, from the remaining unfalsified theories, they can rationally choose those with the greater explanatory and predictive power.

It turns out, according to Popper, that the more improbable a theory is the better it is scientifically, for then it will be rich in empirical content, and thus it could be tested in a number of ways and thereby falsified. Accordingly, scientists are especially interested in statements that are rich in empirical content and low in probability, but that can nevertheless get closer to the truth. Popper emphasizes the tests to which theories can be subjected and by means of which they can be falsified or corroborated. He urges that, even though falsification seems to be a simple principle of logic, it is not so simple in practice. When it comes to falsifying hypotheses in practice, it is always possible that the observations are inaccurate or that the accepted background knowledge is flawed.

According to Popper (1979), "the general *aim of rational discussion* […] *is to get nearer to the truth*" (Popper, 1979, p. 17).[57] However, although "*we are seekers for truth*," Popper argues, "*we are not its possessors*" (p. 47). The aim of science, then, "is truth in the sense of better approximation to truth, or greater verisimilitude" (Popper, 1979, p. 57). Popper thinks that the search for verisimilitude is a "more realistic aim," as he puts it, than the search for truth. With the notion of verisimilitude, Popper (1979) says, "we can explain the method of science, and much of the history of science, as the rational procedure of getting nearer to the truth" (p. 58).

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[57] In the following quotations from Popper, emphasis is in the original.

For Popper (1979), “the aim of the scientist is not to discover absolute certainty, but to discover better and better theories […] capable of being put to more and more severe tests” (p. 361). These theories must be falsifiable, according to Popper (1979), “it is through their falsification that science progresses” (p. 361). Popper argues that a scientific theory is better than its rival when it has a higher level of “verisimilitude.” The notion of verisimilitude is explicated in terms of the logical consequences of theories. The content of a theory is the totality of its logical consequences, which can be divided into two classes: (a) the “truth-content” of a theory is the class of true statements that can be derived from it; (b) the “falsity-content” of a theory is the class of false statements that can be derived from it (which could be empty). Popper proposes two ways of comparing theories in terms of their verisimilitude. On the qualitative way, if the truth-content and the falsity-content of T_1 and T_2 are comparable, then T_2 is closer to the truth than T_1 if and only if either (a) the truth-content, but not the falsity-content, of T_2 is greater than that of T_1 or (b) the falsity-content, but not the truth-content, of T_1 is greater than that of T_2. Accordingly, T_2 has a higher level of verisimilitude than T_1 if and only if the truth-content and falsity-content of T_1 and T_2 are comparable through subclass relationships and either (a) the truth-content of T_2 includes that of T_1 and the falsity-content of T_2 (if there is one) is included in that of T_1 or (b) the truth-content of T_2 includes that of T_1 and the falsity-content of T_2 (if there is one) is included in that of T_1 (Popper, 1979, pp. 52-53).

On the quantitative way, Popper defines verisimilitude by assigning values to contents, where the index of the content of a theory is its logical improbability. Since there is an inverse relation between content and probability, for Popper, the quantitative verisimilitude of a statement ‘*a*’ can be formulated as follows: $Vs(a) = ct_T(a) - ct_F(a)$.

$Vs(a)$ is the verisimilitude of a, $ct_T(a)$ is the truth-content of a, and $ct_F(a)$ is the falsity-content of a.

Either way, Popper argues, it can be said that T_2 is “better” than T_1, in the sense of being closer to the truth, even if T_2 with its higher content is later falsified. On this view, then, scientific progress is construed in terms of the abandonment of partially true, but falsified, theories, for theories with a higher level of verisimilitude. For a theory to have a higher level of verisimilitude is for it to approach more closely to the truth. This notion of verisimilitude allows Popper to argue that science progresses toward the truth by means of the falsification and corroboration of theories. In other words, scientific progress is progress toward the truth and corroboration is an indicator of verisimilitude.

Critics, such as Pavel Tichy and Adolf Grunbaum, pointed out several problems with Popper’s formal account of verisimilitude. They showed that the conditions specified by his formal account of verisimilitude cannot be met (especially in the case of false theories). The details of their objections are not important here.[58] What is important, for present purposes, is Popper’s response. He acknowledged the problems in his formal account. As Popper (1979) writes: “I must admit at once that my proposed ‘definition of verisimilitude’ is faulty” (p. 371). Popper insists, however, that verisimilitude can still be given a formal account. As Popper (1979) writes: “I do think that we should not conclude from the failure of my attempts to solve the problem that the problem cannot be solved” (p. 372). He also claimed that he never suggested that “degrees of verisimilitude […] can ever be numerically determined, except in certain limiting cases” (Popper, 1979, p. 59). He argued that the notion of verisimilitude has a heuristic value, in terms of its intuitive

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[58] See Grunbaum (1976) and Tichy (1974).

appeal, and thus the absence of a formal account is not fatal and the concept can be applied in the assessment of scientific progress.

Following Popper, some philosophers of science tried to come up with a formal account of verisimilitude, whereas others were willing to settle for an informal notion of truthlikeness. Niiniluoto, for example, is one philosopher of science who thought that the fact that Popper’s account of verisimilitude fails does not mean that the problem cannot be solved. Niiniluoto insists that progress consists in approximations to the truth. He argues that truthlikeness can be formalized.[59] Psillos (1999), on the other hand, argues that formal accounts of truthlikeness fail and that an informal notion will do. For Psillos, a description D is approximately true of S if there is another state S^* such that S and S^* are linked by specific conditions of approximation and D is true of S^*.

3.a.ii. Kitcher’s Varieties of Progress

As we have seen, Popper argues that scientific progress involves the abandonment of partially true, but falsified, theories, in favor of theories with a higher level of verisimilitude, i.e., theories that approach more closely to the truth. Whether or not truthlikeness can be formalized is not important for present purposes. For now, it is important to recognize that the idea that science is making progress by getting closer to the truth was not abandoned.

Kitcher’s account of scientific progress may be reasonably seen as a semantic account insofar as it takes truth to be the cognitive aim of science. For Kitcher, however, it is not simply truth that scientists aim for, but significant truth. According to Kitcher (1993), “In conceiving of science as progressive we envisage it as a sequence of consensus practices that get better and better with time” (p. 90). To understand the

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[59] See Niiniluoto (1984) and (1987).

progress of science, Kitcher (1993) claims, “we need to be able to articulate the relations among successive consensus practices” (p. 87). What is a consensus practice, then? According to Kitcher (1993):

> The *consensus practice* of a community at a given time is thus represented by (i) the *core consensus*, the elements of individual practice common to the individual practices of all members of the community, (ii) the *acknowledgments of authority* (themselves parts of individual practice) shared by all members of the community (including, perhaps, criteria for granting deferred authority); (iii) an organization of the community into subcommunities recognized as responsible for and authoritative over particular types of issues, (iv) a *virtual consensus*, generated from (i) by the incorporation of parts of the consensus practice of subcommunities in accordance with the relations delineated in (ii) and (iii) (p. 88).[60]

The “core consensus,” according to Kitcher (1993), consists of individual practices, which are, in turn, composed of the following elements:

1. The language that the scientist uses in his professional work.
2. The questions that he identifies as the significant problems of the field.
3. The statements (pictures, diagrams) he accepts about the subject matter of the field.
4. The set of patterns (or schemata) that underlie those texts that the scientist would count as explanatory.
5. The standard examples of credible informants plus the criteria of credibility that the scientist uses if appraising the contributions of potential sources of information relevant to the subject matter of the field.
6. The paradigms of experimentation and observation, together with the instruments and tools which the scientist takes to be reliable, as well as his criteria for experimentation, observation, and reliability of instruments.
7. Exemplars of good and faulty scientific reasoning, coupled with the criteria for assessing proposed statements (the scientist’s “methodology”) (p. 74).

By “acknowledgment of authority,” Kitcher seems to mean that within a certain scientific community, the views of certain scientist are accepted as authoritative. The scientific community is divided into subcommunities, according to Kitcher, where certain subcommunities are regarded as authoritative on certain issues. The “virtual consensus”

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[60] In the following quotations from Kitcher, emphasis is in the original.

includes the elements of the consensus that are not explicitly accepted by all the members of the community, but would be accepted, in principle, if they were spelled out.

As Kitcher acknowledges, his notion of “consensus practice” and Kuhn’s notion of “paradigm” (i.e., disciplinary matrix) are similar in several respects. However, he insists that his notion is not in accordance with what he sees as Kuhn’s commitment to “significant epistemological differences between the course of science within a segment and the intersegment transitions” (Kitcher, 1993, p. 87, note 39). For Kitcher, progress is to be assessed by comparing successive consensus practices within the same field. As Kitcher (1993) writes: “We judge the progressiveness of a sequence of practices by thinking about the pairwise relations between practices” (p. 91). The progress of consensus practices can be compared based on several distinct dimensions.

In science, according to Kitcher (1993), “What we want is *significant* truth” (p. 94). He argues that there is no problem with talking about scientific progress in terms of truth. As Kitcher (1993) observes: “Philosophical reticence about the attainment of significant truth is the result of a failure of nerve, induced by thinking about the problem in a faulty way” (p. 118). We simply have to realize that “farming the crucial issues in terms of truth *for whole theories*,” is misguided and “far too ambitious” (Kitcher, 1993, p. 118). Instead, we have to think in terms of individual statements. For Kitcher (1993), “A significant statement is a potential answer to a significant question” (p. 118). With this change of focus in mind, i.e., zooming in on individual statements, rather than taking scientific theories as monolithic wholes, Kitcher (1993) argues that “What we strive for, when we can get them, are *true* significant statements, that is, true answers to significant questions” (p. 118).

Kitcher (1993) gives the following examples of true significant statements: (a) “DNA molecules consist of two helical strands, wound around one another in opposite directions”; (b) “Part of Western California is at the junction of two plates that slide past one another”; (c) “Gangs of male Florida scrubjays are sometimes able to expand the territories in which they reside” (p. 118). According to Kitcher (1993), (a) is significant because it answers the significant question, “What is the structure of the genetic material?” and it is true because it is “a restricted generalization about the DNA molecules typically found in cells in nature throughout most of the phases of the life of the cell,” and counterexamples, such as single-strand DNA molecules used in molecular biology, “fall outside its intended scope” (p. 118). The second example, (b), is significant because it plays a role in answering the significant question “Why is there an earthquake zone in Western California?” And (c) is significant because it plays a role in answering the significant question “Why do male Florida scrubjays often return to the parental nest and assist in the feeding of siblings?” (p. 119). This does not mean, says Kitcher (1993), that all the individual statements of science “will endure as parts of the consensus practices of future fields of science” (p. 119). However, Kitcher thinks that the problem lies with significance rather than truth. If we turn to scientific journals from fifty years ago, Kitcher says, we will find statements that are more or less correct, but that do not seem to be significant any longer.

Focusing his account on cognitive progress, where the “most obvious pure epistemic goal is truth,” and setting aside practical progress (in the sense of contributing to “the relief of man’s estate”) (Kitcher, 1993, pp. 92-93), Kitcher identifies several varieties of scientific progress:

1. Conceptual progress: “Conceptual progress is made when we adjust the boundaries of our categories to conform to kinds and when we are able to provide more adequate specifications of our referents” (Kitcher, 1993, pp. 95-96).
2. Explanatory progress: “Explanatory progress consists in improving our view of the dependencies of phenomena” (Kitcher, 1993, p. 105).
3. Erotetic progress: “We make erotetic progress when we have an erotetically well-grounded consensus practice in which we pose genuinely significant questions that were not previously asked” (Kitcher, 1993, p. 114).
4. Instrumental (experimental) progress: “we make *instrumental* (or *experimental*) progress by adopting a practice in which the former instrument (technique) replaces the latter” (Kitcher, 1993, p. 117).
5. Progress in the statements scientists accept: “We make progress with respect to the set of accepted statements in a number of ways. Sometimes, scientists eliminate falsehood in favor of truth, abandon the insignificant, add significant truths, or reconceptualize already accepted truths. Moreover, […] they often replace statements that are further from the truth with those that are closer to the truth” (Kitcher, 1993, p. 117).
6. Methodological progress: “We make progress in our methodological principles by formulating strategies that give us greater chances of making conceptual progress, explanatory progress, erotetic progress, or progress in the statements we accept” (Kitcher, 1993, p. 120).

“Conceptual progress,” according to Kitcher (1993), “should be assessed in terms of proximity to the ideal state” (p. 104). The ideal state is for scientists to obey the following maxims: (a) “*Conformity*: Refer to that which others refer”; (b) “*Naturalism*: refer to natural kinds”; (c) “*Clarity*: Refer to that which you can specify” (p. 104). For example, conceptual progress has been made in the use of the term ‘planet’, according to Kitcher, because the extension of the term has expanded since pre-Copernican times to include more members of the kind planet, thereby improving our understanding of what a planet is. For Kitcher (1993), then, conceptual improvements “come about by abandoning modes of reference that are not in accord with one of the maxims or by adding modes of reference that would be in accord with both *clarity* and *naturalism*” (p. 105).

As for explanatory progress, such progress is made when there are improvements in the explanatory schemata of an earlier consensus practice to a later one. As Kitcher (1993) writes: “Fields of science make explanatory progress when later practices introduce explanatory schemata that are better than those adopted by earlier practices” (p. 106). For Kitcher, these schemata take the form of a question that is answered by a line of argumentation. Each of the premises in the schema is a schematic sentence that replaces non-logical expressions with variables. In addition, there are “filling instructions,” which may not be explicitly stated, for what sort of entities should figure in the schemata. Kitcher (1993) identifies the following as instances of explanatory progress: (a) “the introduction of correct schemata” (p. 109), (b) “the elimination of incorrect schemata” (p. 110), (c) “the generalization of schemata, rendering them able to deal correctly with a broad type of instances” (p. 109), and (d) “explanatory extension, when the picture of dependencies is embedded within some larger scheme” (p. 109).

Erotetic progress, according to Kitcher, is progress in terms of asking questions that are better and significant. On the one hand, there are what Kitcher calls “application questions." These are questions that arise as a result of attempts to show that explanatory schemata apply to natural phenomena (Kitcher, 1993, p. 112). On the other hand, there are what Kitcher (1993) calls "presuppositional questions," which arise when "instantiations of some accepted schema presuppose the truth of some controversial claim (p. 113). This situation, where there is a coherent instantiation of an explanatory schema, but some of its presuppositions are questionable, generates research to resolve the apparent tension.[61] Erotetic progress is made not only when scientists ask questions that are more significant but also when scientists ask "more tractable questions" (p. 114). Scientists make progress in this sense when they substitute vague questions with more precise questions. Moreover, "we can make erotetic progress not only by adding significant new questions but by decomposing some of the significant questions of prior practice" (p. 115). Given a certain schema, questions may arise about its instantiations.

Instrumental or experimental progress is made, according to Kitcher, when new instruments or experimental techniques are developed that allow scientists to answer significant questions. "Instruments and experimental techniques are valued," according to Kitcher (1993), "because they enable us to answer significant questions" (p. 117). In a footnote, however, Kitcher (1993) says:

> while some of [his] relations of progressiveness concern the attaining of certain epistemic desiderata, others involve achieving the means for going further. Thus, if we are making conceptual or explanatory progress we have already gathered some good things (like the firm that is already making a profit). If we are making instrumental or erotetic progress than we have prepared ourselves well for gathering good things in the future (like the firm that has invested wisely in new ventures) (p. 117, note 28).

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[61] In a footnote, Kitcher says that his presuppositional questions are somewhat similar to Kuhnian ideas and Laudan's conceptual problems.

So it seems that Kitcher is claiming that instrumental/experimental, erotetic, and perhaps also methodological progress are means to an end, as it were. These varieties of progress facilitate cognitive progress, i.e., conceptual and explanatory progress. What we really want in science—the "good things" or the "epistemic desiderata"—is truth or verisimilitude. As Kitcher (1993) goes on to say, "Making this conception of progress precise requires us to look more carefully at progress in the set of accepted statements: if we know what counts as improving the set of accepted statements, then we can characterize instrumental and experimental progress by recognizing the increased power of instruments and techniques to deliver improved statements" (p. 117).

By "improved statements," Kitcher seems to mean statements that are closer to the truth, for he goes on to discuss verisimilitude in the next section. "Progress with respect to the set of accepted statements," according to Kitcher (1993), can be made in a number of ways: (a) when "scientists eliminate falsehood in favor of truth," (b) "abandon the insignificant," (c) "add significant truths," (d) "reconceptualize already accepted truths," (e) "replace statements that are further from the truth with those that are closer to the truth" (p. 117). Kitcher argues that scientists are not trying to come up with universal generalizations that have no exceptions. Rather, they strive for statements that are true answers to significant questions. Some of these answers, perhaps even most of them, are generalizations with restrictions in scope (Kitcher, 1993, p. 118). Such generalizations can approximate the truth when later generalizations are restrictions of earlier ones that avoid some of the original false consequences. More explicitly, "if there is a class *A* whose members have property *B* except under rare conditions *C*, *D*, …, then the generalizations 'All *A*'s are *B*' will be relatively close to the truth (exceptions will be infrequent)" (p. 121). From there, scientists "will move closer to the truth by restricting the generalization to exclude one or more of the exceptional cases (*C*, *D*, etc.) and adding a true generalization about the properties of those *A*'s that are subject to the exceptional condition" (p. 121).

3.b. Functional-Internalist Accounts of Progress

3.b.i. Kuhn on Progress

Before Thomas Kuhn, the semantic conception of scientific progress, according to which science progresses by the increasing approximation of theories to the truth, was more or less the prevailing view. According to Kuhn, however, the development of science is not as uniform as the semantic account would suggest. In the 1970 Postscript to his seminal *The Structure of Scientific Revolutions* (1962), Kuhn attacks the idea that scientific theories can be regarded as more or less close to the truth as incoherent.[62] For Kuhn, science has alternating "normal" and "revolutionary" periods. Normal and revolutionary phases differ not only quantitatively but also qualitatively. Normal science resembles the semantic view of scientific progress in terms of its cumulative aspect. However, for Kuhn (1996), "the aim of normal science is not major substantive novelties" (p. 35). Normal science is "puzzle-solving" rather than truth-aiming. According to Kuhn, scientists, just like crossword puzzles enthusiasts, expect to have a reasonable chance of solving the puzzle. That means that, just like puzzle solvers, they expect that finding solutions to puzzles will depend mostly on their own abilities. Moreover, the puzzles and the methods used to solve them will be familiar to them. This is part of the reason why science seems to progress rapidly, according to Kuhn (1996), because "its practitioners concentrate on problems that only their own lack of ingenuity

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[62] I will revisit Kuhn's attack on truth in Chapter 5.

should keep them from solving" (p. 37). Accordingly, normal science can accumulate a large number of puzzle-solutions because the puzzles and their solutions are familiar to the scientists as puzzle solvers.

On the other hand, revolutionary science is not cumulative in this way. According to Kuhn, scientific revolutions involve a revision of accepted scientific beliefs or practices. Scientific revolutions, for Kuhn (1996), are "non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one" (p. 92). After a scientific revolution, even the achievements of the preceding period of normal science may not be preserved. Indeed, after a revolution, scientists may find themselves without a solution to a problem that previously had a satisfactory solution in the earlier period of normal science. For example, in the case of phlogiston theory, it "accounted for a number of reactions in which acids were formed by the combustion of substances like carbon and sulphur," and it also "explained the decrease of volume when combustion occurs in a confined volume of air" (Kuhn, 1996, p. 100). This feature of scientific revolutions is known as a "Kuhn-loss."

According to Kuhn, normal science progresses just in case the scientific community is strongly committed to its shared theoretical beliefs, values, instruments, methods, and metaphysical assumptions. These shared commitments are what Kuhn calls a "paradigm." By "paradigm," Kuhn (1996) means "to suggest that some accepted examples of actual scientific practice—examples which include law, theory, application, and instrumentation together—provide models from which spring particular coherent traditions of scientific research" (p. 10). In the 1969 Postscript, he refers to "paradigm" as "disciplinary matrix": "'disciplinary' because it refers to the common possession of the practitioners of a particular discipline; ‘matrix’ because it is composed of ordered elements of various sorts, each requiring further specification” (Kuhn, 1996, p. 182). For Kuhn, then, normal science can be progressive when there is a shared commitment in the scientific community to the disciplinary matrix. As a result, training successful scientists involves an inculcation of the commitment to the disciplinary matrix. However, since scientists usually have a desire for innovation as well, this creates a tension between that desire and the commitment to the disciplinary matrix. Kuhn calls this “the Essential Tension.”[63]

In scientific practice, the conservative commitment to the disciplinary matrix results in resistance to scientific revolutions, or at least in that revolutions are not sought after except under unusual circumstances. According to Kuhn, during periods of normal science, scientists do not try to confirm or disconfirm the fundamental theories that constitute the disciplinary matrix. In fact, Kuhn claims that even anomalous results will not bring about a rejection of the assumptions that comprise the disciplinary matrix during normal science. During periods of normal science, anomalies are ignored or explained away. However, a disciplinary matrix may be in danger when there are several especially challenging anomalies. For an anomaly to be particularly troublesome for a disciplinary matrix, it must undermine the practice of normal science. For example, suppose a widely used instrument turns out to be inadequate insofar as it yields inaccurate results and thereby casts doubt on the underlying theory. Since it is widely used, normal science relies on this instrument, and it will be difficult for scientists to continue using this instrument confidently. The anomaly must be addressed. If it is not addressed satisfactorily, then normal science may be in what Kuhn calls a “crisis.” According to

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[63] See Kuhn (1959) and (1977).

Kuhn (1996), in the case of Ptolemaic astronomy and Aristotelian theory of motion, for example, “the awareness of anomaly had lasted so long and penetrated so deep that one can appropriately describe the fields affected by it as in a state of growing crisis” (p. 67). A crisis, then, is a pervasive undermining of the confidence in one or more of the assumptions of the disciplinary matrix.

A scientific revolution takes place when a scientific community responds to a crisis by revising the disciplinary matrix in such a way that the most pressing anomalies are eliminated and the most outstanding puzzles are solved. However, Kuhn insists that there are no principles for assessing the significance of puzzles and for comparing puzzles and their solutions. The decision to revise a disciplinary matrix or the decision to adopt a particular revision is not rational. That is why, according to Kuhn, revolutionary periods in science are marked by competition. As Kuhn (1996) puts it, the “transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced” (p. 151). He says that “Individual scientists embrace a new paradigm for all sorts of reasons and usually for several at once” (p. 152). Some reasons are non-scientific, such as Kepler’s sun worship. Others have to do with idiosyncrasies of autobiography and personality. He also mentions nationalities of leading scientists as factors that might influence the outcomes of scientific revolutions. It is important to note, however, that Kuhn emphasizes that these external factors do not determine the outcomes of revolutions in science. He seems to suggest that such factors are internal, especially when the puzzle-solving power of competing theories is concerned.

Kuhn (1996) seems to insist, however, that science does progress, even through revolutions. As he writes:

> Later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied. That is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress (p. 206).

But he thinks that "Kuhn-losses" present a problem for cumulative views of scientific progress, especially those construed in semantic terms (e.g., truth and verisimilitude). For Kuhn, in a scientific revolution, the search for an alternative paradigm is motivated by the fact that the current paradigm does not solve certain significant anomalies. Hence, an alternative paradigm, worthy of replacing the old one, should solve these anomalies, at least most of them. Now, even though there might be some Kuhn-losses, the alternative paradigm that will be selected must also keep much of the problem-solving power of the old paradigm. As Kuhn (1996) puts it:

> scientists will be reluctant to embrace [a new paradigm] unless convinced that two all-important conditions are being met. First, the new candidate must seem to resolve some outstanding and generally recognized problem that can be met in no other way. Second, the new paradigm must promise to preserve a relatively large part of the concrete problem-solving ability that has accrued to science through its predecessors (p. 169).

The new paradigm must retain much of the problem-solving power of the old paradigm, especially in terms of quantitative problems, though it may lose some problem-solving power in terms of qualitative problems, i.e., explanatory power.

For Kuhn, then, scientific revolutions can result in an overall increase in problem-solving power when the number and significance of the puzzles solved by the new paradigm is greater than the number and significance of the puzzle-solutions of the old paradigm, which are no longer available due to a Kuhn-loss. Kuhn insists that none of this suggests any approximation to truth. Scientific progress, for Kuhn (1996), is evident in the "list of problems solved by science and the precision of individual problem-solutions [that] grow and grow" (p. 170). He urges that we must give up "the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth" (p. 170). He explicitly notes that he has been careful not to use the term 'truth' (p. 170). And in the 1969 Postscript, he argues that the notion of approximating or getting closer to truth is meaningless (p. 206).[64] He seems to draw an analogy between scientific progress and evolution. As Kuhn (1996) writes, "The developmental process described in this essay has been a process of evolution from primitive beginnings—a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature" (p. 170). As in the case of evolution, where the development of an organism does not mean that there is a goal to which the organism is approaching or an ideal that the organism is approximating, there is no ideal truth to which scientific theories are getting closer. According to Kuhn, science improves when theories develop in response to problems and progress is measured by the success of theories in solving those problems.

3.b.ii. Laudan on Progress

Like Kuhn, Larry Laudan is also skeptical of accounts of scientific progress in terms of the accumulation of (approximate) truth. If we insist, Laudan (1977) argues, "that theories are designed only to explain 'facts' (i.e., true statements about the world), we shall find ourselves unable to explain most of the theoretical activity which has taken place in science" (p. 16). He advances an account of progress in terms of the problem-solving effectiveness of theories. Laudan distinguishes two kinds of problems—empirical and conceptual—though he admits that there is a continuum between these two kinds of

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[64] I will revisit to this argument in Chapter 5.

problems. Nonetheless, he explicates the two extreme ends of the continuum for the sake of simplicity (Laudan, 1977, p. 48).

According to Laudan (1977), empirical problems are first-order problems. These are the kinds of problems that are unreflectively associated with science and that concern the subject matter of scientific theories. As he writes, they concern “anything about the natural world which strikes us as odd, or otherwise in need of explanation” (p. 15).[65] He calls them “empirical problems” because “we *treat* empirical problems as if they were problems about the world” (p. 15). We determine “the adequacy of solutions to empirical problems,” according to Laudan, “by studying the objects in the domain” (p. 15). He says that he does not mean that empirical problems are extra-theoretical facts or what he calls “state of affairs.” He says that empirical problems are not to be understood as what is “directly given by the world as veridical bits of unambiguous data” (p. 15). Rather, empirical problems, and problems of all sorts, “*arise within a certain context of inquiry* and are partly defined by that context” (p. 15). In other words, problems are to be thought of as constructs rather than as “facts” in the domain in question. For Laudan, this distinction is significant partly because a problem can have a solution that is not true. That is, there can be problems that do not correspond to anything in the world, even though scientists may believe that they do. For Laudan (1977), “all that is required is that it be *thought to be* an actual state of affairs by some agent” (p. 16). And elsewhere Laudan (1981a) writes, “a theory solves an empirical problem when it entails, along with approximately initial and boundary conditions, a statement of the problem” (p. 148). This is a crucial point. For Laudan, problem *P* is solved by theory *T* when *P* can be deduced from *T*. Laudan does not require that *T* is true. Nor does he require that *P* is even a real

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[65] In the following quotations from Laudan, emphasis is in the original.

problem. So, to use Laudan's example, Nicole d'Oresme believed that diamonds can be split by hot goat's blood. On Laudan's view, if d'Oresme were to come up with a theory from which the splitting of diamonds by hot goat's blood can be deduced, he would have a solution to the problem. Laudan would also give credit to Galileo for solving the problem of falling bodies, "even though [Galileo's] solution holds only if the ratio of the distance of fall is to the distance to the center of the earth is zero" (Losee, 2004, p. 121).

Laudan (1977) divides empirical problems into three types: "(1) *unsolved problems*—those empirical problems which have not yet been adequately solved by *any* theory; (2) *solved problems*—those empirical problems which have been adequately solved by a theory; (3) *anomalous problems*—those empirical problems which a *particular* theory has not solved, but which one or more of its competitors have" (p. 17). Unlike Kuhn, Laudan does not consider as anomalies only those observations that contradict the theory in question. For Laudan, problems that contradict a theory are not considered anomalies unless a rival theory has solved them. Anomalous problems, according to Laudan (1977), count as "evidence *against* a theory, and unsolved [problems] simply indicate lines for future theoretical inquiry" (p. 18).

As far as scientific progress is concerned, the important empirical problems are those that have a solution. As Laudan (1977) writes, "*one of the hallmarks of scientific progress is the transformation of anomalous and unsolved empirical problems into solved ones*" (p. 18). According to Laudan (1977):

> a theory may solve a problem so long as it entails even an approximate statement of the problem; in determining if a theory solves a problem, *it is irrelevant whether the theory is true or false, well or poorly confirmed*; what counts as a solution to a problem at once time will not necessarily be regarded as such at all times (pp. 22-23).

According to Laudan, then, for a theory *T* to solve a problem is for the statement of the problem to be deduced from *T*. Laudan seems to want to acknowledge that many results in science are approximations rather than exact matches to the predictions made by theories. However, this seems to suggest that two theories can solve a problem even if their solutions “are formally inconsistent” (p. 24). Hence, for Laudan, a solution to a problem need not be and should not be considered as correct. As Laudan (1977) writes, “one need not, and scientists generally do not, consider matters of truth and falsity when determining whether a theory does or does not solve a particular empirical problem” (p. 24). For example, “Lavoisier’s theory of oxidation, whatever its truth status, solved the problem of why iron is heavier after being heated than before” (pp. 24-25). In general, “*any theory, T, can be regarded as having solved an empirical problem, if T functions (significantly) in any schema of inference whose conclusion is a statement of the problem*” (p. 25). In addition, solutions to problems may count as solutions even if they are not permanent. For example, “given eighteenth-century standards of experimental accuracy and canons for adequacy of problem solution, the kinetic theory was a far cry from an adequate problem solver, especially so far as certain ranges of data were concerned” (pp. 24-25). This resulted, according to Laudan, in the development of solutions that met contemporary standards of problem solving, and he cites van der Waals’ equation as an example.[66]

Conceptual problems, according to Laudan (1977), “are higher order questions about the well-foundedness of the conceptual structures (e.g., theories) which have been devised to answer the first order questions” (p. 48). They are “characteristics of theories

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[66] Johannes Diderik van der Waals’ (1837-1923) equation is an equation of state, which is an equation that relates the pressure *p*, volume *V*, and thermodynamic temperature *T* of an amount of substance *n*. The simplest equation of state is the Ideal Gas Law. I will say more about this in Chapter 7.

and have no existence independent of the theories which exhibit them" (p. 48). There are two ways in which conceptual problems arise for a theory, *T*, according to Laudan: (a) "When *T* exhibits certain internal inconsistencies, or when its basic categories of analysis are vague and unclear; these are *internal conceptual problems*"; (b) "When *T* is in conflict with another theory or doctrine, *T`*, which proponents of *T* believe to be rationally well founded; these are *external conceptual problems*" (p. 49).

Among internal conceptual problems, Laudan (1977) identifies two types: logical inconsistency within a theory and "*conceptual ambiguity or circularity within the theory*" (p. 49). Both types are "important in the process of theory appraisal," but not as important as external conceptual problems (p. 50). According to Laudan (1977), "the ambiguity of concepts is a matter of degree rather than kind" (p. 49). He thinks that "Some degree of ambiguity is probably ineliminable in any except the most vigorously axiomatized theories" (p. 49). Since ambiguity is ineliminable, according to Laudan, and it may sometimes even be advantageous, only "systematic and chronic ambiguity or circularity within a theory [is] highly disadvantageous" (p. 49). One important way of addressing internal inconsistencies is by the "increase of the conceptual clarity of a theory through careful clarifications and specifications of meaning" (p. 50). William Whewell called it "the explication of conceptions," and Laudan seems to agree that it is "one of the most important ways in which science progresses" (p. 50).

Laudan contends that external conceptual problems have been more important historically than internal problems. He identifies three types of external conceptual problems: inconsistency, implausibility, and mere compatibility. As an example of inconsistency, he gives Ptolemy's system, which, according to Laudan (1977), made “assumptions about the behavior of celestial bodies (e.g., the hypothesis that certain planets move around empty points in space, that planets do not always move at constant speed, and the like) which were in flagrant contradiction with the then universally accepted physical and cosmological theories about the nature and motion of the heavenly bodies” (pp. 51-52). Two theories may be logically compatible but jointly implausible, according to Laudan, “when the acceptance of either one makes it less plausible that the other is acceptable” (p. 52). As an example of joint implausibility, he gives mechanistic, “Cartesian inspired theories of physiology,” which were “consistent with Newtonian physics,” according to Laudan, “but it did seem highly implausible, given Newtonian physics, that a system as complex as living organism could function with only a limited range of the processes exhibited in the inorganic realm” (p. 52). Mere compatibility is a problem “when a theory emerges which ought to reinforce another theory, but fails to do so and is merely compatible with it” (p. 53). That is, an external conceptual problem arises when T_2 is expected to reinforce T_1 when, in fact, it turns out that T_2 is merely compatible with T_1, i.e., T_2 entails nothing about T_1. For example, Laudan says, in the seventeenth century, “it was expected that any physical theory should be positively relevant to, and not merely compatible with, Christian theology” (p. 54).

According to Laudan, the problem-solving function of theories is defined by the number and importance of solved empirical problems relative to the number and importance of the anomalies and conceptual problems that they generate. How exactly this may be carried out requires some further explanation of Laudan’s weighting criteria. His weighting criteria are to be understood as guidelines for weighting problems rationally. Laudan (1977) emphasizes that he is interested in “the *cognitively rational weighting* of scientific problems” (p. 32).

According to Laudan (1977), in “a domain in which no adequate, systematic theories have yet been developed, almost all empirical problems are on a par” (p. 33). When there are a number of theories, however, then they can be weighted. Laudan lists eight weighting criteria. As for conceptual problems, which are more significant than empirical problems, other things being equal, “the greater the tension between two theories, the weightier the problem will be” (p. 65). Where there is a tension between two theories, the significance of this tension for T_1 is proportional to the degree of the problem-solving ability of T_2. When the problem-solving effectiveness of T_1 has been demonstrated, its compatibility with T_2 will be weighted most heavily. Moreover, if there are two competing, rather than complementary, theories, T_1 and T_2, that “exhibit the *same* conceptual problem(s), then those problems count no more against one than against the other and become relatively insignificant in the context of comparative theory appraisal” (p. 65). However, Laudan says, “if T_1 generates certain conceptual problems which T_2 does not, then those problems becomes highly significant in the appraisal of the relative merits of T_1 and T_2” (p. 65).

