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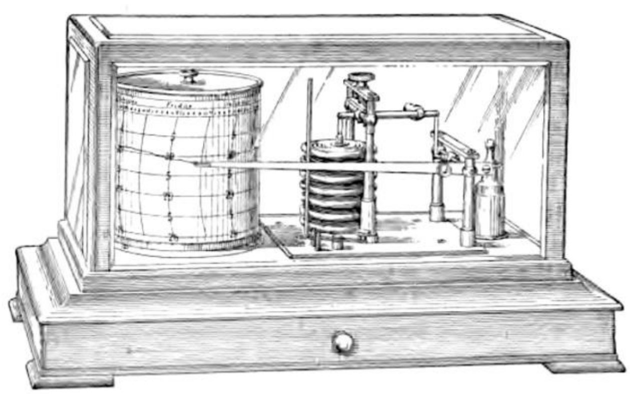
small-e empiricism



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The cover image is of an aneroid barograph from Rober A. Millikan and Henry G. Gale, *Practical Physics*. Boston: Ginn & Co., 1922, p.32.

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Preface

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It was inevitable that I would eventually write on empiricism. However, it was not obvious to me, until recently. For a long time, I did not see that much of my work relied tacitly on the idea that experience has a unique capacity to inform us of contingent facts. That idea, I now see, forms an essential part of the solution to a problem that has driven my research from the outset. I have long harbored a fascination with science and its far-reaching successes. How could we meager humans discover so much of such profound depth about the world? I had to understand how this magic worked.

A large part of the solution lay in inductive inference. It is the logic that connects our experience of the world with those profound discoveries. The goal of my work on the material theory of induction was to find an account of inductive inference rich enough to be applied to specific cases in the history of science, while, at the same time, vindicating the cogency of the inferences.

In these efforts, I had given little attention to experience as the font from which the inductive inferences drew. My optimism about science inclined me towards the scientific realism that came to prominence over the last half century. Scientific realism was then engaged in mortal combat with an implacable foe, empiricism, or, more precisely, the constructive empiricism devised by Bas van Fraassen. This empiricism is profoundly skeptical of the successes of science so prized by realists. It was an unappealing doctrine. I was loath to embrace empiricism. What resulted was an enduring, but hidden tension in my work, which drew heavily on the power of experience. My critiques of thought experiments and the philosophical literature on causation and possibility were, it became clear to me, driven by a deep commitment to empiricism.

The defining turn was a recognition of something I had long known but to which I had paid little attention. The empiricism routinely expressed by scientists is different from the skeptical empiricism of the past few decades of philosophy of science. The scientists had recognized that their successes depend essentially on a solid foundation in experience. That foundation is just a beginning. From it, they can infer securely to core results in their science that are quite remote from experience. The skeptical empiricism of philosophy of science has two parts. First, experience has the unique capacity to inform us of the world. Second, all we can learn from experience is the content of that experience itself. The scientists had fully embraced the first part, but, perhaps unwittingly, fully dispensed with the second.

The scientists were right in their conception of empiricism and we philosophers were wrong. The scientists' empiricism formed the starting point for my present reconfiguration of empiricism as a doctrine capable of recounting and vindicating the successes of science. Once I had decided on this project, it was immediately clear that the very concept of experience itself needed extensive revision. The notion still routinely employed in philosophy and philosophy of science was one introduced in the seventeenth and eighteenth century. Experience was equated with the excitations of our sense organs; and the processing of that experience was still often understood in terms of our mental processes. Modern science has moved far from this conception. Experience now includes instrumental sensings and the mechanized processing of what is sensed.

We need, I realized, an empiricism that responds to how science is in the twenty-first century. As I write these words, it is nearly four years since I began writing on this project. The result contains many more words than I had expected. There are now thirteen chapters that recount the history of empiricism and develop and defend the version of empiricism I advocate, "small-e empiricism." As a separate project, I also prepared a second volume of roughly nine chapters that explores the consequences of small-e empiricism for philosophy of science and epistemology.

In this overall project I have been helped by many, who have politely suffered my rambling speeches over my latest excitement and my peculiar queries over what to them must surely seem to be on the outermost edge what is reasonable. My habit is to offer specific thanks to individuals in the chapters to which their help is pertinent. As to the overall project, it has benefited greatly from the stimulating intellectual environment provided by the faculty and graduate students in my Department of History and Philosophy of Science; and by the Fellows visiting in the Center for Philosophy of Science, both at the University of Pittsburgh.

I owe a special thanks to the students who participated in my graduate seminar on "Empiricism in Science" in my department in the spring of 2024. My thanks to Jim Brown, who casually noted, before I realized it, that my 1980s analysis of thought experiments was empiricist. He planted a seed I initially resisted. James Norton has kindly endured my repeated pleas for help in the philosophy literature and has patiently tolerated my testing of new ideas.

My thanks to faculty and students at the University of Melbourne. They participated in a reading group, organized by Sam Baron, on the nearly completed draft of this overall project in March and April, 2026. I thank all the participants for their attention and discussion and especially Sam Baron, Tsz Ho Chan, Martin Leckey, Aiden Meyer, Reba Nelson, Cristian Philippi and Howard Sankey. Needless to say, none of those mentioned here are culpable for errors of detail and errors of conception in this volume. They tried to help but I resisted.

Finally, my greatest thanks are to my partner in life, Eve. For the years of the writing of this text, she tolerated a spouse whose head was buried in a computer screen and whose mind was in distant lands of thought, wrestling with sinister beasts. This work is dedicated to her. It is scant compensation for my inattention.

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Since small-e empiricism reconceptualizes experience as a physical process that connects continuously with the system of interest, it does not divide the experiential from the non-experiential content of a science. It allows only that some stages of the process are closer to the system of interest than others. Corrections to results inferred can be made by "winding back" through the stages of the process.

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Big-E Empiricism's quest for a terminus in experiential processes that constitutes experience *simpliciter* has been endlessly troublesome. No such clear division has been found and it has invited unsustainable, skeptical complaints under the heading of the “theory ladenness of observation.” Small-e empiricism has no such purely experiential terminus and has no need for it.

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The revisions to empiricism's conception of experience are collected to provide a synoptic statement of small-e empiricism and some of the consequences that follow from it. The defense argues that experience has the capacity to provide strong inductive support for the contingent propositions of science; and that all other modes have failed and are merely able to affirm results after they have been identified and secured empirically.

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The basis for small-e empiricism's rejection of the inductive skepticism of Big-E Empiricism are collected, elaborated and defended.

Afterword

1. Prospectus

Chapter 1

Prospectus

1. Why Another Book on Empiricism?

The core idea of empiricism is that experience, and it alone, has the unique capacity to inform us of contingent truths of the world.[1] This simple but powerful idea is of immense importance both to science and to philosophy. What led me to write this book is that, with only a few exceptions, present writing on empiricism in philosophy and philosophy of science has failed to capture the proper import of this core idea in both areas.

Empiricism, as commonly developed in the philosophy and philosophy of science literature, is deeply skeptical and psychological. It is filled with old ideas and attitudes that belong in earlier centuries of empiricist thinking. Experience is narrowly confined to the excitations of human sense organs and their ensuing processing by mental activity. This anthropocentrism has long been surpassed by the practices of modern science. There, instrumental sensings and algorithmic processing routinely replace and greatly improve on human sensing and mental processing. The philosopher's empiricism is pessimistic about the power of experience to inform us and often embraces a severe form of inductive skepticism. According to it, all we learn from experience is the content of the experience itself. Too much philosophical writing on empiricism struggles to explain away the immense success enjoyed by science in its discovery of many foundational truths of nature. A scientist's major discovery is to be discounted as merely a useful instrument for connecting possible experiences.

The empiricism advocated by scientists is, in contrast, more optimistic and more relaxed. Scientists have recognized that their enterprise has met with extraordinary successes and discoveries of profound import only as long as their endeavors have remained empirical. They insist, quite correctly, that science must be empirical. The empiricism advocated by scientists silently overlooks the excesses and aberrations of philosophical writing. It knows nothing of the philosophical empiricist's skepticism and employs a more expansive notion of experience that is

[1] I thank Balazs Gyenis for helpful comments on an earlier draft of this chapter.

not rigidly limited to the excitations of human sense organs. The emphasis is on the part of empiricism that has served science well: the unique capacity of experience to inform us.

A pessimism has proliferated in empiricist writing in philosophy and philosophy of science. It holds that scientists are naïve about the reach of experience and that they have failed to recognize the limits to their ambitions uncovered by philosophy. My view is different. It is we, the philosophers, who are failing. We are too quick to mount artful theses that deny the reach of experience and dispute the plainly visible achievements of science. In so far as we adhere to these artful theses, we are unable to account for the enormous successes of science. We need to try harder. The optimistic empiricism of the scientists is the better view.

The project of this book is to develop an optimistic version of empiricism that is well-adapted to modern science and to revive its importance to philosophy of science and to philosophy itself. It is to reaffirm the central tenet of empiricism, the unique privilege of experience, while discarding the skepticism too often associated with philosophical writing in empiricism. It will be founded on an expanded conception of experience, modeled on the successes of modern science, and will be sufficiently rich to vindicate those successes. The pathways to these successes, to be sure, are never as simple as our later, simplified histories relate. We must recognize that these complications are not fatal obstructions but challenges that scientists can and do overcome through dedication and ingenuity. My hope is to reawaken among empiricist philosophers of science the optimism that science genuinely can discover through experience contingent truths that form the foundations its theories. Correspondingly, my hope is to revive the idea amongst non-empirical metaphysicians that they must look to experience if they are to recover contingent, foundational truths about the traditional topics of metaphysics: space, time, matter, causality, possibility and the like; and to accept that modern, empirical science has enjoyed remarkable success in this project.

2. A Précis of small-e empiricism

The following are the basic elements of the version of empiricism to be developed here:

Small-e, not Big-E

Two components are common in many versions of empiricism. The first grants experience the unique capacity to inform us of contingent facts. The second insists that the factual content of experience is the totality of what experience informs us of the world. Everything else has an instrumental or fictional character whose sole function is to connect experiences with experiences. Big-E Empiricism endorses both components. It is a deeply pessimistic and skeptical doctrine quite at variance with the idea that science actually makes discoveries that go beyond mere associations among experience. Small-e empiricism endorses only the first component. According to it, experience informs us of contingent truths that go well beyond the content of experience itself. It is optimistic in crediting our best science with true discoveries.

Liberation from psychology

The modern tradition in empiricism was founded in the seventeenth century by Hobbes and Locke with the idea that experience consists of what human sense organs deliver to the mind and that its processing is carried out by the mental operations of thought. They wrote before present sciences were developed. The analysis of the operation of our sense organs is now best left to neuroscience and physiology; and how the mind processes ideas is now best left to empirical psychology. Their analysis is not the responsibility of small-e empiricism. Whatever its source, experience enters small-e empiricism once it has been given a communicable, propositional form, such as when it is described in scientific publications.

Experience generalized

The notion of experience is generalized to include instrumental sensings in which human sense organs play no part; and the processing of the instrumental sensings must include those executed outside the human mind. For much of the major discoveries of recent science comes from instrumental detection and their subsequent mechanized processing in which human mental processes play no role.

Experience as a process

In this generalized conception, experience consists of physical processes that connect us continuously with the systems of interest in the world. Their continuity precludes a strict division between experience and the results derived from it, such as is required by Big-E Empiricism. We can only make relative judgments. This stage can be judged to be closer in an experiential process to the system of interest and that stage is farther from it.

The authority of experience

The authority of experience does not reside in any special power of human sense organs or human cognition. Its authority derives from its constitution as a continuous physical process that connects us with the system of interest in the world.

Inductive structure

Experiential processes are structured by relations of inductive support among the propositions that describe the stages of the processes. A proposition describing one stage provides inductive support for propositions describing those closer to the system of interest. For example, propositions describing patches of light and dark in an astronomical photograph inductively support a proposition asserting the location of a light source in the sky. These supported propositions then also provide inductive support for more general propositions beyond the direct scope of the experiential process. Propositions concerning the location of the light sources in the sky over time provide inductive support for the proposition that these light sources are illuminated masses moving under an inverse square law of gravity.

Inductive optimism

Empiricism accords experience the *unique* capacity to inform us of contingent facts. This capacity is realized in the relations of inductive support that structure experiential processes and reach beyond them. Small-e empiricism takes inductive support for a proposition to be support for the truth of the proposition. Strong inductive support is strong support for truth and is the basis of the security of our mature sciences.[2] One is free not to accept this optimistic appraisal of inductive inference. However, to reject the import of inductive support is a poor choice. It is to be *irrational*, for rationality just consists in conforming with the dictates of reason, where these dictates are codified in an inductive logic.

Inductive fallibility

Inductive support is, by definition, support for conclusions that are logically stronger than the premises employed. It follows that inductive support, no matter how strong, is fallible. This fallibility provides no basis for skepticism, such as the skepticism that controls Big-E Empiricism. In cases of very strong inductive support, a failure is very unlikely, for that is the meaning of very strong inductive support. A failure to accept this fallibility as benign has proved repeatedly to be troublesome in philosophy. On one extreme, it supports a blanket, unsustainable inductive skepticism. On the other, it leads to an unsuccessful search for infallible certainty.

[2] For some philosophers, suggestions of inductive success trigger the automatic response: “What about Hume’s problem?!” Chapter 6 of Norton (2024) argues that the material theory of induction so alters the nature of inductive inference that standard forms of Hume’s problem of induction can no longer be formulated and that efforts to recreate them in the material theory fail.

Induction is material

Any account of inductive inference could be employed in small-e empiricism. Here I will treat inductive inference materially, in accord with the material theory of induction as developed in my Norton (2021, 2024). This account of inductive inference follows from an application of the ideas of small-e empiricism. Inductive inferences provide support for propositions that go beyond what is given in their premises. They are expanding our repertoire of contingent facts. Small-e empiricism asserts that such expansion can only be made on the basis of experience. Accordingly, inductive inferences in the material theory of induction are not warranted by universal rules that apply everywhere. They are warranted by contingent, material facts specific to the domain in which the specific inductive inferences proceed. In a mature science, these warranting facts in turn have inductive support that derives ultimately from experience.

3. Defending small-e empiricism

3.1 small-e empiricism Summarized

These last elements are combined to form the summary statement of small-e empiricism:

> *Science is empirical.* The propositions of experience provide inductive support for the truth of the contingent propositions of science; and the inductive support of experience is the only means of providing support for these contingent propositions.

That is, the only way we are informed of the contingent propositions of science is through their inductive support by experience. The case for small-e empiricism is based on the reconception of experience that is sketched above and is given in greater detail in Part II of the chapters that follow. In brief, the case comes in two parts, corresponding to the two clauses of the summary statement above.

3.2 For the Adequacy of Experience

The first clause asserts that experience has the capacity to provide inductive support for the contingent propositions of science. This capacity is founded on the notion of experience as a continuous physical process that connects us with the system of interest. Its processes form the channel through which we are informed inductively of the systems of interest. The results so discovered are then amplified beyond the scope of the experiential processes by further inductive inferences to results of greater generality. This much affirms the capacity of experience to inform us of contingent truths of the world that go well beyond experience. That these results embrace the totality of successful science can then be established by considering the sciences one by one and noting that in every case their foundation lies in experience.

The case for this first clause is enhanced by a resource derived from the inductive optimism of small-e empiricism, but denied to Big-E Empiricism. Experience itself provides strong inductive support for the reality of the physical processes that embody experience. On our best empirical evidence, there really are gravitational waves; and they really can inform us of the black hole coalescences that produced them. More generally, small-e empiricism supports the ordinary ontology of science which affirms the capacity of experiential processes to inform us of the world.

3.3 Against Other Modes

The second clause asserts the exclusive power of experience to inform us of contingent facts in the world. Making a case for it is more challenging since it must preclude existing modes that compete with it and perhaps even address the fanciful possibility of other as yet unknown modes.

That existing, competing modes fail is a matter of history. Part II below will categorize them under the headings of Platonic insight, innate or intuitive ideas and oracular revelation. A heading-by-heading survey will argue that none have succeeded. Their uniform failure may not be immediately apparent since each claims successes. These claims should be discounted since a single mode of failure afflicts all of them. Once empirical investigations have a result well-secured by experience, these modes can reproduce it. However, if these modes seek results not already affirmed by experience, they fail, routinely and reliably. They are only wise after the fact.

Their failure is inevitable because they cannot draw on processes that can inform them of contingent propositions in the world, other than the experiential processes of empiricism. There is no clear articulation of the means through which Platonic insight can inform us or through which our minds can somehow intuit contingent facts in the world. This failure stands in contrast with the success of small-e empiricism, which finds its experiential processes in the ordinary ontology of science. We can learn empirically of contingent facts of the world through light waves, or X-rays, or infra-red waves, or radio waves, or sound waves, or the diffusions of molecules in chromatographic media. We can do so for the simple reason that they are all real processes.

This difference in turn gives small-e empiricism a unique capacity to reaffirm its results or to correct errors in them. For the processes it employs are themselves open to investigation and scrutiny. Whether faster-than-light neutrinos were really found in CERN in 2011 can be and was checked by reviewing step by step the physical processes that seemed to affirm the result. These other modes have no such capacity for affirmation and correction. We must just accept that they work when they do and that they fail when they do, without any deeper account of why they worked and failed when they did.

As to the possibility of other as yet undiscovered modes, as long as they remain undiscovered, they are irrelevant to any well-developed philosophy, since so little can be positively asserted of them. It is hard to harbor optimism for their discovery. Millennia of efforts to find such modes have failed. Every claim that one has been found eventually fails. That failure is precisely what empiricism has long expected.

4. Against Big-E Empiricism.

There is a quiet appeal in Big-E Empiricism. It asks us to adopt less in our positions and we are thereby exposed to fewer opportunities for error; and it asks us to limit acceptance to what it supposes is the most secure parts of our science, that which we directly experience.

This appeal should be resisted. The most important reason, in my view, is that it is needlessly skeptical. We can secure absolute security in the positions we adopt by the severe method of adopting none. Then we will never err. The cost, however, is that we will never be right. We need to steer a middle course between complete skepticism and complete credulity. When it comes to being informed of contingent truths, we have one, and only one, resource to guide us: inductive inference from experience. We arrive at a conservative but still rich compromise between the two extremes by favoring propositions that are strongly supported inductively. We can thereby build a secure repertoire of propositions, such as populate our mature sciences. We are assured that each individually is unlikely to be wrong, for that is what strong inductive support means. To do otherwise, as I noted above, is possible, but it is irrational in the strict sense of refusing to accept the import of reason as codified in inductive logic.

This appeal of Big-E Empiricism faces two further problems of a more technical nature. First, the *totality* of experiences is privileged. However *individual* experiences are fallible and more fragile. An empiricism has to provide a means of correcting them when they err. The general strategy of Big-E Empiricism is to draw on many other experiences to correct the one suspected of erring. If however we truly revere experience over all else and harbor an inductive skepticism, as does Big-E Empiricism, we should just accept the anomalous experience as it is. It may not sit well inductively with these other experiences, but it will not contradict them.

Here, small-e empiricism has the advantage. It can provide a principled basis for doubting and correcting an anomalous experience in the more general facts it has secured inductively. Consider, for example someone's claim to have experienced a successful perpetual motion machine. A Big-E Empiricist might want to dispute the claim by recalling the failure very many other attempts. What is the force of this recollection? There is no logical contradiction between this being a perpetual motion machine, where those many others were not. In contrast, small-e empiricism uses the failure of very many attempts at perpetual motion machines as strong inductive support for the conservation of energy, a general, contingent fact. The novel claim of a successful perpetual motion machine is discounted for contradicting this inductively well supported fact.

The second problem facing Big-E Empiricism is that it has to suppose a clear distinction between experience and the results derived from it. Sustaining such a distinction has proved to be a long-standing problem for empiricisms that require it. Following the discussion above, experience is better represented as a continuous physical process that connects us with the system of interest. We can make a relative judgment of which stages of the process are closer or farther from the system of interest. There is no absolute stage which can be identified a "experience" *simpliciter*, such as Big-E Empiricism needs.

5. The Chapters

The chapters of this work develop at length the themes sketched above.

5.1 Part I. A Selective History of Empiricism

This first part provides a history of empiricist thinking, so that small-e empiricism can be located within the larger tradition. The most important conclusion to be drawn from the history is that, through its two millennia, the notion of empiricism has been malleable. There is no ancient, authoritative text to which all later versions must hold. It is quite unlike, say Platonism, Aristotelianism or even Euclidean geometry. For each, the writings of Plato, Aristotle and Euclid are authoritative. Accounts that differ from them are qualified as, as neo-Platonic, scholastic or non-Euclidean.

Empiricism persists through its history in a looser notion. It is that experience is privileged as the sole basis of our access to the contingent facts of the world. More precise versions of empiricism arise when this looser notion is implemented in larger accounts of how we come to learn facts of world. They fill in the details left open by the looser notion. What are the elements of the account? Ideas? Propositions? What counts as experience? Excited sense organs? Observational or experimental reports? How are they processed? By mental operations? By an abstract logic? How much beyond experience can we learn from experience? Different versions answer these questions differently.

Chapter 2, 3 and 4 will sketch the development of empiricist thinking from antiquity to the later part of the twentieth century. It evolved from the doctrine of a sect of physicians who were roundly decried as quacks in the seventeenth century. The reputation of empiricism was then so poor that the "British empiricists" who founded the modern tradition in empiricism did not themselves use the term to describe their view. Through the course of the nineteenth century, the term was gradually rehabilitated as a doctrine free of medical specifics but centered on the privileging of experience. In the first part of the twentieth century, the term had become so untainted that many leading philosophers casually identified themselves as empiricists, including notably those that worked in the logical positivist tradition.

A full chapter 4 is devoted to van Fraassen's constructive empiricism, since it has had a controlling influence on present conceptions of empiricism. It will be clear from my narrative that I regard his empiricism as a retrograde step. It is not so much an empiricism as a severe form of anti-inductive skepticism. In spite of their excesses, I have more sympathies for the versions of empiricism that developed in the twentieth century prior to van Fraassen's. To me, the most appealing of these earlier versions is Reichenbach's logical empiricism. It is distinctive in adopting a view that we would now categorize as scientific realism. It renounces psychological processes in favor of logical relations. It is overtly fallibilist in using probabilistic relations as the primary relations among propositions.

Chapters 5, 6 and 7 develop specific element in the history that have a special connection with the details of small-e empiricism. Chapter 5 surveys briefly how the so-called "British empiricists" introduced a cognitive notion of experience that I believe should now be abandoned in philosophical writing. Chapter 6 reviews the emergence of the notion of "empirical science." It is looser than a fully developed empiricism in merely emphasizing the importance of empirical evidence as a foundation for science. It has become widespread among scientists and is even universally accepted by them. This looser conception is congenial to small-e empiricism for its emphasis on empirical foundations and for its lack of overt skepticism. Chapter 7 reviews mid-twentieth century formulations of principles of empiricism. They provide a background for comparison with the terse, summary statement of small-e empiricism provided in this work.

5.2 Part II. Small-e empiricism

This part contains the core material of this project. It articulates and defends the doctrine of small-e empiricism. The first four of its chapters establish the framework in which small-e empiricism is defined. They develop and generalize the notion of experience to one that conforms with the practice of present empirical science.

Chapter 8 argues that, to this end, we need to minimize the anthropocentric elements in our notion of experience. We should abandon the still popular treatment in philosophical writing of experience as a cognitive state associated with excitations of our sense organs; and we should also forgo efforts to understand the import of these cognitive states by examining the mental operations that they trigger. For the traditional methods of philosophical analysis, armchair introspection, are poorly suited to these tasks. They are now better handled by independently developed sciences such as empirical psychology and physiology.

More directly, problems that arise through the peculiarities of human sense organs and human mental processing are not problems specific to science. No scientific problem is solved by deciding whether Penzias and Wilson were mistaken in their perception that their microwave antenna walls were grey. What is a scientific problem is deciding whether the 7.35 cm microwaves their antenna detected were of a cosmic origin and sampled from a thermal cosmic microwave background.

To maintain a focus on problems specific to science, small-e empiricism begins its analysis of experience once experience has been given propositional form, such as in a scientific publication; and the import of these experiences is found from an examination of relations of inductive support among propositions.

Chapter 9 argues that an empiricism well adapted to science must employ a concept of experience that allows for purely instrumental detections. For the authority of science does not derive from any special characteristic of human sense organs. Rather, that authority derives from its implementation of a physical process connects us continuously to the system of interest.

Since instrumental sensings employ these same processes, they derive their authority in the same way as do human sensings. When there is an earthquake, both humans and seismographs are excited by vibrations passed to them in a continuous physical process from the hypocenter of the earthquake. Instrumental sensings, however, can deliver results far richer in content and with greater reliability. The inked trail left by the seismograph reading is a more sensitive, more enduring and an objectively better interpretable representation of the event than is the human recollection. Many instrumental sensings do not compete with human sensings but vastly outstrip them. Consider LIGO, the Laser Interferometer Gravitational Wave *Observatory*. It *observes* black hole coalescences through a continuous physical process, gravitational waves, whose detection is completely precluded to direct human sense perception.

This general idea of the superiority of instrumental sensing has taken root in the sciences to great effect. The last century or so has seen a shift from human sensing to instrumental sensing in almost every science. Chapter 9 reviews just a few illustrations of this transition in microscopy, chemistry, astronomy and biology.

Chapter 10 develops the further consequences of the reconception of experience as a continuous physical process that connects us with the system of interest. The first consequence is immediate. There is no strict division between experience and the non-experiential results derived from it. Rather there is only a comparative relation among propositions describing the various stages or the experiential process. Some are closer to the system of interest and others are farther from it. These propositions stand in relations of inductive support: those farther from the systems of interest inductively support those closer to them. The chapter provides examples of these result that employ three different types of physical processes: Galileo's observation of mountains on the moon (electromagnetic processes); the reports of the 1883 Krakatoa volcanic eruption (acoustic processes); and forensic chains of custody (processes of physical transport of samples).

A further consequence of experience as a process is the provision of a definite means of affirming or correcting experiential reports that may be erroneous. This is the process of "winding back." It allows us not merely to suspect that some experiential report errs, but to reaffirm it or to diagnose how it came to err. Through it we wind back through the various stages of the experiential process away from the system of interest. In so doing we check the inductive support for propositions describing stages closer to this system by those farther from it. Chapter

10 provides two detailed examples: Fermi's mistaken claim of the discovery of element 93 and Hubble's erroneous estimate of galactic distances in his 1929 report of the recession of the galaxies.

The prospects for winding back are limited, however, by the availability of records. Two examples illustrate the problem. Michelson's 1920s measurements of the speed of light were tainted by then unrecognized seismic disturbances corrupting their distance measurements. The steady stream of reports of mysterious unidentified flying objects or, as they are later called, unidentified anomalous phenomena, defy ready interpretation for a chronic lack of rich enough documentation.

Chapter 11 examines what I call the "terminal obsession." It is the idea that experiential processes terminate at the observer end in some stage of pure experience. Such a stage would capture our experience of the condition of the system of interest, without any intrusion from our other theoretical ideas. That there is such a stage is important for Big-E Empiricism since it posits a strict division between the experiential and non-experiential content of scientific theories; and it endows the cleanly demarcated experiential content with special epistemic powers.

This terminal obsession has been an enduring source of trouble for empiricism. Its proponents have struggled, repeatedly and unsuccessfully, to identify precisely this purely experiential terminus. Advocates of various forms of the thesis of theory ladenness of observation have responded that no such terminus is identifiable because, they assert, experience and theory is indissolubly intertwined. Critics use the thesis to impugn the authority of experience.

Once the notion of experience as a continuous process is adopted, these problems disappear. Small-e empiricism discards the assumption of Big-E Empiricism of a strict division between the experiential and non-empirical content of science. The stage of an experiential process furthest from the system of interest is no different from other stages. Its reporting can be subjected to evidential scrutiny as can any other stage. The scrutiny might employ a tracing back of the experiential process to stages closer to the system of interest or comparison with the reporting of other experiential processes connected with the same system of interest.

Skeptical theses concerning the theory ladenness of observation also fail. Much of the argumentation for them is poor in depending on tendentious analogies and metaphors. The positive difficulty is the treatment of “theory” as an undifferentiated mass that pervades experience. A better analysis considers individual theoretical terms as they appear in propositions describing the stages of the experiential process. The operation of winding back allows us to regress to a stage prior to the introduction of any specific theoretical term, so that the term is no longer involved in the associated theory choice. Since Einstein’s discovery of special relativity is a favorite example of this literature, I show how a more careful treatment of the history of the discovery shows how easy it was to wind back and find expressions for key experimental results free of troublesome theoretical terms.

Chapter 12 draws together the elements of small-e empiricism from the earlier discussion of a revised notion of experience, formulates a summary statement of small-e empiricism and gives an extended argument for it. The principal content of this chapter has already been summarized above in Section 3, “Defending small-e empiricism.”

The chapter adds further reflections. It reaffirms a traditional commitment of empiricism that restricts science to content that can be inductively supported by experience. This form of the commitment is modest. It renounces the severe form advocated by the logical positivists. They asserted, as meaningless, propositions whose truth cannot be tested against experience. Small-e empiricism has no need to dismiss them all at a stroke as meaningless. Rather, that they cannot be tested against experience precludes them from empirical science.

The principal claims of small-e empiricism pertain to Reichenbach’s context of justification. The content of science consists of those contingent propositions that are supported inductively by experience. Small-e empiricism does not presume a strict separation between Reichenbach’s two contexts, discovery and justification. Rather, they are entangled. The actual processes of discovery in science consist of incremental steps whose directions are guided by small justifications of their correctness. Hence the inductive rationality central to small-e empiricism has an important role in scientific discovery.

Small-e empiricism is formulated in terms of inductive relations of support among propositions. The cognitive state of belief plays no essential role in these judgments.[3] However,

[3] This inverts the approach of subjective Bayesians who first form a probabilistic representation of beliefs and from it derive confirmation relations. This inverted procedure is troublesome since it requires us first to develop a global theory of belief, before we form assessments of

the results of empirical analyses are relevant to our beliefs. Small-e empiricism does not include a well-developed theory of how these relations of inductive support are to be incorporated into the construction of a comprehensive cognitive system of beliefs. The chapter is limited to recalling Hume's maxim "A wise man, therefore, proportions his belief to the evidence."

The chapter concludes with a brief discussion of the traditional debate of great interest to the history of philosophy, the rationalism-empiricism debate.

Small-e empiricism implements a major shift in empiricist thinking in abandoning the epistemic pessimism of Big-E Empiricism in favor of an epistemic optimism. Because of the importance of the shift, Chapter 13 collects the reasons for the shift as given in fragmentary form in earlier chapters and expands on them as needed. There are four. First is the failure of Big-E Empiricism to identify a clean division between experiential and non-experiential content in science. Second is that an excessive deference to the authority of *individual* experiences neglects that each is corrigible and subject to correction by the greater authority of a well-developed and experientially well-supported science. Third, in denying the inductive import of experience for results deeper in a science, Big-E Empiricism advocates inductive irrationality. Finally, through its inductive skepticism, Big-E Empiricism forgoes an ontology of science that is itself capable of vindicating the authority of experience.

6. History and Philosophy of Science

This volume is conceived as a project in history and philosophy of science. Such a project, in my understanding, is based on a rich exchange between historical and philosophical analysis. That exchange, in his volume, goes largely in one direction. I have tried wherever possible to provide illustrations in the history of science of the various philosophical claims made. This is *not* an exercise in deriving philosophy of science by generalizing from a few historical examples. That is a dangerous practice, since it is all too easy to restrict the analysis to selected examples that conform with our prior prejudices.

The methodology is different. The main claims are supported by philosophical argumentation that is, as much as possible, independent of the history. The history of science provides illustrations and realizations of these claims. They can sometime play a heuristic role in

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confirmation, where these latter are both easier to form and antecedent to rational belief formation.

suggesting philosophical claims. The principal burden remains of securing support for the claims by independent argumentation.

Without these historical illustrations, it is too easy to drift off into pleasant but untethered speculations in our philosophical analyses. They might be supported by oversimplified, idealized narratives of how science has proceeded or even by convenient fictional histories. To illustrate the narrative's philosophical claims with real examples from the history of science is a powerful control on such excesses. It is a form of self-discipline whose use I encourage. It is hard work. The real practice of science is complicated and messy. A full appreciation of all its subtleties requires diligence, dedication and a willingness to develop a rich understanding of the science itself.

A similar self-discipline has led me to quote extensively from the literature in philosophy and philosophy of science itself. Once again, in repeated assessments and reassessments of it, it is too easy to drift off into convenient caricatures of what that literature has asserted. An effective self-discipline is to illustrate, copiously, claims attributed to the literature by quoting the words of those who proposed them. I apologize to readers who will find the volume of quotation burdensome. There is a good reason for the quoting. I also hope that it helps readers to stay close to the actual claim made in the literature.

7. General Reflections

My goal in this volume is to restore the place in philosophy and philosophy of science of the core idea of empiricism, that experience is our sole means of accessing contingent facts of the world. I seek to provide a version of empiricism that implements this core idea in a manner that is well adapted to modern science and affirms that modern science has been enormously successful in learning contingent facts from experience that extend well beyond it. My hope is that a revival of empiricist thinking will correct trends in philosophy of science and in philosophy that have drifted away from a respect for experience. I look forward to the day that the doctrine will cease to be treated warily as an idea to be tolerated and not acted upon, but as a useful ally in developing richer and stronger philosophical analyses.

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Part I

A Selective History of Empiricism

2. The Early Empiricist Tradition

Chapter 2

The Early Empiricist Tradition

0. A Selective History

This work advances a version of scientific empiricism, "small-e empiricism," that draws on the empiricist tradition in philosophy of science. This chapter and the others in this Part I will provide an historical overview of that tradition. The task of identifying that tradition appears straightforward, initially. For the unifying theme in empiricist thought is a privileging of experience; and we might expect that tracing the origin and development of this idea would be straightforward. Matters prove otherwise once we look more closely.

The immediate difficulty for writing the history is that the terms "empiricism" and "empiricist" are now employed in ways that contradict their historical usages. Until the later nineteenth and early twentieth century, to be an empiricist was a term often limited to schools of thought in medicine. Its privileging of experience was not a laudable attempt to proceed prudently in investigations. It was denounced as an excuse for the empiricists' ignorance of the fundamentals of the applicable science. Figures that we now routinely include as core to the empiricist tradition, such as Bacon, Hobbes, Locke, Berkeley, Hume and Mill, did not identify themselves as empiricists. Bacon and Mill both actively disparaged what they called "empiricists" or "empiricism."

This aversion to the label of empiricism was inverted by the twentieth century. It became routine for philosophers of science in the twentieth century to identify as empiricists, even though the specifics of their views might vary greatly. The label came to denote a modesty in one's philosophy, a prudent respect for experience. This meant, inevitably, that versions of empiricism proliferated. In short, prior to 1900, there were too few self-avowed empiricists; after 1900 there are too many.

My goal in these historical chapters is to provide some sense of the rich historical background to modern empiricism; and to provide enough of the history to show the place of small-e empiricism within it. My first criterion for selection of material was to seek work in which "empiricism" is endorsed by name; or at least the adjective "empirical" is used. This criterion is incomplete, since it excludes too many figures from the history that later writers have identified as empiricists, even if these writers do so without any apology that those figures would not have accepted the label.

To proceed, then, I had to do what all historians of empiricism end up doing, tacitly or explicitly. We identify a collection of ideas that are central to empiricism and then write a history that reports their origin and development. To this end, I view two theses as playing central roles in empiricism:

(*source*) Experience is the sole way that we can be informed of contingent truths of the world.

(*totality*) The content of experience is all we can know of the contingent truths of the world.

Empiricists use these ideas to sustain polemical assaults on some elements of the contemporary literature. Their targets vary over time. They may be intuitive ideas, or metaphysical posits, or meaningless statements, or even just claims of realism in science.

Empiricism has been developed within two sorts of analyses that are often intermingled. A psychological analysis takes as its basic elements the thoughts and beliefs of a cognizer, the concepts held by the cognizer and the cognizer's habits of mind. A logical analysis is independent of the minds of cognizers. It investigates both deductive and inductive relations among propositions, some of which are identified as more or less closely related to experience.

Small-e empiricism endorses *source* but renounces *totality*; it continues to reject much of metaphysics; and its analysis lies fully within the logical tradition. Among early twentieth century versions of empiricism, Reichenbach's proves to be most congenial for small-e empiricism. Thus, the history seeks to identify the lineage that passes through Reichenbach's work to small-e empiricism; and neglects later developments that diverge from the lineage.

This need to have some conception of empiricism before writing the history means that other modern versions of empiricism encourage histories with different selections and emphases. In modern epistemology, empiricism is still treated with a mix of psychological and logical analysis. For example, the debate between empiricists and rationalists was formulated originally in the seventeenth century in terms of the mental states of cognizers. This formulation is retained in present epistemology. Since the present debate is treated as a continuation of the seventeenth century debate, the modern narratives automatically include a history that traces this lineage.[4] This particular history is unimportant for the concerns of small-e empiricism. For them, as elaborated in Chapter 12, this debate is only of historical interest.

Other formulations of empiricism lead to different histories again. We shall see in Chapter 4 that van Fraassen's well-known constructive empiricism evolved in later writings into a severe inductive skepticism. Someone who accepts this skepticism would be inclined to emphasize the more critical elements in past empiricism. Nancy Cartwright, in works such as Cartwright (1989, 2000), gives her version of empiricism. It seeks empirical support for the concept of causation that plays a foundational role in her analyses. This conception diverges from small-e empiricism, which, we can see on reflection, must assail the idea of a single, universally applicable concept of causation as failed metaphysics. Cobb (2022) writes a history of empiricism heavily informed by Cartwright's conception of empiricism. Cobb's conception of empiricism includes a "realist view of causality" (p. 115) that is uncongenial to small-e empiricism.

These considerations also serve as a prelude to my apologies for the many versions of empiricism that fail to appear in this history. They are not part of the lineage of small-e empiricism. The most noticeable omissions will be some of the empiricisms from the twentieth and twenty-first centuries.

Some are excluded since they are not really forms of empiricism, even though their names may suggest otherwise. Since it invokes the name of Hume, it is tempting to include within the empiricist canon David Lewis' Humean supervenience and the results that depend upon it. In my view, however, Lewis' views were not empiricist, but just the sort of a priori metaphysics that has long been an anathema to empiricists. It is hard to defend the founding of his notion of the Humean mosaic on the idea of spatially localized properties, when quantum theory has repeatedly affirmed the existence of non-separable quantum systems. Lewis' (1986, p. xi) announcement that he is "not ready to take lessons in ontology from quantum physics as it now is" reflects no empiricist sensibilities. It repudiates what has been an established fixture in empirically supported quantum theory since the 1930s. He is right that there is some flux in

[4] See, for example, the treatments given in Markie and Folescu (2023) and Nelson (2005).

studies of the foundations of quantum theory. However non-separability has been so enduring that it is hard to see it vanishing with further developments in quantum theory. Lewis (p. xi) justified pursuing his spatially localized ontology as a means of protecting the idea from assaults by (his emphasis) "*philosophical* arguments," independently of empirical considerations. That affirms that his ontological proposal was not responsive to empirical evidence, even though it asserts fundamental physical facts. It was a part of a philosophical analysis that proceeds contrary to empirical evidence.

More recent contributions to empiricism have not been included in the history since they have a role in the articulation of small-e empiricism. Notable here is the work of James Bogen and James Woodward (1988) and especially that of Nora Boyd (2018). Their work provides a decisive advance on earlier work in empiricism in developing a conception of experience better adapted to science. Boyd's work will figure in the discussion of experience in Chapter 9 and 10 below.

Finally, I offer apologies to readers whose favorite versions of empiricism have been neglected or omitted here.

The remainder of this chapter will recount the development of the empiricist tradition from antiquity to the end of the nineteenth century. Chapter 3 will review the exoneration of doctrines labeled "empiricist" in the first half of the twentieth century through their endorsement by influential philosophers of the time, such as those of the Vienna Circle. Chapter 4 will review van Fraassen's constructive empiricism, but not because it lies in the lineage leading to small-e empiricism. Rather, it is because this version of empiricism became the default view associated with empiricism in the later part of the twentieth century. Chapters 5, 6 and 7 will recount special topics in the history of relevance to the formulation of small-e empiricism.

1. Introduction

In the remainder of this chapter, Section 2 will review the appearance of a view named "empiricism" as a medical doctrine in Greek antiquity. It considered only symptoms of diseases and eschewed investigation of their causes. By the seventeenth and eighteenth centuries in England, the "Empiricks" proceeded as did their ancient namesakes. We shall see in Section 3 that their views were criticized by Francis Bacon and the empiricists themselves were vigorously denounced by the medical establishment as "quacks." It was only in the nineteenth century that the strongly negative connotations of empiricism started to fade. We shall see in Section 4 that a major factor in this transition was Kant's highly influential writings in philosophy. In seeking a philosophy that melded empirical and rationalist elements, Kant used the term "empirical" in a way quite free of its earlier negative connotations. The German language literature rapidly adopted this rehabilitated sense and the English language literature soon followed. Section 5 recounts how nineteenth century historians of philosophy retrospectively applied the term "British empiricism" to the work of earlier British philosophers who did not themselves identify with the term. Section 6, 7 and 8 reviews the work of the leading members of this group. In the writings of Thomas Hobbes and John Locke, we find one of the most consequential developments: the locus of inquiries into nature is shifted to the mental activities, thoughts and conceptions in the minds of the investigators. George Berkeley's curious development was to take seriously the notion that these thoughts could replace nature. Finally, David Hume[5] introduced a powerful, skeptical analysis of induction, causation and more. His are works of philosophical brilliance that rightly continue to draw attention today.

Section 9 recalls the nineteenth century introduction of positivism through Auguste Comte's "positive philosophy." The work of Ernst Mach, reviewed in Sections 10 and 11, is routinely described as "positivism," although Mach himself did not actively seek the label. His view was "sensationalist" and awkwardly close to Berkeley's idiosyncratic idealism. Mach portrayed the totality of the world as consisting just of sensations. We shall see in Section 12 that later philosophers, working in Mach's tradition, attached and secured the label of "positivism" to the tradition. Section 13 sums up the equivocal attitude to empiricism at the close of the nineteenth century. The strict doctrine was still a target for sustained criticism. However, tamed versions were entertainable and there was a begrudging acceptance that empiricism played a part in the success of science. Section 14 offers a synoptic summary.

2. Ancient Empiricism

Different views on how we gain knowledge of the world permeate ancient scholarship. To what extend do we rely on reason and a priori thinking? To what extent do we draw on experience? Many juxtapositions are possible. It is traditional to contrast Plato's use of a priori ideas and mathematical models with Aristotle's astute observations of real processes, especially

[5] "British"—since he was a Scot, but not English.

among living things. When a view privileges experience, it is routine to identify empiricist thinking within it. Empiricism in this looser form is dispersed throughout ancient writings.

A tacit respect for experience, however, falls short of an explicit empiricist philosophy. The sharpest articulation of empiricism as a philosophical position and even the word itself arose within the literature on ancient medicine. A rivalry between what were then called "empiricists" and "rationalists" began in the middle of the 3rd century BCE and a mediating "methodist" position was added over two centuries later (according to Frede, 1985, p. ix). Our best account of the views comes from Galen's second century ACE writing. His opening formulation in *On the Sects for Beginners* has a surprisingly modern ring to it: (Walzer and Frede, 1985, p. 3)

> Some say that experience alone suffices for the art, whereas others think that reason, too, has an important contribution to make. Those who rely on experience [*empeiria*] alone are accordingly called empiricists. Similarly, those who rely on reason are called rationalists. And these are the two primary sects in medicine. The one proceeds by means of experience to the discovery of medicines, the other by means of indication. And thus they have named their sects empiricist and rationalist.

These are extreme views. Galen soon (p. 10) turns to recount another approach, that of the "methodists." It mediates between the empiricist and rationalist position by selectively drawing some ideas from each.

The terms "empiricist" and "rationalist" are much used in later writing in the history of philosophy.[6] Empiricists rely solely on experience. Rationalists employ *a priori* arguments. Galen's usage of the term does not map well onto this later usage. His medical empiricists concentrated on immediate symptoms of ailments and observations of which treatments produced which results. The analysis is restricted to what we can experience. The rationalists were eager to delve beneath these appearances to find the inner workings of living things and thus be able to treat them better. They were quite willing to dissect bodies, even living people, in order to see first-hand the structures and processes within. We would now find these rationalists

[6] See Markie and Folescu, (2023) for a synopsis. Galen continues the quote above to note that empiricists are also called "observational" and rationalists also known as "dogmatics" and "analogists."

closer in their investigative enterprise to the empirically-minded experimentalists in sciences of later eras. We now regard therapies that treat only symptoms as superficial and prefer medicine informed by an understanding of the real processes underlying our ailments.[7]

Perhaps the ancient writer best known, mistakenly, as an empiricist is Sextus Empiricus. The mistake is easily made. His empirical credentials are apparently there in the name by which he is now commonly known. His extant writings advance skepticism, not empiricism. Sextus himself distinguished medical empiricism from his skepticism in his *Outlines of Skepticism* (Annas and Barnes, 2000, pp. 62-64). The "Empiricus" likely designates his affiliation with the medical community of his time, presumably the empirical medical community. According to Bett (2005, p. xiii), he wrote now lost medical works.

His views are not empirical in so far as empiricism requires the privileging of experience as the most secure or even only secure form of knowledge. Sextus Empiricus' skepticism has no such goal. Sextus takes two views: that we have knowledge of the world; and that we do not have knowledge of the world. He seeks to demonstrate in *Against the Logicians* and *Outlines of Skepticism*, that the arguments for and against each of these positions are of equal strength. Thus, we are in no position to decide between them.[8]

Sense-perception is discussed throughout *Against the Logicians*. However, it is not offered as privileged knowledge. Rather it figures in his attempts to impugn the idea that there are "signs." These are items of evidence supporting further inferences, such as the supposed connection of the sign of smoke to a hidden fire. Sense-perceptions, he argues, cannot be shown unequivocally to be such signs. In his *Outlines of Skepticism*, this attack on the veracity of sense-perception itself is repeated. He discounts systematically the veracity of sight, sound, smell and taste: (p. 33)

> … our eyes contain membranes and liquids inside them. Since, then, what we see is not observed without these, it will not be apprehended accurately; for it is the mixture which we grasp, and for this reason people with jaundice see everything as

[7] For lively details of the ancient empiricist and rationalist traditions, see Cosans (1997).

[8] For terse statements by Sextus Empiricus of this goal, see *Against Logicians,* p. 120 and *Outlines of Skepticism,* p. 5.

> yellow, while people with a blood-suffusion in the eye see things as blood-red. And since the same sound appears different in open places and in narrow winding places, and different in pure and in contaminated air, it is likely that we do not have a pure grasp of sound; for our ears have winding passages and narrow channels, and are contaminated by vaporous effluvia which are said to be carried from the region of the head. Further, since certain kinds of matter exist in our nostrils and in the regions of taste, it is together with these, not purely, that we grasp what we taste and smell.
>
> So because of the admixtures our senses do not grasp what external existing objects are accurately like.

This blanket skepticism about sense-perception is noteworthy since it is an early and cogent critique of the privileging of experience by empiricism. It depends on approaching the problem from what we would now find to be a scientifically respectable manner: examining the physical operation our sense organs.

3. Interlude: Empiricism and Quackery

By the seventeenth and eighteenth centuries in England, empiricism had few prominent supporters. Francis Bacon's 1620 *Novum Organum* had no praise for it. He listed empiricism as one of the causes of error and false philosophy (Aphorism 62, p. 68). His parable of the ant, spider and bee (Aphorism 95, p. 105) replicates Galen's contrast of empiricists (ants) versus rationalist/dogmatists (spiders) and mediating methodists (bees) and has no praise for the ants. He criticized empiricism for merely relating experience to experience, where he felt the proper goal should be the discovery of causes and axioms in nature. His complaint still stands today if directed at strict empiricist of the modern era. He wrote: (Aphorism 117, p. 119)

> My way and method, however (as I have often clearly stated and would like to state again), is not to extract works from works, nor experiments from experiments as the empiricists do, but from works and experiments to extract causes and axioms, and again from those causes and axioms to extract new works and experiments, as a legitimate interpreter of Nature.

The denouncing of empiricism and its association with a specific type of medicine persisted well into the eighteenth century. The latter underwent a transition from indicating a merely experientially based approach to medicine to designating ill-informed and fraudulent medical practice and outright quackery. The transition is documented in the eighteenth century literature itself. Anon (1715) commences (p. 7) with a sympathetic recounting of the approach of ancient "Empiricks" that was quite close to Galen's original characterization. It then turns to "Modern Empiricks." They are unqualified and incompetent medical practitioners and frauds. The volume is devoted to a colorful exposé of them. Guybon's (1712) *An Essay Concerning the Growth of Empiricism; Or the Encouragement of Quacks* was similarly forthright in its denunciation of medical quackery and demands for regulation of the medical profession. He categorized "quack" and "empirick" together as the target of his ire (e.g. p.2) and gave this definition: (p.3)

> … by a Quack or Empirick, I mean any and every one of those who pretend to practise Physick without Knowledge of the *Prerequisita* to that most useful but most difficult Art.

The usage continued in the leading journal literature. In the *Philosophical Transactions of the Royal Society, London,* Banyer's (1743) "Postscript Against Empiricism" denounced incompetent medical practitioners under the same label. Priestly's (1775) *Philosophical Empiricism* turns out to be an animated defense against a charge that he plagiarized some experimental results. Aside from the title, the term "philosophical empiricism"[9] appears only once in its pages (p. 59), where Priestly explains that he uses it to label his critic's incompetence in chemistry.

4. The Rehabilitation of the Empirical

This pejorative sense for empiricism persisted well into the nineteenth century. As late as its 1825 edition, Johnson's celebrated English dictionary defined "Empiric" as "A trier; an experimenter; such persons as have no true education in, or knowledge of physical practice, but venture only hearsay and observation only." (p. 615) and "Empiricism" as "Dependence on experience without knowledge or art; quackery." The eighth edition of the *Encyclopedia Britannica*[10] of 1855 still included among several senses of "Empiric" the pejorative: "a common designation for one who enters on practice without regular professional education; and, in

[9] "Philosophical" here likely denoted "pertaining to natural philosophy." The term is better rendered for us now as "scientific empiricism."

[10] *Encyclopedia Britannica* Vol VIII. 1842. Edinburgh: Adam and Charles Black.

general, for a quack, or pretender to medical skill." The entry for Empiric was eliminated by the 9th edition of 1889.

The beginnings of the rehabilitation of empirical approaches are evident in the writing of Immanuel Kant. The term "empirical" (*empirisch*) appears throughout his *Critique of Pure Reason* (1787) and his *Prolegomena to Any Future Metaphysics* (1783). It is used throughout as an adjective, such as in "empirical cognition" (*empirische Erkenntnis*) (1787, p. 136). The term is defined to mean "have their ground in immediate sense-perception" (Kant, 1783, p. 54). Kant's project was to seek some sort of synthesis of the empirical and the rational. In this usage, it does not carry the negative connotations we have seen elsewhere. As far as I can see, it was not used directly by Kant to identify such figures as Hume as "empiricists."

The free and frequent use of the term "empirical" by Kant in his massively influential philosophical writing is novel, or so it seems. On a less than thorough search of prior German language writings, the use of the German *empirisch* is rare and sporadic and it is no special focus of attention. After Kant's free use of the term, *empirisch* was almost immediately treated as a significant and central term in philosophical analysis in his tradition, free of its negative, medically connected connotations.[11]

An immediate manifestation of its rehabilitation is in Georg Mellin's massive, multivolume *Encyclopedic Dictionary of Critical Philosophy*. The second volume (Mellin, 1799) covers only the letters D and E, even though it is over 500 pages in length. The entry for *empirisch* starts with the general definition: (p. 290)[12]

> *a posteriori*, from experience, from behind, *ἐμπειρίκον, empiricum, ex experientia ortum, empirique*, is that intuition or concept that contains sensation (which presupposes the actual presence of the object).

The entry continues for four pages (pp. 290-293) and explores multiple applications of this basic notion. None have the older, negative connotation. The fourth volume, part II, includes a lengthy

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[11] I thank Katherine Brading for urging me to review the German language writing in philosophy from this time.

[12] "*a posteriori*, aus der Erfahrung, von hinten her, *ἐμπειρίκον, empiricum, ex experientia ortum , empirique*, ist diejenige Anschauung oder derjenige Begriff, worin Empfindung (die die wirkliche Gegenwart des Gegenstandes voraussetzt) enthalten ist."

analysis of “empiricism” (*Empirismus*) within the entry on “rationalism,” against which empiricism is presented as a counterpoint. *Empirismus* is defined as: (1802, p. 758)[13]

> The assertion … that all our knowledge must be derived from experience and comes to us through the senses, is called empiricism, and if it admits no rational knowledge at all, it is called *pure empiricism*.

Mellin’s dictionary was limited in scope, if we are to accept the statement of purpose given on the title page of the second volume: “[an] attempt at a comprehensive and complete explanation of the concepts and results contained in Kant’s critical and dogmatic writings.”

As late as the mid nineteenth century there was still a sense that the term *empirisch* was an arcane term of the philosopher’s art. The celebrated, multivolume German dictionary of the brothers Grimm had no separate entries for *empirisch* or *Empirismus* in the pertinent volume (Grimm, 1862). However, related terms did appear in the definitions of the German word *Erfahrung* (experience) and its derivatives. In the entry for *Erfahrung* (p. 793), the brothers note that:

> For present-day philosophers, experience (*Erfahrung*) is a technical expression and synonymous with *empirie, εμπειρια.*

They continue with a reporting in some detail on experience in Kant’s philosophy that includes multiple uses of the adjective *empirisch*. Other entries are terser. For example, *Erfahrungswahrheit* (truth of experience) is defined as *empirische wahrheit* (empirical truth).

These hesitations had passed by the time of the publication of Duden’s German dictionary at the close of the nineteenth century. It supplied terse and modern definitions (1898, p. 86) for *Empirie*, *Empirik*, *Empiriker* as “one who derives knowledge from experience” and gives the adjective *empirisch*.

Whatever hesitations may have been felt by dictionary writers, a benign use of the term *empirisch* entered German language publications almost immediately after Kant’s endorsement. An 1819 treatise on heat (Buquoy, 1819) included *empirisch* in its title that began: *The Fundamental Laws of the Phenomena of Heat, empirically founded, and their meaning…* A later,

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[13] “Die Behauptung hingegen, dass alle unsere Erkenntniss aus der Erfahrung müsse abgeleitet werden , und durch die Sinne in uns komme , heisst der Empirismus, und wenn er gar keine rationale Erkenntniss zu giebt , *der reine Empirismus*.”

mid-century work (Büchner, 1856) that aimed at a popular audience apparently had no reservations about using the term in its title: *Force and Matter: Empirical-Naturphilosophical Studies. In Generally Understandable Presentation.* A final index of how benign the term had become is its casual use by Ernst Mach in the first 1883 edition of the work now known in English as the *Science of Mechanics*. The term appears just once (1883, p. 175) in reporting Kepler's "three empirical (*empirische*) laws for the motion of the planets."

The German language literature led in its rehabilitation of the notion of the empirical. It is a plausible conjecture that this rehabilitation was carried over by English language authors' reading of German philosophy. In any case, by the second half of the nineteenth century, a more familiar understanding of empiricism was sufficiently established in the English language literature for Fleming's 1860 volume on philosophical terminology to give a sympathetic recounting of it and to summarize it as "*Empiricism* allows nothing to be true nor certain but what is given by experience, and rejects all knowledge *a priori*." (p. 157)

5. The British Empiricists

Given the negative connotation of the term "empiricism" in their time, it is no surprise that seventeenth and eighteenth century British philosophers like John Locke, George Berkeley and David Hume did not identify themselves as empiricists. The term "British empiricism" came into later use. We can see the transition in Masson's 1865 *Recent British Philosophy*, which was written when the term "empiricism" was losing its negative connotations. His was a rollicking, polemical work designed to arouse British national pride in its philosophy. To this end, he attributed to Hegel the rebuke that: (1865, p. 3)

> … England was the only country in Europe where the word Philosophy had become synonymous with natural science, where the barometer and thermometer were spoken of as "philosophical instruments," and where a so-called *Philosophical Journal* treated of agriculture, housekeeping, cookery, and the construction of fire-places.

Masson applied the term "empiricism" to the work of British philosophers, even though he acknowledged in a footnote (pp. 36-37) a popular prejudice that associated a scornful ("opprobrious") sense to "Empiric." He used the term "British Empiricism" (such as on pp. 53, 110, 119, 208, 214) for the work of British philosophers spanning centuries. It seems to embrace work by Francis Bacon, Thomas Hobbes, John Locke, George Berkeley, David Hume and more figures from the nineteenth century.

It is unclear to me whether Masson (p. 214) represented John Stuart Mill as a British Empiricist. Mill himself would not have accepted the empiricist label. His 1843 *System of Logic* disparaged empiricism. He decried the fallacy of confusing mere empirical laws with true causation: (1843, Vol. 2, p. 416)

> … the distinction is confounded between empirical laws, which express merely the customary order of the succession of effects, and the laws of causation on which the effects depend.

He then equated (pp. 416-17) "bad generalization *à posteriori*" with "empiricism properly so called." Mill's dismissal of empiricism is sharp in his summary hope for a future social science, guided by the principles he has outlined: (p. 612)[14]

> When this time shall come, no important branch of human affairs will be any longer abandoned to empiricism and unscientific surmise: the circle of human knowledge will be complete, and it can only thereafter receive further enlargement by perpetual expansion from within.

The term, "British empiricism," introduced perhaps through Masson's nationalistic pride, lived on, but not with great prominence in the literature. By the time of the mid 20th century volumes of Copleston's history of philosophy, use of the term seems to be common. We find in Volume IV: (1958, p. 15)

> It is customary to divide pre-Kantian modern philosophy into two main streams, the one comprising the rationalist systems of the Continent from Descartes to Leibniz and his disciple Christian Wolff, the other comprising British empiricism down to and including Hume.

The volume then identifies Locke, Berkeley and Hume as central figures (p. 16).[15] Locke is identified (p. 16) as "real father of classical British empiricism."

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[14] This negative appraisal of the first edition of 1843 survived to the last, eighth edition of 1882.

[15] The identification of this trio was already established in the nineteenth century. The University of Michigan library catalog, for example, lists an 1889 Master's thesis, Elisha Monroe Hartman, *The Scepticism of British Empiricism*, and indicates its subjects as the three philosophers, Locke,

6. Empiricism as a Theory of Mental Life

Hobbes (1651) *Leviathan* and Locke's (1689) *Essay* solidified what is, in my view, the most significant transformation of the empiricist tradition. Locke described and took credit for the transformation in his introductory, "Epistle to the Reader." Discussions with five or six friends on an unspecified subject stalled until Locke saw the way forward:[16]

> After we had a while puzzled ourselves, without coming any nearer a Resolution of those Doubts which perplexed us, it came into my Thoughts, that we took a wrong Course; and that, before we set ourselves upon Enquiries of that Nature, it was necessary to examine our own Abilities, and see what Objects our Understandings were, or were not fitted to deal with.

Work in the empiricist tradition so far had concerned a privileging of knowledge of those things in the world most directly accessible to our experience. In the new analysis, the locus shifted from things in the world to the ideas in our heads.

Writing only a few decades earlier than Hobbes and Locke, Francis Bacon (1620) embodied the earlier view. He recommended that inductive investigations begin with tables drawn from experience. To begin investigations of the form of heat, for example, he assembled Tables of Existence and Presence [of heat]; of Absence in Proximity; and of Degrees. Their entries were the starting points in experience for inductive inferences towards the form of heat. They were mind-independent things in the world to which we have ready access in experience. The Tables of Existence and Presence, for example, included (pp. 144-45) "rays of the sun," "fiery meteors" and "burning thunderbolts."