For Laudan (1977), “*the problem-solving effectiveness of a theory depends on the balance it strikes between its solved problems and its unresolved problems*” (p. 67). Assessing progress is a comparative matter. It is useful, however, to begin with the single empirical problem solved by a theory: “T_1 has solved its initial empirical problem, *p*, and to that extent, we can say that ‘progress’ has been made” (p. 67). This is not yet progress in any robust sense. For Laudan, we must consider “problem-solving effectiveness,” which requires taking the weights and significance of problems into consideration. As he writes: "*the overall problem-solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves and deducing therefrom the number and importance of the anomalies and conceptual problems which the theory generates*" (p. 68). Then, Laudan says, we can have "a rudimentary notion of scientific progress." "Given that the aim of science is problem solving," according to Laudan, "*progress can occur if and only if the succession of scientific theories in any domain shows an increasing degree of problem solving effectiveness*" (p. 68). According to Laudan, there "are many ways in which such progress can occur" (p. 68). First, T_1 can be replaced by T_2, which solves more empirical problems, and thus bring about "an expansion of the domain of solved empirical problems" (p. 68). Second, T can be modified so that the modification "eliminates some troublesome anomalies or which resolves some conceptual problems" (p. 68). Third, progress occurs "as a result of all the relevant variables shifting subtly" (p. 68).

This problem-solving account of progress, according to Laudan (1977), tells us "how to evaluate the problem-solving effectiveness of individual theories" (p. 107). However, as will be important for present purposes, what if we require a small or fine-grained analysis of episodes from the history of science? "Localizing the notion of progress to specific situations rather than to large stretches of time," Laudan writes, "we can say that *any time we modify a theory or replace it by another theory, that change is progressive if and only if the later version is a more effective problem solver (in the sense just defined) than its predecessor*" (p. 68). The sense just defined, as mentioned above, is that "*progress can occur if and only if the succession of scientific theories in any domain* *shows an increasing degree of problem solving effectiveness*" (p. 68). Laudan emphasizes that progress is defined as comparative superiority in problem solving. As Laudan (1977) writes:

> All evaluations of research traditions and theories must be made *within a comparative context*. What matters is not, in some absolute sense, how effective or progressive a tradition or theory is, but, rather, how its effectiveness or progressiveness compares with its competitors (p. 120).

Progress, then, must be assessed comparatively, i.e., the progressiveness of theories or research traditions must be assessed by comparing the weighted number of the empirical problems solved by T_1 minus the weighted number of the anomalies and the conceptual problems of T_1 with the weighted number of the empirical problems solved by T_2 minus the weighted number of the anomalies and the conceptual problems of T_2.

Laudan's account of progress in terms of problem-solving is motivated in part by his contention that attempts to articulate semantic accounts of progress in terms of approximations to truth have failed. According to Laudan (1977), "no one has been able even to say what it would mean to be 'closer to the truth', let alone to offer criteria for determining how we could assess such proximity" (pp. 125-126). Laudan does not object to the cumulative aspect of semantic accounts. He accepts that there are common threads that run through the history of science: "*it is basically the shared empirical problems which establish the important connections between successive research traditions*" (p. 140). This allows for much continuity in an evolving research tradition. "*From one stage to the next*," Laudan says, "there is a preservation of most of the crucial assumptions of the research traditions" (p. 98). He seems to think, however, that mere accumulation is not sufficient for progress. He wants to construe progress in terms of the scope of solved problems. For Laudan, the following are the core assumptions of a problem-solving account of progress: “(1) *the solved problem*—empirical or conceptual—*is the basic unit of scientific progress*; and (2) *the aim of science is to maximize the scope of solved empirical problems, while minimizing the scope of anomalous and conceptual problems*” (p. 66).

The trouble is that Laudan (1977) does not explicitly say what he means by “scope,” only that the “more numerous and weightier the problems are which a theory can adequately solve, the better it is” (p. 66). So, it seems that by “scope” Laudan means the weighted number of adequately solved problems. When assessing progress, then, the numbers of solved and unsolved problems alone are not enough to evaluate the relative progress of theories. We must take into consideration the significance of solutions and solved and unsolved problems as well.

It is noteworthy that in later chapters, Laudan begins talks about research traditions and admits that he used the term ‘theory’ on some occasions in earlier chapters to talk about research traditions (Laudan, 1977, p. 69). So far, I have assumed that when Laudan talks about tensions between theories, he means theories from one research tradition that are in conflict with theories from another research tradition, since he gives examples of theories from different domains when he talks about conceptual problems. For Laudan, a research tradition is not merely the sum of the individual theories that are part it. The theories of a certain research tradition partially constitute and exemplify it. A research tradition “provides a set of guidelines for the development of specific theories” (p. 79). It is “associated with a series of specific theories, each of which is designed to particularize the ontology of the research tradition and to illustrate, or satisfy, its methodology” (p. 81). Theories are “empirically testable for they will entail (in conjunction with other specific theories) some precise predictions," according to Laudan, whereas research traditions "*are neither explanatory, nor predictive, nor directly testable*" (pp. 81-82). A research tradition may be recognized by some of its other characteristics, however, such as "the *metaphysical* and *methodological* commitments which, as an ensemble, individuate the research tradition and distinguish it from others" (p. 79).

The methodological components of the research tradition specify the "legitimate *methods of inquiry* open to a researcher within that tradition," including "experimental techniques, modes of theoretical testing and evaluation, and the like" (Laudan, 1977, p. 79). As Laudan puts it: "*a research tradition is thus a set of ontological and methodological 'do's' and 'don'ts'*" (p. 80). It seems unclear how this claim fits with Laudan's insistence that "Every research tradition has a number of specific theories which exemplify and partially constitute it" (p. 78). Perhaps what he means is that research traditions are constituted by individual theories as well as methodological and ontological assumptions.

What Laudan calls the "evolution of research traditions," then, seems to involve changes of the component theories of the research tradition. These changes are prerequisites for progress because problem-solving effectiveness increases only when individual theories are changed or new ones are added. As Laudan (1977) writes, the "most obvious way a research tradition changes is by *a modification of some of its subordinate, specific theories*" (p. 96). Such modifications can be achieved by slight "alterations in previous theories, modifications of boundary conditions, revisions of constants of proportionality, minor refinements of terminology, expansion of the classificatory network or a theory to encompass newly discovered processes or entities" (p. 96). Nevertheless, the overall research tradition is being altered rather than overturned.

Unlike Kuhn's paradigms (i.e., disciplinary matrix) and Lakatos' research programmes, where, if the fundamental commitments are changed, then a new paradigm or programme emerges, it seems that Laudan's research traditions can survive changes in their core elements. However, at the same time, Laudan (1977) also says that "certain elements of a research tradition are sacrosanct, and thus cannot be rejected without repudiation of the tradition itself" (p. 99). This apparent tension is dealt with by insisting that "*the set of elements falling in this (unrejectable) class changes through time*" (p. 99). As examples, Laudan cites the "Cartesian and Newtonian research traditions" (p. 99).

To sum up, assessing scientific progress is a comparative exercise for Laudan. We have to look at the problem-solving effectiveness of a later theory and compare it to the problem-solving effectiveness of its predecessor. If the later theory has a higher problem-solving effectiveness, where problem-solving effectiveness is construed in terms of the number and significance of solved empirical problems relative to the number and significance of the anomalies and conceptual problems that the theories generate, then progress has been made. Again, as Laudan (1981a) puts it:

> the aim of science is to secure theories with a high problem-solving effectiveness. From this perspective, *science progresses just in case successive theories solve more problems than their predecessors* (p. 145).

For Laudan, then, the aim of science is not true theories, but rather theories with higher degrees of problem-solving effectiveness.[67]

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[67] Another account of progress that should be mentioned, but may be subsumed under the heading of "functional-internalist" accounts, is Imre Lakatos' methodology of scientific research programmes.

3.c. The Epistemic Account of Scientific Progress

So far, we have seen accounts of scientific progress that focus exclusively on (approximate) truth, on the one hand, and accounts of progress that aim to avoid talk about truth altogether, on the other. On semantic accounts, the units of progress are (approximate) truths. On functional-internalist accounts, the units of progress are puzzle-solutions. In light of the historical investigation of Chapter 2, however, it seems rather peculiar that philosophers of science rarely talk explicitly about scientific knowledge and its growth. As Kitcher (2002) observes:

> 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 (p. 385).

As we have seen in Chapter 2, the idea of the growth of knowledge is quite dominant 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*."[68] More recently, George Sarton (1927) expressed the idea as follows: "*The acquisition and systematization of positive knowledge is the only human activity which is truly cumulative and progressive*" (I, pp. 3-4, original emphasis). Sarton also thought that scientific progress was intrinsically linked to social and political progress. That seems questionable. Our focus here remains scientific progress and the growth of scientific knowledge.

According to Lakatos, a research program is progressing when it increases in content that has independent corroboration. A research programme may be degenerating, however, when it obeys the negative heuristic to protect the hard core by reducing its scope or adding *ad hoc* hypotheses. See Lakatos (1978). In addition, Leplin (1981) has argued that "there is a form of progress that consists in extension of the scope of observation, in the ability to observe newly postulated entities" (p. 206).

[68] The Hebrew passage in Daniel 12:4 reads "many shall wander, and knowledge shall increase."

A contemporary exception to the trend noted by Kitcher is Alexander Bird. According to Bird, the answer to the question "What is scientific progress?" is very simple. As Bird (2007) writes:

> 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 (p. 64).

As Bird admits, and as we have seen in Chapter 2, this conception of scientific progress is not original. However, Bird is also puzzled by the fact that philosophers of science have ignored it, at least since Kuhn's *The Structure of Scientific Revolutions* (1962).

Bird (2008) distinguishes between the following accounts of scientific progress. (In this chapter, I have followed his classification of accounts of scientific progress):

> (E) An episode constitutes scientific progress precisely when it shows the accumulation of scientific knowledge. [Epistemic account of progress]
>
> (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. [Semantic account of progress]
>
> (FI) An episode shows scientific progress precisely when it achieves a specific goal of science, where that goal is such that its achievement can be determined by scientists at that time (e.g., solving scientific puzzles) [Functional-internalist account of progress] (p. 279).

By considering several cases, Bird (2007) argues that (i) "our intuitions about whether there is progress show that progress matches changes in knowledge, but not changes in truth or in problem-solving," (ii) (S) "has no theoretical advantages over" (E), and (iii) "the alleged reasons for taking [(FI)] do not stand up to scrutiny" (p. 65).

Against (S), Bird (2007) propounds the following arguments, which show that cases of increase in truth, without justification, are not cases of progress. That is, increase in truth alone is not sufficient for progress.

1. A sequence of lucky guesses or irrational beliefs with increasing verisimilitude will be progressive on (S). Intuitively, however, and on (E), it is not.
2. Suppose a scientific community accidentally forms approximately true beliefs based on an irrational method. One scientist realizes that the method is irrational, and the scientific community rejects its previously held beliefs. On (S), this community was making progress, and then regressed (after it was discovered that the method is irrational). Intuitively, however, and on (E), this community made progress after the discovery that the method is irrational.
3. Suppose that we were to discover that there are entities answering to René Blondlot's description of N-rays. On (S), this would be a case of progress because the belief is true. But Blondlot's reasons for positing N-rays were entirely spurious and irrational, hence not knowledge. (Apparently, his belief was largely the result of self-deception.)
4. On (S), it would have been more progressive to have accepted Alfred Wegener's theory of continental drift at the time. On (E), however, it was rational to reject it, or at least suspend judgment, at the time because Wegener didn't have sufficiently compelling evidence in its support.

According to Bird, these arguments show that increase in truth is not sufficient for progress. As Bird (2007) writes: (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" (p. 65). These intuitions, according to Bird, "imply that epistemic characteristics are essential to progress" (p. 65).

Against (FI), Bird (2007) puts forward the following arguments, which show that cases of increase in puzzle-solutions, without increase in truth, are not instances of progress. That is, increase in problem-solving function does not constitute progress.

1. According to Kuhn, a puzzle is solved when a proposed solution is sufficiently similar to a relevant paradigmatic puzzle-solution. The key factor in choosing a new paradigm is its ability to preserve as far as possible the problem-solving power of its predecessor while permitting the solution or dissolution of as many outstanding anomalies as possible. On Kuhn's view, then, if Nicole d'Oresme can "solve the problem" of how to split diamonds using hot goat's blood by coming up with a solution that is sufficiently similar to an appropriate paradigm, then he has made progress. Now, suppose that someone shows that Oresme's solution cannot work. On Kuhn's view, we had a solution before, now we have none; that is regress. Intuitively, however, and on (E), that person contributed to progress in a small way, by giving us knowledge that something previously thought true is actually false. Oresme's false solution to a false problem is not progress, but the

knowledge that his false solution is indeed false is some small contribution to progress.[69]

2. According to Laudan, a problem *P* is solved by *T* when one can deduce a statement of *P* from *T* (*T* doesn't have to be true; *P* may not even exist).[70] On Laudan's view, then, if Nicole d'Oresme can "solve the problem" of how to split diamonds using hot goat's blood by coming up with a theory from which he can deduce the splitting of diamonds by hot goat's blood, then he has made progress. Now, suppose that someone shows that Oresme's solution cannot work. On Laudan's view, we had a solution before, now we have none; that is regress. Intuitively, however, and on (E), that person contributed to progress in a small way, by giving us knowledge that something previously thought true is false. Oresme's false solution to a false problem is not progress, but the knowledge that his false solution is indeed false is some small contribution to progress.

According to Bird, these arguments show that increase in problem-solving function is not constitutive of progress. Kuhn and Laudan accept contributions to scientific progress that do not involve knowledge. Laudan thinks we never have scientific (theoretical) knowledge because of the Pessimistic Meta-Induction and Kuhn is critical of the notions of truth and verisimilitude.[71] Kuhn's account of scientific change is an attempt to remain neutral with respect to truth and knowledge. However, as Bird argues, collections of data and observations are contributions to scientific knowledge, even though they are not solutions to puzzles and they may not be very exciting. As Bird (2007) writes:

> It is clear that not all scientific knowledge is a matter of knowing the solution to some puzzle. Astronomers and naturalists of the eighteenth and nineteenth century spent lives collecting data on stars and comets, or on new species and habitats. These were contributions, albeit not dramatic, to scientific progress. At

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[69] Recall that, for Kuhn, exemplars, rather than the application of rules, play a dominant role in scientific cognition. A puzzle-solution may be accepted or not, according to Kuhn, depending on its similarity to paradigmatic puzzle-solutions. For Kuhn, determining the similarities between puzzle-solutions is not a matter of following or applying rules. See Bird (2000).

[70] According to Laudan (1981a), "the problem-solving approach allows a problem solution to be credited to a theory, independent of how well established the theory is, just so long as the theory stands in a certain formal relation to (a statement of) the problem" (p. 148). In that respect, Laudan's account is like Hempel's Deductive-Nomological model of explanation (at least superficially) to the extent that, according to the DN model, "the *explanandum* [i.e., a statement describing the phenomenon to be explained] must be a logical consequence of the *explanans* [i.e., the class of those statements that are adduced to account for the phenomenon]" (Hempel, 1965, pp. 247-148).

[71] I will revisit these arguments in Chapter 5.

> the same time there are serendipitous discoveries that progress science whose importance is clear without their being solutions to any puzzles (p. 68).

For Bird (2007), “it is natural to think that success in problem-solving is *evidence for* the progress of science, when the latter is understood as the accumulation of knowledge” (pp. 68-69).

Furthermore, Bird argues, on Kuhn’s view, scientific change (revolutions) is not progressive in any straightforward way. For Kuhn, there is progress through revolutions because the new paradigm may solve more problems than its predecessor. However, as Bird argues, while the later paradigm may solve more problems, it need not solve (or dissolve) all the problems that were previously solved. For example, in the *Principia philosophiae* (1644), Descartes explains planetary motion in terms of his vortex theory. This theory accounts for the motion of the planets by postulating the existence of vortices, it explains why the planets are co-planar and have the same sense of rotation, and it provides an account of the mechanical causes of gravity. Isaac Newton (1642-1727) criticizes Descartes’ vortex theory in his *Philosophia naturalis principia mathematica* (1687). In the General Scholium to Book II in the second edition of the *Principia* (1713), Newton argues that the “hypothesis of vortices is pressed with many difficulties” (Cohen & Whitman, 1999, p. 940). He then goes on to propose his own theory of gravity in Book III.

On Kuhn’s view, in this episode of scientific change from Descartes’ theory to Newton’s theory, there seems to be a loss of puzzle-solutions, for the puzzles about the co-planar motion of the planets, their similar sense of rotation, and the mechanical causes of gravity do not have a Newtonian solution. This reduction in problem-solving function is known as a “Kuhn-loss.” On Kuhn’s view, it seems that we would have to say that the

Descartes-Newton episode is not progressive despite the impressive achievements of Newton’s theory. To describe this episode as progressive in Kuhnian terms, one would have to claim that progress occurred despite loss of problem-solving function. In other words, one would have to claim that the Newtonian theory provided more solutions to other puzzles that outweigh the aforementioned loss. That, in turn, would require that one be able to individuate puzzle-solutions, quantity problem-solving function, and be able to weigh the relative importance of puzzle-solutions.

To see the problem, we have to consider this episode in more detail. Descartes’ vortex theory explains celestial phenomena, including planetary orbits and the motion of comets, by positing the existence of vortices. A vortex is a circling band of corpuscles. Planets and comets are situated in these huge vortices. The Cartesian plenum is thus a network of vortices. The vortex bands consist of either primary or secondary matter (subtle matter). Macroscopic bodies are composed of tertiary matter. For Descartes, these three kinds of matter, along with his laws of nature, account for celestial phenomena as well as for gravity.

According to Descartes, a planet (or a comet) becomes situated in a vortex when its outward tendency (*conatus*) to escape the center of rotation (centrifugal force) is matched by a tendency of the subtle matter that composes the vortex. If the planet’s centrifugal force is greater than that of the minute corpuscles of the vortex, then it will ascend to a higher vortex. If the planet’s centrifugal force is weaker than that of the minute corpuscles of the vortex, then it will descend to a lower vortex. This explains gravity, according to Descartes, for the minute corpuscles around the earth are responsible for terrestrial gravity in essentially the same way. As Descartes writes in Part IV of his *Principia*, entitled "Of the Earth":

> Although the particles of heavenly matter are agitated at the same time by many diverse movements, yet all of their actions harmonize and, as it were, counterbalance one another in such a way that, due solely to their encounter with the bulk of the earth which resists their movements, they strive to move away equally in all directions from its vicinity, as if from its centre; unless by chance some exterior cause introduces diversity into this matter (Miller & Miller, 1983, pp. 193-194).

For Descartes, then, terrestrial gravity is the result of a mechanical interaction or motion of the minute corpuscles (subtle matter) that are whirling around the earth, and whose centrifugal force propels other corpuscles (tertiary matter) toward the earth. The vortex theory not only describes in detail the mechanical causes of gravity but also explains why the planets are co-planar and have the same sense of rotation.

Evidently, Newton wrote his *Principia* with Descartes' *Principia* in mind. First, whereas Descartes wrote the principles of philosophy, Newton sought to improve upon Descartes' work by writing the *mathematical* principles of *natural* philosophy (Cohen, 1985a, p. 211). Second, Newton argues that Descartes' vortex theory cannot possibly explain celestial phenomena. In the Scholium to Book II, Newton writes:

> The hypothesis of vortices is ultimately irreconcilable with astronomical phenomena, and rather serves to perplex than explain heavenly motions. How these motions are performed in free spaces without vortices, may be understood by the first Book, and I shall now more fully treat of it in the following Book (Cohen & Whitman, 1999, p. 392).

Newton demonstrates that vortices cannot yield a planetary system that exhibits orbits in accordance with Kepler's laws. Furthermore, he shows that "a vortex cannot be a self-sustaining system, but continues in uniform motion only as long as an external force continues to turn its central body" (Westfall, 1971, p. 153).

Newton's *Principia* offers an impressive unification of natural phenomena. It shows how Copernicus' heliocentric system, Kepler's laws of planetary motion, and Galileo's analysis of acceleration and parabolic trajectories are the consequences of one universal law. According to Cohen (1985a), Newton

> shows that a single universal force (a) keeps the planets in their orbits around the sun, (b) holds the satellites in their orbits, (c) causes falling objects to descend as observed, (d) holds objects on the earth, and (e) causes the tides. It is the force called *universal gravity*, and its fundamental law may be written $F = G\, mm`/D^2$ (p. 146).

According to the law of universal gravitation, there is a mutual attractive force between any two bodies, which attract each other with a force that is directly proportional to the product of their masses and inversely proportional to the square of the distance that separates them.

Newtonian mechanics explains other natural phenomena as well. Edmund Halley (1656-1742), who initially encouraged Newton to publish the *Principia*, "found that when he went from London to St. Helena it was necessary to shorten the length of his pendulum in order to have it continue to beat seconds" (Cohen, 1985a, p. 175). Newton's theory not only explains this variation but also predicts that the shape of the earth is an oblate spheroid. This prediction was confirmed by the French mathematician Pierre L. M. de Maupertuis (1698-1759), who measured the length of a degree of arc along a meridian in Lapland, and then compared the result with the length of a degree along the meridian near the equator.

In the *Principia*, Newton also shows that the motion of comets is similar to that of planets insofar as they are both moving in orbits that are conic sections. In particular, some comets move in ellipses, and so they can be observed again as they move through the solar system. Based on Newton's theory, Halley predicted that a particular comet would reappear in 1758. The comet, now named after Halley, indeed reappeared as predicted, and the Newtonian theory gained further support.

Newton's extraordinary achievements notwithstanding, on Kuhn's view, it seems that we would have to characterize the Descartes-Newton episode as one that displays regress rather than progress. For this episode seems to involve at least two instances of a "Kuhn-loss." First, the Cartesian theory explains why the planets are co-planar and have the same sense of rotation, whereas the Newtonian theory does not have a solution to this puzzle. Second, the vortex theory gives a detailed account of the mechanical causes of gravity. Gravity, for Descartes, is the result of the mechanical motion of the vortices. This is another "Kuhn-loss" because the Newtonian theory offers no solution to this puzzle. Indeed, Christiaan Huygens (1629-1695) and Gottfried Wilhelm Leibniz (1646-1716) criticized Newton's theory on these grounds.

In addition to his priority dispute with Newton over the calculus, Leibniz objected to Newton's theory of gravity. In *Against Barbaric Physics* (1710-16), Leibniz writes:

> it is astonishing that there are those who now, in the great light of our age, hope to persuade the world of a doctrine so foreign to reason (Ariew & Garber, 1989, p. 312).

Leibniz expounds his objections in his correspondence with Samuel Clarke (1675-1729). For Leibniz, postulating an attractive force that acts between any two bodies across empty space is incompatible with the mechanical philosophy. Newtonian physics thus introduces occult qualities in the form of a mysterious attractive force. It also implies that the motion of bodies is a perpetual miracle because of this mysterious force.[72]

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[72] See Alexander (1956).

Huygens shared Leibniz's doubts about attraction. According to Huygens, attraction cannot be explained in terms of the mechanical philosophy and the laws of motion. In order to explain natural phenomena, one must refer to the motion of corpuscles and their interactions by direct contact. Although he disagreed with Descartes about the existence of a void, Huygens followed the Cartesian theory in supposing that "gravitation is the effect of corpuscular motion propagated by contact-action through vortices" (Snelders, 1989, p. 218).

As Bird (2007) points out, contrary to Kuhn's view, it seems reasonable to say that the Descartes-Newton episode is progressive, Kuhn-losses notwithstanding. Even Newton's critics, such as Huygens, acknowledged his success in developing mathematical tools for explaining celestial phenomena, and recognized that his law of gravitation is essentially correct. The epistemic account of progress can easily accommodate this episode. As Bird argues, on the epistemic view, Kuhn-losses are not progressive because the puzzle-solutions that are lost are not knowledge. Since gravity is not explained by vortices, Descartes did not know what explains the co-planar orbits of the planets and their similar sense of rotation. Nor did he know the mechanical causes of gravity. On the epistemic view, all we have lost is false beliefs. If anything, we have thereby made progress (Bird, 2007, p. 71).

For Bird, then, there is cognitive progress in science whenever there is an increase in scientific knowledge. It is important to note that Bird does not take knowledge to be justified true belief. He thinks that his arguments about scientific progress support

Timothy Williamson's view that knowledge is a foundational concept in epistemology and that it does not have an analysis.[73] According to Bird (2007):

> A major feature of [Williamson's] conception [of knowledge] is the view that belief aims at knowledge. It is as if we only have the concept of belief in order to describe the mental state we are in when we have attempted to know but have failed. While Williamson does not argue for this claim directly, the motivating role it plays in his overall conception means that the success of his arguments would strongly support that view. From this perspective, the arguments for the knowledge view of scientific progress can be seen as a contribution to the same project (p. 85).

So, according to Bird, if belief aims at knowledge, then scientific belief aims at scientific knowledge. Cognitive success in science would then be attaining knowledge rather than merely truth. This is another advantage of the epistemic account of scientific progress over the semantic account, according to Bird (2007), not only because aiming at truth, unlike aiming at knowledge, does not entail that one desires to avoid falsehood as well, but also because knowledge is harder to achieve, and thus more stable than true belief (p. 83).[74]

To sum up, I have discussed three types of philosophical accounts of scientific progress: semantic accounts (S), functional-internalist accounts (FI), and the epistemic account (E):

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

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[73] See Williamson (1997) and (2000).

[74] Henceforth, I will take knowledge to be unanalyzable in the same way that Williamson and Bird do. Although I think that there are other conceptions of knowledge that would work just as well with the epistemic account of scientific progress. It is crucial, however, to construe knowledge, or justification, along externalist rather than internalist lines. Roughly, internalism is the view that a subject needs to have some kind of access to her grounds for knowledge or justified belief for her beliefs to count as knowledge. Externalists deny that a subject can always have this kind of access or be aware of her grounds for knowledge or justified belief. See the Appendix "The Analysis of Knowledge."

> (FI) An episode shows scientific progress precisely when it achieves a specific goal of science, where that goal is such that its achievement can be determined by scientists at that time (e.g., solving scientific puzzles) (Bird, 2008, p. 279).

Our next task will be to examine these accounts from a naturalistic perspective. Just as scientific theories are responsible to the workings of nature, and are evaluated relative to natural phenomena, a theory about the assessment of scientific progress is a theory about an evaluative practice in science, and is responsible to this practice. To paraphrase Quine, Chapter 4 will be an attempt to find out how these philosophical accounts of scientific progress fare when they “face the tribunal of” scientific practice.

Chapter 4
Philosophical Accounts and the Tribunal of Scientific Practice

In Chapter 1, I suggested that the naturalistic approach requires philosophical accounts of scientific progress to do justice to the scientific practice of assessing progress. Naturalism in philosophy of science requires that philosophical accounts be judged relative to scientific practice just as scientific theories are judged relative to natural phenomena. As we have seen in Chapter 2, the scientific practice of assessing progress is couched in epistemic terms. That is to say, scientists evaluate progressive episodes in science on the basis of epistemic criteria, specifically knowledge, as opposed to truth alone. Their criterion for judging scientific discoveries as progressive is simply to assess what contributions to scientific knowledge these discoveries have made. If, due to and after a certain discovery, we know more about nature, then progress has been made. “Knowing more” should not be construed in terms of theoretical knowledge alone. As we have seen in Chapter 2, scientists consider other sorts of knowledge to be just as important as theoretical knowledge (explanations and hypotheses). These include empirical knowledge (“data”), practical knowledge (immediate and long term practical applications), and methodological knowledge (methods and techniques of learning about nature).

In this chapter, I would like to examine the philosophical accounts of scientific progress outlined in Chapter 3, to see whether or not they mesh with the actual practice of scientists. From a naturalistic standpoint, if assessing progress is a scientific practice, then a philosophical account of progress should do justice to this practice. As previously mentioned, I take it that no one denies that science is making progress; the question is in what this progress consists. Semantic accounts take scientific progress to consist in the accumulation of (approximate) truths or approaching truth. Functional-internalist accounts take scientific progress to consist in the accumulation of puzzle-solutions or increase in problem-solving function. And the epistemic account takes scientific progress to consist in the accumulation of scientific knowledge. I would like to find out whether or not these accounts of scientific progress can accommodate the scientific practice of assessing progress.

4.a. Semantic Accounts and the Tribunal of Scientific Practice

On semantic accounts of scientific progress, we get the following picture of science. The scientific enterprise is a truth-aiming enterprise. That is to say, the aim of scientific inquiry is (approximate) truth. Scientists are making progress by approaching this goal. More explicitly, they make progress either by accumulating more (approximately) true beliefs about nature or by getting increasingly closer to the truth. On this account, we are going somewhere worth going, i.e., truth. We are also collecting something worth collecting, i.e., true beliefs. What could be more epistemically worthy than truth?

There are several objections to this picture of science, as advanced by the likes of Kuhn and Laudan. I will discuss these objections in Chapter 5. For now, I simply wish to see how semantic accounts fare when judged relative to the scientific practice of assessing progress. As we have seen in Chapter 2, scientists assess scientific progress on the basis of criteria that are literally epistemic, i.e., that have to do with knowledge. 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. The naturalistic approach requires that a philosophical account of scientific progress be able to account for this scientific practice.

Truth is indeed epistemically worthy. Since knowledge entails truth, there can be no scientific knowledge without truth. However, semantic accounts seem to be somewhat detached from actual scientific practice insofar as they focus on theoretical truth almost exclusively, whereas the scientific practice of assessing progress reveals that scientists take knowledge of various sorts to be the relevant cumulative unit of progress. This means that justification and methodology are as important as truth as far as scientists are concerned. For example, in the case of Pavlov's work on the physiology of digestion, it was judged to have been progressive in terms of theoretical and empirical knowledge:

(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 practical and methodological knowledge 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), 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” (p. 326). Before Pavlov, experiential physiologists worked mostly with anesthetized animals. However, 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) 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” (p. 327).

As for practical knowledge, Wood (2004) 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” (p. 327). 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).

According to Hacking (1983), science is as much about experimenting as it is about theorizing. Experimentation is not simply reporting, according to Hacking, but doing, and not merely doing things with words (p. 173). As Hacking (1983) 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*. This is odd, because 'experimental method' used to be just another name for scientific method (p. 149, my emphasis).

Hacking shows that the claim that an experiment must be preceded by theory is ambiguous. The weak version of this claim is that one must have some ideas in mind about one's apparatus and some expectations about nature before one conducts an experiment. But that is trivial. According to Hacking, no one would dispute this weak version. It seems that even Bacon, who is often (erroneously) thought of as advocating a method of discovery based on mindless observation and the accumulation of data alone, realized that "completely mindless tampering with nature, with no understanding or ability to interpret the result, would teach almost nothing" (Hacking, 1983, p. 153). This is suggested by Bacon's distinction between the Empirics, who, like the ants, simply collect and use, and the Rationalists, who, like the spiders, make out of their own substance. The true natural philosopher, according to Bacon, embodies a union of the empirical and rational faculties, like the bees that transform natural materials.[75] Bacon

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[75] See Rossi (1996).

also distinguishes between experiments of light (*lucifera experimenta*) and experiments of fruit (*fructifera experimenta*).

The strong version of the claim that experiment must be preceded by theory, according to Hacking (1983), says that “your experiment is significant only if you are testing a theory about the phenomena under scrutiny” (p. 154). Hacking argues that this strong version is untenable. He provides several examples to show that it is not the case that there must be a hypothesis under test in order for experiments to make sense. According to Hacking (1983), “One can conduct an experiment simply out of curiosity to see what will happen” (p. 154). One example of a “noteworthy observation,” as Hacking calls them, is the discovery of polarization by reflection in 1808 when E. L. Malus, (1775-1812), a colonel in Napoleon’s corps of engineers, was experimenting with Iceland Spar. He “noticed the effects of evening sunlight being reflected from the windows of the nearby Palais du Luxembourg” (Hacking, 1983, p. 157). Now, sometimes experiments are done and data are accumulated, but then they have to be put on hold for a while until someone comes along and realizes how they can be used. For example, Robert Brown (1773-1858) reported on the irregular movement of pollen suspended in water in 1827. But no one knew what to make of Brownian motion until Jean Baptiste Perrin (1870-1942) and Albert Einstein (1879-1955) came along.

Accordingly, it seems that any blanket statement about theory preceding experiment is bound to leave out some instances of momentous discoveries in science. But this is not surprising. It seems that there are no fixed patterns in this case. It seems that we have to take it on a case by case basis. As Hacking (1983) writes:

> Some profound experimental work is generated entirely by theory. Some great theories spring from pre-theoretical experiment. Some theories languish for lack

> of mesh with the real world, while some experimental phenomena sit idle for lack of theory (p. 159).

There are also happy, albeit rare, occasions in which theory and experiment meet in harmony. For example, the experimental work of Arno Penzias and R. W. Wilson, who discovered the Microwave Background Radiation in 1965, meshed beautifully with the work that was done by R. H. Dicke and others at Princeton on the Big Bang theory at the time.[76]

Likewise, it seems to me that Baird and Faust (1990) correctly point to the problem of focusing exclusively on theories in philosophy of science when they write:

> Philosophers speak of *science* in terms of theory *and* experiment. Yet when they speak of the *progress of scientific knowledge*, they speak in terms of theory *alone*. According to most philosophers, experiments are run in order to promote theory; improvements in the ability to experiment are merely instrumental goods that promote the final good of justifying the assertions of a wider and wider domain of sentences. Thus, according to most philosophers, improved theories account for the progress of scientific knowledge. It is our contention that this asymmetry is a mistake. Technicians, engineers and experimenters, in a vast number of instances, 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 (p. 147, original emphasis).[77]

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 part of the reason for doing experimental work is to provide data that is the material of theoretical work. I will come back to Baird and Faust's diagnosis of the source of this mistaken asymmetry in Chapter 6, and I will propose a way to avoid it. For

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[76] See also Leplin (1997), pp. 94-97. For Leplin, this counts as "novel predictive success" according to his analysis of novelty.

[77] Baird and Faust seem to be suggesting that philosophers of science talk about knowledge, but when they do, it is theoretical knowledge exclusively. It seems to me, however, that Kitcher (2002) rightly points out that, within the philosophy of science, "there is little explicit discussion of scientific knowledge" (p. 385).

now, I simply wish to point out that semantic accounts of scientific progress, given their emphasis on theoretical truth exclusively, leave out important aspects of science (i.e., experimentation, methodology, and practical applications) that scientists seem to take seriously when they evaluate progress. If this is correct, then semantic accounts of scientific progress fail to do justice to the history of science and mesh with the actual practices of scientists.

4.b. Functional-Internalist Accounts and the Tribunal of Scientific Practice

On functional-internalist accounts of scientific progress, we get the following picture of science. The scientific enterprise is a problem-solving enterprise. That is to say, the goal of scientific inquiry is puzzle-solutions. Scientists make progress by accumulating more and more puzzle-solutions. On this view, are we going somewhere worth going? Are we collecting something worth collecting? The problem seems to be that it is not clear how problem-solving function and puzzle-solutions are epistemically valuable.

It seems that functional-internalist accounts of progress do not mesh with the scientific practice of assessing progress. It seems that such accounts cannot do justice to the scientific practice of assessing progress not simply because of the obvious reason that scientists talk about progress in terms of knowledge rather than puzzle-solutions, but also because they leave out growth in empirical knowledge. Recall that, on functional-internalist accounts, such as Laudan’s, problems do not have to be genuine problems and solutions do not have to be correct solutions. But that seems to undermine their epistemic worth. For example, it seems to me that we wouldn’t be impressed if, say, Landsteiner were able to deduce a solution to an unreal problem involving blood clumping. On

Laudan's view, however, a theory "solves a problem" if a statement of the problem can be deduced from it, and that counts as progress. For Laudan (1977), "A problem need not accurately describe a real state of affairs to be a problem: all that is required is that it be *thought to be* an actual state of affairs" (p. 16). As we have seen in Chapter 2, however, the problem of agglutination was not merely thought to be real by Landsteiner himself. If that were the case, he probably would not have received the Nobel Prize. It is the case, as Landsteiner knew, that mixing blood from two human beings can lead to agglutination. And it is the case that clumped red blood cells can crack and cause toxic reactions. What impressed Landsteiner's peers and the Nobel Committee was that his explanation in terms of blood groups was a solution to a real problem.[78]

Functional-internalist accounts thus underestimate the role that empirical knowledge plays in science and in assessments of scientific progress. It seems that Kuhn and Laudan want to allow scientific progress even if nothing of epistemic significance is accumulated. Both Laudan and Kuhn are skeptical about scientific knowledge. Laudan thinks that the Pessimistic Meta-Induction shows that theoretical knowledge is unattainable, whereas Kuhn attacks the concepts of truth and verisimilitude. In their attempts to eschew theoretical knowledge, however, functional-internalists give up other kinds of scientific knowledge as well, especially empirical knowledge. As we have seen in Chapter 2, however, this sort of knowledge is also important to scientists when they evaluate scientific discoveries.

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[78] Recall that, for Laudan, the solution of a problem *P* by a theory *T* means that the "statement of the problem" is deduced from *T* (See Chapter 3, section 3.b.ii). For Laudan, *T* doesn't have to be true. He doesn't even require that *P* really exist. It is interesting to note, however, that in the first chapter of *Science and Values* (1984), Laudan says that he offers his solutions to "two puzzles about science," and then he says that "before I can expect my solutions to be taken seriously, I need to show that the problems I am grappling with are both *real* and as yet unresolved" (p. 1, my emphasis). Why does Laudan think that, when it comes to problems about science, he must show that they are "real," but when it comes to scientific problems, they don't have to be "real"?

For example, take that case of Ehrlich and Metchnikoff. In 1882, after observing phagocytes surrounding and attempting to devour a splinter he had introduced into the transparent body of a starfish larva, Metchnikoff proposed his “phagocytosis theory,” according to which cells called “phagocytes” (from Greek, ‘phago’, “to devour,” and ‘cytos’, “cell”) play a role in host defense by engulfing and breaking down foreign particles that invade the body.[79] Ehrlich proposed that harmful compounds can mimic nutrients for which cells express specific receptors. He realized that receptors on host cells specifically triggered by harmful substances were produced in excess as a way to neutralize them.[80] Although their theories were perceived as being at odds for a while, both Metchnikoff’s phagocytosis theory and Ehrlich’s side-chain theory were based on the following empirical knowledge:

(EK) knowing that organisms can acquire immunity against certain diseases (as when organisms that have gone through an infectious disease will acquire protection against being attacked by that disease).

Based on this knowledge, Metchnikoff made the following contribution to the growing body of theoretical knowledge in immunology:

(TK) knowing that microorganisms can be destroyed by the activity of cells in the organism, in particular, cells known as “phagocytes” can destroy harmful microbes that invade the organism and also render certain bacterial poisons harmless.

And Ehrlich made the following contribution to the body of theoretical knowledge in immunology:

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[79] See Tauber & Chernyak (1991).

[80] See Kaufmann (2008).

(TK) knowing that poisons that are produced by certain bacteria cause the production of “antibodies” in organisms that have an antagonistic effect against those poisons.

In addition, and equally important, Metchnikoff and Ehrlich’s discoveries were judged to have been progressive in terms of practical and methodological knowledge as well. Metchnikoff’s work was especially progressive in terms of practical knowledge:

(PK) knowing how to prognosticate certain diseases using the degree of the phagocytosis as a prognostic; knowing how to use the phagocytosis as an indicator of the opsonic strength of the blood (an opsonin is any blood-serum protein that increases the susceptibility of microorganisms to phagocytosis); knowing how to increase phagocytosis around a certain area of the body during surgery to ward off infections (e.g., by using quinine).[81]

Whereas Ehrlich’s work was especially progressive in terms of methodological knowledge:

(MK) knowing how to use chemical dyes for selective cell staining; knowing how to standardize antisera for passive vaccination against tetanus and diphtheria and optimize immunization regimens to reach high antibody titers.

It seems that functional-internalist accounts of progress cannot accommodate the importance of empirical knowledge as relevant to assessments of progress because they conceive of the scientific enterprise as providing solutions to problems. But not all scientific knowledge is solutions to problems. Host defense, for example, does not seem

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[81] See Tauber (2003).

to be a solution to any problem. It is a natural phenomenon that Metchnikoff, Ehrlich, and others tried to explain. Even if we grant that scientific knowledge is simply solutions to problems, either conceptual or empirical, it seems difficult to explain, on functional-internalist accounts, why we care about this kind of knowledge. More importantly, it seems difficult to explain why scientists care about this kind of knowledge. As we have seen in Chapter 2, however, the question of the mechanism of host defense was not merely thought to be a real question by Metchnikoff or Ehrlich. If that were the case, their colleagues would probably not have deemed them worthy of the Nobel Prize. It is a fact that organisms have defense mechanisms against bacterial invaders. Metchnikoff and Ehrlich's colleagues, and the Nobel Committee, were impressed with their work because they thought that they have managed to uncover parts of the mechanisms of host defense.

Another problem with functional-internalist accounts is that they seem to underestimate the role that the adducing of evidence and arguments plays in scientific change, and thus in assessments of progress. The example of Harvey's discovery of the circulation of the blood seems to illustrate this point. As we have seen in Chapter 2, there were some significant threads of continuity between Galen's theory and Harvey's theory of the circulation of the blood (e.g., the question of anastamoses in the heart's septum). According to Kuhn, however, scientists who work in different paradigms "live in different worlds." For example, Kuhn (1996) claims that, "after Copernicus, astronomers lived in a different world" (pp. 116-117). This claim is ambiguous; it may be construed in at least two ways. First, it may be construed metaphorically in that scientists who work in different paradigms live in different worlds, as it were, insofar as their beliefs about the world are different (perhaps even at odds). Second, it may be construed realistically insofar as the physical world in which scientists live is different depending on the paradigm in which they work. The latter seems highly implausible to me,[82] so I will take Kuhn as claiming the former.

From the claim that scientists who work in different paradigms live in different worlds psychologically, as it were, insofar as their beliefs about the world are radically different, it seems to follow that there could be no sharing of reasons that would serve as common grounds for adducing arguments for and against competing theories. On this view, arguments for and against theories are always inconclusive, and so there must be other reasons why scientists accept one theory rather than another, the most important one for progress being the problem-solving power of the theory, as we have seen in Chapter 3.

Now, it seems to me that Kuhn's account cannot accommodate the case of Harvey's discovery of the circulation of the blood. On Kuhn's view, it might seem that there is a "Kuhn loss" because Galen's theory explains digestion and nutrition and Harvey's theory does not.[83] But, then, how can we say that progress was made when Galen's theory was abandoned and Harvey's theory was finally accepted? It seems to me that we can't, unless we look at the reasons for and against each theory as they were advanced by practitioners at the time. The Riolan-Harvey exchange is illuminating in this regard. Jean Riolan (1577-1657) was an ardent advocate of Galen's theory. However, even he was forced to concede that blood circulates in the major vessels like the aorta and the vena cava. He remained obstinate in part because of his wish to salvage the ancient

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[82] See Goldman (1999), chap. 1; Wilson (1996); Freudenthal (1984).

[83] For Galen, digestion is a fermentation process involving heat within the stomach. This heating of the food, according to Galen, converts it into chyle, and then into the four humors, blood, phlegm, yellow, and black bile, which are then absorbed in the body.

medicine (Whitteridge, 1971, pp. 181-182). Because of that, perhaps, he did not or could not reply to Harvey's *Anatomical Disquisitions* (1648), in which Harvey replies to his objections to the circulation.

Contrary to Kuhn's claim that the initial adoption of a new paradigm is a matter of personal, political, or aesthetic preferences, because there is no common ground based on which practitioners can debate the merits of competing theories, it seems that reasons for and against a theory do play a role in scientific change. To see how, consider Harvey's theory of the circulation of the blood, which involves the following claims:

(BC1) The pulse of the heart delivers a quantity of blood from the vena cava into the arteries that ingested food alone cannot account for; the blood is transferred in such a way that the whole mass of blood passes through the heart in a short time.

(BC2) The arterial pulse drives a quantity of blood into every part of the body that is much greater than what is required for nutrition.

(BC3) The veins bring the blood from every part of the body back to the heart (Bowie, 1889, p. 71).

Harvey supported these claims with the following evidence and arguments:

1. *The quantitative argument*: Harvey considered the size of the ventricle, the amount of its contents ejected with each contraction, and the rate of its beat. If there is no circulation, he reasoned, then the amount of blood ejected by the heart into the arteries in half an hour would be greater than the amount of blood in the whole body and the heart would eject a greater weight of blood in a day than the weight of the whole body. Since that is not the case, it must be that the same

blood is continuously passing through the heart. In other words, blood circulates around the body (Bowie, 1889, pp. 63-65).

2. *Ligature experiments*: Harvey used ligatures (tourniquets) to show that arteries carry blood away from the heart and that veins carry it back to the heart. Since arteries are deeper than veins in the limbs, a tight ligature around the arm compresses both ligatures and veins. In this case, there is no change in the hand or veins. A less tight ligature compresses the veins but not the arteries. In this case, the rush of blood causes the hand to swell. When a tight ligature is loosened, the rush of blood to the limb causes the hand the veins to swell. This shows, according to Harvey, that blood flows from the heart through the arteries to the peripheral tissues and then from the arteries to the veins. Furthermore, venous valves allowed blood to flow only toward the heart (Bowie, 1889, pp. 68-70).
3. *Comparative anatomy*: Harvey weighted the blood ejected per unit of time from a severed main artery of a sheep. He also found that when he compressed the vena cava of a live snake, the heart blanched and emptied. But when the aorta was compressed, the heart became swollen and purple (Bowie, 1889, pp. 51-52, p. 56).
4. *Explanatory power*: Harvey argued that the difference in the thickness of the walls between arteries and veins can be explained by his theory of circulation. The thicker walls of the arteries are required to withstand the impulsion of blood thrust out from the left ventricle of the heart. Harvey also observed that arterial wall thickness decreased with increasing distance from the heart until the extremities in which the wall thickness of the arteries and the veins is the same (Bowie, 1889, pp. 89-90). In addition, Harvey could explain why the left ventricle

and arteries are often empty of blood in post mortem, whereas the veins are full, since blood flows from the veins to the arteries only through the heart and lungs. Because the heart continues to pump blood for a while after the lungs stop functioning, the left ventricle continues to pump blood out of the arteries into the veins but receives no further blood through the lungs (Bowie, 1889, pp. 141-142).

As Mowry (1985) shows, these arguments, and Harvey's replies to critics, such as Riolan, provided good reasons in support of his theory of the circulation of the blood in the early stages of this episode of scientific change (circa 1630, after the publication of *De motu cordis* in 1628).

By the end of this episode, circa 1650, there were good reasons for accepting Harvey's theory. According to Mowry (1985), these reasons included the following:

1. Harvey provided a unified explanation of diverse phenomena, such as difference in wall thickness between right and left ventricles, post mortem findings showing the left ventricle and arteries empty of blood and the veins full of blood, etc.
2. Harvey's findings were independently supported by the work of others, such as Johannes Walaeus (1604-1649),[84] Johann Vesling (1598-1649),[85] and P. M. Schlegel (1605-1653).[86] For example, (BC3) was supported by Walaeus' experimental work in which he placed a ligature on the vena cava above and below the heart. He thus showed that the stretch of vein between the ligature and the heart was empty, and that the heart became pale and small, in both cases (Mowry, 1985, p. 74). Walaeus also expanded on Harvey's work by applying the

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[84] See Schouten (1974).

[85] See Lubitz (2004).

[86] See Willis (1878), pp. 170-179.

> ligature method in order to show that chyle derived from the gut and flowed via the lacteal vessels into the blood circulation (Pagel, 1976, pp. 115-117).

Functional-internalists, it seems, would be hard pressed to explain why this gradual accumulation of evidence in favor of Harvey's theory, if not persuaded his critics, then at least left them speechless (e.g., Riolan) in the face of this mounting compelling evidence in support of his theory.

4.c. The Epistemic Account and the Tribunal of Scientific Practice

According to Bird (2007), "Given that science is an epistemic activity it seems almost tautologous to suggest that its success and so progress should be measured by epistemic standards" (p. 65). On the epistemic view of scientific progress, knowledge, as opposed to truth alone, is the concept we need in order to understand scientific progress. The epistemic account of progress says that a scientific episode constitutes scientific progress precisely when it shows the accumulation of scientific knowledge. Bird shows that the epistemic and semantic accounts diverge insofar as taking beliefs with insufficient epistemic support to count as knowledge is concerned. Whereas functional-internalist accounts differ from the epistemic account insofar as they consider all scientific knowledge to be a matter of finding a solution to a puzzle, where knowledge is clearly not understood in the traditional way as requiring truth.

In addition to Bird's arguments in support of the epistemic account of scientific progress, outlined in Chapter 3, I would like to point out the following advantages of this account in light of the examination of the scientific practice of assessing progress in Chapter 2:

- The epistemic account captures the accumulation of empirical knowledge (“data”) about nature.
- The epistemic account captures the adducing of evidence and arguments for and against theories.
- The epistemic account does justice to the scientific practice of assessing progress. Scientists not only conceive of the aims of science in epistemic terms (i.e., knowledge) but also assess scientific progress on the basis of epistemic criteria (i.e., the accumulation of knowledge).

Accordingly, semantic accounts of scientific progress neglect some aspects of progress that are evident in the scientific practice of assessing progress. These aspects include types of knowledge that are not strictly theoretical, which are nonetheless taken under consideration by scientists when they make judgments about progress. It seems that functional-internalist accounts fare no better when judged relative to the scientific practice of assessing progress. Functional-internalists, like Kuhn and Laudan, do not think that theoretical knowledge is attainable. In eschewing theoretical knowledge, however, they fail to account for other kinds of knowledge that scientists take into consideration when they evaluate scientific progress. In doing so, functional-internalists also underestimate the role that evidence and arguments play in episodes of scientific change.

When it comes to accounting for scientific progress, the epistemic account of scientific progress seems more promising than the semantic and functional-internalist accounts insofar as it seems to do justice to the scientific practice of assessing progress. On the epistemic view, science is a knowledge-seeking enterprise. Scientific progress consists in the accumulation of scientific knowledge. A scientific episode is progressive when it shows the accumulation of knowledge rather than verisimilitude or puzzle-solutions. The epistemic account 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.[87]

I think that Bird is right about knowledge being the concept we need in order to understand scientific progress. But I also think that the epistemic account needs to be revised in light of the investigation of Chapter 2. I will propose a revised version of the epistemic account of scientific progress in Chapter 6. Before I do so, however, there is a more pressing matter to address. If the aforementioned considerations are correct, there remains an obvious question: If an answer to the question "What is scientific progress?" in terms of the accumulation of scientific knowledge seems so natural, perhaps even tautologous, as Bird argues, and if this is indeed the way scientists themselves evaluate progress, then why is it that philosophers of science (with the exception of Bird, most recently) rarely propose such an answer? I think that this has to do with skeptical arguments, particularly against the attainment of theoretical knowledge in science, advanced by the likes of Kuhn and Laudan. So, in the next chapter, I will address these skeptical arguments. I will argue that these skeptical arguments do not provide compelling reasons for skepticism and pessimism about the accumulation of scientific knowledge at the theoretical level. Then, in Chapter 6, I will propose that there are additional reasons to be optimistic about the accumulation of scientific knowledge, provided we count as scientific knowledge the types of knowledge that scientists actually

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[87] 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.

value. This, in turn, will require extending Bird’s epistemic account of progress, which was outlined in Chapter 3.

Chapter 5
Skepticism and Pessimism

Given that scientists take scientific progress to consist in the accumulation of scientific knowledge, as we have seen in Chapter 2, I argued that naturalists should articulate an account of progress that does justice to this scientific practice. Taking a naturalistic stance vis-à-vis the question of scientific progress, we want an account of progress that meshes with the history of science and the actual practices of scientists. In Chapter 4, I argued that, of the accounts of progress outlined in Chapter 3, the epistemic account is best suited for this task. The epistemic account simply says that scientific progress consists in the accumulation of scientific knowledge. Why is it, then, that philosophers of science have largely ignored this account of progress? I think this has to do with skeptical arguments, particularly against theoretical knowledge, which are advanced by the likes of Kuhn and Laudan.

In this chapter, I will address these arguments. The arguments in question are usually advanced by anti-realists against scientific realism.[88] These are Kuhn's attack on truth and the related thesis that observation is theory-laden, the underdetermination of theory by data, and Laudan's Pessimistic Meta-Induction. If any of these succeeds, then it might seem as if theoretical knowledge in science cannot be attained. I will argue that these arguments do not provide compelling reasons for skepticism and pessimism about the growth of theoretical knowledge in science.

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[88] There are many different versions of scientific realism in the literature. Roughly, scientific realism is usually construed as involving one or more of the following theses: (a) "The Metaphysical Thesis: The world has a definite and mind-independent structure"; (b) "The Semantic Thesis: Scientific theories should be taken at face-value. They are truth-conditioned descriptions of their intended domain, both observable and unobservable. Hence, they are capable of being true or false. The theoretical terms featuring in theories have putative factual reference. So, if scientific theories are true, the unobservable entities they posit populate the world"; (c) "The Epistemic Thesis: Mature and predictively successful scientific theories are well-confirmed and approximately true of the world. So, the entities posited by them, or, at any rate, entities very similar to those posited, inhabit the world" (Psillos, 1999, p. xix).

5.a. Kuhn and the Transcendence of Truth

At the outset, I would like to make a clarification. If one is skeptical about knowledge in general, then clearly one would not be impressed by scientific knowledge as well. So, a prerequisite for this discussion, as it were, is to grant that ordinary knowledge is sometimes attainable. For example, I know that I am sitting in front of my computer right now. My epistemic justification or evidence for my belief that I am sitting in front of my computer right now might consist in my sensory perceptions (or that perception is usually a reliable belief-forming process): I see that I am sitting in front of my computer; I feel my fingers pressing against the keys on the keyboard, I hear the sound of the keys as I am typing these words, and so on. If I am not sure about this, I can always reassure myself by asking my wife to validate my beliefs. She can then tell me, based on her sensory perceptions, that I am in fact sitting in front of the computer right now.

Now, I am not suggesting that to doubt that we can know such things is absurd. This kind of global skepticism is interesting and important. Metaphysicians and epistemologists take it very seriously, as they should. However, when we introduce this kind of global skepticism, it seems to me that we are no longer doing philosophy of science. We are leaving this area of inquiry and entering the realm of metaphysics or epistemology. So, I am suggesting, as many philosophers of science do, that we set this kind of global skepticism aside and focus on science.[89] This means, at the very least, that doubts about knowledge in this context must be local doubts about scientific knowledge, i.e., how scientific knowledge is attainable, rather than how knowledge in general is attainable (given skeptical scenarios involving evil demons and the like). With this

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[89] See, e.g., Shapere (1984).

preliminary note, I now would like to suggest that Kuhn’s attack on truth blurs the line between global skepticism and local doubts.

In the Postscript to the second edition of *The Structure of Scientific Revolutions*, Kuhn (1996) writes:

> One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizations like that refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, to the match, that is, between the entities with which the theory populates nature and what is “really there.”
>
> Perhaps there is some other way of salvaging the notion of ‘truth’ for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like ‘really there’; the notion of a match between the ontology of a theory and its “real” counterpart in nature now seems to me illusive in principle. Besides, as a historian, I am impressed with the implausibility of the view. I do not doubt, for example, that Newton’s mechanics improves on Aristotle’s and that Einstein’s improves on Newton’s as instruments for puzzle-solving. But I can see in their succession no coherent direction of ontological development. On the contrary, in some important respects, though by no means in all, Einstein’s general theory of relativity is closer to Aristotle’s than either of them is to Newton’s (pp. 206-207).

Kuhn seems to argue that a correspondence conception of truth leads to skepticism about truth. The argument seems to run as follows: On the correspondence theory of truth, the truth of statement *S* is a matter of its matching the fact *F*. If the truth of *S* is a matter of its matching *F*, then knowing that *S* is true is knowing that *S* and *F* match. Knowing that *S* and *F* match requires considering *S* and *F* independently and then realizing that they match. However, where *S* is a scientific theory *T*, access to *F* is always mediated by theories such as *T*. Since we do not have unmediated access to *F*, i.e., we cannot consider *F* independently of *T*, we cannot know that *T*.

Notice, however, that this argument seems to be treading the fine line between global skepticism and local doubt. If this argument were sound, then it would seem to undermine any conception of truth that relies on the commonsensical idea that truth depends on what is the case or the way the world is. For Kuhn assumes that knowledge of the truth of *T* requires independent access to *T* and to the domain in nature that *T* purports to describe, i.e., *F*. But why should we require separate access to *T* and *F*? Why can't knowledge of *F* be instrumentally-mediated or mediated by inference? For instance, we might think that the matching between *T* and *F* is the best explanation for some statement *P* that is deduced from *T*. Alternatively, we might think that *F* is the best explanation for some other facts, and then we could infer the truth of *T* from *F*.

Kuhn, of course, would not be impressed by these considerations because he thinks that observation is theory-laden. We have already seen in Chapter 4 that the thesis that theory always precedes experiment is ambiguous. The weak version is trivial, whereas the strong version is unwarranted. It seems that the same considerations apply to the thesis that observation is theory-laden. This thesis may be construed as the claim that one must have some assumptions and expectations about the domain of interest before one makes an observation. As in the case of experimentation, this is a trivial claim. As Hacking (1983) says, "if you want to call every belief, proto-belief, and belief that could be invented, a theory, do so" (p. 176). That would simply make the claim about observation being theory-laden trifling. He provides examples of noteworthy observations from the history of science that have included no theoretical assumptions at all. To Hacking's examples, we might add Darwin's observations of finches on the Galapagos Islands.[90] This example is doubly instructive here, for it seems that those who might construe the thesis that observation is theory-laden in this way, i.e., covering any belief, assumption, or expectation one may have before conducting an observation, are

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[90] See Kohn (1980). Cf. Sulloway (1982).

making a mistake that is similar to the one creationists and intelligent design advocates are making when they accuse evolution by natural selection of being “just a theory.” A scientific theory is not just any belief, hunch or guess.[91]

Now, I would like to suggest that the thesis that observation is theory-laden, construed as the claim that one must have some beliefs, assumptions, and expectations about the domain of interest before one makes an observation, would not only be trifling but also borderline skeptical on a global rather than local scale. To see why, consider the following short version of the story about how we came to trust the thermometer as a reliable instrument for measuring temperature. According to Chang (2007):

> Our initial confidence in something like a column of mercury as a trustworthy indication of temperature comes from the correlation between its behavior and our own sensations: seeing it rise when we put it into a place that feels warm to us, seeing it rise rapidly when we put it near a fire, seeing it drop when we wet it and blow on it, etc. But once we adopt the thermometer as a reliable standard, then we begin to use its readings not only to confirm but also to correct our own sensations (p. 8).[92]

So, what we have here is a series of observations: seeing the mercury column rising when we put it into a place that feels warm to us, seeing the mercury column rising rapidly when we put it near a fire, seeing the mercury column drop when we wet it and blow on it, and so on. Are these observations theory-laden? With what sort of theory might these observations be loaded? Is it a theory about human perception? In that case, the thesis that observation is theory-laden turns out to be a thesis of global skepticism. These observations seem to be no more theory-laden than my observations of my computer in front of me, unless we call every hunch, belief, or guess, a “theory.” In that case,

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[91] See Kitcher (1983), chap. 2.

[92] See also Chang (2004).

however, the question of scientific knowledge becomes irrelevant and the thesis that observation is theory-laden becomes trivial. As Hacking (1983) points out:

> In the case of seeing tables, our statements similarly contain no theoretical assumptions connected with the objects under inquiry, namely tables, even if (by an abuse of the words ‘theory’ and ‘contain’) they contain theoretical assumptions about vision (p. 185).

Accordingly, those who want to make sense of the thesis that observations are always loaded with theory might try to do so by abusing the words ‘theory’ and ‘loaded’. But then they would be introducing global skepticism about perception rather than local doubts about scientific knowledge.

To this it might be objected that the example above involves relatively simple observations, which do not require highly complex instruments. More sophisticated observations, such as the ones done with telescopes and microscopes, are surely theory-laden. Taking that into consideration, the thesis that observation is theory-laden might be construed as the claim that instrumentally-aided observation is loaded with theory, specifically theories about the instruments used. According to Hacking, however, this construal of the thesis is unwarranted. He points out that there have been important observations in the history of science that have included no theoretical assumptions at all, e.g., William Herschel’s discovery of radiant heat (Hacking, 1983, pp. 171-176). He also argues that the tendency to infer from stories, such as that of the positron, that those who report, on looking at a photographic plate, “that’s a positron,” are thereby asserting some theoretical assumptions is misguided. According to Hacking (1983), an assistant can be trained to recognize those tracks without having a clue about the theory (p. 179). In the case of microscopes, Hacking (1983) writes:

> One needs theory to make a microscope. You do not need theory to use a microscope. Theory may help to understand why objects perceived with an interference-contrast microscope have asymmetric fringes around them, but you can learn to disregard that effect quite empirically. Hardly any biologists know enough optics to satisfy a physicist. Practice—and I mean in general doing, not looking—creates the ability to distinguish between visible artifacts of the preparation or the instrument, and the real structure that is seen with the microscope. This practical ability breeds conviction. The ability may require some understanding of biology, although one can find first class technicians who don't even know biology. At any rate physics is simply irrelevant to the biologist's sense of microscopic reality. The observations and manipulations seldom bear any load of physical theory at all, and what is there is entirely independent of the cells or crystals being studied (p. 191).

Accordingly, as Wilson (1995) puts it, "the principle that to employ an instrument with confidence one needs a correct theory of the instrument seems to be false" (p. 217).

5.b. The Underdetermination of Theory by Data

If the considerations advanced in section 5.a. are correct, then Kuhn's attack on truth and the associated claim that observation is theory-laden do not provide compelling reasons for skepticism about theoretical knowledge in science. Another skeptical attack on theoretical knowledge is what Richard Boyd (1983) calls "the empiricist argument." Also known as the argument from the underdetermination of theory by data, this is an argument that purports to show that theoretical knowledge in science is unattainable. According to this argument, since empirically equivalent theories are evidentially indistinguishable, it follows that scientific knowledge cannot extend to "unobservables."

Pierre Duhem is perhaps most often associated with the notion of underdetermination. What became known as the Duhem Thesis is the claim that "infinitely many logically incompatible but experimentally indistinguishable theories can always be given of the same range of phenomena" (Worrall, 1982, p. 213). According to

Duhem, this thesis applies not only at the level of theories but also at the level of facts.[93] Duhem's thesis was taken up by Quine. In "Two Dogmas of Empiricism," Quine (1953) argues against what he calls "the second dogma of reductionism," which is the belief that "each meaningful statement is equivalent to some construct upon terms which refer to immediate experience" (p. 20). Quine argues that this dogma persists among empiricists in its subtle form, i.e., that each statement taken in isolation can admit of confirmation or disconfirmation. Contrary to this dogma, Quine (1953) argues that "our statements about the external world face the tribunal of sense experience not individually, but only as a corporate body" (p. 41). In Quine's "empiricism without the dogmas," knowledge is like a field of force in which "a conflict with experience at the periphery occasions adjustments in the interior of the field" (p. 42). On Quine's view, "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system" (p. 43).

Quine's brand of empiricism and Duhem's thesis came to be known as the Duhem-Quine thesis, which includes the following claims: (a) empirical statements cannot be disconfirmed in isolation; (b) we can hold a particular statement true by adjusting the other statements to which it is connected. In philosophy of science, the Duhem-Quine thesis is also known as "confirmation holism." Confirmation holism is the idea that, since the empirical content of a theory cannot be clearly separated from the other components of the theory, when the theory makes a prediction that is not borne out, it will not be obvious which component of the theory should be rejected. So we know that there must be a false statement in the set of statements that implies the false prediction, but we do not know which one.

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[93] See Duhem (1954), Pt. II, chaps. III and IV.

Duhem argued for his thesis against a realist conception of science. Duhem (1969) rejected the realist view that theories are attempts to accurately describe nature, given the possibility of theories that "cannot simultaneously be true but lead to identical conclusions" (p. 102). If Duhem is right, then theoretical knowledge in science might seem impossible to attain. Here is how the argument from underdetermination might run:

(U1) T_1 and T_2 are empirically equivalent iff they make the same predictions about "observable" phenomena.
(U2) It's always possible, given *T*, to construct arbitrarily many alternative theories that are empirically equivalent to *T* but which offer contradictory accounts of the nature of "unobservable" phenomena.
(U3) Since scientific evidence for or against a theory consists in the confirmation or disconfirmation of one of its observational predictions, *T* and each of the theories empirically equivalent to it will be equally well confirmed or disconfirmed by any possible observational evidence.
(U4) Therefore, no scientific evidence can bear on the question of which of these theories provides the correct account of "unobservable" phenomena.
(U5) Since this construction is possible for any theory *T*, it follows that scientific evidence can never decide the question between theories of "unobservable" phenomena, and thus knowledge of "unobservable" phenomena is impossible (Boyd, 1983, pp. 46-48).

More recently, van Fraassen advanced similar considerations against scientific realism and in support of his brand of anti-realism. I will return to van Fraassen shortly. For now, I wish to discuss two problems with underdetermination. If these are genuine problems for the underdetermination argument, then perhaps it does not provide compelling reasons for skepticism about theoretical knowledge in science after all.

The first problem, recently pointed out by Samir Okasha, is that the notion of underdetermination itself is problematic. In particular, there is a tension between underdetermination and confirmation holism. As Okasha (2002) puts it:

> Confirmation holism implies that the inference from 'T_1 and T_2 are empirically equivalent' to 'No possible evidence can decide between T_1 and T_2' is not a good one; since the underdetermination argument depends on precisely that inference, the force of the argument is thereby weakened. Far from holism 'lending support'

> to underdetermination, the two ideas potentially conflict with each other (pp. 307-308).

Okasha (2002) calls this "the holistic objection to the underdetermination argument," which shows that "Once the holistic character of confirmation is acknowledged, the possibility of finding a way to discriminate between two empirically equivalent theories on empirical grounds cannot be ruled out" (p. 307). For this can be done by frame-shifting the holistic point to the level of theories. That is, "two distinct theories, when conjoined with each other, may imply testable statements which neither of them implies alone" (p. 307). Okasha (2002) provides the example of the special theory of relativity (SR) and the "contraction hypothesis" of Lorentz and Fitzgerald (LF), where "we see in a real case how the holistic nature of confirmation invalidates the inference from 'T_1 and T_2 are empirically equivalent' to 'No possible evidence can decide between T_1 and T_2'" (p. 308). For "SR could, but LF could not, be embedded in the general theory of relativity, and the empirical evidence for general relativity thus constituted the empirical grounds for rejecting LF" (p. 308). As Okasha (2002) notes, anti-realists might reply to his holistic objection by "going global," i.e., by recasting the underdetermination argument at the level of "global theories" or "total science." As Okasha (2002) also notes, however, it is not clear that the notion of a "global theory" or "total science" is coherent (p. 314). Even if it is, there is another problem.

The second problem with the underdetermination argument is the theory/data distinction on which it undoubtedly depends. The argument assumes that there are empirically equivalent theories, and hence evidentially indistinguishable by all possible empirical data. Two theories are empirically equivalent just in case they have the same empirically testable or observational consequences. But if the distinction between what is empirically testable (or what is observational) and what is not cannot be drawn coherently, then the notion of empirical equivalence is dubious. If this is so, then the underdetermination argument fails to show that any theory has an evidentially indistinguishable rival. In other words, without a principled distinction between theory and empirical data, the assertion that the former is underdetermined by the latter is problematic.

One way to try to draw a theory/data distinction, it seems, is along the lines of the "observable"/"unobservable" distinction. Indeed, the argument from underdetermination seems to assume a theory/data distinction precisely along these lines. Van Fraassen, perhaps most notably, has argued for a view about science that draws a distinction between what is "observable" and what is not. Constructive empiricism is the view that scientific theories simply "save the phenomena." As far as scientific theories are concerned, the constructive empiricist recommends suspending belief with respect to the existence of the "unobservable" entities, events, or processes postulated by theories. Van Fraassen argues that entities can be classified on the basis of their "observability," i.e., whether or not they are "observable" by us. As van Fraassen (1980) writes:

> The term 'observable' classifies putative entities (entities which may or may not exist). A flying horse is observable—that is why we are so sure that there aren't any—and the number seventeen is not. There is supposed to be a correlate classification of human acts: an unaided act of perception, for instance is an observation. A calculation of the mass of a particle from the deflection of its trajectory in a known force filed, is not an observation of that mass (p. 15).