Chapter 1 of Hobbes' *Leviathan* (1651) begins with an examination of mental contents. The chapter is titled "Of Sense" and its opening words are "Concerning the Thoughts of man…" It continues with a recounting of how our various senses produce a mental image that can persist after the object is removed. This is called by Hobbes (p. 5), "imagination" and, as it decays, it is

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Berkeley and Hume.
https://search.lib.umich.edu/catalog/record/990127465090106381?query=Scepticism%20of%20British%20Empiricism&utm_source=lib-site-search

[16] The Epistle is unpaginated. The text quoted is on its third page.

called "memory." What now follows is the completed redefinition of experience as purely mental (Hobbes' emphasis):

> Much memory, or memory of many things, is called *Experience*.

The text continues to explore how the mind uses these imaginations. Simple ones are combined to produce compounded imaginations, such as when the sights of a man and of a horse are compounded to a Centaur. The text passes through "Traynes of Thought, or Mentall Discourse" (p. 9) to ever more elaborate imaginations. The ambition appears to be to recover all mental content from such operations. We can gauge the extent of the ambition from its concern to embrace the notion of infinity, which we would expect by its nature to be beyond the simple compounding of experiences. Hobbes' addressed the issue squarely by denying that an idea of it can be formed in our minds: (p. 11, his emphasis)

> Whatsoever we imagine, is *Finite*. Therefore there is no Idea, or conception of any thing we call *Infinite*. No man can have in his mind an Image of infinite magnitude; nor conceive infinite swiftness, infinite time, or infinite force, or infinite power. When we say any thing is infinite, we signify onely, that we are not able to conceive the ends, and bounds of the thing named; having no conception of the thing, but of our inability.

Hobbes then immediately gave a similar analysis for that for which, in Hobbes' formulation, "the Name of *God* is used." (his emphasis)

Locke's (1689) *Essay concerning Human Understanding* developed similarly. His founding theme is that all ideas come from experience. This theme is pursued with greater vigor than did Hobbes. The mind is like a blank sheet of paper[17] upon which experience writes: (p. 67, Locke's emphasis)

> Let us then suppose the Mind to be, as we say, white Paper, void of all Characters, without any *Ideas*; How comes it to be furnished? Whence comes it by that vast Store, which the busy and boundless Fancy of Man has painted on it, with an almost

[17] This representation of the mind as white paper was almost immediately absorbed into an existing tradition in which the mind is represented as a blank slate—*tabula rasa*. Locke does not use the term here in this *Essay* but does use it elsewhere in other writings. For an account, see Duschinsky (2012).

> endless Variety? Whence has it all the Materials of Reason and Knowledge? To this I answer, in a word, from *Experience*: In that, all our Knowledge is founded; and from that it ultimately derives itself.

Locke then explained how we arrive at the totality of all our idea from experience (p. 69, his emphasis):

> The Understanding seems to me not to have the least Glimmering of any *Ideas*, which it doth not receive from one of these two. External objects furnish the Mind with the Ideas of sensible Qualities, which are all those different Perceptions they produce in us: and the *Mind furnishes the Understanding with* Ideas *of its own Operations.*

What Locke calls "primary qualities" are the properties of the bodies that produce sensations, such as solidity, extension, figure and motion. (p.98). The ideas they produce are called "secondary qualities" (p. 98). They include colors, smells, tastes and sounds. These preliminaries are followed by elaborate attempts spanning many pages of the text to recover many familiar ideas. As with Hobbes, the idea of infinity attracts special attention. It is not obvious how it can be derived from experience. Locke's analysis is labored and fills a thirteen page Chapter XVII. The conclusion is similar to Hobbes'. We can have no positive idea of any infinite magnitude, but only the idea of its indefinite extendibility.

7. Berkeley's Idealism

Both Hobbes and Locke presumed that our experiences are caused by independently existing bodies. Once they have relocated experience into our mental activities, might not the privileging of experience suggest to someone that this mental activity is not merely privileged but is the totality of it all? Devoted philosophical analysis has the power to warp our thinking. There is, I believe, no idea so absurd that some philosophers, somewhere have not talked themselves into it. George Berkeley fulfils this expectation. He argued in his *Principles of Human Knowledge* (1734) that all that exists are perceptions in minds: (p. 25)

> It is indeed an opinion strangely prevailing amongst men, that houses, mountains, rivers, and in a word all sensible objects have an existence natural or real, distinct from their being perceived by the understanding. … For what are the forementioned objects but the things we perceive by sense, and what do we perceive besides our

> own ideas or sensations; and is it not plainly repugnant that any one of these or any combination of them should exist unperceived?

He continued to introduce an eternal spirit as a catch-all perceiver who perceives when we do not: (p. 26)

> … all those bodies which compose the mighty frame of the world, have not any subsistence without a mind, that their being is to be perceived or known; that consequently so long as they are not actually perceived by me, or do not exist in my mind or that of any other created spirit, they must either have no existence at all, or else subsist in the mind of some eternal spirit :…

We need not linger here to discern the theological motivations that made the notion appealing to the Bishop. We can certainly understand the theological appeal of the idea that all existence arises from the perception of an eternal spirit. We should also note Berkeley's efforts to dispel the notion that the world is just a figment of humanity's imagination. For Berkeley insists that "real things" are "imprinted on the senses by the Author of Nature." (p. 37) They are unlike the irregular perceptions of our imagination. He reassures us (p.38):

> I do not argue against the existence of any one thing that we can apprehend, either by sense or reflection … The only thing whose existence we deny, is that which philosophers call matter or corporeal substance.

What matters for our purposes is that empiricist thinking can lead to skeptical thinking; and here it has done so in the most extreme manner. The precise form of Berkeley's skepticism, however, is open to historical debate.

8. Hume's Skepticism

Hobbes and Locke's relocation of our understanding of experience to mental processes was important for science. Once it was recognized that mere introspection is insufficient for a reliable science of the mind, an empirically grounded science of psychology could emerge. However, the relocation proved to be harmful for epistemology. For consideration of what we are justified in learning from experience was replaced by introspection on how the mind happens to proceed from one idea to another. Many epistemologists now call this "inference." That our thoughts proceed from this idea to that is no basis for concluding that its arrival is justified. Whether it is justified is a matter of mind-independent logic. Much modern epistemology still uses introspection to comprehend an inference as a habit of mind, where these efforts should properly now be pursued by empirically-grounded psychology.

It is to David Hume's credit that his *Treatise* (1739) and later *Enquiry* (1777) revived the consideration of the justification of the connections made by the mind. Its revival is to be welcomed, even if his notorious, skeptical conclusion vexed philosophers for centuries.

Hume's overall framework was that of Hobbes and Locke. His overt concern was how the mind forms ideas and proceeds to other ideas. What became known as "Hume's fork" is a development of the two modes of formation of ideas just sketched in remarks from Locke above. That is, in his *Treatise*, Hume distinguished "Relations of Ideas" from "Matters of Fact" (1739, p. 25). However now Hume sought to determine the basis on which we should accept each of these.

Relations of ideas are the simpler case. They are discoverable by the mere operation of thought. An example is arithmetic. We need no experience of the world to know that three times five is equal to half of thirty. Such relations of ideas are demonstrably certain. Their falsity would imply a contradiction. We are justified in accepting them.

Matters of fact are otherwise. They may be true or false. Without experience, we cannot know which. His example concerns the proposition that the sun will rise tomorrow. Its falsity would imply no contradiction. Hume then formulated the question whose skeptical answer became the most celebrated part of his analysis: (1739, p. 26)

> … what is the nature of that evidence which assures us of any real existence and matter of fact, beyond the present testimony of our senses, or the records of our memory.

Our senses tell us the sun rose today. How are we assured of the matter of fact that it will rise tomorrow? To proceed, Hume assumed what is, to many modern readers like me, an excessive restriction on how we may learn matters of fact: (p. 26)

> All reasonings concerning matter of fact seem to be founded on the relation of Cause and Effect. By means of that relation alone we can go the evidence of our beyond memory and senses.

We are inclined by a habit of mind to accept that the fire that has always burned us will continue to do so; and that the bread that always nourished us will continue to do; and that the sun that has always risen will continue to do so. We feel justified in these acceptances since just such acceptances in the past have met with success. We are mistaken in this feeling, Hume claimed famously, for this reasoning is circular: (1739, p. 36)

> We have said that all arguments concerning existence are founded on the relation of cause and effect; that our knowledge of that relation is derived entirely from experience; and that all our experimental conclusions proceed upon the supposition that the future will be conformable to the past. To endeavour, therefore, the proof of this last supposition by probable arguments, or arguments regarding existence, must be evidently going in a circle, and taking that for granted, which is the very point in question.

Hume's analysis vexed his philosophical readers. Most famously, Immanuel Kant (1783, p. 7) credited it for "interrupt[ing] my dogmatic slumber." After a hiatus in the nineteenth century, Hume's analysis was revived and generalized in the early twentieth century as one of the most recalcitrant philosophical problems.[18]

9. Nineteenth Century Positive Philosophy: Auguste Comte

While its proponents do not label themselves as "empiricists," the nineteenth century tradition following Comte's work is closest to that century's implementation of the ideas characteristic of the empiricist tradition. Auguste Comte's *Cours de Philosophie Positive* is a massive, six volume work, published over thirteen years from 1830 to 1842, that sought to give a synoptic picture of the entirety of then present science. Its importance was widely recognized. Martineau's (1880) translation went through multiple editions in England and the US. John Stuart Mill (1866) wrote an entire monograph on it: *Auguste Comte and Positivism*. This monograph also went through multiple printings.

Comte's *Cours* is best remembered for the account of the growth of sciences that was presented in the opening pages of Volume 1, Chapter 1. Each science, he asserted, passes through three states: theological, metaphysical and positive. They are described as: (1830, pp. 3-4; translation Martineau, 1880, p. 26)

[18] For a brief survey of the history of Hume's problem of induction, see Norton (2024, Ch. 6). There I argue that the material theory of induction escapes the modern formulation of Hume's problem since the theory has no universal rules of inductive inference.

> In the theological state, the human mind, seeking the essential nature of beings, the first and final causes (the origin and purpose) of all effects—in short, absolute knowledge—supposes all phenomena to be produced by the immediate action of supernatural beings.
>
> In the metaphysical state, which is only a modification of the first, the mind supposes, instead of supernatural beings, abstract forces, veritable entities (that is, personified abstractions) inherent in all beings, and capable of producing all phenomena. What is called the explanation of phenomena is, in this stage, a mere reference of each to its proper entity.
>
> In the final, the positive state, the mind has given over the vain search after absolute notions, the origin and destination of the universe, and the causes of phenomena, and applies itself to the study of their laws—that is, their invariable relations of succession and resemblance. Reasoning and observation, duly combined, are the means of this knowledge. What is now understood when we speak of an explanation of facts is simply the establishment of a connection between single phenomena and some general facts, the number of which continually diminishes with the progress of science.

This account contains the two ideas central to positivism.[19] First, the burden of the sciences is merely to discover the connections between phenomena. Second, these connections are the totality of the science. Forces, causes, entities and the like are merely transitional conceptions on our way to the positive account. Here "metaphysics" has an extremely broad meaning and seems to cover anything extending beyond phenomena.

10. Mach's Economy

The positivist approach was pursued by others in the nineteenth century. The work of Richard Avenarius and Ernst Mach seems to have attracted most attention. Mach's prominence outlived that of Avenarius, presumably because of Mach's prodigious contributions in other fields and especially for the influence of his writings on Einstein. We shall see that Mach's

[19] Comte himself calls his view "positive philosophy" ("*la philosophie positive*.") The term "positivism," however, came rapidly into use as the title of Mill's (1866) commentary shows.

positivism was an extreme form of skepticism. It disavowed all counterfactuals in science. It prohibited assertions of what would have happened if circumstances had been different. That skepticism is underwritten by some version of idealism. The totality of the world resides entirely in sensations, wheras there are no sensors.

Mach's memorable formulation of the approach is terse. "Physics is experience arranged in economical order," he asserted in his 1882 address "The Economical Nature of Physical Inquiry." (1898, pp. 186-235) He made clear just how sparse is this economical arrangement with a simple example: (p. 193)

> … to save the labor of instruction and of acquisition, concise, abridged description is sought. This is really all that natural laws are. Knowing the value of the acceleration of gravity, and Galileo's laws of descent, we possess simple and compendious directions for reproducing in thought all possible motions of falling bodies. A formula of this kind is a complete substitute for a full table of motions of descent, because by means of the formula the data of such a table can be easily constructed at a moment's notice without the least burdening of the memory.

Mach denounced Newton's conception of Absolute Space and Absolute Time as untenable metaphysics. It is the view for which he was and is still widely known. Using his economy of description, these Newtonian conceptions were to be replaced by relations among observables. Claims concerning times are, in Mach's reconception, merely statements of relations among observables. The various positions adopted by a rotating earth serves, for example, as a convenient reference. He argued in his *Science of Mechanics:* (1919, p. 223, Mach's emphasis)

> When we say a thing A changes with the time, we mean simply that the conditions that determine a thing A depend on the conditions that determine another thing B. The vibrations of a pendulum take place *in time* when its excursion *depends* on the position of the earth.

No such reconception is possible for claims concerning Newton's Absolute Time. Its fate is sealed: (p. 224)

> This absolute time can be measured by comparison with no motion; it has therefore neither a practical nor a scientific value; and no one is justified in saying that he knows aught about it. It is an idle metaphysical conception.

The severity of Mach's approach should not be underestimated. The economical descriptions of science are no more that summaries of actual experiences. To go beyond them is merely a creative embellishment in our thoughts. Thus, he said: (1898, p. 199, my emphasis)

> Suppose we were to attribute to nature the property of producing like effects in like circumstances; just these like circumstances we should not know how to find. *Nature exists once only.* Our schematic mental imitation alone produces like events. Only in the mind, therefore, does the mutual dependence of certain features exist.

Here we see a strong affinity to Hume's critique of causation. That this has followed that gives us no basis to infer that this will continue to follow that. That it will be so arises merely as a habit of our thought.

From this perspective, Mach's celebrated analysis of Newton's bucket experiment is not advocating a physical mechanism that couples the water of the bucket to the stars.[20] He remarked: (p. 232)

> No one is competent to say how the experiment would turn out if the sides of the vessel increased in thickness and mass till they were ultimately several leagues thick.

Later authors like Einstein took this to suggest that a fattened vessel wall would manifest the physical mechanism. Yet Mach's point is precisely to deny this suggestion: "No one is competent to say…" he wrote. Lest there be any doubt, he immediately added:

> The one experiment only lies before us, and our business is, to bring it into accord with the other facts known to us, and not with the arbitrary fictions of our imagination.

Here Mach repeated what he asserted in his 1882 address: "Nature exists once only."

11. Mach's Sensationalism

This much of Mach's analysis is already identifiable as a quite extreme view that denies the admissibility of predictions and of counterfactual propositions. A fuller reading of Mach's corpus reveals that his view is still more extreme. Berkeley had urged that the totality is just experience. In his *Analysis of Sensations* (1897), Mach argued for something similar. We are

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[20] My apologies to readers who do not know the details of Newton's thought experiment and Mach's analysis. An account can be found in Norton (1995).

mistaken, he argued, in identifying the contents of the world with the familiar notions of matter, bodies, space and time. They are nothing beyond relatively stable relations—"complexes"—among sensations: (p. 6)

> Thing, body, matter, are nothing apart from their complexes of colors, sounds, and so forth—nothing apart from their attributes.

Also (pp. 7-8):

> The physiology of the sense, however, demonstrates, that spaces and time may just as appropriately be called sensations and colors and sounds.

Mach's critique of Newton's Absolute Space and Time gains some credibility from Einstein's later praise for it. However, unlike Einstein's discovery of special relativity, Mach's critique is not grounded in any significant, factual discovery. Mach's critique is entirely the result of his *a priori* reflections on sensations. These same extremist reflections led to Mach's opposition to the atomic theory: (p. 152, Mach's emphasis)

> If ordinary "matter" must be regarded merely as a highly natural, unconsciously constructed mental symbol for a complex of sensuous elements, much more must this be the case with the artificial hypothetical atoms and molecules of physics and chemistry. The value of these implements for their special, limited purposes is not one whit destroyed. As before, they remain *still* economical symbolisations of the world of experience.

Mach was uncompromising in this reduction of the totality to senses. Prior to this dismantling of bodies, space and time, Mach had already dismantled the notion of the self as something fundamentally distinct from the larger collection of sensations: (p. 3)

> As relatively permanent, is exhibited, further, that complex of memories, moods, and feelings, joined to a particular body (the human body), which denominated the "I" or "Ego."

Body and ego, Mach soon averred, are "makeshifts": (p. 11)

The idea that the self is merely a relatively stable collection of sensations conforms with the analyses of Locke and Hume. Where Mach advanced the notion at the outset of his analysis and thus gave it great prominence, Locke and Hume were slower to present the view. Much later in his text, Locke (1689, pp. 292-93) had argued that personal identity resides not in a substance, but in consciousness. Socrates asleep and Socrates awake are not the same person, since their consciousnesses are distinct. Similarly, Hume opposed the conception of a soul as delimiting the self. The self is instead (1739, p. 252) “nothing but a bundle or collection of different perceptions, which succeed each other with an inconceivable rapidity, and are in a perpetual flux and movement.”

There can be no doubt of the strength of Mach’s commitment to the notion that there is nothing beyond sensations in the world. He wrote “The assertion, then, is correct that the world consists only of our sensations” (1897, p. 10) and a startling “… the ego can be so extended as ultimately to embrace the entire world.” (p. 10)

Such extreme claims leave us unsure of just what is a “sensation.” The ordinary meaning of the term requires a sensor in which the sense arises and something else that is sensed. While Locke retained those components, Berkeley eliminated the something else sensed. To preserve existence when there is no human sensing, Berkeley called upon the sensing of an “eternal spirit.” Mach resorted to no such device. Without sensors and things sensed, we may well wonder how the term “sensation” can be used at all. (Mach, 1897, p. 18) recognized the problem and proposed to replace “sensations” by “elements”:

> Usually, these elements are called sensations. But as vestiges of a one-sided theory inhere in that term, we prefer to speak simply of elements, as we have already done. The aim of all research is to ascertain the mode of connexion of these elements

Analysts more sophisticated than me can perhaps understand what Mach intends these elements to be. Mach lists “colors, sounds, temperatures, pressures, space, times and so forth…” (p. 2) as examples of sensations. If there is no sensor, what are they? What is a color without the eyes that sense it? Why are these the fundamental constituents of nature if there are no sensors to privilege them?

It was already noted above that Hobbes, Locke and those who followed them conflated the study of the operation of the mind with an analysis of justification in epistemology. The first is properly advanced as an empirical science and second by traditional philosophical analysis. This conflation is present is the most extreme form so far in Mach’s writings. The remarks quoted above identify the world as nothing but sensations. They come in the opening pages of a work, the bulk of which is devoted to the physiology of our sense organs and the mental processes associated with them. These parts of the work, like so much of Mach’s corpus of writings, are a contribution to physiology and psychology.

Mach did not keep the two separated.[21] One example illustrates the lack of separation. His 1897 lecture, "On Sensations of Orientations" (1897, pp. 282-308), reports his analysis of how we living beings orient or balance ourselves in space. It includes astute observations of the experience of vertigo; laboratory experiments of Mach's design, in which subjects were spun in a cradle; and analysis of the physiology of the semi-circular canals in our ears that give us a sense of balance. In the midst of his analysis, Mach reverted to his opposition to Newton's concept of Absolute Space: (pp. 289-90)

> Had Newton known them [our sometimes spurious inner sense of motion] and had he ever observed how we may actually imagine ourselves turned and displaced in space without the assistance of stationary bodies as points of reference, he would certainly have been confirmed more than ever in his unfortunate speculations regarding absolute space.

Since these perceptions are immaterial to Newton's arguments for Absolute Space, it is hard to see the cogency of Mach's remark. They are cogent, however, if one fails to separate psychology from epistemology.

12. Positivism

While neither Richard Avenarius nor Ernst Mach identified themselves as positivists or their views as positivism,[22] they rapidly became known as such. Moritz Schlick, in his 1918 *General Theory of Knowledge [Allgemeine Erkenntnistheorie]* freely associated both with positivism as if it is no novelty to his readers. He wrote, for example, of the "Avenarius-Machian positivism" (p. 261). The attachment of the positivist label had begun over a decade earlier. Joseph Petzoldt's 1906 *The Problem of the World from the Positivist Standpoint [Das Weltproblem von positivistischen Standpunckt aus]* is dedicated to Ernst Mach "in gratitude and loyalty." Its preface (p. v) attributes the foundation of the "positivistic conception" to Wilhelm Schuppe, Ernst Mach and Richard Avenarius.

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[21] Mach surely was aware of the merging of the two fields and presumably, but incorrectly, thought it admissible.

[22] Mach does accept the label "positivism" when applied to his ideas by Max Planck. See Bradley (2013, p. 206) for details.

That the later Vienna Circle should adopt the label "positivism" in their logical positivism is not a surprising development. Positivism had rapidly become a philosophical movement in Germany. A Society for Positivistic Philosophy [*Gesellschaft für positivistische Philosophie*] had been formed and had published the first issue of its *Journal for Positivistic Philosophy* [*Zeitschrift für positivistische Philosophie*] in 1913. Its first issue begins on page one with a paper, "Positivistic Philosophy," by Petzoldt (1913). It was an extended text of the lecture he had given at the opening session of the Society on November 11, 1912.

13 Empiricism at the Start of the Twentieth Century

When the twentieth century began, the doctrine that carried the name "empiricism" was still, in some quarters, in a transition from its earlier, widely shunned formulation to something more respectable. An enlightened 1909 encyclopedia entry still captured the sense that empiricism had been an extreme and untenable view. Now the weakness reported may have reflected Kantian reservations about a pure empiricism without rationalist elements, as opposed to the earlier connection to medical quackery. The basic doctrine was summarized tersely as: (United Editors Association, 1909, no page number, entry "Empiricism")

> The doctrine that all knowledge is gained through or based on experience.

It proceeded immediately with a most welcome distinction that indicated that, at least for this entry, the new science of psychology was distinguished from the philosophers' epistemology. "Empiricism in psychology" pertained to the contents of consciousness. A different sense of empiricism applied "in logic and epistemology" where "the sole test of truth is verification in experience." Bacon, Locke, Berkeley, Hume and Condillac were identified as empiricists and their views deprecated as:

> … extreme absurdities … refuted by Kant … who introduced a new view called "critical Empiricism" or "Criticism"…

The article then identified a tamed empiricism whose name could be attached retrospectively to the work of philosophical luminaries of the past century:

> Since Kant's time, extreme Empiricism has been thought untenable, and by Empiricism now is usually meant the attitude of those who regard actual sense experience and concrete scientific observations and experiments as more important tests of truth than purely logical tests, such as consistency and comprehensiveness of thought.

> Taking Empiricism in this last sense, among the chief empiricists of the last hundred years may be mentioned, J. S. Mill, W. S. Jevons, Venn, K. Pearson, and L. T. Hobhouse. Auguste Comte and Herbert Spencer may also be reckoned as empiricists, while William James is a "radical empiricist ."

We can see one manifestation of this transition in the critique of empiricism mounted by Henry Sidgwick, who harbored some Kantian sympathies. He was, in his time, a prominent philosopher and economist at the University of Cambridge, where he worked until his death in 1900. The burden of his "Incoherence of Empirical Philosophy" (Sidgwick, 1882) is clear from its title. His principal complaint against empiricism is that inductive inference is too impoverished to carry us fully as empiricism required from experience to the knowledge contained within the science of his time. His later "Criteria of Truth and Error" (Sidgwick, 1900) gave a vivid and terse summary of his complaints. He provided a quite serviceable description of empiricism: (p. 15)

> I take the principle of Empiricism, as an epistemological doctrine, to be that the ultimately valid premises of all scientific reasonings are cognitions of particular facts; all the generalisations of science being held to be obtained from these particular cognitions by induction, and to depend upon these for their validity.

His denunciation of empiricism followed immediately:

> I do not accept this principle I think it impossible to establish the general truths of the accepted sciences by processes of cogent inference on the basis of merely particular premises; and I think the chief service that J. S. Mill rendered to philosophy, by his elaborate attempt to perform this task, was to make this impossibility as clear as day.

Sidgwick's then solidified his critique of empiricism by reaffirming his confidence in scientific knowledge. Empiricism, he could now conclude, lacked the means to underwrite this confidence. His 1882 "Incoherence…" paper ended with this point (p. 543):

> If, finally, the reader who has got through this paper should say that my cavils cannot shake his confidence in experience, or in the aggregate of modern knowledge that has progressed and still progresses by accumulating, sifting, and systematising experience—I can only answer that my own confidence is equally unshaken. The question that I wish to raise is not as to the validity of received

> scientific methods, but as to the general epistemological inferences that may legitimately be drawn from the assumption of their validity. It is possible to combine a practically complete trust in the procedure and results of empirical science, with a profound distrust in the procedure and conclusions especially the negative conclusions—of Empirical Philosophy.

A similar conclusion was added in an appendix to 1900 "Criteria…" paper in a collection of his papers (Sidgwick, 1905, p. 461, his emphasis):

> On the whole, then, I have to reject the claims of Empiricism no less than of Rationalism to put forward a simple infallible criterion for the kind of knowledge which is to be taken as the ultimately valid basis of all else that is commonly taken for knowledge. I regard both criteria as *useful*, as a means of guarding against error, but neither as infallible.

Something close to Sidgwick's critique, we shall see in the next chapter, reappears in 1915 in an influential work by Bertrand Russell.

14. Conclusion

With the close of the nineteenth century, the general idea that experience is an important source of knowledge for science was widespread. Works that we identify now as lying in the empiricist tradition, such as those of Hume and Mach, espoused quite strong forms of skepticism. The terms "empiricist" and "empiricism" retained lingering negative connotations and were adopted or endorsed only hesitantly. Hume's skepticism roused Immanuel Kant to his influential critique. It was Kant's free use of the term "empirical" in this critique that, I believe, played a decisive role in separating empiricism from its earlier, negative connection to medical quackery. Kant, however, mounted a direct challenge to empiricism with the claim that we possess synthetic, a priori knowledge. It consists of contingent factual propositions ("synthetic") whose truth could be known independently of experience ("a priori"). Many found this response to Hume appealing. Kant became one of the most influential—even the most influential—philosopher of the era. His ideas flourished into various forms of non-empirical, neo-Kantian philosophy. The fortunes of empiricist thinking seemed dim. Matters were about to change.

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3. Logical Positivism and Logical Empiricism

Logic and Meaning

Chapter 3

Logical Positivism and Logical Empiricism: Logic and Meaning

1. Introduction

In the twentieth century, the reluctance to use the terms "empiricism" and "empiricist" evaporated. Those who advocated a privilege status for experience were willing to adopt the terms without apology. We shall see below in Section 2 that the members of Vienna Circle in the 1920s embraced the terms with no sense of the past difficulties. Empiricism became the term for the mainstream of thought that privileged experience. The agenda for early twentieth century empiricism was set by the short-lived Vienna Circle, whose ideas are reviewed in Section 2 here. Two were very influential: that empiricism provided the basis for the meaningfulness of propositions; and that formal, symbolic logic was the appropriate medium for precise analysis of empiricism. These two ideas were characteristic for what came to be known as logical positivism.

Much of the ensuing writing in empiricism took the Vienna Circle's approach as their starting point. They incorporated these ideas and wrote in reaction to them. There was much to which to react, for the Circle had extreme views. The most extreme was their verifiability criterion of meaning. It allowed proposition to be meaningful only if they directly expressed experiences or were deduced from experiences. At a stroke, vast portions of normal discourse were reduced to meaninglessness, the equivalent of nonsensical babble.

Within a decade, Rudolf Carnap, a key figure in the logical positivist movement, had already retreated from the severe condition that meaning could only be ascribed to propositions deduced from experience. While many other writers sought to correct these excesses, below, in Section 3, I will recount the efforts of Hans Reichenbach. He formulated what came to be known as logical empiricism. It was a milder, prudently tempered version of the logical positivists' extremism. At its core was the assignment of probabilities to propositions. He continued the logical positivists' focus on meaning. His introduction of a probabilistic theory of meaning meant that he could now treat as meaningful propositions in science remote from experience.

A counterpoint to Reichenbach's comfort with inductive inference was an anti-inductivist tempering of empiricism. It manifested in Bertrand Russell's earlier work of the 1910s and, as Section 4 recounts, found its fullest expression in the work of Karl Popper. While he was also working in Vienna at same time as the Vienna circle, Popper developed his distinctive version of deductive empiricism in which all notions of inductive inference were abandoned. He replaced the logical positivists criterion of testability by falsifiability. It was to demarcate not the meaningful, but the scientific. Unlike the ideas of the logical positivists and empiricists, Popper's analysis along with his name retains a strong presence in modern scientific discourse.

Another expression of empiricism in terms of meaningfulness arose independently at the same time as the logical positivist and empiricist movements and also proved to have a longer-lived influence. This is the operationalism of Percy Bridgman, which is reviewed in Section 5. Concepts were, according to the view, only meaningful if we could specify the operations through which the concept is measured; and that specification exhausts the meaning of the concept. A concept, Bridgman insisted, is synonymous with—that is has the same meaning as--the operations that measure it.

2. Logical Positivism[23]

2.1 The Manifesto

The "Vienna Circle," (*der Wiener Kreis*) as they named themselves, was an informal discussion group centered in Vienna. Their members included philosophers, scientists and mathematicians. In their manifesto[24] (Hahn, Neurath, Carnap, 1929, p. 318), they listed their members as: Gustav Bergmann, Rudolf Carnap, Herbert Feigl, Philipp Frank, Kurt Gödel, Hans Hahn, Viktor Kraft, Karl Menger, Marcel Natkin, Otto Neurath, Olga Hahn-Neurath, Theodor

[23] The secondary literature on logical positivism is enormous. For an entry into the movement overall, see Richardson and Uebel (2022). Of all figures in the Vienna Circle, Carnap has attracted most attention in this literature. For an entry, see Leitgeb and Carus (2022).

[24] The author intended for the manifesto is ambiguous. The forward is signed by Hahn, Neurath and Carnap. However, the typography of the title in the 1929 German edition suggests that the work's title is *Wissenschaftliche Weltauffassung* [Scientific World Conception] with the author *Der Wiener Kreis* [The Vienna Circle].

Radakovic, Moritz Schlick and Friedrich Waismann. The Circle set out not just to articulate a new empiricist philosophy but to form a movement that would redirect the course of philosophy. While the Circle and its members dissipated within a decade, it succeeded in its goal of influencing profoundly the philosophy of the ensuing years.

There were many views held within this large group. Their manifesto, however, collected the claims common to, or at least assented to, by all. That was essential for the manifesto to serve its purpose of recruiting, aggressively, new thinkers to its movement. The views of the manifesto were unified by what it called empiricism. The term appears repeatedly, favorably, throughout the manifesto, which provides a terse and serviceable definition (Hahn, Neurath, Carnap, 1929, p. 308):

> It is precisely in the rejection of the possibility of synthetic knowledge a priori that the basic thesis of modern empiricism lies. The scientific world-conception knows only empirical statements about things of all kinds, and analytic statements of logic and mathematics.

In still terser terms, this just says that empiricism is founded on a rejection of Kantian and neo-Kantian philosophies. There is no trace here of the reservations about empiricism that still lingered elsewhere.[25] Whereas the Circle rejected Kant's philosophy, it retained Kant's rehabilitated conception of empiricism that, as we saw in the last chapter, had become standard in the German language philosophical literature.

The two core ideas of the Circle were summarized as: (Hahn, Neurath, Carnap, 1929, p. 309, emphasis in original)

> We have characterised the *scientific world-conception* essentially by two features. *First* it is *empiricist* and *positivist*: there is knowledge only from experience, which rests on what is immediately given. This sets the limits for the content of legitimate science. *Second*, the scientific world-conception is marked by application of a certain method, namely *logical analysis*.

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[25] Russell's (1915) work, as we shall see below, acknowledged as an inspiration by the Circle, used a version of Hume's problem of induction to conclude (p. 56): "… it affords a refutation of the older empiricists. They believed that all our knowledge is derived from the senses and dependent upon them."

These two features supply the two components of the label "logical positivism" by which the Circle's conception came to be known. The term does not appear in the manifesto and was not favored by Carnap. He remarked only a few years after the writing of the manifesto (Carnap, 1936/37, p. 422)

> [The position of the Vienna Circle] has sometimes been called Logical Positivism, but I am afraid this name suggests too close a dependence upon the older Positivists, especially Comte and Mach. We have indeed been influenced to a considerable degree by the historical positivism, especially in the earlier stage of our development. But today we would like a more general name for our movement, comprehending the groups in other countries which have developed related views. ... The term "Scientific Empiricism" (proposed by Morris …) is perhaps suitable.

The adjective "scientific" is prominent here and in the manifesto. However, its import is left to the reader to discern. There was an indirect rhetorical element. Science and technology in the 1920s were thriving and use of the adjective suggested a modern, forward-looking orientation to be contrasted with the staid viewpoints of those criticized by the Circle. The more direct meaning, however, appears merely to flag the Circle's goal of using the methods of science. Russell's (1915) monograph was repeatedly reported as an inspiration to the Circle. Its subtitle made plain its allegiance to what would be represented in the text as the scientific method: "*Our Knowledge of the External World: As a Field for Scientific Method in Philosophy.*" The only explicit clue in the manifesto itself is the reported agreement among Circle members over: (Hahn, Neurath, Carnap, 1929, p. 304)

> … the specifically scientific attitude: "What can be said at all, can be said clearly" (Wittgenstein); if there are differences of opinion, it is in the end possible to agree, and therefore agreement is demanded.

2.2 Verifiability and the Elimination of Metaphysics

The first of the two core ideas of the Circle, empiricism and positivism, found its strictest expression in the elimination of metaphysics. "The scientific world-conception rejects metaphysical philosophy," they wrote in the manifesto (p. 307). This rejection was not just a dismissal of metaphysics as ungrounded speculation. They intended something more brutal.

Metaphysical notions and propositions were equated with nonsensical babble.[26] Their statement was (pp. 306-307):

> Then it appears that there is a sharp boundary between two kinds of statements. To one belong statements as they are made by empirical science; their meaning can be determined by logical analysis or, more precisely, through reduction to the simplest statements about the empirically given. The other statements, to which belong those cited above, reveal themselves as empty of meaning if one takes them in the way that metaphysicians intend.

This is the verifiability criterion of meaning: a proposition is meaningful just in case there is some way to verify it in experience. It was given a terser formulation, first by Friedrich Waismann in his (1930, p. 229):

> A statement describes a state of affairs. The state of affairs either exists or it does not exist. There is no middle ground, and therefore there is no transition between true and false. If it cannot be stated in any way if a sentence is true, then the sentence has no meaning at all; for the meaning of a sentence is the method of its verification. [... denn der Sinn eines Satzes ist die Methode seiner Verifikation.]

The criterion was reported later by Hans Reichenbach and attributed a lineage that traced back to Wittgenstein: (1938, p. 49)

> This is the idea expressed by Wittgenstein in his formula: the meaning of a proposition is the method of its verification.

In a footnote to this summary formula, Reichenbach reported on its use among the Vienna Circle and its attribution to Wittgenstein:

> Although this formula is not verbally contained in Wittgenstein's *Tractatus Logico-philosophicus* (London, 1922), it expresses his ideas very adequately and has been used, with this intention, within the "Vienna Circle."

[26] That is my rendering. The manifesto had a kinder sounding "say nothing but merely express a certain mood and spirit." (p. 307) Nonsensical babble can do that.

Here Reichenbach repeated the frequent acknowledgment by the Circle of their debt to Wittgenstein. Presumably the idea of the verifiability criterion of meaning is found in Propositions 4.003 of Wittgenstein's *Tractatus* (1922, p. 63):[27]

> Most propositions and questions, that have been written about philosophical matters, are not false, but senseless. We cannot, therefore, answer questions of this kind at all, but only state their senselessness. Most questions and propositions of the philosophers result from the fact that we do not understand the logic of our language.
>
> (They are of the same kind as the question whether the Good is more or less identical than the Beautiful.)

The Circle manifesto is short and gives only brief clues to the metaphysics it targets for elimination: "God", "entelechy" (life force) (p. 306) and "scholastic metaphysics and that of the systems of German idealism, but also the hidden metaphysics of Kantian and modern *apriorism*." (p. 308) Presumably the authors of the manifesto expected its readers to be all too familiar with their targets.

A later paper by Carnap gives a more expansive inventory of their metaphysical targets. Words with no meaning are listed here: (Carnap, 1931, p. 67)

> Just like the examined examples "principle" and "God," most of the other specifically metaphysical terms are devoid of meaning, e.g. "the Idea," "the Absolute," "the Unconditioned," "the Infinite," "the being of being," "non-being," "thing in itself," "absolute spirit," "objective spirit," "essence," "being-in-itself," "being-in-and-for-itself," "emanation," "manifestation," "articulation," "the Ego," "the non-Ego," etc.

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[27] Perhaps readers will forgive me for expressing my frustration at the needlessly obscurity of the *Tractatus.* It lies not it its ideas but in its opaque presentation. It is a book of solutions without the problems stated. To make sense of the project, readers must search for the problems that Wittgenstein neglected to specify. It is akin to Deep Thought's declaration in Douglas Adam's *Hitchhiker's Guide to the Galaxy* (Pocket Books, 1979, p. 180) that "Forty-two" is the answer to the question of life, the universe and everything. It may be so, but necessary details are missing.

Carnap then proceeded to sentences “from that metaphysical school which at present exerts the strongest influence in Germany.” According to his earlier description, they “have no sense, assert nothing, are mere pseudo-statements.” They are: (Carnap, 1931, p. 69, his emphasis and quote marks)[28]

> “What is to be investigated is being only and—*nothing* else; being alone and further—*nothing*; solely being, and beyond being— *nothing*. *What about this Nothing? … Does the Nothing exist only because the Not, i.e. the Negation, exists?* Or is it the other way around? *Does Negation and the Not exist only because the Nothing exists?* … We assert: *the Nothing is prior to the Not and the Negation*. … Where do we seek the Nothing? How do we find the Nothing. … We know the Nothing. … *Anxiety reveals the Nothing*. … That for which and because of which we were anxious, was 'really'—nothing. Indeed: the Nothing itself—as such—was present. … *What about this Nothing?—The Nothing itself nothings.*”

2.3 Anti-metaphysics in Mach and the Vienna Circle

The Vienna Circle made plain its debt to and lineage from the work of Ernst Mach. Moritz Schlick, to whom the manifesto was dedicated, was a guiding spirit; and he held the academic Chair in Vienna previously held by Mach. The manifesto itself was marked as published by the *Verein Ernst Mach*, The Ernst Mach Society, which had also hosted the many discussions of the Circle.

It is easy then to imagine that the antipathy to metaphysics of the Circle and Carnap especially was merely a continuation of Mach’s opposition to metaphysics. That is not so. The Circle’s version was more severe. Mach disparaged many ordinary conceptions, such as body and ego, as makeshifts (Mach, 1897, p. 11). They might function as rough and ready conveniences of a larger reality composed of elements. Their nature, I noted in the last chapter, is mysterious to me. They cannot be sensations, literally, since there in no foundational distinction in the world between sensors and things sensed.

Mach had focused his anti-metaphysical critique on elements of successful, or soon to be successful, science, such as Absolute Space and Time and the notion of atoms. His denunciation of Newton’s Absolute Time was quite specific (Mach, 1919, p. 224):

[28] Carnap’s footnote identifies the sentences as derived from Heidegger’s *Was Ist Metaphysik?*

> This absolute time can be measured by comparison with no motion; it has therefore neither a practical nor a scientific value; and no one is justified in saying that he knows aught about it. It is an idle metaphysical conception.

The notion has no place in a careful, positivist analysis. We can know nothing about it. It is idle but nonetheless still a conception. Talk about it is not babble, as was metaphysical talk in the view of the Vienna Circle.

The Circle and especially Carnap had more immediate targets. Their anti-metaphysical fulminations were aimed in great measure at the abstruse proclamations of the German idealists of the era. Carnap's example was Heidegger's 1929 proclamation in his Freiburg inaugural lecture: "*Das Nichts selbst nichtet."—"The Nothing itself nothings."* His was a battle with present enemies over the excesses of obscure philosophy.

2.4 Reduction to the Given

The elimination of metaphysics constituted the negative, destructive component of the new positivist program. The productive part of the program consisted in a massive reduction of the totality of all scientific knowledge to the given, that is, to experience: (Hahn, Neurath, Carnap, 1929, pp. 309)

> The aim of scientific effort is to reach the goal, unified science, by applying logical analysis to the empirical material. Since the meaning of every statement of science must be statable by reduction to a statement about the given, likewise the meaning of any concept, whatever branch of science it may belong to, must be statable by step-wise reduction to other concepts, down to the concepts of the lowest level which refer directly to the given. If such an analysis were carried through for all concepts, they would thus be ordered into a reductive system, a 'constitutive system'.

While the description of this ambitious project in the manifesto is brief, there would be little doubt about its more detailed form. For just such a reduction has been attempted in Carnap's (1928) *Aufbau*, *The Logical Construction of the World*. Here "construction" describes in reverse the same process as the reduction just envisaged in the manifesto. The construction occurs when we trace back from the given to what was reduced to it. Bertrand Russell's earlier work provided an inspiration. The *Aufbau*'s epigram is (p. 5):[29]

> The supreme maxim in scientific philosophizing is this: Wherever possible, logical constructions are to be substituted for inferred entities. RUSSELL

What results is a system of four levels whose analysis is spread throughout the volume:

- Autopsychological [*eigenpsychischen* = self or proper psychic] loosely, the content of our own minds
- Objects: (Carnap, 1928, p.5) "The word 'object' is here always used in its widest sense, namely, for anything about which a statement can be made."
- Heteropsychological [*fremdpsychischen* = other or foreign psychic] loosely, the content of the minds of others
- Cultural objects [*Die geistigen Gegenstände*] Examples are very broad and include religious ideas.

The first autopsychological level is epistemologically primary and all the rest can be constructed from it. The key relation in this step-by-step construction is reducibility. It is introduced as follows (Carnap, 1928, p.6):

> An object (or concept) is said to be reducible to one or more other objects if all statements about it can be transformed into statements about these other objects.

The ultimate goal of the reduction is replace all the statements of this hierarchy by those that merely use the vocabulary of the first, autopsychological level. It seems that Carnap's intention is for these reduction relations to be deductive; and that makes the aspirations of the reduction especially severe. That this is the intent is, I take it, given in summary form when Carnap (1928 [1961], p. vii], in his introduction to the second edition, conceded that reduction using explicit definitions alone was too severe.

An explicit reason was given repeatedly for reducing everything so severely to the autopsychological, that is, to the content of one's own mind. It is that assertions that go beyond the autopsychological are not factual. In the summary of the Aufbau system in Carnap's Pseudoproblems (1928, p. 339), Carnap gave another reason for the severity of this reduction. It

[29] The epigram is not an exact quote but a paraphrase of similar remarks in Russell (1915, p. vi) and Russell (1924, p. 34).

is the possibility of error if anything larger is used. More precisely, he is concerned that the reduction not stop at "*b*," which is the heteropsychological level of other minds, for otherwise errors can enter. We must continue to "*a*," the "nucleus of the experience," which turns out to be the autopsychological and the objects constructed from it. In his (translated) words: (p. 339)

> … the theoretical content of *b* can rest upon an error even though *a* been correctly recognized.
>
> … For, the scientific justification of the recognition of content *b* always refers to *a*; moreover, on the basis of *a* we can always be deceived about *b*.

While this possibility is not otherwise mentioned, this fear of error would explain why Carnap was determined to implement such a severe reduction that, he acknowledged repeatedly in his *Aufbau*, has the appearance of a solipsism that he does not endorse.[30] It would also explain why Carnap insisted on the dubious requirement that the reduction relations are deductive, for an inductive logical relation would leave open the possibility of error.

2.5 What is Real

The Vienna Circle sought to use this reduction to the given to bring a close to an old debate in philosophy. In their characterization, the debate was between two views, realism and idealism. Realism asserts that there is a world independent of our sensations. This independent world includes everyday notions such as bodies and the minds of others. Idealism asserts that what we take to be this external world is merely a construction of our sensations. The Circle's resolution was radical. They asserted that *both* positions are untenable, since both involve meaningless propositions, that is, propositions that cannot be reduced to the given. Here is how the assertion appears in the Circle's manifesto: (Hahn, Neurath, Carnap, 1929, p. 308; their emphasis)

> In rejecting overt metaphysics and the concealed variety of apriorism, all adherents of the scientific world-conception are at one. Beyond this, the Vienna Circle maintain the view that the statements of (critical) *realism* and *idealism* about the reality or non-reality of the external world and other minds are of a metaphysical

[30] Carnap (1928, p. 101) "The autopsychological basis is also called solipsistic. We do not thereby subscribe to the solipsistic view that only one subject and its experiences are real, while the other subjects are nonreal."

> character, because they are open to the same objections as are the statements of the old metaphysics: they are meaningless, because unverifiable and without content. *For us, something is 'real' through being incorporated into the total structure of experience.*

Essentially the same formulation is found in Carnap's "Testability and Meaning": (1936/37, p. 428-29)

> It is a pseudo-thesis of idealism and older positivism, that a physical object (e.g. the moon) is a construction out of sense-data. Realism on the other hand asserts, that a physical object is not constructed but only cognized by the knowing subject. We—the Vienna Circle—neither affirm nor deny any of these theses, but regard them as pseudo-theses, i.e. as void of cognitive meaning. They arise from the use of the material mode, which speaks about 'the object'; it thereby leads to such pseudo-questions as the "nature of this object", and especially as to whether it is a mere construction or not.

This rather extreme position depends upon the logical positivists' severe restriction on which propositions are meaningful. Observation propositions about the moon are meaningful, because they are verifiable. Assertions about the moon are only meaningful in so far as they are reducible to such observation propositions. Assertions by realists of the independent existence of the moon as an object are not so reducible. Hence they are meaningless. Since the negation of a meaningless proposition is also meaningless, the denial of the realists' assertion by idealists is correspondingly meaningless.

Carnap's earlier *Aufbau* (1969, p. 333) has a striking example of how this view is applied. It is a parable of a realist and an idealist geographer who map out a mountain somewhere in Africa. They agree on all propositions concerning the observations made during their expedition. Nothing in those observations distinguishes the further realist claim, that the mountain is real, from the idealist claim, that the mountain is merely constructed from their mountain-like sensations. Hence the problem of deciding between them is a pseudoproblem.

2.6 Logical Atomism and Logical Analysis

The second component of logical positivism is the replacing of ordinary language by symbolic logic. Ordinary language, the manifesto insisted, invites misunderstanding: (Hahn, Neurath, Carnap, 1929, pp. 307-308)

> Ordinary language for instance uses the same part of speech, the substantive [noun], for things ('apple') as well as for qualities ('hardness'), relations ('friendship'), and processes ('sleep'); therefore it misleads, one into a 'thing-like' conception of functional concepts (hypostasis, substantialization). One can quote countless similar examples of linguistic misleading, that have been equally fatal to philosophers.

There was a solution. The manifesto praised Russell and Wittgenstein for their use of logic in assailing metaphysical excesses. Formal, symbolic logic would be the means to purge our discourse from the misunderstanding induced by ordinary language: (Hahn, Neurath, Carnap, 1929, pp. 309)

> Only modern symbolic logic ('logistic') succeeds in gaining the required precision of concept definitions and of statements, and in formalizing the intuitive process of inference of ordinary thought, that is to bring it into a rigorous automatically controlled form by means of a symbolic mechanism.

Their praise of Wittgenstein indicates that his *Tractatus Logico-Philosophicus* was an example of the sort of reformulation into a formal, symbolic language that they envisaged. Wittgenstein's ambitions in his *Tractus* were not just grand but grandiose. In the compact summary provided by Russell's introduction, the analysis rests on atomic propositions that express the simplest of particular facts, such as "Socrates is wise." The totality of these atomic propositions exhausts all that is factual in the world. All other propositions can be derived from them by simple, truth functional logic. The goal of the *Tractatus* is to develop sufficient logical apparatus to make this possible. As Russell put it: (Wittgenstein, 1922, p. 15)

> Wittgenstein is enabled to assert that propositions are all that follows from the totality of atomic propositions (together with the fact that it is the totality of them); that a proposition is always a truth-function of atomic propositions; and that if *p* follows from *q* the meaning of *p* is contained in the meaning of *q*, from which of course it results that nothing can be deduced from an atomic proposition.

Carnap's own implementation of this project came shortly in his "Testability and Meaning" (1936/37). There Carnap laid out the basic elements of a first order predicate logic, which was to be the symbolic logic that would sweep away the misunderstandings. Only a few years had passed since the writing of the manifesto. Yet Carnap had already realized that he must give up the grandiose vision inherited from Wittgenstein of a full construction of all meaningful propositions by explicit definitions from atomic propositions. Carnap now recognized that the full verification of meaningful propositions allowed by this construction was not achievable. Instead, his system was to be based on the inductive notion of confirmation.[31] Carnap's account of confirmation is rudimentary and little more than a slightly embellished version of hypothetico-deductivism. Allowing that Carnap would later be the author of his massive *Logical Foundations of Probability* (1962), his negative appraisal of a probabilistic account of confirmation is striking: (1936/37, p. 427)

> It seems to me that at present it is not yet clear whether the concept of degree of confirmation can be defined satisfactorily as a quantitative concept, i.e. a magnitude having numerical values.

In place of the explicit definitions of his earlier construction project, Carnap proposed that dispositional concepts could be introduced with reduction sentences that merely give sufficient conditions for the applicability of the concept or its failure. Their form is: (p. 441)

$$Q_1 \supset (Q_2 \supset Q_3)$$

$$Q_4 \supset (Q_5 \supset \sim Q_6)$$

where $\supset$ is merely truth functional, material implication. Loosely, the first tells us that in test condition Q_1 (e.g. this salt is put in water), if Q_2 (the salt dissolves) then Q_3 (the salt is soluble). The second formula is read analogous for the failure of the property to obtain.

In retrospect, the failings of Carnap's project in "Testability and Meaning" are now clear. The problems he sought to understand are difficult and delicate. That his account of confirmation was rudimentary in its fundamentals and bound to fail was obscured by the elaborateness of the

[31] That such a weakening was needed was also apparent to Alfred Ayer, whose 1936 *Language, Truth and Logic* was a widely read, English language introduction to logical positivism. He argued for a "weak" sense of the verifiability principle (Ayer, 1946, p. 37). "[A proposition] is verifiable, in the weak sense, if it is possible for experience to render it probable." Ayer's use of the term "probable" is informal and, loosely speaking, means something akin to "likely." He makes clear (p. 100) that his use of the term is distinct from its use in the mathematical theory of probability.

logical apparatus in which it was formulated. That apparatus introduced enticing but irrelevant technical problems sufficiently rich to distract a generation of theorists.

The essential problem was of a spurious illusion of precision. Unfortunately, it recurs. Each generation in philosophy of science faces delicate and difficult problems. It is enticing to many to expect that they can be solved by some simple formal apparatus. That the formalism is too simple for the task is easy to overlook. Each brings challenging technical problems whose appeal obscures the fundamental inadequacy of the apparatus for the delicacy of problem addressed.

3. Reichenbach's Logical Empiricism[32]

At the same time as logical positivism was being developed, Hans Reichenbach was developing his own version of empiricism. He called it "logistic empiricism" (Reichenbach, 1938, p.v). The view was soon renamed "logical empiricism," as the title of Salmon (1979) indicates. It is natural to group Reichenbach's version of empiricism with logical positivism. The similarities are strong, as Salmon (1977, p.40-41) reports. Reichenbach and Carnap co-edited the new journal, *Erkenntnis*, which was the favored medium of publication for the logical positivists. Reichenbach, like the logical positivists, formulated his empiricism in terms of language. His basic principles determined which propositions are meaningful. Their meaningfulness derives from the appropriate link to experience. Reichenbach, like Carnap, sought to formulate his empiricism in its most precise version using "logistics," or as we now say, symbolic logic.

Once we pass beyond these similarities, we find differences in their approaches that are deep enough for logical empiricism to be not just a variant of logical positivism, but a competitor. Within his development of logical empiricism, Reichenbach criticized logical positivism repeatedly and quite sharply.

On my reading, Reichenbach's critique was well-targeted. It sought to replace the excesses of the logical positivist movement by more modest proposals. Yet his logical empiricism seems to have had less prominence in the later literature. One possibility is that Reichenbach's most explicit formulation of his views, *Experience and Prediction* (1938), is curiously obscure in its writing. Reichenbach employed a needlessly convoluted and indirect

[32] The secondary literature on Reichenbach's work is extensive and its survey is beyond the reach of this section. For an entry into this literature, see Glymour and Eberhardt (2022).

mode of exposition. He did not give his account at the outset but repeatedly took the reader on a prolix and exhausting survey of other accounts. They absorb most of the reader's efforts before Reichenbach finally revealed his view through responses to the views he has rejected. In contrast, Ayer's *Language, Truth and Logic* (1936), a popular exposition of logical positivism, is brightly and directly written and is far more inviting to readers.[33] Google Scholar finds Ayer's work to have roughly two and half as many citations as Reichenbach's.

3.1 Probability Theory of Meaning

The most significant difference from logical positivism is that Reichenbach's logical empiricism is thoroughly probabilistic. Most of the remaining differences derive from this difference. That his account was essentially probabilistic reflected Reichenbach's long-standing engagement with probability theory. His doctoral dissertation at Erlangen (1916) was "The Concept of Probability for the Representation of Reality." The interest developed in later works such as Reichenbach (1932, 1935).

In his *Experience and Prediction*, Reichenbach summarized his probabilistic expansion of the verifiability theory of meaning with two principles: (1938, p. 54)

> First principle of the probability theory of meaning: a proposition has meaning if it is possible to determine a weight, i.e., a degree of probability, for the proposition.

This first principle extends the reach of propositions that can be found meaningful. Reichenbach illustrated how this principle could be applied to the proposition that the interior of the sun is ascribed some high temperature. No such temperature can be *deduced* from experience. However, it is well within the capacities of solar physics to provide estimates with some probability. The second principle constrains meaning in a way conformable with empiricism:

> Second principle of the probability theory of meaning: two sentences have the same meaning if they obtain the same weigh, or degree of probability, by every possible observation.

It follows that a difference in the meanings of two propositions must come from a difference in the probabilities of the propositions based on observation. This condition gives observation a controlling role in the meaningfulness of propositions.

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[33] This is from personal experience. I read Ayer's work in my early 20s and found it compelling, even though I had no secure grounding in philosophy sufficient to assess it.

These principles allowed Reichenbach to expand the verifiability theory of meaning of the logical positivists: (p. 55, his emphasis)

> The probability theory of meaning may be considered as an expansion of the truth theory of physical meaning in which the postulate of verifiability is taken in a wider sense, including the physical possibility of determining either the truth-value or a weight. We shall therefore include both theories under the name *verifiability theory of meaning*. The narrower sense of verification will be expressed by "absolute verification."

The notion of probability meaning is eventually (by pp. 62-63) located within a four-part, nested[34] hierarchy of propositions that have

"physical truth meaning,"
"probability meaning,"
"logical meaning" and
"super-empirical meaning."

Propositions have physical truth meaning, as far as I can tell, if their verification is admitted by physical laws. Examples (from p. 38) include propositions about the construction of a bridge across the Atlantic Ocean or a visit to the moon, since they are physically possible even if technologically impractically difficult. An excluded example concerning the construction of a perpetual motion machine, since that its construction is prohibited by the physical law of the conservation of energy.

Propositions have probability meaning if meaning is granted by either of the two principles just quoted.

Propositions have logical meaning if they involve no internal, logical contradiction. Propositions concerning perpetual motion machines have logical meaning, but propositions concerning quadrangular circles or railways without rails do not. (Examples from p. 39.)

Super-empirical propositions have the same status in Reichenbach's system as the maligned metaphysical propositions of the logical positivists. Reichenbach, however, was slow

[34] A Venn diagram on p. 63 indicates that the nesting is such that each proposition type is contained within the next in the hierarchy. This containment must fail for the super-empirical propositions, which appear to be represented as the all-embracing set. Reichenbach surely did not intend this.

to commit to a blanket rejection. He dismissed propositions for which we cannot ascertain truth values as useless as a basis for deciding actions (pp. 64-65). The truth value of some super-empirical proposition can, he allows, still be ascertained. His examples (pp. 65-68) are statements of religious belief. The proposition of the divinity of cats has, in Reichenbach's understanding, the consequence for actions that cats must not be killed. These consequences for actions are, for Reichenbach, the meaning of such super-empirical propositions.

Perhaps most surprising in Reichenbach's development is his treatment of observation propositions. Their identification was a natural starting point for logical positivist analysis, such as given in Hempel (1950, §2). Reichenbach was loath to give them univocal meaning. The complication is that observation requires some sense of possibility and Reichenbach has several of them. This multiplicity confounds the notion of observation sentence (p. 47):

> This term [observation proposition] seems to have a clear meaning; but we find that it depends on the definition of the possibility of observation. To observe the temperature in the interior of the sun, in the same sense as we observe the temperature of our chamber, is logically possible but not physically. So all these categories of sentences have no absolute meaning but vary with the definition of meaning.

3.2 A Probabilistic Logistic

Carnap sought to secure precision in the inferences of philosophy by formalizing them in a first order predicate logic. Reichenbach attempted something similar. He used the same sort of predicate logic as Carnap and merely added one new symbol,[35]

$$\ni_{\mathrm{p}}$$

Reichenbach called it "probability implication." It corresponds to the more familiar representation of conditional probability. Where we might now write "$P(B|A) = p$", the conditional probability of B given A is p, this relation would be represented in the formalism as "$A \ni_p B$." Merely adding this symbol profoundly changed the logic, for now Reichenbach needed to provide a serviceable account of probability suitable for his novel idea of probability

[35] As asserted in Reichenbach (1949, p. 45).

meaning. He sought to use a relative frequency interpretation of probability. The full analysis provided to be formidable. An informal treatment was provided in a lengthy final Chapter 5 of *Experience and Prediction*. The more technical aspects were already in place with Reichenbach's earlier paper (1932) and his monograph (1935, 1949).

In his "Testability and Meaning," Carnap (1936/37) had mentioned Reichenbach's ideas on probability theory and his 1935 monograph (p. 426), but (as already noted above) Carnap proceeded to dismiss the approach: (p. 427)

> It seems to me that at present it is not yet clear whether the concept of degree of confirmation can be defined satisfactorily as a quantitative concept, i.e. a magnitude having numerical values.

Given the depth of Reichenbach's engagement with a probabilistic foundation of his empiricism, Carnap's dismissal must have stung. Carnap and Reichenbach's differences over the meaningfulness of probabilistic propositions had already been aired in discussion in 1929[36]. Reichenbach (1938, pp. 76-77) could now assert with obvious satisfaction that, with his introduction of the notion of "degree of confirmation," Carnap's view and that of the Vienna Circle were moving closer to Reichenbach's own. He then issued a mild rebuke for Carnap in the form of a challenge: "if the interpretation in terms of probability is not accepted by Carnap, he must develop a theory of his own about degrees of confirmation."

3.3 Fallibilism and Failure of Positivism

Underlying Reichenbach's probabilism was a fallibilism about propositions. We can, in the last analysis, never be absolutely sure of any proposition. That the "positivists," as Reichenbach called them, asserted otherwise was, in his view, a most harmful error.

Reichenbach's assertions of his fallibilism built slowly in the course of his exposition in *Experience and Prediction*. A fairly complete statement of it only emerges roughly half-way through his text. There he finally comes to reject the absolute verifiability of propositions about observation propositions, that is, concrete physical facts; and to reject the absolute verifiability of

[36] As Reichenbach (1938, p. 77, fn. 17) notes, they are reported in *Erkenntnis*, 1(1930), pp. 268-70.

impression propositions, that is, propositional formulations of our sense experiences. He continued: (pp. 187-88)

> We have discovered now that even this is not tenable, that impression propositions also can only be judged by the category of weight. Thus there are left no propositions at all which can be absolutely verified. The predicate of truth-value of a proposition, therefore, is a mere fictive quality; its place is in an ideal world of science only, whereas actual science cannot make use of it. Actual science instead employs throughout the predicate of weight. … So the predicate of weight has entirely superseded the predicate of truth-value and remains our only measure for judging propositions.

This fallibilism was the basis of Reichenbach's repeated reproval of the positivist theory of meaning, which, Reichenbach urged, was based on the failed "principle of retrogression." He formulated the principle in this way (p. 49):[37]

> According to this principle, the meaning of the indirect proposition is obtained by constructing the observation propositions from which the indirect proposition is inferred; the principle of retrogression maintains that this inference is to be interpreted as an equivalence and that the meaning of the conclusion of the inference is the same as the meaning of the premisses of the inference…. This is the idea expressed by Wittgenstein in his formula: the meaning of a proposition is the method of its verification.

That is, this principle assumes that observation sentences can be absolutely verified and their truth known infallibly. Their absolute verification is then inherited by the indirect propositions whose meaning is given exhaustively by the observations.

To prepare for his reproval, Reichenbach recalled Hume's skeptical contributions to the positivist tradition and their later incorporation into a two-valued logic, that is, one that assigns only values of truth or falsehood to propositions, but not degrees. He then delivered his assessment of the malign influence of infallibilism on the tradition (p. 74):

[37] Indirect propositions "are propositions which cannot be directly verified, but which can be reduced in a certain way to other propositions capable of direct verification." (p. 47)

> Thus resulted the modern positivistic theory, a strange combination of common-sense elements with a doctrinaire radicalism, which contradicted every unbiased view of the intentions of science. The postulate of absolute verifiability, when pronounced within science, has been mitigated by inconsequent application and therefore could do no harm; but in the hands of philosophers it was exaggerated to a radicalism which questioned the legitimacy of the very aim of science—the prevision of the future. Wittgenstein, the most radical mind among modern positivists, writes: "That the sun will rise to-morrow is a hypothesis; and that means that we do not know whether it will rise." [38] He does not realize that there are degrees in the domain of the unknown, such as we have expressed by the predicate of weight. Keeping strictly to the postulate of absolute verifiability, he arrives at the conclusion that nothing can be said about the future.

As his text unfolds, Reichenbach's customarily reserved prose begins to falter and betrays a buried passion. He laments (p. 76) the "too narrow logicism which characterizes this form of positivism, of the unwarranted simplification which does violence to the actual structure of science." This eventually devolves to an *ad hominem* attack on unnamed figures, presumably those advocating logical positivism under the inspiration of Wittgenstein (p. 189):

> But in the hands of pretentious and consistent logicians this schematized conception has produced serious misunderstandings of science and has led to grave distortions in the interpretation of scientific methods.

The distortions are then summarized (p. 189):

> This procedure [of identifying the unverifiable with the meaningless] is carried through with more or less consistency; but none of its representatives has as yet had the courage to carry his principle through to its ultimate consequence and to admit that there are no meaningful sentences at all left in science.

3.4 Realism

We saw above that the Vienna Circle found both realism and idealism to be meaningless: neither can be verified in experience. To fill the void left by their elimination, the Circle offered their conception of what it is to be real: "For us, something is 'real' through being incorporated

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[38] Reichenbach's footnote cited Proposition 6.36311 of Wittgenstein's *Tractatus* (1922, p. 181).

into the total structure of experience." This notion of reality would appear to be solipsistic, especially given that propositions asserting the existence of other minds are meaningless. Or perhaps it is a redefinition of the very notion of "real."

The Circle justified their treatment of the real by their view that factual propositions can only be meaningful if they are deduced from experience. Reichenbach did not ascribe to this strict binary division of propositions into the meaningful and the meaningless. His notion of probability meaning allowed for propositions well beyond those of observation to be meaningful. The attribution of a temperature to the interior of the sun was one of his examples. While observation propositions retained a privileged status in Reichenbach's logical empiricism, they too only had a probability meaning. They did not enjoy the infallibility accorded to them by the logical positivists. Thus, Reichenbach could assign different probability weights to a wide range of propositions that spanned from ordinary observational propositions through to propositions of the theoretical sciences and even to some super-empirical propositions that the Circle would denounce as meaningless. Here is his example of propositions concerning the sun (p. 131):

> The statement, "The sun is a ball of glowing gases of high temperature," is not equivalent to any set of statements about physical facts outside the sun, even if the set is infinite and even if it comprehends all points of a surface surrounding the sun; we get by these observations a set of elements from which we may with probability infer the sun's existence and qualities, but which is by no means of equivalent meaning.

Freed of these strictures of logical positivism, Reichenbach could offer a more expansive view on what is real. Once again, his development was labored and indirect. The primary vehicle for the development was a parable, somewhat reminiscent of Plato's cave. Reichenbach (§13) asked us to image the shadows cast by birds under various lighting condition. He eventually sketched an elaborate "cubical world" (§14). Observers are within a cube of translucent material. They can see the shadows cast by the birds outside the cube, but not the birds themselves. A system of mirrors ensures that the shadows are duplicated on two sides of the cube.

The important datum for observers within the cube is that the shadows on the two walls are strongly correlated in their form and behavior. Reichenbach's reasoning (pp. 119-122) seems to proceed as follows. A strict positivist would only admit as meaningful a proposition asserting the correlation among the shadows. The further proposition that the shadows are multiple projections of fewer birds is meaningful only in so far as it asserts the original correlation among the shadows. Reichenbach's logical empiricist cannot assert absolutely that the shadows are cast by birds external to the cube. However, this logical empiricist can infer from the correlations that there is an increased probability of the existence of these external birds.[39] Thus the proposition asserting their existence has a probability meaning distinct from that of propositions concerning the shadows.

There are two statements here. The first merely asserts a correlation among the shadows. The second asserts the existence, external to the cube, of the birds. Of them, Reichenbach writes (p.121):

> The positivistic conception demands that two statements have the same meaning if they are equally determined as true or false by all possible facts; the probability conception demands the same meaning only if the statements obtain the same weight by all possible facts. It is to be admitted that the observable facts do not furnish a difference as to absolute truth or absolute falsehood of the two theories in question; but the weight conferred on them by the facts observable within the cube is different. Whereas the positivistic definition of meaning must therefore consider the two theories in question as having the same meaning, the probability definition of meaning furnishes a different meaning for both theories-although the domain of observable facts is the same, and although only the postulate of physical possibility is employed in the definition of meaning.

The bird shadows and the cubical world are parables. The shadows are the analogs of our impressions or sense experiences; and the birds are the analogs of the external world that produces these impressions. Positivists limit meaning to propositions about the shadows, that is, to impressions. Logical empiricists, however, can attribute what Reichenbach calls surplus meaning to the external reality of the birds, that is, to the contents of the external world that

[39] Readers of Reichenbach's later writings will have no trouble identifying this inference as an application of what Reichenbach later called the "principle of the common cause" (Reichenbach, 1956, §19).

produce the impressions by a process of projection. This for Reichenbach is an expression of realism or, as he called it, the "realistic conception of the world" (p. 129):

> By analogy with the example of the cubical world our contention reads: Impressions are only effects produced within our body by physical things, in the same sense as the shadows are effects of the birds. Thus impressions are only external elements relative to the physical things; these things are projected to our impressions but not reduced to our impressions. The "external world" therefore has an existence of its own, independent of our impressions.
>
> This is the so-called realistic conception of the world.

An earlier statement is comparable in its import (p. 111):

> …there is no equivalence between propositions concerning external things and propositions concerning impressions; there is only a probability connection. This relation is thus a projection and not a reduction; the existence of the external things is not reducible to the existence of impressions. The external things have an independent existence.

This approach led Reichenbach to decide in favor of Boltzmann and against Mach's skepticism in the venerable debate over the existence of atoms (pp. 212-13):

> The atoms have been discovered by the physicists in a way analogous to the discovery of the birds in the cubical world. Certain observed relations between macroscopic bodies—such as expressed in Dalton's law of multiple proportions—made it very probable that all bodies are built up of very small particles, though these particles could not be directly observed; this was the first substantiation given by the physicists to the theory of atoms.

3.5 Reichenbach's Conventionalism in Theories of Space and Time

Reichenbach's realism is defined in its opposition to the Vienna Circle's view. It resides in an insistence that propositions about the external world have a meaning that goes beyond our sense impressions.

Here I have gone into some detail in specifying the character of Reichenbach's realism because his writings elsewhere have a non-realist import. Most famously, his field-defining monograph in the philosophy of space and time (1928/1958) defended a conventionality for the geometry of space and for the simultaneity of spatially-separated events in special relativity. In more recent years, this has led to many casual remarks in discussions that characterize Reichenbach as an antirealist. The sense of realism used in these remarks is different. It relates to the later version of scientific realism that we shall see below as the foil for van Fraassen's constructive empiricism.

I still today have a vivid recollection of what would happen when such remarks were made in the presence of Wes Salmon, Reichenbach's accomplished student. He would command the attention of the room and, with the full authority befitting a most senior member of the profession, he would declare "Reichenbach was a realist!"

Reichenbach's own answer is found in *Experience and Prediction* (pp. 127-29). There he discusses his interpretation of Einstein's 1905 analysis of the simultaneity of two spatially separated events in a single inertial frame of reference. Many such relations are possible. There are no facts in the background capable of giving even a probability meaning to the preference for one such relation over many others. In this sense, the choice of a simultaneity relation must be made conventionally. This is an antirealist thesis, but it is very limited in its scope and is not part of a general antirealism.

3.6 Logic not Psychology

Reichenbach was also quite explicit in renouncing the attachment of empiricist thinking to the mental life of cognizers. This attachment had become prominent through the work of Hobbes and Locke and had found an extreme expression in Machian sensationalism. It is understandable that Hobbes and Locke should engage in analyses that, in later terms, merged logic and psychology. For in their time there was no distinct psychology as a science. They were part of its historical initiation. After psychology had become identifiable as an independent science, conflating it with logical analysis in epistemology ceased to be excusable.

Thus, it was fitting that, in spite of their reverence for Mach, the logical positivists had begun to move their epistemology away from a theory of mental life. Their emphasis became the meaning of propositions and the project of construction of the world by logical means. Nonetheless, Carnap's *Aufbau* was still quite dependent on psychological ideas. His construction was concerned with the autopsychological and the heteropsychological. His epistemology was still entangled, even if only in name, in the mental life of cognizers.

The traces of a move away from psychology do appear in Carnap's writings as a consequence of Carnap's emphasis on logic. To allow his logical analyses to incorporate experience, it entered as sentences, a basic unit of logical analysis. More specifically, it entered as the "protocol sentences." Then, whether experience verified some result was removed from a matter of psychology to one of logical relations among sentences. Carnap (1934, p. 42) defined "protocol sentences" that formed a "protocol language" as:

> Verification is based upon "protocal [sic] statements," a term whose meaning will be made clearer in the course of fu[r]ther discussion. This term is understood to include statements belonging to the basic protocol or direct record of a scientist's (say a physicist's or psychologist's) experience.

This text is a liberal translation with typographical errors of the German: (Carnap, 1931a, p. 437)

> Die Nachprüfung geschieht an Hand der 'Protokollsätze.' Hierunter sind die Sätze verstanden, die das ursprüngliche Protokoll etwa eines Physikers oder Psychologen enthält.

It is notable that the translator, Max Black, felt the need to explain the German term "Protokoll." At that time and still today, in English, "protocol" has a narrower meaning, commonly associated with formal documents in diplomacy.[40]

Where Carnap's account made no clear separation from psychology, it was otherwise with Reichenbach. He was eager to purge psychology from epistemology and emphasized, quite correctly in my view, that the two are distinct. He wrote: (1938, p. 5)

> There is a great difference between the system of logical interconnections of thought and the actual way in which thinking processes are performed. The psychological operations of thinking are rather vague and fluctuating processes; they almost never keep to the ways prescribed by logic and may even skip whole groups of operations which would be needed for a complete exposition of the subject in question. … It would be, therefore, a vain attempt to construct a theory of knowledge which is at the same time logically complete and in strict correspondence with the psychological processes of thought.

He continued to emphasize that the real subject of epistemology is a normative logic. It is not how we *do* think, but the standard that determines ideally how we *ought* to think:

[40] Or so I find in the 1911 edition of the *Concise Oxford English Dictionary.*

> The only way to escape this difficulty is to distinguish carefully the task of epistemology from that of psychology. Epistemology does not regard the processes of thinking in their actual occurrence; this task is entirely left to psychology. What epistemology intends is to construct thinking processes in a way in which they ought to occur if they are to be ranged in a consistent system; or to construct justifiable sets of operations which can be intercalated between the starting-point and the issue of thought-processes, replacing the real intermediate links. Epistemology thus considers a logical substitute rather than real processes.

Drawing explicitly on an expression from Carnap's *Aufbau*, Reichenbach called the process of arriving at this logical substitute "rational reconstruction" [*rationelle Nachkonstruktion*]

4. Russell and Popper's Anti-inductivism

Inductive inferences from experience had a stable if unremarkable role in logical positivism. It rose to a central position in Reichenbach's logical empiricism because of the constitutive role of probability theory in his probability theory of meaning. Inductive inferences did not fare so well in the work of other philosophers, who we might expect otherwise to be sympathetic to empiricism. One of the reasons was the revival of Hume's problem of induction in the early part of the twentieth century, after it had all but disappeared from the philosophical literature in the nineteenth century.[41]

4.1 Russell's Reluctance

The problem of induction was revived in a prominent place in Chapter VI of Russell's (1912) widely accessible *Problems of Philosophy*. Russell soon outlined the threat it posed to empiricism in his 1915, *Our Knowledge of the External World*. Russell approached the problem (1915, pp. 34-37) from several perspectives. He determined that some sort of principle is needed if induction by simple enumeration is to be "valid," as he called it. One formulation was: (p. 37)

> … every instance of a proposition … being true increases the probability of its being true in a fresh instance, and that a sufficient number of favourable instances

[41] For an account of the revival of the problem of induction in the early twentieth century, see Norton (2024, Ch. 6, §6-8).

> will, in the absence of instances to the contrary, make the probability of the truth of a fresh instance approach indefinitely near to certainty.

He then impugned this principle by replicating Hume's celebrated critique, although Hume himself is not named (Russell's emphasis):

> But this brings us to our other question, namely, how is our principle known to be true? Obviously, since it is required to justify induction, it cannot be proved by induction; since it goes beyond the empirical data, it cannot be proved by them alone; since it is required to justify all inferences from empirical data to what goes beyond them, it cannot itself be even rendered in any degree probable by such data. Hence, *if* it is known, it is not known by experience, but independently of experience. I do not say that any such principle is known: I only say that it is required to justify the inferences from experience which empiricists allow, and that it cannot itself be justified empirically.

The negative consequences for empiricism then followed (p. 37):

> Thus logical knowledge is not derivable from experience alone, and the empiricist's philosophy can therefore not be accepted in its entirety, in spite of its excellence in many matters which lie outside logic.

Russell expanded on the problem, later in the text (p. 56, Russell's emphasis):[42]

> Thus general truths cannot be inferred from particular truths alone, but must, if they are to be known, be either self-evident, or inferred from premisses of which at least one is a general truth. But all *empirical* evidence is of *particular* truths. Hence, if there is any knowledge of general truths at all, there must be *some* knowledge of general truths which is independent of empirical evidence, *i.e.* does not depend upon the data of sense.

The restriction on empiricism now followed:

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[42] Russell *assumed* that particular truths must be supplemented by general truths that are not themselves empirically supported, if the particular truths are to support inductions to generalities. This assumption fails in a material theory of induction, as sketched in Norton (2024, Chapter 6, Section 1, "Regress Start."), so that Russell's objection to empiricism fails as well.

> The above conclusion, of which we had an instance in the case of the inductive principle, is important, since it affords a refutation of the older empiricists. They believed that all our knowledge is derived from the senses and dependent upon them.

It is striking that the formulation of the complaint against empiricism offered by Russell parallels closely that given earlier by Henry Sidgwick and reported in the last chapter. It is quite likely that something more than mere coincidence is at work. Sidgwick held a professorship at Cambridge during Russell's time there. Russell later recalled Sidgwick as an influence on him that was exceptional in not being directed towards German idealism. He recalled, perhaps ruefully (Russell, 1959, p. 27):

> At the time, I, in common with other young people, did not give him nearly as much respect as he deserved. We called him 'Old Sidg' and regarded him merely as out of date.

4.2 Popper's Falsificationism

Perhaps I am overstepping in sensing a begrudging reluctance in Russell's pessimistic appraisal of empiricism. However, I find no such reluctance in Popper's anti-inductivism. The first chapter of his *Objective Knowledge* (1979, p.1) begins with a boast, clothed in feigned modesty:

> Of course, I may be mistaken; but I think I have solved a major philosophical problem: the problem of induction.

Popper's solution was published in his *Logik der Forschung* (1935) and its expanded translation, *Logic of Scientific Discovery* (2005). The solution was severe. It allowed that the problem rendered inductive inference unsalvageable by any means. Unlike Sidgwick, Russell and Reichenbach, Popper concluded that inductive inference was to be abandoned entirely. He wrote (2005, pp. 6-7, Popper's emphasis):

> The theory to be developed in the following pages stands directly opposed to all attempts to operate with the ideas of inductive logic. It might be described as the theory of *the deductive method of testing*, …

This method, the now widely-known method of conjectures and refutations, consists of a cycle of operations. We can enter the cycle at the point at which a scientist conjectures a new theory and, to test it, makes predictions with it that are not predictions of the existing theory. Popper's narrative (2005, p. 10, Popper's emphasis) continues:

> Next we seek a decision as regards these (and other) derived statements by comparing them with the results of practical applications and experiments. If this decision is positive, that is, if the singular conclusions turn out to be acceptable, or *verified*, then the theory has, for the time being, passed its test: we have found no reason to discard it. But if the decision is negative, or in other words, if the conclusions have been *falsified*, then their falsification also falsifies the theory from which they were logically deduced.
>
> It should be noticed that a positive decision can only temporarily support the theory, for subsequent negative decisions may always overthrow it. So long as theory withstands detailed and severe tests and is not superseded by another theory in the course of scientific progress, we may say that it has 'proved its mettle' or that it is 'corroborated' … by past experience.

The key property of this procedure is that it is independent of inductive inferences (p. 10):

> Nothing resembling inductive logic appears in the procedure here outlined. I never assume that we can argue from the truth of singular statements to the truth of theories. I never assume that by force of 'verified' conclusions, theories can be established as 'true', or even as merely 'probable'.

Popper then developed one of his most popular theses: the condition of falsifiability as the criterion for distinguishing or demarcating science from other systems (p. 18, Popper's emphasis):

> … I shall certainly admit a system as empirical or scientific only if it is capable of being tested by experience. These considerations suggest that not the *verifiability* but the *falsifiability* of a system is to be taken as a criterion of demarcation. … In other words: I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense: *it must be possible for an empirical scientific system to be refuted by experience*. …

Although this proposal differs from that of other empiricists of his time, Popper can be categorized as an empiricist for his agreement with a fundamental tenet of empiricism: the privileging of experience, but now for its decisive role in the method he delineated. He wrote of it (2005, p. 17, Popper's emphasis):

> 'Experience', on this view, appears as a distinctive *method* whereby one theoretical system may be distinguished from others; so that empirical science seems to be characterized not only by its logical form but, in addition, by its distinctive method.
>
> …
>
> The theory of knowledge, whose task is the analysis of the method or procedure peculiar to empirical science, may accordingly be described as a theory of the empirical method—*a theory of what is usually called 'experience'*.

Although it may now appear benign to us to label Popper as an "empiricist," he explicitly adopted the label only in early writings. In drafts written in the years 1930 to 1933 and subsequently published as Popper (2009), he wrote (pp. 78-79, Popper's emphasis):

> The position advanced here is *empiricist* by virtue of its fundamental principle (the *fundamental thesis of empiricism*) that only *experience* can decide the truth or falsity of an empirical statement.
>
> According to the *deductivist-empiricist* view advocated here, there is only *one* relationship between natural laws, theories and *universal* empirical statements, on the one hand, and s*ingular empirical statements* (the "empirical basis"…) on the other, namely that of logical deduction. With the help of theories, *predictions* are deduced and tested by experience.

Whereas Popper emphasized the differences between his falsificationism and the views of the logical positivists, they are very close in their overall framing. One way to see this is to note that core propositions of each can be interconverted merely by the substitution of two words. The logical positivists asserted that:

A proposition is *meaningful* just if it can be *tested* by experience.

Popper asserted that:

A proposition is *scientific* just if it can be *falsified* by experience.

The substitutions do change the meaning of the assertions in important ways. Its mere possibility, however, show how close they are in their overall conceptions of their projects. Logical relations with experience are to be used to distinguish a meritorious property in systems of propositions.

In spite of their mutual affinity, Popper's relationship with the logical positivists was combative. He was irked that they paid scant attention to his ideas, complained that they misunderstood them and sought to establish temporal priority for the discovery of his ideas. What made this last claim awkward was that Popper's publication of his ideas came after the early published work of the Vienna Circle. The earliest of Popper's publications was a 1933 letter to the logical positivists' journal, *Erkenntnis*,[43] as Popper acknowledged in *Objective Knowledge* (1979, p. 2). Presumably to repair this lacuna, Popper wrote heroic, autobiographical recollections that included his preferred chronology of discovery, such as the first essay "Science: Conjectures and Refutations" in his Popper (1962, Ch. 1).

4.3 An Appraisal of Popper's Falsificationism[44]

Popper's empiricism is distinctive among those reviewed in this chapter. For, unlike these other accounts, his empiricism and a recognition of his name maintain a strong presence even today in discussions of the foundation of science in the larger public sphere, outside the narrower confines of philosophy of science. For this reason, I include here an appraisal of Popper's account.

First, I emphasize the real strengths of Popper's account. In the later part of the twentieth century, there was a shift towards excessively skeptical appraisals of science. It became fashionable to doubt that science had achieved its goal of objective knowledge of the world and to argue that the methods of science were too flawed ever to achieve these goals. Popper's account persisted as a visible counterbalance to this skepticism. It defended the core rationality of science and the privileged status of scientific knowledge.

Many, like me, were alarmed at the readiness of the literature to accept shoddy argumentation if it supported some form of skepticism. That alarm should not blind us to the inherent weaknesses in Popper's account. We should know what they are so that we can seek

[43] The 1933 letter is published in translation in the 1959 edition of *Logic of Scientific Discovery*, along with a synopsis of Popper's grievances with the logical positivists.

[44] For a more extensive version of this appraisal, see Norton (2026).

better ways of identifying and defending the rationality of science. To this end, I provide a brief synopsis of the weaknesses of Popper's account.

The principal weakness lies at the foundation of Popper's account and is ultimately responsible for the others that follow. Science is an inductive enterprise and effective scientists seek empirical evidence that inductively supports its theories. Once Popper renounces all recourse to inductive inference, he has set himself an insoluble problem. He must find some replacement for inductive inferences that both secure the rationality of science and conform with the activities of scientists. His efforts fall short and inevitably so.

What privileges the major results of our mature science is that they are supported strongly by the empirical evidence. Those inductive relations of support can be displayed for us to examine in a way that is independent of the particular pathways followed by the scientists to establish them. The evidence for, say, big bang cosmology and the evolutionary origin of biological species can be inspected and assessed without a need to consider whether the scientists arrived at these relations of support laudably, by painstaking, critical investigations, or, less laudably, by haphazard adventures.

Popper's account admits no such static assessment. To affirm the privilege of some result in science, we must ask for the process that the scientists followed to arrive at the result; and it must conform with the method Popper prescribed. A clear case of the difficulties that follow is the Popperian deprecation of *ad hoc* hypotheses; that is, hypotheses that are contrived to protect some theory from falsification. Such hypotheses are inadmissible, in Popper's (1962, p. 37) account, because of the history of their formulation. The one hypothesis, introduced by different scientists in different processes, might be appraised either as admissible or not. Inductivists face no such difficulties. The hypothesis stands or falls on the evidence that supports it inductively, independently of how it came to be proposed.

Where an inductivist would assess some theory as well supported inductively, Popper's surrogate is that it is well corroborated. That is, the hypothesis has survived "serious but unsuccessful attempt to falsify the theory." (Popper, 1962, p. 36) The definitive critique of the replacement of confirmation by corroboration is Salmon (1981): that some theory is well corroborated provides no basis for us to use it to make predictions. Corroboration is merely a statement about the past.

There are further problems. For a theory to be well corroborated, it must be subjected to severe testing. At its simplest, such testing presumes the testing scientists are genuinely (above: "seriously") trying to falsify the theory. That is, corroboration depends on our ability to affirm that the scientists were virtuous in this sense. We may try to circumvent the difficult question of detecting this virtue in the scientists by identifying what counts as a serious test. Popper (1962, p. 36, Popper's emphasis) gave this summary:

> [Corroborations] should count only if they are the result of *risky* predictions; that is to say, if, unenlightened by the theory in question, we should have expected an event which was incompatible with the theory--an event which would have refuted the theory.

This formulation does not sit well with Popper's abandoning of inductive inference. That we should have expected this event in the future requires a prediction that would rest on inductive notions abandoned by Popper.

More generally, successful corroboration is quite limited in its resources. They are roughly comparable to those of simple hypothetico-deductive confirmation. We know that something has gone well when a prediction of a theory is verified by experience. The weakness is that we do not know what in the theory is responsible for the success. Newton's mechanics enjoyed prodigious predictive success, although his absolute state of rest played no role in those successes. For an inductivist the superfluity of the absolute state of rest is revealed by the fact that we have no evidence specifically for it.

Popper's demarcation criterion is superficially appealing for its simplicity. For this reason, it has been taken up by many scientists. It is an easy and apparently damning critique of an opposing theory, if the critic can argue that the targeted theory is unfalsifiable. Although being unfalsifiable is a warning that something is amiss, making it the entirety of the criterion comes at an unsustainable cost. The simple criterion enables any fringe theory to claim cheaply the distinction of being scientific. It merely needs to interweave something falsifiable into the tangle of its claims. At the other extreme, an otherwise quite responsible science can fail to be falsifiable by novel predictions if all pertinent evidence has been collected and is known. Here we might imagine the excavation of an archaeological site, in which everything that can be known about it has been collected and catalogued.

The criterion and the cycle of conjectures and refutations promotes a historically unrealistic image of scientists as self-effacing and modest, all too eager to accept refutation of their pet theories and abandon their ideas. Science has not worked that way. New theories rarely emerge so perfectly formed that they are trouble-free. They require proponents to endure setbacks tenaciously so that their theories can survive long enough for an eventual vindication, in the rarer cases when it is to come.

If we are to seek a logical positivist response to Popper's work, we can look to Hempel's (1950, pp. 47-48) critique of Popper's criterion. It is mildly stated but devastating. Hempel, whose primary expertise and orientation was in logic, noted that the property of propositions being scientific under Popper's criterion behaves badly under simple logical operations such as "not," "and" and "or." If a proposition *S* is judged scientific, then its negation may not be so judged. If we conjoin—represented by "&"—a non-scientific, unfalsifiable proposition *N* to *S*, we arrive at something falsifiable and thus scientific, *S&N*. If we disjoin—represented by—"v"—an unfalsifiable *N* to a scientific *S*, we arrive at an unscientific, unfalsifiable *S*v*N*.[45]

When the problem is expressed in such simple terms, it is easy to dismiss them as nuisances of a logician's pedantry. Who would be troubled by such transparent contaminations of scientific propositions? The difficulty is that the contaminations need not be so transparent. The first arises when a physicist is adjudicating between:

All electrons are spin one half.

and its negation

There are electrons that are not spin one half.

The decision is between the first, a scientific proposition, and the second, an unscientific one. The adjudication, we are to believe, poses a question that lies outside science.

The second case is that of conjunction conferring the status of being scientific too easily. It is the logic that underwrites the example above of a fringe theory cheaply acquiring the status of science. The third example of disjunction is, in the general case, that being scientific need not be preserved under deductive inference, for *S* deductively entails *S*v*N*. A scientist may start with a scientific theory and deduce some apparently innocuous conclusion from it that turns out to be unscientific. For example, a falsifiable proposition in gravitational astronomy is:

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[45] This last case is not in Hempel's paper, but an obvious extension of his critique.

The planet Vulcan, with its suitably computed mass and orbit, allows Newtonian theory to account for the anomalous motion of Mercury.

It is falsifiable. The supposed planet Vulcan was not observed in the computed positions and the proposition was falsified. We can deduce another proposition from this scientific proposition:

There are ways that Newtonian theory can account for the anomalous motion of Mercury.

Since the scope of the "ways" indicated is unlimited, this second proposition is unfalsifiable and thus unscientific. After that small deductive step, we are no longer practicing science.

In contrast, inductivists have no need for a special criterion of demarcation to distinguish what is science. What matters for the inductivist is whether a hypothesis or a theory is well supported inductively by the empirical evidence. The expectation is that a mature science is well supported inductively. A theory or hypothesis falls short if it fails to be well supported inductively by empirical evidence. There is no need for further conditions to distinguish the merit of some system of propositions.

A final difficulty for Popper's account is that its abandoning of inductive inference is motivated by Popper's conviction that the problem of induction is insoluble. There is no great shame in this last conviction. Some of the most astute of his contemporaries, Bertrand Russell and Hans Reichenbach,[46] agreed. However, we now know that induction eludes the problem of induction if we reconceive induction materially; or so I have argued in my Norton (2024, Ch.6). Philosophers of science can endorse the use of inductive inference in science with clear consciences.

5. Operationalism

5.1 Turmoil in Physics

The Nobel Prize winning physicist, Percy Bridgman, outlined what he called the "operational point of view"[47] in his 1927 *Logical Foundations of Physics*. He wrote at a time of

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[46] Reichenbach formulated his pragmatic solution to excuse his allegiance to inductive inference. See Norton (2024, Ch.6) for details.

[47] For example in (1927, pp. 9, 25, 31 and elsewhere). The term "operationalism" does not appear in his volume (1927). Bridgman (1938, p. 114) later affirmed that "he had never talked of

great turmoil in physics. Einstein's two theories of relativity had overturned the prior certainties of physics and the emerging quantum theory promised still greater disruptions. Bridgman's diagnosis was that a defect in our concepts left us prone to the disruptions. He wrote of Einstein's two theories of relativity (1927, p. 1):

> Reflection on the situation after the event shows that it should not have needed the new experimental facts which led to relativity to convince us of the inadequacy of our previous concepts, but that a sufficiently shrewd analysis should have prepared us for at least the possibility of what Einstein did.

The older attitude introduced concepts through their properties. Newton had introduced his Absolute Time by specifying its properties (1927, p. 4). This, Bridgman determined, was the core of the trouble that eventually ensued.

5.2 Bridgman's Remedy

In place of this approach, as a remedy, Bridgman urged that we define a concept exhaustively by the operations used to measure it. He took the concepts of length in special relativity as his illustrative example (1927, pp. 5, 9-23). We measure the length of an object at rest by a simple procedure of comparison with a measuring rod. If the object is moving, we must employ a different set of operations. It will require the identification of the location of the ends of the moving object *at the same moment of time*. Thus, operations informed by simultaneity are required, where these were not required for operations measuring the length of the resting object. Suitably configured clocks, for example, might be required to introduce simultaneity into the measuring operation.

The different conditions of the object—rest or motion—require different measuring operations for length. Further differences require still more, distinct operations to determine lengths. To illustrate this need, Bridgman considered length measurements over a wide range of scales. He considered larger distance measures on terrestrial, solar and stellar scales; and shorter measures over microscopic and molecular distances. All required different operations. He summarized the outcome and his overall operational conception as follows (1927, p. 5, his emphasis):

'operationalism' or 'operationism." He feared such "grandiloquent words" would mislead in suggesting "a profound new theory of the of knowledge or of meaning."

> The concept of length is therefore fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations*.

The essential point is that a different set of operations defines a different concept. It is through this feature that our conceptual system will always be ready for new discoveries. We will not be taken unawares again by the novelty of such discoveries as relativity theory and the quantum theory. Memorably, he avers, we may "hope to render unnecessary the services of the unborn Einsteins." (1927, p. 24)

Here, as Bridgman recalled (pp.7-9), he is taking his cue directly from Einstein. For the starting point of Einstein's special theory of relativity was the recognition that judgments of the simultaneity of events differ according to the state of motion of the inertial reference frame in which they are made. There is no single concept of simultaneity, but one for each inertial frame of reference. Einstein revealed this through a celebrated operational analysis that used light signals to determine the simultaneity of spatial separated events. It showed that events judged simultaneous in one inertial frame of reference may not be judged so in another. Had we employed Einstein's operational conceptions from the start, our conceptual systems would have been prepared for the discovery of these differing judgments.

Correspondingly, this operational approach would have protected us from Newton's failed notion of Absolute Time, for there are no operations that measure it. "Therefore," Bridgman concluded (p. 6), "the previous statement that absolute time does not exist is replaced by the statement that absolute time is meaningless."

5.3 Mental Operations

The operationalist point of view is most commonly recalled in terms of physical measurement operations. It should have been evident from the start that purely physical measurement operations would be insufficient to define fully all the concepts then used in physics. This inadequacy was likely evident to Bridgman at the outset. Presumably for this reason, Bridgman allowed in his essay (1927, pp. 6-7) what he called "mental" operations. His example (1927, p. 6) is "mathematical continuity… those [operations] by which we determine whether a given aggregate of magnitudes is continuous." The concept requires something beyond physical operations. A property of a continuous magnitudes is density: between any two values we can always find a third. Iteration leads to a regress of infinitely many, arbitrarily close magnitudes whose full existence cannot be demonstrated by the physical operations available to us. However, we readily conceive of the continuity by mentally picturing an intervening magnitude between any two others. Perhaps we might even imagine what Bridgman later called a "paper and pencil" operation: the intermediate value is calculated arithmetically by averaging the two bounding values.

The difficulty with allowing mental operations is that they have creative powers that far outstrip the requirements of a modest empiricism. They can conjure up the many universes of multiverse cosmology and the enormous number of alternative particle theories of the string theory landscape. In 1927, Bridgman was loath to restrict their range and import. Instead, he obscured the difference between physical and mental operations and diminished the importance of the distinction (pp. 6-7):

> It is not intended to imply that there is a hard and fast division between physical and mental concepts, or that one kind of concept does not always contain an element of the other; this classification of concept is not important for our future considerations.

It became apparent over subsequent decades of Bridgman's writing that this treatment of mental operations was hasty. They play an important role in physical theorizing. A variety of them, "paper and pencil operations," appear over a hundred times by name in Bridgman's 1941 treatise on thermodynamics.[48] Writing decades later, Bridgman recognized that he had been hasty in his 1927 treatment of mental operations. He reported (1959, p. 522):

> If I were writing again I would try to emphasize more the importance of the mental or "paper-and-pencil" operations. Among the very most important of the "mental" operations are the verbal operations. These play a much greater role than I realized

[48] They appear almost always as vague allusions to incompletely specified calculations in the surrounding text. I found just two more precise instances. The density of a matter flux is calculated in a paper and pencil operation by dividing the flux by the velocity (1941, p. 39). The measured electric and magnetic field strengths E and H are combined in a paper and pencil operation for the electromagnetic field energy density $(E^2 + H^2)/8\pi$ (p. 91).

at the time, and I have tried to emphasize this in my later writings, particularly in *The Way Things Are*.

By this time, Bridgman made explicit what was already becoming implicit in his earlier writings: that mental operations could serve only as an intermediate in analyses that should eventually rely only on physical operations. This is, I believe, the import of his synopsis in his 1959 *The Way Things Are*. He wrote (1959a, p. 153):[49]

> In discussing operations, we have repeatedly emphasized that if our operations are to be of significance in a scientific context they must be capable of emerging eventually onto the instrumental level, and not remain indefinitely on the verbal level. The fact that here we have not been able to give complete instrumental meaning to the concepts of field or of action at a distance leads me to suspect that in any theoretical correlation of instrumental results we will never be able to make the instrumental emergence complete, but there always will remain some verbal or "paper-and-pencil" component.

5.4 Bridgman's Empiricism and Antirealism

Underlying his analysis, Bridgman reported, is a commitment to empiricism. "Thus the fundamental attitude of this essay is empiricism…," he wrote (1927, p. vi). More fully (p. 3):

> The attitude of the physicist must therefore be one of pure empiricism. He recognizes no *a priori* principles which determine or limit the possibilities of new experience.

This disavowal of the *a priori* conformed with the comparable doctrines of the logical positivists and logical empiricists. Bridgman was correspondingly scornful of metaphysics. He lamented "… the utter unintelligibility to the physicist of many metaphysical speculations and the sterility of such speculations in yielding physical results." (p. vii) Correspondingly, he refused to ascribe reality to anything not directly accessible to operational measurement. The refusal was severe.

[49] He also wrote in (1951, p. 260) "…such operations must be capable of eventually, although perhaps indirectly, making connection with instrumental operations. Only in this way can the physicist keep his feet on the ground or achieve a satisfactory degree of precision; instrumental contact affords the only 'reality' which he accepts as pertinent for him." There are similar remarks in Bridgman (1938, p. 124).

While an electric field strength can be ascertained operationally at any point in space, that operational fact is insufficient to accord reality to the electric field itself (p.57):

> I believe that a critical examination will show that the ascription of physical reality to the electric field is entirely without justification. I cannot find a single physical phenomenon or a single physical operation by which evidence of the existence of the field may be obtained independent of the operations which entered the definition.

Here we see an alignment with many of the views of the logical positivists. Yet his views were likely not derived from them. As the early publication date of his essay (1927) suggests, Bridgman's views seem to have developed independently of those of the Vienna Circle. Their members make no appearance in his essay. He there reported (p. v) reading only Clifford, Stallo,[50] Mach and Poincaré, but denied (p. vi) any conscious debt even to them. A later autobiographical report emphasized the independence of the thinking that led to his 1927 essay. He recalled (1938, p. 115) that it came "only after at least ten years of more or less continuous pondering." Bridgman later wrote on Carnap and logical positivism, but only to disavow allegiance to their views (1959, p. 522):

> However, the fact that I appreciate the importance of the verbal operations does not mean that I now ascribe the exclusive significance to a linguistic sentential analysis that many of the logical positivists do, as exemplified for example by Carnap. It has always seemed to me that linguistic analysis of this sort is capable of giving only a partial and distorted picture of the whole situation.

5.5 Meaning

The identification of meaning is the principal vehicle used by Bridgman to protect physics from spurious ideas. The celebrated formulation of operationalism in the 1927 essay was quoted above: "we mean by any concept nothing more than a set of operations" and "*the concept is synonymous with the corresponding set of operations*." Here "synonymous" is just "has the same meaning." Troublesome concepts, such as Newton's Absolute Time, are dispatched as meaningless. Bridgman devoted several pages (1927, pp. 28-31) to discussion of "Meaningless

[50] Stallo, presented himself as a reformed Hegelian (1884, p. 11) and lamented "the metaphysical malady which seems to be one of the unavoidable disorders of intellectual infancy."

Questions." These are questions whose answers would require operations that do not exist. A simple example is the question of whether a star is at rest. Presumably Bridgman intended that no operation can distinguish its rest in special relativity from its inertial motion.[51]

That Bridgman is prepared to attribute meaning to a question indicates that he is more a physicist than a philosopher. For philosophers with a grounding in logic attribute meaning to terms and propositions, not to questions. Bridgman's meaningless questions correspond to the logical positivists' "pseudo-questions." We may wonder just what the term "Logic" in the title of Bridgman's 1927 essay designates. It presumably designates some aspect of the informal inferential practices of physics.

Bridgman's later synopsis gives a more succinct statement of the centrality of meaning in his analysis (1951, p. 257):

> The fundamental idea back of an operational analysis is simple enough; namely that we do not know the meaning of a concept unless we can specify the operations which were used by us or our neighbour in applying the concept in any concrete situation.

Bridgman then added a puzzling qualification: a specification of the operations is necessary for meaning, without assuring sufficiency. That is, he wrote (1951, pp. 257-58) "In making an operational analysis we are dealing with necessary, as distinguished from sufficient, conditions."

This qualification seems to contradict the above 1927 formula "*the concept is synonymous with the corresponding set of operations*." For synonymy makes the operations both necessary and sufficient for the meaning of the concept. Perhaps Bridgman's intent here was to allow for other looser meanings that concepts may have. He noted elsewhere (1959, p. 523):

> … in every-day usage "meaning" has much wider connotations. For some sort of "meaning" can be ascribed to any expression whatever, and in fact it is always possible to give an operational analysis of any such meaning, provided only that the accepted operations are selected from a sufficiently extensive repertory.

[51] The example fails in the FLRW spacetimes of modern cosmology. The motion of a star with respect to the cosmic rest frame can be determined by measuring the Doppler shift in the cosmic background radiation within the rest frame of the star.

Specifying operations would be necessary if the concept is to have a meaning suitable for scientific applications. How their specification would not provide the wider connotations of meaning in every-day life.

5.6 Influence

While the work of the logical positivists and logical empiricists exercised a controlling influence in some sectors of philosophy for a few decades, their influence came to be largely negative. New schools of thought defined themselves through their opposition to logical positivism and empiricism, even while often only having a thin grasp of the views opposed. Such was not the fate of Bridgman's operationalism. The utility of seeking operational definitions has persisted as a convenient tool of analysis, while commonly used without a commitment to a larger operationist orientation. The identification of an operational definition of a concept or the realization that such a definition is lacking can advance a foundational analysis. It can secure an empirical foundation for the concept or suggest the lack of it. Here the imprecision of Bridgman's writing may have been helpful. It leaves room for later authors to mold operational thinking to their liking.

Behaviorism in 20th century psychology was based on the explicit adoption of an operationalist orientation. The term "operationism" (as opposed to "operationALism") was coined within the behaviorist literature.[52] In present day philosophy of science, the last stronghold of operationalist thinking lies in the interpretation of subjective probabilities. Agents are understood to harbor probabilistically weighted beliefs only in so far as those probabilities are expressible as actions. These actions may be the range of wagers the agents are willing to undertake when engaging with a bookie at a racetrack or, more generally, the agents' preferences in contexts requiring decisions.

Hempel's (1966, pp. 91-96) analysis is generally recognized as the most telling criticism of operationalism in the philosophical literature. The concern is that a strict adherence to operationalism renders science unworkable. Even the slightest change in a measuring operation introduces a new conception. A flood of conceptions is triggered that will overwhelm our cognitive powers. To illustrate his point, Hempel considered numerous operations for measuring lengths and temperatures. There can be no default supposition that each new conception agrees

[52] For details see Chang (2021, §1.4).

with the prior conceptions. Determining agreement or lack of it expands the analytic burden massively and would make the science unworkable. That they may not agree is precisely the motivation for Bridgman's introduction of operational definitions. We might read Bridgman's (1938, pp. 121-22) remarks as an earlier recognition and response to Hempel's objection. The concern is granted, but Bridgman insists that it is never "safe" to make the default assumption of agreement among different operations.

6. Conclusion

Earlier empiricisms, including that of the logical positivists and operationalists, harbored a severe form of skepticism. They recognized that propositions venturing beyond experience were less secure. However, they had no means with which to accord them a lesser, but still respectable status. It was all or nothing. Propositions were to be celebrated or discounted; and only expressions of experience were to be celebrated. In the most extreme version, the logical positivists had reduced anything that strayed beyond experience to meaningless babble.

Reichenbach's corrections to the excesses of logical positivism afforded the means of avoiding this skepticism and provides, in my view, the best expression of empiricism of the approaches reviewed in this chapter. Although the meaningfulness of propositions was still central to Reichenbach's analysis, the use probabilistic analysis allowed a wide range of propositions to be meaningful. Its use of probabilistic analysis avoided the insoluble problems created for Popper by his renunciation of inductive inference. The result was a fallibilist empiricism in which propositions that ventured beyond direct experience could still be treated as meaningful. Their probabilistic weight assured us of how seriously they should be taken, while at the same time preserving empiricism since those weights were controlled by experience. Hence Reichenbach could advocate his version of realism concerning the external world. Propositions about immediate experience and about states of affairs remote from experience were meaningful up to the probabilities assigned to them. This fortunate accommodation of empiricism and realism would not last.

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4. Constructive Empiricism

Chapter 4

Constructive Empiricism

1. Introduction

In his 1980 *The Scientific Image*, Bas van Fraassen proposed a version of empiricism called "constructive empiricism." It appeared at first to be a sober response and correction to the excesses of earlier empiricist accounts. It dispensed with the logical positivists' emphasis on formal logic and with their focus on meaning. Core statements of a scientific theory were to be understood literally, even if they were not to be believed. The monograph entered the literature just as the doctrine of scientific realism was gaining widespread acceptance. Van Fraassen's account became an irresistible target for the new generation of scientific realists; and the interest in constructive empiricism persisted for decades. One index of its prominence is that there were several volumes of critical essays written by leading philosophers of science on constructive empiricism. Van Fraassen himself included his lengthy replies in two of these volumes, in his van Fraassen (1985, 2007).[53]

The monograph, *The Scientific Image*, appeared to be the sober corrective just sketched. There were, however, curious elements in the view. It urged us to accept only the empirical adequacy of scientific theories. But it gave no guide as to which theories merited this acceptance and which did not. It was if the choice was just voluntary. The view was disconcertingly anthropocentric. It singled out for acceptance just what we humans could observe, unaided by any instrumentation. Critics pressed van Fraassen to concede that acceptance could be extended even if only slightly by our most secure of instrumental aids. Van Fraassen proved dogged in his resistance to even modest extensions. These were oblique indications of an extremism underlying the view.

A study of the original monograph and van Fraassen's later elaborations shows that a reappraisal is needed. What lies behind the view is an absolute skepticism about inductive inference. This skepticism was initially tacit and barely visible. Recognizing it is essential to

[53] The volume of writing on van Fraassen's constructive empiricism is so great that I have abandoned hope of surveying it and have limited the exposition here to a recounting of van Fraassen's own narratives.

understanding the larger project. Once it is seen, the project of constructive empiricism becomes clear. It is to give some account of science while *denying* that we can learn inductively from experience. Here the view is more extreme than the logical positivism of Carnap or the logical empiricism of Reichenbach. Both gave learning from experience inductively a central role. It is more extreme than Popper's anti-inductivism, since Popper offered his method of conjectures and refutations as a surrogate for inductive inference. Since empirical science is routinely conceived as our premier project for learning about the world from experience, it is hard to see how such a skeptical project as van Fraassen's can succeed.

That this inductive skepticism lies behind the original project was not initially apparent to van Fraassen himself. He reported that he only later came to realize the depth of his skepticism. In correcting what he later labeled as "My Mistake," he wrote (2007, p. 367):

> … a slogan as 'Experience is our only source of information'—or anything of that ilk—makes no literal sense, and cannot be the core of an empiricist position.

He could then turn to the curious boast of his (2002) characterization of empiricism: "experience is not mentioned at all."

If the author of the monograph of 1980 did not then appreciate its foundation in skepticism, then it is no wonder that readers like me have found the view opaque. It seems to be curiously dogmatic over what should otherwise be unimportant points, or, better said, points that would matter only to extreme skeptics. We have already seen that Sextus Empiricus was, contrary to his name, no empiricist in the philosophical sense by any definition of the term used over the millennia. He was a skeptic. We now see that van Fraassen's "constructive empiricism" is no empiricism at all, as the empiricist doctrine came to be understood in the twentieth century. A view that denies that we can learn from experience is not empiricist in this more recent sense, but deeply skeptical.

The sections that follow recount the thesis of the original monograph, its basic elements and the gradual emergence of the skepticism beneath them.

2. Constructive Empiricism of *The Scientific Image*

Constructive empiricism depends upon a division between what is observable and what is unobservable according to a theory of interest. A later example conveys the distinction well. It is, to quote the first section heading in van Fraassen (2001), "Light is not observable." The case of the rainbow indicates what van Fraassen had in mind. What we all communally observe is a spectrum of colors arrayed in an arch. That is the observable. We do not believe, however, that there is a real material thing that lies behind what we observe. There is, literally understood, no colored arch in the sky. We should, he urged, treat in the same way observations with optical instruments such as microscopes and telescopes; and indeed all observations engendered by instrumentation. We should not be lured into reifying as things something behind the observations. He wrote (2001, p. 157, his emphasis):

> The success of that family of instruments (microscope, electron microscope, radio telescope) derives in part from the possibility of representing their products as images of real things existing independently of any relations to those instruments. *But their products are images*; they are optically produced, publicly inspectable images. It is these images that are like the rainbow (they cannot themselves be represented as independent things). The difference, that we cannot think of the rainbow as [an] image of a real arch, while we can think of the microscope image as of a similarly structured object, is important but irrelevant for my point.

The generalization of this example comprises the canonical statement of constructive empiricism found in *The Scientific Image* (1980, p.12-13, his emphasis):

> *Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate.* This is the statement of the anti-realist position I advocate; I shall call it *constructive empiricism*.

The empirical adequacy of a theory resides just in its successful accommodation of observations. Theories of the rainbow are empirically adequate in just this sense. We see an arc of circular section at 40 to 42 degrees around the direction of sunlight, containing colors spread from red to violet; and that is just what we expect to see according to the optical theories of rainbows as light reflected from raindrops.

What attitude are we to take to the unobservable parts of a theory: the light as opposed to the observations it produces? Here the view is recovered from its denial of another view, scientific realism. The canonical statement of that view is given as (1980, p. 8, his emphasis)

> *Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true.* This is the correct statement of scientific realism.

Responding to the literal construal of theories of the realist, van Fraassen identified his version of antirealism. It reverses the commitments of an antirealism that seeks truth but not literal construal (1980, p. 10)

> … the language of science should be literally construed, but its theories need not be true to be good. The antirealism I shall advocate belongs to the second sort.

Literal construal does not require belief. Indeed, belief is accorded sparingly according to the formulation given a few pages earlier in the text (1980, p.4):

> On the view I shall develop, the belief involved in accepting a scientific theory is only that it "saves the phenomena," that is, correctly describes what is observable. But acceptance is not merely belief.

Providing this alternative to realism was, we are assured in the very first sentence of the volume, motivating (1980. P. vii) "The aim of this book is to develop a constructive alternative to scientific realism,…" The "constructive" in constructive empiricism reflects this agnosticism about the unobservable parts of a scientific theory (1980, p. 5)

> I use the adjective "constructive" to indicate my view that scientific activity is one of construction rather than discovery: construction of models that must be adequate to the phenomena, and not discovery of truth concerning the unobservable.

Talk of a colored arc of circular section in theories of the rainbow mean just what they say. There is an arc with the requisite colors. However, the talk is not to be believed. There is no real arc in the sky. It is our construction and serves to return our observations of rainbows.

3. Antirealism and Anti-metaphysics

In its antirealism, constructive empiricism had chosen a new opponent. The target of the logical positivists was the metaphysics of the nineteenth and early twentieth century German idealists. Their claims were deprecated as meaningless gibberish. The target for constructive empiricism was not abstruse philosophical pronouncements. It was the core claims of routine science; or, more precisely, that they should be believed as true. Van Fraassen maintained the polemic against metaphysics of the earlier positivists and logical positivist. He explained in his 1980 monograph that its suppression was an achievement of constructive empiricism (1980, p. 73):

> However, there is also a positive argument for constructive empiricism—it makes better sense of science, and of scientific activity, than realism does and does so without inflationary metaphysics.

However here the notion of metaphysics was transformed into something that might seem benign. He later clarified the term (2002, p. 37):[54]

> What exactly are the targets of the empiricist critique? As I see it, the targets are forms of metaphysics that (a) give absolute primacy to demands for explanation and (b) are satisfied with explanations by-postulate, that is, explanations that postulate the reality of certain entities or aspects of the world not already evident in experience.

A more complete portrait of this target is given in *The Scientific Image*. It is a particular argument for scientific realism, now commonly known as the "no miracles argument." The success of science, the argument goes, would be miraculous if the science were incorrect. Its truth is the best explanation of its success. The version van Fraassen gives at the outset of Chapter 5 "The Pragmatics of Explanation" is (1980, p. 98):

> … science aims to find explanations, but nothing is an explanation unless it is true (explanation requires true premisses); so science aims to find true theories about what the world is like. Hence scientific realism is correct.

The chapter undoes the argument by identifying a pragmatic element in explanations. They succeed in satisfying a pragmatic interest that will vary from questioner to questioner. The link between successful explanation and truth is thereby broken.[55] The 2002 characterization of the metaphysics targeted continues with a characterization of empiricism as requiring precisely the denial of this metaphysics (2002, p. 37)

> The empiricist critiques I see as correspondingly involving
>
> (a) a rejection of demands for explanation at certain crucial points and

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[54] An almost identical formulation is found in the earlier van Fraassen (1994, p. 311).

[55] Related to this rebuttal is a celebrated riposte earlier in the text (1980, p. 40): "I claim that the success of current scientific theories is no miracle. … For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which in fact latched on to actual regularities in nature."

> (b) a strong dissatisfaction with explanations (even if called for) that proceed by postulation.
>
> I will refer to these as the first and second characteristics of empiricism. Others may be added as we go along.

Where Carnap dismissed as gibberish the metaphysics of Heidegger's "*Das Nichts selbst nichtet,*" van Fraassen assailed metaphysics by adopting agnosticism[56] over whether we truly see a bacterium through a microscope.

4. Set Theory, not Formal Logic

What has been left open above is precisely how van Fraassen's constructive empiricism characterized theories. In the older logical positivist and logical empiricist tradition, theories were collections of sentences, organized axiomatically. Their structure was captured most precisely by representing the theories in a formal logic, most commonly first order predicate logic. The theorists of this earlier tradition were greatly impressed by the major strides then being made in symbolic logic that ultimately issued in Kurt Gödel's extraordinary undecidability and incompleteness proofs. The hope was that this fertility could be transferred into philosophy of sciences. It was not to be. Impressive as these results were, they obscured foundational problems in science behind a screen of problems that were artefacts of the formal logic and not of the science itself.

A correction was overdue and, by the time of *The Scientific Image*, it was well underway through the work of such philosophers as Patrick Suppes. He advocated the use of far simpler set theory in place of the formal logic and metalogic of the logical positivists. Van Fraassen reported this correction with approval (1980, p. 65-66, his emphasis):

> When Patrick Suppes first advocated this sort of picture of theories in his studies of mechanics (with the slogan that *philosophy of science should use mathematics, and not meta-mathematics*), he proposed a canonical form for the formulation of theories. This used set theory.

[56] The term is van Fraassen's (1980, p. 72) "I wish merely to be agnostic about the existence of the unobservable aspects of the world described by science—but sense-data, I am sure, do not exist."

Van Fraassen identified, correctly in my view, how the focus on formal logic had led philosophers of science astray (1980, p. 56):

> The syntactically defined relationships are simply the wrong ones. Perhaps the worst consequence of the syntactic approach was the way it focused attention on philosophically irrelevant technical questions. It is hard not to conclude that those discussions of axiomatizability in restricted vocabularies, "theoretical terms," Craig's theorem, "reduction sentences," "empirical languages," Ramsey and Carnap sentences, were one and all off the mark—solutions to purely self-generated problems, and philosophically irrelevant. The main lesson of twentieth-century philosophy of science may well be this: no concept which is essentially language-dependent has any philosophical importance at all.

In place of the propositions and well-formed formulae of predicate logic, van Fraassen offered simple structures in set theory. A simple example—my example—gives enough of a sense of it.[57] Assume we wish to present a theory of a real-number-valued magnitude, such as the masses of bodies. We could write a set of axioms that specify the properties of the real numbers and make them the axioms of mass. The result would be complicated and needlessly so. For all that matters can be recovered simply by positing that mass can take on all positive real number values. That is, we specify a mathematical structure—the set of positive real numbers—as the model for mass.

This model has continuum many possible values. They are far more than ever could be identified empirically. Any actual measurement of mass using some scale or balance will tell us that the mass of interest is such and such a multiple of some chosen unit mass. Crucially, the multiple cannot be recovered with infinite precision. While there may be a mass of magnitude π, even our most accurate measurements will only return some truncated approximation for it. It might be 3.14 = 314/100, or 3.14159 = 314,159/100,000 or something longer. All such measurement results will be rational numbers. They are the observationally or empirically accessible portions of the theory.

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[57] Van Fraassen's (1980, Ch. 3) examples are more elaborate and involve geometry and formulations of Newtonian and quantum mechanics.

More precisely, the set of positive rational numbers comprises the empirical substructure of the theory. This is the observable part of the theory to which constructive empiricism directs acceptance. We are to understand that the theory says that masses can have any real value. But we are not to believe it, since observation will only ever return rational values.

Here is how van Fraassen (1980, p. 64) described the general case:

> To present a theory is to specify a family of structures, its models; and secondly, to specify certain parts of those models (the empirical substructures) as candidates for the direct representation of observable phenomena. The structures which can be described in experimental and measurement reports we can call appearances: the theory is empirically adequate if it has some model such that all appearances are isomorphic to empirical substructures of that model.

This semantic replacement of the earlier syntactic view of theories proved especially welcome in the philosophy of physics of the 1980s. For a newly energized philosophy of physics was drawing heavily on the representations given to physical theories by mathematically-minded physicists. Their approach was to specify a theory through its models, where a model would most commonly be a tuple of mathematical structures, just like those described by van Fraassen. While this semantic approach was appealing to philosophers of physics, it was less so to philosophers of other sciences whose foundational conceptions were poorly suited to simple, set theoretic capture.

5. Observation

A distinctive feature of constructive empiricism is its strict division of the unobservable from the observable. The division is unapologetically anthropocentric. What is observable is whatever we unaided humans can observe. The obvious worry is the contingency of our human powers of observation. If they were somehow to change, would the range of the observable correspondingly change? Van Fraassen has, from the first writings, given a resounding *yes* to this question. He wrote (1980, p. 18):

> At present, we count the human race as the epistemic community to which we belong; but this race may mutate, or that community may be increased by adding other animals (terrestrial or extra-terrestrial) through relevant ideological or moral decisions ("to count them as persons"). Hence the anti-realist would, on my proposal, have to accept conditions of the form

> If the epistemic community changes in fashion Y, then my beliefs about the world will change in manner Z.

This view is baffling to those who expect an empiricist philosophy of science to give some account of how we can learn something more about the world from experience, even if the compass of what we learn is narrow. For the constructive empiricist, anything outside our immediate observations is beyond our reach. The excess of the view is so palpable that it seemed to critics that only a slight nudge would be needed to move van Fraassen to a more expansive view.

These nudges came in many forms, many of them already familiar to van Fraassen. They were in Grover Maxwell (1962), a work that *The Scientific Image* had already lauded as (1980, p. 14) "the locus classicus for the new realists' contention that the theory/observation distinction cannot be drawn." One approach conjures up a slippery slope along which any placement of the observable/theory distinction seems arbitrary. Maxwell (1962, p. 7) wrote of:

> … a continuous series beginning with looking through a vacuum and containing these as members: looking through a windowpane, looking through glasses, looking through binoculars, looking through a low-power microscope, looking through a high-power microscope, etc., in the order given. The important consequence is that, so far, we are left without criteria which would enable us to draw a nonarbitrary line between "observation" and "theory."

Or we may imagine some minor or not so minor augmentation of our human powers of observation. Maxwell (1962, p. 11) imagined:

> … a human mutant is born who is able to "observe" ultraviolet radiation, or even X rays, in the same way we "observe" visible light.

These and many variations arise so easily that van Fraassen must surely have wearied of their familiar repetition. In responding to critics in a volume of essays on his empiricism, he remarked (1985, pp. 255-56) on "…the many independent challenges to where I 'draw the line' on observability…." He listed challenges by Paul Churchland in the volume as (p. 256):

> First, there is the case of the man all of whose sensory modalities have been destroyed and who now receives surrogate sensory input electronically. … Next, he asks us to envisage an epistemic community consisting entirely of beings in this predicament. Thirdly, Churchland imagines that we encounter a race of humanoids

> whose left eyes have the same structure as a human eye plus an electron microscope. Science tells us that virus particles and individual DNA strands are observable to them.

Van Fraassen remained unmoved in the face of these challenges. We need not rehearse his replies since they are predictable: what is observable is just what is observable to the community of human observers, properly conceived. Any extension is simply to be denied.

Van Fraassen's recalcitrance is baffling only as long we harbor the thought that his empiricism seeks to understand just how much we, as empiricists, can learn of the world from experience. If, however, van Fraassen's view is not empirical in this sense but deeply skeptical, then we should expect a dogged retraction to the absolute minimum that must be conceded from human observations. For insisting on the minimum is what skeptics do.