The "observable"/"unobservable" distinction is crucial for constructive empiricism because, according to van Fraassen (1980), constructive empiricism is the view that "*Science aims to give us theories that are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate*" (p. 12, original emphasis).

For van Fraassen, “to accept a theory is (for us) to believe that it is empirically adequate—that what the theory says *about what is observable* (by us) is true” (p. 18, original emphasis).

Accordingly, the “observable”/“unobservable” distinction is also important for distinguishing constructive empiricism from scientific realism. The scientific realist argues that a belief in the approximate truth of mature science’s best theories is warranted, whereas the constructive empiricist claims that a more modest attitude is appropriate, namely, one of “acceptance” rather than belief. To “accept” a theory, according to van Fraassen, is to believe that the theory’s claims about the “observable” are true and to remain agnostic regarding its claims about the “unobservable.” Clearly, then, the “observable”/“unobservable” distinction is at the core of the constructive empiricist view of science.[94]

According to van Fraassen, the “observable”/“unobservable” distinction is based on the classification of some human acts as acts of observation. He claims that an act of observation must be instrumentally unmediated. That is to say, an act of observation that involves instruments, such as microscopes, might enable us to detect certain entities. However, van Fraassen insists, detection and observation are not the same. For example, when we look through a microscope, according to van Fraassen (2001), we do not see paramecia and the like. Rather, we only see an image of a paramecium. That is, we detect, but do not observe, paramecia (p. 154).

In addition to being instrumentally unmediated, an act of observation must be conceptually unmediated. This seems to be what van Fraassen is suggesting by drawing a distinction between ‘observing’ and ‘observing that’. As van Fraassen (1980) writes:

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[94] Cf. Fine (2001).

> It is also important here not to confuse *observing* (an entity, such as a thing, event, or process) and *observing that* (something or other is the case). Suppose one of the Stone Age people recently found in the Philippines is shown a tennis ball or a car crash. From his behaviour, we see that he has noticed them; for example, he picks up the ball and throws it. But he has not seen *that* it is a tennis ball, or *that* some event is a car crash, for he does not even have those concepts. He cannot get that information through perception; he would first have to learn a great deal (p. 15, original emphasis).

As van Fraassen explains, however, “To say that he does not see the same things and events as we do […] is just silly” (p. 15). So, “To say that *x* observed the tennis ball […] does not imply at all that *x* observed that it was a tennis ball” (p. 15). In other words, an act of observation does not involve the observer believing or knowing what the observed entity is. An observer may observe a tennis ball without believing or knowing that it is a tennis ball.

As Grover Maxwell (1962) points out, ‘is observable’ is a vague predicate. Indeed, van Fraassen seems to have conceded that much to critics.[95] He insists, however, that we should not expect a precise demarcation between “observable” and “unobservable.” The distinction can still be a useful distinction in the philosophy of science, according to van Fraassen (2001), so long as there are clear cases of “observables” and clear cases of “unobservables” (p. 153). Van Fraassen (1980) construes “observability” as follows:

> X is observable if there are circumstances which are such that, if X is present to us under those circumstances, then we observe it (p. 16).

He insists that this “is not meant as a definition, but only as a rough guide to the avoidance of fallacies” (p. 16). It is important to note that “observability” is relativized to “us.” For the constructive empiricist, what counts as “observable” is relative to the epistemic community to which the observer belongs.

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[95] See van Fraassen (1985).

Paul Churchland (1985), among others, has challenged the clarity of the “observable”/“unobservable” distinction. On the constructive empiricist view, it seems that distant macroscopic entities, such as galaxies and stars, are “observable.” Even if we cannot see most of them from earth, we would be able to observe them if we were close enough. According to Churchland, the importance the constructive empiricist attaches to size, as opposed to spatiotemporal proximity, is unprincipled. The distinction between things that are unobserved but “observable,” and things that are “unobservable,” according to Churchland (1985), “is only very feebly principled and is wholly inadequate to bear the great weight that van Fraassen puts on it” (p. 40).

Van Fraassen’s reply to this objection is instructive because it seems to suggest that the distinction is supposed to play a primarily epistemological role in constructive empiricism. Van Fraassen (1985) says that “scientific realists tend to feel baffled by the idea that our opinion about the limits of perception should play a role in arriving at our epistemic attitudes toward science” (p. 258). For the constructive empiricist, then, the distinction is pertinent to our epistemic attitudes. Van Fraassen (1985) claims that “experience is the sole legitimate source of information about the world” (p. 258). And in a later work van Fraassen (2007) claims that “there is no purely epistemic warrant for going beyond our evidence” (p. 343). Accordingly, it is quite appropriate that what we can experience shapes our epistemic attitudes. The constructive empiricist recommends epistemic modesty: we should resist going well beyond the deliverances of experience and believe that what scientific theories say about “unobservable” entities is true. Rather, we should “accept” scientific theories only as “empirically adequate.” To say that a theory is “empirically adequate” is to say that what it asserts about “observables” is correct, but to remain uncommitted about the “unobservables” of the theory.

With this understanding of the “observable”/“unobservable” distinction in hand, I now turn to examine two cases from science. I think that they are problematic cases for the constructive empiricist, especially in light of van Fraassen’s assertion that what counts as “observable” cannot be determined a priori, but rather is the subject of scientific inquiry (1980, pp. 56-58). If these cases are indeed problematic for constructive empiricism insofar as they show that a clear “observable”/“unobservable” distinction cannot be drawn, then a clear theory/data distinction cannot be drawn along these lines either. If that’s the case, then the “empiricist argument” fails.

According to Kitcher, on the constructive empiricist view, it is not clear whether a dinosaur is “observable” or not. As Kitcher (1993) writes:

> Nobody has ever had unaided observation of a dinosaur or of the members of countless other species which inhabited our planet before we evolved. Are these organisms unobservable or simply unobserved? Perhaps we can think of ourselves, in the style of van Fraassen’s though experiment, as transported back in time (as we might be transported in space to Jupiter for the close-up view of the moons) and provided with an opportunity to confront Triceratops. But is time travel *physically* possible? If it is not, then it appears that van Fraassen’s test will not allow this type of explanation of the observability of dinosaurs (p. 152).

Although I think Kitcher is right, I will grant the constructive empiricist that time travel is possible *in principle*. That is to say, the constructive empiricist may insist that, even though we may not be able to go back in time and observe a dinosaur *in practice*, we could do so *in principle*, and that is why dinosaurs count as “observable” entities. In other words, a dinosaur is “observable” because if I were standing in front of one in broad daylight, then I would see it. However, in the case I would like to examine next, namely, that of Mitochondrial Eve, it is not clear that the entity in question counts as "observable" in a straightforward way.

Mitochondrial Eve is a theoretical entity.[96] She is the hypothetical female that is supposed to be the most recent common ancestor of all humans alive today in terms of matrilineal descent.[97] Based on the analysis of mitochondrial DNA (mtDNA) from diverse peoples all over the world, researchers have suggested that this female lived around 200,000 years ago. They also suggested that she most likely lived in Africa and that is why she is also known as "African Eve." According to Cann, et al (1987):

> We *infer* from the tree of minimum length [a genealogical tree for 134 types of human mtDNA] that Africa is a *likely* source of the human mitochondrial gene pool. This *inference* comes from the observation that one of the two primary branches leads exclusively to African mtDNAs […] while the second primary branch also leads to African mtDNAs […]. By *postulating* that the common ancestral mtDNA […] was African, we *minimize the number of intercontinental migrations needed to account for* the geographic distribution of mtDNA types. *It follows that* b is a *likely* common ancestor of all non-African and many African mtDNAs (p. 33, my emphasis).

The Mitochondrial Eve hypothesis has generated a debate and some of the details are still being sorted out.[98] For present purposes, however, it is not important whether the hypothesis is well-confirmed or not. What is important, for present purposes, is whether or not Mitochondrial Eve, if she existed, counts as "observable" or not.

The case of Mitochondrial Eve seems to be problematic for constructive empiricism. According to Musgrave, any statement of the form '*X* is not observable by humans' cannot be a statement about something "observable" by humans, and so the constructive empiricist cannot believe that '*X* is unobservable by humans'. Musgrave

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[96] I owe this example to Alberto Cordero.

[97] The Mitochondrial Eve hypothesis is often misunderstood and (erroneously) taken to have religious or theological significance. See Sykes (2001).

[98] See, e.g., Loewe & Scherer (1997).

(1985) argues that “the consistent constructive empiricist cannot draw a workable observable/unobservable distinction” (p. 208). Commenting on Musgrave’s criticism, Andre Kukla (1998) writes:

> Musgrave is right when he claims that Van Fraassen can’t allow himself to believe that electrons are unobservable. But there’s no reason why he shouldn’t believe that electrons are unobservable *if they exist*. What anti-realists refuse to believe is any statement that entails that theoretical entities exist. But the claim that theoretical entities are unobservable-if-they-exist doesn’t violate this prescription (p. 139).

Now, since Mitochondrial Eve is a theoretical posit, whose existence is postulated on superempirical grounds, such as simplicity, to account for mtDNA data, it seems that the constructive empiricist should remain agnostic about her existence.[99] If she existed, however, it seems that she should be “observable.” That is, if there were such a human female, then she would be “observable.” Just as dinosaurs are “observable,” so should Mitochondrial Eve be “observable.” We could—if not in practice, then in principle—go back in time and observe the individual female that is Mitochondrial Eve. So, on the one hand, if the constructive empiricist were to insist that Mitochondrial Eve is “unobservable,” since she is a theoretical entity, then that would seem rather counterintuitive. On the other hand, if the constructive empiricist were to admit that Mitochondrial Eve is indeed “observable,” he would be thereby admitting that a theoretical posit could be “observable.”[100]

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[99] Criticizing a premise in Nicholas Maxwell’s Master Argument against Constructive Empiricism, Muller (2008) says that this premise, which is “going from observable behavior to unobservable mental states, smacks too much of an Inference-to-the-Best-Explanation (IBE), which is a mode of inference that Van Fraassen is very critical about, in particular when it concerns an *explanandum* about observables only and an *explanans* which is also about unobservable” (p. 143). See also van Fraassen (1989), pp. 131-150.

[100] Cf. Muller (2004). According to Muller, “In all interesting cases of unobservables that occur in accepted scientific theories, the objects are unambiguously unobservable (electrons, forces, gluon-fields, black holes, tau-neutrino’s, superstrings, and so forth), which makes the case of ambiguous unobservables largely ‘academic’ (in its pejorative sense)” (p. 85, note 5). If the aforementioned considerations are correct, then

To this the constructive empiricist might reply by recalling the distinction between 'observing' and 'observing that'. Perhaps we could *observe the individual*, but we could not *observe that* she is Mitochondrial Eve. In other words, even if we managed to come across the human female that is Mitochondrial Eve, we could not observe that she is Mitochondrial Eve, i.e., that she has the property of being the ancestor of all humans alive today on the female line. Such properties, like being Mitochondrial Eve, are not "observable," for the most part. This might be a satisfactory response, on behalf of the constructive empiricist, although it seems to suggest that prima facie "observable" properties, such as having an elliptical orbit and many others, turn out to be "unobservable" as well. For, in principle, we could be in a spatiotemporal position, just as we could in principle go back in time, such that we could observe the elliptical orbit of a particular planet (though, admittedly, over a long period of time). I suppose that is partly why Churchland protested against the seemingly unprincipled importance attached to size, as opposed to spatiotemporal proximity, by the constructive empiricist. Be that as it may, I will now turn to the second case, which, I think, is more problematic as far as the "observable"/"unobservable" distinction is concerned.

In 2008, Osamu Shimomura, Martin Chalfie, and Roger Tsien were awarded the Nobel Prize in Chemistry "for their discovery and development of the green fluorescent protein, GFP."[101] According to the Royal Swedish Academy of Sciences (2008), "When scientists develop methods to help them see things that were once invisible, research

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Mitochondrial Eve is perhaps an ambiguous unobservable. It seems to me that we cannot simply dismiss this case as "largely academic."

[101] Available at <http://nobelprize.org/nobel_prizes/chemistry/laureates/2008/index.html>.

always takes a great leap forward."[102] It seems that GFP, or rather the array of tools developed as a result of the discovery of the GFP, is one such method.

GFP is found in a species of jellyfish known as *Aequorea victoria*. In *Aequorea victoria*, a protein called *aequorin* releases blue light when binding with calcium. GFP absorbs this blue light and gives off green light. The green color of GFP appears under blue and ultraviolet light. According to the Royal Swedish Academy of Sciences (2008):

> the discovery of GFP relates to a miraculous property of the chromophore that is responsible for its fluorescence. This chromophore is formed spontaneously from a tri-peptide motif in the primary structure of GFP, so that its fluorescence is "automatically" turned on in every organism where it is expressed. In other words, the maturation of the tri-peptide-based chromophore in GFP only requires oxygen and does not depend on the presence of enzymes or other auxiliary factors. *GFP and its related variants thereby provide universal genetic tags that can be used to visualize a virtually unlimited number of spatio-temporal processes in virtually all living systems* (p. 1, my emphasis).[103]

The discovery of GFP has enabled researchers to develop a set of tools that can be used with laboratory animals, such as flatworms, algae, *E. coli*, and pigs, in order to illuminate growing cancer tumors, track the development of Alzheimer's disease, and show the growth of pathogenic bacteria. According to the Royal Swedish Academy of Sciences (2008):

> These GFP-like proteins allow the monitoring in time and space of an ever-increasing number of phenomena in living cells and organisms like gene expression, protein localization and dynamics, protein-protein interactions, cell division, chromosome replication and organization, intracellular transport pathways, organelle inheritance and biogenesis, to name but a few. In addition, the fluorescence from single GFP molecules has made it feasible to image at a spatial resolution higher than the diffraction limit (p. 1).[104]

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[102] Available at <http://nobelprize.org/nobel_prizes/chemistry/laureates/2008/info.pdf >.

[103] Available at <http://nobelprize.org/nobel_prizes/chemistry/laureates/2008/chemadv08.pdf>.

[104] Ibid. In general, the best images one can obtain using light can make out features no smaller than about half the light's wavelength, approximately 200 nanometers using the shortest-wavelength visible light.

By now, GFP and GFP-like proteins constitute a “tool box” of genetic tags that is used extensively in laboratories for studying dynamic processes in all living systems.[105]

Consider two studies in which these GFP and GFP-like proteins are used. In one study, a team of Harvard researchers genetically modified the nerve cells of mice to produce varying amounts of three GFP-like proteins that fluoresce yellow, orange, and red. The result was mice that glowed in the colors of the rainbow, and thus the project was called “the brainbow.” The brainbow enables researchers to follow nerve fibers from individual cells in the dense network in the brain.[106] In another study, researchers used GFP and GFP-like proteins to genetically modify arsenic-resistant bacteria, so that it will glow green in the presence of arsenic.[107] The same method has been used by other researchers for detecting the presence of explosive trinitrotoluene (TNT) and heavy metals, such as cadmium or zinc.

Now, the constructive empiricist may not be impressed with GFP and GFP-like proteins as methods for tagging and marking microscopic entities and intracellular processes because, after all, microscopes must still be used to observe these entities and processes by means of these genetic tags. Recall that, for the constructive empiricist, the act of looking through a microscope is not an act of observation. That is, when we look through a microscope, we do not directly observe a paramecium; we merely observe an *image* of a paramecium. By looking through a microscope, we *detect*, but do not observe, a paramecium (van Fraassen, 2001, pp. 158-160).

As Alspector-Kelly (2004) points out, “The claim that we can only see what the human senses can detect without aid or supplementation of some sort is not a conceptual

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[105] See, e.g., Zimmer (2005).
[106] See Livet, et al (2007).
[107] See Hsiu-Chuan, et al (2005).

truth; or, if it is, it is far from obvious that it enjoys that status" (p. 332). Alspector-Kelly argues that van Fraassen needs an argument for this epistemic privilege that he grants to immediate experience. I think that Alspector-Kelly is right about this. But I will grant the constructive empiricist that "observables" are those entities that we can see with instruments and without instruments. However, recall that in order to get out of the problem posed by the Mitochondrial Eve case, it seems that the constructive empiricist needs to invoke the distinction between 'observing' and 'observing that'. That is, we can observe the individual human female that is Mitochondrial Eve, but we cannot *observe that* she is Mitochondrial Eve. Taking this line, however, seems to get the constructive empiricist into trouble as far as the GFP case is concerned. For, in this case, we may not be able to observe the entities under the microscope in a way that would satisfy the constructive empiricist, but we do *observe that* they fluoresce green (or red, yellow, and other colors besides). Furthermore, we could vindicate that in a way that is not instrumentally mediated, because GFP and GFP-like proteins glow in their respective colors under blue light. Researchers have already done that with nude transgenic mice. Under blue light, every cell in the body of these nude transgenic mice that contains actin fluoresce green. The same technique was used with other organisms, such as salamanders, fruit flies (and the sperm of male fruit flies), mosquitoes and mosquito larvae, *C. elegans* worms, as well as brain tumors and cancer cells.[108] Now, it seems that, *in principle*, we could do the same with van Fraassen's notorious "unobservable"—the paramecium. If the constructive empiricist were to insist that we can't, because paramecia are just too small, then he would be attributing epistemological significance to size in an unprincipled way.

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[108] See Zimmer (2009), the GFP website <http://gfp.conncoll.edu >.

Perhaps van Fraassen’s “new epistemology” might shed some light on the constructive empiricist’s agnosticism with respect to “unobservables.” According to van Fraassen (1989):

> Any act of decision can be evaluated in two ways. If we evaluate it beforehand, we ask how *reasonable* it is, and afterward, we ask to what extent it was *vindicated*. The two cannot be the same since the agent cannot have knowledge beforehand of the exact outcome and consequences of his action—vindication or the lack thereof lies as yet beyond his ken. But there must be a connection, since the point of deciding or acting lies in the outcome (broadly construed). Therefore a minimal criterion of reasonableness is that *you should not sabotage your possibilities of vindication beforehand* (p. 157, original emphasis).

Accordingly, the constructive empiricist might argue that it is unreasonable to believe in “unobservables” because we are thereby “sabotaging our possibilities of vindication beforehand.” But why think that we are “sabotaging our possibilities of vindication beforehand”? After all, the constructive empiricist insists that vindication, or lack thereof, lies beyond our ken. If anything, the GFP case seems to show that we are not sabotaging our possibilities of vindication. Our beliefs in the existence of “unobservables” might be vindicated in yet unforeseen ways. It seems to me that suspending belief in unobservables on the assumption that vindication is (likely) not forthcoming is just as epistemically immodest, by empiricist lights, as believing in unobservables and insisting that vindication is at hand. In other words, it seems that the constructive empiricist recommends agnosticism about “unobservables” because he assumes that vindication is unlikely. But this assumption, as the GFP case seems to show, is unwarranted.

Unlike the case of Mitochondrial Eve, where the problem is how the constructive empiricist would characterize the entity in question (i.e., as “observable” or “unobservable”), the GFP case presents a different problem for constructive empiricism. The problem seems to be that the constructive empiricist can’t have it both ways, i.e., he can’t label certain entities as “unobservable,” while, at the same time, look to science to tell us which entities are “observable” and which aren’t. More explicitly, the constructive empiricist claims that we should turn to science to find out which entities are “observable” and which are “unobservable.” According to van Fraassen (1980):

> If there are limits to observation, these are a subject for empirical science, and not for philosophical analysis. Nor can the limits be described once and for all, just as measurement cannot be described once and for all (p. 57).

As the GFP case seems to suggest, however, an entity that we may have considered “unobservable,” may turn out to be “observable” after all. So the GFP is a case where science can yield a verdict of “observable” regarding entities that the constructive empiricist characterizes as “unobservable,” such as paramecium. It seems that the constructive empiricist should welcome such verdicts from science. The problem is, however, that by labeling entities as “unobservable,” the constructive empiricist seems to be ruling out (perhaps a priori) science’s role in determining what is “observable.” It seems that the constructive empiricist should leave open the possibility that advances in science might enable us to observe that which was unobserved thus far. If this is correct, however, then there are no grounds for labeling entities as “unobservable.” The constructive empiricist should heed his own call for epistemic modesty and resist the temptation of going well beyond the deliverances of experience. The fact that we haven’t observed an entity thus far is not a compelling reason for thinking that we will never be able to observe it by any means, or to vindicate our belief in that entity by any means, and thus it is not a sufficient reason for labeling that entity “unobservable.”

As F. A. Muller (2004) points out, critics of the “observable”/“unobservable” distinction usually object to drawing the distinction within constructive empiricism (p.

81). That is to say, critics argue that the distinction clashes with other principles of constructive empiricism.[109] If the aforementioned considerations are correct, then the lessons of the GFP case can be put in a similar way. More explicitly, the GFP case suggests that the distinction clashes with the constructive empiricist's recommendation for epistemic modesty, especially given his commitment to turning to science for answers as to what is "observable." According to van Fraassen (1980):

> science itself delineates, at least to some extent, the observable parts of the world it describes. Measurement interactions are a special subclass of physical interactions in general. The structures definable from measurement data are a subclass of the physical structures described. It is in this way that science itself distinguishes the observable which it postulates from the whole it postulates. The distinction, being in part a function of the limits science discloses on human observation, is an anthropocentric one. But since science places human observers among the physical systems it means to describe, it also gives itself the task of describing anthropocentric distinctions (p. 59).

Based on this passage, it seems that it should be an empirical matter for the constructive empiricist whether a certain entity is "observable" or not. If this is correct, then it seems that the distinction clashes with this methodological principle of constructive empiricism. To see why, consider what van Fraassen (1980) says about flying horses: "A flying horse is observable—this is why we are so sure that there aren't any" (p. 15). More recently, van Fraassen and Muller (2008) emphasized this point again:

> Before we know whether Pegasus exists or not, we classify it as observable; it is in part because flying horses *are observable* that we are so sure there aren't any (p. 202, original emphasis).[110]

On what grounds do we classify Pegasus as "observable"?

It seems that the answer to this question is not clear. For, on the one hand, the constructive empiricist insists that science tells us what is "observable" and what is not.

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[109] See, e.g., Friedman (1982) and Musgrave (1985).

[110] Cf. Dicken & Lipton (2006).

For the constructive empiricist, “To delineate what is observable […] we must look to science […] for that is also an empirical question” (van Fraassen, 1980, p. 57). On the other hand, observation, in the constructive empiricist sense, is supposed to be instrumentally and conceptually unmediated. And the question “what is observable,” is “a theory-independent question” (van Fraassen, 1980, p. 57). It is not clear, however, how science can give an answer to this theory-independent question, especially the answer that the constructive empiricist wants, namely, that some things are “unobservable” or “observable” before we even know whether or not they exist.

To see why, consider the flying horse again. The constructive empiricist seems to suggest that we classify Pegasus as “observable” before we know whether or not it exists. Is this classification grounded in empirical facts? How can we be “so sure” that Pegasus doesn’t exist even if it is “observable”? Perhaps the constructive empiricist argues as follows:

(FH1) If there were any flying horses, we would have observed them (at least one of them, since flying horses are “observable”).
(FH2) We haven’t observed any flying horses.
(FH3) Therefore, there are no flying horses.

But if this is why “we are so sure there aren’t any” flying horses, then aren’t we doing exactly what the constructive empiricist recommends we should not do, i.e., go well beyond the deliverances of experience?[111]

Perhaps an analogous case would be helpful here. It seems that the constructive empiricist should grant a distinction analogous to the “observable”/“unobservable”

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[111] I admit that this reconstruction seems rather odd. Perhaps one would prefer to construe the reasoning in question as an inference to the best explanation (IBE): The fact that we haven’t observed any flying horses for so long is best explained by supposing that (probably) there aren’t any. As an anti-realist, van Fraassen clearly rejects explanation as a guide to theoretical truth. But does he accept explanation as a guide to empirical adequacy? In *The Scientific Image* (1980) it seems that he does. In his later works, however, it seems that he wants to reject IBE altogether.

distinction, namely, the audible/inaudible distinction. After all, it seems reasonable to suggest that the sense of sight is not the only source of information humans have about the world; there are other sensory modalities, such as hearing, smell, taste, and touch.[112] Presumably, the constructive empiricist would not want to privilege vision as the only source of information about the world. If this is so, then how can we classify a certain sound as audible or inaudible, in the same way that the constructive empiricist classifies flying horses as "observable"? The problem becomes even more acute when it is noted that the constructive empiricist demands that proper acts of observation be instrumentally and conceptually unmediated. So, in the case of sound, this seems to mean, at the very least, that we haven't detected the sound in question yet. How can we tell whether a sound we haven't detected yet is audible or inaudible? How can we tell whether it is an ultrasound (sound above the 20 kHz limit of our hearing) or infrasound (sound at frequencies lower than what we can hear, approximately below 20 Hz)?

The constructive empiricist would urge us to keep the "observable"/"unobservable" and the existent/non-existent distinction separate: "the 'observable/unobservable' classification is quite independent, logically, of the 'existent/non-existent' distinction" (Muller & van Fraassen, 2008, p. 202). Even if we do not infer "non-existent" from "unobservable," however, the problem remains how to classify entities as either "observable" or "unobservable" before we know that they exist, unless we somehow do it a priori. It seems that we cannot turn to science for an answer, which is problematic for the constructive empiricist, given his insistence that science delineates what is "observable." We can see why this is so by considering the analogous

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[112] Not to mention the vestibular system, kinaesthesis, and proprioception, in the human case, and electroreceptors, magnetic sense, and infra-red vision in the case of other species.

audible/inaudible distinction again. Indeed, if we turn to science, we are told that some animals can hear the high frequencies we cannot. For example, dogs can hear up to about 40 kHz, which is why dog whistles work, bats can hear up to about 100 kHz, some fish can hear at about 180 kHz. And some animals hear infrasonic frequencies. For example, elephants can hear infrasonic frequencies at levels inaudible to humans (approximately 15 Hz).[113] The point, then, is that we can't classify things as "observable" or "unobservable" in the way that the constructive empiricist seems to want, i.e., by scientific inquiry, unless we go beyond the deliverances of experience, which is not what the constructive empiricist recommends.[114]

If this is correct, then there seems to be a tension between the following tenets of constructive empiricism:

(CE1) The "observable"/"unobservable" distinction: distinguishing between what is "observable" by us (even in principle, though not in practice) and what is not.

(CE2) Epistemic modesty: resisting going beyond the deliverances of experience. (In the case of scientific theories, this means taking theories as "empirically adequate," i.e., believing what the theories say about "observables," while remaining agnostic about the "unobservables").

(CE3) Methodological principle: turning to science for answers to questions about what is "observable" and what is not.

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[113] How do we know that these sounds are present? In the case of infrasound, for instance, they affect us even though we can't hear them. For example, air conditioners, boilers, airplanes, and cars produce infrasound that may not lead to hearing loss, but is known to cause dizziness, nausea, headache, etc. In the case of cars, traveling at high speeds, this explains why some people get carsick.

[114] It seems reasonable to suggest that hearing simply is detecting sounds by means of a particular sensory system just as smelling simply is detecting odors by means of a particular sensory system. Why doesn't the same point apply to the case of vision? Seeing is simply detecting… Perhaps that is the problem. What exactly are we detecting when we use our sense of sight? The standard account is that light is refracted by the cornea in the eye through the pupil in the iris and onto the lens. The lens focuses images onto the retina. These images are received by light-sensitive cells called photoreceptors (cones and rods). These photoreceptors convert light stimulation into electrical nerve impulses which then stimulate the optic nerve and are transmitted via that nerve to the brain. For van Fraassen (2001), however, light itself is "unobservable." Cf. Alspector-Kelly (2004). Whatever it is that we are detecting when we see, it seems unreasonable to ask for a way of detecting that thing without using the sense of sight itself, for that would be analogous to asking us to detect sounds without using our hearing system or detecting odors without using our olfactory system.

The tension is suggested by the two cases discussed above. The case of Mitochondrial Eve is problematic for the constructive empiricist because Mitochondrial Eve seems to be a theoretical posit that should be “observable.” The constructive empiricist may insist that, although entities, such as human beings, may be “observable,” properties, such as being the most recent common ancestor of all humans alive today on the matrilineal line of descent, are usually not. We can usually observe other humans, and so, in principle, we could observe Mitochondrial Eve. But we could not observe *that* she is our most recent common ancestor.

This insistence, however, seems to be at odds with (CE3). For the constructive empiricist, science aims to give us empirically adequate theories, and acceptance of a theory involves the belief that it is empirically adequate. A theory is empirically adequate if what it says about “observables” is true. If the constructive empiricist is right, however, then “unobservables” are beyond our ken and science is not in the business of finding out about them. If that is the case, how can we expect science to tell us what is “observable” and what is not? To put it another way, the property of being “unobservable” is itself “unobservable,” and thus beyond the scope of science by the constructive empiricist’s lights, since he takes the aim of science to be empirical adequacy (i.e., what is true about the “observable”).

Contrary to (CE2), it seems that the constructive empiricist is going beyond the deliverances of experience when it comes to the “observable”/“unobservable” distinction. The case of the GFP seems to show that it is possible that some entities that are considered “unobservable,” may be observed, and thus turn out to be “observable” after all. This is a verdict that science can issue. It seems that the constructive empiricist wants science to be able to issue such verdicts. But (CE3) seems to be at odds with classifying entities as “observable” and “unobservable,” i.e., with (CE1). As the GFP case seems to illustrate, it seems that we are justified in talking only about “observed” and “unobserved” entities rather than “observable” and “unobservable” entities. At the very least, it seems that we should remain open to the possibility that the empire of the senses may be enlarged and strengthened, as Robert Hooke had hoped.

If this is correct, then why would van Fraassen insist on drawing a hopelessly muddled distinction? I think the answer is that without a firm “observable”/“unobservable” distinction to rely on, constructive empiricism seems to lose its claim to being a philosophy of actual science. For van Fraassen (2001) insists that “Constructive Empiricism, the view introduced in *The Scientific Image*, is a view of science, and answer to the question ‘what is science?’” (p. 151). So, constructive empiricism is supposed to be a doctrine about what the aim of science actually is. But to answer the charge of being epistemically immodest, by empiricist lights, the constructive empiricist urges that the doctrine that the aim of science is truth about what has been observed would fly in the face of scientific practice. As Monton and van Fraassen (2003) admit:

> there would be no scientific reason for someone to do an experiment which would generate a phenomenon that has never been observed before. But one of the hallmarks of good scientists is that they perform experiments pushing the limits of what has been observed so far (p. 407).

So the constructive empiricist needs the distinction in order to account for actual science, in particular, for the fact that scientists are attempting to gain knowledge about the unobserved. To do so, the constructive empiricist claims, scientists must believe that they are dealing with “observable” entities. But if the aforementioned considerations are correct, this cannot be done in the way that the constructive empiricist requires. In other words, if talk about “observable” and “unobservable” entities in the constructive empiricist’s sense is unwarranted, as I have argued, and if the doctrine that the aim of science is truth about what has been observed “fails to capture our idea of what it is to do good science” (Monton & van Fraassen, 2003, p. 407), then the constructive empiricist has nothing to resort to in order to capture that idea of “good science.”

The epistemic account of scientific progress, it seems to me, can easily avoid these problems, for it takes science to be an epistemic enterprise. As William James (1897) pointed out, knowers are guided by two distinct imperatives: “believe truth” and “shun error” (p. 18). Emphasizing one more than the other can lead to different strategies. On the one hand, if we take “believe truth” as more important, we might believe as many things as we can on the assumption that we would thereby maximize truth by increasing our chances of believing some truths. On the other hand, if we take “shun error” to be more important, we might adopt a strategy similar to what the constructive empiricist is proposing in order to minimize the risk of error. But then why stop at the “observable” level? Why not go all the way?[115] It seems to me that scientific practice in general, and the episodes discussed in Chapter 2 in particular, show that scientists take both epistemic imperatives seriously. They not only seek truth but also shun error. In that respect, scientists may be more accurately characterized as knowledge-seekers rather than simply truth-seekers. For the concept of knowledge includes the concepts of truth as well as justification. That is why the concept we need in order to understand scientific progress is (scientific) knowledge.

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[115] See also Neta’s example of the light switches in the Appendix “The Analysis of Knowledge.”

If this is correct, then a theory/data distinction cannot be drawn along the lines of the “observable”/“unobservable” distinction. Without a theory/data distinction, however, the “empiricist argument” cannot run. Another way to try to draw a theory/data distinction is to make it relative to a particular context of inquiry. This seems to have been the strategy of philosophers of science who argue that the underdetermination argument does not depend on a dubious theory/data distinction. For example, Paul Horwich (1991) argues that “the objection [that all observation is theory-laden] does not […] undermine the idea that new theories are not simply observed to be true, but have to gain their credibility by means of inference” (p. 59).[116] Likewise, Yemima Ben-Menachem (1990) argues that “it is a plausible assumption about any investigation that some sentences are treated as data while others are taken to be theoretical” (p. 277). What these philosophers seem to have in mind is what Okasha (2002) calls “the platitude”:

> It is an obvious fact, a platitude, that scientists invent hypotheses whose truth-values cannot be directly assessed, but can only be determined, if at all, by first determining the truth-values of other statements which they imply. The former are theoretical, the latter observational. Thus the existence of a theory/observation distinction is uncontroversial (p. 316).

This platitude gives us a theory/data distinction that is relative to a particular context of inquiry. That is to say, statements do not qualify as theoretical or empirical absolutely, but rather in relation to a given context of inference. The same statement can count as theoretical in some contexts and as observational in other contexts. Is this non-absolute, context-variant theory/data distinction enough to run the underdetermination argument?

It might appear as if this theory/data distinction—although it does not give us an absolute, context-invariant distinction—is enough to run the underdetermination argument. As Okasha points out, however, on closer inspection, it turns out to be

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[116] Cf. Laudan & Leplin (1991).

insufficient for the skeptical conclusion it purports to support. For, as we have seen, in order to respond to Okasha's holistic objection, the anti-realist has to "go global," i.e., to reformulate the underdetermination argument to apply to "global theories" or "total sciences."[117] But this move, Okasha argues, presumes that an absolute theory/data distinction can be drawn. As Okasha (2002) puts it:

> The concept of a global theory entered the picture in reply to the holistic worry that incorporation into a larger theory could decide between two empirically equivalent theories; anti-realists tried to defuse this worry by construing their argument in relation to the 'global theory of the world'. This global theory is supposed to be maximally inclusive, to save all the phenomena that there are; anti-realists suggest that the totality of these phenomena underdetermine the global theory. But *if this suggestion is to make sense, it must be possible to say of any true statement whether it belongs on the 'theory' side or whether it describes one of the 'phenomena' which have to be saved* (p. 317, my emphasis).