6. Anthropocentrism

Van Fraassen's account of observation and belief is curiously anthropocentric. Our beliefs are held hostage by the specific observational powers of our community. Migratory birds have the ability to sense the earth's magnetic field directly. Were we to gain that faculty, we would then be authorized by constructive empiricism to form beliefs about the existence of this field. Absent this faculty, we may believe that we have detected and mapped out the configuration of the earth's magnetic field factually in detail using compass needles and other methods. Constructive empiricism denies us this belief. Someone unrestrained by constructive empiricism may well find that our probing of the earth's magnetic field actually does better at identifying this field than the sensory faculty of migratory birds.

That van Fraassen must deny this indicates a very different conception of how we engage with the world through our experiences. A striking passage in van Fraassen (1994, p. 312) may provide a clue to the anthropocentrism of van Fraassen's conception and is worth quoting in its entirety:

> Suppose that, in a philosophical way, I do not understand ethics or science or religion. It might be one thing to take me by the hand and lead me into relevant experience. That might allow me to acquire a deeper sense of insight into those aspects of human existence. It would be quite another thing—and to the empiricists of little or no value—to postulate that there are certain entities or realms of being about which ethics (or science, or religion) tells us a true story. Yet that is what

> philosophers have often tended to do: to "explain" ethics by the contention that ethical principles are just the (putative) truths about Values, scientific theories the putative true summary of the Laws of Nature, and religious doctrines the putative true description of a divine, extramundane reality. Such philosophical accounts tend to be backed up with the assertion that unless we can think of the relevant text as purporting to be a true story, there is no explaining or understanding the subject at al1.[58] The empiricist response is to deny firstly the value of any such 'explanation', and secondly the reasons anyone might have for thinking it to be true, and then furthermore to reject the legitimacy and appropriateness of that demand for explanation itself.

Van Fraassen here recounts a view of ethics and religion, in which ethical and religious experiences provide deeper insights in those realms. Those insights do not amount to learning truths about an ethical or a divine reality. The experiences in these two areas are highly personal and thus automatically anthropocentric.

One reaction to this non-factive view of ethics and religion is that reform is needed. Ethics and religion should make factual claims about the world and these factual claims should be supported by evidence in the same way as are the factual claims of a science. That is, we might ask ethics and religion to adopt the attitudes and methods of empirical science, as realists understand them.

Constructive empiricism proceeds in the reverse direction. It asks science to adopt the attitudes that van Fraassen reports as being within ethics and religion. In them, personal experience is central and inferences to facts beyond them are disavowed. The anthropocentrism of constructive empiricism thereby arises automatically. At the same time, ethics and religion are

[58] Van Fraassen's footnote (his emphasis): "A fourth example is the assertion: if mathematics is not the true description of a platonic realm of abstract entities, and also does not just consist of logical tautologies, then you can't explain *why* it is useful for science."

protected from complaints that they are unscientific. Ethics, religion and science would equally eschew the search for facts beyond human experience.[59]

7. Acceptance

The central notion of constructive empiricism is that of "acceptance." It figures in the canonical statement of the view "*…and acceptance of a theory involves as belief only that it is empirically adequate.*" At first and in earlier of van Frassen's writings, the notion of acceptance seems straightforward. The canonical statement explains the term "empirically adequate" hesitantly (1980, p. 12):

> For now, I shall leave that with the preliminary explication that a theory is empirically adequate exactly if what it says about the observable things and events in this world, is true—exactly if it 'saves the phenomena.' A little more precisely: such a theory has at least one model that all the actual phenomena fit inside. I must emphasize that this refers to *all* the phenomena; these are not exhausted by those actually observed, nor even by those observed at some time, whether past, present, or future.

Similar sentiments appear a few pages later (1980, p. 69):

> … empirical adequacy goes far beyond what we can know at any given time. (All the results of measurement are not in; they will never all be in; and in any case, we won't measure everything that can be measured.)

Were this any other author, the import of these words would be easily understood. While we can only ever affirm a tiny part of the observational consequences of a theory, success there can be strong enough inductively to assure us of the continued success of the theory observationally.

The difficulty is that *this* author has disputed energetically that the observational success of a science supports its truth, as scientific realists have urged it does. Can a constructive empiricist then allow the same inference over the observational portion of a theory? Van Fraassen's explicit recognition of the problem was triggered by a remark that is otherwise routine in constructive empiricism (1980, p. 71):

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[59] Hence van Fraassen can conclude (1985, p. 248) " … belief in angels or electrons or the truth of theories in molecular biology does not *ipso facto* make one irrational." For more on this and van Fraassen's rejection of inductive reasoning, see Chapter 11.

> … when the theory has implications about what is not observable, the evidence does not warrant the conclusion that it is true.

He then immediately sensed the peril:

> The danger is clearly that, by parity of reasoning, my arguments would, if successful at all, establish that the evidence never warrants a conclusion that goes beyond it. This is already quite unacceptable, for we do in our daily life infer, or at least arrive at, conclusions that go beyond the evidence we have, and will resist as sophistical any philosophical theory which calls us irrational for that reason alone.

The following two pages of text purport to give a preliminary answer and escape. I have not been able understand them.

The tension continues. We see it appearing in his 1985 responses to criticism, where he sought to separate acceptance from belief (1985, p.295):

> What was valuable in the literature on induction were its more or less careful accounts of reasons for acceptance of theories. Of these, reasons to believe are a proper subclass, and the great tragedy of this subject, which turned the idea of induction into an impossible ideal, was to confuse reasons for acceptance *überhaupt* with reasons for belief. In my view, as long as we try to maintain that conflation, we cannot make sense of either scientific practice or methodology.

If—and I am unsure that this is so—"acceptance" here includes a constructive empiricist's acceptance of the empirical adequacy of a theory, then its acceptance can be something other than belief. Then a belief in the theory's continuing success with observables would be lost. What might be left? Here we might recall that the accounts of acceptance of empirical adequacy routinely add extra elements. For example (1980, 12):

> … acceptance involves a commitment to confront any future phenomena by means of the conceptual resources of this theory. It determines the terms in which we shall seek explanations.

And (1985, p. 281):

> What is there in acceptance besides belief, …? … In addition, the acceptance involves a commitment to maintain the theory as part of the body of science. That means that new phenomena are confronted within the conceptual frame of the

> theory, and new models of phenomena are expected to be constructed so as to be embeddable in some models of that theory.

It is attitudes and behaviors specified here, not beliefs. Eventually, with the development of the notion of a "stance" sketched below, they are all that will remain. The simple notion of belief will be lost.

8. Anti-inductivism

That there is something missing from the accounts of both scientific realism and constructive empiricism only becomes noticeable when it is pointed out. And then its omission is hard to ignore. In both canonical formulations of Section 2 above, acceptance figures centrally. In each we either accept a theory as literally true or as merely empirically adequate. However we surely do neither without further conditions. We need some assurance that these acceptances are appropriate. They are surely not merited by every theory. In statements of scientific realism elsewhere, that assurance comes, in some form, typically through the weight of evidence. In motivating his characterization of scientific realism, van Fraassen first gave a formulation due to Richard Boyd, quoted from the writings of Hilary Putnam. It begins (1980, p. 8, my emphasis):

> That terms in *mature* scientific theories typically refer…, that the theories accepted in a *mature* science are typically approximately true,…

Reference and truth are accorded only to *mature* scientific theories. In the source from which van Fraassen quotes, Putnam (1975, p. 73) does not elaborate on what distinguishes a scientific theory as mature. My default presumption is that maturity comes when a theory has been secured so massively by the weight of evidence that serious competition is not expected or even entertained. Whatever construal may be intended, very few of all possible theories can be so designated. Something similar is needed here. We would not judge a theory empirically adequate if it has merely had one, minor predictive success.

We might imagine that this extra condition does not belong in a treatise on constructive realism. Might these inductive considerations belong elsewhere and simply be assumed here? Possibly. But even so, there should be some indication, some tip of the hat as given by Putnam, to the idea that evidential or inductive considerations must enter in some manner in the decision to accept. But there is none in van Fraassen's narratives. Without them, acceptance is treated as a decision willfully made, voluntarily. We shall see shortly that this later becomes van Fraassen's explicit view, the empirical stance.

It may be tempting to discount the omission of inductive considerations as something whose explicit discussion lies outside the scope of the discussion of constructive empiricism. Perhaps it is present tacitly, as in Putnam's mention of *mature* theories. We can see that matters are otherwise. Elements of anti-inductivism are already present in the rejection of scientific realism in *The Scientific Image* (Ch.2, §3, pp. 19-23, emphasis in original). There it notes that "[t]he main rule of inference invoked in arguments of this sort is the *rule of inference to the best explanation*." The goal of the ensuing discussion is to defeat this abductive pathway to scientific realism by weakening the scope of the rule. A later summary of Chapter 2 on realism directly denies that evidence can ever establish the truth of unobservables (1980, p. 71): "when the theory has implications about what is not observable, the evidence does not warrant the conclusion that it is true."

A later analysis uses the metaphor of "a man in a glass booth" as a vehicle for urging that the notorious problem of the priors defeats Bayesian accounts of learning theory from observational evidence (1985, p. 250, emphasis in original):

> The basic picture of the deliverances of experience the *revelation model of evidence* is of a man in a glass booth with a ticker tape that prints out statements which he treats as divine revelation. Each time a new such statement appears, he becomes fully certain that it is true, and the only, other thing he does is to adjust his prior opinions to accommodate it (i.e., he conditionalizes on this evidence). It is clear that, if the ticker tape has been delivering only nontheoretical statements, then his theoretical opinions derive mainly from the prior opinions he brought with him to this situation. No proofs about how the evidence can, in the long run, swamp any given prior opinion can take the edge of this damning point: that he can today have no significant theoretical opinions unless his prior opinions also had significant theoretical content.

These denials fall short of a complete repudiation of entire project of inductive logic. Such repudiations soon follow. They come in the form of a startling announcement (1985, p. 295):

> Inductive logic is a make-believe theory; no one has ever written its principles. Attempts to do so have always landed in incoherence or fallen afoul of hilarious

counterexamples.[60] But, reluctant to admit that they were only talking about a gleam in their eyes, philosophers always pretended, at least in terminology, that there is such a discipline as inductive logic.

A comparably startling assessment was given over 20 years later in response to remarks by Nancy Cartwright (2007 pp. 343-44, his emphasis):

> I do not think that there is such a thing as Induction, in any form, and I would also express this in more or less her [Cartwright's] words: *there is no purely epistemic warrant for going beyond our evidence.* If there is to be a rationale—let alone justification—for selecting the observable as the range for the proper epistemic aspect of acceptance, that will have to be *something that is not, or not purely, epistemic warrant.*

9. The Empirical Stance

If the entire project of induction and inductive logic is abandoned, we lose the instrument essential to the project of learning about the world from observation. Whatever his empiricism might be, it cannot support this last project. Van Fraassen later gave autobiographical recollections of this realization. He wrote (2007, pp. 366-67)

> 3.1 My Mistake
>
> My own thinking about empiricism involved to begin a great mistake. Throughout the 1980s I remained in thrall to one of the great deceptions in textbook philosophy. Both in my reply to critics (1985: 286) and in Laws and Symmetry (p. 8) I wrote of empiricism as the position that experience is our one and only source of information about the world.
>
> … The latter day conception of experience signalled in that 'source of knowledge' slogan is a curious philosophical miscreant: something like 'a psychic event involving a single individual, with a ''content'' logically independent of what is

[60] [van Fraassen's footnote, his emphasis] "Some readers have apparently thought that, in chapter 2, section 3 of the *The Scientific Image*, I assert inference to the empirical adequacy of the best explanation to be a correct inductive principle. Not so; I *exhibited* this putative rival principle as part of a demonstration that we can have no good evidence for the psychological hypothesis that people do in fact follow the rule of inference to the best explanation."

happening to that individual.' It is a mystery how anyone could mention this with a straight face as the basis on which scientific knowledge is built—or attribute that view to anyone else. But I was steeped in the textbook history of philosophy which depicts the rationalism–empiricism prelude to Kant in just that way, and taken in by it.

…When I started thinking for myself about this, I became aghast at the implications of what this could mean, and at the disconnect in my own thoughts between this 'official' view of empiricism and what I really knew about it….

Since the content of these moments of self-realization is startling, I have quoted them at some length. Once they had taken hold of him, van Fraassen saw the need to provide a new understanding of the claims of his empiricism. This new understanding, van Fraassen continued, came in the form of "stances," as elaborated in works in a paper (1994) and his 2002 monograph, *The Empirical Stance*.

The first step in these developments of the notion of a stance is negative. It is to impugn the sorts of factual principles that might otherwise be taken to underpin empiricism. One target is the principle (1994, p. 312):

(*) Experience is our one and only source of information.

An extensive analysis in the paper (1994) and the later monograph (2002) leads to the rejection of all principles of this type. The reasons are various but rely in parts on an accusation of self-refuting circularity. One conclusion is important in its decoupling of attitudes and beliefs (1994, p. 326):

The only way out is to deny the initial bias: rationality does not require our attitudes to be justified by beliefs.

Both texts then offer the notion of a "stance" as the right way to understand empiricism. In one version, it is described as (2002, pp. 47-48):[61]

A philosophical position can consist in a stance (attitude, commitment, approach, a cluster of such-possibly including some propositional attitudes such as beliefs as

[61] The same formula appears in van Fraassen (1994, p. 318) "… a stance (attitude, commitment, approach). Such a stance can of course be expressed, and may involve or presuppose some beliefs as well, but cannot be simply equated with having beliefs."

> well). Such a stance can of course be expressed, and may involve or presuppose some beliefs as well, but cannot be simply equated with having beliefs or making assertions about what there is.

This understanding of empiricism leads, in van Fraassen's view, to what would otherwise be regarded as a complete abandoning of empiricism. His autobiographical reflections note (2007, p. 368):

> In my own proposal for a characterization of empiricism (beginning with *The Empirical Stance*, pp. 36–8, 46–8, 62–3) experience is not mentioned at all. If I had succeeded in all I set out to do, the 'sole source of information' slogan about what experience is would not be discussed any more in connection with the formulation of empiricism.

The characterization given in the places cited is almost entirely negative. Its core ideas were quoted in Section 3 above. They are based around a denial that explanatory success authorizes any inferences. This is not empiricism in the twentieth century sense, but a form of inductive skepticism targeted specifically at these forms of inductive inference. They happen to be just those often used in support of scientific realism. An earlier statement of this characterization is more explicit in its dispensing with beliefs. Van Fraassen wrote (1994, p. 317-318, emphasis in original):

> For [the characterization of] empiricists I listed *rejection* of explanation demands, *dissatisfaction* and *disvaluing* of explanation by postulate. Moreover I listed the empiricists' calling us back to experience, their rebellion against theory, their *ideals* of epistemic rationality, what they regard as having significance, their *admiration* for science and the virtue they see in an idea of rationality that does not bar disagreement. Notice that not a single one of these factors is a belief.

A conception of empiricism that does not require beliefs or assertions about what there is would seem to leave little room for a conception of scientific rationality. Yet van Fraassen averred otherwise (2002, p. 63):

> Empiricism may also be approached through reflection on its positive attitude toward science. But this admiring attitude is not directed so much to the content of the sciences as to their forms and practices of inquiry. Science is a paradigm of rational inquiry. To take it as such is precisely to take up one of the most central

> attitudes in the empiricist stance. But one may take it so while showing little deference to the content of any science per se.

Here empiricism is reduced to an admiration for the practices of science, while discounting what these practices purport to have achieved. It is a most curious attitude. If we cannot admire the practices of science for their successes, then what in them is to be admired?

Finally, we can note, tangentially, that this overall approach conforms with van Fraassen's earlier voluntarist approach to rationality. According to it, our beliefs are not compelled upon us by external factors. Rather he formulated his view as follows (1984, p. 256, his emphasis):

> I call it "voluntarist," because it makes judgment in general, and subjective probability in particular, a matter of cognitive commitment, intention, *engagement*. Belief is a matter of the will.

10. Stances Assessed

What are we to make of the conception of empiricism as a stance? We can, if we are so inclined, take it as a voluntary choice to conceive of things in certain ways. When we are free to make such choices, many are available. We might choose among these:

> The world is benevolent.
>
> The world is malevolent.
>
> The world is beautiful.
>
> The worlds is ugly.

We can include in the choices Einstein's famous remark:

> The Lord is subtle, but he is not malicious.

The present question is what they have to do with the empirical project of learning about the world from experience. In so far as they are *only* freely chosen attitudes, they can only be of personal significance to us. We may find solace in the idea of the beauty in or the benevolence of the world. But we have no assurance that these attitudes will be epistemically fertile in the project of learning about the world.

There is another possibility. These sorts of slogans can aid in the project if they coincide with factual properties of the world. This can happen in many ways. If the world is beautiful in the sense of mathematical Platonism, then knowing this fact offers us a significant epistemic advantage. Einstein's remark reflected his belief in a factual aspect of the world: physical laws may be hard to find, but they are such that finding them is not beyond our human capacities. In his later life he had come to believe that these laws could best be found by the Platonic route, that is, by investigating the mathematically simplest laws.[62] When the epistemic power derives from such factual conditions, its success is unconnected with the free choice of attitudes and stances. All that matters is that the attitude chosen happens to coincide, perhaps even by design, with the factual character of the world.

In sum, if we understand empiricists to be engaged in learning about the world from experience, then epistemic stances can be assured to have a role in their project only in so far as they are not freely chosen stances as characterized by van Fraassen.

11. Conclusion

This chapter has taken a more critical approach to constructive empiricism than it has to other views surveyed. It is needed. Constructive empiricism has become the definitive formulation of empiricism over the last half century. Yet it represents a retrograde development in empiricist thought. As I have discussed elsewhere in this volume, there are two elements in empiricist thinking. The first is a privileging of experience in our efforts to learn about the world. The second is a skeptical restriction on what we can learn from experience. Developments earlier in the twentieth century strengthened this first element. Most notably Reichenbach's logical empiricism developed realist themes that came to a fuller expression in the subsequent emergence of scientific realism. The second skeptical element was suppressed; and that suppression was, at least to me, most welcome.

Constructive empiricism represents a degeneration of this positive trend. It is an atavism that returns to what made the empiricism of earlier centuries an unpopular doctrine, widely judged as unacceptable. That is, it has revived the most skeptical strands of empiricism, the second element. It has excised completely what is to me the most essential element of empiricism, the privileging of experience in our efforts to learn about the world.

It is an aberration of later twentieth century philosophy that the term "constructive empiricism" was accepted as its label. It should have been called "constructive skepticism." For it is the *skeptical* view that we learn nothing new from observation. All our efforts to extend

[62] For an account of how Einstein came to this view, see Norton (2000).

knowledge by even the smallest steps beyond what we observe are merely *constructions*, chosen voluntarily by us.

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5. Experience as Sense Perception

Chapter 5

Experience as Sense Perception

1. Introduction

This chapter provides a brief survey of the cognitive notion of experience as a counterpoint to the propositional notion to be developed in the chapters to come. The section to follow reviews this cognitive notion in the tradition of British empiricism. Section 3 recounts how that view persisted in the writings of empiricists in the early twentieth century. The final section recalls how empiricist writing has bifurcated into the literature in philosophy of science and a largely separate literature in epistemology. In so far as the modern literature in philosophy of science takes van Fraassen's constructive empiricism as definitive, it employs a cognitive notion of experience more extreme than that of the early logical positivists and logical empiricists. The notion of experience in modern empiricism is a direct descendent of the cognitive notion employed by the British empiricists.

2 Experience in British Empiricism

The modern tradition in empiricism takes as is founding figures those philosophers who later came to be know at the British Empiricists. One of the earliest is Thomas Hobbes. He commenced Chapter 1 of his *Leviathan* (1651) by declaring that (p. 3) "Thoughts of man …they are every one a *Representation* of Apparence, of some quality, or other Accident of a body without us…" He then developed an account of sense experience as caused by external bodies and arising in a familiar inventory of the types (p. 3, his emphasis):

> The cause of Sense, is the Externall Body, or Object, which presseth the organ proper to each Sense, either immediately, as in the Tast and Touch; or mediately, as in Seeing, Hearing, and Smelling…
>
> And this *seeming*, or fancy, is that which men call Sense; and consisteth, as to the Eye, in a *Light* or *Colour figured*; To the Eare, in a *Sound*; To the Nostrill, in an *Odour*; To the Tongue and Palat, in a *Savour*, And to the rest of the body, in *Heat*, *Cold*, *Hardnesss*, *Softnesse*, and such other qualities, as we discern by *Feeling*.*

He subsequently (Ch. 3, p. 8)[63] recounted the activity of thinking as the connection of thoughts into "traynes," which formed "mental discourse" It was distinguished from "discourse in words," which would align roughly with relations among propositions.

Locke's (1689) *Essay* similarly distinguished words from thoughts. He wrote in the introductory Epistle to the Reader "I know there are not Words enough in any Language, to answer all the Variety of Ideas that enter into Men's Discourses and Reasonings." In so far as logical analysis requires propositions to be expressed in words, Locke's view is that the richness of mental life outstrips logical analysis.

Hume's famous analysis of causation found nothing more in the relation of cause and effect than our habit of mind of connecting the two. He wrote in his *Enquiry* (1777, p. 75, Hume's emphasis):

> ... after a repetition of similar instances, the mind is carried by habit, upon the appearance of one event, to expect its usual attendant, and to believe it will exist. This connexion, therefore, which we *feel* in the mind, this customary transition of the imagination from one object to its usual attendant, is the sentiment or impression from which we form the idea of power or necessary connexion.

For Hume, cause and effect are to be understood within cognitive analysis. It is a mental process connecting cognitive states representing cause and effect. The duality of cognitive and propositional analysis enters Hume's analysis in the negative. Famously, he determined that the passage from the cognitive state of cause to that of effect cannot be vindicated by logical analysis. He wrote ((1739, p. 36):

> We have said that all arguments concerning existence are founded on the relation of cause and effect; that our knowledge of that relation is derived entirely from experience; and that all our experimental conclusions proceed upon the supposition that the future will be conformable to the past. To endeavour, therefore, the proof of this last supposition by probable arguments, or arguments regarding existence, must be evidently going in a circle, and taking that for granted, which is the very point in question.

[63] "By *Consequence* or TRAYNES of Thought, I understand that succession of one Thought to another, which is called (to distinguish it from Discourse in words) *Mental Discourse*."

3. Early Twentieth-Century Scientific Empiricism

The early twentieth century saw a flourishing of epistemology that paid special attention to science. One might have expected that this new tradition would break with the heavily psychological notion of experience in favor of one less dependent on the ever-troubling vagaries of individual minds. Or perhaps if "science" meant "empirical science," we might hope that a psychological treatment of experience would draw on empirical investigations in psychology as opposed to introspection and armchair theorizing. Instead, we find little empirical advance over earlier treatments of experience.

In the British Cambridge tradition, G. E. Moore introduced a term in 1909-10 that came to be used heavily: "sense-data." It was quite explicitly introduced within reflections on psychology, as the title of his paper "The Subject-Matter of Psychology" indicates. He formulated it in terms that should surprise no modern reader. The formulation began (1909-10, p. 57):

> [My question] concerns those entities, which are often called "sensations" or "sense-presentations," but which I shall call, by preference, "sense-data." By sense-data I understand a class of entities of which we are very often directly conscious, and with many of which we are extremely familiar. They include the colours, of all sorts of different shades, which I actually see when I look about me; the sounds which I actually hear; the peculiar sort of entity of which I am directly conscious when I feel the pain of a toothache, and which I call "the pain"; and many others which I need not enumerate.

The term was picked up by Bertrand Russell and figures prominently in his 1915 *Our Knowledge of the External World.* We can see that he had become enmeshed in the psychological problems associated with this conception of experience. His 1914 "The Relation of Sense-data to Physics" posed this problem (p. 109):

> Thus if physics is to be verifiable we are faced with the following problem: Physics exhibits sense-data as functions of physical objects, but verification is only possible if physical objects can be exhibited as functions of sense-data. We have therefore to solve the equations giving sense-data in terms of physical objects, so as to make them instead give physical objects in terms of sense-data.

It might, I hope, now seem curious that a responsible empiricism for physics would require the solution of a problem that would tax greatly even our latest psychology and neuroscience.

The philosophers of logical positivism and logical empiricism also enmeshed their empiricism with problems of psychology. For them, sense perception arises at the level of the individual mind. We saw in Chapter 3 that the Vienna Circle manifesto proclaimed (Hahn, Neurath, Carnap, 1929, p. 309) "…there is knowledge only from experience, which rests on what is immediately given." That the "immediately given" refers to the sense perception of individuals is made clear by the discussion that follows. The meaning of statements and concepts is to be recovered reductively from "the given" through a hierarchical "constitutive system" whose lowest level lay in individual minds:

> … the lowest layers of the constitutive system contain concepts of the experience and qualities of the individual psyche; in the layer above are physical objects; from these are constituted other minds and lastly the objects of social science. …

This conception, sketched only programmatically in the manifesto, was given a fuller treatment in Carnap's influential *Aubau*. We saw in Chapter 3 that the lowest level in Carnap's *Aufbau* construction of the world was the "autopsychological," which pertains to content of our minds.

Reichenbach's *Experience and Prediction* also included discussion of individual sense perception. His §10 "Impressions and the problem of existence" allowed that there are "immediately given facts" of individual experience, which he designated as "impressions" or "sensations." There were accepted, only provisionally, within this section as "sentences capable of absolute verification." The problem of existence arises, Reichenbach continued, when our claims make the transition from our own subjective experience to the world beyond the confines our individual minds. Then we can doubt that there are objects that exist independently of our impressions. Reichenbach recognized that doubting their existence contradicts common sense. However, he went to some pains to argue that these doubts comprise a worthy problem in philosophy. He wrote (1938, p. 92):

> It is true that the question of existence, as it is usually expressed, needs a correction; and it is precisely the task of the philosopher to clarify the question first before an answer can be given. But it is not legitimate to cut short the question by sophistical remarks. It has been argued by certain philosophers that a man who doubts the existence of external things ought to have his forehead knocked against a wall to

> convince him of the reality of the wall. I do not think this is philosophical reasoning.

Reichenbach accepted that the existence of objects beyond our experience posed a worthy problem in philosophy. He did not allow that problem to have a controlling influence on his empiricism. He gave his solution to the problem of existence (p. 111): impressions provide *probabilistic* support for the independent existence of external things.

Carl Hempel, who worked under Reichenbach in Berlin, became one of the longest-lived members of group of philosophers associated with the Vienna Circle and Reichenbach's Berlin Circle. We can see how he sought to moderate the tension between the cognitive sense of experience and the propositional notion. His analyses privileged formal logical analysis, which is most congenial to a propositional notion of experience. Thus, he sought to admit cognitive experience into his system as intersubjectively affirmable statements. He wrote in his "The Theoretician's Dilemma" (1958, his emphasis, p.42):

> … empirical science aims for a system of publicly testable statements, and that, accordingly, the observational data whose correct prediction is the hallmark of a successful theory are at least thought of as couched in terms on whose applicability in a given situation different individuals can decide, with high agreement, by means of direct observation. Statements which purport to describe readings of measuring instruments, changes in color or odor accompanying a chemical reaction, utterances made, or other kinds of overt behavior shown by a given subject under specified observable conditions-these all illustrate the use of *intersubjectively applicable* observational terms.

4. Present Day Empiricism and Epistemology

Bas van Fraassen's constructive empiricism was the most influential formulation of scientific empiricism in the late twentieth century and perhaps even the decades following. In comparison to the moderating tendency in the writings of Reichenbach and Hempel, van Fraassen's constructive empiricism was a retrogression to an extreme skepticism. We are not to assign reality to anything beyond our human observations. His account is unapologetically anthropocentric. What is observable is determined by our human sensory capacities; and the division between the observable and unobservable would migrate if those human sensor capacities were to change. This curious skepticism proved most provocative and became the focus of an extended debate in philosophy of science in which one philosopher after another expressed dismay. Further comment is not needed here since the controversy is recounted in Chapter 4.

The conceptions of empiricism in recent epistemology and in recent philosophy of science have started to drift apart. Within recent epistemology, empiricism remains largely as the doctrine modern philosophers attribute to the British empiricists and retains the psychological notion of experience. This modern literature is too expansive to admit a cogent survey here. However, we can recover enough of it to see this last point. For example, the entry on "Empiricism" in the 2010, second edition of the *Blackwell Companion to Epistemology* begins (Dancy et al., 2010, p. 326):

> **empiricism** An epistemological movement according to which: (1) nothing around us can be known to be real unless its existence is revealed in or inferable from information we gain directly in sense experience, or in introspection, or later recall; …

"Sense experience," we soon learn is a mental state that is not assured to be a veridical representation of reality:

> We might describe an experience as one of "that rotten egg smell" or of "seeming to smell a rotten egg". However, it is typically thought, what is not revealed directly is whether the odour is actually that of a rotten egg or ever has been, or whether it even inheres in or is produced by anything that exists independently of our experience.

It is tempting to presume that everyone in this literature understands the notion of experience to be just as Hobbes sketched: it is the world impressing ideas upon our minds through our sense organs. However, the great variety of different conceptions of experience in this literature precludes this generalization; or so we are assured by the account of experience given in the *Routledge Shorter Encyclopedia of Philosophy* (Alston, 2005):

> It is difficult to give an illuminating analysis of 'experience'. Let us say that it includes any mode of consciousness in which something seems to be presented to the subject, as contrasted with the mental activity of thinking about things. Experience, so understood, has a variety of modes sensory, aesthetic, moral,

religious and so on – but empiricists usually concentrate on sense experience, the modes of consciousness that result from the stimulation of the five senses.

That the range of versions is great is shown by the existence of extreme recharacterizations of the notion of sense experience that deviate from Hobbes' model. We saw in Chapter 2 that Berkeley's idealism inverted the relationship of objects perceived and the perceiver's idea: the latter caused the existence of the former. Mach's sensationalism dispensed entirely with the conception of a world independent from our sensations: the sensations are the world.

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6. The Twentieth Century Ascendance of "Empirical Science"

Chapter 6

The Twentieth Century Ascendance of "Empirical Science"

1. Introduction

In Chapter 2, we saw how empiricism slowly shed its negative associations with medical quackery in the course of the nineteenth century, even if it still was accorded a lesser status in scientific methodology. This chapter traces a different strand in the history. Its concern is not empiric*ism*, the general doctrine as elaborated by philosophers. It is concerned with the emergence of the notion of "empirical science" among scientists. The two may seem so close that one might wonder if they can be separated. The difference is that the term empiricism, strictly speaking, designates a well-developed philosophy concerning how we learn of the world and its nature. "Empirical science," as it emerges in the course of the twentieth century in the writing of scientists, is a much looser view that arose as an application of the privileging of experience by empiricism.

The conception of science as empirical presents a general picture of science, done well, as deriving its content from experience. It generally lacks the skepticism of Big-E Empiricism. Empirical science, as understood in ordinary scientific discourse, is an optimistic view that allows experience to inform us of contingent propositions in science that extend well beyond experience. The term, or the idea behind it, has become common and even ubiquitous in the writings of practicing scientists. For simplicity and continuity with discussion in other parts of this text, I will call an allegiance to this conception of empirical science as the "scientists' empiricism."

Scientists and philosophers of science treat the notion of "empirical science" differently. For practicing scientists, the notion is almost universally accepted as a default fact about good science. It is beyond serious challenges. When proposals in science seem to threaten it, the empirical character of good science is to be defended energetically. That is so, even if most practicing scientists could not give a well-articulated account of precisely what the term means, once they have gone beyond the idea that it requires science to be grounded in observation and experiment.

For the philosophers of science, it is a term of art that requires careful dissection in accord with their specific version of empiricism. Just how might we bridge the gap between experience and results deeper in the science? How are we to think of these deeper results? Philosopher of science have given different answers. Reichenbach allowed probabilistic relations to bridge the gap and for results in science to enjoy probabilistic security. Popper insisted that no such inductive link is possible and that the propositions of a science enjoy only a provisional status as well tested, but not yet refuted. Van Fraassen argued that we should resist assigning truth to any claims beyond those of direct experience.

This chapter will review how the scientists' conception of empirical science evolved out of work by philosophers to identify and eventually elevate the place of the empirical in science. Section 2 will review how the first steps towards the notion of empirical science came in the nineteenth century with the idea of an "empirical law" in the philosophical literature. The notion was freed from a connection with medical quackery, but it was still a lesser form of science. For the scientists in the course of the nineteenth century, empirical laws became important and, even though they recognized that they were not entirely secure inductively, they grew to be a respected and important component of the sciences.

Section 3 identifies a clear indication that the fortunes of the idea of the empirical changed, starting in the 1930s and accelerating in the 1950s. An "ngram" plot of terms in which the adjective "empirical" appears shows its sudden rise in popularity. At about the same time, we shall see in Section 4, that philosophers of science now treated the notion of empirical science as an unproblematic default conception for science. Karl Popper's formulations of the empirical character of science proved to be especially influential among scientists and will be recounted in Section 5.

The remaining sections will review how the conception of empirical science migrated into common scientific thinking. That it was a migration and not an independent proposal is already clear from its close association with the term "empirical." It is a term of art of philosophers that originally denoted a disparaged medical tradition. Section 6 will give a characterization of the conception of empirical science as it appears to be used commonly in science. It is:

> *Scientists' conception of empirical science*. Science consists of observational and experimental reports and further results inferred from them. The observational and experimental reports are the empirical evidence indispensable for science.

It is uncommon for scientists to specify just what is intended by their frequent use of the term "empirical." That is a task left for philosophers of science. However, when scientists perceive a threat to empirical science, we can read their conception of empirical science from their responses, for these responses often include a specification of what counts as acceptable empirical science. Sections 7, 8 and 9 will review how such threats arose in forensic science, evolutionary biology and fundamental physics and cosmology; and how the scientists used their conception of empirical science to deflect them.

2. Empirical Laws in the Nineteenth Century

The first step towards the modern notion of "empirical science" came with the nineteenth century recognition of "empirical laws" in science. They have the character of simple generalizations in restricted domains and, it followed, could not be extended securely to universally applicable foundational laws.

What is easily overlooked but notable is the mere fact that the term "empirical" is used at all. These results could have been given many other labels: "observed regularities," "simple inductive laws," and many variants. That "empirical" was used reflects that the term had lost its negative connotations and retained only the idea of a strong connection to experience.

The idea of an empirical law appears throughout the writing of methodologists of the nineteenth century. William Whewell, for example, reproduced one of his essays from 1834 in an appendix to his *Philosophy of the Inductive Sciences*. In it, he juxtaposed merely empirical laws with laws of demonstrable necessity. It is a juxtaposition that is common in nineteenth century writing. He described them as follows: (1834, p. 573, his emphasis)

> "What is the kind and degree of cogency of the best proofs of the laws of motion, or of the fundamental principles of mechanics, exprest [*sic*] in any other way?" Are these laws, philosophically considered, *necessary*, and capable of demonstration by means of self-evident axioms, like the truths of geometry; or are they *empirical*, and only known to be true by trial and observation, like such general rules as we obtain in natural history?

John Stuart Mill, in his influential nineteenth century *System*, gave "empirical laws" a similar lesser place in science: (1882, p. 366, his emphasis)

> Scientific inquirers give the name of Empirical Laws to those uniformities which observation or experiment has shown to exist, but on which they hesitate to rely in

> cases varying much from those which have been actually observed, for want of seeing any reason *why* such a law should exist.

He soon summarized their limitations (pp. 368-69):

> Empirical laws, therefore, can only be received as true within the limits of time and place in which they have been found true by observation; and not merely the limits of time and place, but of time, place, and circumstance for, since it is the very meaning of an empirical law that we do not know the ultimate laws of causation on which it is dependent, we can not foresee, without actual trial, in what manner or to what extent the introduction of any new circumstance may affect it.

It hard not to see a begrudging tone in Mill's treatment of empirical laws. That they have a place in science is undeniable, so he has to fit them into his narrative.

This tension is all the more evident in W. Stanley Jevons' expansive review in his *Principles of Science* of the place of empirical laws in science. In his discussion of the vapor pressure of water (1874, p. 126), he noted that no formula contained in an empirical law would match exactly with the results of experiments. He lamented that this fact "illustrate[s] the feeble powers of empirical inquiry." This lament reflected his reservations elsewhere about empirically derived results. He noted: (1888, p.xxx)

> However useful may be empirical knowledge, it, is yet of slight importance compared with the well-connected and perfectly explained body of knowledge which constitutes an advanced and deductive science.

What is striking, however, in Jevons' narrative in *Principles of Science* is the vastness of the reach of empirical laws in science. Chemistry is a repeated example. He noted (p. 160) "The science of chemistry, however much its theory may have progressed, still presents us with a vast body of empirical knowledge." And then (p.161) "There are whole branches of chemical knowledge which are as yet mere aggregates of disconnected facts." Jevons further recounted (p. 166) the importance of laws first discovered empirically but which are then shown to have a sound basis in a law of nature. Kepler's empirical discovery of regularities in planetary motions (p. 222) is an instance of this discovery process. In reporting how novel experiences can overturn older science, Jevons recorded the still further reach of empirical laws (p. 335): "… the natural sciences of Botany, Zoology, Geology, &c., the laws of which are almost wholly empirical."

Jevons' inventory of empirical laws omitted treatment of empirical psychology. German interest in "*Empirische Psychologie*" had become so well developed that a French commentator, Théodule-Armand Ribot (1879) wrote a synoptic summary entitled *La Psychologie Allemande Contemporaine (École expérimentale)* that reported its empirical character. In a reflection of its importance, an English translation elevated "empirical" into the subtitle (Ribot, 1899), *German Psychology of Today: The Empirical School.*

A notable development in the strand of history traced here is that the term "empirical science" appears in an 1898 cataloguing of conceptions of science. It is not the first use of the term in the later nineteenth century, but it is illuminating for its care in identifying the scope of empirical science. In his *Sphere of Science*, Hoffmann (1898, p. 255) gave a three-part division of science:

> We thus see that there are three kinds of science and only three : the sciences that begin with single facts and arrange them into systems under more general facts, which we will call empirical sciences, because all their data are derived from direct observation and experiment; the sciences that begin with principles and arrange the facts under them, which we will call normative sciences, because they take a principle or norm of reason as their starting-point; and finally the science that seeks to explain all the other sciences and bring them into unity, which is philosophy properly so called.

Hoffmann then gave a tabular representation of the different sorts of sciences, rendered here as Table 1.

	Empirical sciences	*Normative Sciences*	
Physical	Astronomy, Geology, Physics, Chemistry, Biology	Logic, Mathematics, Aethetics, Ethics, Economics Politics	Philosophy
Psychical	Psychology, Sociology, Descriptive Theology		

Table 1. Hoffman's table of sciences

The list of empirical sciences coincides near enough with those we might now designate as empirical sciences. Hoffman's conception of the nature of these sciences did not match our later view. Empirical sciences, in his understanding, were epistemically weaker and apparently amounted to little more than a repository of empirical laws. He described their limitations as follows (pp. 256-57):

> No fact or collection of facts can ever become a principle, but a few facts even cannot be examined with any care without revealing principles. It is for this reason that no scientific mind can ever stop content with empirical science. It must ask, How are the facts related to the principles? What meaning do they have in the light of the principles and how are they to be scientifically arranged under them?

The juxtaposition of two ways of proceeding in science also entered the methodological discussion of major works of science. Walther Nernst's authoritative *Theoretische Chemie* includes with it in his introductory chapter (1898, p. 2, 1904, p. 2)

> The history of the exact sciences teaches us that we may discover new laws of nature in two essentially different ways, one of which may be designated as the empirical, the other as the theoretical. Thus in one way by suitable observations, one collects abundant material, capable wherever possible of mathematical expression, concerning the phenomena between which he suspects a connection; and then by a repeated and purely empirical grouping of the data so obtained, he seeks to approach the desired goal: in this way, for example, were found certain relations between the properties of the elements and their atomic weights. The

> second way, on the other hand, leads from suggested conceptions regarding the nature of certain phenomena, through pure speculation to new information, the correctness of which must be determined by subsequent research; thus by kinetic considerations of the combination and dissociation of substances reacting on each other, the law of chemical mass action was discovered.

The hesitation expressed by methodologists writing in generalities is no longer evident in Nernst's formulation. His example of empirical laws are the laws of thermodynamics, which were empirically derived and proved to have remarkably resilient, universal application. He noted: (1898, p. 2, 1904, p.2)

> … what is unquestionably the most brilliant example of a law of nature empirically proven, viz. the doctrines of thermodynamics, these are applicable to every occurrence which we see transpiring in nature, and therefore demand natural attention in the scientific investigation of every particular phenomenon.

It might perhaps overstate matters if we see a trend here. Those writing in the abstract continue to associate the term "empirical" with a weaker methodology. Yet, in the practice of science, the empirical is becoming essential to scientific inquiry and is eclipsing deductive methods in their fecundity.

3. The Twentieth Century Rise of "Empirical"

The fortunes of the adjective "empirical" changed for the better in the course of the early to mid-twentieth century. An oblique indication of the change is provided by Google's "ngram viewer." It plots the frequency of occurrence of terms in percentages in English language books published in the US for the time period indicated. Figure 1 shows the ngram plot for 1800 to 2000 for a selection of terms using the adjective "empirical."

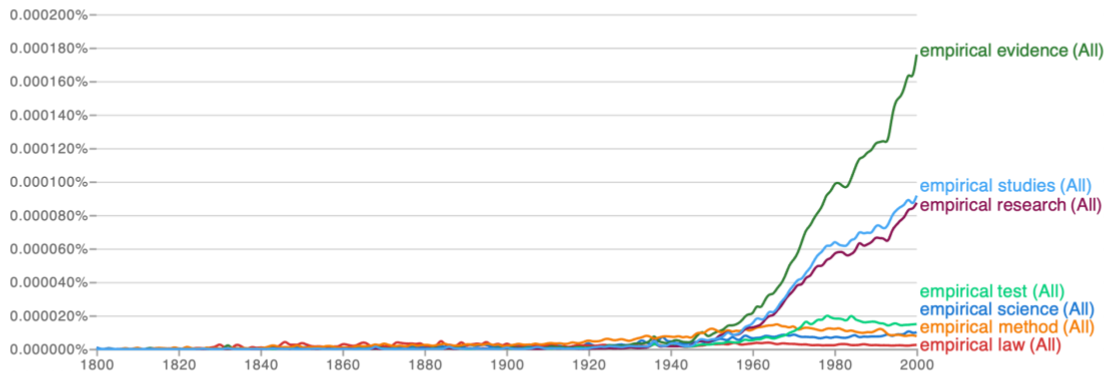

Figure 1. ngram plot of frequency of terms containing "empirical"

The plot shows a small increase in usage roughly around 1930 and then growing quickly starting around 1950. Interpreting the plot is not so straightforward. It might just indicate that this period enjoyed a rise in the publication of science books overall. Since other ngram plots (not shown) indicate no corresponding rise in terms like "science," "evidence" and "experiment," I read the plots of showing an increase in the usage of the adjective "empirical" specifically.

In any case, even if the overall plot is confounded by a rise in scientific publications, that confounding cannot be responsible for the difference between the rise in usage of the term "empirical evidence" compared to other terms containing the adjective "empirical." Its usage in "empirical evidence" increases the most.[64] This increase indicates that evidence derived from experience—empirically—was losing the lesser status afforded to it by nineteenth century writers and becoming more respectable as a foundation for science.

4. Philosophers of Science on Empirical Science

A part of the favorable rise of the adjective "empirical" is the description of science, properly done, as "empirical science." This description became routine among mainstreams of philosophy of science in the earlier twentieth century. An affirmation of the currency of this conception is given by Russell (1927) in the opening line of the chapter "What is an empirical Science?" He wrote: (p. 169)

> It would be generally agreed that physics is an empirical science, as contrasted with logic and pure mathematics.

[64] That it increases the most of terms with the adjective "empirical" is affirmed by an ngram plot (not shown) of "empirical *" that uses the wildcard symbol "*" to match any other word.

The multivolume *International Encyclopedia of Unified Science* codified the ideas of new movements in philosophy of science that were enlivened by the logical positivists of the Vienna Circle. With its first publication in 1938, the two parts of Volume 1 embodied the new empirical conception of science. It was "empirical science," founded on a thorough-going empiricism. The adjective "empirical" appears 201 times in Volume 1, Part 1 (Neurath et al., 1955a) with 94 of these occurrences in "empirical science"; and "empirical" appears 208 times in Volume 1, Part 2 (Neurath et al., 1955b), with 62 of them in "empirical science."

Rudolf Carnap's chapter, "Logical Foundations of the Unity of Science," repeats Russell's characterization. He wrote: (Neurath et al, 1955a, p. 45, Carnap's emphasis)

> The first distinction which we have to make is that between *formal science* and *empirical science*. Formal science consists of the analytic statements established by logic and mathematics; empirical science consists of the synthetic statements established in the different fields of factual knowledge.

This characterization, which might well appear quite benign to modern readers, includes a significant transition in the conception of science. In the nineteenth century, this division was roughly the one emphasized in Kant's work of rationalism versus empiricism. We saw above that nineteenth century writers inclined towards some version of the rationalist approach as the proper basis of secure science. Empirically based science was only admitted begrudgingly as an expedient with the hope that it would be replaced by a principled science more aligned with a rationalist approach.

In his chapter, "Scientific Empiricism," Charles Morris acknowledged the transition. He first recalled the older conception: (Neurath et al, 1955a, p. 64)

> Previously they had functioned as rival methods: mathematics as one way to get knowledge of nature, and experimental observation as another. Those scientists who advocated the former were one species of "rationalists," and the advocates of the latter were one species of "empiricists"— even the philosophical opposition of rationalism and empiricism was at bottom a reflection of what were to be taken as different scientific methods for knowing nature.

After reflecting briefly on the transition, Morris summarized its result: (p. 64)

> The important result was a double shift from a metaphysical to a methodological rationalism, and from a loose-jointed empiricism to an empiricism which utilized

> the techniques and the form of mathematics. Rationalism and empiricism in this way ceased to be rival methods for knowing nature and became complementary components of experimental science with its one observational-hypothetical-deductive-experimental method.

5. The Influence of Karl Popper

Karl Popper was a philosopher of science who worked in the same community as the logical positivists, while insisting that he was not one of them. He too, freely, wrote of "empirical science" in his writings. His most influential work was his 1935 *Logik der Forschung*, which came to prominence in the English language literature through its first English translation of 1959, *Logic of Scientific Discovery*. The first chapter begins with synoptic statements in which the term "empirical sciences" (German "*Die empirischen Wissenschaften*") appears four times: (Popper, 2005, p.3, my emphasis)

> A scientist, whether theorist or experimenter, puts forward statements, or systems of statements, and tests them step by step. In the field of the *empirical sciences*, more particularly, he constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment.
>
> I suggest that it is the task of the logic of scientific discovery, or the logic of knowledge, to give a logical analysis of this procedure; that is, to analyse the method of the *empirical sciences*.
>
> But what are these 'methods of the *empirical sciences*'? And what do we call '*empirical science*'?

Popper's answer to this last question included two notable themes. One was a complete repudiation of inductive inference. Sciences, he held, are not supported inductively by empirical evidence. They can only be tested by empirical evidence and refuted by it. The other was his demarcation criterion. It offered falsifiability as the criterion that demarcates empirical science. It served, Popper noted (p. 11): "… to distinguish between the empirical sciences on the one hand, and mathematics and logic as well as 'metaphysical' systems on the other.."

Popper's writings are pertinent here. Of all the philosophers of science writing in the mid twentieth century, his name is most recognizable by scientists in the twenty-first century. His authority is cited repeatedly for his demarcation criterion. It has proven to be a valuable rhetorical instrument for scientists seeking to deflect heterodox approaches. However, scientists who draw on Popper's authority are quite selective in drawing only on his demarcation criterion. The practice of modern science is ineliminably inductive. Norton (2026) elaborates the complete failure of Popper's anti-inductivism in the present practice of science and the weaknesses and dangers of his oversimplified demarcation criterion.

6. The Scientists' Empirical Science

The idea that science is empirical is well-established in the mainstream of science. The adjective "empirical," such as in "empirical evidence," or the adverb "empirically," such as in "tested empirically," is quite commonplace. There can be little doubt that the term has been drawn from the philosophical discussion. To see this, we need only reflect on what would otherwise be the complete oddity of this widespread use of the term "empirical." Its origin lies in the description of an ancient medical sect and its conception of medical practice was, for a long time, derided as quackery. Why would scientists embrace a term long associated with quackery so willingly? The answer is obvious. Later philosophical analysis exonerated the term and gave it the meaning the scientists have adopted.

The conception of empirical science that is widespread in science is a subset of the variety of commitments assigned to the notion in the philosophical literature. A rough approximation of this more popular conception is:

> *Scientists' conception of empirical science*. Science consists of observational and experimental reports and further results inferred from them. The observational and experimental reports are the empirical evidence indispensable for science.

Empirical evidence is the central idea. It is both sufficient and necessary for the support of these further results. What is notable is what is missing. Formulations of Big-E Empiricism go to pains to restrict what can be factually learned beyond the experience itself. Whether there is a such a limit to experience only is left open in the scientists' conception. It is a detail to be filled in by individual scientists who have embraced the idea of empirical science. My sense is that, tacitly, the overwhelming proportion of scientists are optimistic. For them, the inferential reach of evidence extends to facts far beyond those of experience itself.

Whereas I do believe this optimism is dominant, there are so many scientists and such a diversity of sciences that we should not expect a univocal agreement on this optimism. The pressing difficulties of individual sciences may incline a scientist to some form of pessimism. The notable example is quantum mechanics, whose enduring foundational difficulties try even the most patient. A retraction to instrumentalism over theoretical terms is a common reaction. Kemble's (1937) textbook of quantum mechanics talks of the "empirical" throughout, using such terms as "empirical basis," "empirical results" and "empirical facts." However, he adopted an instrumental interpretation of the central theoretical term in quantum mechanics. He wrote (p. 328): "… the wave function is merely a subjective computational tool and not in any sense a description of objective reality."

It is not so easy to find explicit formulations of this scientists' conception in general science writing. The conception is so pervasive that an insistence on the need for empirical evidence requires no further justification in general scientific writing. The idea is so fundamental to modern science that challenges to it are met with fierce responses. In them, at last, we find scientists articulating the centrality of empirical evidence in their conception of science. The sections that follow give several examples.

7. Empirical Science in Forensic Science

The admission of testimony by scientific experts in the courtroom has required the legal community to grapple with foundational problems of science. There is no shortage of consultants ready to inform a court on the authority of their scientific expertise. The difficulty for a court is to determine whether that expertise is based on a reliable science and whether that science provides the level of certainty claimed. These difficulties led to a synoptic response in September 2016 when "PCAST," the US President's Council of Advisors on Science and Technology, presented their report to the President, *Forensic Science in Criminal Courts: Ensuring Scientific Validity of Feature Comparison Methods* (PCAST, 2016).

The report examined the various forensic methods that, ostensibly, are scientifically well-founded. These methods seek to determine whether various physical properties of physical evidence at a crime scene could be associated with a particular suspect. To do so, different methods attempt to match the evidence with the suspect using DNA samples, bitemarks, latent fingerprints, firearm marks, footwear and hair. The Council's goal was to determine whether these methods addressed in the report were, as the report title indicates, "scientifically valid."

This determination required the Council to specify just what constitutes scientific validity. To this end, the first concept they introduced was "foundational validity." It was defined as: (PCAST, p. 4, their emphasis)

> *Foundational validity* for a forensic-science method requires that it be shown, based on empirical studies, to be *repeatable, reproducible, and accurate*, at levels that have been measured and are appropriate to the intended application.

This concept *requires* that evidence establishes the fitness for purpose of the method considered. The evidence is to come from "empirical studies." That is, empirical evidence is asserted as *necessary*. The sufficiency of empirical evidence is implicit in so far as no other evidential requirement is stated; and that assertions of effectiveness of a method not so supported are to be discounted. That is, the report goes on to state: (p. 6, their emphasis)

> Once a method has been established as foundationally valid based on appropriate empirical studies, claims about the method's accuracy and the probative value of proposed identifications, in order to be valid, must be based on such empirical studies. *Statements claiming or implying greater certainty than demonstrated by empirical evidence are scientifically invalid.*

A large number of experts and authorities are associated by name with the report. There are the Chairs (2), Vice-Chairs (2) and members of the Council (6); staff (3); Senior Advisors (14); and the Working group itself: members (6), staff (2) and a writer (1). This group of people was drawn broadly from universities, industry, commerce and the law. What is most striking is that, in all this large number, there are *no* philosophers or philosophers of science. Since the issue addressed is a core topic in philosophy of science, something has failed. Perhaps the Council has been parochial or negligent in its search for relevant experts; or perhaps the profession of philosophy of science has failed to display its expertise and show its utility.[65]

This omission is to our present advantage, since is assures us that the views expressed in the report are drawn from communities outside philosophy of science.

[65] In my pessimistic moments, I fear the latter. Too many philosophers of science, in an exercise of misplaced cleverness, have convinced themselves of the underdetermination thesis. In its strongest form, it asserts that no body evidence, no matter how extensive can ever determine a scientific theory, in stark contradiction with the routine successes of the sciences. Why would the Council want the opinion of such figures who hold that, as matter of principle, none of the methods under consideration or any other can work?

The PCAST report notes (p.1, 22) that it was in part responding to a report by the National Research Council (NRC, 2009) that found disturbing weaknesses in the scientific foundations of forensic methods. This earlier report also needed to specify what counts as good science, so that deviations from it could be identified. That specification was given in summary in an epigraph at the head of Chapter 4: *The Principles of Science and Interpreting Data.* A naïve reading is that the text is a quote from somewhere in Isaac Newton's named masterpiece. It reads: (p. 111)

> Scientific method refers to the body of techniques for investigating phenomena, acquiring new knowledge, or correcting and integrating previous knowledge. It is based on gathering observable, empirical and measurable evidence subject to specific principles of reasoning.
>
> Isaac Newton (1687, 1713, 1726)
> "Rules for the study of natural philosophy,"
> *Philosophiae Naturalis Principia Mathematica*

The epigraph conforms with the modern idea that good science is based on empirical evidence. As any competent historian and philosopher of science will no doubt recognize instantly, the formulation and even vocabulary are modern and not to be found in Newton's *Principia*. This epigraph, however, tells us that the authors of this 2009 report were eager to establish the historical pedigree of their characterization of science. It is, they are suggesting, not a momentary infatuation, but a long-established standard, already found in our greatest of scientific heroes, Isaac Newton.

There are so many experts and authorities involved in the NRC report that the full list requires four pages in the report. There is the Committee on Identifying the Needs of the Forensic Science Community (17) and their staff (15); and the Committee on Science, Technology and Law (26) and their staff (2). The list includes no philosophers, philosophers of science or historians of science who could have alerted the report writers to the spurious character of the epigraph.[66]

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[66] Here again, in my pessimistic moments, I fear that no historian of science was consulted because too many historians of science, in an excess of misplaced sophistication, have convinced

8. Empirical Science in Evolutionary Biology

So called “creation science” and “intelligent design” movements have presented enduring challenges to evolutionary biology. The diversity of biological species, this movement asserts, did not arise as evolutionary biology asserts through the slow accumulation of inherited adaptations. Rather they arose in a single, creative act. The US National Academy of Science has taken on the task of providing a unified response that defends evolutionary biology. A central theme of their response is that creation science fails to meet the standards of empirical science. To support this response, the Academy literature provided a characterization of what constitutes empirical science. One synoptic formulation was appealing enough to the Academy for it to be given twice in Academy publications: (National Academy of Sciences, 1998, p. 27; 1999, p.1)

> Science is a particular way of knowing about the world. In science, explanations are restricted to those that can be inferred from confirmable data—the results obtained through observations and experiments that can be substantiated by other scientists. Anything that can be observed or measured is amenable to scientific investigation. Explanations that cannot be based on empirical evidence are not a part of science.

The last sentence emphasizes that supporting empirical evidence is necessary for results to count as scientific. The import of this characterization is then applied to creation science: (National Academy of Sciences, 1999, pp.1-2)

> …the claims of creation science lack empirical support and cannot be meaningfully tested. These observations lead to two fundamental conclusions: the teaching of evolution should be an integral part of science instruction, and creation science is in fact not science and should not be presented as such in science classes.

A later Academy publication develops similar themes: (Institute of Medicine, 2008, p. 12, their emphasis)

> Science is not the only way of knowing and understanding. *But science is a way of knowing that differs from other ways in its dependence on empirical evidence and testable explanations.*

---

themselves of the untenable view that all science is “situated” to such an extent that it fails to make objective discoveries.

This insistence on empirical evidence is targeted at the religious motivations apparent in the proponents of creation science. Science, but not religion, is responsible to empirical evidence. For example, the report notes: (Institute of Medicine, 2008, p. 12, their emphasis)

> Science and religion are based on different aspects of human experience. In science, explanations *must* be based on evidence drawn from examining the natural world. Scientifically based observations or experiments that conflict with an explanation eventually *must* lead to modification or even abandonment of that explanation. Religious faith, in contrast, does not depend only on empirical evidence, is not necessarily modified in the face of conflicting evidence, and typically involves supernatural forces or entities. Because they are not a part of nature, supernatural entities cannot be investigated by science. In this sense, science and religion are separate and address aspects of human understanding in different ways. Attempts to pit science and religion against each other create controversy where none needs to exist.

Once again, empirical evidence is declared necessary.

The synoptic characterization of science given by the National Academy of Science in 1998 and 1999 proved popular in treatments of creation science and evolutionary biology. In 2005, parents of students in the Dover Area School District in York County, Pennsylvania, sued in US federal court over the school board's plans to introduce intelligent design, a form of creation science, into ninth-grade science classes. Among its findings adverse to the proponents of intelligent design, the court concluded that intelligent design is not science. This judgment required a characterization of science. In his ruling, Judge John E. Jones III reproduced the Academy's synoptic characterization of science (Jones, 2005, p. 66).

In the discussion prior the reproduction of this characterization, Jones (p. 65) rejected supernatural explanations as part of science and noted:

> This self-imposed convention of science, which limits inquiry to testable, natural explanations about the natural world, is referred to by philosophers as "methodological naturalism" and is sometimes known as the scientific method. … Methodological naturalism is a "ground rule" of science today which requires

scientists to seek explanations in the world around us based upon what we can observe, test, replicate, and verify.

The notion of explanation plays a distinctive role in these defenses of the scientific status of evolutionary biology for a reason that is specific to evolutionary biology. It is a historical science, whose subject consists almost entirely of past events. That means that the bulk of the evidential support for evolutionary biology does not come from novel prediction, but from its explanatory successes in accounting for these past events.

9. Non-Empirical Physics

Developments in physics and cosmology over the last few decades have led some scientists to affirm and delineate the empirical character of science as they conceive it. These affirmations arise from repeated concerns that two general lines of research have strayed too far from sound empirical physics into poorly grounded speculation. One line is string theory in particle physics. It has the lofty ambition to be the "theory of everything." The other is inflationary cosmology. It posits an early era of extremely rapid cosmic expansion just after the big bang and is closely association with the idea of a multiverse, which is the proposal for the existence, loosely speaking, many parallel universes.

In work leading up to and in his 2013, *String Theory and the Scientific Method*, Richard Dawid sought to support string theory by introducing the notion of "non-empirical theory assessment" and "non-empirical evidence." (Dawid, 2013, *passim*) The work triggered a coalescence of then growing concerns about the speculative character of string theory and notions of inflation and the multiverse. Dawid's notion of non-empirical theory assessment as a means to support string theory was too provocative to resist. Critics of string theory and multiverse theories understood it to endorse unreliable speculation without empirical foundations as serviceable science.[67]

These concerns were aired in a workshop, "Why Trust a Theory? Reconsidering Scientific Methodology in Light of Modern Physics," held in Munich in December, 2015. It was

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[67] On my reading, on the contrary, Dawid's proposal is likely an endorsement of the strength and reach of empirical evidence in science. It is that we *already* have enough empirical evidence in cases such as string theory so that *further* empirical evidence is not needed. See Chapter 12 for further discussion.

followed by a collection of papers in Dardashti et al. (2019). In the volume, in his "The Dangers of Non-Empirical Confirmation," Carlo Rovelli reacted to the expression "non-empirical confirmation" used by Dawid in his 2015 coauthored paper (Dawid, et al. 2015). In response, Rovelli stressed the unique status of empirical evidence in confirming science. He wrote: (2019, p. 22, his emphasis)

> The reason only *empirical* evidence can grant "confirmation", in the common sense of the word, is crucial and important: We all tend to be blinded by our beliefs. We pile up non-empirical arguments in support these. The historical success of science is grounded in the readiness to give up beloved beliefs when empirical evidence is against them. We create theories with our intelligence, use non-empirical arguments to grow confidence in them, but then *ask nature* if they are right or wrong. They are often wrong. Witness the recent surprise of many theorists in not finding the low-energy super-symmetric particles they expected.

Rovelli then recalled the successes of modern medicine and that: (p. 122, his emphasis) "…This feat has been achieved by simply *not* trusting non-empirical arguments." He then reaffirmed the unique status of empirical evidence: (p. 122, his emphasis) "…The problem with Dawid is that he fails to say that, for this process, only *empirical* evidence is convincing." The analysis then turned to string theory: (p. 123) "String theory is a living proof of the dangers of excessive reliance on nonempirical arguments." Here we should recall that Rovelli is one of the founders of loop quantum gravity, a prominent competitor of string theory.

In his contribution to the same volume, cosmologist and commentator Sean Carroll sought to protect multiverse theories from the objection that they are not falsifiable. Carroll's proposal was to dispense with the criterion of falsifiability and replace it with two requirements. He wrote (2019, p. 303, his emphasis in boldface)

> Falsifiability gets at two of the (potentially many) important aspects of science:
>
> • **Definiteness**. A good scientific theory says something specific and inflexible about how nature works. It shouldn't be possible to take any conceivable set of facts and claim that they are compatible with the theory.
>
> • **Empiricism**. The ultimate purpose of a theory is to account for what we observe. No theory should be judged to be correct solely on the basis of its beauty or

> reasonableness or other qualities that can be established without getting out of our armchairs and looking at the world.

The second aspect provides a characterization of empiricism in science. It affirms the necessity of empirical evidence, but not that it has an exclusive role in the support for scientific theories. These conditions serve to sustain the primary goal of Carroll's chapter, as he gave it: (p. 301)

> In this chapter, I stake out a judicious middle position. Multiverse models are scientific in an utterly conventional sense; they describe definite physical situations, and they are ultimately judged on their ability to provide an explanation for data collected in observations and experiments. But they are perfectly ordinary science, so the ways in which we evaluate the multiverse as a scientific hypothesis are precisely the ways in which hypotheses have always been judged …

Carroll was writing in support of multiverse theory. George Ellis and Joe Silk wrote as critics of the theory and their *Nature* commentary (Ellis and Silk, 2014) reacted to Carroll's analysis as given in an earlier formulation (Carroll, 2014). They wrote: (p. 322)

> Earlier this year, championing the multiverse and the many-worlds hypothesis, Carroll dismissed Popper's falsifiability criterion as a "blunt instrument" …. He offered two other requirements: a scientific theory should be "definite" and "empirical". By definite, Carroll means that the theory says "something clear and unambiguous about how reality functions". By empirical, he agrees with the customary definition that a theory should be judged a success or failure by its ability to explain the data.

They expand with their concerns by affirming Sabine Hossenfelder's (2014) reaction to Dawid's proposal of non-empirical theory assessment, which she derided as "post-empirical science" and "post-empirical physics."[68] Ellis and Silk (2014, pp. 322-23) wrote:

> We agree with theoretical physicist Sabine Hossenfelder: post-empirical science is an oxymoron … Theories such as quantum mechanics and relativity turned out well because they made predictions that survived testing. Yet numerous historical

[68] Hossenfelder (2014) concluded: "In summary, there's no such thing as post-empirical physics. If it doesn't describe nature, if it has nothing to say about any observation, if it doesn't even aspire to this, it's not physics."

> examples point to how, in the absence of adequate data, elegant and compelling ideas led researchers in the wrong direction, from Ptolemy's geocentric theories of the cosmos to Lord Kelvin's 'vortex theory' of the atom and Fred Hoyle's perpetual steady-state Universe.

What matters for present purposes is not which is correct: Carroll's positive assessment of multiverse theory or Ellis and Silk's negative assessment. It is where they agree. Ellis and Silk above affirm that Carroll's use of "empirical" "agrees with the customary definition" and with Hossenfelder that "post-empirical science" is an oxymoron, a contradiction in terms.

10. Conclusion

This chapter has tracked the emergence of the concept of empirical science in the nineteenth and twentieth century. Elsewhere in this text it is called the "scientists' empiricism." The concept is distinct from the philosophers' various doctrines of empiricism, even if closely connected with them. It is readily understood as an application of the general idea behind the philosophers' empiricisms. It answers the question: according to this general idea, what is the appropriate conception of science?

The difficulty with this question is that different versions of the philosophers' empiricisms can deliver answers that may differ considerably in their details. This chapter has tried to show, however, that, in their importing of the conception from philosophy to science, practicing scientists have suppressed these many possible differences. They have adopted a much reduced and simplified conception of empirical science. It amounts to a requirement that a science, properly developed, simply consists of reports of observation and experiment and what can be inferred from them. That is, empirical science depends exclusively on empirical evidence for support of its content.

Philosophers of science—*we* philosophers of science, since I am one—tend to complicated formulations of our ideas. Our empiricisms and our conceptions of the right sort of science have many interconnected parts. My exposition of small-e empiricism requires several chapters just to specify what the doctrine takes experience to be. The value for the present project of delineating the scientists' conception is that it identifies which part of the philosopher's conceptions matters to scientists, when they need a conception of practical utility in their work. That is a useful service. We should follow their lead and assign a central importance to the notion of empirical evidence in formulation an empiricism well adapted to science.

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7. Principles of Empiricism

Chapter 7

Principles of Empiricism

1. Introduction

One of the tasks of this volume is to provide a compact and serviceable statement of small-e empiricism. In anticipation of it, we should ask if there such a statement already in the existing literature on empiricism? Since empiricism has been a subject of discussion since antiquity, a thorough search of all statements is impractical.[69] We can, however, narrow down the time period in which a serviceable expression is most likely to be found. Prior to the early twentieth century, the psychological and logical senses of empiricism were not clearly distinguished. Since small-e empiricism concerns only the second logical sense, these earlier statements cannot serve without corrections. After 1980, the date of publication of van Fraassen's *Scientific Image*, the position he defended came to be identified as the standard empiricism. It was the position to which both advocates and critics of empiricism deferred. However, as I argued in an earlier chapter, this position is not so much an empiricism as a strict, inductive skepticism.

Synoptic statements of the foundations of empiricism in this intermediate period of the early to mid-twentieth century come in three versions that are summarized here. Although each has its place, we shall see that none matches the needs well enough to serve as a statement of small-e empiricism.

2. Source formulations

The versions that can be characterized as "*source*" formulations reflect most closely the traditional conception of empiricism. They are easy to find. Reichenbach, in his semi-popular *Rise of Scientific Philosophy* gave this formulation (1951, p. 75, Reichenbach's emphasis):

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[69] One of the most successful surveys of different senses of empiricism is Bogen (2015). Of the many senses catalogued, the closest to present needs is (2015, §7, Bogen's emphasis): "5. *Two-term confirmation.* A scientific claim is credible only if it is confirmed by perceptual evidence, where confirmation is a special two-term relation between claim and evidence—the same relation in every case."

> [Ancient empiricists] insist that sense observation is the primary source and ultimate judge of knowledge, and that it is self-deception to believe the human mind to have direct access to any kind of truth other than that of empty logical relations. This type of philosophy is called *empiricism*.

Hempel (1951, p. 41) expressed the view in this way:

> The fundamental tenet of modern empiricism is the view that all non-analytic knowledge is based on experience. Let us call this thesis the principle of empiricism … .

Benjamin (1943, p. 13) has this version:

> … empiricism is not at all easy to define. A first approximation to a definition is a simple matter. Empiricism is that theory of knowledge which maintains that experience is the sole source and the sole guarantee of knowledge.

David Hamlyn's entry, in the then authoritative 1967 Edwards' *Encyclopedia of Philosophy* (Hamlyn, 1967, p.499), starts the entry on empiricism with:

> Empiricism is the theory that experience rather than reason is the source of all knowledge, and in this sense it is opposed to rationalism.

This "source" version is compatible with small-e empiricism but falls short in asserting too little. It is also compatible with Big-E Empiricism in so far as it does not exclude the possibility that sense experience alone is all we can know. The "source" language is compatible with less skeptical versions of empiricism. It suggests that experience provides something that feeds into wider knowledge. However, it falls short in not specifying the channel through which that something is conveyed.

3. Decision Formulation

A second version can be characterized as a "*decision*" formulation. It is most clearly included in Karl Popper's work and appears throughout his writing. The version in his *Logic of Scientific Discovery* is (2005, p. 20):

> … what may be called 'the fundamental thesis of empiricism'—the thesis that experience alone can decide upon the truth or falsity of scientific statements…

A slightly different version in Popper's later *Conjectures and Refutations* (1962, Ch. 1, §IX) replaces "truth" and "falsity" with "acceptance" and "rejection":

> … *the principle of empiricism* which asserts that in science, only observation and experiment may decide upon the acceptance or rejection of scientific statements, including laws and theories.

It is superficially similar to the "*source*" version. The differences, however, are not accidental. Popper has adapted the conception of empiricism to his falsificationism (described earlier in Chapter 3). The "*source*" version allows for the possibility that deeper statements of theory in science are supported inductively by propositions expressing experience. It also leaves open the possibility that these deeper statements are found by some sort of stepwise inductive inference from experience. Both these possibilities are precluded by Popper's anti-inductivism. According to it, experience relates to theory in narrow, specific ways. A prediction of the right sort may match experience, which corroborates the predicting theory; or the prediction may be deductively incompatible with experience, in which case the predicting theory in refuted. This limited relation is summarized in the "*decision*" formulation as "experience alone deciding upon…" Thus, this "*decision*" version is too narrowly formulated to serve as an expression of small-e empiricism, since small-e empiricism endorses a rich use of inductive inference.

Similar verbiage to this "*decision*" version appears elsewhere in, for example, Wesley's Salmon's writing. He characterizes the "statement empiricism" of the logical positivists as (1985, p. 6):

> According to this doctrine, the sole basis upon which we are justified in affirming or denying factual statements is observational evidence. (This doctrine is tantamount, I believe, to the denial that there are synthetic *a priori* propositions.)

Subsequent discussion shows that Salmon did not share Popper's anti-inductive motivations. Salmon moderated the "*decision*" formulation with an admission of some form of inductive inference (p. 6):

> … assuming for the sake of argument (as I am doing and as van Fraassen does) that some rudimentary form of induction is available, we can support statements (including empirical generalizations) about observable properties of observable entities.

4. Meaning Formulations

The third synoptic statement of empiricism is a "*meaning*" formulation and is most closely associated with the Vienna Circle and logical positivism. In their manifesto, the Circle characterizes what “essentially distinguishes recent empiricism and positivism from the earlier version that was more biological-psychological in its orientation.” (Hahn et al. 1929, p. 306). It is a strict division of statements into the meaningful and the meaningless (pp. 306-307):

> … there is a sharp boundary between two kinds of statements. To one belong statements as they are made by empirical science; their meaning can be determined by logical analysis or, more precisely, through reduction to the simplest statements about the empirically given. The other statements, to which belong those cited above, reveal themselves as empty of meaning if one takes them in the way that metaphysicians intend.

In prefacing this formulation, the Circle offered examples (“cited above”) of the sort of statements that they judge as meaningless:

> “there is a God”,
> “the primary basis of the world is the unconscious”,
> “there is an entelechy which is the leading principle in the living organism”

Carnap’s (1936/37) “Testability and Meaning” provided a greatly elaborated version of this conception of empiricism through his definition of a “principle of empiricism” (1936/37, p. 33):

> It seems to me that it is preferable to formulate the principle of empiricism not in the form of an assertion—“all knowledge is empirical” or “all synthetic sentences that we can know are based on (or connected with) experiences” or the like—but rather in the form of a proposal or requirement. As empiricists, we require the language of science to be restricted in a certain way; we require that descriptive predicates and hence synthetic sentences are not to be admitted unless they have some connection with possible observations, a connection which has to be characterized in a suitable way.

This formulation requires specification of the ways that synthetic sentences are to be connected with experience. Carnap proceeded to elaborate four senses: “Requirement of Complete Testability,” “Requirement of Complete Confirmability,” “Requirement of Testability” and “Requirement of Confirmability.”

Hans Reichenbach introduced his own development of this concept of empiricism, which he called at one time “a probabilistic empiricism.” (1938, p. viii) to distinguish it from the earlier “logistic empiricism,” as he called it (p. v). Reichenbach’s version was distinguished by the prominence afforded to probabilities. Most distinctively, Reichenbach introduced a probabilistic conception of meaning through his two "principles of the probability theory of meaning" (p.54), which were discussed above in Chapter 3, Section 3.

This "*meaning*" principle of empiricism is least amenable to small-e empiricism. The focus on meaning has proven to be an adventurous aberration in the development of empiricism. It momentarily served the logical positivists well in so far as it enabled a trenchant response to any metaphysics that displeased them. The metaphysics could be dismissed abruptly as meaningless nonsense. Its severity proved self-defeating, however, since the verifiability principle itself could be subject to the same charge of meaninglessness.

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Part II
small-e empiricism

8. The Propositional Representation of Experience

Chapter 8

The Propositional Representation of Experience

0. Experience in small-e empiricism

Small-e empiricism is a scientific empiricism. That is, it is a version of empiricism that is adapted specifically to science. Formulating it requires an extensive revision of the notion of experience as it has commonly appeared historically in much of the writing in empiricism. This chapter and the three that follow seek to articulate this revised notion of experience. The revision is guided by a general principle:

We should choose a notion of experience in scientific empiricism that

• *only introduces philosophical problems peculiar to science; and*

• *conforms with how experience plays a role in scientific practice.*

To this end, three adaptations are developed in these chapters.

First, the present chapter addresses the representation of experience in small-e empiricism. It asserts that a scientific empiricism should not employ the cognitive notion of experience as a mental state that is aroused by excitations of our human sense organs. For their analyses now lie within the empirical sciences of physiology and psychology. The cognitive notion will be replaced by a propositional notion, such as the propositional representation of experience found in the published scientific literature. In them, individual cognitive idiosyncrasies are suppressed as much as possible. Inductive relations among these propositions lie within the scope of the philosophical analysis of small-e empiricism.

Second, the following Chapter 9 expands what counts as experience in accord with how it has expanded over the past century and more in many sciences. Experience is no longer understood exclusively as the excitation of human sense organs and the mental states induced by it. It has been replaced increasingly by purely instrumental sensings of far greater reach and sensitivity. Their interpretation has been carried out algorithmically in ways the transcend human powers, before any human engages with the results.

Third, Chapter 10 reconceives the nature of experience. It is not merely a localized event of the excitation of a sensing device. Rather experience is taken to be the entirety of a physical process that connects us continuously with the system of interest. The authority of experience lies in the continuity of this connection and not in anything special about human sense organs and mental processing.

This conception has no strict division between experience and the theoretical results beyond it. It is replaced by a relative notion. One stage of the experiential process is closer to the system of interest and the other farther from it, according to their places in the continuous physical process. Chapter 11 recounts how this relative notion escapes the "terminal obsession." It dispenses with the quest for an ever-troublesome and elusive goal: the purely experiential parts of science that form the observational termini of experiential sensings.

1. Introduction

At a broad level of description, two notions of experience appear in traditional accounts of empiricism. One is cognitive. Experience is associated with processes through which our sense organs induce mental states. The other notion is propositional. These are factual statements that are closely informed by our contact with the world, through observation and experimentation. As we saw in the historical survey including Chapter 5, through most of its history, different versions of empiricism have employed both cognitive and propositional notions of experience without too much concern over their fundamental differences. This is true of accounts of empiricism within traditional epistemology and also within philosophy of science.

This mingling of the two notions, I will argue in this chapter, has proven troublesome. Each requires different methods for their analysis and, as a result, each raises different questions and problems. Attempts to use both notions require a disentangling of these different problems and that greatly complicates the burden of analysis.

The version of empiricism developed here will escape these problems by restricting the notion of experience to the second propositional notion. It will be argued that this propositional notion is the one appropriate for understanding how empiricism is realized within science. To include the cognitive notion is to embroil empiricism in science in difficulties that are worthy of philosophical scrutiny but should be treated separately. Some have proven deeply intractable, while their resolution is of minor importance for a scientific empiricism. This restriction implements the clause in the principle above that the notion of experience should be one that "*only introduces philosophical problems peculiar to science.*"

Section 2 below will illustrate the propositional representation of experience in three works of importance in science: Newton's *Principia*, Mendel's experiments on plant hybrids and

Hubble's analysis of recession of the nebulae. In each case we can see that experience enters in propositional form at the initiation of the analyses. Section 3 recalls the need to limit the notion of experience to the propositional. Section 4 specifies how the cognitive and propositional notions of experience differ in their relata, relations and associated methods. Subsequent sections review the problems confronting analysts who adopt the cognitive notion of experience. Research in psychology raises serious doubts about the reliability of introspection as a means of accessing accurately the properties of this form of experience (Section 5). The existing analyses of justification associated with the cognitive notion are commonly underdeveloped, relying mostly on everyday intuitions. They may sometimes extend in speculative ways beyond the existing literature in deductive and inductive inference (Section 6). The problems naturally arising with the cognitive notion, such as Cartesian doubt, are remote from those specific science (Section 7). Through the example of Penzias and Wilson's observation of the cosmic microwave background, Section 8 illustrates again how the problems peculiar to science are associated with the propositional notion of experience and not with the cognitive notion.

2. Experiences as Reported in Science: Newton, Mendel and Hubble

The experiences pertinent to scientific empiricism are propositions that convey the results of observation and experimentation. In their most familiar form, they are reports of such results appearing in the published scientific literature. They seek to present this content in a form that is independent of the idiosyncrasies of the individual scientists involved in the observations and experiments. Great efforts are made to purge them of subjective elements peculiar to the individual scientists. The goal is that their import can be understood equally well and accepted by all scientifically literature readers.

A few examples will illustrate what is surely a familiar idea. The immense volume and variety of such reports makes selection of a representative sample impractical. The reports of Newton on planetary motions, Mendel on the hybridization of peas and Hubble on the recession of nebulae have been chosen because of their ready accessibility, but the choice was otherwise arbitrary. They are, at least, central to physics, biology and astronomy and are sufficiently unremarkable as to remind us of the general character of such reports.

2.1 Newton's Phenomena

In his monumental *Mathematical Principles of Natural Philosophy*, Isaac Newton (1687, Preface, no page) gave a memorable and succinct summary of the totality of his project:[70]

> … for the whole burden of philosophy seems to consist in this—from the phenomena of motions to investigate the forces of nature, and then from these forces to demonstrate the other phenomena…

Experience enters the project in Book III, his *System of the World*, through the reports of what were, in Latin, "*hypotheses*" in the first edition of *Principia* (1687, p. 402 ff) and became "*phaenomena*" in the final, third edition (1726, p. 390ff). Newton listed six phenomena that provided the observational basis of his recovery of the forces governing the planetary motions. For example, the first concerns the moons of Jupiter (p. 401):[71]

> PHENOMENON I
>
> The circumjovial planets, by radii drawn to Jupiter's center, describe areas proportional to the times of description; and that their periodic times, the fixed stars being at rest, are as the 3/2th power of their distances from the centre.

These relations are now commonly called "Kepler's second and third law." The third is that radius R of their orbits, assumed circular, vary with the period T according to $R^3 \propto T^2$.

The report was immediately followed by a brief recounting of the basis of this phenomenon in "[What] we know from observation." He then gave, in a list and tabular form, the observations of the periods and distances of the moons from Jupiter's center that were generalized in this phenomenon. These data, provided in the first edition of 1687, were updated in later editions, such as the third edition of 1726, as shown in Figure 1. They were prefaced with the remark that "… all astronomers agree…" on the summary given in Phenomenon I. It was, Newton was assuring us, a phenomenon purged of any idiosyncrasies of an individual astronomer.

[70] Translation: Newton (1934, Book I, p. xvii-xviii).

[71] Translation: Newton (1934, Book III, p. 401).

1d. 18h. $28'\frac{3}{5}$. 3d. 13h. $17'\frac{9}{10}$. 7d. 3h. $59'\frac{3}{5}$. 16d. 18h. $5'\frac{1}{5}$.

Diſtantiæ Satellitum à centro Jovis.

Ex Obſervationibus	1.	2	3	4	
Caſſini	5.	8.	13.	23.	
Borelli	$5\frac{2}{3}$.	$8\frac{2}{3}$.	14.	$24\frac{2}{3}$.	
Tounlei *per Micromet-*	5,51.	8,78.	13,47.	24,72.	Semidiam.
Flamſtedii *per Microm.*	5,31.	8,85.	13,98.	24,23.	Jovis.
Flamſt. *per Eclipſ. Satel.*	5,578.	8,876.	14,159.	24,903.	
Ex temporibus periodicis.	5,578.	8,878.	14,168.	24,968.	

Satellitum jovialium tempora periodica.

$1^d.\ 18^h.\ 27'.\ 34''.$ $3^d.\ 13^h.\ 13'.\ 42''.$ $7^d.\ 3^h.\ 42'.\ 36''.$ $16^d.\ 16^h.\ 32'.\ 9''.$

Diſtantiæ ſatellitum a centro jovis.

Ex obſervationibus	1	2	3	4	
Borelli	$5\frac{2}{3}$	$8\frac{2}{3}$	14	$24\frac{2}{3}$	
Townlei *per microm.*	5,52	8,78	13,47	24,72	Semidiam.
Caſſini *per teleſcop.*	5	8	13	23	Jovis
Caſſini *per eclipſ. ſatell.*	$5\frac{2}{3}$	9	$14\frac{23}{60}$	$25\frac{3}{10}$	
Ex temporibus periodicis.	5,667	9,017	14,384	25,299	

Figure 1. Newton's Reports of Observations of the Moons of Jupiter from the first (top) and third (bottom) Latin editions of *Principia*

2.2 Mendel's Peas

Gregor Mendel (1865) reported his famous experiment on the hybridization of pea plants in his "Experiments on Plant Hybrids." His goal was to determine the frequencies of dominant and recessive traits in new generations when pea plants are hybridized. The details of these experiments are too well known to need repetition here. Our concern is the level of detail in his reports of his experiences with the experimental outcomes.

The characteristics tracked were given verbal descriptions sufficient for others to identify them unambiguously. The ripe seeds are "either spherical or somewhat rounded, and the depressions, if any, occur on the surface, and are only shallow; or they are irregularly angular and deeply wrinkled" and "The albumen of the ripe seeds is pale yellow, bright yellow, or orange colored; or it possesses a more or less intensive green colour." (Mendel, p. 8/p. 408). Ambiguity is further controlled by reporting results in numerical form. The first two experiments on the first generation of hybrids involved over 500 peas plants. Mendel (1965, p. 12/p. 410) reported the overall results with the famous ratio of 3:1 of dominant to recessive characteristics:

> *First experiment:* Shape of the seeds. From 253 hybrids, 7324 seeds were obtained in the second experimental year. Of these seeds 5474 were round or somewhat rounded, and 1850 were angular wrinkled. The resulting ratio is 2.96:1.
>
> *Second experiment:* Colour of the albumen. A total of 258 plants produced 8023 seeds, 6022 yellow and 2001 green; the former relate to the latter in the ratio 3.01:1.

Since these two experiments involved over 500 plants, Mendel found it impractical to give a complete numerical accounting of his results. He could, however, give readers a flavor of the results by reporting the characteristics recovered from the first ten plants in each experiment through a tabulation of their results, shown in Figure 2.[72] It was followed by a brief recitation of the most extreme distributions of the seed characteristics in individual plants.

1. Versuch.			2. Versuch.	
Gestalt der Samen.			Färbung des Albumens.	
Pflanze	rund	kantig	gelb	grün
1	45	12	25	11
2	27	8	32	7
3	24	7	14	5
4	19	10	70	27
5	32	11	24	13
6	26	6	20	6
7	88	24	32	13
8	22	10	44	9
9	28	6	50	14
10	25	7	44	18

Figure 2. First ten plants of Mendel's first two experiments

The word "experience" (*Erfahrung*) appears only three times in the text. It has a liberal import. For example, Mendel (p. 41/p. 420) reports "…the universally confirmed experience that it is

[72] Mendel (1965, p. 12) Captions: "1. Experiment, shape of the seeds, plant, round, angular; 2. Experiment, color of the albumen, yellow, green"

unimportant for the form of the hybrid which of the original forms was the seed or the pollen plant."

2.3 Hubble's Nebulae

In his 1929 "A Relation between Distance and Radial Velocity among Extra-Galactic Nebulae," Edwin Hubble reported what become the foundational result of modern cosmology. The extra-galactic nebulae—or "galaxies" as we now call them—recede from us with a velocity whose mean is a linear function of the distance from us. Experience entered the report in the form of a tabulation of the velocities of and distances to the 46 nebulae of Hubble's analysis. Once again, subjectivity is suppressed by the reporting of numbers that, supposedly, could be recovered by any astronomer competent in the appropriate observational techniques. Figure 3 shows his tabulation for these data in the case of the 24 nebulae for which Hubble felt that the distances given were known more securely.

TABLE 1

Nebulae Whose Distances Have Been Estimated from Stars Involved or from Mean Luminosities in a Cluster

OBJECT	m_s	r	v	m_t	M_t
S. Mag.	..	0.032	+ 170	1.5	−16.0
L. Mag.	..	0.034	+ 290	0.5	17.2
N. G. C. 6822	..	0.214	− 130	9.0	12.7
598	..	0.263	− 70	7.0	15.1
221	..	0.275	− 185	8.8	13.4
224	..	0.275	− 220	5.0	17.2
5457	17.0	0.45	+ 200	9.9	13.3
4736	17.3	0.5	+ 290	8.4	15.1
5194	17.3	0.5	+ 270	7.4	16.1
4449	17.8	0.63	+ 200	9.5	14.5
4214	18.3	0.8	+ 300	11.3	13.2
3031	18.5	0.9	− 30	8.3	16.4
3627	18.5	0.9	+ 650	9.1	15.7
4826	18.5	0.9	+ 150	9.0	15.7
5236	18.5	0.9	+ 500	10.4	14.4
1068	18.7	1.0	+ 920	9.1	15.9
5055	19.0	1.1	+ 450	9.6	15.6
7331	19.0	1.1	+ 500	10.4	14.8
4258	19.5	1.4	+ 500	8.7	17.0
4151	20.0	1.7	+ 960	12.0	14.2
4382	..	2.0	+ 500	10.0	16.5
4472	..	2.0	+ 850	8.8	17.7
4486	..	2.0	+ 800	9.7	16.8
4649	..	2.0	+1090	9.5	17.0
Mean					−15.5

m_s = photographic magnitude of brightest stars involved.
r = distance in units of 10^6 parsecs. The first two are Shapley's values.
v = measured velocities in km./sec. N. G. C. 6822, 221, 224 and 5457 are recent determinations by Humason.
m_t = Holetschek's visual magnitude as corrected by Hopmann. The first three objects were not measured by Holetschek, and the values of m_t represent estimates by the author based upon such data as are available.
M_t = total visual absolute magnitude computed from m_t and r.

Figure 3. Hubble's report of velocities of and distances to 24 nebulae[73]

The bare recitation of numbers in this chart belies the immense complexity of the analyses of decades preceding that enabled the determination of these distances and velocities. A recitation of those analyses would be superfluous for the small community of expert astronomers

[73] In 2025, Hubble's 1929 paper entered the public domain in the US since it was published more that 95 years ago.

for whom Hubble wrote. Rather Hubble's exposition was more concerned to recount the labyrinthine maneuvers needed to secure distance measurements for the remaining 22 nebulae.[74]

Hubble's (1929) text does not contain the word "experience." However, it is comfortable talking of what is observed. The nebula N.G.C. 404 has an "observed velocity … so small…" (p. 172); and, more generally, the overall result is reported as obtaining between "distances and observed velocities." (p. 173) Hubble is prudent in not describing the distances to the nebulae in his data as "observed" since determining the distances proved to be especially troublesome for 22 of 46 nebulae. The velocities attributed to these nebulae were recovered from the measured red shift in their light. It was a procedure that Hubble found secure enough to elevate with the label "observed."[75]

3. Why Cognitive Experience is Excluded from a Scientific Empiricism

The propositional notion of experience just described differs from the cognitive notion of experience. We saw in the earlier historical survey of Chapter 5 that this cognitive notion has been the one used widely throughout the history of empiricism and epistemology, as early as the seventeenth century. Is such a stark reconception advisable? Might we do well to proceed with both notions? After all, experience is ultimately a cognitive event. In the simplest versions, our sense organs interact with the world and produce the mental states we identify as experiential. Traditionally,[76] the propositional notion of experience is ultimately dependent on this cognitive notion. Newton's phenomena were recovered from the optical observations of astronomers, such as those Newton listed in Figure 1: Borelli, Townlei and Cassini. Mendel's classification of peas

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[74] For a detailed examination of the resulting, complicated relations of inductive support in Hubble's paper, see Norton (2024, Ch. 7).

[75] Even this more secure case faced problems. The velocities "*v*" reported in the table do not correct for the motion of our planetary system in Milky Way. Hubble subtracted his best estimate of this velocity from that of the nebulae before constructing a graphical plot (p. 172) that displays on average a linear relationship between velocity and distance.

[76] This has been true for much of the history of science. The next chapter will argue that sophisticated scientific instrumentation has displace human sense organs and human cognitive states in processes that connect with objects of interest and, I will argue, now serve the role of experience.

as wrinkled or otherwise depended on Mendel actually looking at the peas and making the appropriate judgment.

My answer is that prudence dictates that a scientific empiricism—that is one that is well adapted to the problems peculiar to the sciences—should restrict itself to the propositional notion. The two notions differ. As a result, the analyses appropriate to each differ in the concepts and methods they use. These concepts and methods in turn give rise to the problems addressed in the literatures associated with each mode of analysis. We will see that, because of these differences in concepts and methods, the problems that arise in a scientific empiricism are distinct from those that arise in an empiricism based on the cognitive notion of experience. This is not to discount the cognitive notion. It raises deep and interesting problems in epistemology that deserve the most serious attention. These problems, however, are not peculiar to science. They are of a general type that extends well beyond the specific empirical concerns of a science. Accordingly, I will proceed with the principle stated in the introduction, that we should proceed with a notion of experience that "*only introduces philosophical problems peculiar to science.*"

The sections to follow will recount the many differences in the concepts, methods and problems associated with the two conceptions of experience; and they will indicate why a prudent scientific empiricism should start where the cognitive notion of experience transitions into the propositional notion.[77]

4. Cognitive and Propositional Experience

The differences between these two notions of experience can be summarized if we consider the relata and relations associated with the two forms of experience. To begin, if we restrict ourselves to propositional experience, then the subsequent analysis can continue with propositions:

Propositional analysis

Relata: Propositions drawn from experience and those propositions supported by them.

Relations: Inductive and deductive relations of support among propositions, as specified by the applicable logic.

Methods: Deductive and inductive logics.

[77] The further question of whether a serviceable scientific empiricism *can* be formulated in this manner in the subject of the following chapters.

An analysis of this type is familiar and relatively unproblematic. There is an extensive literature on deductive and inductive logic, including my own contributions in Norton (2021, 2024). Although the nature and foundation of inductive inference remains an open topic of discussion in philosophy of science, the difficulty is not a lack of well-developed accounts, but a surfeit of such competing accounts.

If our empiricism employs the cognitive notion of experience, then the methods appropriate to the analysis are less straightforward. A comparable delineation is:

Cognitive analysis

Relata: cognitive states, including those responding to stimuli of the senses and associated ideas and conceptions.

Relations: Mental processes, conscious and unconscious, connecting these cognitive states.

Methods: Introspection and experimental psychology.

This form of analysis is most common in modern writings in epistemology, whether they be empiricist or otherwise. In that literature, the concepts and methods of this analysis are less well developed. Although their substance concerns empirically testable claims about our mental lives, this literature largely proceeds with introspection and armchair theorizing. This general approach has been criticized sharply by new field of "experimental philosophy."[78]

5. Access to Cognitive States and Processes

In almost all traditional work in empiricism, it is supposed that we have direct access to cognitive states and mental processes simply by introspection and, perhaps, the reports of introspection by others. This access proves to be less reliable than these earlier workers supposed. Modern psychology offers a more systematic, empirically grounded alternative. When Hobbes, Locke and Hume wrote, there was no independent science of psychology. Their efforts were as much a contribution to philosophy and epistemology as they were early steps in the development of the science of psychology. That era is long past. We now have psychology as a specialty within science. It seeks to learn about mental processes by empirical investigations. In so far as cognitive analysis seeks factual claims about mental processes, the project has now been subsumed by psychology.

[78] See Knobe and Nichols (2017) for an introduction to experimental philosophy.

This fact is the basis of Quine's proposal for a "naturalized epistemology." He wrote (1969, pp. 82-83)

> Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science. It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input–certain patterns of irradiation in assorted frequencies, for instance– and in the fullness of time the subject delivers as output a description of the three-dimensional external world and its history.

The proposal may seem appealing initially to a scientific empiricism. Here is a proposal to use empirical science itself to analyze the cognitive states and mental processes associated with cognitive analysis.

Quine's proposal has proven controversial; and with good reason. A telling criticism, made by, for example Kim (1988), is that a purely descriptive psychology cannot supply the normativity so central to epistemology. This point is uncontestable. That our mental processes take us from one idea to another is insufficient to assure us that the first justifies the second without some external standard for justification. Such a standard is absent if the epistemology is completely naturalized, that is, fully given over to the empirically determined description of which mental processes occur and which do not.

There is a second, more serious difficulty. It is not so much a problem for a naturalized epistemology, as a problem revealed by naturalized epistemology. When the cognitive analysis is based on introspection, we presume that we automatically have a reasonable grasp on the character of our cognitive states and on the mental processes that connect them. The empirical research of psychologists routinely reports results contradicting this presumption of armchair psychologists. For example, the field of behavioral economics is based on the empirical finding that our behavior in making economic choices is less rational than we perceive it to be. An early contribution to this literature was Kahneman and Tversky's (1979) "prospect theory," which explored systematic deviations from default suppositions about what constitutes rational decision making. For example, they found that decisions differ according to whether the possibilities in the same choice situation are formulated in terms of losses or gains. The field of "experimental philosophy" works in the same tradition. It tests empirically the extent of acceptance of intuitions assumed by default in philosophy to be widely shared. Machery et al. (2004), for example, found that semantic intuitions guiding philosophical theories of reference vary with culture.

Results within this general literature impinge directly on the introspective methods commonly associated with the cognitive notion of experience. They challenge both our assumption of the veracity of our cognitive experiences and our perceptions of the rationality of our mental process.

A significant example of a challenge to the veracity of our experiences arises in empirical tests of the reliability of eyewitness testimony. Juries in trials are inclined to give great weight to eyewitness testimony. In doing so, they harbor the armchair view that testimony from eyewitnesses is akin to the replaying of a tape recording of the pertinent events. Research in psychology, however, shows that memory recall is less akin to the mechanical reproduction of experience and more akin to a reassembly of scattered fragments. This reassembly can be tainted by many circumstances and even include entirely spurious parts. The result, unfortunately, has been the false conviction of innocent subjects. This has been a persistent problem even in significant criminal cases, in which witnesses seek to report as accurately as they can.[79] These sorts of results are troublesome for foundationalists in epistemology who take the simplest cases of experience to be self-justifying.

The recognition of so-called "confirmation bias" challenges our perception of the extent to which our beliefs are justified by our default mental processes. The bias manifests in many forms. The commonality is that we tend to overestimate evidence that supports beliefs we favor and discount evidence that does not. In a review of the bias, Nickelson (1998, p.175, his emphasis) remarks:

> If one were to attempt to identify a single problematic aspect of human reasoning that deserves attention above all others, the *confirmation bias* would have to be among the candidates for consideration. Many have written about this bias, and it appears to be sufficiently strong and pervasive that one is led to wonder whether the bias, by itself, might account for a significant fraction of the disputes, altercations, and misunderstandings that occur among individuals, groups, and nations.

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[79] For a review, see Howe and Knott (2015).

The presence of this bias challenges the reliability of informal judgements by someone that their beliefs are well founded.

These sorts of empirical results present formidable and interesting problems for epistemology. How are we to arrive at a secure characterization of our cognitive states and processes? If a scientific empiricism begins with the propositional notion of experience, these problems do not arise. Propositional expressions of experience, such as illustrated in Section 2 above and reported in the literature, become the starting point of logical analysis. There is little ambiguity in their content; and pertinent processes that connect them are the logical relations sanctioned by deductive and inductive logics. A particular scientist may mistake the true import of evidence because of that scientist's confirmation bias. That failure is a part of the psychology of the scientist. What matters for a scientific empiricism is the strength of support provided by the applicable logic.

A complete isolation of scientific empiricism from the problems of cognitive analysis is not possible. There is always the possibility that some propositional expression is in turn founded upon some aberrant or anomalous cognitive processing. Such cases do arise and these examples will be discussed further in Chapter 11. An example of aberrations in a simple experience is the "personal equation" in astronomy. Systematic differences among observers in the timing of astronomical transits were corrected by attaching a correction factor, the personal equation, to each observer. The persistence of reports of flying saucers, alien spaceships and the like results from misinterpretations of ambiguous aerial phenomena. The misinterpretation arise through a form of confirmation bias in which observers are too ready to overinterpret these ambiguous phenomena in terms of an extraterrestrial hypothesis that happens to be readily at hand. These sorts of cases are not a locus of major problems in scientific empiricism. They are readily treated on a case by case basis by the psychological analysis.

6. Justification in Cognitive Analysis

A primary burden of an empiricism is to establish how it is possible for experience to justify our picture of the world. Thus, the notion of justification is central to any empiricism. This burden has proven to be troublesome for an analysis that employs a cognitive notion of experience. This analysis tracks how mental processes connect experience as a cognitive state to other mental states and then also the processes that connect these mental states to other mental states. The central question is how well these processes justify the content of the mental states in which they terminate. An answer requires a well-articulated account of justification. However, the accounts of justification in this literature are underdeveloped and scattered. The reason for this scatter lies, at least in part, in the fact that the mental processes serve in two roles in the analysis:

Descriptive: The description of mental processes in the analysis must conform with our actual mental processes, that is, how we actually think.

Normative: The mental processes described must also serve in a justificatory role. That is, we must be able to identify among these mental processes those that lead us or tend to lead us from veridical sense experience to veridical ideas.

We have already seen in the last section that results in empirical psychology show that the two cannot be easily combined. For we are often mistaken in accepting that some mental process has successfully justified a belief. It is essential that we distinguish cases of mental processes that are genuinely confirmatory from those that are not. That distinction requires an independent and well-developed account of justification.

The literature in epistemology is so large as to escape tractable surveys and summaries. The best I can offer is a fairly small sampling of the epistemology literature. It already indicates the underdeveloped character of accounts of justification. This section will address the extent to which a systematic account of justification has been developed.

At the most destructive extreme are accounts that simply deny that our natural mental processes can realize inductive relations of justification. We saw in Chapter 2 the most famous example. David Hume argued that, at best, our accounts can satisfy the first descriptive requirement, but not the second normative requirement. That is, he accepts that our "mind is carried by habit" from the idea of one event to that of a second. That provides no justification for the claim that the first event, as a general matter, brings about or causes the second. Van Fraassen's constructive empiricism is a recent version of inductive skepticism. We saw in Chapter 4 that van Fraassen's later writing made his skepticism clear. "Inductive logic is a make-believe theory…" he wrote.

The mainstream of present writing in epistemology is properly undeterred by such skepticism and routinely accepts that beliefs can justify others. Since this literature is so large, it is with some trepidation[80] that I report my impression that virtually all the analysis proceeds without a well-developed account of justification. *This* is said to justify *that* in the everyday sense of a philosophically untutored lay person. This is seen fairly easily in internalist epistemological writing, in which the basis of a claimed justification is required to be accessible to the consciousness of the cognizer. Edmund Gettier's influential article challenges the notion of knowledge as justified, true belief. His conception of justification in his examples is quite prosaic. One is this (1963, p. 122):

> Let us suppose that Smith has strong evidence for the following proposition:
>
> (f) Jones owns a Ford.
>
> Smith's evidence might be that Jones has at all times in the past within Smith's memory owned a car, and always a Ford, and that Jones has just offered Smith a ride while driving a Ford.

The basis of the justification is clear enough as far as ordinary, everyday thinking goes. The example falls well short of what a precise logical analysis would require. It leaves tacit background facts that would otherwise be essential to displaying the warrant for the inference. Whether some definite rule of inductive or deductive inference has been invoked is also left unsaid.

These sorts of prosaic examples are routine. Here is another, haphazardly chosen example. In his defense of internalist reliabilism, Steup(2004) gives an example that begins (p. 407):

> Consider my 1988 Honda Accord, still in my possession and put to daily use. It's been a remarkably reliable car. My evidence for attributing reliability to it is this: I remember that is has rarely let me down. Put differently, I remember that it has a good track record. Likewise, I remember that my perceptual faculties have rarely let me down. …

[80] Here I was consoled to find Fumerton (2002) in a synoptic article on theories of justification in epistemology writing (p. 204) "The task of explaining and evaluating theories of justification, however, is daunting."

An externalist epistemology denies that the basis of justification is accessible to consciousness. A reliabilist externalist epistemology locates the justification of a belief in the reliability of the process that gave rise to it. Here is Goldman's (1979, p.10) synopsis:

> My positive proposal, then, is this. The justificational status of a belief is a function of the reliability of the process or processes that cause it, where (as a first approximation) reliability consists in the tendency of a process to produce beliefs that are true rather than false.

The illustrations of the proposal once again depend on familiarities of everyday life, as opposed to a well-developed account of justification. For example (p. 10, his emphasis):

> Visual beliefs formed from brief and hasty scanning, or where the perceptual object is a long distance off, tend to be wrong more often than visual beliefs formed from detailed and leisurely scanning, or where the object is in reasonable proximity. In short, the visual processes in the former category are less reliable than those in the latter category. A similar point holds for memory beliefs. A belief that results from a hazy and indistinct memory impression is counted as less justified than a belief that arises from a distinct memory impression, and our inclination to classify those beliefs as "*knowledge*" varies in the same way.

A serious difficulty for this assessment is provided by the case discussed above of the unreliability of eyewitness testimony.

Sometimes, though not commonly, general accounts of inductive inference in the larger literature are drawn on. The core notion for coherentist epistemology is the mutual coherence of a body of beliefs. That coherence arises through the mutual support of the beliefs. Bonjour's (1985, pp. 94-101) conditions for a set of beliefs to be coherent includes that they are logically and probabilistically consistent and explanatory. These conditions reflect two of the major accounts of inductive inference at the time of Bonjour's writing: a Bayesian probabilistic account and an abductive, explanatory account.[81]

Epistemologists have also recognized that their notions of justification extend beyond that of the familiar logical notions of deduction and induction. Chisolm (1989) defends a notion of "internal justification" summarized as (p. 7, his emphasis):

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[81] For more discussion of coherentist notions of support, see Norton (2024, §4.2).

> If a person S is *internally justified* in believing a certain thing, then this may be something he can know just by reflecting upon his own state of mind.

Chapter 2 of Chisolm (1989) provides an elaboration and defense of the notion that includes listing many plausible properties for it. For example, the first axiom "A1" is (p. 12):

> If A is more justified than B for S, then B is not more justified than A for S.

While properties such as these mimic those recoverable in an inductive logic, Chisolm's notion differs from that of an inductive logic. That this is so follows from the fact that internal justification is a monadic property (for person S) of a proposition. In inductive logic, justification is minimally a dyadic relation between the evidence and the proposition supported. Chisolm makes quite clear that his notion of justification goes beyond that of inductive logic. He wrote (p. 10):

> We must take care to distinguish this fundamental epistemic sense of "probable" from the sense that that expression has in statistics and in inductive logic. In those disciplines "probable" is defined in terms of frequency of occurrence—sometimes in terms of "the limit of relative frequency in the long run." But the epistemic concept of probability, although it is closely connected with the concept of frequency, is a concept of a very different sort. (We consider this topic in more detail in Chapter 6).

The elaboration in Chapter 6 directly addresses a dyadic concept of inductive support. The relation considered is (p. 50)

> *e* tends to make *h* evident = Df *e* is necessarily such that it tends to provide *h* with strong inductive support.

The first of two difficulties Chisolm reports is, in effect, that this is a binary relation on propositions (p. 50, his emphasis):

> Our consideration of the evidence of the senses makes it clear that this type of definition does not include everything it should include. My present sense-experiences make it evident to me that I now see a sheep, but these experiences, as we saw in discussing the view of Sextus Empiricus, cannot be said to provide *inductive* support for the proposition that I see a sheep. So, too, for those present facts—if there are any—that tend to make it evident that it snowed here last night.

That justification can be a monadic property of a belief has a continuing presence in epistemology. Audi (2019) has defended the view that for some propositions, merely understanding them can be sufficient justification for belief in them.[82]

This notion of self-evident justification has a central role in foundationalism in halting what is otherwise portrayed as a fatal infinite regress. It is halted by foundational beliefs that require no further justification. Here is Fumerton's (2010) encapsulation of this argument:

> If the only way to justify a belief in some proposition is to justifiably infer it from some other proposition, and the principle of inferential justification is true, then to justifiably believe any proposition one would need to complete not one but infinitely many, infinitely long chains of reasoning. This we cannot do. The solution, the foundationalist argues, is to recognize what common sense itself dictates – we have justification for believing some truths where that justification does not consist in the having of other different justified beliefs.

Of course, the enduring problem for this monadic notion is the familiar problem of Cartesian doubt, to be discussed in the next section. We feel as justified in believing a banal proposition derived properly from sense experience as does a dreamer in the throes of a vivid dream and a madman in the grip of a fearsome hallucination. For further discussion, see Fumerton (2014).

The difficulties of reconciling the descriptive and normative requirements within cognitive analysis are formidable and worthy of serious attention. Their resolution would likely provide only marginal gains, if any, for the sorts of problems specific to a scientific empiricism. (This lack is illustrated in some cases in the next sections.) Thus, it is prudent for a scientific empiricism to avoid the complications of this cognitive analysis. Once a propositional notion of experience is adopted, relations of justification can be specified by the applicable inductive and deductive logics. All that matters to scientific empiricism is the normative aspect, that is, the strength of the justification supplied.

We could ask the descriptive question: are these relations identified in a scientific empiricism implemented by the reasoning of the scientists in each specific case? The answers are

[82] More precisely, the first sentence of Audi's abstract is "Self-evidence is plausibly taken to be a status that marks propositions as capable of being justifiedly believed (and known) on the basis of understanding them."

of great interest as a matter of history. However, unlike the case of the cognitive analysis, the scientists' actual reasoning is not the primary object of analysis. A scientific empiricism can give an account of scientific knowledge that omits all mention of the specific scientists whose reasoning did (or did not) realize the justificatory relations. Such an account is possible, but it would be tedious and lifeless for readers.

7. Problems of Cognitive Experience

The concepts and methods of cognitive analysis in turn have dictated the sorts of problems that occupy a prominent place in modern epistemology. These problems are, generally, unimportant to a scientific epistemology, as long as it is concerned with empirical problems peculiar to the science. This section reviews a few of these problems in epistemology.

In the last section, we saw the problem of selecting between an internalist and an externalist epistemology. Is the basis for the justification accessible to the cognizer (internalism)? Or it is inaccessible to the cognizer (externalism), but perhaps derived from the reliability of the mental processes connecting our mental states (reliabilist externalism)? This problem does not arise in a scientific empiricism as formulated in this text. Whether some proposition justifies some other proposition is a matter of the applicable logic. The fact of justification is independent of the mental states of scientists. Of course, we can hope that scientists are explicitly aware of the presence, absence and strength of justifications. That awareness has no definitional role in the logics.

A second problem of epistemology concerns foundationalism and was also sketched in the last section. Must we accept that, as we trace back chains of justification, that they terminate in experiences that are self-justifying? The question does not arise naturally in the scientific empiricism of this text. We shall see in a subsequent chapter that it employs a relational notion of experience: *this* proposition can be said to be farther in some process from the object of experience than *that*. There is no primitive conception of experience that we might ask to be self-justifying.

As a practical matter, since we cannot trace an experiential process indefinitely, there will be provisional termini that are farthest from the system of interest and closest to us. Their justification presents no special problems for small-e empiricism. If the termini are at a transition to the cognitive realm, their justification can be handed over to epistemologists who specialize in such problems. More generally, it is a mistake to hold that self-justification of such termini is necessary on pain of an infinite regress. The mistake derives from a tacit assumption of a hierarchical structure among relations of inductive support. Norton (2024) has argued at length for a non-hierarchical structure for these relations that escapes the infinite regress. Examples of non-hierarchical relations of support among provisional termini of experiential processes are provided in Chapter 11, Section 8. In them, multiple different termini are mutually supporting, so that none are unsupported.

The most prominent problem associated with cognitive experience is the venerable challenge of Cartesian skepticism. What assures us that our cognitive experiences are veridical? That is, are they informing us truly of an external world? Can we distinguish, René Descartes asked in his first meditation (1641), veridical cognitive experience from vivid dreams and delusions? How do we know, Descartes continued, that an all-powerful God is not deceiving us? Matters would then be so set up that there is no earth and no heavens, contrary to how things appear to us.

A familiar, modern version of this age-old problem is the brain-in-a-vat thought experiment. What if an evil scientist has taken our brains and kept them alive in vats of nutrients, while a supercomputer feeds our nerve endings electrical stimuli that perfectly simulate our familiar world? How could we know that the resulting cognitive experiences are all delusory? This is the most extreme version of many milder forms of the problem. If I sense green, can I be assured that I am seeing grass? Or if I sense red, can I be assured that I am seeing a red brick wall?

The deep intractability of the problem is shown by the fact that, since the time of Descartes, each generation has sought to solve the problem afresh. Putnam (1981), who gave a popular version of the brains in a vat thought experiment, argued that the scenario described is self-refuting. Gupta (2006) argued that we can, after all, make a connection from cognitive experience to know if we draw on a novel tool, the logic of interdependence.[83]

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[83] My response is that we can only work with the inductive import of our experiences; and they overwhelmingly support the banality that the ordinary world of everyday experience is pretty much the way we think it is. That we can concoct fanciful stories in which matters are otherwise is no reason to discount the import of the evidence of our senses. We have ample evidence that water slakes thirst, fire burns and bread nourishes. We have no evidence at all for the specifics of

8. Problems of Scientific Empiricism

An example illustrates how far these concerns are from the major issues in a scientific empiricism. In 1964 and 1965, Arno Penzias and Robert Wilson detected an unexpected excess of electromagnetic radiation of at a frequency of 4080 Mc/s in their horn antenna at Bell Laboratories in New Jersey. The proposition expressing the pertinent experience was reported in Penzias and Wilson (1965) as:

> Measurements of the effective zenith noise temperature of the 20-foot horn-reflector antenna … at the Crawford Hill Laboratory, Holmdel, New Jersey, at 4080Mc/s have yielded a value about 3.5°K higher than expected. This excess temperature is, within the limits of our observations, isotropic, unpolarized, and free from seasonal variations (July, 1964 - April, 1965).

This excess had no discernible terrestrial origin. It was posited to be cosmic in origin and eventually identified as just one component of the full spectrum of thermal cosmic background radiation of 2.7K. This background was in turn determined to be the residue of the cosmological big bang some ten billion years ago.

What is a pressing problem for scientific empiricism is to determine the accuracy of the measurement and, crucially, the extent to which it does provide support for a cosmic big bang some ten billion years ago.[84]

What is not a pressing problem for scientific empiricism is whether the signal detected was supplied to Penzias and Wilson by a deceiving demon; or whether they were unwittingly brains in vats being fed stimuli that simulate their arduous work with the antenna. More prosaically, a scientific empiricism should feel no compulsion to argue that Penzias and Wilson

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the fanciful scenarios. Who is the evil scientist? Where is the vat? Is the science of vat nutrition and perfect computer simulation of stimuli possible? The vat world scenario is merely a logical possibility without evidential support and can surely only trouble someone who demands absolute certainty in their judgments. If that is what supports Cartesian skepticism, it is another casualty of the demand for the infallibility that eludes inductive inference.

[84] Establishing that Penzias and Wilson had detected the afterglow of the big bang was not straightforward and was only settled after several decades. For a recounting of this episode, see Norton (2021, §9.8).

make a self-justifying attribution, when they attribute the grey patch they sense to the walls of their antenna.

What is a matter of central concern to scientific empiricism is just how well Penzias and Wilson can establish by the display of evidence and explicit argumentation in their published paper that they have detected radiation with an extraterrestrial source and that it is "isotropic, unpolarized, and free from seasonal variations."

What is not a matter of central concern is whether Penzias and Wilson's natural mental processes lead them to a belief that the radiation is isotropic, unpolarized, and free from seasonal variations; or whether they are aware of the justificatory force of such processes; or that that justificatory force within their mental processes is inaccessible to them.

What is an important part of the problem for scientific empiricism is to determine whether they were mistaken in some element of their observations or the inferences made from them. Perhaps the signal they detected had a terrestrial source after all, but one they had overlooked. Perhaps, if they had detected a cosmic background radiation, it might not be support for an ancient, cosmological, big bang but merely for an eternally enduring, cold thermal cosmic environment. These are delicate matters of inductive inference that took decades to resolve.

What is not a pressing concern for scientific empiricism is the skepticism of van Fraassen's constructive empiricism. According to it, all their efforts at inductive reasoning are fictional. There is *nothing* that Penzias and Wilson can do to establish the real existence of this cosmic radiation. It is not directly observed by them. Thus, no matter what they do, they must suspend belief in the existence of that radiation. It is a limit to belief that no science can breach and not for a reason peculiar to the science. It is simply that, according to constructive empiricism, we have no warrant, in science or outside science, to believe in the reality of entities beyond observation.

This example illustrates how the propositional form of experience is the appropriate choice. This is not to diminish the importance of the traditional problems associated with cognitive experience. Here Reichenbach was right to insist on their worthiness. Rather it is to say that these problems are not specifically problems in philosophy of science. We can hope for a solution from epistemologists who specialize in these problems of cognitive experience. If they cannot find a solution, we do scientific empiricism no favors in visiting upon it an insoluble problem that is not specific to science.

9. Conclusion

This chapter has argued for a division of labor. The enterprise of empiricism overall extends from the most rudimentary of our sense experiences through to the most abstruse results of science. The proposal is that an empiricism suitable for science should not be responsible for the entirety of this enterprise. Rather, its goal should be to investigate the role of experience in problems peculiar to science. That goal directs us to consider experience once it has been rendered in propositional form, such as would appear in formal, scientific writing. This divides empiricism into a cognitive analysis closer to the sense experience of individuals and a propositional analysis that seeks the import of experience in science, once it has been presented in a communally accessible form.

The division is prudent, I have argued, since the cognitive and propositional analysis employ different concepts and methods and, as a result of these differences, address different problems. They are each best treated separately.

That such a division of labor is advisable is a relatively recent development in history of empirical thinking. In the 1920s, when the modern tradition in empiricism was given new energy by such movements as the Vienna and Berlin Circles, it was still conceivable to their proponents that their analyses could embrace the totality of the empirical enterprise. Since that time, research in empirical psychology has shown that simple introspection gives a poor understanding of our sense experiences and mental processes. Properly accommodating these new results greatly complicates the burden of empiricist analyses that employ the cognitive notion of experience. A similar problem arises for the propositional notion of experience that, I have argued, is appropriate for empiricist analysis within science. Such analysis must now accommodate a philosophy of science that has a greatly enriched repertoire of results. Of special relevance is the growth in our understanding of inductive inference as it is implemented in the sciences.

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9. Experience Generalized

Chapter 9
Experience Generalized

1. Introduction

The goal of small-e empiricism is to provide an updated version of empiricism that is well-adapted to the present character of science. Empiricism privileges experience as a way of being informed about the world because experience gives us a physically continuous connection with those aspects of the world of interest to us. In empiricist writing since the seventeenth century, human sensory experience has been taken to be *the* way that this continuous connection is realized. It has become apparent through the practices of modern science that this privileging of human sensory experience must be rethought.

What matters is the continuity of the connection. The authority of experience derives from it and not from the participation of human sensory organs in the connection. Historically, human sensory organs played an essential role in establishing that connection. They were the best instruments fit for the purpose. We now find that sophisticated scientific instrumentation is quite capable of establishing this connection without human sensory organs playing any essential role. In many cases, these instruments perform far better than does the human sensory system; and in many cases they perform where human sensory experience could not.

Empiricist writing in philosophy of science has been slow to acknowledge this transformation. This may result in part from van Fraassen's controlling influence on the conception of empiricism in recent philosophy of science. His empiricism insisted upon a conception of experience that employed the narrowest form of human sensation. In rejecting this narrowing, I follow Nora Boyd (2018) whose empiricism is based on a recognition that experience as human sensation no longer suffices for an empiricism adequate to modern science.[85] She wrote (p. 404):

[85] After the first draft of these chapters were completed, I found Ladyman's (2025) new paper that also argues for an empiricism in which the concept of experience is generalized beyond

> In the hope of replacing observations with something more suitable to science in practice, we might consider the more generic 'empirical results', where 'results' may be understood to include observations and other sensings but also the results of technology-aided detections and measurements, and 'empirical' may be understood in contrast with 'virtual' and 'imagined' and could be cashed out by appeal to a causal story connecting the target of interest to the generation of that result.

Small-e empiricism seeks to maintain the core empiricist doctrine of the authority of experience in how we are informed about the world. Thus, the adaptation to modern science requires a generalization of the notion of experience. Since its authority derives from the continuity of the connection to the world, not from any special role of human sense organs, the generalized notion of experience characterizes as experience any process that connects us continuously to the aspect of the world of interest to us. The generalized notion includes familiar processes essentially dependent on human sensory organs. It adds many more that are now routinely used in the sciences.

The term "experience" will continue to be used, even though some may find the term incongruous. This usage maintains continuity with the empiricist tradition and its privileging of experience. It is justified since, in my view, the authority of experience never could be justified simply by the participation of human sensory organs. That authority has always derived from the physical continuity of the connection, even if this was not apparent in empiricist writings. That continuity can now be secured without human sensory organs.

This chapter articulates and defends this generalized conception. Section 2 below will recall the imperfections of human sensory experience. Hacking, it will say, used the wrong standard when he asked provocatively "Do we see through a microscope?" Human sight is a poor standard since it is prone to illusions. Microscopes serve empiricism well in so far as they realize processes that connect us with the systems of interest. The examples of ptychography and radar show that such processes serve us well, even if they do not deliver familiar pictorial representations.

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human sensory experience. Traditional human sense-data is replaced by a more general notion of data.

Subsequent sections will track how modern scientific instrumentation has evolved to enhance and replace processes dependent on human sense experience. This evolution is pervasive in science. It can be illustrated only with a few examples. Section 3 will recount how nineteenth century analytic chemistry developed chemicals tests for the analysis of samples in which human sensory experience played a major role. It was essential that the chemist see some specific chemical change or smell some characteristic odor. In the course of the twentieth century, these tests were replaced almost entirely by automated analyzers in which human sensory experience played no essential role. They served the same purpose of determining the chemical nature of a sample, but now do it without the chemists' eyes and noses.

Instrumentation has also replaced an integral part of human sensory perception. We humans do not just perceive an excitation of sensory neurons. Our nervous systems translate the excitations of our sensory organs into mental states that reflect the character of the source of excitation. These are "impressions" to use Hume's expression. Some of the processing is so automatic that it is seamless. We then deepen the interpretation with some cognitive processing. Galileo, for example, saw three or four stars through his telescope in the vicinity of Jupiter that he instantly recognized as lying in straight line. After a few moments' reflection, he realized that the line coincided with the plane of the ecliptic. Subsequent observation led him to affirm that the stars were orbiting Jupiter as its moons.

Section 4 traces this development in astronomy in the discovery of new celestial objects. It recalls discoveries by Brahe and Kepler of novas; of the moons of Jupiter by Galileo; and the dwarf planet Pluto by Tombaugh. All these processes have been replaced by observational data of luminous celestial objects collected and processed automatically. Where we might once have thought that human scrutiny was needed, a computer algorithm now sifts through many instances of the brief dimming of the brightness of star to identify those cases in which the dimming results from the transit of an exoplanet.