Hence, according to Okasha (2002), a theory/data distinction that is context-relative will not be sufficient to run the underdetermination argument at the global level, for this distinction "permits one and the same statement to count as theoretical at some times, as observational at others" (p. 317). This context-relative distinction, then, is not enough to make sense of the claim that the global theory of the world is underdetermined by the totality of empirical evidence; only an absolute theory/data distinction can do that.

For present purposes, then, we can accept "the platitude," and the context-relative theory/data that it yields. It seems to me that the epistemic account of scientific progress doesn't need more than that, i.e., it doesn't require an absolute theory/data distinction. That means that claims to scientific knowledge are distinguished as theoretical or empirical, not absolutely (e.g., by virtue of referring to "unobservable" entities, processes, or events), but relative to the context in which they appear. The same claim to knowledge can thus be theoretical in one context and empirical in another context. In that

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[117] See also Boyd (1983), pp. 55-56.

case, theoretical knowledge (TK) is inferential knowledge and empirical knowledge (EK) is factual or descriptive knowledge. On the epistemic account, then, the statement "[Mitochondrial Eve] is a likely common ancestor of all non-African and many African mtDNAs" counts as theoretical, not because Mitochondrial Eve is "unobservable," but because this statement is inferred from other statements about mtDNA that are taken as "data."

If this is correct, then the underdetermination argument doesn't provide compelling reasons for skepticism about theoretical knowledge in science. This is not to say that there might not be cases of local, effective underdetermination, with respect to which we might have to suspend belief for the time being, e.g., as in the case of the various interpretations of quantum physics.[118] However, as Alberto Cordero (2001) argues, "nothing of global skeptical or agnostic significance follows from the kind of underdetermination presently encountered in fundamental quantum theory" (p. S301).

5.c. The Pessimistic Meta-Induction

If the considerations advanced in section 5.b. are correct, then the "empiricist argument" (the underdetermination of theory by data) doesn't provide compelling reasons for skepticism about theoretical knowledge in science. Another challenge to theoretical knowledge is the skeptical reading of the history of science. According to Kuhn (1992), "All past beliefs about nature have sooner or later turned out to be false" (p. 14). Laudan expands on Kuhn's claim by providing a list of theories that were once successful but later rejected. As Laudan (1981b) writes:

> What the history of science offers us is a plethora of theories which were both successful and (so far as we can judge) non-referential with respect to many of

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[118] See Psillos (2000).

> their central explanatory concepts. […] Let me add a few more prominent examples to the list:
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> -the crystalline spheres of ancient and medieval astronomy;
> -the humoral theory of medicine;
> -the effluvial theory of static electricity;
> -'catastrophic' geology, with its commitment to a universal (Noachian) deluge;
> -the phlogiston theory of chemistry;
> -the caloric theory of heat;
> -the vibratory theory of heat;
> -the vital force theories of physiology;
> -the electromagnetic aether;
> -the optical aether;
> -the theory of circular inertia;
> -theories of spontaneous generation.
>
> This list, which could be extended ad nauseam, involves in every case a theory which was once successful and well confirmed, but which contained central terms which (we now believe) were non-referring (p. 33).

This argument has generated an enormous literature and came to be known as the Pessimistic Meta-Induction (PMI). There are also several objections to it.[119] However, I would like to focus on an aspect of this argument that, as Kitcher points out, we should resist.[120] According to Kitcher, an important part of the argument is an implicit holism. As Kitcher (2002) writes:

> We are invited to think of whole theories as the proper objects of knowledge, and thus, because the theory, taken as a whole, turns out to be false, we have the basis for a "pessimistic induction." *It doesn't follow from the fact that a past theory isn't completely true that every part of that theory is false* (p. 388, my emphasis).

On the epistemic account of scientific progress, we should resist this kind of holism because the proper objects of knowledge are statements (or propositions) rather than whole theories.[121]

By way of illustration, consider the following example, discussed by Leplin (1997, p. 133). Suppose that there is a power outage in my house. Upon looking outside

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[119] See, e.g., Lewis (2001); Lange (2002); Saatsi (2005).
[120] See also Psillos (1996).
[121] See also Kitcher (1993), p. 118.

my window, I see a utility truck parked nearby and some workers digging in the yard. Since I made a call to the phone company earlier about a certain problem with my phone line, I infer that telephone repairmen, who have responded to my earlier call, inadvertently cut the power line to my house. Unbeknownst to me, however, it is not telephone repairmen who have cut the power line but cable repairmen whom I had not expected. Now, if we take this “theory,” i.e., that there is a power outage in my house because telephone repairmen have inadvertently cut the power line to my house, as a monolithic whole, then it is strictly false. However, this theory involves several claims, some are true and some are false. On the one hand, it is not the case that telephone repairmen working in the backyard have inadvertently cut the power line. On the other hand, it is the case that repairmen working in the backyard have inadvertently cut the power line. I may not know the truth, the whole truth, and nothing but the truth about this state of affairs. But I do know some parts about it, and those parts are themselves true.

We can see how this works in the episodes from the history of bacteriology and immunology discussed in Chapter 2. In his *An Inquiry into the Causes and Effects of the Variolae Vaccinae* (1798), Edward Jenner (1749-1823) argues that cowpox originated as grease, a disease common in horses. He claims that it was transmitted to cows when horse handlers helped with milking on occasion. In addition, Jenner (1800) claims not only that cowpox protected against smallpox but also that “what renders the Cow Pox virus so extremely singular, is, that the person who has been thus affected is for ever after secure from the infection of the Small Pox” (p. 7).

Now, if we take the entire *Inquiry* as Jenner’s “theory,” then it is strictly false as a whole. He was wrong about grease being the origin of cowpox. He mistakenly took horsepox for grease, and there was no intermediate passage through cows either. Even though he got some things wrong, he was right about others. His hypothesis, properly construed, is correct. While it is not the case that vaccination provides lifelong protection, as Jenner thought, it is the case that repeated vaccination, properly done, contributes to the control of smallpox. Indeed, Jenner paved the way for this knowledge, and the know-how for selection of correct material for vaccination, with his distinction between true and spurious cowpox. Nowadays, pseudocowpox (milker's nodes) is recognized as a type of spurious cowpox.[122] According to the World Health Organization, "Publication of the *Inquiry* and the subsequent promulgation by Jenner of the idea of vaccination with a virus other than variola virus constituted a watershed in the control of smallpox, for which he more than anyone else deserves the credit" (Fenner, et al, 1988, chap. 6, p. 264).

Another example is Ehrlich's side-chain theory of antibody formation. As we have seen in Chapter 2, Ehrlich proposed that harmful compounds can mimic nutrients for which cells express specific receptors. However, he considered these receptors to be on all cell types. He also did not realize that there are specialized producer cells, such as B lymphocytes. He thought of the entire spectrum of receptors as a single cell because he considered their main task as the uptake of different nutrients. These are parts of Ehrlich's side-chain theory that turned out to be incorrect. It does not follow, however, that the entire theory is wrong. Despite these errors, the theory is based on a correct principle, which is that "specific receptors on cells interact with foreign material in a highly specific way, and this triggers their increased production and release from the cell surface so that they can inactivate foreign material as antibodies" (Kaufmann, 2008, p. 707).

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[122] See Baxby (1999).

If the aforementioned considerations are correct, then, contrary to the Pessimistic Meta-Induction, the epistemic account seems to mesh with the history of science. If theories are not taken as monolithic wholes, then it seems reasonable to suggest that modest contributions to scientific knowledge can be made even if the theoretical background in which they are embedded is not strictly true as a whole. The proper objects of knowledge are statements (or propositions), and so scientific progress should be evaluated on the basis of individual claims to knowledge rather than whole theories. As we have seen in Chapter 2, this seems to be the way in which scientists themselves assess scientific discoveries.

It is also important to emphasize that the kind of knowledge we are concerned with, i.e., scientific knowledge, may be reasonably characterized as fallible knowledge. To say that scientific knowledge is fallible, however, is not to say that scientists can know a certain statement even if it is not true. That would amount to denying that knowledge entails truth. Rather, what is meant by saying that scientific knowledge is fallible is the following:

> (F1) It is possible for *S* to know that *p* even if *S* does not have logically conclusive evidence to justify believing that *p* (Feldman, 1981, p. 266).

As Feldman points out, this means that one can know something on the basis of non-deductive arguments. For naturalists, of course, this comes as no surprise. Most of the ordinary things we take ourselves to know in everyday life are known on the basis of inductive, perceptual, or testimonial evidence that does not entail what it is evidence for.[123] Scientific knowledge and ordinary knowledge are not different in kind. Since most

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[123] See the Appendix, "The Analysis of Knowledge," for more on inductive knowledge.

of what we take ourselves to know can be reasonably said to be fallible knowledge, scientific knowledge may be said to be fallible knowledge as well.[124]

To sum up, if the considerations put forward in this chapter are correct, then the skeptical attacks of those who are pessimistic about the accumulation of theoretical knowledge in science are not as devastating as they might appear to be at first glance. That is to say, these skeptical arguments fail to show that theoretical knowledge in science is unattainable. We have to realize, however, that scientific knowledge is fallible in the sense that it is grounded in non-deductive arguments.[125] In that respect, scientific knowledge is just like ordinary knowledge. Epistemic agents usually apportion their degrees of belief according to the strength of the evidence. Scientists are epistemic agents and it seems that the same strategy is available to them as well (and to those who are optimistic about scientific progress in terms of the accumulation of scientific knowledge, even at the theoretical level).

As we have seen in Chapter 2, the Early Modern natural philosophers realized that an inquiry into the natural world that has any hopes of succeeding must be modest and proceed in a piecemeal fashion, otherwise despair may creep in. We should therefore take a selective approach toward claims to scientific knowledge. Some claims to scientific knowledge may be more warranted than others, depending on the state of the evidence at any given time, and our degrees of belief will change accordingly. As Bertrand Russell (1997) observes, the "scientific temper of mind is cautious, tentative, and piecemeal" (p. 245). We may not know the truth, the whole truth, and nothing but the truth, but we may reasonably believe some claims to scientific knowledge that are sufficiently warranted.

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[124] According to Feldman (1981), if *p* is known fallibly, and *p* entail *q*, and *q* is believed on the basis of *p*, then *q* is also known fallibly even though the evidence for *q* (i.e., *p*) entails *q* (p. 267).

[125] See Bird (forthcoming). Available at <http://eis.bris.ac.uk/~plajb/research/papers/Induction.pdf >.

Chapter 6
Scientific Knowledge

If what I said in Chapter 5 is correct, then the skeptical attacks on scientific knowledge discussed above are not as devastating as they might appear to be at first glance. In other words, these skeptical arguments do not provide compelling reasons for skepticism and pessimism about the accumulation of scientific knowledge. It is important to note, however, that these skeptical arguments are directed against the attainment of theoretical knowledge in particular. The accounts of scientific progress discussed in Chapter 3, namely, semantic, functional-internalist, and Bird's epistemic account all focus almost exclusively on scientific theories. The danger of focusing exclusively on theories in philosophy of science is that we end up with a picture of a "mummified" science, as Hacking (1983, p. 1) puts it, or "Legend," as Kitcher (1993, pp. 3-10) puts it, rather than actual science. To avoid making a mummy of science, I propose to take seriously the insights we have gained from the historical investigation in Chapter 2 and incorporate them in our account of progress.

In this chapter, then, I would like to expand on the epistemic account of scientific progress outlined in Chapter 3. I wish to improve on Bird's epistemic account by incorporating into our notion of scientific knowledge the types of knowledge that scientists actually value. From what has been said so far, it should be clear that, in addition to theoretical (inferential) knowledge, these types of knowledge include the following: empirical (factual) knowledge, practical knowledge, and methodological knowledge. I think that taking these types of knowledge to count as scientific knowledge, and granting that the accumulation of each counts as scientific progress, would allow us to put the skeptical and pessimistic worries discussed in Chapter 5 behind us. In other words, science is not just about theories, and scientists make progress by accumulating knowledge of other sorts as well, not just of the theoretical sort. If this is so, then these are additional reasons to be optimistic about cognitive progress in science.

6.a. Worthless Knowledge

As we have seen in Chapter 2, Early Modern natural philosophers were concerned with the question of the usefulness of natural knowledge. As Wilson (1995) writes, “the question of the usefulness of knowledge of nature kept arising in the second half of the century, and the Royal Society was often trying to reassure itself on this point” (p. 31). Now, on the epistemic view of scientific progress, accumulation of scientific knowledge constitutes progress. What kind of knowledge? Does it have to be useful? Is it worthwhile to pursue knowledge like Shadwell’s virtuoso, Sir Nicholas Gimcrack, “so it be knowledge, ‘tis not matter of what”? (Nicolson & Rodes, 1966, pp. 26-27)

From a naturalistic standpoint, the objection that the accumulation of worthless knowledge does not count as progress can be construed in at least two ways. First, it can be construed as the claim that scientists set out to find out useful things to know about the world. Whatever they deem worthless, they would not seek to know it. The problem with this construal, however, is that scientists cannot know in advance what knowledge counts as worthwhile, before they know it, because they do not know it. So, to say that scientists initially seek out only useful knowledge seems to get us into some sort of *Meno* paradox: “a man cannot try to discover either what he knows or what he does not know; he would not seek what he knows, for since he knows it there is no need of the inquiry, nor what he does not know, for in that case he does not even know what he is to look for."[126] How can scientists know what is worthwhile to know before they know it?[127]

Recently, several epistemologists have argued that there are some truths that are not worth knowing. For instance, according to Catherine Elgin (2002):

> *Not every truth is worth knowing*; nor is every falsehood worth dismissing. Some truths are trivial. Some falsehoods are useful approximations or illuminating idealizations. If we can zero in on the truths and falsehoods that are worth taking seriously, we make cognitive progress (p. 22, my emphasis).

Let us examine the following claim more closely:

(W) Not every truth is worth knowing.[128]

It seems to me that (W) is far from obvious. How can we know that (W) is true? Well, I suppose we need at least one example, *p*, such that *p* is not worth knowing. That is, (W) is equivalent to 'There is at least one truth, *p*, such that *p* is not worth knowing'. So we ask with respect to some *p*: Is *p* worth knowing? On the one hand, it seems that in order to know whether or not *p* is worth knowing, we need to know that *p*. But if we know that *p*, then what is the point of asking if *p* is worth knowing? We might reasonably ask if *p* is worth memorizing. We might reasonably ask if *p* is worth recording. We might reasonably ask if *p* is worth publishing. We might reasonably ask if *p* is worth teaching. It seems pointless, however, to ask if *p* is worth knowing, since we already know that *p*. The question whether or not *p* is worth knowing is no longer relevant given that we know that *p*.

On the other hand, if we don't know that *p*, then how can we know that *p* is not worth knowing? If we don't know that *p*, then we don't know that *p* is not worth knowing

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[126] Plato, *Meno*, 79c, 80d-e.
[127] See also Wilson (1995), p. 99.
[128] Cf. Kvanvig (2008).

(or worth knowing, for that matter). Since this is the case for every *p*, it seems that we cannot know that (W) is true. Again, this gets us into some sort of *Meno* paradox: if we know that *p* is worth knowing, then we know that *p*, and then there is no need to ask whether or not *p* is worth knowing. But if we don't know that *p*, then how can we tell whether or not *p* is worth knowing?

To this it might be objected that, in order to know whether or not *p* is worth knowing, we need not know that *p*. We may simply believe that *p*, and then ask if it would be worthwhile to know that *p*. However, it seems that we would not be able to make a reasonable, well-informed judgment about whether or not it is worth knowing that *p* based solely on our belief that *p*. To see why, consider an example that is usually cited in support of (W). According to Pritchard (2007), knowing the measurements of every grain of sand on a given beach is a pointless truth. As Pritchard (2007) puts it, "no fully rational agent is curious about the measurements of every grain of sand on a given beach" (p. 102). Indeed, what might appear more dull and futile than collecting and measuring sand? It is hard to think of something more seemingly useless. Apparently, however, this is precisely what Robert Holman, an oceanographer, is doing. His collection of sand has led to the development of Argus, a computerized photography system that researchers now use to study beaches.[129] Using the Argus system, researches can make measurements of underwater topography, formation and movement of sandbars along coasts.[130] Knowing more about these sandbars turns out to be very important for understanding beach erosion and rising sea levels that is the result of climate change. Using data from Argus, researchers can now track the formation of sandbars. So, what seemed worthless

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[129] See The Coastal Imaging Lab. Available at <http://cil-www.coas.oregonstate.edu>.

[130] Sandbars are ridges of sand that are built up as a result of water currents in coastal waters.

at first glance, turned out to be of great value epistemically. Could Holman have anticipated this increase in knowledge before he set out to collect sand and study it? Probably not; the lesson, then, is "never give up observing," as Holman puts it.[131]

This last point seems to apply to science quite generally. That is to say, science is in the business of finding out about the unknown. As Richard Feynman (1995) puts it, "we do not yet *know* all the basic laws: there is an expanding frontier of ignorance" (p. 2, original emphasis). Likewise, sociologists of science often point out that there is a "continuous branching of science into new areas of ignorance."[132] This is typically explained in terms of competition. For instance, according to Stephan Fuchs (1993):

> Since scientists must recognize and use one another's work, they compete for their peers' attention. Because audiences do not want to listen repeatedly to statements that are old and familiar, scientists must produce statements that are, in some ways and to some extent, new. *The highest rewards go to the scientists who are seen to advance the state of knowledge*. This is the critical connection between competition and change in science: *competition drives change because it forces people to say something new* (p. 937, my emphasis).

As we have seen in Chapter 3, Kuhn thinks that there is a precarious balance between innovation and tradition. He calls it the "essential tension." On Kuhn's view, the balance tips in favor of innovation only rarely, i.e., in periods of "revolutionary science." Most of the time, i.e., in periods of "normal science," tradition is more dominant than novelty. Kuhn (1970) criticizes Popper for focusing too much on "revolutionary science" (pp. 12-13). It seems to me, however, that Kuhn focuses too much on "normal science," and thus underestimates the significance of innovation in science, which doesn't have to be "revolutionary" in Kuhn's sense.

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[131] Cornelia (2009). Available at <http://www.nytimes.com/2009/01/06/science/06prof.html>.

[132] See Lemaine, et al (1976).

But perhaps my criticism of (W) is not quite fair. So far, I assumed that (W) does not come with a “given limited time and resources” clause. Perhaps (W) should be construed with such a clause. In that case, (W) would amount to saying that epistemic pursuits begin with questions to be answered, and that we can make judgments about which question is worthy of an answer, and then invest our time and resources accordingly. For example, though probably apocryphal, is it often said that medieval scholastics were concerned with the following question: “How many angels can dance on the head of a pin?” If they really invested their time and efforts trying to find an answer to this question, I suggest, it is because they thought that this question does have an answer. To medieval scholastics, an answer to this question might have had intrinsic value insofar as it might have revealed something new about the nature of creation. To us, this question may seem utterly valueless, both intrinsically and instrumentally, and not worthy of our consideration, precisely because we think there is no answer to this question, let alone a true answer, given that we think that there aren’t any angels. However, the point of (W), it seems, is to assert that *true* answers to certain questions are worthless, i.e., even if true, they have neither intrinsic nor instrumental value, and thus should not be pursued.

If this is correct, then perhaps we cannot know a priori whether a certain truth has intrinsic or instrumental value. This seems to be so in the case of scientific knowledge in particular. As the Argus example seems to illustrate, scientific knowledge may have unforeseen value. Sometimes we have to pursue certain projects only to find out that they lead to dead-ends. But I take it that the point about worthless truths is not meant to be a practical one about allocating resources, but an epistemological one about the value of knowledge. Otherwise, I think it is trivial that some epistemic pursuits are (currently) beyond our reach.

As we have seen in Chapter 2, the value of a scientific discovery is something that can be evaluated after the fact. The scientific practice of assessing progress, as it is instantiated in the institution of the Nobel Prize, shows that it is not easy to make progress in science and that progress may often be recognized and assessed only with the benefit of hindsight. On the epistemic account of scientific progress, we have to know that we know in order to know that progress has been made.[133] However, we do not always know that we know, and thus we may not know that progress has been made. As Bird (2007) puts it:

> for S to know that she has made progress, S must know that she knows that T is true. S may well be in such a position, but since one does not necessarily know that one knows, it will also be possible to be in the position of having made progress but not knowing that one has done so. This is plausibly the case when T is as the cutting edge of a field and when new methods and techniques are used in confirming T. Far from being internally accessible, like many of the best things in life, the most exciting contributions to progress are often recognizable as such only with the benefit of hindsight (p. 87).

It takes time to assess the progressive nature of scientific discoveries, i.e., whether or not they are genuine contributions to scientific knowledge. We can see this in the institution of the Nobel Prize where it usually takes a few years from the time of discovery until Nobel Laureates receive the prize.[134]

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[133] Or at least be justified in believing that we know.

[134] In a recent interview, 2009 Nobel Laureate in Physiology or Medicine, Carol W. Greider, was asked the following question: “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? (Dreifus, 2009). “I certainly hope so,” Dr. Greider replied. “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” (Dreifus, 2009).

In the case of Krebs, for example, it took almost twenty years for his work on the citric acid cycle to result in the reception of the Nobel Prize. Krebs neglected his study of the processes by which foodstuffs decompose in the energy-producing reactions of animal tissues for a while, but then he resumed his studies in the summer of 1935 after he moved to the Department of Pharmacology at the University of Sheffield. He first published the results of his studies in 1937[135] and in 1938.[136] Krebs was awarded the Nobel Prize in Physiology or Medicine in 1953 after the citric acid cycle had been studied extensively by others and was found to serve as a common terminal pathway in many organisms. As Krebs (1953) said in his Nobel Lecture:

> It is indeed remarkable that all foodstuffs are burnt through a common terminal pathway. About two-thirds of the energy derived from food in higher organisms is set free in the course of this common pathway; about one-third arises in the reactions which prepare foodstuffs for entry into the citric acid cycle.[137]

It was also found that “the cycle occurs in all reprising tissues of all animals, from protozoa to the highest mammal” (Krebs, 1953). In the case of microorganisms:

> Many observations, especially from isotope experiments, support the view that in some micro-organisms the cycle primarily supplies intermediates rather than energy, whilst in the animal and most other organisms it supplies both energy and intermediates (Krebs, 1953).

According to Krebs (1953), “the remarkable fact that the reactions of the cycle have been found to occur in representatives of all forms of life, from unicellular bacteria and protozoa to the highest mammals,” has advanced not only biochemistry in particular but also biology in general. As Krebs (1953) said in his Nobel Lecture:

> We have long been familiar with the fact that the basic constituents of living matter, such as the amino acids and sugars, are essentially the same in all types of life. The study of intermediary metabolism shows that the basic metabolic

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[135] See Krebs & Johnson (1937).
[136] See Krebs & Salvin (1938).
[137] Available at <http://nobelprize.org/nobel_prizes/medicine/laureates/1953/krebs-lecture.pdf>.

> processes, in particular those providing energy, and those leading to the synthesis of cell constituents are also shared by all forms of life.

The second construal of the objection that the accumulation of worthless knowledge does not count as progress is the following: after attaining knowledge about a certain phenomenon, scientists may deem it useful or not. If it is useful, then progress has been made. If it is not useful, then no progress has been made. As we have seen in Chapter 2, however, in scientific practice, usefulness is one criterion of progress assessment, but not the only one. What is important for progress assessment is the sort of knowledge gained. As we have seen in Chapter 2, the scientific practice of assessing progress reveals that scientists take progress to consist in the accumulation of knowledge of the following sorts:

(EK) Empirical (factual) knowledge usually comes in the form of experimental results, observations, instrumental readings and measurements, and any other sort of "data."

(TK) Theoretical (inferential) knowledge usually comes in the form of explanations and well-confirmed hypotheses.

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

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

Recall how Godfrey-Smith (2007) frames the problem of scientific progress. The crucial questions are the following: Are we going somewhere worth going? Are we collecting something worth collecting? On semantic accounts of progress, it seems that we are going somewhere worth going, i.e., truth, and we are collecting something worth collecting, i.e., approximately true theories. But the problem is that, on semantic accounts, the same question of worth may be asked: Are there approximately true theories that are more worthwhile than others? If so, which is the proper aim of science? Kitcher tries to address this problem by identifying significant truth as the aim of science (see Chapter 3). I would like to suggest that we can do better. If we turn to the scientific practice of assessing progress for insights into progress, we can identify the sorts of knowledge that scientists deem valuable [i.e., (EK), (TK), (PK), and (MK)].

The objection under consideration is even more troublesome, it seems to me, for functional-internalist accounts of progress. For, on such accounts, it seems that there are no clear answers to the questions posed by Godfrey-Smith. Are we going somewhere worth going? Are we accumulating something worth accumulating? Is it worthwhile to accumulate puzzle-solutions? What is their epistemic worth? On Laudan's account, for example, how can puzzle-solutions have any epistemic worth if they do not have to be genuine solutions to real problems? What is useful or worthwhile about having a fabricated solution to a problem that does not pertain to a real state of affairs? Kuhn (1996) claims that, even if his position is seen as relativism, he "cannot see that the relativist loses anything needed to account for the nature and development of the sciences," by giving up truth (p. 207). It seems to me, however, that something is lost after all. If not our conception of scientific progress entirely, then, at the very least, it seems that we lose the ability to explain why we care about scientific knowledge.

6.b. Knowing That and Knowing How

If the considerations advanced in Section 6.a. are correct, then we can avoid the problem about the worth of scientific knowledge, and hence whether or not it counts as scientific progress, by taking the types of knowledge that scientists actually value to count as scientific knowledge. As we have seen in Chapter 2, scientists evaluate scientific discoveries as progressive on the basis of the accumulation of knowledge of the following sorts: empirical (factual), theoretical (inferential), practical, and methodological. For the most part, philosophers of science have focused on theoretical knowledge, while underestimating the importance that scientists attribute to empirical (factual) knowledge. As the Nobel Lectures show, however, this sort of knowledge plays a role when it comes to evaluating discoveries in science. If a certain scientist is credited for having discovered something that was previously unknown, then that counts as progress, even if it doesn't involve an elaborate theoretical framework like the ones philosophers of science often focus on (e.g., quantum mechanics). Pavlov's work on the physiology of digestion is a case in point as well as Laveran's discovery of the causative agent of malaria.

Another kind of knowledge of interest to scientists that is largely ignored by philosophers of science is practical knowledge (i.e., immediate and long-term practical applications). Kitcher, for instance, acknowledges practical progress as a goal of science. But then he sets it aside and goes on to give an account of cognitive progress. As we have seen in Chapter 2, however, the idea of practical progress looms large not only in Early Modern science but also in modern science. The problem is that "the notion of practical progress proves far more difficult than we might have thought" (Kitcher, 1993, p. 92). However, I think that we can begin to make sense of it, in the larger context of scientific progress, from the standpoint of the epistemic account of progress. That will allow us to account for the types of knowledge that scientists are interested in. To do so, however, we would have to give up a distinction that is common in philosophy, but seems to be of no use when it comes to understanding scientific progress, namely, the distinction between ‘knowing that’ and ‘knowing how to’.

Gilbert Ryle was perhaps the first to distinguish explicitly between ‘knowing that *p*’ (propositional knowledge) and ‘knowing how to *A*’ (knowledge of skills).[138] Since then, it seems that philosophers have focused almost exclusively on the former. As we have seen in Chapter 2, however, practical and methodological knowledge (‘knowing how to’) seem to be of equal importance in science when it comes to assessing progressive discoveries. For example, 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. Moreover, on closer inspection, it seems that ‘knowing how to’ may simply be a subspecies of ‘knowing that’, at least as far as scientific knowledge is concerned, involving propositional knowledge about methods of doing things. For example, suppose that person *A* is getting a blood transfusion and the donor is person *B*. Suppose that *A* has blood type AB. In order to perform a safe blood transfusion, we have to know the blood type of *B*. In other words, *knowing how to* administer safe blood transfusions requires *knowing that A* has blood type AB and *B* has blood type, say, AB.

When it comes to scientific knowledge, then, it may appear as if ‘knowing how to’ and ‘knowing that’ are distinct kinds of knowledge. This appearance, however, may be misleading. According to Wilson (1996):

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[138] See Ryle (1946) and (1949).

> If the thesis that we make knowledge in virtue of our expressing this knowledge verbally were true, it ought to be true quite generally: we would make it true that humans cannot breathe underwater and that iron rusts when exposed to damp air. Yet no one tries to defend this thesis. The reason for the asymmetry is that we can conceive of non-verbal, action-guiding knowledge of the commonplace facts, whereas we cannot easily conceive of an inarticulate knowledge of the scientific facts; these appear to depend in some way not on the world but on our creation of words. But *this impression is an illusion*. We do not possess two distinct kinds of knowledge, one of which we make. *Commonplace knowledge and scientific knowledge belong to the same species*, and one aim of scientific praxis is to reduce the perceived distance between them (p. 170, my emphasis).

The same sort of illusion may be operative in the case of the distinction between 'knowing that' and 'knowing how to' as far as scientific knowledge is concerned. In fact, when we realize that practical knowledge is a form of scientific knowledge, it becomes easier to conceive of action-guiding knowledge of scientific facts, as the blood typing example seems to illustrate. This is not to say, of course, that the distinction between 'knowing that' and 'knowing how to' is utterly useless. Rather, the suggestion is simply that it is not useful when it comes to understanding scientific knowledge, and thus scientific progress. Even if 'knowing how to' is not a subspecies of 'knowing that',[139] it still seems reasonable to suggest that the former involves cognitive capacities that can be reasonably seen as relevant to cognitive progress in science. In other words, if 'to know' is an achievement verb, then 'knowing how to *A*' seems no less an instance of cognitive success than 'knowing that *p*'.[140]

According to Baird and Faust (1990), when most philosophers of science speak of scientific progress, they speak in terms of theory alone, and they neglect other aspects of

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[139] Those who would point to natural language as a way to mark the distinction between 'knowing that' and 'knowing how to' should note that in French the verb *savior* is used in the sense of 'to know that' as well as 'to know how', whereas the sense of 'being acquainted with' is reserved for the verb *connaitre*.

[140] See Adams (1958). I will say more about cognitive success and progress in Chapter 7.

science, such as experimentation, instrumentation, and methodology. They offer their diagnosis of the source of this mistaken asymmetry:

> Classically, knowledge has been analyzed in terms of true belief. While, perhaps, theories may be conceived as a species of belief, it is difficult to conceive of experiment as a species of belief. Experiment essentially involves the manipulation of bits of the world; belief may help direct the manipulation, but belief cannot take the place of this manipulation. When approached from the classical analysis of knowledge, it is hard to say exactly *what* is progressing, when speaking of the progress of *experimental* scientific knowledge (p. 148, original emphasis).

They urge a conception of scientific progress "broad enough to include the production of new scientific instruments and instrumental techniques" (Baird & 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 & Faust, 1990, p. 148). They also argue that scientific knowledge "consists in the ability to *do* things with nature, not *say* things about nature" (Baird & Faust, 1990, p. 147). We can accommodate these insights, I suggest, by granting that know-how counts as scientific knowledge. Scientific knowledge, as the scientific practice of assessing progress seems to show, 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. To understand that, however, we have to give up the distinction between 'knowing that' and 'knowing how to', which may be useful in other ways, but not when it comes to understanding scientific knowledge, and hence scientific progress.

To this it might be objected that there are scientific fields that do not seem to have any practical applications, and thus no practical knowledge. It might appear as though fields like cosmology are inherently non-practical. In reply, I would like to emphasize that there is nothing in the scientific practice of assessing progress that suggests discoveries must make contributions with respect to empirical, theoretical, practical, and methodological knowledge all at once. Nor does the epistemic account of scientific progress require that discoveries contribute with respect to these four types of knowledge simultaneously and immediately. For example, some discoveries may not have any immediate practical applications, but then turn out to have some initially unexpected practical and methodological consequences. Harvey's discovery of the circulation of the blood seems to illustrate this point.

Galen believed that blood is continually produced in the liver and then spreads around the body for nourishment. This blood nourishes the body and then the body extricates material in the form of urine, sweat, etc. Consequently, it was also believed that haemorrhoidal and menstrual blood were also normal forms of excrement. Hence, certain forms of bloodlettings were considered "normal," i.e., the body's natural way of ridding itself of waste products.[141] In medical practice, bloodletting was believed to be a form of treatment for fever and inflammation because they were believed to be caused by too much blood in the body (hence the redness, swelling, increased body temperature and pulse). Harvey's discovery of the circulation of the blood did not diminish the popularity of bloodletting. Perhaps that was due to the fact that Harvey himself did not argue against the practice explicitly.[142] Still, this is a practical application of Harvey's theory that could have easily been realized, if not by Harvey, then by someone else later on. For, as Mowry (1985) shows, "various medical practices such as the varied use of ligatures in blood-letting, amputation and removal of tumors—incorrectly explained on Galen's theory—were elucidated on Harvey's theory" (p. 56). In particular:

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[141] See Coutinho & Segal (1999), chap. 2.

[142] See Davis (1971).

> Since blood flows to the periphery via the arteries and returns to the heart via the veins, it makes sense to apply a medium-tight ligature to the upper arm, thus allowing blood to enter through the arteries but preventing it from leaving through the veins. This blood can be drawn off easily from the distended forearm veins. But if, as Galen's theory holds, blood flows to the periphery via the veins, the veins proximal to the ligature should swell and blood should be taken easily from *these* veins; this, however, does not occur; and Galen's theory can offer only a most obscure account of blood-letting in terms of the ligature 'turning away' venous blood which retracts to the opposite side of the body (Mowry, 1985, p. 73, original emphasis).