Section 5 traces a similar development in the replacing of human sensory processing. Genetic material in living cells were first identified by human visual sensing through optical microscopes. Subsequently X-ray diffraction images were recognized as arising from a helical structure. Human genome sequencing, however, was successfully achieved entirely by computer algorithms piecing together the fragments of human DNA into the long strands of human's 23 chromosomes.

Section 6 recounts what is likely the most esoteric replacement for human sensory experience. Where Tycho Brahe and Johannes Kepler saw the blazing light of new stars, the massive interferometers of the LIGO project observe black hole coalescences through a medium quite inaccessible to human sensory organs, pulses of gravitational waves produced by these coalescences.

The discussion in these sections reports only a few examples of how scientific instrumentation has replaced human sensory organs. This replacement has become so pervasive in modern scientific practice that no synoptic survey can do it justice in a single chapter. With apologies to those whose favorite examples of automated scientific instrumentation have been neglected, my selection of examples is haphazard. They are instances that I happened upon that turn out to illustrate the instrumental supplanting of the various phases of human sensory processes sketched above.

2. We Do Not Need to *See* Through a Microscope

2.1 Hacking and the Microscope

Ian Hacking's celebrated (1981) "Do We See Through a Microscope" was one of many expressions of discomfort with van Fraassen's extremely limited view of the reach of experience. Van Fraassen (1981, p. 214) had sought to answer negatively the questions (among several) "Can we observe through an electron microscope? Through an optical microscope?" Hacking's response was both successful and unsuccessful. It was successful in his dissatisfaction with the paucity of understanding of the actual science of microscopy in the philosophy literature. He lamented quite correctly (p. 305):

> … the modern microscopist has far more amazing tricks than the most imaginative of armchair students of perception. What we require in philosophy is better awareness of the truths that are stranger than fictions.

His paper proceeded with a rather unfocussed account (on my reading) of the historical development of modern microscopy from its seventeenth century origins in van Leeuwenhoek's work. Throughout the narrative, Hacking examined how well the evolving instruments could produce images comparable to those familiar to naked eye vision. To deliver such images, microscopists had to compensate for many visual infidelities. They began with aberrations in lenses, such as the spherical and the chromatic; and they become increasingly complicated as the design of microscopes became increasingly sophisticated. After many complications, Hacking did decide (p. 321) that "we *are* convinced" of the reality of structures seen through microscopes; and I think he intends that we *should* be convinced, even though he italicized the "are."

Where was Hacking's paper unsuccessful? It was in his answer to the question of whether "see" through a microscope. He sought the extent to which the instrument could yield an image comparable those of ordinary visual experience. This betrays a needless reverence for ordinary visual experience. In my view, in an empiricism well-adapted to science, all that matters is whether the physical processes in the microscope connect with the system of interest in such a way that we can infer the system's properties. In his analysis of polarizing microscopes, Hacking does momentarily admit something in the direction of abandoning the standard of ordinary vision. Since we cannot perceive polarization directly, he avers in italics (p. 313):

> *We could use any property of light that interacts with a specimen in order to study the structure of the specimen.* Indeed we could use any property of any kind of wave at all.

This promising remark, however, proved not to be a first step to disavowing the necessity of images akin to ordinary visual perception.

Why disavow the need for images akin to those of ordinary visual perception? There are two reasons. Briefly stated, first, ordinary visual perception is just not good enough to serve as an ultimate standard for the faithful representation of systems of interest. Second, what matters is being informed about the system of interest; and we can do that with any physical process that accesses it, as long as the process delivers something that can be usefully interpreted.

2.2 Visual Illusions

First, we learn from ordinary visual illusions that human vision is imperfect. It is a mistake is to think that our visual perception is like a static photograph, each of whose elements correspond faithfully to some element of reality. Time and again what we perceive visually is a combination of the reality and our brain's best, faltering attempt to interpret it. For example, Figure 1 below is a completely static image that seems to many viewers as beset with rolling wavelike motions.

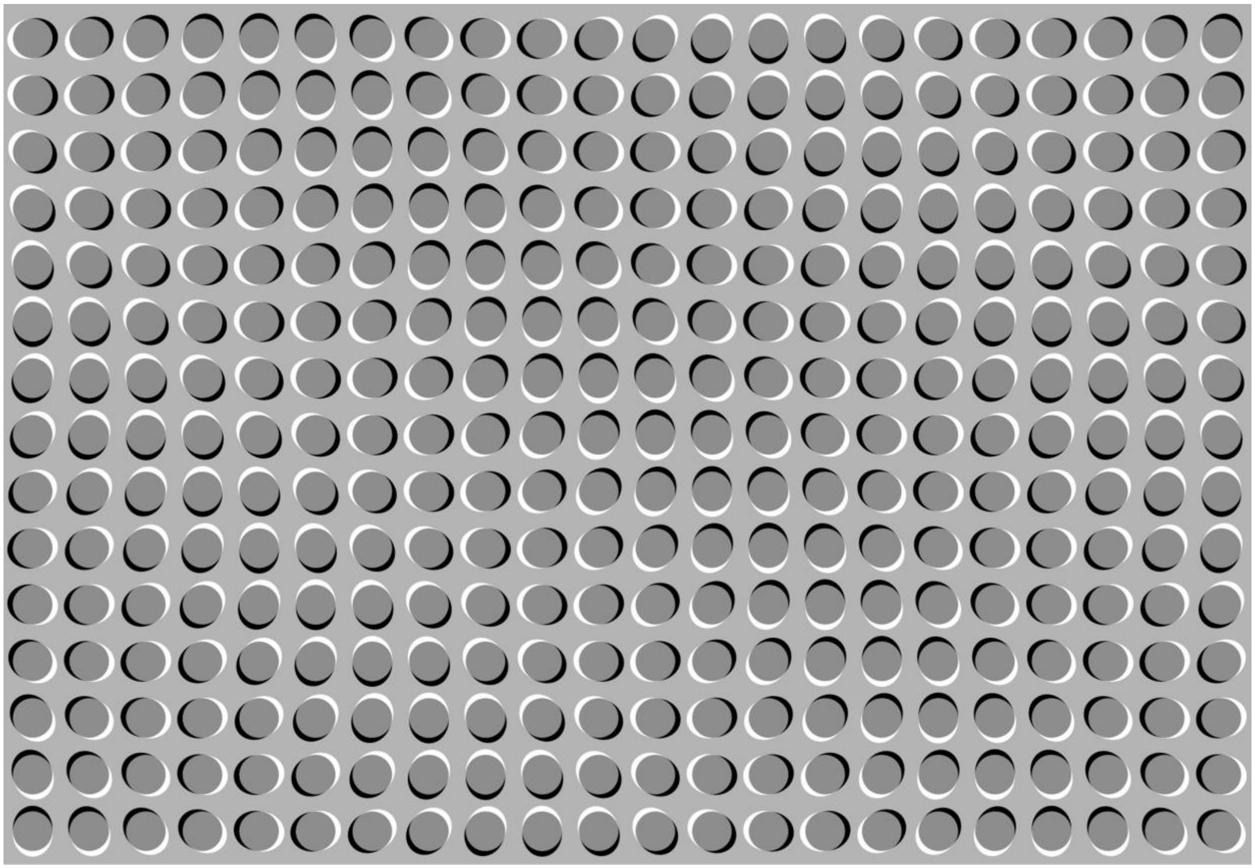

Figure 1. Motion optical illusion[86]

Our vision can also give us the illusion of reality with vivid images of objects that cannot possibly exist, such as in Figure 2.

[86] The illusion arises if we move our gaze over the surface of the figure. Based on https://en.wikipedia.org/wiki/File:Anomalous_motion_illusion1.svg "This file is licensed under the Creative Commons Attribution-Share Alike 3.0 Unported license."

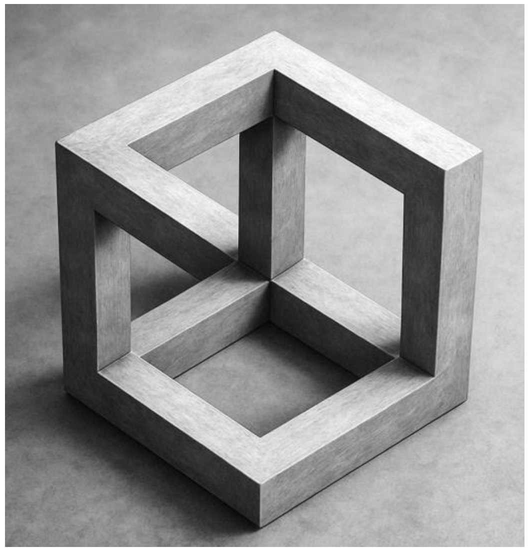

Figure 2. An impossible object[87]

Given the fallibility of human vision, when are we justified in accepting the images in our vision as veridical? It is when we can affirm that the images arise through a process that connects with a system of interest in the world; it does so by a process that faithfully reflects its properties; and we are able to distinguish those real properties from artifacts of our perception. That condition, of course, can be realized by processes that only incidentally involve human sensory organs and need not produce ordinary visual images.

2.3 We Can Do Better Instrumentally

This last point is the second reason for disavowing the images of human vision as the standard. We can often do better without them. An example arises in microscopy. In "ptychography," many microscopic images of some object are taken, each slightly displaced. The resulting set of images is somewhat uninformative to ordinary visual examination. However, the optics of the imaging has been so set up that the images contain phase information, not directly interpretable on visual inspection, but that can be synthesized by a computerized algorithm to deliver a visually informative image. Figure 3 is an example.

[87] Created by chatgpt from prompt "create a hyperrealistic image of an impossible object greyscale" https://chatgpt.com/c/6806fe74-689c-8000-b07a-b2d6c90ac734

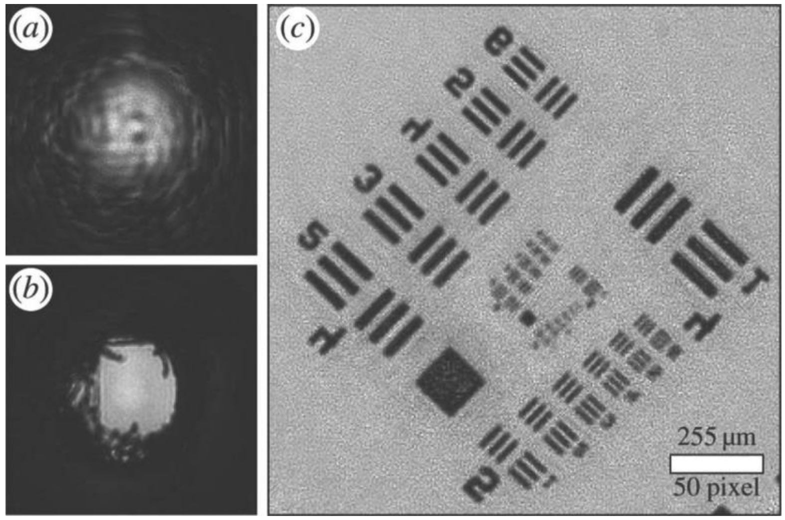

Figure 3. Real and constructed images in pytchography[88]

The actual images taken look like those on the left at (a). They are an unhelpful blur.[89] After a computer has synthesized them, it generates an informative image on the right at (c). There is no sense in which we "see" this last image through the microscope. It is a synthetic creation of the computer algorithm, but one that informs us well of the object examined.

A handbook article on ptychography notes the deviation from familiar practices in microscopy (Rodenburg and Maiden, 2019, p. 821):

> Unlike the immediacy of a conventional microscope, ptychography puts a huge obstruction between the microscopist and the image. First, we must wait while at least the two interference patterns are recorded; the experiment takes time. Second, we have to rely on the computer to reconstruct the image from the data. The data usually look nothing at all like the object of interest; we must wholly trust a computer algorithm to deliver our results, something that unnerves quite a lot of scientists.

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[88] Image from https://commons.wikimedia.org/wiki/File:Ptychography_experiment_with_visible_light_in_a_laboratory.jpg "This file is licensed under the Creative Commons Attribution-Share Alike 4.0 International license."

[89] Figure 3 is a greyscale rendering of the original color image, where color differences encode phase differences, but are not much more informative to visual inspection.

It then notes how this method improves on conventional microscopy. For example, it can work with images produced by poor lenses or even no lenses at all.

2.4 Non-Pictorial Imagery

In ptychography, the results are eventually delivered in the form of an image such as we imagine we would see if were shrunk down to the microscopic size of the object. It is convenient for we humans to have results delivered to us in such a familiar form. Such presentations are likely to be comprehended more easily and more quickly than others. That mode adapts to the results to our human idiosyncrasies. There may be nothing intrinsic to the results that requires this adaptation to the pictorial form familiar to us. What matters is whether the instrument gives us some representation that enables us to infer properties of the system of interest. Such a representation may even be better than a pictorial representation.

Hacking briefly discussed radar. The early history of radar, not mentioned by Hacking, is an example of how imaging can usefully tell us about a system of interest without producing the sorts of pictorial images of ordinary vision. Figure 4 shows radar displays from early, mid 1940s radar sets, as described in Navy Department (1946, pp. 13-14). The screen on the right is the display now familiar to us. It gives something that looks like an overhead view of the surroundings, such as we imagine we might see visually if were to observe the surroundings from an overhead position. A rotating radial line, corresponding to the direction of the radar beam, leaves bright spots corresponding to the locations to the objects reflecting the beam. Another early screen type is shown on the left. There is no sense that it mimics the familiar pictorial images of ordinary vision. In the "L-type scope," the distance vertically in the display of the "blip" indicates the distance to the object reflecting the beam. The horizontal deflection in the display shows the magnitude of the reflection. The direction to the object is determined independently by noting the direction in which the radar antenna is pointing.

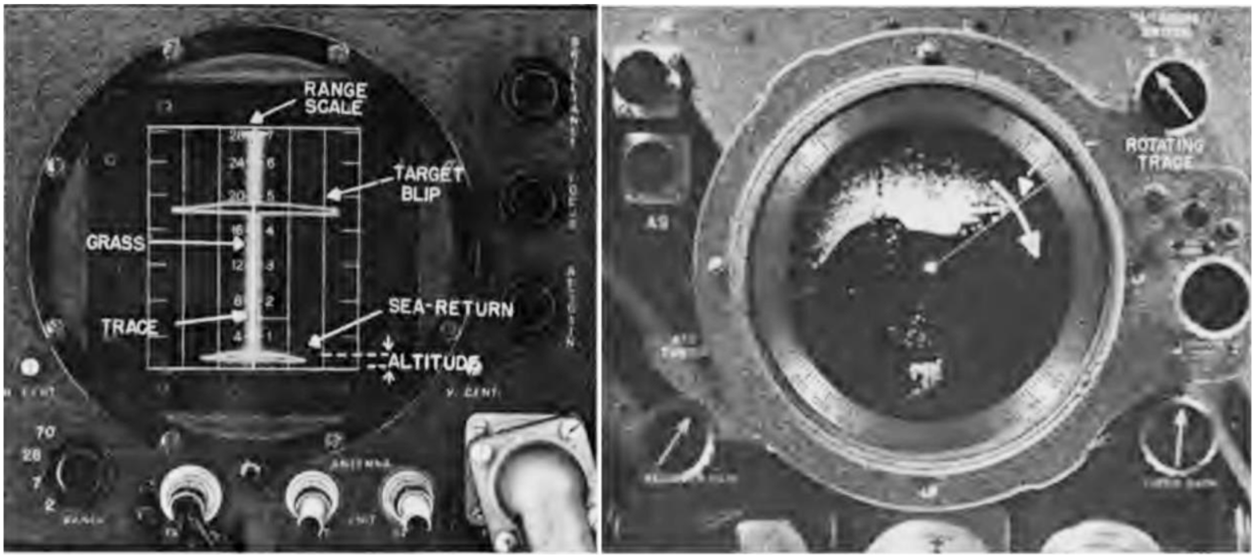

Figure 4. "L-type scope" and "PPI-type scan" in early radar sets[90]

The non-visual presentation of the L-type scope gives quite serviceable information on the distances to the echoing objects. It may even provide better information than the more familiar display since the L-type scope isolates the echoes from a specific direction and can give a more detailed display of the magnitude of the echo.

3. Instruments Replace Human Sensory Organs: Analytic Chemistry

In traditional human sensory experience, the first stage of the process is the excitation of sensory organs as a way of registering the result of a process that connects us with the system of interest. Throughout the sciences, human sensory organs have been replaced by instrumentation. This transition is illustrated in this section by the transition in the methods of analytic chemistry over the last century from the well-trained eyes and noses of analytic chemists to the automation of analytic instruments.

It is a transition I experienced on a compressed timescale. As a teenager, fascinated by chemistry, I pored over old chemistry texts for interesting effects that I could recreate in my parents' garage. I routinely prepared many gases that I learned to identify by their smell. Some were easy, such as hydrogen sulphide ("rotten egg gas"), ammonia and chlorine. Might a red-hot

[90] Images are from Navy Department (1946, pp. 13-14), which is a US government publication and thus in the public domain.

copper wire immersed in methyl alcohol vapor produce formaldehyde in a self-perpetuating catalytic reaction? I tried and yes, it does, copiously; and I thereby learned to identify formaldehyde by its distinctive smell. (This was an earlier age in which little heed was paid to the question of which substances might be carcinogenic.)

A few years later, as a junior chemical engineer in an oil refinery, the means of identifying gases no longer needed my sense of smell. I would draw a gas sample from a process line with a thick rubber bladder and ride it over to the laboratory. There an analyst would pass it through a gas chromatograph and provide a quantitative breakdown of the composition of the gas sample. It far transcended what my risky sniff test could have revealed. We were testing for hydrogen, which is odorless. It had to be completely purged from our high temperature pressure vessels before oxygen could be introduced, lest the entire unit explode.

3.1 The March Test

James Marsh's (1836) sensitive test for arsenic was a celebrated triumph of nineteenth century analytic chemistry. Arsenic trioxide was freely available in the nineteenth century as, for example, rat poison. It is also poisonous to humans. It is a white powder, resembling flour, and its presence in food is not readily detectible by casual consumers. The symptoms of arsenic poisoning resemble the gastric symptoms of cholera, a common illness in the nineteenth century, so that poisoning by arsenic trioxide may not be correctly identified. As a result, arsenic trioxide had become the favored medium for murder by poisoning. Marsh's test was a celebrated addition to forensic science, since it made it possible to detect arsenic poisoning, even from trace quantities of arsenic. A footnote in Marsh's (1936, p. 229) paper notes that "The Large Gold Medal of the Society of Arts of London, was presented to Mr Marsh for the above valuable communication…"[91]

Marsh's test was not the first analytic test for arsenic. Rather its value lay in its greatly improved sensitivity. It would be applied to arsenic containing fluids, such as suspect food or drink, or the stomach contents of its poisoning victims. The test employed a sulfuric acid solution into which some pieces of zinc have been added. This is the standard method of producing hydrogen gas. If the sample is added to this solution, arsenic in it will be converted by

[91] See Hempel (2013) for further details of the historical context.

the hydrogen gas to arsine gas, or "arsenuretted hydrogen," as Marsh called it. The arsine gas was allowed to escape through a jet and, if ignited, burns in the air.

The trained senses of the analyst were essential to the test. Marsh (1836, p. 232) noted the presence of arsenic is already evident in the color of the flame, compared with that of the burning of the pure hydrogen: "If no arsenic be present, then the jet of the flame as it issues has a very different appearance…" A later treatment (ICS, 1900, p. 49) elaborates "If arsenic should be present, the flame of hydrogen burning at the end of the tube will, often in a few seconds, change its color, becoming whitish…" The flame is directed towards a cold surface. Marsh (p. 230) suggested a pane of window glass. Then, a distinctively visible layer of metallic arsenic is deposited. If the flame is directed into a glass tube, the analyst's sense of smell can complete the analysis. Marsh noted: (p. 230)

> In this case, if the tube, while still warm, be held to the nose, that peculiar odour, somewhat resembling garlic, which is one of the characteristic tests of arsenic, will be perceived. Arsenuretted hydrogen itself has precisely the same colour [as hydrogen?], but considerable caution should be used in smelling it, as every cubic inch contains about a quarter of a grain of arsenic.

Figure 5 shows the simpler apparatus Marsh (1836) proposed on the left and, on the right, a later, more elaborate apparatus described in ICS (1900, p. 49). It is called "Marsh's apparatus" and has added components to filter out possible contaminants in the arsine gas stream.

Figure 5. Apparatus for the Marsh test

3.2 Nineteenth Century Analytic Methods

The Marsh test illustrates two characteristics of nineteenth century analytic chemistry. First, the test depends narrowly on the specific chemistry of arsenic. It could not be used to detect other metals or metalloids in the sample.[92] Second, trained human sensory capacities comprise an essential step in the analysis. The analyst should recognize the difference between a hydrogen flame and an arsine flame, how a metallic deposit of arsenic on the cooled surface looks and the distinctive garlic-like smell of arsine.

We see these same characteristics in the huge range of different analytic tests devised by nineteenth century chemists. Muter (1898) is a massive catalog of chemical reactions and related physical properties that can be used to determine the chemical composition of a sample. It is divided into two parts: the first covers qualitative analysis; and the second covers quantitative analysis. No simple summary is possible since the text just consists of a massive compilation of distinct methods. They are given is a terse form to facilitate quick consultation by practicing

---

92 Antinomy was an exception, as noted in ICS (1900, p. 16). Antinomy deposits can be distinguished from arsenic, the source notes, since they are less volatile and not removed by a solution of chlorinated lime.

chemical analysts. An example, chosen quite arbitrarily, concerns the detection of manganese compounds and gives an indication of the methods: (p. 21)

> NH_4HS *in the presence of* NH_4Cl *and* NH_4HO (*group reagent*) precipitates a flesh-coloured manganous sulphide—MnS—soluble in dilute and cold hydrochloric acid (distinction from the sulphides of Ni and Co). It is also soluble in acetic acid (distinction from zinc sulphide). This precipitate forms sometimes very slowly and only after gently warming. If a good excess of NH_4Cl has not been added, or if, after adding the excess of ammonium hydrate, the solution be exposed to the air, a portion of the manganese will sometimes precipitate spontaneously, as manganic dioxyhydrate—$Mn_2O_3(HO)_2$— and be found with the iron, etc., in the first division of the third group. In this case its presence will be easily made manifest during the fusion for chromium by the residue being green. It is therefore evident that small quantities of manganese cannot be perfectly separated from large quantities of iron by NH_4Cl and $NH_4H\grave{O}$ only.

Later, the means of detecting hydrochloric acid are listed: (p. 30)

> Hydrochloric Acid—HCl—may be recognised—
>
> 1. By its acidity and its giving off Cl_2 when heated with MnO_2.
> 2. By producing dense white fumes when a rod dipped in ammonium hydrate is held over the mouth of the bottle.[93]
> 3. By giving a curdy white precipitate of argentic chloride with argentic nitrate, instantly soluble in ammonium hydrate.

In these, and many, many more paragraphs like them, we see that the analyst is actively engaged in selecting and undertaking a sequence of tests that would enable the discrimination among many possible substances. The results require observation by a skilled analyst who has no trouble discerning just what is meant by a "flesh-coloured" precipitate, how it differs visually from the sulphide precipitates of cobalt, nickel and zinc, by a "curdy white precipitate" and that a pungent-smelling gas given off is chlorine gas (Cl_2).

[93] As a teenager, I was greatly amused by this acid's ability to produce these dense white clouds of ammonium chloride when exposed to vapors of a strong ammonia solution.

3.3 Instrumental Methods

In the course of the twentieth century, analytic methods were transformed. The new methods no longer focused on the chemical properties of the substances to be analyzed. Instead, they worked on the physical properties, such as the masses of the individual atoms or molecular fragments, the different rates at which they might diffuse in media and the characteristic frequencies of radiation they may emit or absorb. The transformation in analytic chemistry has been massive. The older "wet" methods, such as developed in the nineteenth century, have been replaced almost entirely by automatic instrumentation. Crucially for our purposes, human sensory organs no longer play a central role in analysis. Rather the presence of atomic or molecular species in the sample is read, for example, from the location of peaks in a graph produced by the instrument. An operator no longer needs a trained nose to distinguish benzene from acetone. Which is present and to which extent is read from the location and magnitude of peaks in a graph produced by the instrument.

A recent textbook (Robinson et al., 2021) surveys the range of analytic instrumentation according to the physical principles implemented in each. The survey is divided into sections reflecting the principles employed. The methods are:

Spectroscopy: divided into visible and ultraviolet molecular spectroscopy; infrared, near-infrared, and Raman spectroscopy; magnetic resonance spectroscopy; atomic absorption spectrometry; atomic emission spectroscopy; x-ray spectroscopy; mass spectrometry.

Chromatography: divided into gas chromatography; chromatography with liquid mobile phases; electroanalytical chemistry.

Thermal Analysis

In X-ray dispersal spectroscopy, a sample is irradiated by X-rays that induce the emission of secondary X-rays. The emission occurs when electrons in the irradiated atoms jump to higher energy states; and then, on returning to their lower energy states, reradiate the energy acquired during their irradiation. For each element irradiated, the energy of the X-ray quanta carried by the emitted radiation can take on only a small set of values, according to the characteristic energy differences between the energy states allowed for the atom's electrons. The energies of this emitted radiation are so distinctive of each chemical element that they can serve as a fingerprint that identifies the presence of that element.

Figure 6 shows an example of X-ray dispersion spectroscopy. The sample is a mineral crust on the vent shrimp *Rimicaris exoculate*. The plot in the figure shows the intensities of the emitted radiation, separated out according to their energies. Each peak can then be associated with a definite chemical element: C, carbon; O, oxygen; Fe, iron; Mg, magnesium; Si, silicon; P phosphorus; and so on.

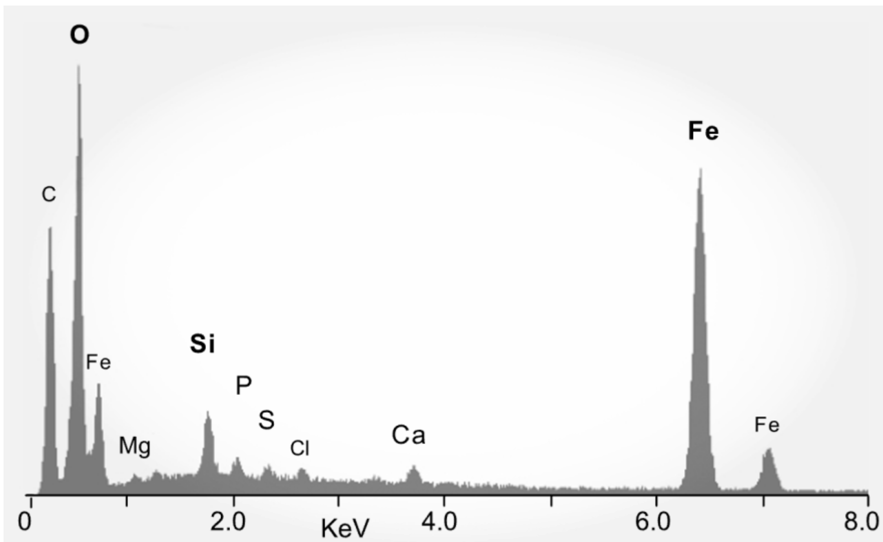

Figure 6. Energy Dispersal X-ray Spectrum (EDS)[94]

The method allows detection of the range of elements present in one analytic process. This replaces the multiple tests that nineteenth century methods required; and it dispenses with the need for chemists trained to distinguish the pungent odor of Chlorine (Cl) from that of hydrogen chloride (HCl).

Mass spectrometry employs a physical principle already well understood in the nineteenth century. A beam of charged particles, atoms or molecular fragments in an evacuated chamber, can be deflected by electric and magnetic fields. The magnitude of the deflection is fixed by the mass to charge ratio of the charged species and by the strengths of the deflecting fields. This process was employed, famously, by J J Thomson (1897) on cathode rays, which he believed to consist of charged particles, later known to us as electrons. His belief in the particle

[94] Source: Corbari et al. (2008), Figure 6. "This work is distributed under the Creative Commons Attribution 3.0 License."

character of the beam was affirmed when varying the strengths of the deflecting fields allowed him to determine a unique mass to charge ratio for its constituent particles.

A few decades later, A. J. Dempster and F. W. Aston employed the same technique for chemical analysis. It can be applied to any sample that can be vaporized and ionized, so that the sample can form an electrically charged beam of ions. The ions can then be sorted according to their mass to charge ratios. The sorting is carried out by varying the strength of the deflecting fields. Figure 7 shows a schematic of Dempster's (1918) original apparatus.

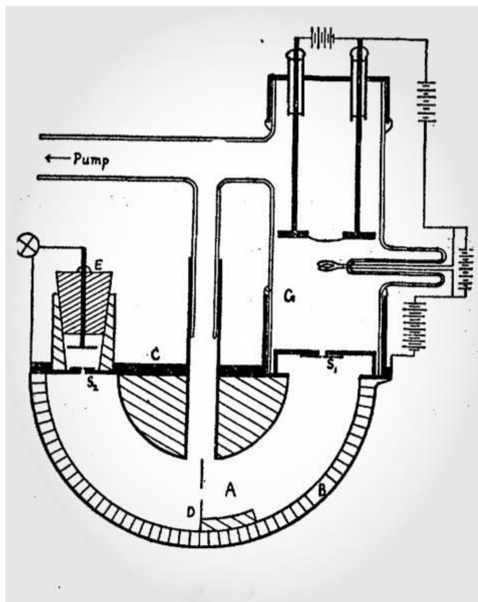

Figure 7. Dempster's mass spectrometer[95]

The beam of charged ions is generated at the left. It is then deflected in a semi-circular arc through the semi-circular channel in the bottom half of the figure to the detector at the right. Only charged ions with just the right mass to charge ratio can fully traverse the channel. Changing the strength of the deflecting field, allows a separation of the charged ions according to their mass to charge ratios to complete the analysis.

Marsh's test for arsenic remained the favored test well into the twentieth century. Mass spectrometry proved to be a better option. Tanaka et al. (1996) showed the effectiveness of the method for forensic purposed for detecting arsenic in blood and stomach contents. In a single

[95] Image source: Dempster (1918), Fig. 1, p. 317.

test, their mass spectrum allowed them to determine the presence and extent of many elements, including potassium, calcium, iron, zinc, copper and arsenic, without the need to detect the garlic-like scent of arsine.

4. Intelligent Instruments: Astronomical Detection of New Stars

The replacing of human sensory organs by instrumentation is only a small part of the transition in the empirical foundations of science. Another part of the human sensory experience happens automatically. We do not just recognize that one or other sensory receptor has been stimulated. We recognize that, through the smoky glass at a solar eclipse, we are seeing the sun's disk with a moon shaped piece occluded; or that we just heard the distinctive squeal of an ignited hydrogen/oxygen mixture; or that we are smelling roasted coffee[96] and not cocoa or onions. The recognition is immediate and separating it from the mere fact of stimulation is artificial. Consider the photograph below in Figure 8. It is not perceived as a pattern of various shades of grey. It is instantly recognized as an image of the Egyptian Sphinx. Only a few moments of subsequent reflection tells us that the image is likely quite old and comes from the first excavations of the artifact.

[96] Coffee tasting has long been the province of human experts. Gabrieli et al. (2022) report success in creating what they call an "AI-assisted electronic tongue" that could discriminate among 21 varieties of coffee.

Figure 8. The Egyptian Sphinx[97]

Across the sciences, instrumentation is replacing this automatic faculty of human sensory processes; and often in a way that far exceeds human capacities. The example to be explored in this section is the development of automatic techniques in astronomy for detecting and characterizing new celestial objects. The following Section 5 describes how the detection and characterization of genetic material has been automated.

4.1 Naked-Eye Astronomy

Simple observation of the night sky has, for millennia, sufficed for the identification of comets and other bright novelties. Two have a special place in sixteenth and seventeenth century astronomy. Their appearance was useful evidence that, contrary to ancient Greek thought, the heavens were not immutable. In November 1572, Tycho Brahe observed a new star in the constellation Cassiopeia. Figure 9 is his representation of it in his Brahe (1573). The "new star" (*nova stella* in Latin) is marked *I*:

[97] Photograph by Beniamino Facchinelli (1839-1895). Source https://gallica.bnf.fr/ark:/12148/btv1b10508565d/f1.item#

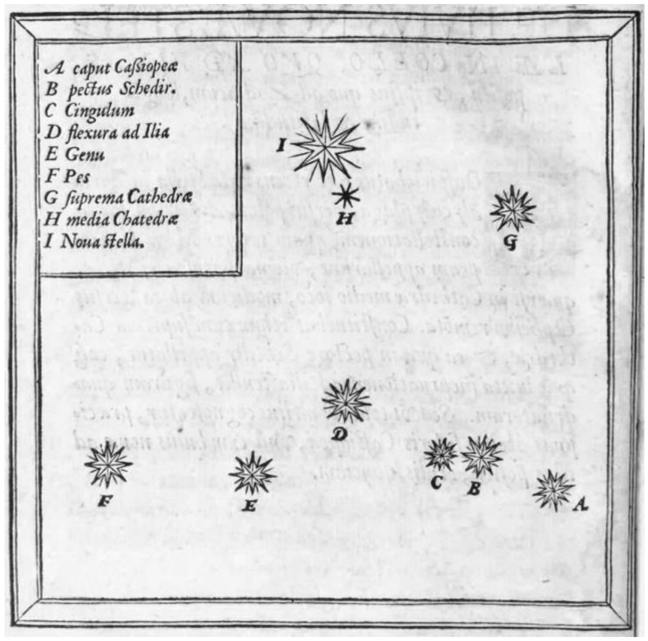

Figure 9. Tycho Brahe's *Nova Stella*

Johannes Kepler (1606) then recorded his observations of a second new star appearing in 1604 in the constellation *Ophioukhos* (*Serpentarius*, in the title of Kepler (1606)).

4.2 The Telescope

Famously, the first step beyond naked-eye astronomy towards observations through mediating instruments came within a few years. Galileo commenced his observations of the heavens using the new instrumentation of the telescope. In January 1610, he began nightly observations of the stars around the planet Jupiter. He saw three and then four stars arranged in a straight line that was parallel to the plane of the ecliptic. Over subsequent nights in January through to March, he carefully noted how these stars rearranged their positions. As a result, Galileo could identify them as moons, orbiting Jupiter. He called them the "Medicean planets" in deference to his patron, Cosmo II de' Medici. In his (1610) *Sidereus Nuncius*, Galileo printed images of 63 of his nightly observations. The first seven only are show in in Figure 10.

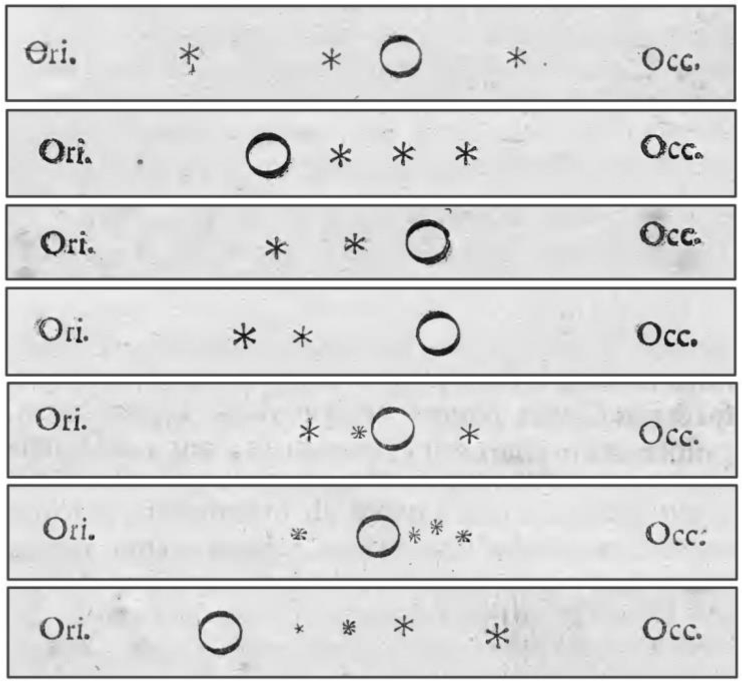

Figure 10. Galileo's first seven images of Jupiter and its surrounding moons on successive nights[98]

Once he had identified the stars as moons, Galileo immediately pointed out how this deflated an objection to the new Copernican, heliocentric planetary system. It seemed anomalous that our Earth's moon should be the exceptional body that did not directly orbit the sun. Galileo could now report other moons orbiting another planet. The Earth's moon was not a lone exception.

4.3 The Blink Comparator

The introduction of astrophotography altered the methods used for detecting new celestial bodies. Instead of painstakingly scrutinizing the heavens night upon night for new objects, astronomers could take multiple photographic plates over many nights as records of the state of some small patch of the heavens. The search for new celestial objects now reverted to a painstaking comparison of these plates, tiny region by tiny region. The tedium was relieved by the introduction of the blink comparator. Two plates of images of the same part of the night sky were loaded into the comparator for inspection by the user. The comparator would flip back and forth at roughly one second intervals between some portion of the first plate and the

[98] Ori. = Oriens, East. Occi. = Occidens, West.

corresponding portion of the second plate. Any difference between the two plates would appear dynamically.

The most celebrated use of the blink comparator was Clyde Tombaugh's January 1930 discovery of a long-suspected planet outside the orbit of Uranus. This was the "trans-Neptunian" planet long sought by Percival Lowell (1915). It was named "Pluto," with the first letters "PL" matching Lowell's initials, and only 76 years later demoted to the ignominy of a "dwarf planet." Figure 11 shows a pair of images taken by Clyde Tombaugh in January 1930 in which the position of what will be identified as Pluto changes as marked.

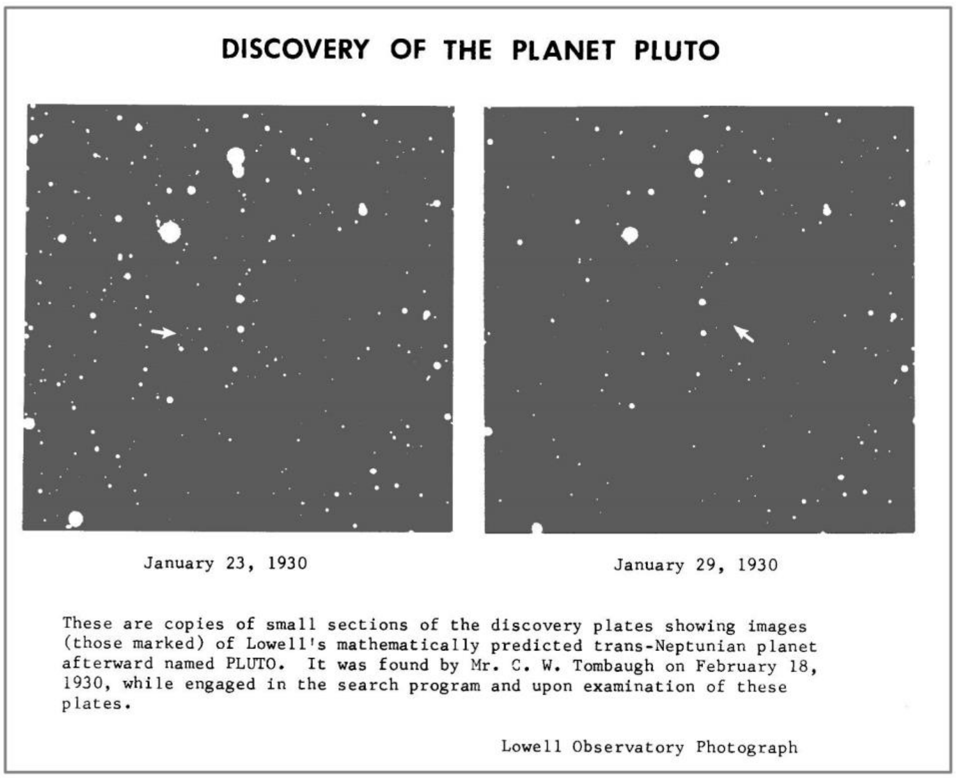

Figure 11. Clyde Tombaugh's images in which Pluto was found[99]

[99] Image source: Lowell Observatory archives, https://collectionslowellobservatory.omeka.net/items/show/1247 The image is used under the provisions of fair use. The photos enter the public domain in January 2026, which is 95 years after their creation.

Popular accounts reduce Tombaugh's discovery merely to noticing a moving celestial object in a part of the sky. For example, the noted astronomer H. N. Russell (1930), in his July 1930 *Scientific American* article described the discovery as follows: (p. 21)

> Photographs of moderate exposure covering the whole region of the heavens around the predicted position were obtained and on one of them, taken on January 21, 1930, Mr. Tombaugh of the observatory staff found "a very promising object." It was identified from its motion past the numerous fixed stars as revealed on plates of the same star field while being compared under the blink comparator. This showed that one faint star among many thousands had shifted its place by a certain expected order of distance, in the interval between the taking of the two plates.

This report, no doubt abbreviated for a popular audience, understates the amount of interpretation needed. Tombaugh had to do more than merely see something moving in a suitable part of the sky. He had to determine that it was not a spurious sighting of something other than a new planet. In a later recollection, Tombaugh (1946) recounted how he precluded the possibility that his sighting was of an asteroid or other "suspicious object." (pp. 76-77)

> Much time, effort and expense may be lost in running down planet-suspects that turn out to be only asteroids near their apparent stationary points where they imitate the slow motion of a more distant planet. The simple expedient was to photograph each region near its "opposition point" (180° from the Sun), where the apparent retrograde motion is a maximum for all planets outside the Earth's orbit, and the daily shift in position against the star background is roughly inversely proportional to the distance of the object. As a consequence, the asteroids, on the average, moved about 7 millimeters per day on the plates, and exhibited short trails during the hour's time of exposure, whereas Pluto moved only 1/2 millimeter per day. This criterion was useful in estimating at sight the distance of any suspicious object, and extremely convenient in computing a rough ephemeris when it was necessary to re-photograph a region later in running down a promising planet suspect. The known asteroids number a few thousand and are widely scattered between the orbits of Mars and Jupiter.

Even this apparently quite mechanical observation required considerable intelligent interpretation.

4.4 The Discovery of Exoplanets

Both Galileo and Tombaugh applied human insight to complete their identification of novel celestial bodies. The need to preclude spurious sightings persists in the modern search for new celestial bodies. Because of the large number of candidate objects delivered by modern telescopes, the sort of insights Galileo and Tombaugh provided had to be automated. There are just too many candidate objects for practical human scrutiny.

This automation has been a central part of the recent discovery of thousands of "exoplanets." They are planets that orbit stars in our galaxy outside our solar system. The search for these exoplanets was furthered decisively by the masses of new data supplied by the Kepler space telescope mission. It was launched in 2009 and decommissioned in 2018. There are several methods of detecting exoplanets. One involves a detection of the dimming of the light from a star when an exoplanet passes in front of it. Figure 12 shows the dip in brightness due to an exoplanet transit for the Kepler mission's first five exoplanet discoveries.

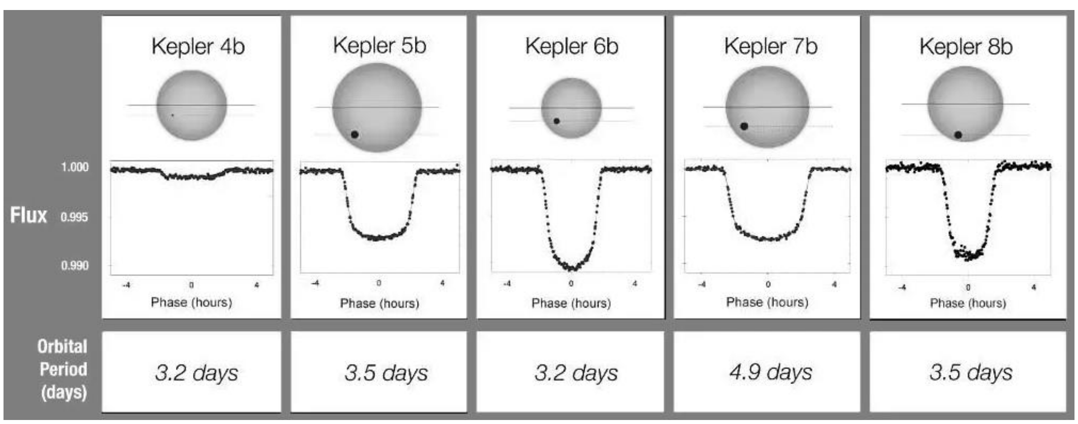

Figure 12. Brightness dip from exoplanet transit[100]

The age-old difficulty arises with this detection method. These sorts of dimmings can come about in other ways. For example, they may result from a binary star when the alignment of the pair of stars is such that one eclipses the other; or they may arise from some other chance alignments. The challenge was to discriminate these so-called "false positives" from the genuine

[100] Source: https://www.nasa.gov/image-article/light-curves-of-keplers-first-5-discoveries/ Public domain image from US Governement website.

detection of an exoplanet. The huge number of candidate “Kepler objects of interest” precluded easy human scanning. Morton et al. (2016) reported the successful result of an automated search of Kepler’s database of survey results by means of the “VESPA” procedure.[101] To use it, they fitted a trapezoidal shape to the curve of the light dimming in the signals from the Kepler survey. The procedure then applied a simple Bayesian model to distinguish which of the modeled signals were most likely to result from an exoplanet as opposed to another source. Among 7056 Kepler “objects of interest,” they found 1935 that were most likely exoplanets. That is, there was a less than 0.1% posterior probability that they were false positives. For our purposes, what matters is that this search was not conducted by humans, scanning case after case. It was fully automated. “This work,” Morton et al (2016, p.2) were proud to report, “presents results from applying VESPA *en masse* to the entire Kepler catalog.”

The program of discovery of exoplanets has continued to advance since the time of these earlier studies. In a 2024 account of the different methods of exoplanet detection, Kaushik *et al.* (2025) report a new tally of over 5500 exoplanet discoveries; and the number continues to grow. As Morton *et al.* (2023) report, the VESPA procedure has been recommended for retirement. The detection of false positives is now implemented successfully by machine learning algorithms, such as reported in Armstrong *et al.* (2021).

The on-going search for exoplanets is just one of the programs of astronomical investigation in which the search for celestial objects has become automated. Another example is the All-Sky Automated Survey for Supernovae (ASAS-SN). It was initiated in the later 2010s in order to search for supernovae and other similar objects. Its scope has since expanded to include the search for a wider range of variable stars. The number of variables recovered is so enormous that automated techniques are essential. A 2021 report (Jayasinghe, T. *et al.*, 2021) noted that the survey had then already cataloged over 60 million sources in which about 426,000 variable sources were located by machine learning methods, of which about 219,000 were new discoveries.

4.5 The Event Horizon Telescope

In astronomy, there are many more important instances of the use of instrumentally mediated observations. A most striking example has been provided by the Event Horizon

[101] “Virtual European Solar and Planetary Access”

Telescope Collaboration. On April 10, 2019, the Collaboration published its first image (Figure 13) of the black hole at the center of the galaxy M("Messier")87.

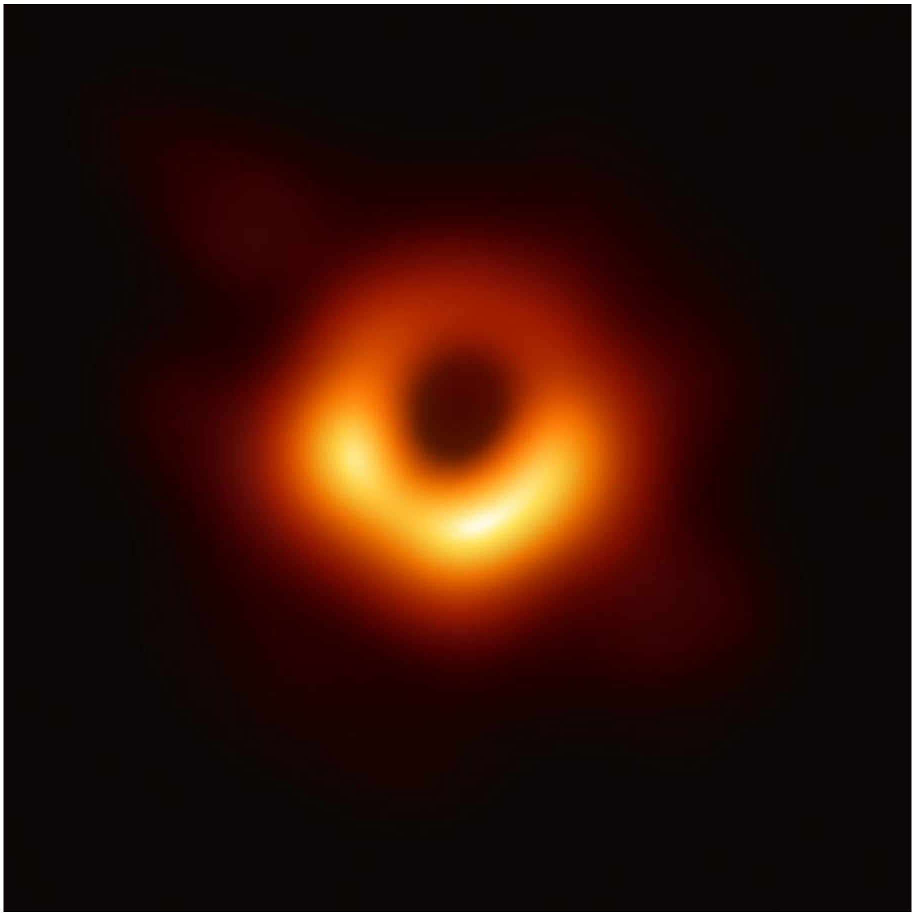

Figure 13. Event Horizon Telescope image of the black hole in galaxy M87[102]

The announcement was greeted with enthusiasm in the press. On the day of the Collaboration's announcement, The *New York Times* published a celebratory report (Overbye, 2019) with the full headline: "Darkness Visible, Finally: Astronomers Capture First Ever Image of a Black Hole: Astronomers at last have captured a picture of one of the most secretive entities in the cosmos." Casual readers of the headline would be forgiven for imagining an astronomer,

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[102] Image source: https://en.wikipedia.org/wiki/File:Black_hole_-_Messier_87_crop_max_res.jpg "This file is licensed under the Creative Commons Attribution 4.0 International license."

peering through the eyepiece of some immense device—the new "event horizon telescope"—and, in astonishment, seeing something most extraordinary. In great excitement, a camera shutter clicks and an image is captured for us all to see of the most elusive beast in the astronomical menagerie.

The reality of the production of the image is much different. The famous image was never seen by some eager astronomer, hovering over the eyepiece of a fancy new telescope. To begin, the image is from electromagnetic radiation in the gigahertz frequency range used by radio telescopes. It is far from the terahertz frequencies of visible light. The black hole shadow captured is far too small an object in the sky to be resolved by any single radio telescope. A device with an aperture the size of the earth itself is needed to resolve it. The Collaboration managed to simulate such a device by collecting huge amounts of data from many radio telescopes, spread across the surface of the earth. Collecting that data was just a first step. What followed was massive processing over two years of the data by multiple teams in parallel. The celebrated image was produced as a consensus of their work. See Ochigame et al. (manuscript) for a careful analysis with attention to epistemological details of the production of the image.

In 1610, in his *Siderius Nuncius* (1610, pp. 8-11), Galileo published his hand-drawn images of the surface of the moon derived on his telescopic observations. The changing patterns of light and dark over time in those images provided observational evidence of high mountains and low seas. It was the beginning of a new, instrumentally mediated tradition of observation in astronomy. Four centuries later, images of light and dark, derived from radio telescopes, provide observational evidence of a black hole's event horizon.

5. Identification of Genetic Material in Cells

5.1 Flemming's Chromatin

In the nineteenth century, genetic material in cell nuclei was identified using optical microscopes. Walther Flemming (1882) saw rounded clumps containing coiled threads in the cell nuclei. He called them (p. 130) "chromatin," because of their special affinity in taking up dyes, which made them more easily visible under optical microscopes. (We now recognize chromatin as consisting of coiled DNA and associated proteins.) Figure 14 is Flemming's (1882, p. 319) representation of the cell division of cells of a green algae, spirogyra. It shows the fission of the chromatin into two and the formation of two cell nuclei. Flemming (1882, p. 376) later called the division "mitosis."

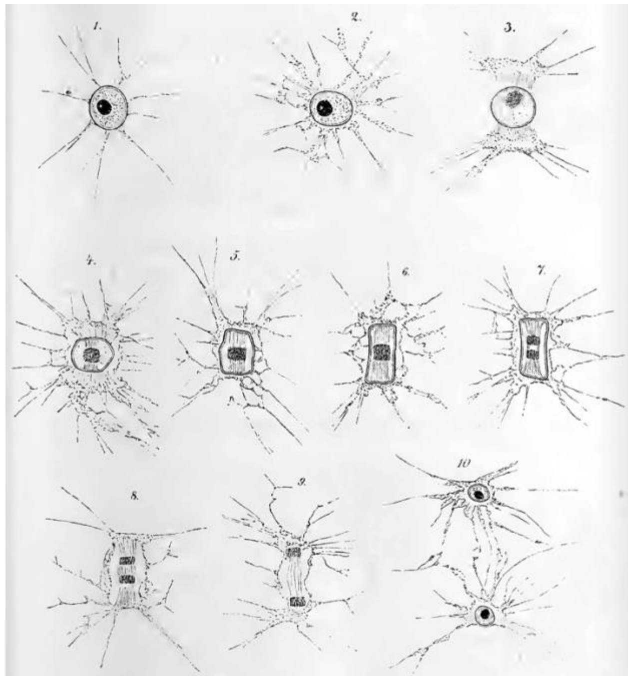

Figure 14. Flemming's depiction of cell division

This image in unusual because Flemming gives some details of its creation. It was by traditional optical instruments. He wrote (p. 319) "Images 1–9 were taken with a camera clara [an old form of a *camera obscura*], all at the same magnification (photographically reduced)." The images are not photographs, but, apparently, hand drawn from them. He wrote (p. 401): "The text images [in this volume] … are drawn directly from the specimens."

5.2 X-ray Diffraction Imagery

Flemming's investigations dealt with images visible through an optical microscope. They were accessible to ordinary visual experience. The identification of rounded clumps and threads was automatic since the microscopic images give familiar, pictorial representation of their subjects. In the mid twentieth century, the further investigation of chromatin employed X-rays. They no longer provided pictorial images that could be interpreted automatically with normal human vision. They were the next stage in the replacing of human experience by instrumentation.

X-ray diffraction imagery play a major part in the famous story of Crick and Watson's Nobel Prize winning identification of the double helix structure of DNA. In the April 1953 issue of *Nature*, Watson and Crick (1953) published their still tentative structure for the DNA molecule.[103] Behind their announcement lay an energetic competition to be the first to determine the molecular structure of DNA. In their 1953 note, Watson and Crick disputed their chief competitor, the triple chain model of Linus Pauling and others.

An important component of Crick and Watson's evidence for the double helix lay in X-ray diffraction images of crystalline DNA. When X-rays diffract off the regular lattice of a crystal, they produce patterns that are captured photographically. The images of chromatin that Flemming saw through his microscope were pictorially akin to their source. A clump looks like a clump. A thread looks like a thread. X-ray diffraction photographs, however, as shown in Figure 15 below, are not pictorial. They consist of streaks and spots; and it requires expertise in X-ray crystallography to decode the crystal structure that produced them.

This interpretive step was then still carried out by human cognition. Such a step is recounted by Watson in his autobiographical account of the discovery. Rosalind Franklin and her student Raymond Gosling had prepared painstakingly some of the best X-ray diffraction images of DNA. They included a "B" form of DNA that arises when the molecule is surrounded by large amounts of water. In January 1953, Watson visited Franklin's laboratory at King's College, London. During the visit, Franklin's colleague, Maurice Wilkins, without Franklin's knowledge and possibly improperly, showed Watson one of Franklin's X-ray images of the B form of DNA. Watson (1968, Ch. 23) recalled the moment as one of high personal drama:

> The instant I saw the picture my mouth fell open and my pulse began to race. The pattern was unbelievably simpler than those obtained previously ("A" form). Moreover, the black cross of reflections which dominated the picture could arise only from a helical structure. With the A form, the argument for a helix was never straightforward and considerable ambiguity existed as to exactly which type of helical symmetry was present. With the B form, however, mere inspection of its X-

[103] "So far as we can tell, [the structure proposed] is roughly compatible with the experimental data, but it must be regarded as unproved until it has been checked against more exact results." (p. 737)

ray picture gave several of the vital helical parameters. Conceivably, after only a few minutes' calculations, the number of chains in the molecule could be fixed.

Figure 15 shows the difference between the X-ray diffraction patterns of the A and B forms.

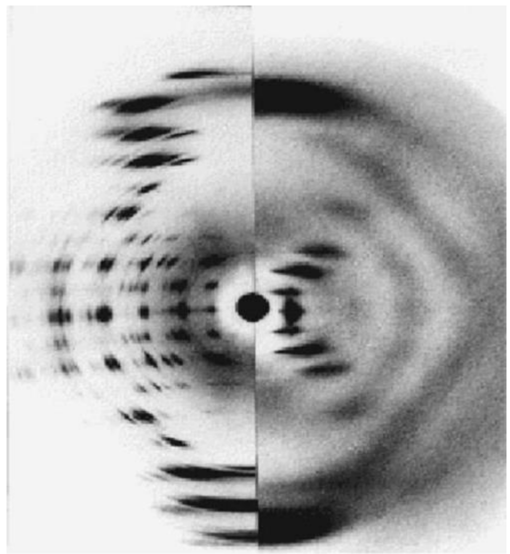

Figure 15. X-ray diffraction patterns for two forms of DNA[104]

We should read the narrative details of this moment with a little caution. It is part of a self-aggrandizing narrative that is also unconscionably dismissive of Franklin. For our purposes the main point is likely secure. Watson had become quite adept at understanding the sorts of X-ray diffraction patterns that would be produced by helical structures. Visual inspection of Franklin's X-ray image was sufficient for him to recognize its source as a helical structure. "Mere inspection," Franklin noted, sufficed.

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[104] Image source: https://commons.wikimedia.org/wiki/File:ABDNAxrgpj.jpg "Physical Chemistry of Foods, vol.2, van Nostrand Reinhold: New York, 1994." "This work is free and may be used by anyone for any purpose. … The Wikimedia Foundation has received an e-mail confirming that the copyright holder has approved publication under the terms mentioned on this page." "This file is licensed under the Creative Commons Attribution-Share Alike 3.0 Unported license."

5.3 Human Genome Sequencing

In the late nineteenth century, Flemming began research into the genetic material in a cell nucleus through his visual, microscopic examination of chromatin. The modern development of this program is the sequencing of genomes in the DNA of different organisms. That is, a genome is sequenced when we have a full map of order of the bases A, G, C, T (adenine, guanine, cytosine, thymine) in each strand of the organism's DNA. One of the foremost achievements of this development was the sequencing of the human genome. It was carried out by a massive collaboration, the International Human Genome Sequencing Consortium, from 1990 to 2003.[105] The final stage of the sequencing is yet another instance in which the recognition of the structure to be learned is no longer possible for human cognition. It must be carried out by computers.

In overall concept, the final stage of the sequencing of the human genome is akin to solving a jigsaw puzzle. Human cognition suffices to solve such a recreational puzzle. When two puzzle pieces match along their edges, they likely belong together in solved puzzle. A solver scrutinizes the various puzzle pieces for what the solver identifies as matching patterns at the edges. These matches enable a painstakingly assembly of the complete puzzle. A large and challenging puzzle may have as many as 1,000 pieces.

In DNA sequencing, the individual puzzle pieces are fragments of DNA of sufficient size that overlaps between their parts makes it likely that the two fragments belong together in the final sequence. The corresponding puzzle addressed in human genome sequencing is vastly larger than a recreational jigsaw puzzle. Human DNA consists of 23 chromosomes that contain approximately 3.1 *billion* base pairs. The assembly of the DNA fragments into a completed sequence could only be done by computers. The consortium (IHGSC, 2001, pp. 863-64) reported two related approaches: the "hierarchical shotgun sequencing" approach was implemented by the consortium; and the "whole-genome shotgun approach" was implemented by a biotechnology company, Celera Genomics. Agreement between the results of the two approaches affirmed the correctness of the final sequencing. Figure 16 shows how one of the "front-line participants" conceived of the shotgun approach.

[105] For reports on the completion of the sequencing, see IHGSC (2001, 2003).

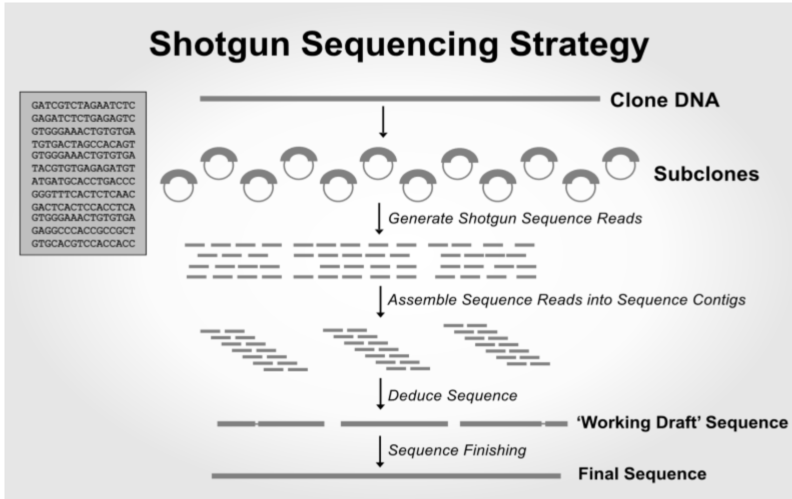

Figure 16. The shotgun approach as presented in a Human Genome Project Powerpoint slide[106]

6. LIGO: Laser Interferometer Gravitational Wave Observatory

Experience derives its authority from its realization as a physical process that connects us with the object of interest. It is the presence of that process that matters. That human sensory organs may be involved is incidental. Any suitably interpretable physical process can serve as experience in this generalized notion of experience, no matter how esoteric the process may be. Perhaps the most esoteric of these processes is supplied by LIGO.

Essentially all[107] astronomical investigations, since the earliest moments, have employed electromagnetic radiation. They were initially limited to visible light; that is, the small frequency band to which human eyes are sensitive. Recent developments, such as X-ray astronomy, have opened the usable band considerably. A most significant advance came with the 2015 success of the LIGO Scientific Collaboration and Virgo Collaboration. It employed a new channel for observation. In place of electromagnetic radiation, it employed gravitational waves.

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[106] Based on a slide from Powerpoint, Eric Green, "The Story of The Human Genome Project (HGP)…as Told by a Front-Line Participant" 9_1_Human_Genome_Project.pptx. Downloaded from website, The Human Genome Project, https://www.genome.gov/human-genome-project link at "Human Genome Project Overview Slides." Since the image is drawn from a US Government website, there is a presumption of public domain.

[107] The few exceptions include the examination of meteorites and, more recently, cosmic rays.

That there are waves propagating in a gravitational field was long supposed. It is a default expectation of any relativistic theory of gravity. Since special relativity is usually understood to prohibit effects propagating at faster than the speed of light, it seemed inevitable that a change in gravitational masses in some distant place must produce an effect that propagates towards us at no more than the speed of light. This expectation was realized theoretically in the 1910s with Einstein's discovery of the general theory of relativity. It is still our best theory of gravity and entails the existence of gravitational waves. Nevertheless, gravitational waves remained a troubled, theoretical speculation. For a while, Einstein even doubted their existence. Matters worsened with Joseph Weber's failed experimental efforts in the 1960s to detect gravitational waves.[108]

The formidable obstacle in detecting gravitational waves is that those likely accessible to us are extremely weak. Local detection is all but precluded since their effects are of the order magnitude of the thermal noise in even the best shielded detector. Their successful detection in 2015 by LIGO resulted from an extraordinary technological achievement. LIGO employs interferometers of enormous size. Each consists of two, four-kilometer-long evacuated tubes that are oriented perpendicularly. It follows from general relativity that a gravitational wave slightly alters the spatial geometries of bodies. When a gravitational wave passes through the interferometers, one arm contracts slightly and the other expands; and then the reverse; and so on. To detect these changes, the lengths of the arms are monitored by reflected laser beams. In spite of the huge size of the interferometers, the length changes that must be measured to detect gravitational waves are minuscule. Length changes of as small as 1/1,000th[109] the width of a proton are measurable by the LIGO interferometer. These minuscule perturbations have to be distinguished from background noise. That was achieved in 2015, with the first successful detection, by comparing the readings on two widely separated interferometers. One is in Hanford, Washington State; the other in Livingston, Louisiana, some 3,000 kilometers away. A gravitational wave would be judged as detected only when the changes in lengths ("strain")

[108] For a survey of the historical tribulations of gravitational waves, see Kennefick (2007).

[109] The LIGO website in 2025 expects this factor to be reduced to 1/10,000th. https://www.ligo.caltech.edu/page/facts

measured in both interferometers were well correlated. For it is unlikely that local noise at the two distant locations would be so correlated otherwise.

The Collaboration reported that their gravitational wave interferometer had detected a binary black hole merger in Abbott et al. (2016). The plots of the strain measurements against time in the two locations are given in a figure in Abbott *et al.* (2016). A section of the figure that shows the strains measured at each detector is given here as Figure 17.[110]

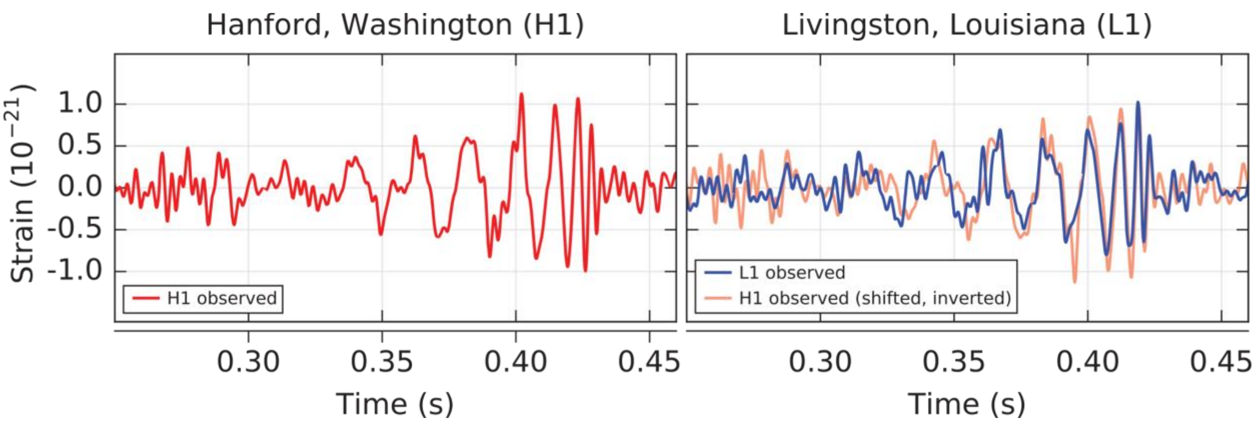

Figure 17. Strain measurements for LIGO event GW150914

That the event detected results from a distant black hole merger was determined by comparing the shape of the detected curve with a library of template curves, computed numerically for such events, on the basis of the general theory of relativity.

Unlike Brahe and Kepler's observations of novas, human sensory organs played no essential role in the detection and analysis of the merger. The initial detection was announced by an alert issued by computers engaged in real time monitoring of the data streams from the two interferometers. What followed was an elaborate reanalysis and interpretation of the detection, again all carried out by computers. The extent of these data processing operations is extraordinary. A paper devoted merely to these processes, Abbott et al. (2020), has over 150 authors.

The first and obvious achievement of the Collaborative was the empirical affirmation of a result predicted by general relativity, the existence of gravitational waves. The more significant

[110] From Figure 1 in Abbott (2016, p. 061102-2). "Published by the American Physical Society under the terms of the Creative Commons Attribution 3.0 License."

achievement for future work was heralded in an early publication by the LIGO Consortium. The title of Abbott et al. (2016a) talked of "the Era of First Discovery" and the abstract described the first discovery as "launching the era of gravitational-wave astronomy." That this was their ambition from the outset was heralded by their naming of the Consortium: "Laser Interferometer Gravitational Wave *Observatory*."

Rigorously skeptical empiricists may want to dispute whether processes as esoteric and hard to detect as gravitational waves can serve as a medium of observation. The scientists of LIGO clearly harbored no such reservations; and I think they are right. LIGO is an observatory in the sense that matters to empiricism. It employs physical processes, gravitational waves, that connect us continuously to events of interest, such as black hole mergers and other massive gravitational events.

A decade later, it had become clear that LIGO's ambitions to be an observatory had been realized. In a March 2025 press release (Burtnyk, 2025), the LIGO Collaborative announced the 200th gravitational wave detection in the then ongoing observational run; and the 290th detection in the overall life of the Consortium. These amount to the detection of many massive gravitational events. Most are black hole mergers, but also include neutron star mergers and neutron star-black hole mergers.

7. Conclusion

Empiricism has long privileged human sensory experience as the final arbiter of scientific fact. Might there really be egg-laying mammals with a duck bill in the new antipodean colony of New South Wales? Even eyewitness reports of platypuses published in 1802 did not convince all anatomists in the distant Northern hemisphere.[111] A sample that could be examined by them helped, but even then fears of a hoax specimen had to be allayed. It was a dispute that had to be settled by the eyewitness testimony of experts.

Such was the long-standing tradition of empiricism. Modern science has overturned this tradition. The ambitions of modern science now reach far beyond what eyewitness testimony can supply. There are no human sensory organs that can see, hear or smell the cataclysmic merger of two black holes. No human has the time and patience to scrutinize by eye the vast number of celestial objects now detected telescopically in a search for variable stars and exoplanets. Even

[111] See Hall (1999) for a recounting of this historical episode.

the most adept of human puzzle solvers are completely defeated by the challenge of assembling vast numbers of DNA fragments to reconstruct the human genome. All these discoveries are now made and interpreted automatically by instruments. They are the new authority that replaces the human eyewitness in an empiricism adapted to modern science.

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10. Experience as a Process

Chapter 10

Experience as a Process[112]

1. Introduction

A core presumptions of Big-E Empiricism as applied to science has proved to be one of its most enduring weaknesses. It is the supposition that the content of a science divides into that part identified as experience and the remaining part. The first is usually associated with observational and experimental results; the second part is commonly designated as "theoretical." The factual content of a scientific theory is asserted to reside solely in this experiential part. To sustain a claim this strong, Big-E Empiricism has to identifying precisely where this division of content lies. In spite of persistent efforts, it has failed to do so. Without that identification, it is a thesis that employs a division whose existence is conjectured but not demonstrated. The outcome is that this empiricism must weather continuing complaints that the division cannot be made and that the claimed theory ladenness of observation compromises the independent evidential import of experience.

Small-e empiricism escapes these problems since it discards the troublesome supposition of a strict division between experience and theory. It can discard this supposition since it recognizes that the authority of experience does not derive from some special privilege of a distinct body of pure experience. Rather its authority derives from a physical process that connects us continuously to the system of interest. Experience is identified with that process. There is no need to elevate any stage of the process to the pure experience in which all factual content is found.

Following the discussion of Chapter 8, insofar as they are to be a part of small-e empiricism, the physical condition of the stages of these processes is described propositionally. There is no strict division into experiential and non-experiential propositions. Some describe stages closer to the system of interest; and some farther from it. In place of this strict division is a

[112] I thank Nora Boyd for helpful discussion on an earlier draft of this chapter.

relational concept: "closer to the system of interest." This binary relation replaces the strict monadic division of most empiricisms.

This chapter articulates this relational conception of experience. It reviews its role in small-e empiricism and how it solves outstanding problems associated traditional empiricists' conception of experience. Section 2 will review one of the few accounts in present empiricist writing that defends similar themes. It is Nora Boyd's reformulation of empiricism in which the strict division of scientific content into experiential and non-experiential is replaced by "lines of evidence," which correspond loosely with the continuous physical processes of this chapter.

Section 3 describes the sorts of physical processes that implement the process notion of experience. They include electromagnetic propagations, such has been the almost exclusive process connecting us with celestial objects; vibrational modes of propagation, such as sound, which generated experiential reports of the Krakatoa volcanic eruption of August 1883; and physical transport, which is the process implemented in sampling.

The various stages of the experiential process are described propositionally in small-e empiricism. Section 4 recalls how the propositions describing stages closer to the system of interest are supported inductively by those farther from it. The relations are illustrated with three examples: Galileo's telescopic discovery of the mountains of the moon; the reports of the sound of the Krakatoa volcanic eruption of August 1883; and how relations of inductive support are essential in sampling and in the forensic concept of the chain of custody.

Section 5 recounts how, as a practical matter, any propositional description of an experiential process will have a terminus, farthest from the system of interest. That terminus is selected for convenience. That is, we judge its propositional description to be sufficiently freed of results, deeper in the theory of the system of interest, to represent what we can usefully learn of the target system from the experiential process. To regress further would, we judge, encumber the propositional description with details that would not enhance to goal of learning about the system of interest.

A summary statement of the import of an experiential process is commonly given in a proposition describing the system of interest. The security of that proposition depends on the cogency of its inductive support by propositions describing stages farther from the system of interest. The possibility of regressing to stages farther from the system of interest allow us to correct flaws in these relations of inductive support. This process of correction, elaborated in

Section 6, is called "winding back." In it, we step back through the relations of support towards stages farther from the target system. At each step, we check the cogency of the relations of inductive support and note especially if a step relies on presumptions that have lost support or are otherwise unreliable. Winding back, when possible, allows corrections to the results reported from experience processes and ensures a high level of security in reports from experience.

The following Sections 7 and 8 illustrate of how successful winding back corrected erroneous reports of experience. The first is the correction of Fermi's erroneous 1934 claim that he had created a new element with atomic number 93. Winding back revealed that Fermi's analysis had incorrectly restricted the range of possible nuclear processes in his experiments. The second is the correction of Hubble's incorrect estimates from 1929 and later of the distances to the galaxies in the context of his discovery of the expansion of the galaxies. Hubble had incorrectly assumed that the Cepheid variable stars that anchored his estimates of distances were all of the same type. Examples, such as these, illustrate how winding back allows small-e empiricism to correct for problems arising through the theory ladenness of observation. [113]

The prospects of this process of winding back are limited if there are insufficient records of the stages of the experiential processes. Section 9 provides two examples. The first is Michelson's measurement of the speed of light in the 1920s. The results were confounded when it turned out that the distance between neighboring Californian mountain peaks was not securely known due to neglected seismic activity. The more enduring case concerns the repeated reports of sightings of UFOs, where incomplete records in case after case preclude proper interpretation of the reports.

2. Boyd's Empiricism

In a series of important publications starting with Boyd (2018a, b), Nora Boyd has developed an empiricism for science that dispenses with the idea of a strict division between experiential and non-experiential content. To begin, she identified how proper attention to the actual practice of science requires us to replace the cognitive conception of experience of older empiricisms with something better adapted to scientific practice. She wrote: pp. 403-404

> The fact that the output of scientific instrumentation eventually needs to make a transcranial journey in order to be of any real epistemic interest ought not mislead

[113] See Chapter 11 for further discussion of the theory ladenness of observation.

> us into thinking that the empirical is best understood as 'observable' or 'sensible'. … In the hope of replacing observations with something more suitable to science in practice, we might consider the more generic 'empirical results', where 'results' may be understood to include observations and other sensings but also the results of technology-aided detections and measurements, and 'empirical' may be understood in contrast with 'virtual' and 'imagined' and could be cashed out by appeal to a causal story connecting the target of interest to the generation of that result.

She soon proposes to "leave behind talk of 'experience' right away and speak instead of empirical evidence." (2018a, p. 406). The richest form of empirical evidence is summarized in a paragraph offset in her text as the definition: (2018a, pp. 406-407, her boldface)

> **Enriched Evidence**. The evidence with respect to which empirical adequacy is to be adjudicated is made up of lines of evidence enriched by auxiliary information about how those lines were generated. By "line of evidence" I mean a sequence of empirical results including the records of data collection and all subsequent products of data processing generated on the way to some final empirical constraint. By auxiliary information, I mean the metadata regarding the provenance of the data records and the processing workflow that transforms them. Together, a line of evidence and its associated metadata compose what I am calling an "enriched line of evidence." The evidential corpus is then to be made up of many such enriched lines of evidence."

The key notion is the "enriched line of evidence." It consists of artifacts, such as physical data records, and propositional content that enables a connection between the artifacts and the broader content of the science. The propositional content is the metadata that comes in two forms (2018a, p. 410): "provenance" metadata describes how the data were collected; and "work-flow" describes how the data were processed. The metadata is essential if the data is to serve as evidence; otherwise, the data are merely artifacts. This important role for the metadata precludes a clean separation of the experiential and non-experiential components of science.

This new empiricism, better adapted to actual scientific practice, differs from small-e empiricism in two ways. First, the notion of enriched evidence includes artifacts and its propositional content is limited in scope by its attachment as metadata to these artifacts. In small-e empiricism, experiential content is solely propositional and can have further scope. The propositions are required only to apply to the physical processes that connect us continuously to the system of interest.

Second, this new empiricism retains without special emphasis the traditional empiricists' skeptical reservations about scientific content that goes beyond experience. Enriched evidence enables us to judge the empirical adequacy of a science and in this capacity to constrain theory. It does not assert that the further results of the science can be factual, that is, truths of the world, even if that status is only inductively secured. It requires only that they mediate in successful accommodation of a fuller body of evidence.

Boyd (§3) identified three benefits of the new form of empiricism. All of them mitigate the skepticism usually associated with empiricism. The first two are called by her "accumulation" and "amalgamation." The notion of enriched evidence allows for the successful accumulation of results in science, even when an old scientific theory is replaced by new one. If the evidence for an old theory has been properly curated, it is sufficiently specific that the new evidence merely adds to the evidence for a new theory. With properly curated evidence, it is possible to amalgamate different lines of evidence, even though they may derive from very different sources. Proper curation will specify enough of the details of the evidence at a sufficiently primitive level that nothing in it contradicts the new theory. This gives science a sense of irreversible progress through an ever-growing body of stable empirical evidence. In one example, she showed how early Chinese astronomical records (2018a, p. 408), from as early as 1054 and 1056BCE, can still serve as evidence of supernovae in modern astrophysics, as long as we formulate the associated enriched evidence carefully.

The third benefit, "breaking underdetermination," is one that small-e empiricism will also exploit. The theory ladenness of observation, she reported, is often supposed to preclude the capacity of observational evidence to decide between competing theories. The ensuing presumption of an unavoidable underdetermination of theories by evidence is "broken," to use her term ("breaking," 2018a, p. 417), by paying attention to the metadata. When the metadata includes suppositions that presume one or other of the competing theories, it is often practical to step back to an earlier stage in the data collection or processing that does not employ those suppositions. Then the enriched evidence can be brought to bear on both theories and aid in deciding between them. An example she provided (2018a, p. 417) is the evidence of the anomalous rotation curves for the orbital motion of stars in galaxies. These curves, if curated carefully, can be applied to competing theories of the nature of the dark matter taken to be responsible for the anomalous motions.

3. Experience as Process

In small-e empiricism, experience designates a physical process that connects us continuously with the system of interest. The authority of experience derives from the continuity of the connection to the system of interest. Earlier chapters have provided many illustrations of the variety of experiential processes. In their most familiar form, these processes are simple ones that terminate in human sense organs. As we saw in the last chapter, the development of scientific instruments of increasing sophistication has rendered human sense organs an inessential component of these processes. What follows is a brief reminder of just a few illustrative examples of the very many processes that can serve in this experiential role.

The most familiar form of experiential process involves is the propagation of light, or, more generally electromagnetic radiation, from the system of interest. A simple example is naked eye astronomy, such as when Tycho Brahe observed a new star ("*nova*") with his naked eye in 1572. Optical instrumentation has now greatly enhanced the range of electromagnetic processes accessible to astronomers. Galileo's introduction of his telescope was merely a first step. Twentieth century astronomy introduced devices that exploit vastly more than the narrow band of frequencies visible to human eyes. We have systems that access the radio, infrared and X-ray parts of the electromagnetic spectrum.

A second process arises through the vibrational modes of wave propagation in media. The simplest form are sound waves sensed by human ears. The August 27, 1883, volcanic eruption on the island of Krakatoa was heard over three thousand kilometers away in Perth, Australia. The sonar detection of submarines underwater employ the process of sound waves propagating in water. A still more sophisticated form of this process are the various seismic waves that propagate through the earth after an earthquake. Their detection by seismographs has enable a reconstruction of the interior structure of the earth.

A third process is sampling; that is, the physical conveyance of a small part of the system of interest elsewhere for analysis. The simplest case arises with the human sense of smell. Analytic chemists affirmed the presence of arsine in a sample undergoing chemical analysis by its garlic-like aroma. This physical conveyance of a sample to a laboratory is an essential part of routine chemical analysis, whether it be by the wet methods of the nineteenth century or by the instrumental methods of the twentieth century.

Not all chemical analysis requires this physical conveyance. A celebrated example is Norman Lockyer's discovery and identification of a new element Helium in the sun. It was based on the observation in 1868 of a new line in the emission spectrum of the solar chromosphere during a solar eclipse. The supposition that it was produced by a new element was subsequently confirmed with the discovery of Helium on the earth. In this case, the most important experiential process is that electromagnetic radiation propagating from solar Helium to Lockyer's telespectroscope.

Perhaps the most exotic of these experiential processes is the use of gravitational waves to observe calamitous gravitational events in distant space by LIGO: Laser Interferometer Gravitational Wave Observatory.

Following the discussion in an earlier chapter, human sense organs are not needed for the implementation of experiential processes. In scientific applications, human sensory organs will likely be engaged at some stage. Human eyes may read a result on an inked paper chart or in a photograph or on a computer screen. Such human sensings need not be an essential element in the continuity of the experiential process.

That human sensings are inessential is illustrated by systems which act automatically in response to the result of the experiential process without any human intervention. A familiar illustration arises with automatic control systems. In such systems, sensors monitor and control the condition of a process. In chemical plant, sensors routinely monitor temperatures, pressures, flow rates and other plant variables. These sensings are experiential processes. Their results are fed into a later stage of the control system that acts to adjust the process variable. In chemical plant, the action is usually to bring the sensed temperature, pressure or flowrate back to within some desired range. This cycle of sensing and acting proceeds automatically without a human intervening or even reading the value of the sensed variable.

These simple, negative feedback systems seem rudimentary compared with the artificial intelligence now employed in production processes. On a food production line, AI powered cameras almost instantly sense defective items and trigger a blast of air to eliminate them individually from the line; and they do so faster and more efficiently than could any human.

4. Inductive Support in Experience

The stages of an experiential process closer to the system of interest condition those farther from it by means of the continuous physical process that connects them. In an explicit analysis, these stages are described propositionally. The inductive relations of support in this analysis proceed in the opposite direction. The propositions describing these farther stages provide inductive support for the propositions describing the stages closer to the system of interest.

If the system of interest is involved in multiple experiential processes, then the propositions describing the farther stages of these processes, in aggregate, can give strong support for the propositions describing those closer to the system itself and thus also provide strong support for the reported character of the system of interest itself.

That the relations of support are inductive does not amount to an unavoidable compromising of the utility to empiricism of these processes. In empirically well-supported sciences, these relations of inductive support are strong and the results derived from the experiential processes are well-secured.

A serviceable analysis of this security requires an examination of the associated relations of inductive support. The account of inductive inference native to small-e empiricism, the material theory of induction of Norton (2021, 2024), provides an analysis especially well-adapted to these experiential processes. The key proposal of the material theory is that all inductive inferences are warranted by background facts. The strength of support of some inductive inference is determined by identifying those warranting facts and the extent to which they warrant the inductive inference. Our security in this assessment is in turn based on an assessment of the security of those warranting facts themselves.

The utility of the material theory derives from one of its important advantages over universally applicable, formal accounts of inductive inference. If we wish to assess the strength of some relation of inductive support within such a formal theory, we end up engaging with deeply intractable problems. In which precise sense, we might have to ask, does the later proposition explain the earlier one? By what criteria is it the better or the best explanation? Can we really treat all relations of inductive support as covert probabilistic relations? What is the origin of the probability measure applied? These general problems do not arise in a material account. The analysis remains local and is concerned with the particular background facts in the specific scenario that warrant the relations of inductive support.

4.1 Galileo and Mountains and Seas of the Moon

The application of the material theory of induction to these experiential processes can be illustrated with a few examples. In his 1610 *Siderius Nuncius*, Galileo reported his observations of what we now call the mountains and seas of the moon. It is easy to imagine that Galileo's telescope enabled him to see these mountains and seas, much as we might view mountains and seas through coin-operated binoculars at a popular mountain-top tourist destination. The reality of Galileo's observation is more complicated. He wrote: (1610, trans. 1989, p. 13)

> By oft-repeated observations of them we have been led to the conclusion that we certainly see the surface of the Moon to be not smooth, even, and perfectly spherical, as the great crowd of philosophers have believed about this and other heavenly bodies, but, on the contrary, to be uneven, rough, and crowded with depressions and bulges. And it is like the face of the Earth itself, which is marked here and there with chains of mountains and depths of valleys. The observations from which this is inferred are as follows.

His observation of mountains and valleys is a "*conclusion*," thus a proposition, and was "*inferred*" from more primitive observations, which he proceeded to describe as his narrative unfolded. For example, he described what he saw as the division between light and dark advanced on the moon's surface. Bright spots appeared ahead of the advancing division, grew in size and eventually merged with it. This, he noted, corresponds to the way that mountains on earth are illuminated at sunrise.

The propositions describing the various stages of growing brightness of the spots provide inductive support for the proposition that each is a mountain top gaining successively greater illumination; and the collection of these propositions provide inductive support for the general proposition that the moon's surface is covered in such mountains. This is an analogical inference, as suggested by Galileo's remark above: "… like the face of the Earth itself suggests…" Its inductive character has been described in Norton (2021, Ch.4, Section 4.8). The warranting fact is that the processes producing shadows are the same on the Earth and the Moon.

4.2 The 1883 Krakatoa Volcanic Eruption

A second example is the sound of the volcanic eruption at Krakatoa in August 1883. It was heard around the world. A typical propositional report is found in the 1911 *Encyclopaedia Britannica* (Anon, 1911):

> The actual sounds of the volcanic explosions were heard over a vast area, especially towards the west. Thus they were noticed at Rodriguez, nearly 3000 English miles away, at Bangkok (1413 m.), in the Philippine Islands (about 1450 m.), in Ceylon (2058 m.) and in West and South Australia (from 1300 to 2250 m.).

Once again it is easy to imagine that this is some sort of primitive report. People at the locations indicated heard the bang. Prior to giving the issue any thought, I naively imagined that they would say something like "Uh oh—there goes Krakatoa!" much as I might instantly associate the sound of a delayed bang with the sight of exploding fireworks overhead.

The reality is far different. It required much careful research to connect securely reports of sounds heard around with world with the Krakatoa eruption. The massive effort of data collection and analysis is summarized tersely in a densely detailed, 32 page report with additional figures by Strachey (1888). After collecting reports from around the world, Strachey characterized the atmospheric disturbance caused by the eruption as: (p. 63)

> (p. 63) …the passage over them [observing stations] of an atmospheric wave or oscillation, propagated over the surface of the globe from Krakatoa as a centre, and thence expanding in a circular form, till it became a great circle at a distance of 180°from its origin; after which it advanced, gradually contracting again, to a node at the antipodes of Krakatoa; whence it was reflected or reproduced, travelling backwards again to Krakatoa, from which it once more returned in its original direction; …

The speed with which the sound of Krakatoa's eruption propagated could also be determined from the reports. It varied, but had a most probable speed of 713 miles per hour (p. 68). Since the sites at which the sounds were heard were as much as thousands of miles from the source, considerable effort was needed to correlate the timing of a report of a sound with the time at which the sound of the eruption would be expected at that site.

Table IX of Strachey's report spans nine pages and includes reports of sounds heard. Many have verbatim observers' quotes. They include reports from nearby sites, such as Batavia, in Java (Indonesia) which is merely 94 miles from Krakatoa:

> "On 26th, about 4 p.m., a series of detonations was heard; towards night they grew louder; till in the early morning the reports and concussions were simply deafening." Report by Lloyd's agent in Batavia.

The most distant report is from Rodrigues, Chagos Islands (now Mauritius), 2968 miles distant from Krakatoa:

> "Several times during the night of the 26th-27th reports were heard coming from the eastward, like the distant roars of heavy guns. These reports continued at intervals of between three and four hours, until 3 p.m. on the 27th (= 5.48 p.m. local time at Krakatoa), and the last two were heard in the direction of Oyster Bay and Port Mathurie." Report by Mr. James Wallis, Chief of Police.

The table also included reports from ships at sea.

The reports are aggregated to provide the synoptic account of the spread of the audible sounds of the eruption around the globe. An inductive inference proceeds from the reports, such as those quoted above, to the conclusion that each report is of the sound of the Krakatoa eruption; and then to the general conclusion that the sound was heard around the globe. The warranting fact is the summary of how the atmospheric disturbance produced by the eruption propagated around the globe. The inference is inductive since there is a chance of error. In the reports from the more distant locations, there is always a chance of noises from other sources arising coincidentally at the appropriate time. A further background fact, present only tacitly, is that it quite unlikely that confounding noises would arise at that time. In each case, the chance of spurious identification is quite small. There are 94 reports overall, so the chance that one of them is spurious might not be so small.

4.3 Sampling and the Chain of Custody

A third example arises in cases of sampling, in which a portion of the system of interest is physically transported for analysis. The goal is to provide samples that faithfully represent the properties of concern in the system of interest. That is, propositions describing the condition of the samples must provide strong inductive support for a proposition describing the condition of the system of interest from which the sample was drawn.

The simplest and most familiar condition for strong inductive support by the sample of the condition of the system of interest is that the sample was drawn so that it is suitably representative of the larger system. Sampling theory is a well-developed branch of statistics and guides researchers in how large a sample is needed if it is to represent a larger, homogeneous population.

The problem of assessing this faithfulness of a sample extends well beyond what theorems in statistics can supply. For these theorems can only be employed if their antecedent conditions hold for the system of interest. Consider the many "authentic moon rocks" that can be purchased from vendors on the internet. Prudence is advisable for someone interested in using them to explore lunar geology. Lunar rock samples taken by NASA's Apollo missions are not available on the open market; and lunar meteorites are rare.

How to sample faithfully is a serious concern in exploration and mining geology. Abzalov (2011) lists three sources of error in the sampling process that can arise in successive stages of sampling. The first is that the sample is not large enough in relation to the heterogeneity of the material sampled. This first source of error corresponds to the traditional sampling error of statistics. The second source of error arises from the wide range of failures possible in the implementation of the sampling protocol itself. They include problems such as the contamination of the samples, the inadvertent mixing of samples and errors in preparing and weighing samples. The third source of error comes last in the process and arises from failures or misuse of the analytic instruments used to produce the final assay.

That none of these errors has compromised the final assay is determined by a cascade of inductive inferences that tracks the successful avoidance of all these errors through the stages of the sampling process. It can then be concluded the propositions describing the sample provide strong inductive support for propositions describing the system from which the sample was drawn.

An explicit example of the propositional representation of stages of a process of sampling is provided by the concept of chain of custody in forensic science. After forensic evidence has been collected, typically from a crime scene, the sample will be transported to a laboratory for analysis or to be presented in court. The admissibility in court of the analytic results requires an assurance that the sample drawn and the one analyzed or presented are the same. The procedures associated with the chain of custody are designed to provide that assurance.

The Technical Working Group on Biological Evidence Preservation, working under the aegis of the US Department of Commerce and the National Institute of Standards and Technology prepared a handbook of best practices for the handling of biological evidence (Ballou et al., 2013). The types of samples addressed included hair, tissue, bones, teeth, blood, semen, or other bodily fluids and anything containing DNA. Since such samples are subject to degradation, an early section of the handbook contains careful guidelines on sample preservation. Evidence that these guidelines, or something equivalent, has been follows provide inductive support for the proposition that the sample has not degraded.

The requirements of the chain of custody pertain to the transport and storage of the sample. The Working Group provides this definition: (p.v)

> An accurate chain of custody identifies and tracks the evidence from the time it was collected—including the method by which it was obtained—through final disposition for each individual who had possession and responsibility.

To secure an accurate chain of custody, the Working Group recommends that the following documentation be maintained for the various stages of the transport of the samples: (p. 25)

> Chain-of-custody documentation should include the following:
> • description of the evidence unique case identifier (e.g., case number)
> • where the evidence was collected
> • where the evidence was stored
> • who was in possession of the evidence and for what purpose
> • what was done to the evidence (e.g., analysis or re-packaging)
> • date and time information

When samples are moved, such as to the courthouse (p. 33), the Working Group provided similar recommendations on documentation.

The documentation recommended provides part of the propositional description of the various stages of experiential process that is the chain of custody. Through their presence, propositions describing later stages of chain of custody provides strong inductive support for the propositions describing the condition of the sample in stages of the chain closer to the system sampled.

5. The Distinction between Theory and Observation Dissolved

It is traditional in empiricist writing to distinguish observation from theory. Observations, whether carried out passively or in the context of an experiment, provide the foundation in experience for science. The further results of science are designated as theoretical and may even be accorded a lesser status. They may be merely a means to establish correlations among experiences and possible experiences.

This division has been endlessly troublesome for empiricism, as will be discussed in greater detail in Chapter 11. The division does not arise in small-e empiricism. The view admits only a relative notion: some stages and propositions that describe them are closer to the system of interest and some are farther from it.

In practice, the propositional representation of any experiential process will have a propositionally described, terminal stage that is farthest from the system of interest among those described propositionally. That terminal stage has been selected for convenience only. It does not represent some pristine distillation of pure experience. This is easily seen since it is generally possible to regress to stages still farther from the system of interest, if there is a need to do so and if enough of the details of the process has been preserved to enable it.

Newton's treatment of the phenomena in his *Principia* discussed in Chapter 8, provides an illustration of this regress. The first phenomenon included the assertion that the moons of Jupiter orbit conform with what we now call "Kepler's third law." The radius R of their orbits, assumed circular, vary with their periods T according to $R^3 \propto T^2$. This relation encodes already within it an inverse square dependency on distance of the centripetal accelation of the moons toward Jupiter.[114] Because of the rapidly of this recovery of the inverse square dependency, most later reporting simply takes this as the observational datum. The focus is on what follows from it. There is little sense that an understanding of the empirical foundations of Newton's system requires an exploration of the astronomical observations that support this first phenomenon.

That we halt at this stage of the experiential process is a matter of convenience. We could, if we are so inclined, trace how this first phenomenon is itself derived from more specific

[114] The centripetal acceleration A is V^2/R, for moon speed $V=(2\pi R)/T$. Combined with the phenomenon that $R^3 \propto T^2$, we have $A=V^2/R=(2\pi R)^2/T^2 \cdot (1/R) \propto (R^2/R^3) \cdot (1/R)=1/R^2$.

observations. As we saw in Chapter 8, Figure 1, Newton did provide these more specific observations. He listed specific measurements of orbital radii and periods for the four known moons of Jupiter and associated them with different astronomers by name. This earlier stage provides the inductive basis of Newton's first phenomenon. The inference from it to the phenomenon is inductive. We have to suppose that the relations between the radii and period found in the past will persist into the future.

The analysis of stages farther from the system of interest need not halt here. We could regress further and seek the basis for this last stage in modern astronomical measurements of the motions of Jupiter's moons. A more historical investigation, free from entanglements with results found after Newton's time, would pursue the details of how these measurements were made in Newton's time. Then the radii were determined by telescopic micrometer readings and the periods from the times of lunar eclipses. Newton's sources included, notably, John Flamsteed, the first Astronomer Royal. In the first edition of *Principia*, in the discussion of this first phenomenon, Newton (1687, p. 402) thanks Flamsteed specifically for communicating results to him. This acknowledgment is, curiously, omitted in the corresponding place in the third edition (1687, pp. 390-91). However Newton retained the acknowledgment to Flamsteed in his posthumously published *System of the World* (1728, p.7; 1731, p. 13).

Tracing back to these source measurements goes beyond what is practical here. One issue in such an analysis would be of special interest to empiricism. Just how are the inductive inferences made from the specific measurements to the relation between lunar orbit radii and period? An attempt to answer might well be aided significantly by a step-by-step recipe for determining these parameters for Jupiter's moons provided by Flamsteed (1685).

This example illustrates how the selection of a terminus in the experiential process is made. The selection balances two factor. First, we need to regress for enough to satisfy the primary goal: that the propositional description of the terminus captures a clear reflection of the system of interest without untoward encumbrance of the deeper theory of the system of interest. Second, we terminate the regress when further regression would encumber the representation with details that would not advance the primary goal.

6. The Corrigibility of Experience: Winding Back

Although experience is accorded the ultimate authority in small-e empiricism, no particular experiential report is unchallengeable. Each is corrigible and that corrigibility sustains the authority of experiential reports taken collectively. The simplest vehicle for correction lies in the possibility of multiple experiential processes that engage with the same system of interest. If they agree, the following Chapter 11 illustrates how they support each other.

Another means of correction arises from the nature of experience as a process. It is provided by the possibility, described above, of regressing to stages still farther from the system of interest. I will call this process "winding back."[115] This sort of winding back is quite common in the practice of science as a mode of correction. In principle, such winding back is always possible. In practice, it might not be so if suitable records have not been kept.

This process of winding back allows a check of the chain of inductive inferences that proceed from the terminus farthest from the system of interest to the proposition that describes the system of interest directly. It enables an identification of more general assumptions that may have entered the analysis improperly through these inductive inferences. Such assumptions constitute the supposedly perilous "theory ladenness of observation," when it is used to challenge the authority of evidence.[116] Their identification enables us to remove or replace these assumptions. The analysis is enhanced if it is carried out using the material theory of induction. For that theory enjoins us to identify the material facts used to warrant the inductive inference and to check the further inductive support of these warranting facts. The resulting examination of propositions drawn from the wider context encourages an integration of the relations of inductive support in the experiential process with the other relations of support in the science more broadly.

The next two sections describe two cases in which significant errors were made and corrected by winding back. Enrico Fermi, in 1934, mistakenly announced that he had prepared the first known transuranic element. Edwin Hubble, in 1929, gave an estimate of the rate of the expansion of the galaxies that was almost an order of magnitude too large. In both cases, there were sufficient details of the observational processes and associated analyses for subsequent

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[115] I believe I learned the metaphor of "winding back" from Nora Boyd. It appears in Boyd (2018b, pp. 57-58) through her report on the "Astronomy Rewind" citizen science project. It recalls an earlier era in technology in which audio and video were recorded on long tapes. They were rewound to go back to the earlier parts of the recording.

[116] See Chapter 11 for more discussion of why this challenge fails.

analysts to wind back and correct the errors. The section following recounts Michelson's 1920s measurement of the speed of light. It is an example of an error whose source can only be partially corrected due to the insufficiency, but not total absence, of the requisite measurements and documentation. The result is still positive in the sense that we know that the measurement was troubled, why it was troubled and that it is not to be relied upon. The section also recounts concerns over the identification of "Unidentified Anomalous Phenomena," once called "unidentified flying objects"—"UFOs." It is a case in which there is insufficient details of the experiential processes to wind back reliably. That means that the results cannot be used to sustain significant conclusions.[117]

7. Winding Back: Enrico Fermi and the Misidentification of Element 93

7.1 Element 93

On May 19, 1934, the prestigious journal, *Nature*, published a letter from the Italian researcher in Rome, Enrico Fermi (1934a). It reported the results of experimental work with his collaborators, O. D'Agostino, E. Amaldi and E. Segrè. They had systematically exposed ("bombarded," to use Fermi's term) a long list of chemical elements to the recently discovered neutrons. They found that the exposure induced radioactivity in the elements and their transformation to other elements. Theirs was a preliminary report, which concluded "The experiments are being continued in order to verify these results and to extend the research to other elements."

The research continued as promised and within a month Fermi could report something extraordinary. Uranium was then the known element with the highest atomic number in the periodic table of the elements. In a June 1934 paper in *Nature*, Fermi reported that they might have produced experimentally an element higher in the table. It would be what was soon called a "transuranic" element. The paper's title indicated Fermi's hesitation: "Possible Production of Elements of Atomic Number Higher than 92." The transuranic element produced, if they had

[117] Two further examples of winding back that will not be pursued here are these: The late 1960s discovery of "polywater" turned out merely to be contaminated water. The 2011 discovery by the OPERA collaborative of faster-than-light neutrinos turned out to rest on instrumental error, including a bad cable connection.

indeed done so, was most likely atomic number 93, but Fermi could not rule out atomic numbers 94 and 95.

Fermi's announcement was picked up by the popular press, where the announcement was sensationalized. The *New York Times* exulted in its June 6, 1934, issue: (p. 20)

> … the announcement in Rome that Professor ENRICO FERMI has succeeded in creating an artificial radioactive element which must be given the number 93 is of the highest importance. It shows that the theorists who have been turning the universe inside out within the last twenty years and who have been creating new styles of atoms every few months are better prophets than they may have realized themselves.

The *Science News Letter* in June, 1934, reported twice with similar enthusiasm (Anon, 1934; Langer, 1934). The first came with the headline:

> Italian Discovery May Be First of Super-Elements: No. 93, Reported by Fermi, May Be Unstable Element Thought Impossible Only a Few Years Ago

The press had decided that Fermi's announcement was an event of great significance in science.

Fermi's announcement was also embraced by the scientific establishment. Otto Hahn, of the prestigious Kaiser-Wilhelm-Institut fuer Chemie in Berlin-Dahlem, along with his collaborators, Lise Meitner and Fritz Strassmann, worked intensively to develop Fermi's claims. Some of their efforts were reported in Hahn, Meitner and Strassmann (1936). This initially promising research program proved recalcitrant and eventually collapsed.[118] It was only later, in 1939, that the core difficulty was discovered. Fermi, Hahn and his collaborators had not included the process of nuclear fission in their analysis. Under neutron bombardment, Uranium atoms split into atoms of atomic numbers smaller than considered by Fermi. Their neglect undermined the research program.

7.2 Fermi's Analysis

The result might be regarded as a striking failure of nuclear chemistry; or, more plausibly, the laborious preparation needed for the discovery of nuclear fission, as announced by Meitner and Frisch (1939). How secure was Fermi's original experimental result? The answer is found by winding back through the inferences supporting the final result to the descriptions of

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[118] Otto Hahn's biography (1966, pp. 139-44) provides a synopsis of the collapse of the program.

the results describing stages more remote from the radiating samples and assessing the strength of the inductive support provided by them for the final result. There is some scope for this winding back since Fermi described the experimental protocol he and his collaborators, F. Rasetti and O. D'Agostino, used and their subsequent analysis.

The foundation of Fermi's analysis was that, under neutron bombardment, Uranium yielded new elements that undergo β-decay, that is decay which produces electrons. Each element undergoing β-decay does so with a distinctive half-life. In principle, Fermi could determine which elements were present merely by tracking the decline in β-decay activity, which he measured with a Geiger counter. The determination was complicated by the presence of several elements undergoing β-decay with different half-lives. He determined that there were elements produced with half-lives of 10 seconds, 40 seconds, 13 minutes and several more with periods from 40 minutes to one day.

Fermi's attention narrowed to the element with a β-decay half-life of 13 minutes. He carried out series of chemical separations that enabled him to rule out the possibility that this element was a known element with an atomic number close to that of Uranium at 92, or an isotope of Uranium. He summarized these results as: (Fermi, 1934b, p. 899)

> In this way it appears that we have excluded the possibility that the 13 min.-activity is due to isotopes of uranium (92), palladium (91), thorium (90), actinium (89), radium (88), bismuth (83), lead (82). Its behavior excludes also ekacaesium (87) and emanation (86).

The notable conclusion followed:

> This negative evidence about the identity of the 13 min.-activity from a large number of heavy elements suggests the possibility that the atomic number of the element may be greater than 92.

He followed this conclusion with some chemical reasons for why the new element might have atomic number 93, although he felt that 94 and 95 could not be excluded.

7.3 Winding Back

For our purposes, the key inductive inference is from the identification of the half-life of the new element as 13 minutes to the conclusion that it must be of an element with atomic number greater than that of Uranium. Fermi provided the warranting facts for this inference in his paper. First, there is a mechanism associated with neutron bombardment that would produce at element with atomic number 93. In preliminary remarks, he recalled that: (p.898)

> …absorption of the bombarding neutron produces an excess in the number of neutrons present inside the nucleus; a stable state is therefore reached generally through transformation of a neutron into a proton, which is connected to the emission of a β-particle.

If a Uranium nucleus increases its proton count by one, as this process would indicate, then its atomic number increases from 92 to 93. More generally, after a short sketch of past results, Fermi listed the nuclear transformations he judged possible for nuclei under neutron bombardment: (pp. 898-99)

> This evidence seems to show that three main processes are possible: (a) capture of a neutron with instantaneous emission of an α-particle; (b) capture of the neutron with emission of a proton; (c) capture of the neutron with emission of a γ-quantum, to get rid of the surplus energy.

The key feature of all the processes is that they change the atomic number of the bombarded nuclei by only one or two numbers; and that number might be increased a little if the products underwent further transformations. These propositions warrant the inference that the Uranium nuclei can only be transformed into nuclei of elements with atomic numbers close to Uranium. Thus, if Fermi ruled out elements with atomic numbers less than or equal to Uranium's 92, all that is left are elements with an atomic number greater than 92.

This reasoning would certainly have looked secure to Fermi in 1934 and to other nuclear chemists. Hahn, Meitner and Strassmann (1936, p. 905) repeated Fermi's list of the three possible processes. It is only with well-informed hindsight that their analyses can be faulted for not including the process of nuclear fission. It would allow much larger changes in atomic number among the decay products and would require checking of a larger range of lower atomic number elements in the search for the element with the 13 minute β-decay half-life.

In an interesting footnote to the history of the discovery of nuclear fission, Ida Noddack (1934), a chemist at the Physikalische Technische Reichsanstalt, in Berlin, published a note critical of Fermi's 1934 conclusion concerning a transuranic element. The criticism itself was thin. Its main complaint was that Fermi's analysis had not been thorough enough, even though Fermi's hesitant announcement already conceded this. The complaint would likely have been completely forgotten excepting for one remark, made in passing at the end of a paragraph. It was intended to speculate on a way that lower atomic number products might be produced:

> It would be conceivable that, in the bombardment of heavier nuclei, these nuclei decay into several large fragments that are still isotopes of known elements, but are not neighbors of the irradiated elements.

As she remarked in her later response, Noddack (1939) was exasperated that the subsequent recognition of the neglect of fission by Hahn and his collaborators did not even include a citation to her suggestion.

Whereas a citation might have been appropriate, not much more was warranted. Noddack's suggestion was just a speculation without any experimental or theoretical foundation. In contrast, both foundations were given in Lise Meitner and Otto Frisch's (1939) announcement of nuclear fusion. This assessment seems to be that of Hahn as well. An editorial note to Noddack (1939) in *Naturwissenschaften* reported Hahn and Strassmann's reaction as asserting that the idea of such fission "… has already been discussed by many others, without any experimental consequences being drawn from it."

In sum, Fermi's tentative proposal erred. Since he provided sufficient records of the various stages of his experimental process and the assumptions used to guide inferences among them, it was possible then and now to "wind back" and identify where the analysis erred. His experimental result was corrigible and was corrected.

8. Winding Back: Hubble and the Expansion of the Nebulae

8.1 Hubble's Analysis

A major event in the development of modern cosmology was Hubble's (1929a) determination that the galaxies—he called them "extra-galactic nebulae"—are receding from us with a speed proportional to their distance. The result itself seems to be one of the simplest in observational astronomy. Hubble merely needed to match the distance to each galaxy with its velocity of recession. The reality was quite different. Hubble's result was a hard-won synthesis of many individual observations. The velocities of recession of the galaxies were determined with fewer complications from a measurement of the red shifts in the light they emitted. Even this simplest of observations was complicated by our solar system's motion within our galaxy, the Milky Way. This motion had to be found by a regression analysis and subtracted from the apparent velocities of recession of the galaxies.

The greater challenge in Hubble's 1929 analysis was the determination of the distances to the galaxies. He had determinations of the velocities of recession of 46 galaxies, but he had distance determinations for only 24 of them. To include all 46 galaxies in his 1929 analysis, Hubble needed to resort to a quite complex network of interrelated inferences, whose structure has been reviewed in more detail in Norton (2024, Ch.7).

The determination of distances to the galaxies remained an enduring problem for the next few decades. It turned out that Hubble's distance estimates were in error and underestimated distances to the galaxies by a factor of up to 6 or 7. Hubble's own analyses over the subsequent decade or so failed to detect the error. It was only in the 1950s that the corrections were found to be needed and were made.

8.2 Hubble's Distance Determinations Cepheid Variable Stars

In his 1929 paper, Hubble employed several means of determining the distances to celestial objects. The simplest required knowledge of the absolute brightness of a star or galaxy. The luminosity of a light source diminishes with the square of distance. If we are twice as far from the source, its apparent luminosity diminishes by a factor of four. The difficulty in using this method was the need to determine the elusive absolute luminosity of distant galaxies.

The solution came from a discovery by Henrietta Leavitt (1912). Certain stars that came to be known as Cepheid variable stars vary in absolute brightness with a period of several days. Leavitt found that there was a definite relationship between this period and the absolute luminosity. If one of these Cepheid variable stars could be found in a distant galaxy, then its absolute luminosity could be inferred from its period and a distance to the galaxy could be determined.

This is precisely what Hubble had done in earlier work. In his Hubble (1925), he had identified Cepheid variable stars in two nebulae Messier 31 (Andromeda) and Messier 33. From their periods, he determined that both were 285,000 parsecs distant, which corresponded to a distance of 930,000 light years. This great distance put these two nebulae outside our Milky Way galaxy and settled the Great Debate over whether the bulk of all cosmic matter resided in our Milky way alone. These nebulae were other galaxies. A careful reassessment, published in

Hubble (1929b, p. 103) made the minor adjustment of the distance to Andromeda of 275,000 parsecs.

With the distance to these galaxies now established, Hubble could use this known distance to calibrate other measures of distances that could be used with other galaxies. For example, he could determine the absolute luminosity of the brightest stars in Andromeda from their apparent luminosity and their Cepheid-determined distance. He noted in Hubble (1929b, p. 103):[119] "Preliminary surveys suggest that few stars in M 31 are brighter than [absolute magnitude] *M*= –6 (distance, that indicated by the Cepheids) and that large numbers are to be found only for *M* fainter than –5." He then assumed that the brightest stars in all galaxies had the same absolute luminosity. If he could resolve these stars in other galaxies, he could then determine the distance to them.

Overall, Hubble's use of the period-luminosity relation for Cepheid variables was the core of his determination of the distances to the galaxies and to his determination of the linear relationship between their velocities of recession and their distances from us. In his 1929 paper, Hubble settled on a constant ratio of velocity to distance of 500 km/sec/megaparsec. Hubble remained confident that this was roughly the correct value for over a decade. A careful reassessment in Hubble and Humason (1931, p. 76) doubled the number of galaxies considered and increased the constant to 560 km/sec/megaparsec. In his popular *Realm of the Nebulae*, Hubble (1936, p. 170) gave a similar value of 530 km/sec/megaparsec, which he stated in other units as 101 miles/sec/million light years. This figure of reappeared as "about 100 miles per second per million light years of distance" in Hubble's (1942, p.104) more popular survey of what he called the "expanding universe."

This value—now known as the Hubble constant—was far too high. A modern value is roughly 70 km/sec/megaparsec. Within less than two decades of Hubble's original 1929 estimate, values in this lower range came to be accepted.

8.3 Winding Back

What went wrong with Hubble's determinations? They all depended on using Cepheid variable stars to determine the distances to nearby galaxies such as Andromeda; and then to use those distances to calibrate the other means used by Hubble to determine distances to the

[119] "*M*= –6." The more negative the magnitude, the brighter the star.

remaining galaxies. Its basis was an inference from the apparent luminosity and period of Cepheid variable stars in nearby galaxies to their absolute luminosity. That inference was warranted by the relationship supposed between the absolute luminosity and the period of Cepheid variables. Hubble, it soon turned out, had used the wrong relationship. He was warranting his inference with a falsehood.

That Hubble had erred emerged from the work of Walter Baade. In his Baade (1944), he reported that there are actually *two* types of Cepheid variable stars, called type I and type II, differing in the colors of their spectrum. It became apparent, as detailed in Baade (1956), that the two types of Cepheid variable stars are governed by different period-luminosity relationships. The relationships differed only by a constant factor. Baade (1956, p. 12) reported that, for Cepheids of same period, a Type II Cepheid is brighter than a Type I Cepheid by a magnitude difference of 1.5. The magnitude scale is a logarithmic function of the luminosity, which is the rate at which luminous energy is emitted by the object. The difference Baade reported is a constant, arithmetic difference. An arithmetic difference in magnitude of 1.5 corresponds to a multiplicative difference in luminosity of 4.[120]

The original determination of the period-luminosity relationship had been made using observations of Cepheid variable stars in the nearby Magellenic clouds. These are Type I Cepheids. The Cepheids in Andromeda, however, are the brighter Type II Cepheids, which are four times brighter in luminosity. Since apparent luminosity diminishes with the inverse square of distance, that meant that the Cepheid variable stars of the Andromeda galaxy are twice as distant as Hubble had long presumed. The overall effect was that Hubble's calibration of all distances to the galaxies was incorrect. All his distances needed to be doubled. Hubble (1953, p.662) freely acknowledge the correction needed:

> The results clearly indicated that the classical Cepheids in M 3I are nearly $1^{m}.5$ brighter than previously supposed, and, consequently, all our current estimates of absolute distances must be nearly doubled.

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[120] Magnitude m and luminosity L are related by $m_2 - m_1 = -2.5 \log_{10} (L_2/L_1)$. Recalling that negative magnitudes are brighter, a magnitude difference $m_2 - m_1 = -1.5$, corresponds to a luminosity ratio $L_2/L_1 = 10^{(1.5/2.5)} = 3.981$.

That the correction required a simple doubling of all distances was fortuitous. It meant that the linearity of the relationship between the velocity of recession and the distance could be preserved. All that changed was the constant relating them.

The correction was reported in an elaborate reanalysis in Humason, Mayall, Sandage (1956). The increase in distance estimates along with other corrections led to a reduction in their estimate of the Hubble constant (p. 97, p.160) by a factor of roughly 3 to 180 km/sec/megaparsec. In reporting the error in Hubble's calibration of distances, Humason, Mayall, Sandage (1956, p. 159, their square brackets "[log *cz*, *m*]") describe how the correction is to be effected by the procedure here called "winding back":[121]

> Hubble's calibration of 1936 was obtained from the [log *cz*, *m*] relation for the brightest resolved objects in a sample of nearby resolved nebulae. These objects were identified at that time as bright supergiant stars. The absolute magnitudes of those objects were assumed to be known from previous calibration of blue supergiants in M31 and in M33 with respect to the cepheid variables. The zero-point of the period-luminosity law for the cepheids was assumed known from the statistical parallax calibration first by Hertzsprung (1913) and later by Shapley (1918) and by R. E. Wilson (1923, 1939). Evidence accumulated in the past five years has shown the need to examine anew each step of this procedure.

Still further corrections were needed. They were collected and reported in Sandage (1958). Hubble had misidentified as the brightest stars in some distant galaxies what were really "HII" regions, which contain stars and luminous, ionized hydrogen (hence "H"). The overall effect was a further reduction in the estimate of the Hubble constant to approximately 75 km/sec/megaparsec (p. 513), which is close to the modern value.

9. Problems Winding Back

The last two sections have reviewed cases in which winding back succeeded in correcting empirical errors. The winding back was possible because enough of the details of the earlier empirical analysis were known and accessible to further investigation. That is how things are when they go well. Winding back is dependent on later analysts having sufficient access to the experiential processes and inferences made about them. In many cases, this access is not

[121] [log *cz*, *m*] z = redshift. m = apparent magnitude.

available. If there are any proper concerns about the empirical result claimed, the outcome is that the result is properly taken to be tentative at best. The first example of such a tentative outcome is provided by Michelson's troubled measurement of the speed of light in the 1920s. The second example is that many reports of "Unidentified Anomalous Phenomena" remain undecided as individual reports in cases in which no winding back was possible.

In cases in which winding back is not possible, evidential scrutiny may still be possible. If there are other experiential processes that connect with the same system of interest, agreement in their results may provide support for them, as will be discussed in more detail in the following chapter.

9.1 Michelson and the Speed of Light

A rather striking example of the need to wind back is provided by Albert A. Michelson and collaborators' experimental measurements of the speed of light in California in the 1920s.[122] The method is, in concept, simple. All we need do is time how long light takes to traverse a known distance. We just need two numbers: a time and a distance. One might imagine that the need for extremely short time measurements would make the time determination most troublesome. As it happened, however, it was measurements of the distance that, unexpectedly, left the most open problems. Michelson's measurements of 1924 to 1926 were undertaken by timing light pulses reflected between Mount Wilson and nearby Mount San Antonio in California. Michelson (1924, p. 256) reported that the distance between the peaks was measured by the United States Coast and Geodetic Survey and was accurate to within one part in two million. Given the distance reported in Michelson (1927, p.3) of 35.4251km, this amounted to an error of merely 1.77cm = 0.70in.

Michelson and his collaborators, Francis G. Pease and F. Pearson (1935, pp. 26-27), were soon concerned that the method of triangulation used for this distance measure might have introduced more errors. In an effort to increase accuracy, they initiated a series of measurements

[122] I learned of this curious example from Allan Franklin.

in 1929 to 1933[123] of light propagating in a mile-long, evacuated[124] tube at the Irvine Ranch, near Santa Ana, also in California. Its length could be measured directly.

Michelson, Pease and Pearson (1935, pp. 56-59) reported a curious anomaly. Their velocity measurements over a two-year period displayed cyclic variations that correlated with tidal cycles in nearby coastal regions, if the latter were displaced 10 hours forward in time. They conjectured the possibility of lunar tidal forces affecting both the distance measures ("earth expansions," they said) and the period of the timing pendulum used.

These results raised questions about the earlier Mount Wilson measurements. Birge (1941, p. 93) reported the possibility of determining whether these earlier measurement displayed similar cyclic disturbances. It could be done since Michelson's notebooks had been preserved:

> A subsequent examination of Michelson's original notebooks, I am told, has shown that similar variations were present in his work, although he had failed to notice them at the time.

More strikingly, Michelson, Pease and Pearson (1935, p. 44) noted that seismic activity had altered their distance measurements:

> An earthquake which occurred on March 10, 1933, may explain the reduced value of the base line measured by Pease in July, 1933.

Birge (1941, p. 43) used this remark to alert his readers to the possibility of even more serious seismic disturbances in the Mount Wilson distance measurements:

> As the late D. C. Miller has noted, between the time that Michelson's 35-km. base line was measured and his actual measurements on *c* [speed of light] were made, there occurred the disastrous Santa Barbara earthquake, and his actual base line may well have suddenly changed by several feet!

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[123] Michelson died in 1931. The new series of measurements were completed by his collaborators.

[124] The advantage of an evacuated tube was that it eliminated a concern for another possible source of errors: measurements of the speed of light in air require corrections for the refractive index of air. Birge (1941, pp. 93-94) soon criticized the methods of correcting for this refractive index in the Mount Wilson measurements.

The error conjectured of several feet far exceeds Michelson's earlier estimate of the possible error of 1.77cm = 0.70in.

In sum, there were sufficient indications that the distance measurement used to interpret the original experiments of 1924 to 1926 was erroneous. However not enough measurements had been made to enable a winding back to correct the experimental results reported. The key inference was from the measurement of the distance by the United States Coast and Geodetic Survey to the conclusion that this was the true distance traversed by the light signals (to within one part in two million). That inference was warranted by the presumption of the geological stability of the terrain between the two peaks. That presumption, it turned out, might not be factual. The inference had lost its warrant.