Moreover, even in cosmology, which seems to be the typical non-practical scientific field, it is not obvious that there are no concerns for practical applications at all. 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.[143] By now, the alleged paradoxes of time travel have been shown to be harmless. For example, if we distinguish between "historical time" and "personal time," there is no problem in saying that the time-traveler has gone back in "historical time," not her "personal time." In addition, it is often claimed that if time travel were possible, then one would be able to kill one's parents and thereby prevent one's own birth. In reply, however, it may be argued that, given that the time-traveler was born, no effect she has on the past can change that. In other words, time travel does not mean that the time-traveler has a license to change the past.[144] Of course, none of this shows that time travel is physically possible. But this is precisely what cosmologists are trying to find out. One proposal, for example, is that time travel may be physically possible through wormholes in space-time. Kurt Gödel (1949) has shown that there are models of space-time that are consistent with general relativity and that allow time travel into the past.[145] Whether or

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[143] See, e.g., Davies (2003) and Gott (2002).

[144] See Horwich (1987).

[145] See also Pfarr (1981).

not time travel will be feasible remains to be seen. For present purposes, it is important to recognize that scientists are concerned with the potential practical applications of their work, even in fields like cosmology, in which it may not appear to be so at first glance.

In addition to this concern for potential practical applications in cosmology, there is also a noticeable emphasis on methodological advancements. At the 1995 conference on “Key Problems in Astronomy and Astrophysics,” Allan Sandage, a student of Edwin Hubble (1889-1953) at the California Institute of Technology, 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” in 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 (Baryshev, 2008, my emphasis).[146]

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.

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[146] Available at <http://ppc08.astro.spbu.ru/index.html>.

By being able to account for both cognitive and practical progress, the epistemic account also exhibits theoretical virtues, such as simplicity and unification. It is a simple view of progress because it accounts for the varieties of progress mentioned above by means of one concept. This revised concept of scientific knowledge allows us to account for these varieties of progress in a unified manner, incorporating both practical and cognitive progress. In that respect, the epistemic account is the best of both worlds. In other words, it preserves what is useful and illuminating about the semantic and functional-internalist accounts, namely, that truth is necessary for progress and that problem-solving is evidence for progress, and combines it together into a coherent view of scientific progress.

It is noteworthy that some philosophers of science have argued that theoretical virtues are often taken to be the aims of science. In other words, science is in the business of producing theories that exhibit properties, such as consistency, precision, scope, simplicity, unity, explanatory power, intelligibility, fecundity, and testability (the so-called “superempirical virtues”). For example, according to Laudan (1984):

> Many scientists espouse values or goals that, under critical challenge, they cannot characterize in a succinct and cogent way. They may be imprecise, ambiguous, or both. Such familiarly cited cognitive goals as simplicity and elegance often have this weakness, because most advocates of these goals can offer no coherent definition or characterization of them (p. 52).

However, whether we think that theoretical virtues have epistemic value or merely pragmatic value,[147] it seems to me inaccurate to say that they are the goals of scientific inquiry. It seems to me that we wouldn’t care much for consistent, precise, or unified theories, unless we thought that they are more likely to yield knowledge by having these

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[147] See, e.g., van Fraassen (1980).

virtues (whether or not they actually do). So, perhaps it might be useful to distinguish between the ends of scientific inquiry and the means for achieving those ends.

For instance, Lorraine Daston and Peter Galison (2007) identify objectivity as the goal of scientific inquiry. Daston and Galison relate objectivity to “epistemic virtues,” which are associated with certain periods in science. Epistemic virtues are “norms that are internalized and enforced by appeal to ethical values, as well as pragmatic efficacy in securing knowledge” (Daston & Galison, 2007, p. 40). Although they identify various forms of objectivity, i.e., “truth-to-nature,” “mechanical objectivity,” and “trained judgment,” the basic idea is that the role of the practitioner must be as limited as possible in order to obtain this goal.[148] For instance, Daston and Galison (2007) say the following about mechanical objectivity:

> By mechanical objectivity we mean the insistent drive *to repress the willful intervention of the artist-author*, and to put in its stead a set of procedures that would, as it were move nature to the page through a strict protocol, if not automatically (p. 121, my emphasis).

Could objectivity be the end of scientific inquiry? Are scientists trying to be objective simply for the sake of being objective? Do they prefer theories that have this property just for the sake of having such theories? Does objectivity have instrumental or intrinsic value in science?[149]

Consider an analogous case: some media outlets are objective, i.e., they report the news without commentary and interpretation, or at least they try to do so. But, surely, this is not their goal. That is, they do not objectively report the news for the sake of objectively reporting the news. Rather, they do so because they think that this is what

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[148] Cf. Janack (2002).

[149] Daston and Galison (2007) claim that science is about objectivity, not truth (p. 214). In some instances, however, they talk about the means to attain truth (p. 377) and they also claim that the epistemic virtues served a common goal: a faithful representation of nature (p. 318).

their audience wants. Their aim may be ratings, revenues, and profits, just like any other business.[150] If we say that these media outlets are aiming to be objective for the sake of objectivity itself, then it seems we are confusing means and ends. They want to give their audience what it wants (e.g., objective reporting) in order to get what they want (e.g., profits). This may seem like a rather cynical view of the media. However, the analogy should not be taken too far. The point is simply that objectivity, in the media as well as in science, is not an end in itself. Rather, objectivity is a means to achieving certain ends. In the case of the media, the ends may be ratings and profits. Even if some media outlets operate on the belief that it is in the best interest of their audience to know the truth, whether their audience likes it or not, it is still the case that objectivity would be a means to achieving that aim. Similarly, scientists have learned that the more objective they are, the more likely they are to learn something about the domain of interest. They want scientific theories to have theoretical virtues, such as consistency, not for their own sake, but insofar as they think that, by having them, it is more likely that these theories will give them knowledge about the world.

Lastly, Michael Devitt discusses the notion of methodological progress (i.e., methods and techniques of learning about nature). According to Devitt (1984), "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" (p. 163).[151] Like practical knowledge, methodological knowledge is also a form of know-how, i.e., knowing how to learn about nature. The case of Harvey's discovery of the circulation of the blood seems

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[150] It is noteworthy that state-owned media outlets, i.e., those that are not concerned with revenues as a business, are usually not considered to be objective. This may be so, I suppose, because they are also perceived as having no concern for the truth, especially in non-democratic states.

[151] See also Kitcher (1993), p. 140.

to be an instance of methodological progress. Arguably, the methods that Harvey used to demonstrate the circulation of the blood were as important as the discovery itself for the advancement of physiology and the medical sciences. As we have seen in Chapters 2 and 4, Harvey used common methods, such comparative anatomy and dissection, as well as new techniques, such as ligature experiments and quantitative analysis, in his work. For example, Harvey used ligatures (tourniquets) to show that arteries carry blood away from the heart and that veins carry it back to the heart. Since arteries are deeper than veins in the limbs, a tight ligature around the arm compresses both ligatures and veins. In this case, there is no noticeable change in the hand or veins. A less tight ligature compresses the veins but not the arteries. In this case, the rush of blood causes the hand to swell. When a tight ligature is loosened, the rush of blood to the limb causes the hand the veins to swell. This shows, according to Harvey, that blood flows from the heart through the arteries to the peripheral tissues and then from the arteries to the veins. Furthermore, venous valves allowed blood to flow only toward the heart.[152]

As for quantitative analyses, Harvey supported his theory of circulation with the following calculations. He considered the size of the ventricle, the amount of its contents that it ejects with each contraction, and the rate of its beat. If there is no circulation, he reasoned, then the amount of blood ejected by the heart into the arteries in half an hour would be greater than the amount of blood in the whole body and the heart would have to eject a greater weight of blood in a day than the weight of the whole body. Since that is not the case, it must be that the same blood is continuously passing through the heart. In other words, blood circulates around the body. According to Cohen (1985b), this

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[152] See Mowry (1985).

“quantitative method was not then in general use and Harvey was fully aware that his numerical reasoning was as radical in its method as in its results” (p. 193).

To sum up, in this chapter I argued that focusing exclusively on theoretical truth runs the risk of making a mummy of science. As naturalists, we want to avoid making a mummy of science. To that end, we have to consider seriously the insights of the historical investigation of Chapter 2. These are that (a) scientists take scientific knowledge to be the aim of scientific inquiry; (b) scientists make judgments about scientific progress based on the accumulation of scientific knowledge; (c) scientists take scientific knowledge to include not only theoretical (inferential) knowledge but also empirical (factual), practical, and methodological knowledge. Each contribution to scientific knowledge in terms of one of these types of knowledge counts as progress. Science advances owing to not only the grand scale theoretical frameworks of theoreticians but also the painstaking work of experimenters and data collectors, lab technicians, field workers, and the like.[153]

If this is correct, then we should revise Bird’s epistemic account of scientific progress to include not only theoretical knowledge but also these other types of knowledge. We can do so rather easily, I suggest, if we grant that ‘knowing how’ (practical and methodological knowledge) is a subspecies of ‘knowing that’ (theoretical and empirical knowledge). Even if it isn’t, it still seems reasonable to take advances in practical and methodological knowledge as instances of cognitive success, and hence cognitive progress. Granted that practical and methodological knowledge both count as scientific knowledge, and hence that their accumulation counts as scientific progress, the revised epistemic account of scientific progress is the view that scientific progress is

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[153] See the quotation from Rutherford in Chapter 1.

constituted by the accumulation of scientific knowledge. A scientific episode constitutes scientific progress when it shows the accumulation of scientific knowledge, where scientific knowledge consists of each the following: empirical (factual), theoretical (inferential), practical, and methodological knowledge. Each of these counts as scientific knowledge; the accumulation of each advances science.

This raises another question, which I will address in the concluding chapter of this dissertation. I suggested that both ‘knowing that’ and ‘knowing how’ constitute cognitive success, and hence that both should count as cognitive progress in science. It is common among epistemologists, however, to distinguish between knowledge as a type of cognitive achievement and understanding as a type of cognitive achievement. Indeed, Duncan Pritchard (forthcoming) argues that understanding is a type of cognitive achievement, whereas knowledge is not, because, unlike knowledge, cognitive achievements are compatible with what he calls “environmental epistemic luck.”[154] For example, *S* doesn’t know that there is a barn in front of her, even if it is true, given that, unbeknownst to her, she is in barn-façade County.

However, among philosophers of science, as Pritchard points out, it is common to take understanding as a species of knowledge. Are philosophers of science justified in taking understanding as a species of knowledge? Perhaps scientific knowledge, as an instance of cognitive success, may be more accurately characterized as understanding rather than knowledge? If so, what is the difference between understanding and knowledge? I will address these questions in the concluding chapter of this dissertation.

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[154] Available at <http://www.philosophy.stir.ac.uk/staff/duncan-pritchard/documents/PritchardOnUnderstanding.pdf>.

Since I am concerned with cognitive progress in science, however, I will discuss these questions only in so far as scientific understanding is concerned.

Chapter 7
Conclusion

In Chapter 6, I argued that we should expand our notion of scientific knowledge. Focusing exclusively on theoretical truth, as philosophers of science often do, gives us an inaccurate picture of science, a “mummy,” as Hacking puts it. Naturalists should clearly want to avoid making a mummy of science. To do so, I suggested that we learn from the historical investigation of Chapter 2. This investigation shows that, for scientists, scientific knowledge is not strictly theoretical (inferential) but also empirical (factual), practical, and methodological. As the scientific practice of assessing progress shows, when scientists make evaluative judgments about scientific discoveries, they consider the accumulation of each of these types of knowledge as advancing science. Accordingly, given this revised notion of scientific knowledge, we also get a revised version of the epistemic account of scientific progress. According to this revised version, scientific progress occurs whenever there is accumulation of scientific knowledge, where scientific knowledge includes knowledge of the following sorts: theoretical (inferential), empirical (factual), practical, and methodological.

In this chapter, I would like to consider a final objection before making some concluding remarks. In light of what I have said so far, it might be objected that instances of cognitive success in science may be more accurately characterized as instances of understanding rather than knowledge. In other words, if knowledge and understanding are distinct, it might be argued that knowledge is not a scientific aim after all. It might seem as if the goal of scientific inquiry is understanding rather than knowledge. If this is so, then we need to answer the following question: “What is the difference between understanding and knowledge?” especially since there seems to be a consensus among philosophers of science that understanding is a species of knowledge. It is to this question that I now turn, though I will limit the discussion to scientific understanding in particular.

7.a. Scientific Understanding

According to Grimm (2006), philosophers of science seem to agree that scientific understanding is a species of knowledge, and hence factive;[155] however, epistemologists tend to disagree.[156] In epistemology, the standard view seems to be that understanding is not a species of knowledge. If this is so, then it might be argued that knowledge is not a scientific goal after all. It might be argued that the aim of science is understanding rather than knowledge. But what is the difference between understanding and knowledge? Several epistemologists have argued that understanding, unlike knowledge, is not factive. To say that knowledge is factive is to say that if *S* knows that *p*, then *p* is true. In the case of understanding, however, *S* may understand *p* even if *p* is not true.

Catherine Elgin (2007) argues that "a factive conception [of understanding] is too restrictive" (p. 33).[157] In the case of knowledge, if *S* knows that *p*, then *p* is true. When it comes to understanding, however, Elgin argues that *S* may understand *p* even if *p* is not true. She does not deny that we use the term 'understanding' in several ways, some of which are factive. For example, we often use the term 'understanding' as a device for hedging, as when we say things like "I understand that she is not going to show up," thereby indicating that I am not certain but have good reasons to believe that this is so. So

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[155] See, e.g., Toulmin (1961); Salmon (1993); Schurz & Lambert (1994); Weber (1996). Cf. Kosso (2007). Like other philosophers of science, Kosso does not deny the factivity of scientific understanding. Rather, he argues that "Knowledge of many facts does not amount to understanding unless one also has a sense of how the facts fit together" (p. 173). Though I am not sure why "having a sense of how the facts fit together" does not amount to knowing more facts about the relations (e.g., inferential, structural, causal, etc.) that hold between those facts.

[156] See, e.g., Zagzebski (2001); Kvanvig (2003); Elgin (2007).

[157] In what follows, I focus on Elgin's argument in particular because it is the most relevant to the question of scientific progress, as Elgin herself argues (2001), and because Elgin relies on examples from science in particular to make her case for non-factive understanding.

Elgin does not deny that there are factive uses of the term ‘understanding’. But these are not the uses she is interested in. Elgin (2007) is “concerned with cases in which understanding is a sort of cognitive success” (p. 34). In such cases, if *S* understands *p*, then *S* has a claim to epistemic entitlement. Her “argument is that a factive conception cannot do justice to the cognitive contributions of science and that a more flexible conception can” (Elgin, 2007, p. 34). As Elgin explicitly talks about scientific progress and draws her examples from science, I will focus on her arguments, in particular the argument from scientific idealizations.

According to Elgin (2007), a factive conception of understanding “does not reflect our practices in ascribing understanding and it forces us to deny that contemporary science embodies an understating of the phenomena it bears on” (p. 33). More importantly, Elgin argues that the notion of understanding as factive doesn’t do justice to contemporary science. She supports this claim by advancing examples of idealizations in science that, as she argues, do not mirror the facts. In particular, her primary example is the Ideal Gas Law. If Elgin is right, then the kind of cognitive success we attribute to scientists when we say that they understand a certain phenomenon (e.g., the behavior of gases) does not involve attributing success in terms of gaining knowledge about a particular domain in nature.

Why does Elgin (2007) think that, “for an epistemology that seeks to accommodate science, an explication that construes ‘understanding’ as nonfactive is more serviceable than one that construes it as factive” (p. 34)? This has to do, I think, with her argument from idealizations in science. But before I examine this argument, a clarification is in order. According to Elgin, the objects of understanding are not individual propositions, as in the case of knowledge, but rather “bodies of information.” As Elgin (2007) writes:

> Understanding is primarily a cognitive relation to a fairly comprehensive coherent body of information. The understanding encapsulated in individual propositions derives from an understanding of larger bodies of information that include those propositions (p. 35).

This seems to correspond to Kvanvig’s distinction between “objectual understating” and “propositional understanding.” The former is the sort of understanding that is claimed for a certain object, such as a particular subject matter, whereas the latter is understanding that something is the case. For Kvanvig, both objectual and propositional understanding require that *S* successfully grasp how *S*’s beliefs in the relevant propositions cohere. As Kvanvig (2003) writes:

> Objectual understanding is, of course, not straightforwardly factive, for only propositions can be true or false. Still, the uses I wish to focus on are ones in which factivity is in the background. For example, to understand politics is to have beliefs about it, and for this objectual understanding to be the kind of interest here requires that those beliefs be true (p. 191).

According to Kvanvig, then, for *S* to understand a subject matter, *S* must have true beliefs about that subject matter.[158] Elgin argues that understanding is not factive in this sense. She says that the notion of understanding she is concerned with must have some fidelity to the facts. The question is, however, how it must do so. “Following Plato,” Elgin (2007) writes, “let us call the required connection, whatever it may be, between a comprehensive, coherent body of information and the facts it bears on an understanding’s tether” (p. 35). But she disputes the thesis that understanding is factive in the sense that Kvanvig defends.

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[158] In a recent reply to critics, Kvanvig (2009) seems to suggest that, in the case of objectual understanding, perhaps most of the propositions and all of the central propositions that constitute *S*’s coherent take on that subject matter must be true. Perhaps a few peripheral propositions might be false, and *S*’s understanding might thereby be degraded, but not destroyed. In that case, objectual understanding is quasi-factive.

As Pritchard (forthcoming) points out, “when understanding is of a proposition, it does seem to be pretty much synonymous with knows” (p. 11). He then says that he takes the “paradigm usage of ‘understands’ to be in a statement like ‘I understand why such-and-such is the case’” (p. 11), and goes on to distinguish between “holistic understanding,” which applies to subject matters, as in “I understand quantum physics,” and “non-holistic understanding,” which is understanding why such-and-such is the case, as in “I understand why my house burned down” (p. 12). As Pritchard (forthcoming) points out, philosophers of science take understanding of this sort, i.e., “non-holistic” understanding-why, to be a species of knowledge. For example, “I understand why my house burned down” is equivalent to “I know why my house burned down, which is in turn tantamount to “I know that my house burned down because of faulty wiring” (p. 12). Like epistemologists, philosophers of science take coherence to be essential to understanding. For instance, Toulmin (1961) argues that “The business of science involves more than the mere assembly of facts: it demands also intellectual architecture and construction” (p. 108). Unlike epistemologists, however, it seems that philosophers of science take understanding to be a species of knowledge because they construe understanding in the sense of ‘understanding why such-and-such is the case’. Indeed, it seems reasonable to think that all genuine—as opposed to apparent—understanding is also knowledge. To understand why something is the case is to know what causes, processes, or laws brought it about. For example, consider the following exchange:

Q: Did Fabricius know about the valves in the veins?

A: He knew about the valves in the veins but, unlike Harvey, he didn’t understand their function.

Q: Why did Fabricius fail to understand the function of the valves?

A: Unlike Harvey, Fabricius didn't know that the function of the valves is to make blood flow in one direction.

Q: Did Fabricius have an idea about what the function of the valves might be?

A: Yes. But he was wrong to think that the valves obstruct the flow of blood, so that the veins wouldn't rupture.

This example seems to illustrate that genuine understanding-why is a species of knowledge, and hence factive. And that brings me back to Elgin's argument.

Elgin (2007) contends that "the cognitive achievements embodied in [scientific] theories should be a central concern for epistemology" (p. 38). Epistemology, Elgin argues, should explain what makes current scientific understanding better. Even if we accept Kvanvig's proposal that these uses of understanding should be considered as honorific (Kvanvig, 2003, p. 201), we should still explain why certain topics merit certain attributions of honor. The factive conception of understanding will not do in this regard, according to Elgin (2007), because of "science's penchant for idealization" (p. 38). As Elgin (2007) writes:

> Science streamlines and simplifies. It devises and deploys comparatively austere models that diverge from the phenomena it seeks to explain. The ideal gas law accounts for the behavior of gases by describing the behavior of a gas composed of dimensionless, spherical molecules that are not subject to friction and exhibit no intermolecular attraction. There is no such gas. Indeed, there could be no such gas. Nonetheless, scientists purport to understand the behavior of actual gases by reference to the ideal gas law (p. 38).

This is the argument I would like to examine closely. Does it show that scientific understanding, i.e., the cognitive success that we attribute to scientists, is not factive?

To answer this question, some terms must be clarified. First, what is the Ideal Gas Law? The Ideal Gas Law is based on the following gas laws, which are considered special cases of the general law:

> Boyle's Law: The volume (V) of a given mass of gas at a constant temperature (T) is inversely proportional to its pressure (p). $pV = constant_B$
>
> Charles' Law: The volume (V) of a given mass of gas at a constant pressure is directly proportional to the absolute temperature. $V = constant_C\ T$
>
> Avogadro's Law: At a given temperature and pressure, equal volumes of gas contain equal numbers of moles. $V = constant_A\ n$

For a given mass of gas kept at constant volume, the pressure law states that the pressure is directly proportional to the thermodynamic temperature. These gas laws can be combined in the Ideal Gas Law: $pV = nRT$, where n is the number of moles, and R is the universal molar gas constant.

Second, what is an ideal gas? An ideal gas (or a perfect gas) is a gas that obeys the gas laws. It is supposed to consist of molecules that occupy negligible space and have negligible forces between them. All collisions that occur between molecules and the walls of the container or between molecules and other molecules are supposed to be perfectly elastic, because the molecules have no means of storing energy except as translational kinetic energy. To derive the Ideal Gas Law, scientists generally assume the following:

(GL1) Gas molecules are moving in random directions in the sample. If, for some reason, the gas molecules were all moving strictly up and down (or from left to right) in the container, then the law would not hold.

(GL2) Interactions between gas molecules can be neglected. In some gases, there are unusually large attractions or repulsions between molecules. For the most part, however, the average distance between molecules in a typical gas sample is large enough to be neglected in the calculations.

(GL3) The average energy of gas molecules is proportional to the temperature of the gas.

So, does the Ideal Gas Law describe the behavior of ideal gases or real gases? It seems to me that framing the question in this way is a bit misleading. In some sense, it seems that there are no “ideal gases” and “real gases.” There are just gases. But since scientists themselves talk in terms of “real gases” and “ideal gases,” I will follow this terminology. But notice that an ideal gas is, by definition, a gas that obeys the gas laws. This seems to suggest that scientists are aware that there are deviations from the laws, i.e., that there are gases that do not obey the gas laws under certain conditions.[159]

It is important to keep in mind that the Ideal Gas Law is the simplest equation that relates the pressure p, volume V, and thermodynamic temperature T of an amount of substance n. The equation $pV = nRT$ does not take into account the volume occupied by the gas molecules and any forces between the molecules. However, there are more accurate equations of state, such as $(p + k)(V - nb) = nRT$, where k is a factor that reflects the decreased pressure on the walls of the container as a result of the attractive forces between particles, and nb is the volume occupied by the particles when the pressure is infinitely high. The behavior of gases usually agrees with the predictions of the Ideal Gas

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[159] See, e.g., Pan, et al (1998).

Law to within ±5% at normal temperature and pressures (Bonder, 2004).[160] At low temperatures or high pressures, gases deviate more significantly from the Ideal Gas Law. When that happens, scientists can do one of the following. First, when gases deviate from the ideal behavior predicted by the Ideal Gas Law at states near the saturation region and the critical point, scientists can account for this deviation at a given temperature and pressure by introducing a correction factor known as the "compressibility factor," which is defined as $Z = pV/RT$. Second, they can use more accurate equations of state, like the van der Waals equation. This equation explains the deviations from the Ideal Gas Law and it describes the behavior of gases over a wider range of temperatures and pressures.

According to Elgin (2007), "There is no expectation that in the fullness of time idealizations will be eliminated from scientific theories" (p. 38). I think that Elgin rightly says that an expectation to fully eliminate idealizations from theories seems to be impracticable. However, it seems to me that there is an expectation to make idealizations more accurate and precise, i.e., to have them describe the intended domain in nature to a greater approximation. It seems that the case of van der Waals illustrates this point. He knew that the kinetic molecular theory assumes that gas particles occupy a negligible fraction of the total volume of the gas. He realized that this holds at pressures close to 1 atm. As the gas is compressed, however, this is no longer the case. Real gases are not as compressible at high pressures as ideal gases. The volume of a real gas is larger than what the Ideal Gas Law predicts at high pressures. He then proposed that subtracting a term from the volume of the real gas before it is substituted in the ideal gas equation should account for this fact. He then introduced a constant (b) into the ideal gas equation that was equal to the volume actually occupied by a mole of gas particles. The term that

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[160] Available at <http://chemed.chem.purdue.edu/genchem/topicreview/bp/ch4/deviation.php#analysis>.

is subtracted from the real volume of the gas is equal to the number of moles of gas multiplied by b (because the volume of the gas particles depends on the number of moles of gas in the container). So, when the pressure is relatively small and the volume is high, the nb term is too small to make a difference in the calculation. At high temperatures, however, when the volume of the gas is small, the nb term accounts for the deviation from the ideal gas equation.

Indeed, van der Waals also knew that, according to the kinetic molecular theory, the force of attraction between gas molecules is zero. He realized, however, that there is a small force of attraction between gas molecules that tends to hold the molecules together. This attraction accounts for the way gases condense to form liquids at low temperatures and for the fact that the pressure of a real gas is sometimes smaller than what the ideal gas equation predicts. So, to correct the deviations from the ideal gas equations (i.e., that the pressure of a real gas is smaller than what the equation predicts), he added a term to the pressure in the equation. He introduced a second constant (a), and thus he got the following equation: $(p + an^2/V^2)\ (V - nb) = nRT$.[161]

According to Elgin (2004), idealizations in science are fictions or "felicitous falsehoods" (p. 116). As Elgin (2007) writes:

> The ideal gas is a fiction that exemplifies features that exist, but are hard to discern in actual gases. The idealization affords epistemic access to those features, and enables us to explore them and their consequences by prescinding from complications that overshadow the features in real cases. It is valuable because it equips us to recognize these features, appreciate their significance, and tease out subtle consequences that might be obscured in the welter of confounding factors that obtain in fact. It serves as a focus that facilitates indirect comparison, where direct comparisons are unilluminating or intractable. *We understand the phenomena in terms of their deviations from the ideal*. Such idealizations are not true, do not purport to be true, and do not aspire to be replaced by truths. But it is hard to deny that they are cognitively valuable (pp. 40-41, my emphasis).

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[161] See Dilworth (2007), chap. 10. See also Losee (2004).

By way of illustration, Elgin considers the case of Copernicus and the heliocentric model. She argues that it is a problem for the Copernican model that the orbit of the Earth is not circular. Such understandings, which contain propositions that are false, are in need of improvement. As Elgin (2007) puts it, “They are just way stations toward a better understanding of the subjects they concern” (p. 41). On the other hand, she argues that it is not a problem for the Ideal Gas Law that there are no ideal gases. Such idealizations do not require improvement. Unlike false propositions embedded in understandings, idealizations like the Ideal Gas Law “are not mere way stations to something better” (Elgin, 2007, p. 41).

I would like to suggest that the case of the gas laws is an instance of cognitive success, not because it is a “felicitous falsehood,” as Elgin argues, but because it is an instance of describing a given natural phenomenon to a greater approximation. The history of the gas laws, and in particular, the case of van der Waals, seems to show that idealizations do need improvement, and often are improved. It seems that we would not attribute cognitive success, and scientists would not say that they understand the behavior of gases, unless we (and they) thought that their approximations are getting more and more accurate (i.e., that they are describing the behavior of gases with greater accuracy). In that respect, idealizations are “way stations to something better,” but not merely so.

To see why, consider the heliocentric model again. Why does Elgin think that it is a problem for the heliocentric model that planetary orbits are not circular, but it is not a problem for the Ideal Gas Law that there are no ideal gases? The heliocentric model is supposed to model a certain phenomena, namely, planetary orbits, just as the Ideal Gas Las is supposed to model a certain phenomena, namely, the behavior of gases under certain conditions of temperature and pressure. What is the difference between the two? According to Elgin (2002), “The difference is plain” (p. 21). As Elgin (2002) writes:

> That there exists no ideal gas does not discredit the ideal gas law. But that there exists no phlogiston decisively discredits the laws of phlogiston theory. The difference is plain. *The ideal gas law is a fiction. So its falsity does not tell against it*. Since phlogistic laws purport to be factual, their falsity is their undoing (p. 21, my emphasis).

This difference doesn’t seem so plain to me, however. On the one hand, if by saying that the Ideal Gas Law is false Elgin means that the equation that relates pressure, temperature, and volume of gases is wrong, i.e., that every time the equation is applied to actual gases it yields the wrong results, then that is not true. There are some gases (though not all gases) that obey the Ideal Gas Law, and since an ideal gas is, by definition, a gas that obeys the gas laws, it follows that there are ideal gases. On the other hand, if by saying that the Ideal Gas Law is false, Elgin means that the assumptions made to derive the law are wrong, i.e., (GL1)-(GL3), then I don’t see how it follows that the whole thing is false. The Ideal Gas Law doesn’t give us the truth, the whole truth, and nothing but the truth about the behavior of gases. Rather, it tells a partial, true story about the behavior of gases. Admittedly, there are deviations from ideal-gas behavior. But scientists know that and, presumably, the law is not meant to be universal in scope.[162]

Apparently, then, if it is a problem for the heliocentric model that planetary orbits are not circular, it would be a problem for the Ideal Gas Law if there were are no ideal gases. Indeed, it seems that if Ptolemaic astronomers were to introduce more epicycles to their model in order to “save the phenomena,” we wouldn’t say that their understanding

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[162] According to Cartwright (1999), theoretical statements are not meant to have universal scope; there are no laws or principles that hold across the board and across all domains of inquiry. But this doesn’t rule out “a variety of different kinds of knowledge in a variety of different domains across a range of highly differentiated situations” (p. 23).

of the world thereby improved. We wouldn’t say that, it seems, precisely because scientific understanding is factive. Since there are no epicycles the way the geocentric model says there are, then accounting for the phenomena in terms of such epicycles in not an instance of the cognitive success we call “understanding.” Similarly, if there were no ideal gases, i.e., no gases that obey the gas laws, we wouldn’t say that scientists understand the behavior of gases. We wouldn’t attribute understanding to them because, if they were to try to describe the behavior of gases by making predictions based on the gas laws, they would get things wrong most of the time. The laws would predict one thing and gases would do another. If this were so, then it seems that this situation would be more aptly characterized as an instance of failure rather than success.

Accordingly, it seems that if we deny that scientific understanding is factive, we cannot attribute to scientists the kind of cognitive success or epistemic entitlement that is the mark of the sort of understanding in question. But if understanding is factive, then we can attribute this sort of cognitive success to scientists when they employ idealizations, such as the Ideal Gas Law, precisely because they mirror the facts to some approximation. That is to say, in the case of the Ideal Gas Law, it is precisely because of the agreement between the predictions of the gas laws and the behavior of gases (±5% margin of error in the case of the Ideal Gas Law) that we attribute cognitive success to scientists in this case. Otherwise, it seems, we would say that scientists have no idea how gases behave.

To this it might be objected that these idealizations are not *strictly* true, i.e., they do not describe the behavior of gases with absolute accuracy across the board. In reply, I wish to point out that the intended domain of the equations must be kept in mind. Of course, if scientists were to claim that all gases obey the gas laws under all circumstances without any exceptions, and thus they understand the behavior of all gases, then that would be incorrect. But that is not what they say. They are aware of the fact that most gases obey the Ideal Gas Law only under certain conditions (i.e., relatively normal ranges of temperature and pressure). Under more extreme conditions of low temperatures or higher pressure, there are deviations from ideal-gas behavior. And that is precisely why more sophisticated equations were proposed by scientists like van der Waals. So, if anything, this seems to suggest that their understanding is getting better because they understand the behavior of gases in terms of the gas laws as well as the conditions under which the gas laws hold true.

To illustrate this point, consider an analogous, and more familiar, case. Strictly speaking, Newton's laws of motion are not true. However, they are still very good approximations when it comes to middle-sized objects traveling at speeds not too close to the speed of light and close to the surface of the Earth. As Bird (2007) points out, "Newton's laws provide a good approximation for middle-sized dry goods travelling at moderate speeds within the Solar System and not too near the Sun" (p. 81). When we restrict the domain in this way, Newton's laws of motion are correct and extremely useful as well. Indeed, this is how we send satellites into orbit, by using essentially Newtonian mechanics to make our calculations. That is to say, our orbital mechanics for artificial satellites is essentially Newtonian. So, physicists used to think that Newton's laws of motions were universal (i.e., that they applied to all bodies under all conditions everywhere). But then they realized that these laws hold only under certain conditions of velocity, acceleration, proximity to massive objects, and so on. If this is correct, then it seems reasonable to say that physicists had a limited understanding of motion then, whereas now their understanding has improved a great deal, partly due to learning new facts about the conditions under which certain laws hold. This understanding is factive, since statements of the form ‘System *s* is a Newtonian system (i.e., it obeys the laws of Newtonian mechanics) under conditions *C*’ are true. Similarly, we can attribute to scientists who use the Ideal Gas Law understanding that is factive because the objects of their understanding are statements of the form ‘Gas *g* is an ideal gas (i.e., it obeys the Ideal Gas Law) under conditions *C*’ (where *C* specifies conditions of temperature, pressure, and the like) and ‘Gas *g* deviates from ideal-gas behavior under conditions *C**’.

Kitcher makes a similar point about restricted generalizations. If we construe a statement about the double-helical structure of DNA as a strictly universal generalization, then it will likely be false. Molecular biologists make use of single-stranded DNA artificially and ssDNA also occurs naturally under certain circumstances (e.g., replication). The fact that there are such “counterexamples,” however, is not at odds with the intended interpretation of the statement about the structure of the DNA molecule. The statement is meant to be interpreted “as a *restricted* generalization about the DNA molecules typically found in cells in nature throughout most of the phases of the life of the cell” (Kitcher, 1993, p. 118). The same point, I suggest, applies in the case of the gas laws. We simply have to bear in mind that the laws are not meant to be strictly universal and hold under all conditions. Rather, we must be aware of “the limits and margins of error within which the [law] holds true” (Bird, 2007, p. 81). Indeed, overall understanding seems to get better when we know the scope of scientific laws.

Realizing that certain laws apply only within certain ranges contributes to overall understanding, not because the laws themselves get things approximately right, but because the objects of the scientists' understanding are recognized as approximations. In other words, the object of scientific understanding is not a body of information about a particular subject matter, as Elgin argues. Rather, the objects of scientific understanding are statements about the state of affairs to which the law is an idealization (or approximation). If the cognitive object is something that acknowledges the idealization or approximation (the "boundary conditions"), then that object is true, and hence understanding, in this case, is factive. To say that the object of understanding is an idealization or approximation is not to say that it is a "felicitous falsehood." Rather, it is to say that the conditions under which the law holds true must be specified. Bird (2007) shows how this could work as follows:

> Let $<p_1,\ldots, p_k>$ be a series of hypotheses, accepted in that order over time, that monotonically get closer to the truth. […] We can see that there is also a distinct but related sequence of propositions, entailed by the first, that exhibits the accumulation of truth and (potentially) the accumulation of knowledge. Let $A(\ldots)$ be a propositional operator whose meaning is given thus: $A(p)$ iff approximately p. Assuming first, for simplicity, that all the p_i are approximately true, the sequence of propositions $<A(p_1),\ldots, A(p_k)>$ will be a sequence of propositions each of which is fully true and adds to the truth provided by its predecessors (p. 77).

According to Bird (2007), this shows that the "improving precision of our approximations can be an object of knowledge," for "where there is accumulation of verisimilitude there is also accumulation of truth" (p. 77). Similarly, I suggest, the improving precision of our idealizations can be an object of (factive) understanding. Just as truths about approximations are still truths (i.e., statements of the form 'approximately *p*’ can be fully true), facts about idealizations are still facts (i.e., statements about ‘the state of affairs to which law *L* is an idealization’ can be fully true).

This kind of understanding is the kind that is typical of science because it is tied to explanation. According to Wesley Salmon (1998), “we have *scientific understanding* of phenomena when we can fit them into a general scheme of things” (p. 87, original emphasis). Salmon argues that scientists are able to fit phenomena into a general scheme of things when they uncover the causal connections between them. For example, in Chapter 2 we have seen that the likes of Pasteur and Koch were credited with advancing scientific knowledge for uncovering the causes of infectious disease. The fact of contagion, however, was known long before Pasteur and Koch. Indeed, in his *History of the Peloponnesian War* (431 B.C.E.), Thucydides (c. 455-400 B.C.E.) reports on a plague that broke out during the war and recounts how healthy Athenians “caught the infection in nursing each other” (Book 2, Chapter VII), and how “even the previously healthy soldiers [caught] the infection from Hagnon’s troops” (Books 2, Chapter VII).[163] So, one might argue, Thucydides knew about infection, but his knowledge stops there, and he didn’t understand how or why the disease was passed from one person to another. As Longrigg (1998) puts it:

> Thucydides certainly observes and records the *fact* of contagion. However, this is not to say that he clearly enunciated the *doctrine* of contagion or possessed an ‘*understanding* of contagion and immunity’ or had any conception at all of its true cause (p. 126, original emphasis).

If we go a step further, however, and ask why Thucydides failed to understand contagion, the most reasonable answer seems to be that he didn’t know the “true cause.” Not

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[163] Available at <http://ebooks.adelaide.edu.au/t/thucydides/crawley/index.html>.

knowing that the cause is microorganisms or germs, he failed to understand how healthy humans can catch a disease by being around those who are sick.[164]

As Elgin (2007) points out, "The ideal gas law lies at the core of statistical mechanics, and some such law is likely to lie at the core of any successor to current theories" (p. 38). However, gas laws have undergone several transmutations since the time of Boyle, Mariotte, Charles, Gay-Lussac, and even van der Waals (e.g., the Beattie-Bridgeman equation and the Benedict-Webb-Rubin equation). These changes improved the gas laws insofar as they now describe the behavior of gases with greater accuracy. Increasing accuracy and improving precision of approximations and idealizations can be the objects of (factive) understating, since what is understood is an approximation or idealization (or the state of affairs to which the law is an idealization). That is to say, the cognitive object of scientific understanding is acknowledged by scientists themselves as an approximation. But that means that the cognitive object itself is factive.

We must keep in mind, however, as scientists often do, the intended domain of the gas laws. Is it true that all gases obey the gas laws under any circumstances? No. However, it is true that the behavior of gases usually agrees with the predictions of the Ideal Gas Law to within ±5% at normal temperature and pressures. And it is also true that the van der Waals equation is doing even better when it comes to high pressures. That is why, I suggest, scientists claim that they understand the behavior of gases. And that is also why we think that they have achieved this sort of cognitive success. If this is correct, then it seems that Elgin's argument from scientific idealizations fails to show that scientific understanding is not factive, since it mischaracterizes scientific understanding, and it fails to take into consideration the fact that the cognitive objects of scientific

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[164] Cf. Kosso (2007).

understanding are approximations and that understanding can improve by finding the limits and margins of error within which laws hold true. If this is correct, then scientific understanding is grounded in facts because scientists acknowledge that the laws they use are approximations, they refine them by learning more about the conditions under which they hold true, and they actively seek more accurate equations. If this is correct, then philosophers of science are justified in taking scientific understanding to be a species of knowledge, given that scientific understanding, just like scientific knowledge, is factive.

The apparent disagreement between epistemologists and philosophers of science about the nature of scientific understanding, it seems to me, is rooted in the fact that philosophers of science construe scientific understanding as an externalist notion, whereas epistemologists construe understanding as an internalist notion. For philosophers of science, on the one hand, the objects of understanding are natural phenomena. For instance, according Schurtz and Lambert (1994), scientific understanding is "an intersubjective (objective) notion and independent of the psychological features of given persons" (p. 66).[165] For Schurtz and Lambert (1994), "To *understand* a phenomenon *P* is to know how *P* fits into one's background knowledge" (p. 66, original emphasis). Schurtz and Lambert (1994) explicate this notion of understanding as follows:

> To understand means to be able to give an answer to an *understanding seeking how-question*, namely, "How does *P* fit into the cognitive corpus *C*?" An explanation seeking why-question—"Why *P*?"—can also be *implicitly* an understanding seeking question because an answer of the form "Because *X*" shows implicitly how *P* fits into *C*. But it is only an answer to the above how-question which *explicitly describes* how *P* fits into *C*, for example, by stating that *P* is inferable from *X*. The basic idea, then, can be regimented as follows: A sentence *A* yields understanding of *P* if and only if it answers the question, "How does *P* fit into *C*?" (p. 67, original emphasis)

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[165] In other words, scientific statements are intersubjectively testable, i.e., different researchers should be able to obtain evidence for a statement. See Piccinini (2003).

Note that a phenomenon is taken as a nonlinguistic entity, which may or may not obtain (state of affairs), and that can be expressed by a declarative statement (which may be singular or general, true or false). Hence, we may talk rather loosely about how phenomenon *P* fits into *C*, but what is meant is that the statement expressing *P* fits into *C* (Schurtz & Lambert, 1994, p. 68). As Schurtz and Lambert (1994) put it, "the intended objects of scientific [understanding] are not primarily scientific statements or other linguistic structures, but the *real phenomena*" (p. 73, original emphasis). This gives us a notion of scientific understanding that is not only factive but also progressive in a straightforward way. For "understanding is a feature of the development of *C* into *C** (*C* → *C**)" (Schurtz & Lambert, 1994, p. 68).

For epistemologists, on the other hand, "the view of understanding outside of epistemology such that understanding is a species of knowledge is false" (Pritchard, forthcoming, p. 17) because "understanding is construed along epistemically internalist lines" (Pritchard, forthcoming, p. 12) and the objects of understanding are subject matters, or whole theories in the case of science, as in "I understand quantum physics" (Pritchard, forthcoming, p. 12). It seems, then, that epistemologists take the objects of understanding to be scientific theories, rather than natural phenomena, as philosophers of science do. But perhaps scientific theories are sometimes the objects of scientific understanding. As Feynman (1965) once said, "I think I can safely say that nobody understands quantum mechanics" (p. 129). That seems to suggest that scientists are trying to understand not only natural phenomena but also their theoretical representations. If this is so, then perhaps this kind of understanding should be construed as objectual understanding.[166]

However, we have already seen why we should reject this kind of holism about scientific theories. Given that our main concern here is scientific progress, such a construal of scientific understanding might open the door to skeptical attacks of the pessimistic sort. As we have seen in Chapter 5, the Pessimistic Meta-Induction assumes an implicit holism about scientific theories. So, if we construe scientific theories as the objects of objectual understanding, where there could be some propositions in the periphery that are false, but those at the core are true, then the pessimist might argue that, since the theory is not strictly true, then the whole thing is false. If we wish to avoid this kind of pessimism about scientific progress, however, I think we should resist this kind of holism about theories. To do so, I suggest that we focus on individual claims to knowledge as opposed to whole theories. Statements (or propositions) are the proper objects of knowledge as well as understanding, since understanding is a species of knowledge (both are factive). If this is correct, then scientific progress consists in the accumulation of those statements that qualify as knowledge, not only of the theoretical (inferential) variety, but also of the empirical (factual), practical, and methodological variety. As for quantum mechanics, I suppose many physicists find the scientific understanding that it affords less than satisfactory precisely because there is a great deal

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[166] In response to Elgin, Kvanvig (2009) writes, “The issue here concerns the object of understanding. One might understand the model or theory itself, as when one understands phlogiston theory. One does not thereby understand combustion, however. Understanding the world scientifically is not simply a matter of understanding the given model but involves, rather, some relationship between the model and reality. Scientific understanding of the sort in question consists in the possession of a model and a realization of the extent to which it is an idealization and what aspects of reality the model is intended to shed light on” (p. 342).

about the theory and its applications that we still don’t know (Schurtz & Lambert, 1994, pp. 103-107).

7.b. Concluding Remarks

Before I settled on philosophy, I was studying computer science. We used to tell the following anecdote about a logic professor whose logic class was attended by both computer scientists and mathematicians. One time, the professor lined up the boys at one end the classroom and the girls at the other end and told them that once every minute they could move forward exactly half of the remaining distance between them. When the professor said “go,” all the mathematicians stayed put because they knew they would never get there, but all the computer scientists stepped forward because they knew they would get close enough to make it count.

If my arguments are successful, then scientists in general (at least in the biosciences) are more like computer scientists than mathematicians.[167] That is to say, scientific knowledge counts as knowledge if we grant that the objects of scientific knowledge are individual statements with a restricted domain of application, rather than whole theories, and that it is fallible insofar as it is based on non-deductive arguments. Not only theoretical (inferential) knowledge counts as scientific knowledge but also empirical (factual), practical, and methodological knowledge. The accumulation of each of these types of knowledge counts as scientific progress because, in science, ‘knowing

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[167] Although, if I am right, then progress in science overall is more continuous and cumulative like progress in mathematics. According to Dauben (1992), “Like the microscopist, moving from lower to higher levels of resolution, successive generations of mathematicians can claim to understand more, with a greater stockpile of results and increasingly refined techniques at their disposal. As mathematics becomes increasingly articulated, the process of resolution brings the areas of research and subjects for problem-solving into greater focus, until solutions are obtained or new approaches developed to extend the boundaries of mathematical knowledge. Discoveries accumulate, and some inevitably lead to revolutionary new theories uniting entire branches of study, producing new points of view, sometimes wholly new disciplines that would have been impossible to produce within the bounds of previous theory” (p. 63).

how’ is just as significant as ‘knowing that’. That is how scientists evaluate progress, and that is how philosophers of science (at least those who are naturalists) should evaluate progress as well.

Appendix
The Analysis of Knowledge

The traditional analysis of knowledge is known as the JTB account, which analyzes knowledge as justified true belief. According to this account, *S* knows that *p* if and only if:

(B) *S* believes that *p*
(T) *p* is true
(J) *S* is justified in believing that *p*

According to the JTB account, then, *S* knows that it is raining, for instance, if and only if it is true that it is raining, *S* believes that it is raining, and *S* is justified in so believing.

In "Is Justified True Belief Knowledge?" Edmund Gettier (1963) advances two counterexamples against the JTB account. Each case involves (or at least intuitively seems to involve) instances of justified true belief that nonetheless fail to be instances of knowledge. Although it seems that most epistemologists have accepted that Gettier-like cases (at least those that do not involve a false lemma) are genuine counterexamples to the JTB account (i.e., they are genuine examples of situations in which the questions "Does *S* know that *p*?" and "Does *S* have a justified true belief that *p*?" have different answers, and thus show that the JTB account of knowledge is either wrong or incomplete), it is not clear what is the lesson to be learned in the wake of Gettier's seminal paper.[168]

So what have we learned from Gettier's cases and the deluge of literature that followed the publication of his article? Well, it seems that there is no clear answer to this question. In the aftermath of Gettier's article, it seems that epistemologists have taken

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[168] See Shope (1983).

almost every conceivable position with respect to the analysis of knowledge. Here is how Frank Jackson (2002) describes the situation:

> What did we learn from the Gettier incident and its fall-out? A few say that we learnt nothing. Knowledge is true justified belief despite Gettier's and like examples. Some say that we learnt that knowledge is not true justified belief and we must seek a fourth condition to add to truth, belief and justification. Perhaps they have a candidate ready to hand. Some say that we learnt that knowledge is not true justified belief but that Gettier's examples mislead us. We should not look for a fourth condition but should rather look for a suitable replacement for the justification condition. Some say that we learnt as much from the fall-out as we did from Gettier's examples. His examples prompted a search for an analysis of knowledge that can only be described as failure. This tells us that there is no unitary concept of knowledge but rather a series of interlocking and to some extent vague notions, each of interest and value in its own way. Finally, some agree that we learnt as much from the fall-out as from the examples but urge that what we learnt is that our single concept of knowledge is unanalysable (p. 516).

This messy state of affairs might lead one to think that the concept of knowledge does not admit of a conceptual analysis in terms of necessary and sufficient conditions.

According to Timothy Williamson (2000), Gettier's examples taught us that our single concept of knowledge is unanalyzable. We can construe the claim that the concept of knowledge is unanalyzable in at least two ways. In its weak sense, it is the claim that there is no unitary concept of knowledge, but rather a series of interconnected, and to some extent perhaps vague, notions. In other words, there is no single notion that underlies our ordinary use of 'knowledge'. In its strong sense, it is the claim that there is a single notion of knowledge. However, it is unanalyzable, like many other unanalyzable notions. It seems that Williamson argues for the strong rather than the weak sense. He argues that knowledge is a key concept in epistemology. For Williamson, knowledge does important theoretical and explanatory work that cannot be done by justified true belief or any other variant. Knowledge has an explanatory and predictive value that belief lacks. For example, lengthy persistence is better explained by initial knowledge than by initial true belief. If I know that there is a beer in the refrigerator, I am more likely to look longer for it than if I merely believe, even truly or justifiably, that there is a beer in the fridge.

Another important thesis Williamson argues for is the thesis that evidence is what we know. He expresses this thesis as follows: E = K. For Williamson (1997), every item of evidence is something we know, sometimes knowledge that things seem so and so (p. 718). Belief and knowledge, for Williamson, are both mental states. But belief is a mental state that aims at knowledge, not merely truth. If Williamson is right about this, then it seems that we use the concept of belief to describe the mental state we are in whenever we try to know something but fail.

None of the aforementioned considerations amounts to denying that knowledge entails truth. Indeed, Williamson accepts that knowledge entails truth and that knowledge and belief are mental states, but he does not think that we can come up with a conjunctive analysis of knowledge with belief as one of the conjuncts. He also insists that there are differences between truth-seekers and knowledge-seekers. As William James (1897) pointed out, human cognition is characterized by two distinct “imperatives”: “Believe truth!” and “Shun error!” (p. 18). A truth-seeker who follows the former but not the latter might satisfy her desire for true beliefs by venturing a guess. In other words, in the absence of considerations pointing one way or another, a truth-seeker should believe at random as many things as she can because that will maximize true belief. Indeed, she might even believe contradictions and thereby guarantee at least one true belief. However, if we take the aim of belief to be knowledge rather than truth alone, it seems that we can easily account for these aspects of human cognition and belief-forming behavior. A knowledge-seeker is *ipso facto* a truth-seeker, because knowledge entails truth. But a knowledge-seeker is also a falsehood-avoider, for a belief-forming mechanism that might have easily generated a false belief rather than a true one is not a reliable belief-forming mechanism, and hence cannot yield knowledge (even when it actually generates true beliefs on occasion). When we think of knowledge as the aim of belief, it seems that we can account for this safety from error and randomness in belief-formation that is required for knowledge. As Williamson (2000) writes: “If one knows, one could not easily have been wrong in a similar case. In that sense, one’s belief is safely true” (p. 147).

I think that Williamson’s view that belief aims at knowledge is congenial to the epistemic account of scientific progress. If the aim of inquiry is knowledge, then the aim of scientific inquiry is scientific knowledge. As Bird points out, arguments for the epistemic account of progress would lend support to the view that belief aims at knowledge, and arguments for the view that belief aims at knowledge would lend support to the epistemic account. We can see why this is so by considering the difference between truth-seekers and knowledge-seekers again. According to Bird (2007):

> The aversion to falsity—the requirement of reliability that truth-seeking cannot explain alone—is explained by knowledge-seeking precisely because a mechanism that might easily have given a false belief cannot generate knowledge even when it in fact generates true belief. Randomness in belief-formation is inconsistent with belief aiming at knowledge (p. 86).

Conceiving of scientific knowledge as the aim of scientific inquiry would explain why scientists are concerned with not only gaining new knowledge but also avoiding error and correcting erroneous beliefs.[169]

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[169] See Chapter 2 of this dissertation.

As Bird (2007b) also points out, Williamson's E = K thesis provides a way to respond to skeptical arguments, such as the following:

> (OBS) all evidence is observational.
> (INF) from observational premises only observational conclusions may be rationally inferred.
> *therefore*
> (SCEP) only observational propositions can be known (p. 69).

If we adopt E = K, then (OBS) can be reformulated as follows:

> (OBS*) all knowledge is observational (p. 70).

But then it turns out that (OBS*) and (SCEP) assert the same proposition, namely, that we can only have observational knowledge. In that case, Bird argues (2007b), this skeptical argument is clearly question-begging (p. 71).

Williamson's account is by no means widely accepted among epistemologists. I think it is safe to say that most epistemologists take knowledge to be true belief plus a "third condition," which involves some sort of epistemic justification or evidence. This "third condition" has to somehow avoid the Gettier problem. According to Zagzebski (1999), for an account of knowledge to avoid the Gettier problem, the satisfaction of the "third condition" must entail the satisfaction of the truth condition (p. 104). One way to make an account of knowledge "Gettier-proof" in this way is to make attributions of knowledge sensitive to context. Recently, Ram Neta (2002) has argued for the following contextualist account of knowledge:

> (K) *S* knows that *p* if and only if *S* reasonably believes that *p* on the strength of *S*'s conclusive evidence for *p* (p. 670).

To have "conclusive evidence for *p*," according to Neta (2002), "is to have evidence for *p* that one couldn't have if *p* weren't true" (p. 670). It might appear as though (K) is inconsistent with the way I characterize scientific knowledge as fallible in Chapter 5. According to Feldman (1981), fallibilism is the claim that:

> (F1) It is possible for *S* to know that *p* even if *S* does not have logically conclusive evidence to justify believing that *p* (p. 266).

This amounts to claiming that we can know things on the basis of non-deductive arguments. So are (K) and (F1) inconsistent? That depends on what "conclusive evidence" is.

It might seem as if inductively formed beliefs must be based on inconclusive evidence. However, Neta argues that (K) does not rule out inductive knowledge. Neta (2002) gives the following example:

> I see a row of 50 switches and, adjacent to it, a row of lights. I press switch A and the light next to it turns on. Then I press switch B and the light next to it turns on. The same happens with switches C and D. On the basis of these observations, I form the belief that each switch controls the light next to it. Do I have *conclusive* evidence for this belief? That depends on what my trials have shown. If my trials *show* that each switch controls the light next to it, then I do have conclusive evidence: my trials can't *show* what isn't so. But it may seem as if my trials do not show this. But then what do they show? Do they show that switches A, B, C, and D control the lights next to them? Or do they show that switches A, B, C, and D control the lights next to them at the moments at which I pressed those switches? Or do my trials not show anything about what controls what, since the noticed correlations may be accidental? Or do my trials not show even that there are any lights or switches, or any external objects? If we do not allow that my trials show that each switch controls the light next to it, then why should we allow that my trials show anything at all about a world external to my mind? (p. 672)[170]

According to Neta, this example illustrates that the apparent inconsistency between (K) and inductive knowledge is an instance of the apparent difficulty in understanding how any knowledge of the external world is possible, where knowledge is construed in terms of (K). This is where the context-sensitivity of attributions of evidence comes in.

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[170] I think this example also illustrates how skepticism about theoretical knowledge and "unobservables" is unprincipled. See Chapter 5 of this dissertation.

What is involved in having evidence? Part of the answer, according to Neta (2002), has to do with the following:

1. For "*S* to have evidence for *p* is for *S* to have evidence that favors *p* over some alternative(s) that are relevant in the context of epistemic appraisal" (p. 673).
2. "*S* can have evidence that favors *p* over some relevant alternative *q* only if there is some difference between the way that things would be for *S* if *p* were true and the way that things would be for *S* if *q* were true" (p. 673).
3. "*S* can have evidence for *p* without knowing that *p*, without believing that *p*, and without knowing that she has evidence for *p*" (p. 673).

Accordingly, the notion of evidence in (K) is context-sensitive in the following sense:

> We can truthfully claim that *S* has evidence for *p* only if *S* is in some introspectible state that she would be in if *p* were true, but that she would not be in if relevant alternative to *p* were true. What's available to *S*'s introspection thereby indicates that *p*, rather than any of its relevant alternatives, is the case (Neta, 2002, p. 673).

What counts as evidence is relative to a context of attribution of evidence, according to Neta. Two hypotheses, h1 and h2 are "introspectively indistinguishable for *S*" if and only if:

(a) If h1 were true, then *S* would be in mental state M1, and

(b) If h2 were true, then *S* would be in mental state M2, and

(c) Any difference between M1 and M2 is not introspectively available to *S* (Neta, 2002, p. 674).

Suppose that hypothesis *H* is "an uneliminated counterpossibility" with respect to *S*'s knowing that *p* at *t* if and only if "(i) *H* implies that *S* doesn't know that *p* at *t* and (ii) *H* and '*S* knows that *p* at *t*' are introspectively indistinguishable for *S*" (Neta, 2002, p. 674). Then, "An appraiser *X* 'raises' an uneliminated counterpossibility with respect to *S*'s knowing that *p* at *t* just in case *X* (seriously and sincerely) treats that counterpossibility as relevant to the appraisal of *S*'s epistemic state, and relevant by virtue of being an uneliminated counterpossibility" (Neta, 2002, p. 674). Neta (2002) states this as a rule to add to his account of knowledge:

> (R) When one raises an hypothesis *H* that is an uneliminated counterpossibility with respect to *S*'s knowing that *p* at *t*, one restricts what counts in one's context of appraisal as *S*'s body of evidence at *t* to just those mental states that *S* has, and would have, at *t* whether or not *H* is true (p. 674).

Together, (K) and (R) give a contextualist account of knowledge that traces the context-sensitivity of knowledge ascriptions to the context-sensitivity of evidence ascriptions. On this account, inductive knowledge is possible. As Neta (2002) writes:

> *S* inductively knows that *p* just in case *S* believes that *p* on the strength of evidence that *shows that p*. But whether *S*'s evidence shows that *p* depends upon what *S*'s evidence includes. What *S*'s evidence includes is relative to a context of ascription, and so too, therefore, is the possibility of *S*'s possession of inductive knowledge (p. 674).

I find this account congenial to the epistemic account of progress because it seems to point to the way in which background knowledge guides scientific research.

Philosophers of science have long pointed out that ampliative inferences, the kind of inferences that are ubiquitous in science, are sensitive to background information and context, especially when explanations are concerned. Consider the following example, discussed by Peter Achinstein (1983). It is commonly known that if a person ingests a pound of arsenic, she will die within 24 hours or so. So, from the fact that person *A* has ingested a pound of arsenic, we may infer that she died within 24 hours from the time of ingesting a pound of arsenic. Suppose, however, that, unbeknownst to us, *A* was killed by a bus before the arsenic could take effect. So, in fact, the fact that *A* ingested a pound of arsenic doesn't explain her death. In this case, in addition to the inference, background knowledge is required in order to figure out the explanation. The point, I think, is a familiar one. Unlike deductive reasoning, non-deductive reasoning is non-monotonic, i.e., the addition of premises may annul what was initially a good inductive inference.

Some might be worried that knowledge based on inductive inferences is impossible because of the Humean problem of induction. Thus understood, Hume's problem is a problem concerning the capacity of inductive reasoning to yield knowledge. As Bird (1998) puts it:

> Hume's problem suggests that inductive knowledge is impossible. The inductive sceptic requires that, in order for an inductive argument to yield knowledge, I must be able to demonstrate that my use of induction will yield knowledge. This demonstration must itself amount to knowledge. So, in order for induction to yield knowledge, I must know that it yields knowledge (p. 216).

As Bird (forthcoming) points out, Hume's problem assumes some version of internalism. That is to say, "it assumes that in order for some method to yield knowledge, that method needs to be justified, in the sense that we must be in possession of good reasons for thinking that this method of reasoning is one that yields true conclusions when given true premises" (p. 9).[171] This may be considered as a version of internalism about justification, since the internalist would require that a method may justify our beliefs only if its capacity to do so is apparent to a sufficiently reflective subject. The externalist denies that. For the externalist, the capacity of a method to justify beliefs may depend on some feature of the world that is not accessible to the subject who is using that method.

One externalist view that may be useful in addressing this problem about inductive knowledge is reliabilism. According to reliabilist epistemology, knowledge is true belief acquired by a reliable method. On a reliabilist account of justification, inductively inferred beliefs may be justified as long as certain conditions obtain. In the case of inferred beliefs, one condition that must obtain is that the subject has sufficient

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[171] Available at <http://eis.bris.ac.uk/~plajb/research/papers/Induction.pdf>.

and relevant evidence for the inductive inferential rule to apply. The reliability of the inference would require that nature is in fact law-governed in certain respects. There is no requirement, however, that the subject be aware that these conditions about nature hold. The condition regarding nature being law-governed is external and need not play a role in the subject's reasoning. So, externalism about justification in general, and reliabilism in particular, allow us to think about inductive beliefs as justified when some external conditions hold, even if we are not aware that they do.[172]

Some may find this externalist move quite unsatisfactory and demand a justification for the belief in the reliability of inductive inferences. Indeed, we do often think that we have warranted beliefs concerning certain belief-forming processes and methods. For example, we usually take biopsy to be a more reliable process of forming beliefs about a person's medical condition than chiromancy (palm reading). However, the inductive skeptic warns that any attempt to justify use of a certain method or inductive rule of inference by relying on that same method or rule would be viciously circular.

In the spirit of externalism, or reliabilism in particular, a reply to this charge of circularity proceeds by drawing a distinction between "premise-circularity" and "rule-circularity."[173] On the one hand, premise-circularity, i.e., an inference that purports to establish the truth of *p* by using *p* itself as one of the premises, is viciously circular. On the other hand, rule-circularity is not an instance of vicious circularity. Rule-circularity occurs when we use an argument to establish a proposition concerning a rule *R* (e.g., that *R* is reliable) and the form of this argument is an instance of *R*. Unlike premise-circularity, an argument that displays rule-circularity does not contain a premise that

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[172] See Bird (forthcoming), pp. 9-12. See also Mellor (1991), chap. 15.

[173] See Braithwaite (1953).

asserts the reliability of *R*, and thus the conclusion is not among the premises. Since this is so, rule-circularity is not vicious circularity.[174] As an externalist response to the inductive skeptic, this move is especially effective because it will not do for the inductive skeptic to complain that we are tacitly assuming the reliability of *R* by employing *R*. For, according to reliabilism, the subject need not have any tacit beliefs or assumptions about the reliability of the rules that she is using. For knowledge and justification, it is usually enough that the rule is in fact reliable.[175]

In conclusion, I would like to emphasize that this appendix is by no means intended to be a comprehensive survey of analyses of knowledge. Rather, I simply presented accounts of knowledge that I find especially congenial to the epistemic account of scientific progress. It seems to me that these accounts of knowledge can serve to illuminate the notion of scientific knowledge. I also find them useful as far as addressing skeptical attacks on scientific knowledge is concerned. I prefer Williamson's account of knowledge as unanalyzable for the reasons pointed out by Bird and discussed above. Since Williamson's account is by no means generally accepted among epistemologists, however, I mentioned other accounts of knowledge that I find congenial to the epistemic account of scientific progress. I think that the epistemic account of scientific progress would work just as well with any one of the analyses of knowledge discussed in this appendix.

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[174] See Bird (forthcoming), p. 11.

[175] See Psillos (1999), p. 83. Cf. Iranzo (2008).

Bibliography

First Charter, granted to the President, Council, and Fellows of the Royal Society of London, by King Charles the Second, A.D. 1662. (1662). *Charters of the Royal Society*, from http://royalsociety.org/Charters-of-the-Royal-Society/

Second Charter, granted to the President, Council, and Fellows of the Royal Society of London, by King Charles the Second, A.D. 1663. (1663). *Charters of the Royal Society*, from http://royalsociety.org/Charters-of-the-Royal-Society/

The Nobel Prize in Chemistry 2008. (2008). *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/chemistry/laureates/2008/

The Nobel Prize in Chemistry 2008: How the Jellyfish's Green Light Revolutionised Bioscience. (2008). *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/chemistry/laureates/2008/info.pdf

Scientific Background on the Nobel Prize in Chemistry 2008: The green fluorescent protein, discovery, expression and development. (2008). *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/chemistry/laureates/2008/chemadv08.pdf

The Coastal Imaging Lab. (2009). *Argus*, from http://cil-www.coas.oregonstate.edu/

Achinstein, P. (1983). *The Nature of Explanation*. New York: Oxford University Press.

Adams, E. M. (1958). On Knowing that. *The Philosophical Quarterly, 8*, 300-306.

Alexander, F. G. (Ed.). (1956). *The Leibniz-Clarke Correspondence, Together with Extracts from Newton's Principia and Opticks*. New York: Philosophical Library.

Allchin, D. (2005). William Harvey & Capillaries. *The American Biology Teacher, 67*, 56-59.

Alspector-Kelly, M. (2004). Seeing the Unobservable: Van Fraassen and the Limits of Experience. *Synthese, 140*, 331-353.

Andrade, E. N. (1965). The Birth and Early Days of the Philosophical Transactions. *Notes and Records of the Royal Society of London, 20*(1), 9-27.

Ariew, R., & Garber, D. (Eds.). (1989). *G. W. Leibniz: Philosophical Essays*. Indianapolis: Hackett Publishing Co.

Baird, D., & Faust, T. (1990). Scientific Instruments, Scientific Progress and the Cyclotron. *British Journal for the Philosophy for Science, 41*, 147-175.

Baryshev, Y. (2008). *Practical Cosmology and Cosmological Physics*. Paper presented at the Problems of Practical Cosmology, Saint-Petersburg.

Baxby, D. (1999). Edward Jenner's Inquiry: A Bicentenary Analysis. *Vaccine, 17*, 301-307.

Ben-Menachem, Y. (1990). Empirically Equivalent Descriptions. *British Journal for the Philosophy for Science, 41*, 261-279.

Birch, T. (1756-1757). *The History of the Royal Society of London for the Improving of Natural Knowledge, from Its First Rise 1660-1687*. London.

Bird, A. (1998). *Philosophy of Science*. Montreal: McGill-Queen's University Press.

Bird, A. (2000). *Thomas Kuhn*. Princeton, NJ: Princeton University Press.

Bird, A. (2007a). What Is Scientific Progress? *Nous, 41*(1), 64-89.

Bird, A. (2007b). Underdetermination and Evidence. In B. J. Monton (Ed.), *Images of Empiricism: Essays on Science and Stances, with a Reply from Bas C. van Fraassen* (pp. 62-82). New York: Oxford University Press.

Bird, A. (2008). The Historical Turn in the Philosophy of Science. In S. Psillos & M. Curd (Eds.), *The Routledge Companion to the Philosophy of Science* (pp. 67-77). Abingdon: Routledge.

Bird, A. (2008). Scientific Progress as Accumulation of Knowledge: A Reply to Rowbottom. *Studies in the History and Philosophy of Science, 39*(2), 279-281.

Bird, A. (forthcoming). Inductive Knowledge. In D. Pritchard (Ed.), *Routledge Companion to Epistemology*: Routledge.

Bonder, G. M. (2004). Deviations from Ideal Gas Law Behavior. *Bonder Research Web*, from http://chemed.chem.purdue.edu/genchem/topicreview/bp/ch4/deviation.php#analysis

Bowie, A. (Ed.). (1889). *On the Motion of the Heart and the Blood in Animals by William Harvey M.D.* London: George Bell & Sons.

Boyd, R. (1983). The Current Status of the Issue of Scientific Realism. *Erkenntnis, 19,* 45-90.

Braithwaite, R. B. (1953). *Scientific Explanation*. Cambridge: Cambridge University Press.

Cann, R. L., Stoneking, M., & Wilson, A. C. (1987). Mitochondrial DNA and Human Evolution. *Nature, 325,* 31-36.

Cartwright, N. (1999). *The Dappled World: A Study of the Boundaries of Science*. Cambridge, MA: Cambridge University Press.

Chang, H. (2004). *Inventing Temperature: Measurement and Scientific Progress*. New York: Oxford University Press.

Chang, H. (2007). Scientific Progress: Beyond Foundationalism and Coherentism. *Royal Institute of Philosophy Supplement, 82*, 1-20.

Churchland, P. M. (1985). The Ontological Status of Unobservables: In Praise of the Superempirical Virtues. In P. M. Churchland & C. A. Hooker (Eds.), *Images of Science: Essays on Realism and Empiricism, with a Reply from Bas C. van Fraassen* (pp. 35-47). Chicago: University of Chicago Press.

Cohen, I. B. (1985a). *The Birth of a New Physics* (Revised and Updated 1985 ed.). New York: W. W. Norton & Co.

Cohen, I. B. (1985b). *Revolution in Science* (6 ed.). Cambridge, Mass.: Belknap Press of Harvard University Press.

Cohen, I. B., & Whitman, A. M. (Eds.). (1999). *The Principia: Mathematical Principles of Natural Philosophy*. Berkeley: University of California Press.

Cope, J. I., & Jones, H. W. (Eds.). (1958). *History of the Royal Society by Thomas Sprat*. Saint Louis, Missouri: Washington University.

Cordero, A. (2001). Realism and Underdetermination: Some Clues from the Practices-Up. *Philosophy of Science, 68*(3), S301-S312.

Cornelia, D. (2009). So Much to Learn about the Oceans from Sand. *The New York Times*. Retrieved from http://www.nytimes.com/2009/01/06/science/06prof.html

Cottingham, J., Stoodhoff, R., Murdoch, D., & Kenny, A. (Eds.). (1991). *The Philosophical Writings of Rene Descartes Volume III: The Correspondence* (Vol. 3). New York: Cambridge University Press.

Cottingham, J., Stoodhoff, R., & Murdoch, D. (Eds.). (1985). *The Philosophical Writings of Rene Descartes*. New York: Cambridge University Press.

Coutinho, E. M., & Segal, S. J. (1999). *Is Menstruation Obsolete?* New York: Oxford University Press.

Daston, L., & Galison, P. (2007). *Objectivity*. New York: Zone Books.

Dauben, J. (1992). Conceptual Revolutions and the History of Mathematics: Two Studies in the Growth of Knowledge. In D. Gillies (Ed.), *Revolutions in Mathematics* (pp. 49-71). Oxford: Clarendon Press.

Davies, P. (2003). *How to Build a Time Machine*. New York: Penguin Books.

Davis, A. B. (1971). Some Implications of the Circulation Theory for Disease Theory and Treatment in the Seventeenth Century. *Journal of the History of Medicine and Allied Sciences, XXVI*, 28-39.

De Beer, E. S. (Ed.). (1955). *Diaries of John Evelyn*. Oxford: Clarendon Press.

Debus, A. G. (1954). *Man and Nature in the Renaissance*. New York: Cambridge University Press.

Devitt, M. (1997). *Realism and Truth* (2nd ed.). Princeton, N.J.: Princeton University Press.

Dicken, P., & Lipton, P. (2006). What can Bas Believe? Musgrave and van Fraassen on Observability. *Analysis, 66*, 226-233.

Dilworth, C. (2007). *Scientific Progress: A Study Concerning the Nature of the Relation between Successive Scientific Theories*. New York: Springer.

Dreifus, C. (2009, October 12, 2009). A Conversation With Carol W. Greider On Winning a Nobel Prize in Science. *The New York Times*. Retrieved from http://www.nytimes.com/2009/10/13/science/13conv.html?emc=eta1

Duhem, P. (1969). *To Save the Phenomena*. Chicago: University of Chicago Press.

Duhem, P. M. (1954). *The Aim and Structure of Physical Theory* (P. Wiener, Trans.). Princeton, NJ: Princeton University Press.

Ehrlich, P. (1908). Nobel Lecture: Partial Cell Functions. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/medicine/laureates/1908/ehrlich-lecture.pdf

Elakana, Y., & Goodfield, J. (1968). Harvey and the Problem of the 'Capillaries'. *Isis, 59*, 61-73.

Elgin, C. (2002). Creation as Reconfiguration: Art in the Advancement of Science. *International Studies in the Philosophy of Science, 16*, 13-25.

Elgin, C. (2004). True Enough. *Philosophical Issues, 14*, 113-131.

Elgin, C. (2007). Understanding and the Facts. *Philosophical Studies, 132*, 33-42.

Feldman, R. (1981). Fallibilism and Knowing that One Knows. *The Philosophical Review, 90*, 266-282.

Fenner, F., Henderson, D. A., Arita, I., & Ladnyi, I. D. (1988). Smallpox and Its Eradication. *History of International Public Health, 6*. Retrieved from http://whqlibdoc.who.int/smallpox/9241561106.pdf

Feynman, R. P. (1965). *The Character of Physical Laws*. Cambridge, MA: MIT Press.

Feynman, R. P. (1995). *Six Easy Pieces: Essentials of Physics Explained by Its Most Brilliant Teacher*. Pasadena, CA: California Institute of Technology.

Fine, A. (2001). The Scientific Image Twenty Years Later. *Philosophical Studies, 106*, 107-122.

Ford, B. J. (1991). *The Leeuwenhoek Legacy*. London: Farrand Press.

Franklin, K. J. (Ed.). (1963). *William Harvey: The Circulation of the Blood and other Writings*. London: J. M. Dent & Sons Ltd.

Fred, E. B. (1932). Antony van Leeuwenhoek: On the Three-Hundredth Anniversary of his Birth. *Journal of Bacteriology, XXV*(1), 1-18.

Freudenthal, G. (1984). The Role of Shared Knowledge in Science: The Failure of the Constructivist Program in the Sociology of Science. *Social Studies of Science, 14*, 285-295.

Friedman, M. (1982). Review of The Scientific Image, by Bas C. van Fraassen. *Journal of Philosophy, 79*, 79-92.

Fuchs, S. (1993). A Sociological Theory of Scientific Change. *Social Forces, 71*, 933-953.

Gettier, E. (1963). Is Justified True Belief Knowledge? *Analysis, 23*, 121-123.

Gilde, J. M. (1970). Shadwell and the Royal Society: Satire in The Virtuoso. *Studies in English Literature 1500-1900, 10*, 469-490.

Gillispie, C. C. (1960). *The Edge of Objectivity: An Essay in the History of Scientific Ideas* (Fourth Princeton Paperback Printing ed.). Princeton, NJ: Princeton University Press.

Glanvill, J. (1668/1958). *Plus ultra; or, The progress and advancement of knowledge since the days of Aristotle (1668): A facsimile reproduction with an introd. by Jackson I. Cope*. Gainesville, FL: Scholars' Facsimiles & Reprints.

Glass, D. J., & Hall, N. (2008). A Brief History of the Hypothesis. *Cell, 134*, 378-381.

Gödel, K. (1949). An example of a new type of cosmological solutions of Einstein’s field equations of gravitation. *Review of Modern Physics, 21*, 447-450.

Godfrey-Smith, P. (2007). *Progress and Procedures in Scientific Epistemology*. Paper presented at The 2007 Reichenbach Lecture at UCLA. Retrieved from http://philoscience.unibe.ch/documents/TexteFS09/Godfrey-Smith2007.pdf

Goldman, A. I. (1999). *Knowledge in a Social World*. New York: Oxford University Press.

Gott, J. R. (2002). *Time Travel in Einstein’s Universe: The Physical Possibilities of Travel through Time*. New York: Houghton Mifflin Books.

Greenberg, R. A., & Piper, W. (Eds.). (1973). *The Writings of Jonathan Swift*. New York: W. W. Norton & Co.

Grimm, S. R. (2006). Is Understanding a Species of Knowledge? *British Journal for the Philosophy for Science, 57*, 515-535.

Grunbaum, A. (1976). Is the Method of Bold Conjectures and Attempted Refutations Justifiably the Method of Science? *British Journal for the Philosophy for Science, 27*, 105-136.

Hacking, I. (Ed.). (1981). *Scientific Revolutions*. New York: Oxford University Press.

Hacking, I. (1983). *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*. New York: Cambridge University Press.

Hall, A. R. (1957). *The Scientific Revolution 1500-1800: The Formation of the Modern Scientific Attitude*. Boston, MA: The Beacon Press.

Hall, M. B. (1962). *The Scientific Renaissance 1450-1630* (1994 ed.). New York: Dover Publications, INC.

Hall, R. A., & Hall, M. B. (Eds.). (1965-1986). *The Correspondence of Henry Oldenburg*. Madison: University of Wisconsin Press.

Hammarsten, E. (1953). Presentation Speech. The Nobel Prize in Physiology or Medicine. *Nobel Lectures,* from http://nobelprize.org/nobel_prizes/medicine/laureates/1953/press.html

Hedrén, G. (1930). Presentation Speech. The Nobel Prize in Physiology or Medicine. *Nobel Lectures,* from http://nobelprize.org/nobel_prizes/medicine/laureates/1930/press.html

Hempel, C. (1965). *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*. New York: Free Press.

Heywood, R. B. (Ed.). (1947). *The Works of the Mind*. Chicago: University of Chicago Press.

Holmes, F. L. (1993). *Hans Krebs: Architect of Intermediary Metabolism, 1933-1937* (Vol. 2). New York: Oxford University Press.

Hoole, S. (Ed.). (1800). *The Select Works of Antony van Leeuwenhoek, Containing His Microscopical Discoveries in Many of the Works of Nature*. London: Printed for the translator by G. Sidney.

Horwich, P. (1987). *Asymmetries in Time*. Cambridge, MA: Cambridge University Press.

Horwich, P. (1991). On the Nature and Norms of Theoretical Commitment. *Philosophy of Science, 58,* 1-14.

Houghton, W. B. J. (1941). The History of Trades: Its Relation to Seventeenth-Century Thought. *Journal of the History of Ideas, 2,* 33-60.

Houghton, W. B. J. (1942). The English Virtuoso in the Seventeenth Century Part I. *Journal of the History of Ideas, 3*, 51-73.

Houghton, W. B. J. (1942). The English Virtuoso in the Seventeenth Century Part II. *Journal of the History of Ideas, 3*, 190-219.

Hsiu-Chuan, L. V., & Kun-Lin, O. (2005). Development and testing of a green fluorescent protein-based bacterial biosensor for measuring bioavailable arsenic in contaminated groundwater samples. *Environmental Toxicology and Chemistry, 24*, 1624-1631.

Hunter, M. (1975). *John Aubrey and the Realm of Learning*. London: Duckworth.

Hunter, M. (1976). The Social Basis and Changing Fortune of an Early Scientific Institution: An Analysis of the Membership of the Royal Society, 1660-1685. *Notes and Records of the Royal Society of London, 31*(1), 9-114.

Hunter, M. (1981). *Science and Society in Restoration England*. New York: Cambridge University Press.

Hunter, M. (Ed.). (1998). *Archives of the Scientific Revolution: The Formation and Exchange of Ideas in Seventeenth-Century Europe*. Woodbridge: The Boydell Press.

Hunter, M., & Davis, E. B. (Eds.). (1999). *The Works of Robert Boyle*. London: Brookfiled.

Hunter, M., & Wood, P. B. (1986). Towards Solomon's House: Rival Strategies for Reforming the Early Royal Society. *History of Science, 24,* 49-108.

Iranzo, V. (2008). Reliabilism and the Abductive Defence of Scientific Realism. *Journal of General Philosophy of Science, 39*, 115-120.

Jackson, F. (2002). Critical Notice of Knowledge and Its Limits By Timothy Williamson. *Australian Journal of Philosophy, 80*, 516-521.

Jacob, J. R. (1975). Restoration, Reformation, and the Origins of the Royal Society. *History of Science, 13*, 155-176.

James, W. (1897). *The Will to Believe and other Essays in Popular Philosophy*. New York: Longman Green & Co.

Janack, M. (2002). Dilemmas of Objectivity. *Social Epistemology, 16*, 267-281.

Jenner, E. (1800). *An inquiry into the causes and effects of the variolae vaccinae: a disease discovered in some of the western courtiers of England, particularly Gloucestershire, and known by the name of the Cow Pox* (2 ed.): Printed for the Author by Sampson Low.

Kargon, R. H. (1966). *Atomism in England from Hariot to Newton*. New York: Oxford University Press.

Kaufmann, S. H. E. (2008). Immunology’s Foundation: The 100-year Anniversary of the Nobel Prize to Paul Ehrlich and Elie Metchnikoff. *Nature Immunology, 9*, 705-712.

Kitcher, P. (1983). *Abusing Science: The Case Against Creationism*. Cambridge, MA: The MIT Press.

Kitcher, P. (1993). *The Advancement of Science: Science without Legend, Objectivity without Illusions*. New York: Oxford University Press.

Kitcher, P. (2002). Scientific Knowledge. In P. K. Moser (Ed.), *The Oxford Handbook of Epistemology* (pp. 385-407). New York: Oxford University Press.

Kohn, D. (1980). Theories to Work By: Rejected Theories, Reproduction and Darwin’s Path to Natural Selection. In W. Coleman & C. Limoges (Eds.), *Studies in History of Biology* (pp. 67-170). Baltimore: Johns Hopkins University Press.

Kosso, P. (2007). Scientific Understanding. *Foundations of Science, 12*, 173-188.

Koyré, A. (1957). *From the Closed World to the Infinite Universe* (John Hopkins Paperbacks ed.). Baltimore: The John Hopkins University Press.

Krebs, H. A. (1953). Nobel Lecture: The Citric Acid Cycle. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/medicine/laureates/1953/krebs-lecture.pdf

Krebs, H. A. (1970). The History of the Tricarboxylic Acid Cycle. *Perspectives in Biology and Medicine, 14*, 154-170.

Krebs, H. A., & Johnson, W. A. (1937). The role of citric acid in intermediate metabolism in animal tissues. *Enzymologia, 4*, 148-156.

Krebs, H. A., & Salvin, E. (1938). The formation of citric and a-ketoglutaric acids in the mammalian body. *Biochemical Journal, 32*, 113-117.

Kronick, D. (1976). *A History of Scientific and Technical Periodicals 1665-1790* (2 ed.). Metuchen, N.J.: Scarecrow Press.

Kuhn, T. S. (1959). The Essential Tension: Tradition and Innovation in Scientific Research. In C. Taylor (Ed.), *The Third University of Utah Research Conference on the Identification of Scientific Talent* (pp. 162-174). Salt Lake City: University of Utah Press.

Kuhn, T. S. (1970). Logic of Discovery or Psychology of Research? In I. Lakatos & A. Musgrave (Eds.), *Criticism and the Growth of Knowledge* (pp. 1-24). New York: Cambridge University Press.

Kuhn, T. S. (1977). *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: University of Chicago Press.

Kuhn, T. S. (1992). *The Trouble with the Historical Philosophy of Science*. Cambridge, MA: An Occasional Publication of the Department of the History of Science Harvard University.

Kuhn, T. S. (1996). *The Structure of Scientific Revolutions* (3 ed.). Chicago: The University of Chicago Press.

Kukla, A. (1998). *Studies in Scientific Realism*. New York: Oxford University Press.

Kusch, P. (1955). Banquet Speech. The Nobel Prize in Physics. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/physics/laureates/1955/kusch-speech.html

Kvanvig, J. L. (2003). *The Value of Knowledge and the Pursuit of Understanding*. New York: Cambridge University Press.

Kvanvig, J. L. (2008). Pointless Truth. *Midwest Studies in Philosophy, XXXII*, 199-212.

Kvanvig, J. L. (2009). Responses to Critics. In A. Haddock, A. Millar & D. Pritchard (Eds.), *Epistemic Value* (pp. 339-353). New York: Oxford University Press.

Lakatos, I. (1970). History of Science and its Rational Reconstructions. *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association, 1970*, 91-136.

Lakatos, I. (1978). *The Methodology of Scientific Research Programmes*. New York: Cambridge University Press.

Lakatos, I., & Musgrave, A. (1970). *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.

Landsteiner, K. (1930). Nobel Lecture: On Individual Differences in Human Blood. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/medicine/laureates/1930/landsteiner-lecture.pdf

Landsteiner, K., & Miller, P. C. (1925). Serological Studies in the Blood of the Primates: II. The Blood Groups in Anthropoid Apes. *The Journal of Experimental Medicine, 42*(6), 853–862.

Lange, M. (2002). Baseball, Pessimistic Inductions and the Turnover Fallacy. *Analysis, 62*, 281-285.

Lansdowne, H. (1927). *The Petty Papers*. London: Constable.

Laudan, L. (1977). *Progress and its Problems: Toward a Theory of Scientific Growth*. Berkeley: University of California Press.

Laudan, L. (1978). The Philosophy of Progress... *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association, 2: Symposia and Invited Papers*, 530-547.

Laudan, L. (1981a). A Problem-Solving Approach to Scientific Progress. In I. Hacking (Ed.), *Scientific Revolutions* (pp. 144-155). New York: Oxford University Press.

Laudan, L. (1981b). A Confutation of Convergent Realism. *Philosophy of Science, 48*(1), 19-49.

Laudan, L. (1984). Explaining the Success of Science: Beyond Epistemic Realism and Relativism. In J. Cushing, C. F. Delaney & G. M. Gutting (Eds.), *Science and Reality: Recent Work in the Philosophy of Science Essays in Honor of Ernan McMullin* (pp. 83-105). Notre Dame, IN: University of Notre Dame Press.

Laudan, L. (1984). *Science and Values: The Aims of Science and their Role in Scientific Debate*. Berkeley: University of California Press.

Laudan, L., & Leplin, J. (1991). Empirical Equivalence and Underdetermination. *Journal of Philosophy, 88*, 449-472.

Laveran, C. L. A. (1907). Nobel Lecture: Protozoa as Causes of Diseases. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/medicine/laureates/1907/laveran-lecture.html

Leewenhoeck, A. v. (1677-1678). Observations, Communicated to the Publisher by Mr. Antony van Leewenhoeck, in a Dutch Letter of the 9th of Octob. 1676. Here English'd: concerning Little Animals by Him Observed in Rain-Well-Sea. and Snow Water; as Also in Water Wherein Pepper Had Lain Infused. *Philosophical Transactions, 12*, 821-831.

Lemaine, G., Mulkay, M., Macleod, R., & Weingart, P. (Eds.). (1976). *Perspectives on the Emergence of New Scientific Disciplines*. Paris: Mouton.

Leplin, J. (1981). Truth and Scientific Progress. *Studies in History and Philosophy of Science Part A, 12*, 269-291.

Leplin, J. (1984). *Scientific Realism*. Berkeley: University of California Press.

Leplin, J. (1997). *A Novel Defense of Scientific Realism*. New York: Oxford University Press.

Lewis, P. J. (2001). Why the Pessimistic Induction is a Fallacy. *Synthese, 129*, 371-380.

Livet, J., Weissman, T. A., Kang, H., Draft, R. W., Lu, J., Sanes, J. R., et al. (2007). Transgenic strategies for combinatorial expression of fluorescent proteins in the nervous system. *Nature, 450*, 56-63.

Loewe, L., & Scherer, S. (1997). Mitochondrial Eve: The Plot Thickens. *Trends in Ecology & Evolution, 12*, 422-423.

Longrigg, J. (1998). *Greek Medicine from the Heroic to the Hellenistic Age: A Sourcebook*. New York: Routledge.

Losee, J. (2004). *Theories of Scientific Progress*. London: Routledge.

Lubitz, S. A. (2004). Early Reactions to Harvey’s Circulation Theory: The Impact on Medicine. *The Mount Sinai Journal of Medicine, 71*, 274-280.

Maxwell, G. (1962). The Ontological Status of Theoretical Entities. In H. Feigl & G. Maxwell (Eds.), *Scientific Explanation, Space, and Time: Minnesota Studies in the Philosophy of Science* (Vol. 3, pp. 3-27). Minneapolis: University of Minnesota Press.

Mazzarello, P. (1999). *The Hidden Structure: A Scientific Biography of Camillo Golgi* (A. Badiani, Trans.). New York: Oxford University Press.

McKie, D. (1960). The Origins and Foundation of the Royal Society of London. *Notes and Records of the Royal Society of London, 15*, 1-37.

Mechnikov, I. (1908). Nobel Lecture: On the Present State of the Question of Immunity in Infectious Diseases. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/medicine/laureates/1908/mechnikov-lecture.html

Mellor, D. H. (1991). *Matters of Metaphysics*. New York: Cambridge University Press.

Miller, V. R., & Miller, R. P. (Eds.). (1983). *Principles of Philosophy*. Boston, Mass.: Kluwer.

Molesworth, W. (Ed.). (1839/1966). *The English Works of Thomas Hobbes of Malmesbury*. Aalen: Scientia Verlag.

Monton, B. (Ed.). (2007). *Images of Empiricism: Essays on Science and Stances, with a Reply from Bas C. van Fraassen*. New York: Oxford University Press.

Monton, B., & van Fraassen, B. (2003). Constructive Empiricism and Modal Nominalism. *British Journal for the Philosophy for Science, 54*, 405-422.

Mörner, K. A. H. (1904). Presentation Speech. The Nobel Prize in Physiology or Medicine. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/medicine/laureates/1904/press.html

Mörner, K. A. H. (1905). Presentation Speech. The Nobel Prize in Physiology or Medicine. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/medicine/laureates/1905/press.html

Mörner, K. A. H. (1908). Presentation Speech. The Nobel Prize in Physiology or Medicine. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/medicine/laureates/1908/press.html

Moser, P. K. (Ed.). (2002). *The Oxford Handbook of Epistemology*. New York: Oxford University Press.

Mowry, B. (1985). From Galen’s Theory to William Harvey’s Theory: A Case Study in the Rationality of Scientific Theory Change. *Studies in the History and Philosophy of Science, 16*, 49-82.

Muller, F. A. (2004). Can a Constructive Empiricist Adopt the Concept of Observability? *Philosophy of Science, 71*, 80-97.

Muller, F. A. (2008). In Defence of Constructive Empiricism: Maxwell's Master Argument and Aberrant Theories. *Journal of General Philosophy of Science, 39*, 131-156.

Muller, M. A., & van Fraassen, B. C. (2008). How to talk about unobservables. *Analysis, 68*, 197-205.

Musgrave, A. (1985). Constructive Empiricism and Realism. In P. M. Churchland & C. A. Hooker (Eds.), *Images of Science: Essays on Realism and Empiricism, with a Reply from Bas C. van Fraassen* (pp. 196-208). Chicago: University of Chicago Press.

Nathans, D. (1978). Banquet Speech: The Nobel Prize in Physiology or Medicine 1978. *Nobel Lectures,* from http://nobelprize.org/nobel_prizes/medicine/laureates/1978/nathans-speech.html

Neta, R. (2002). S Knows That P. *Nous, 36*, 663-681.

Newton-Smith, W., & Lukes, S. (1978). The Underdetermination of Theory by Data. *Proceedings of the Aristotelian Society, 52*(Supplementary Volumes), 71-91+93-107.

Nicolson, M. H. (Ed.). (1965). *Pepys’ ‘Diary’ and the New Science*. Charlottesville: University Press of Virginia.

Nicolson, M. H., & Rodes, D. S. (Eds.). (1966). *Thomas Shadwell: The Virtuoso*. Lincoln: University of Nebraska Press.

Niiniluoto, I. (1984). *Is Science Progressive?* Boston, MA: Kluwer Academic Publishers.

Niiniluoto, I. (1987). *Truthlikeness*. Boston, MA: Kluwer Academic Publishers.

Okasha, S. (2002). Underdetermination, Holism and the Theory/Data Distinction. *The Philosophical Quarterly, 52*, 303-319.

Olscamp, P. J. (Ed.). (2001). *Discourse on Method, Optics, Geometry, and Meteorology*. Indianapolis, IN: Hackett Publishing.

O'Malley, C. D. (1965). *Andreas Vesalius of Brussels 1514-1564*. Berkeley: University of California Press.

Overbye, D. (2008, June 3). Dark, Perhaps Forever. *The New York Times*. Retrieved from http://www.nytimes.com/2008/06/03/science/03dark.html?_r=1&em&ex=1212638400&en=ada1834d925276c4&ei=5087%0A

Pagel, W. (1976). *New Light on William Harvey*. Basel: S. Karger.

Palm, L. C. (Ed.). (1999). *The Collected Letters of Antoni van Leeuwenhoek*. Lisse, Netherlands: Swets en Zeitlinger.

Pan, H., Ritter, J. A., & Balbuena, P. B. (1998). Examination of the Approximations Used in Determining the Isosteric Heat of Adsorption from the Clausius-Clapeyron Equation. *Langmuir, 14*, 6323-6327.

Pavlov, I. (1904). Nobel Lecture: Physiology of Digestion. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/medicine/laureates/1904/pavlov-lecture.html

Peltonen, M. (Ed.). (1996). *The Cambridge Companion to Bacon*. New York: Cambridge University Press.

Pfarr, J. (1981). Time Travel in Gödel’s Space. *General Relativity and Gravitation, 13,* 1073-1091.

Piccinini, G. (2003). Epistemic Divergence and the Publicity of Scientific Methods. *Studies in the History and Philosophy of Science, 34*, 597-612.

Popper, K. (1968). *Conjectures and Refutations: The Growth of Scientific Knowledge*. New York: Harper & Row.

Popper, K. R. (1979). *Objective Knowledge: An Evolutionary Approach* (Revised 1979 ed.). Oxford: Clarendon Press.

Popper, K. R., & Bartley, W. W. (1982). *Realism and the Aim of Science*. Totowa, N.J.: Rowman and Littlefield.

Pritchard, D. (2007). Recent Work on Epistemic Value. *American Philosophical Quarterly, 44*, 85-110.

Pritchard, D. (forthcoming). Knowledge, Understanding and Epistemic Value. In A. O’Hear (Ed.), *Epistemology*: Cambridge University Press.

Psillos, S. (1996). Scientific Realism and the 'Pessimistic Induction'. *Philosophy of Science, 63*(Supplement), S306-S314.

Psillos, S. (1999). *Scientific Realism: How Science Tracks Truth*. London: Routledge.

Psillos, S. (2000). The Present State of the Scientific Realism Debate. *British Journal for the Philosophy for Science, 51,* 705-728.

Psillos, S., & Curd, M. (Eds.). (2008). *The Routledge Companion to the Philosophy of Science*. Abingdon: Routledge.

Purver, M. (1967). *The Royal Society: Concept and Creation*. London: Routledge.

Quine, W. V. O. (1953). Two Dogmas of Empiricism *From A Logical Point of View*. Cambridge, MA: Harvard University Press.

Quine, W. V. O. (1969). Epistemology Naturalized *Ontological Relativity and Other Essays* (pp. 69-90). New York: Columbia University Press.

Robinson, H. W., & Adams, W. (Eds.). (1935). *Diaries of Robert Hooke*. London: Taylor and Francis.

Ross, G. R. T. (Ed.). (1911). *The Philosophical Works of Rene Descartes*. New York: Cambridge University Press.

Rossi, P. (1996). Bacon’s Idea of Science. In M. Peltonen (Ed.), *The Cambridge Companion to Bacon* (pp. 25-46). New York: Cambridge University Press.

Rowbottom, D. (2008). N-Rays and the Semantic View of Scientific Progress. *Studies in the History and Philosophy of Science, 39*(2), 277-278.

Russell, B. (1997). *Religion and Science. With a new introduction by Michael Ruse*. New York: Oxford University Press.

Rutherford, E. (1947). The Scientist. In R. B. Heywood (Ed.), *The Works of the Mind*. Chicago: University of Chicago Press.

Ryle, G. (1946). Knowing How and Knowing That. *Proceedings of the Aristotelian Society, XLVI*.

Ryle, G. (1949). *The Concept of Mind*. New York: Barnes & Noble.

Saatsi, J. T. (2005). On the Pessimistic Induction and Two Fallacies. *Philosophy of Science, 72*, 1088-1098.

Salmon, W. (1993). The Value of Scientific Understanding. *Philosophica, 51*, 9-19.

Salmon, W. (1998). *Causality and Explanation*. New York: Oxford University Press.

Sargent, R. (2002). Francis Bacon and the Humanistic Aspects of Modernity. *Midwest Studies in Philosophy, XXVI*, 124-139.

Sarton, G. (1927). *Introduction to the History of Science*. Baltimore: Carnegie Institution of Washington.

Schouten, J. (1974). Johannes Walaeus (1604-1649) and His Experiments on the Circulation of the Blood. *Journal of the History of Medicine and Allied Sciences, XXIX*, 259-279.

Schultz, S. G. (2002). William Harvey and the Circulation of the Blood: The Birth of a Scientific Revolution and Modern Physiology. *News in Physiological Sciences, 17*, 175-180.

Schurz, G., & Lambert, K. (1994). Outline of a Theory of Scientific Understanding. *Synthese*, *101*(1), 65-120.

Shapere, D. (1982). The Concept of Observation in Science and Philosophy. *Philosophy of Science, 49*(4), 485-525.

Shapere, D. (1984). *Reason and the Search for Knowledge*. Dordrecht: Reidel.

Shapin, S., & Schaffer, S. (1985). *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life: Including a Translation of Thomas Hobbes, Dialogus physicus de natura aeris by Simon Schaffer*. Princeton, N.J.: Princeton University Press.

Sherman, I. W. (1998). *Malaria: Parasite Biology, Pathogenesis, and Protection*: ASM Press.

Shope, R. K. (1983). *The Analysis of Knowledge: A Decade of Research*. Princeton, NJ: Princeton University Press.

Sieveking, A. F. (1923-4). Evelyn’s ‘Circle of Mechanical Trades’. *Transactions of the Newcomen Society, 4*, 40-47.

Snelders, H. A. M. (1989). Christiaan Huygens and Newton’s Theory of Gravitation. *Notes and Records of the Royal Society of London, 43*, 209-222.

Soderbaum, H. G. (1912). Presentation Speech. The Nobel Prize in Chemistry. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/chemistry/laureates/1912/press.html

Sosa, E., & Greco, J. (Eds.). (1999). *The Blackwell Guide to Epistemology*. Oxford: Blackwell.

Spedding, J., Ellis, R. L., & Heath, D. D. (Eds.). (1857-74). *The Works of Francis Bacon*. London: Longman.

Sprat, T., & Cowley, A. (1734). *The history of the Royal Society of London, for the improving of natural knowledge*. London: Printed for J. Knapton.

Stotz, K. (2009). Philosophy in the Trenches: From Naturalized to Experimental Philosophy (of Science). *Studies in the History and Philosophy of Science, 40,* 225-226.

Sulloway, F. J. (1982). Darwin's Conversion: The Beagle Voyage and its Aftermath. *Journal of the History of Biology*, *15,* 325-396.

Sundberg, C. (1907). Presentation Speech. The Nobel Prize in Physiology or Medicine. *Nobel Lectures,* from http://nobelprize.org/nobel_prizes/medicine/laureates/1907/press.html

Sykes, B. (2001). *The Seven Daughters of Eve: The Science that Reveals our Genetic Ancestry*. New York: Norton.

Tatum, E. (1958). Nobel Lecture: A Case History in Biological Research. *Nobel Lectures*, from http://nobelprize.org/nobel_prizes/medicine/laureates/1958/tatum-lecture.html

Tauber, A. I. (2003). Metchnikoff and the phagocytosis theory. *Nature Reviews: Molecular Cell Biology, 4*, 897-901.

Tauber, A. I., & Chernyak, L. (1991). *Metchnikoff and the Origins of Immunology: From Metaphor to Theory*. New York: Oxford University Press.

Thucydides. (431 B.C.E/2007). History of the Peloponnesian War. In R. Crawley (Eds.) Available from http://ebooks.adelaide.edu.au/t/thucydides/crawley/index.html

Tichy, P. (1974). On Popper's Definitions of Verisimilitude. *British Journal for the Philosophy for Science, 25,* 155-160.

Toulmin, S. (1961). *Foresight and Understanding*. New York: Harper & Row.

van Fraassen, B. C. (1980). *The Scientific Image*. New York: Oxford University Press.

van Fraassen, B. C. (1985). Empiricism in the Philosophy of Science. In P. M. Churchland & C. A. Hooker (Eds.), *Images of Science: Essays on Realism and Empiricism, with a Reply from Bas C. van Fraassen* (pp. 245-308). Chicago: The University of Chicago Press.

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

van Fraassen, B. C. (2001). Constructive Empiricism Now. *Philosophical Studies*, *106,* 151-170.

van Fraassen, B. C. (2007). From a View of Science to a New Empiricism. In B. Monton (Ed.), *Images of Empiricism: Essays on Science and Stances, with a Reply from Bas C. van Fraassen* (pp. 337-383). New York: Oxford University Press.

Warner, D. (1994). Terrestrial Magnetism: For the Glory of God and the Benefit of Mankind. *Osiris, 9*(Instruments), 66-84.

Weber, E. (1996). Explaining, Understanding and Scientific Theories. *Erkenntnis, 44,* 1-23.

Webster, C. (1975). *The Great Instauration: Science, Medicine and Reform 1626-60*. London: Duckworth.

Westfall, R. S. (1971). *The Construction of Modern Science: Mechanisms and Mechanics* (1994 ed.). New York: Cambridge University Press.

Whitteridge, G. (1971). *William Harvey and the Circulation of the Blood*. New York: American Elsevier Inc.

Williamson, T. (1997). Knowledge as Evidence. *Mind, 106*, 717-741.

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

Willis, R. (1878). *William Harvey: A History of the Discovery of the Circulation of the Blood*. London: C. Kegan Paul & Co.

Willis, R. (Ed.). (1910). *Scientific Papers: Physiology, Medicine, Surgery, Geology, with Introductions, Notes and Illustrations* (Vol. 38). New York: P. F. Collier and Son.

Wilson, C. (1988). Visual Surface and Visual Symbol: The Microscope and the Occult in Early Modern Science. *Journal of the History of Ideas, 49*(1), 85-108.

Wilson, C. (1995). *The Invisible World: Early Modern Philosophy and the Invention of the Microscope*. Princeton, NJ: Princeton University Press.

Wilson, C. (1996). Instruments and Ideologies: The Social Construction of Knowledge and Its Critics. *American Philosophical Quarterly, 33*(2), 167-181.

Wilson, C. (2008). *Commentary to Gianni Paganini "Learning from Doubt"*. Paper presented at the NYU Conference on Issues in Modern Philosophy, New York University.

Wilson, C. (2009). Organic Mechanism and the Soul of Science. *CBMH/BCHM, 26*(1), 85-97.

Wilson, E. O. (1999). *Consilience: The Unity of Knowledge*. New York: Random House.

Wood, D. J. (2004). The First Nobel Prize for Integrated Systems Physiology: Ivan Petrovich Pavlov, 1904. *Physiology, 19*, 326-330.

Worrall, J. (1982). Scientific Realism and Scientific Change. *The Philosophical Quarterly, 32*, 201-231.

Zagzebski, L. (1999). What Is Knowledge? In J. Greco & E. Sosa (Eds.), *The Blackwell Guide to Epistemology* (pp. 92-116). Malden, MA: Blackwell Publishing.

Zagzebski, L. (2001). Recovering Understanding. In M. Steup (Ed.), *Knowledge, Truth, and Duty: Essays on Epistemic Justification, Responsibility, and Virtue* (pp. 235-256). New York: Oxford University Press.

Zilsel, E. (1945). The Genesis of the Concept of Scientific Progress. *Journal of the History of Ideas, 6*(3), 325-349.

Zimmer, M. (2005). *Glowing Genes: A Revolution in Biotechnology*. Amherst, NY: Prometheus Books.

Zimmer, M. (2009). Green Fluorescent Protein (GFP). from http://gfp.conncoll.edu/

Zuckerman, H. (1996). *Scientific Elite: Nobel Laureates in the United States* (2 ed.). New Brunswick, NJ: Transaction Publishers.