9.2 Unidentified Anomalous Phenomena

Reports of mysterious objects flying in the sky have a long history and provide many instances of dubious observational reports for which "winding back" is precluded. These reports are common in ancient religious texts and mythologies. The biblical second book of Kings reports a startled Elisha watching Elijah swept up to heaven by a fiery chariot. A major stimulus to the modern tradition came in 1947. A pilot of a small airplane in Washington State on June 24, 1947, saw anomalous objects in the sky. They were soon reported by a sensationalizing press with the description "flying saucer." For example, the page two headline in the *Chicago Sun* on June 25, 1947, read "Supersonic Flying Saucers Sighted by Idaho Pilot."

Reports of similar sightings of "unidentified flying objects" ("UFOs") continued in the decades that followed to the present. UFOlogists insisted that these sightings are evidence of visitations to Earth by extraterrestrial beings and demanded government action. Repeated reports commissioned by US government agencies have failed to affirm positively anything other than mundane sources for the sightings. Demands for repeated investigations persist. The 2024 US Department of Defense's (AARO, 2024) *Report on the Historical Record of U.S. Government Involvement with Unidentified Anomalous Phenomena (UAP)* listed 22 US investigations and 6 by other governments.

The large volume of sightings and the incompleteness of their reporting make winding back difficult or impossible in many cases. For example, here is an intriguing example among many from the US Airforce commissioned "Condon Report" (Condon, 1968, Vol. 1, pp. 189-92). It is a report of a rapidly moving object, looking like "a brilliant white light bulb," by a pilot and co-pilot of a Viscount turbo-prop airliner, near Mobile, Alabama, on the night of November 14, 1956.

> The object began maneuvering "darting hither and yon, rising and falling in undulating flight, making sharper turns than any known aircraft, sometimes changing direction 90° in an instant—the color remained constant, —and the object did not grow or lessen in size." After a "half minute or so" of this maneuvering, the object suddenly became motionless again. Again, the object "began another series of crazy gyrations, lazy eights, square chandelles, all the while weaving through the air with a sort of rhythmic, undulating cadence." Following this last exhibition, the object "shot out over the Gulf of Mexico, rising at the most breath-taking angle and at such a fantastic speed that it diminished rapidly to a pinpoint and was swallowed up in the night."

The Condon report was unable to explain the sighting using their usual repertoire. It was not Venus or some other bright celestial object diffracted as a mirage. The report concluded of it and a second case (p. 192): "In summary, these two cases must be considered as unknowns."

The Executive Summary of the AARO (2024, p. 7) report affirms what is here called the failure of "winding back":

> Although many UAP reports remain unsolved or unidentified, AARO assesses that if more and better quality data were available, most of these cases also could be identified and resolved as ordinary objects or phenomena. Sensors and visual observations are imperfect; the vast majority of cases lack actionable data or the data available is limited or of poor quality.

If each such report is taken in isolation, the prudent assessment would just be to reserve judgment. However, they should not be taken in isolation and more can be said. The first major conclusion of the AARO (2024, p. 7) report, in their boldface italics, is:

> ***AARO found no evidence that any USG investigation, academic-sponsored research, or official review panel has confirmed that any sighting of a UAP represented extraterrestrial technology.***

These supposed extraterrestrials are imperfect at hiding. They are, supposedly, routinely seen and, supposedly, occasionally crash, leaving debris. With such fallibility supposed, the repeated failure to find any unequivocal evidence of extraterrestrial technology is telling. It makes the alternative explanation much more likely: all these reports are mistaken.[125] That seems to be what the Executive Summary also concludes (p. 7):

> The vast majority of reports almost certainly are the result of misidentification and a direct consequence of the lack of domain awareness; there is a direct correlation between the amount and quality of available information on a case with the ability to conclusively resolve it.

10. Conclusion

The authority of experience does not reside in any special powers of human sense organs or human cognition; or in some elusive but evidentially potent notion of pure experience. Rather it derives from a physical process that connects us continuously with the system of interest. The goal of this chapter has been to articulate the character of these experiential processes and to illustrate them with examples from the history of science. It argues that the important corrigibility of these processes is provided by the analytic operation of winding back through the relations of inductive support in the experiential process. The operation checks that propositions describing stages closer to the system of interest are inductively well supported by those farther from the system of interest. We shall see in the next chapter how this view resolves long-standing problems for empiricism: the failure of empiricists to demarcate experience cleanly from further results; and an anti-empiricist skepticism behind the thesis of the theory ladenness of observation.

The identification of experience with a process may call to mind the ontology of process theory. According to it, the world at a foundational level is not constituted by things, but by processes. The notion of things is derived from these processes. Process theorists will, I expect, take comfort from the fact that experience enters into small-e empiricism as a process. My position on process ontology is neutral. Assertions about the fundamental character of the world are to be decided empirically by the import of evidence; and that exploration is not undertaken here.[126]

[125] Elvis is dead, despite repeated reports of sightings that suggest otherwise.

[126] For an account of process theory in physics, see Penn (2023).

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11. The Terminal Obsession

Chapter 11

The Terminal Obsession

1. Introduction

Big-E Empiricism maintains a strict division between the experiential and non-experiential content in science. The division is essential to Big-E Empiricism since it is founded on assigning different statuses to each. What results is an enduring problem for Big-E Empiricism. It arises in two ways. In the first, Big-E Empiricists have long struggled without success to articulate just how the division is to be effected. In the second, critics of empiricism have used the thesis of the theory ladenness of observation to argue that the division cannot be effected. That failure is used to support challenges to the authority of experience.

This pair of problems evaporates once we adopt the notion in small-e empiricism of experience as a process.

First, since the process is continuous, there is no sharp boundary between experiential and non-experiential content. All we can say is that some stages of the experiential process are closer or farther from the system of interest. The observer stage, which has proven to be just so troublesome to Big-E Empiricism, is merely that stage farthest from the system of interest among the stages we happen to have considered. It is otherwise like any other stage and is open to the same evidential scrutiny.

Second, theoretical terms will appear in propositions describing the stages of the experiential process. The operation of winding back described in Chapter 10 allows us to eliminate any nominated theoretical term from the descriptions of the stages of the experiential process by reverting to descriptions of stages prior to the term's introduction, farther from the system of interest. Although their complete elimination is probably unattainable, the presence and role of any particular theoretical term is controllable and thus can be rendered benign.

Big-E Empiricism is preoccupied with the idea of the terminus of the experiential process, farthest from the system of interest, as uniquely efficacious, epistemically. Big-E Empiricism's critics have correspondingly been preoccupied with trying to establish that such a terminus is not identifiable as a state of pure experience, untainted by theoretical contamination.

From the perspective of small-e empiricism, the sound and fury of the ensuing debates is misplaced. There is no such theoretically untainted observer terminus. A viable empiricism has no need of one. Thus, I call its pursuit the "terminal obsession."

The earlier Sections 1 to 3 of this chapter address the difficulty of demarcating experiential and non-experiential content. As samples of an enormous literature, Section 2 reports empiricist presumptions of such a division in the writings of Carl Hempel and Victor Lenzen. Section 3 reports how W.V. O. Quine and Wilfrid Sellars argue against the possibility of such a division.

Sections 4 to 8 recount how small-e empiricism escapes the problem through its conception of experience as a physically continuous process. Section 4 reports that the security of results inferred from some provisional terminus of the process can be checked and corrected, if needed, by several means. If we have access to the physical stages of the experiential process that ends in the terminus, we can check that these stages would properly issue in the terminus. If more stages in the experiential process still farther from the system of interest are accessible to analysis, we can check that propositions describing those farther stages do inductively support propositions describing the terminus.

That a stage is implemented in the excitation of some human sense organ and its associated mental state might seem to make it distinctive and thus distinctively troublesome. The problem is not universal since, as Section 5 notes, many experiential processes have no essential, human elements. Even when such an element is present, the sciences of psychology and physiology allow us to scrutinize the human elements and to control and correct reports derived from them. Sections 6 and 7 give two examples in the personal equation from astronomy and the psychological analysis of dubious UFO reports.

If these last checks are precluded practically, it is argued in Section 8 that the terminus can still be subjected to evidential scrutiny by its relations to other experiential processes connected with the same system of interest. If other experiential processes lead to compatible termini, then each provides support for the others.

Sections 9 to 14 address the thesis of the theory ladenness of observation. Section 9 recalls how the operation of winding back allows the influence of any particular theoretical term to be controlled and eliminated. The remaining sections report the formulations of the general idea in the work N. Russell Hanson, Paul Feyerabend and Thomas Kuhn. Each is treated separately since they differ widely. The views of Hanson and Kuhn are, in my assessment, the least effective. They depend, as I argue in Sections 10 and 13, on conflations and tendentious metaphors. Hanson's account conflates seeing in the physiological and psychological senses. Kuhn's account depends heavily on treating theory change, psychological gestalt switches and problems of translation in ordinary languages as essentially similar, whereas they fail to be similar in aspects relevant to the rationality of theory choice.

Feyerabend's analysis is, in my assessment, the most rigorously argued. Sections 11 and 12 argue that his case fails in both its philosophy and its history. It exaggerates the malign import of the ineliminability of theoretical terms in describing experience and its accounts of historical examples contradict the history. As an illustration of the last case, I review in greater details the decision between Lorentz's ether theory of electrodynamics and Einstein's special theory of relativity. Einstein identified the experiments that favored his theory over Lorentz's and he had no trouble in drawing on descriptions of them that used no theoretical terms that would prejudice the choice.

2. The Problem: Separating Experience from Theory

That experience can be separated from further results is routinely asserted by empiricist writers. They do so, often optimistically without reservations and sometimes more cautiously by allowing for ambiguity in the division. Here are two examples.

Carl Hempel's "The Theoretician's Dilemma" contains a classic statement of the division (1958, p. 42):

> … we will assume that the (extra-logical) vocabulary of empirical science, or of any of its branches, is divided into two classes: observational terms and theoretical terms. In regard to an observational term it is possible, under suitable circumstances, to decide by means of direct observation whether the term does or does not apply to a given situation.

Hempel does concede some sense of arbitrariness in the division. He suggests with optimism that his discussion will be "be independent of how narrowly or how liberally the notion of observation is construed."

Other authors in Hempel's tradition are more explicit in the malleability of the division. Victor Lenzen, in his contribution "Procedures of Empirical Science," to the Neurath-Carnap-

Morris *International Encyclopedia of Unified Science* allows that the partition between object and observer depends on the science (Lenzen, 1955, p. 310):

> The problem of the physicist ends when he reaches the boundary of the organism, but the biologist displaces the partition into the body of the observer. The optometrist places the partition at the retina of the eye, and the physiologist of the nervous system displaces it still farther into the organism.

The partition even moves according to the purposes of the scientist (p.309):

> If a physicist is looking at a pointer on a scale, its status depends on the purpose of the observation. If he is using the instrument to measure an electric current, the pointer is an extension of the observer; the object is the electric current. If the physicist is calibrating his instrument, the pointer is part of the object of observation; the light by which the pointer is seen is then an instrument which belongs to the observer.

3. Objections

This optimism is excessive. The whole analysis of Hempel's paper depends upon assigning different statuses to observational and theoretical claims. Simply to presume that the division is possible presumes too much, especially given the repeated complaints that such a division is not possible. As long as the division is not well-defined, Hempel's analysis has no well-defined foundation. It risks not being about anything.

Whereas Lenzen did not acknowledge directly that the malleability of the division is troublesome for Big-E Empiricism, many others did. In a celebrated version, the concern is one of the two "dogmas of empiricism" in W. V. O. Quine's classic (1951). His target is "radical reductionism" according to which: (p. 36)

> Every meaningful statement is held to be translatable into a statement (true or false) about immediate experience.

Quine takes Rudolf Carnap's *Aufbau* as embodying this notion of reductionism in its search for a "sense-datum language." Quine's objections are brief and not especially detailed. Not much more was needed, however, since Carnap's own formulation of the reduction was sketchy and incomplete. Quine merely pointed this out.

Another influential development of the problem is Wilfrid Sellars' "myth of the given." Sellars' target was the then widely accepted notion that knowledge is founded on the "given."

The Vienna Circle manifesto, for example, expresses it as: (Hahn, Neurath, Carnap, 1929, p. 309)

> … there is knowledge only from experience, which rests on what is immediately given [*auf dem unmittelbar Gegebenen*].

This is the view that both Sellars and Quine dispute. Where Quine's writing is frustratingly brief and replaces rigorous clarity for rhetorical elegance, Sellars' objection is convoluted both in its argument and presentation. deVries and Sachs (2024, §4) present a synopsis of Sellars' argument in a sequence of thirteen propositions. They reduce it to the claim of an irremediable tension between two requirements for the given. It is a cognitive state that is independent of others, since it must serve as the irreducible foundation of knowledge. That independence conflicts with the very need to serve as a logical foundation for further knowledge.

These are just two well-known formulations of a recurring objection to Big-E Empiricism. We have seen in Chapter 4 that this same concern reappeared as one of the most persistent objections to van Fraassen's constructive empiricism.

4. The small-e resolution

Small-e empiricism does not suppose a strict division between experience and what is inferred from it. It is unlike foundationalism in epistemology, which treats basic beliefs as non-inferentially justified by direct acquaintance.[127] For small-e empiricism, experience is a physical process that connects us continuously with the system of interest. There will be a terminus in the sense of a stage that is farthest from the system of interest among the stages considered. It is not distinctively troublesome and can be subject to the same sort of evidential scrutiny as any other stage. What matters is not the terminus, but the process taken as a whole.

The evidential scrutiny of the terminus can come in many forms. We may reaffirm the reporting of the farthest stage by examining the physical stages in the experiential process between it and the system of interest. Consider, for example, the 1883 volcanic eruption of Krakatoa. The sound waves propagating from it comprise the experiential process. There were many points of detections. If there are doubts about the report of a detection at some designated

[127] Mention of this contrast was prompted by a query to me from Howard Sankey on Popper's "basic statements." Popper (2002, Section 29) himself argued that any basic statement was also open to further testing if deemed needed.

location, we can in principle check that the sound was also detected at the right time at intermediate locations on its way to the designated location. Even in cases in which practicalities preclude us from tracing back through the experiential process in this way, we can still bring further evidential scrutiny to bear. We can consider relations of support with other evidential processes connected with the system of interest. Illustrations will be provided in Section 8, below.

5. Inhuman Experience

Debates over the terminus are commonly formulated in terms of human sensory experience. Then the observer terminus arises in some suitable combination of an excited human sense organ and a corresponding mental state. We may be concerned that there is something distinctively troublesome about the human substratum carrying this stage. That these concerns cannot be general is easily seen if we consider experiential processes that are fully implemented instrumentally, without human sense organs playing any essential role. For then there is no distinct human sense organ and mental state serving as an essential stage of the process.

Chapter 9 provided examples of such experiential processes. They include the spectroscopic analysis of chemical samples, the algorithmically aided discovery of exoplanets, DNA typing and LIGO's detection of gravitational waves. In all these processes, the provisional terminus is some mechanically created record. It might be a computer file, or a computer screen display, or even just a paper print-out. If questions arise, we might seek to trace the experiential process beyond these stages to those that produced them. Humans will look at the mechanically created record or learn of its content in some manner. However, human sensory organs and human cognitive processing are not essential parts of the process.

For example, in LIGO's detection in 2015 of a black hole merger by the gravitational waves emitted, the terminus of the experiential process lay in the determination of the coincidence of two interferometer strain measurements from the interferometers at Hanford, WA, and Livingston, LA. The coincidence was displayed visually in the superimposed plots in the time of the two strain measurements in Chapter 9. The judgment of their coincidence is not derived from a human scan of the plots and a human judgment of coincidence. That they coincide is determined by elaborate statistical analyses that preclude the agreement as arising by chance. That preclusion in reported in propositions such as Abbott, B. P. *et al.* (2016, p. 061102-6):

> This corresponds to a probability $< 2 \times 10^{-6}$ of observing one or more noise events as strong as GW150914 during the analysis time, equivalent to 4.6σ.

The human element has been eliminated.

6. The Personal Equation in Astronomy

It is distinctive in these analyses of instrumental processes that each stage of the experiential process is understood through the applicable science. A similar analysis is also possible when human sense organs and human cognition do play an essential role in the provisional terminus. In this case, the observer terminus is a mental state. Psychology and physiology are the sciences that allow us to proceed past it to the farther stages that produced it. We do not need to accept, as irreducibly final, any report of what is sensed by a human observer's sense organ. If there is a need, these sciences can moderate them. This is true, at least, in principle. In each case, it presumes that sufficient records are available and that the relevant science has been sufficiently developed. This and the next section provide two examples of how such sciences allow us to trace beyond and to correct some human sensory report.

One of the earliest cases of an explicit application of psychological analysis is the "personal equation" in astronomy. A fundamental datum in observational astronomy is the determination of the precise time that a given star transits the meridian, that is, passes its highest point in its nightly motion from east to west. The timing is carried out by an observer watching as each star advances past spaced lines on a reticule in the visual field of a telescope. The time at which the star passes each line is determined by coordinating the event with a chronometer by various means. The exact timing of the transit of the meridian can then be interpolated from these times.

This measurement seems straightforward enough. But it was not. There were curious and persistent differences of as large as a second in the timings reported by otherwise careful and competent astronomical observers. These differences were large enough to be a problem for celestial navigation. An error of a second in the timing translates into an error of 0.29 miles at the equator.

Friedrich Wilhelm Bessel of the Königsberg observatory in German presented an early analysis of the problem in a preface (Bessel, 1823) to his report of the observatory's observations of 1822. He related (p. III) the alarm of the British Astronomer Royal, the "immortal [*verewigte*] [Nevil] Maskelyne," on finding that his assistant, David Kinnebrook, systematically reported times for the transits that were 0.5 to 0.8 seconds later than Maskelyne's own measurements. Where Maskelyne fired Kinnebrook for some sort of misbehavior, Bessel recognized that the difference was not one of inattention by the observer, but somehow intrinsic to each observer. He concluded:

> … one can no longer doubt that an involuntary, constant difference can occur between two observers, which far exceeds the limits of accidental uncertainty, and which not only deserves closer investigation in relation to astronomical observations, but also seems to be extremely remarkable from an anthropological point of view.

Bessel proceeded to determine these systematic differences between several observers. These differences between the times reported by different observers were also investigated by the new Astronomer Royal, John Pond (1833), in Britain. He called the relative differences between observers the "personal equation" (p. iv).

That the astronomers must accommodate this personal equation became a routine matter in astronomy. Loomis' 1855 *Introduction to Practical Astronomy* gave two methods (pp. 80-82) for determining the personal equation, still understood as the systematic difference between the timings offered by two observers. Loomis surely reflected the consensus view of astronomers when he concluded (p. 82) "The amount of this error in skillful and long-practiced observers is truly surprising."

The existence of this personal equation was of sufficient concern that its control attracted considerable efforts by astronomers. They soon were able to replace the equation as measures of difference between observers by what they called the "absolute personal equation," that is, the systematic difference for each observer between the time they report and the true time. The new Astronomer Royal, William Christie (1887), described a device that enabled measurement of the absolute personal equation. It used the motions of an artificial sun, moon and stars in the device, for which true transit times were determinable, to simulate an astronomical observatory in which each observer would make their measurements.

All the work described so far was carried out by astronomers. They were, in effect, making a contribution to empirical psychology. The potential importance of their analyses to empirical psychology was soon picked up by the psychologist, Edmund Sanford (1888), in his article "The Personal Equation" in *The American Journal of Psychology*.

In a trend in observation we have seen throughout science, the human component in the experiential process was eventually eliminated in favor of a purely instrumental process. Littel and Willis (1929) describe a purely instrumental procedure for determining transit times. A star leaves a trail on a photographic plate as the star moves across the sky. Chronometer generated shifts in the photographic plate leave timing tick marks in the trail from which the transit time can be interpolated.

7. The Psychology of UFO Reporting

There is a curious element in many of the reports of flying saucers, UFOs or UAPs:[128] they include elements that, on their face, are implausible. Very commonly, massive objects are moving in ways that contradict established physics; or there are descriptions of objects quite like the metallic painted, cardboard props from a low-budget, science-fiction movie.[129] When not fraudulent deceptions, these curious elements are best understood not as veridical reporting of something existing objectively, but of misinterpretations arising through the vagaries of sensory organs and mental processing. That is, they are best analyzed using psychological theory.

The need for such analysis was recognized in the Condon Report. Its second volume concluded with a chapter length review (Condon, 1968, Vol. 2, pp. 930-943, "Perceptual Problems") by the psychologist, Michael Wertheimer. It reviewed the various stages of perception that underlie a UFO report, as understood by the best work of the contemporary literature in the psychology of perception. Wertheimer reviewed the transmission of a physical signal from the distal source to a human sense organ; how the resulting proximal signal may be a distorted representation of the distal source; problems that can arise in the connection of the

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[128] See Chapter 10 for more details of UFO and UAP investigations.

[129] The Condon report relates a reported sighting by the co-pilot of a Mohawk Airlines DC-3, flying East of Utica, New York, just after noon, on June 23, 1955: (1968, v.1, p. 213)

> It was moving at "great speed." The body was "light gray, almost round, with a center line . . . . Beneath the line there were several (at least four) windows which emitted a bright blue-green light. It was not rotating but went straight." The pilot also saw this UFO; they watched it for several miles. As the distance between the DC-3 and the UFO increased, the lights "seemed to change color slightly from greenish to bluish or vice versa."

stimulated sense organ with the cognitive processing portions of the brain; problems in the cognitive processing of the signal that issues in a communicable report; and how the emotional state of the observer and the social context of reporting can affect the report delivered. Wertheimer identified many ways in which distortions in each stage can lead to spurious reporting. For example, reports of enormous speeds are readily mistaken: (pp. 933-34)

> Estimates of speed are just as ambiguous as estimates of size and distance … The retinal image, and the successive changes in it, can be the same for a small, near object moving slowly as for a large, distant object moving rapidly. Apparent speed depends upon relative displacement within a framework, rather than upon absolute displacement across the retina.

Wertheimer's summary evaluation is cautionary: (pp. 940-41)

> UFO reports are the product of a long chain of events, from distal stimulus through to the final reporting; at every link in this chain there are sources of distortion. Details of specific reports, are, by the very nature of the processes of human sensation, perception, cognition and reporting, likely to be untrustworthy. Thus any report, even those of observers generally regarded as credible, must he viewed cautiously. No report is an entirely objective, unbiased, and complete account of an objective distal event. Every UFO report contains the human element; to an unknown but substantial extent it is subject to the distorting effects of energy transmission through an imperfect medium, of the lack of perfect correlation between distal object and proximal stimulus, and of the ambiguities, interpretations, and subjectivity of sensation, perception and cognition.

8. When the Experiential Process Cannot be Traced

When all we have is the report of an experiential stage farthest from the system of interest, we may be precluded from examining the intervening stages or regressing to stages still farther from the system of interest. Then, something close to an evidential scrutiny of the report might still be possible. It happens when there are other evidential processes connected to the same system of interest. Although there may be some doubt about the report from each process taken individually, if they mutually agree, we may be able to dispatch those doubts through their mutual support.

The simplest case is of a pair of propositions *A* and *B*, such that each reports from a stage farthest from the same target system in two different experiential process. If they agree, proposition *A* inductively supports proposition *B* and proposition *B* inductively supports proposition *A*.[130]

An example appeared already in LIGO's gravitational wave detection of distant gravitational events, recounted in Ch. 9 and above. A deflection on the interferometer in Hanford, Washington State, could only be affirmed to indicate a distant gravitational event, if the second interferometer in Livingston, Louisiana, also found it. But the Livingston, Louisiana, reading could only be affirmed by a corresponding reading on the Hanford, Washington State interferometer.

This last example does not involve human sense organs essentially. However, the basic logic is the same for an example that does: the observation of the supernova of 1006AD. It was one of the most brilliant supernovae to attract mention in historical records. Later scholarship, summarized in Stephenson and Green (2002), has collected as many as 30 separate accounts drawn from observers in China, Japan, the Arab world, and Europe. For example (p. 153), a Buddhist monk, Weng Ying, in roughly AD 1078, wrote the work *Shaofu Yutang Qinghua* and reported:

> Jingde reign period, third year, [= AD 1006–1007], there was a huge (*ju*) star seen (*jian*) in the sky at the west of Di (lunar lodge). Its bright rays were like a golden disc. No-one could determine its significance.

By itself, we would be hesitant to accept on its authority that a new star with a golden disc had indeed appeared then. The veracity of the observational report is affirmed by the many compatible observation reports made independently across a wide part of the globe. Each of these reports is supported by the others.

Consider again the experiential process of the propagation of sound from the volcanic eruption of Krakatoa. The report of the sound from the site closest to the volcano in the Krakatoa Committee Report (Strachey, 1888, p. 80) is from Anjer in Java (now Indonesia). It is just 31

[130] These are yet another instance of the crossing over of relations of inductive support, such as is distinctive of the large-scale structure of inductive inference, as recounted in detail in Norton (2024).

English miles distant from the volcano. Its port was destroyed by a tsunami triggered by the Krakatoa eruption. That the report is of the Krakatoa eruption is supported inductively by comparison with its timing and that of later detections in sites more distant from the volcano and in other directions. Conversely, those later detections are inductively supported by the Anjer report. The calculations displaying the mutual support among these detections are not merely matters of abstract possibility. They were carried out in detail, as summarized by Strachey (1888) in the Krakatoa Committee Report.

9. Theory Ladenness of Observation

One version of the terminal obsession has become, unfortunately, a common basis for skepticism about empiricism in history and philosophy of science. It is the idea that all observations are theory laden. This means that their descriptions involve theoretic terms that invoke some theory in addition to whatever the experience may provide. Thus, it is suggested, there is no such thing as pure experience which can provide an objective, theory-free evidential basis for theory choice.

Matters look quite different within small-e empiricism. The stages of experiential processes are described by propositions. These propositions are formulated in a language, whose vocabulary is inevitably dependent on some sort of theory. The vocabulary may include predicates dependent on rather untroubling generalization, such a "visible light"; or the predicates might involve deeper theory, such as "has [quantum mechanical] spin one half." That rather banal fact by itself fails to support the sort of skepticism that is claimed to follow. The process of winding back allows experience to decide among specific theories without circularity. The comparison is made by winding back through an experiential process to a stage farther from the system of interest at which the terminology used no longer invokes any of the theories to be compared.

The claim that the theory ladenness of observation corrupts the evidential import of experience depends on an oversimplified conception of "theory." It is treated as a univocal mass that is imposed upon experience. The better analysis breaks up this mass into specific theoretical terms. Imagine that some term prejudices the way that experience decides among specific theories. That prejudice is controllable, if we can wind back to a stage of the experiential process whose description does not employ the troublesome terms. It may be that, sometimes, lack of records precludes such a winding back. That is not sufficient to sustain the claim that the theory ladenness of observation necessarily and always corrupts theory choice. Although we may never be able to eliminate theoretical terms entirely from descriptions of the stages of an experiential process, winding back can enable us to so deplete their influence that their presence is benign.

Contrary to the evidential pessimism of the literature in philosophy of science, this theory ladenness can enhance the evidential power of experience. It automatically allows us to see how some experiential process fits with the larger body of evidence that has informed the theory. When we are told of an experiment that supports a "reactionless drive" for a spaceship, the name itself tells us that the drive violates Newton's third law of the equality of action and reaction and thus is inconsistent with all the evidence that favors this law.

The sections that follow will seek to deflate the threat of theory ladenness. It will survey some of the original authors in philosophy of science on the topic: N. Russell Hanson, Paul Feyerabend and Thomas Kuhn. Although their writing bears a superficial similarity, they differ so much in the details of their claims and argumentative methods that they need to be treated separately.

10. Russell Hanson's Patterns of Discovery

The idea that observations are "theory-laden" and this term for it were introduced by Hanson (1958) in his *Patterns of Discovery*. It appeared about halfway through his first chapter on the topic of observation. He wrote: (p. 19)

> There is a sense, then, in which seeing is a "theory-laden" undertaking. Observation of *x* is shaped by prior knowledge of *x*. Another influence on observations rests in the language or notation used to express what we know, and without which there would be little we could recognize as knowledge.

Hanson's text is widely credited as the source of this term and notion. Hanson himself seems to have had little interest in the term. It appears in one other place in his text (p. 60). A related issue is given prominence in Hanson's chapter on observation. It is derived from the artful question (p. 5) "Do [heliocentrist] Kepler and [geocentrist] Tycho see the same thing in the east at dawn?" Hanson then put much effort into trying to establish a sense in which they do not see the same thing.

The term "see" can be used in different contexts. It can pertain to the mechanics of viewers' vision or to the internal mental states of viewers arising upon visual stimulation. The term "see" has different meanings in each context. We will give different answers to Hanson's question according to the context. Tycho and Kepler experienced the same visual stimulation, but they conceived of the stimulus differently. They do so since they interpret the stimuli using different background assumptions. Tycho assumes an immobile Earth. Kepler assumes a mobile Earth.

As long as we keep the different contexts in mind, there is nothing especially noteworthy foundationally about the different answers. We both experience the same visual stimulation of a house, but only I recognize it as my childhood home. It is only when Hanson, in my reading, starts to conflate the two senses that something mysterious happens, that Tycho and Kepler both see and do not see the same thing. The remedy is not, as Hanson suggests, a reevaluation of the very idea of seeing. It is just to be more careful in keeping different senses of "see" separated.

There is also no challenge in the viability of sense experience in the different conceptions of Tycho and Kepler. The difference is not incorrigible and thus not unsurmounable. They differ because they harbor different assumptions. Further investigations are quite able to decide which assumption, if either, is correct. Those further investigations strongly favored Kepler's conception correct and not Tycho's.

Perhaps the best way to understand Hanson's purpose is not as a failed effort to extract skeptical import from the theory ladenness of observation. Rather, as the editor of a collection of his papers suggested (Lund, 2020, p. x), it was part of his effort to extend our understanding of scientific reasoning beyond the confines of deductive logic:

> Since Hanson saw the theory-ladenness of observation as a consequence of the overarching conceptual structure of science, he was not interested in using the theory-laden observation thesis as a ground for arguing against scientific objectivity or scientific realism, as many other philosophers were inclined to do. Instead, Hanson's primary interest lay in discerning the types of good reasoning that take place within the realm of empirical fact.

11. Paul Feyerabend and the Failure of Meaning Invariance

The most philosophically rigorous development of these themes is in the writings of Paul Feyerabend. His "Problems of Empiricism" (1965) explored a wide range of material. A core concern was his advocacy of what he called a "theoretical pluralism" (p. 149). It is the claim that progress in science is best advanced if we have many theories under active consideration. Feyerabend was concerned that this plurality not be compromised too quickly by evidential scrutiny. To this end, throughout the work, he sought to impugn the power of observation and experience to choose among theories. It is here that Feyerabend introduced his version of the theory ladenness of observation: (p. 151)

> In these accounts [that Feyerabend disputes] it is taken for granted that observational results can be stated and verified independently, at least independently of the theories investigated. This is nothing but an expression, in the formal mode of speech, of the common belief that experience contains a factual core that is independent of theories. Such a core must exist, or else we can never be sure that our ideas have any relation to fact. Theoretical pluralism is inconsistent with the idea of a core. The reason is quite simple. Experience is one of the processes occurring in the world. It is up to detailed research to tell us what its nature is, for surely we cannot be allowed to decide about the most fundamental thing without careful research.

To continue his protection of the plurality of theories from observation, Feyerabend recalled how observation sentences acquire their meaning from the theory with which they may be associated: (p. 213, his emphasis)

> According to the point of view I am advocating, the meaning of observation sentences is determined by the theories with which they are connected. Theories are meaningful independent of observations; observational statements are not meaningful unless they have been connected with theories. …
>
> It is therefore the *observation sentence* that is in need of interpretation and *not* the theory.

This otherwise benign observation becomes a potent barrier to the use of observations to decide among completing theories when it is combined with another Feyerabend's distinctive theses. It is the claimed failure of the invariance of meaning under theory change. The term "mass" means something different in each of classical and relativistic physics since they attribute different properties to it.[131] (pp. 168-169)

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[131] I have argued in Norton (manuscript) that the meanings of terms such as mass are realized by a map into a space of referents. That space, I believe, is sufficiently sparse that changes in the

The result is an inevitable failure of observation to be able to decide between competing theories. Here is one of Feyerabend's formulations: (p. 124, his emphasis)

> It is bound to happen, then, at some stage, that the alternatives [theories] do not share a single statement with the theory they criticize. The idea of observation that we are defending here implies that they will not share a single observation statement either. To express it more radically, each theory will possess its own experience, and there will be no overlap between these experiences. Clearly, a *crucial experiment* is now impossible. It is impossible not because the experimental device would be too complex or expensive, but because there is no universally accepted statement capable of expressing whatever emerges from observation.

Feyerabend's analysis is lengthy, painstaking and often illustrated by examples in the history of science. It is easy to be overwhelmed by its erudition and sheer bulk and thereby fail to see that it just does not work.

The clue to the failure is its incompatibility with the historical record. The history of science is replete with occasions in which one theory has been replaced by another on the strength of the evidence. The examples are so familiar that we scarcely need do more than name them: Copernican heliocentrism replaced Ptolemaic geocentrism; Lavoisier's oxygen theory replaced phlogiston theory; Darwin's evolutionary theory replaced Lamarkianism; quantum theories of matter replaced classical theories; and so on. In each case, the evidence can be displayed in a manner than shows the decision to be rational, although commonly the display is more complicated than an oversimplified history of science would suggest.

The fallacy in Feyerabend's reasoning is also apparent. Propositions describing the stages of an experiential project may be assigned different meanings according to the theory with which they are associated. However, for any specified decision between two competing theories, if the records of observation and experiment have been kept, it is possible to wind back through the experiential processes to a stage where the meaning of the observational terms used is no longer affected by which of the two theories may be associated with the observation. Then the observations can serve to adjudicate among the theories without circularity. The illustrations

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properties assumed for what the theory designates as mass are sufficiently error tolerant as to preserve the uniqueness of the referent.

earlier of winding back should have already made this obvious. In the next section, it will be illustrated for the case of special relativity.

12. Feyerabend and Special Relativity

The decision between Lorentz's ether theory and Einstein's special theory of relativity is one of the most popular in this literature. Feyerabend used it and offered an ambitious, general argument against the possibility of observations providing an objective basis for deciding between them: (Feyerabend, 1965, p. 171)

> Let us again take the theory of relativity as an example. It can be safely assumed that the physical thing language of Carnap, and any similar language that has been suggested as an observation language, is not Lorentz invariant. The attempt to interpret the *calculus* of relativity on its basis cannot therefore lead to the *theory* of relativity as it was understood by Einstein. What we shall obtain will be at the very most *Lorentz's interpretation*, with its inherent asymmetries. This undesirable result cannot be evaded by the *demand* to use a different, and more adequate, observation language.

The specific objection raised here by Feyerabend is that evidence that is not Lorentz covariant cannot "lead to" a Lorentz covariant theory. The objection seems compelling for its engagement with the technicalities of relativity theory. However, it is contradicted in even a superficial acquaintance with the history of relativity theory. For neither of the two key items of evidence that Einstein used can be described as Lorentz covariant. The failure of ether drift experiments included mention of a particular state of motion, that of the earth. The failure of emission theories of light was too general to have any specific covariance properties. Nonetheless, the failure of ether drift experiments was evidence for the principle of relativity; and the failure of emission theories of light was evidence for the light postulate. Einstein showed that their conjunction entails the Lorentz covariant special theory of relativity.

Feyerabend's more general claim also fails. Contrary to his claim, observational evidence is not so specific to one theory that it cannot be used to support another. Einstein was quite able to draw on decisive observational and experimental evidence that was originally discovered in the context of theories that differed from special relativity. The process of winding back to a representation of the evidence free of these theories was so straightforward as not even to attract a comment by Einstein.

Einstein spent at least seven years studying problems in electrodynamics before advancing his special theory of relativity. As I report in my Norton (2004), he identified two observational and experimental results as of special importance in these investigations: stellar aberration and the speed of light in moving water.

Stellar aberration was reported by Bradley (1729) after careful measurements of the positions of stars over the course of a year. He found a systematic displacement in stellar positions that correlated with the annual motion of the Earth and its observers. The displacement was just what would follow if light from distant stars propagates at a finite speed. More precisely, he found that the angular displacement of a star's position was determined by the ratio of the suitably directed velocity of the observer on Earth with that of light. He summarized his result in an equality: (p. 648)

> And in all Cases, the Sine of the Difference between the real and visible Place of the Object, will be to the Sine of the visible Inclination of the Object to the Line in which the Eye is moving, as the Velocity of the Eye to the Velocity of Light.

This result is reported as a relation among the angular positions of stars, real and observed, and the velocity of the observer relative to the star. Its expression presumes neither a Newtonian nor a relativistic theory.

Bradley would have derived and understood his general result in Newtonian terms. His observations were carried out at a time in which Newton's corpuscular theory of light prevailed, although it was not mentioned in his analysis. All that Bradley's analysis required was Euclidean geometry and the Newtonian rule for the addition of velocities.[132] Whereas it is unlikely that Einstein consulted Bradley's paper directly, he could have done so with no ill effect. Bradley's

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[132] The exact, Newtonian result, as given in Young (1889, p. 142) is that the angular displacement α depends on the angular elevation θ of the star to the direction of motion of the Earth as $\tan \alpha = \sin \theta\, (v/c) / [1+(v/c) \cos \theta]$, where the speed of the Earth is v and that of light c. Since α and v/c are both very small, $\tan \alpha \approx \sin \alpha$ and the second v/c term can be neglected. The result becomes $\sin \alpha / \sin \theta = (v/c)$, which is the result Bradley stated. Replacing the Newtonian rule for velocity addition with the relativistic rule introduces a factor of $\sqrt{1 - v^2/c^2}$. For very small v/c, the correction to the aberration formula is an even smaller, second order additive term in v^2/c^2 that is observationally negligible.

result, as used by Einstein, applies to an Earth moving at a velocity much smaller than that of light. There, the relativistic rule for composition of velocities gives the same result numerically as Newton's rule for the observable angular changes in position of the stars.

Feyerabend may still worry that Bradley and Einstein mean different things by "velocity." Those differences are irrelevant in the low velocity regime to the numerical rule Bradley enunciated; and that numerical rule is all that Einstein needed. Just reading Bradley's paper or a rendition of the result in the later literature is winding back enough to free the result of any unwanted entanglement with prior theories.

Fizeau's (1851) experiment measured the velocity of light in moving water. For water at rest, the speed of light in vacuo, c, is reduced to c/n, where n is the index of refraction of water. Fizeau found that when the water flows with speed v in the direction of light propagation, its full speed v is not added to c/n, but only a fractional part, $v(1–1/n^2)$, is added. All the quantities involved are established without essential contributions from ether theories or any other theory that may be troublesome for relativity theory. The refractive index of water is recovered from refraction measurements. The speed of the water is measured from volumes delivered in noted times. The speed of the light is determined by displacements of interference fringes. It follows that the result is as usable by an ether theorist as by Einstein working on special relativity. The winding back to this result is again so direct as not to require any special comment by Einstein.

Fizeau derived and understood the result in the context of nineteenth century ether theories as conforming with Fresnel's ether drag hypothesis. Water moving at v drags the luminiferous ether partially at a speed $v(1–1/n^2)$, so that the speed of light in moving water is increased only by this partial amount. The same observed result can be recovered in special relativity without any recourse to ether theories. It follows from using the relativistic rule of composition of velocities to compose the two velocities, c/n and v, where v is much smaller than c.[133]

The importance of these two experiments should not be discounted. In seeking to explain why Einstein would single them out, it became apparent to me in Norton (2004, §7) that they are observational manifestations of the most important novelty of the special theory of relativity, the relativity of simultaneity. That is, if Einstein were to conjoin either of these observational results

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[133] The rule is $c/n \oplus v = (c/n + v)/(1 + (c/n)(v/c^2)) \approx c/n + v(1–1/n^2)$ for $v << c$.

to the principle of relativity, he would arrive directly at the relativity of simultaneity, without any further intervention of theory. He could introduce the principle of relativity here since Einstein had empirical evidence for the principle. Einstein thereby arrived at a Lorentz invariant result, the relativity of simultaneity, contrary to Feyerabend's overconfident assertion that this would be impossible.

The Michelson-Morley experiment of 1887 is also a part of the evidential base of special relativity. Michelson and Morley used a very sensitive optical interferometer in an attempt to detect effects on light propagation due to the motion of the Earth through the luminiferous ether. Famously, they found no such effect. It was as if their interferometer was always at rest in the ether, no matter its orientation with respect to the Earth's motion. The null result—a negligible motion of interference fringes—is expressible in language that presumes neither ether theories nor special relativity.

There has been some historical debate over whether Einstein knew of the experiment when he formulated special relativity and, if so, what was its role. I concur (Norton, 2004, p. 46) with John Stachel's analysis: Einstein knew of the result, but read it as merely conforming with the principle of relativity. That is, the experiment merely showed how the result of an interferometer experiment on light was unaffected by the motion of the Earth. The result, as just stated, has wound back beyond any of the assumptions of ether theories that may have prevailed with the original experiment was carried out.

The issues surrounding this experiment returned when Dayton C. Miller (1926) carried out a painstaking replication of the original experiment and reported a positive result. Miller presented his results in terms of an ether theory. He had detected, he reported, the ether drift, that is, the current of ether blowing over the Earth as a result of its motion. Both proponents and critics of the experiment were able agree that the result, if correct, favored ether theories over Einstein's special relativity. It was obvious how to wind back to a statement of a positive result that was expressed in language neutral to the two theories. It was the visible shifting of interference fringes in the interferometer that depended on the diurnal motion of the Earth. Special relativity entailed that there can be no such effect. Einstein conjectured that the result was spurious, resulting from a failure to correct for temperature gradients over the interferometer. See Norton (2021, Ch.3, §3.6) for more details.

13. Thomas Kuhn on a Neutral Observation-Language

Thomas Kuhn's (1996) *Structure of Scientific Revolutions* and related writings are the best-known works that challenge the power of observation and experiment to decide among competing theories. A part of Kuhn's analysis included the idea that differences in the meaning of terms precludes a fully rational debate that could decide between competing theories. He wrote in his 1969 *Postscript* to *Structure*: (1996, p.198)

> Since the vocabularies in which they discuss such situations consist, however, predominantly of the same terms, they must be attaching some of those terms to nature differently, and their communication is inevitably only partial. As a result, the superiority of one theory to another is something that cannot be proved in the debate.

Kuhn continued to discuss the difficulties faced by scientists in debate while advocating for different theories: (p. 201)

> They cannot, that is, resort to a neutral language which both use in the same way and which is adequate to the statement of both their theories or even of both those theories' consequences.

This assessment includes the impossibility of a neutral observation-language. It is in turn grounded earlier in the main text by considerations from psychology. He described the neutral language as: (p. 125)

> … some neutral observation-language, perhaps one designed to conform to the retinal imprints that mediate what the scientist sees.

Kuhn continued to argue that modern experimental work in psychology precluded such a language. For example: (pp. 126-27)

> The duck-rabbit [illusion] shows that two men with the same retinal impressions can see different things; the inverting lenses show that two men with different retinal impressions can see the same thing.

The analysis is irksome, at least to me, since it perpetuates the conflation of different senses of "see" introduced by Hanson (whose *Patterns of Discovery* is acknowledged by Kuhn on p. 113).[134]

Kuhn's conclusion, however, seems correct to me: we are unlikely ever to devise a language that can describe our sensory experiences without terms that go beyond it. That fact, however, does not establish the conclusion Kuhn sought: that lack of the language is a profound limit to debate among theories. All that is needed is a representation of the observational and experimental results that is neutral between the theories under comparison. And *that* sort of neutrality is readily achieved by the process of winding back. We saw in the last section how key observational and experimental results underlying special relativity were readily expressible in language that was neutral among a Newtonian corpuscular theory of light, an ether theory of light and Einstein's special theory of relativity.

14. The Incommensurability of Theories

Kuhn's objections over a neutral observation-language were only part of a broader skepticism about the possibility of rational theory comparison.[135] It is with some trepidation that I seek to convey Kuhn's thought. Much of it was presented and argued for obliquely by means of analogies and metaphors that invite confusion and multiple interpretations; and Kuhn had an unnerving habit of insisting that commentators who criticize his work have simply misunderstood it, even though their critiques seem quite apt.

The core obstacle for rational theory comparison, in Kuhn's thought, is the idea that successive theories, separated by a revolution, are "incommensurable." The term itself is a metaphor. It recalls a discovery in geometry that profoundly discomfited the ancient

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[134] Curiously, Kuhn (1982, p. 669) seemed quite aware of the conflation without apparent recognition that its use is problematic: "I, for example [in *Structure*], made much use of the double sense, visual and conceptual, of the verb 'to see', and I repeatedly likened theory-changes to Gestalt switches."

[135] We might understand Kuhn's attempt to describe theory choice in terms of (epistemic) values (Postscript, 1996, pp. 184-86) as an attempt to capture the rationality of theory choice. See Norton (2021, Ch. 5) for an account of why I believe it fails, if that is its goal. It merely perpetuates the ambiguities that have obsfucated his writing on scientific rationality.

Pythagoreans. There is no common unit of length that measures in whole numbers the sides and hypotenuse of many right-angled triangles. Thus, it suggests in a loose, metaphorical sense, that we cannot properly compare successive theories and thus cannot rationally decide between them. Since the discovery of the incommensurability in geometry was catastrophic for the Pythagoreans, Kuhn's metaphor conveys a similar sense of a catastrophic, direction-changing break, whether he intended it or not.

That is what the metaphor suggests. What did Kuhn intend? Here is how Kuhn described incommensurability: (p. 103)

> Therefore, at times of revolution, when the normal-scientific tradition changes, the scientist's perception of his environment must be re-educated — in some familiar situations he must learn to see a new gestalt. After he has done so the world of his research will seem, here and there, incommensurable with the one he had inhabited before.

Read literally, this is a claim in the psychological state of scientists. The presence of the term of art in psychology, "gestalt," is not accidental. Kuhn's text proceeds to discuss experiments in psychology that involve gestalt switches, such as the transformation in perception of a subject who wears inverting glasses.

Kuhn's apparent purpose is to use the phenomena of psychology as a metaphor[136] for theory change in science. The metaphor becomes his account. That is, Kuhn then proceeded to assert that these vagaries of human perception are replicated in the practice of science and in the decisions among competing theories. We are to understand the transition from geocentric to heliocentric astronomy in the same way as we do when a subject in a psychological experiment switches perception of the same drawing between a duck and a rabbit.

The difficulty with the metaphor is that it seeks to equate, in their essentials, two very different things. The psychological processes are automatic, essentially private and largely out of our control. Theory comparison is undertaken by carefully controlled argumentation and deep

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[136] Metaphor describes better than analogy Kuhn's assertion that a scientist must "learn to see a new gestalt." Romeo would draw an analogy if he said "Juliet is similar to the sun." It is a metaphor when he says "Juliet is the sun."

reflection; and is a matter of explicit public debate. Perhaps we may find some interesting similarities, but there is scant reason to identify them to the extent that Kuhn does.

Perhaps sensing that his account of incommensurability was too dependent on a fragile metaphor, later in the text, Kuhn sought to summarize incommensurability in three elements (p. 124ff). The first two are straightforward enough. Incommensurable paradigms differ in the list of problems they must seek to solve and in the meaning of various common terms. The third is the most important and the most troublesome for relying on yet another tendentious metaphor:[137] (p. 150)

> … the third and most fundamental aspect of the incommensurability of competing paradigms. In a sense that I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds.
>
> …
>
> … before they can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift. Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt switch, it must occur all at once (though not necessarily in an instant) or not at all.

In his later writing, Kuhn (1982, 1990) did not correct his reliance on this faulty methodology of tendentious metaphors. Rather, in seeking to understand theory change, Kuhn shifted his emphasis from the metaphor of gestalt switches to the metaphor of translation between ordinary languages. Such translations are different from translations between terms in the technical language of a scientific theory. In the metaphor, the incommensurability of theories corresponds to the untranslatability between different ordinary languages. Kuhn's (1982, p. 5) illustration is the difficulty of translating the English "The cat is on the mat." into French.

Kuhn opened *Structure* on page one with a plea for the importance of history of science. It is hard for me to understand that this is the same Kuhn whose narrative is so at variance with

[137] One of the unfortunate legacies of the popularity of Kuhn's work is that it has encouraged philosophical writing in which dubious metaphors are not merely expository aids but replace clear theses and cogent argumentation.

what we find in the history of science. He is asserting that scientists, advocating for different theories, *cannot* fully communicate because, metaphorically, they are "practi[sing] their trades in different worlds" or, in a different metaphor, in the grip of different psychological gestalts; or again, metaphorically, unable to translate fully English into French.

Consider again Einstein and Lorentz and the decision between special relativity and ether theories. They do not come to agreement. But it is not for lack of mutual understanding. They were both intellects in science of the highest order; and the differences between them were matters of great importance to them. There is ample evidence of their deep engagement. They sustained a lengthy correspondence that included the decision between their two theories. They provided truly insightful summaries of their differences. Einstein, for example, in a 1907 review article, formulated it as: (1907, p. 413)

> One needed only to realize that an auxiliary quantity that was introduced by H. A. Lorentz and that he called "local time" can simply be defined as "time."

For his part, Lorentz gave an equally terse and insightful summary in his text, *Theory of Electrons*: (1916, p. 230)

> … the chief difference being that Einstein simply postulates what we have deduced, with some difficulty and not altogether satisfactorily from the fundamental equations of the electromagnetic field.

All the indications are that they communicated well, while each retained their views. They just disagreed on the import of the evidence and the direction that further theorizing should take. It is all too easy, in retrospect, to say who was right and who was wrong. Our histories tend to oversimplify the complexities of the decision among theories. That should not deceive us into attributing the recalcitrance of the loser to some psychological aberration. In the moment, these complexities can mean that scientists can fully understand each other but still disagree.[138]

15. Conclusion

Big-E Empiricism has long been troubled by its attachment to an elusive notion of pure experience. Its proponents have sought unsuccessfully to demarcate the experiential and non-

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[138] A more recent version arises in the philosophy of physics where there are widely different interpretations of quantum mechanics. I have no doubt from first-hand experience that well-informed proponents of each interpretation understand the others very well. They just disagree.

experiential content of science. Critics have claimed that such demarcation is impossible because observation, experience and theory are irremediably intermingled and that this intermingling compromises the authority of experience.

The concept of experience of small-e empiricism as a continuous process resolves both problems. Because of its physical continuity, the farthest stage of the experiential process is like the other stages. It is subject to similar evidential scrutiny and need not be treated as beyond inferential support. The process of winding back allows us to control the role of any nominated theoretical term used in describing the stages. If some specific term interferes with the comparison among theories, we would try to wind back to stages farther from the system of interest whose description no longer employs the troublesome predicate.

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12. Small-e empiricism Defined and Defended

Chapter 12

Small-e empiricism Defined and Defended[139]

1. Introduction

The fundamental idea motivating empiricism is that experience has the unique capacity to inform us of contingent facts of the world. We have seen in the first part of this volume how this motivating idea was expressed historically in various versions of empiricism. None, I believe, is fully adequate to the way that present science engages with the world. The goal of this work is to develop an updated version of empiricism that is well adapted to the successful methods of science. This chapter will articulate the doctrine of small-e empiricism and defend it. Its goal is to give a more precise expression to the vaguer idea of the privileged status of experience.

The principal ways that small-e empiricism amends existing conceptions of empiricism have already been explored in earlier chapters, especially in those that develop a concept of experience well-adapted to modern science. These ways are collected in two general amendments that are developed more fully in Section 2 below. The core doctrine of small-e empiricism is developed in Section 3 and is:

> *Science is empirical.* The propositions of experience provide inductive support for the truth of the contingent propositions of science; and the inductive support of experience is the only means of providing support for these contingent propositions.

The case for this version of empiricism is laid out in Sections 4 to 7 in two steps. Sections 4 and 5 develop the easier part of the case: that the resources admitted by small-e empiricism do have the capacity to lend support to the truth of contingent propositions of a science. This capacity follows from the fact that experiential processes provide physically continuous connections with the systems of interest. The second more difficult step, developed in Sections 6 and 7, is that experience is the *only* way to provide secure support for the contingent propositions of science. This is indicated by the fact that no other mode has succeeded. More precisely, successes enjoyed by other modes are retrospective. They affirm results in a science only after these results

[139] I thank Jim Brown for helpful discussion on several parts of this chapter.

have been secured empirically. That their failure is expected follows from the absence in the ontology of science of channels connected to the systems of interest in the world through which these other modes could operate.

Section 8 affirms that small-e empiricism conforms with the long-standing tradition in empiricism of excluding from science propositions that cannot be given inductive support in experience. It does not, however, endorse the extreme view of the logical positivists that such propositions are meaningless or the falsificationists' project of seeking a demarcation criterion for science. Small-e empiricism also has no place for laws of nature, when they are offered as contingent truths metaphysically more elevated than ordinary contingent regularities. In their place, small-e empiricism has empirically well supported, contingent propositions of wide scope.

Section 9 describes the importance of empiricism for the process of scientific discovery. These processes are slow and incremental and, as new steps are taken, they require the reliable guidance that only empirical evidence can provide. Chapter 10 sketches briefly how the relations of inductive support that are central to small-e empiricism relate to beliefs. An informal guide is provided by Hume's maxim that beliefs should be proportioned to the evidence. Section 11 outlines how the concerns of the venerable rationalism-empiricism debate expand well beyond the narrower ones of small-e empiricism.

In preparing the defense of small-e empiricism in this chapter, I found myself unexpectedly charting new territory. It may well be that I have been inattentive in my reading or have failed to appreciate what I have read. However, my impression is that the long tradition of writing on empiricism has almost everywhere failed to give a sustained, well-structured argument for empiricism as a general doctrine. In its place, there are strong assertions of the primacy of experience and perhaps details of how specific results are supported by experience. More generally, most writing on empiricism has been in the Big-E tradition; and that tradition is skeptical. It is anti-inductivist and committed to strict limits on what experience can give us. Hence a primary goal of argumentation in Big-E Empiricism is to sustain pessimistic conclusions that contradict small-e empiricism and cannot be employed here.

2. An Empiricism for Science

The version of empiricism developed in this volume differs in many important ways from existing versions of empiricism. These differences respond to my enduring concern that empiricist thinking has largely failed to keep up with the accelerating developments in science over the last few centuries. Small-e empiricism seeks to be a serviceable empiricism adequate to the actual practice of empirical science. It arises through several changes to the existing formulations of empiricism.

The first change recalls that empiricist writing of the seventeenth and eighteenth century formulated its doctrines in terms of human cognition and mental states. The project was both a contribution to epistemology and early attempts at formulating a science of psychology. Since we now have a distinct science of empirical psychology, empiricist writing in philosophy can narrow its scope to the epistemic issues that arise once investigations have passed beyond the specifics of mental states. Chapter 5 relates how present empirical philosophy has largely failed to separate itself from the centrality of human cognition. The remedy, described in Chapter 8, is to engage with experience at the stage at which it is given propositional expression, such as when it is reported in the scientific literature.

The authority of experience derives from its connection through continuous physical processes with the systems of interest. In earlier eras of science, human sense organs were an essential element in the processes the afforded the direct connection. As a result, older work defined empiricism in terms of human sense organs. As described in Chapter 9, scientific instrumentation now surpasses human sensory powers, so that small-e empiricism includes instrumental sensing in the concept of experience.

The second change to traditional writing in empiricism is to abandon the inductive skepticism of Big-E Empiricism. The Big-E version divides experience, which is taken to be veridical, from results derived from it. These latter are not veridical and serve only as intermediates in relating experiences to experiences. Small-e empiricism accepts that the inductive support of a result, even if fallible, is support for the truth of the result. Accordingly, as described in Chapter 10, small-e empiricism discards the ever-troublesome, strict district division of Big-E Empiricism between veridical experience and the instrumental results derived from it. In its place, experience enters small-e empiricism as a physical process that connects us continuously with the system of interest.

Here small-e empiricism follows the conception of “empirical science” that has become a routine notion in the larger science literature. Its evolution and present state were surveyed in Chapter 6, where it was summarized as:

> *Scientists' conception of empirical science*. Science consists of observational and experimental reports and further results inferred from them. The observational and experimental reports are the empirical evidence indispensable for science.

The scientists have simply shed the inductive skepticism of Big-E Empiricism and did so likely without realizing the skeptical heritage of the adjective "empirical." In this regard, in my estimate, the scientists have been better philosophers than we philosophers.

3. Small-e empiricism

Chapter 7 reviewed several attempts from the early to mid-twentieth century to capture the core commitment of empiricism in a principle of empiricism. They were grouped into three general versions. Each of them, the chapter reported, fails to capture adequately the core commitment of small-e empiricism. A new formulation is needed that implements the changes indicated in the last section. The formulation of small-e empiricism is:

> *Science is empirical.* The propositions of experience provide inductive support for the truth of the contingent propositions of science; and the inductive support of experience is the only means of providing support for these contingent propositions.

Here, as indicated, "*experience*" designates a physical process that connects us continuously with the system of interest in the world. The states of the stages of the process closer to the system of interest physically condition the states of the stages further from these systems. Thus, light from a distant comet propagates to a terrestrial telescope where it forms a photographic image.

The "*propositions of experience*" describe the states of these stages and express how these stages reflect the relevant features of the systems of interest. Inductive support proceeds in the opposite direction to the physical conditioning relations. The propositions describing stages more remote from the system of interest provide inductive support for those describing stages closer to the system of interest. The propositions describing the photographic image of the comet, for example, inductively support propositions concerning the position of the comet when the photograph was taken.

In a subsequent stage of investigation, the propositions describing the various stages of the experiential process then inductively support more general propositions in the science. The propositions determined photographically of the position of the comet at particular times inductively support a proposition describing its continuous trajectory at more times.

This formulation does not specify the strength of the support provided by experience. That strength can vary. It will be noted in the section that follows that, fortunately, experience has been able, historically, to provide strong support for a wide range of sciences. Whether such strong support can be secured depends on which experiences are available to us. We can readily conceive of cases in which sufficient experience is not presently available and may never be. For example, novel discoveries in particle physics are limited by the energies attainable in particle accelerators. Processes that require much greater energies may remain outside what present accelerators can provide. Discoveries in an archeological site may be forever precluded if some disaster obliterates the site.

These cases should not be an occasion for an excess of pessimism. Time and again, claims that some domain lies beyond possible experience have been proven wrong. A well-known instance is Auguste Comte's then highly plausible assertion in his 1835 *Cours de Philosophie Positive* that we cannot know the chemical composition of the Sun. Within a few decades, Norman Lockyer had found novel spectral line in sunlight that subsequently proved to be due to a new element, Helium, that was discovered first in the Sun.[140]

Because of its close affinity with empiricism, the account of inductive support best adapted to small-e empiricism is supplied by the material theory of induction, as developed in my works elsewhere (2021, 2024). One of its results (2024, Ch. 2) is that there are self-supporting inductive structures and that these are instantiated by mature sciences. That is, the empirical evidence for a mature science is sufficiently rich that it alone is sufficient to provide inductive support of sufficient strength to secure the stability of mature sciences.

In requiring that experience is the "*only means of providing support,*" the sorts of support considered are quite general. They include any mode that purports to establish the truth of contingent propositions. The requirement does allow intermediates. These may be other propositions that do not themselves count as propositions of experience; or even those that may appear to be non-inductive modes of support. The "*only means*" requires that *ultimately*, whatever the intermediate may be, the support it provides can be traced back to the inductive support derived from experience.

[140] For details, see Hearnshaw (2010).

An example of this sort of mediation is the derivation of a fundamental law of a science from a Platonic principle that the true law must be the simplest conceivable mathematically. We shall see below that Einstein hoped that this principle would lead him to the basic laws of his unified field theory. Small-e empiricism requires that this principle is either ineffective or, if it does yield results, its success depends on a tacit or even explicit foundation of the principle in experience.[141]

4. The Adequacy of Experience

The first part of the case for small-e empiricism is an argument in three parts for the first clause of the doctrine as stated above:

> "…The propositions of experience provide inductive support for the truth of the contingent propositions of science; …"

It is developed in the following three subsections. In brief, experience has a privileged authority to provide this support. Inductive inference is a medium capable of delivering that support. We have good empirical grounds for the reality of the experiential processes and thus their capacity to inform us.

4.1 The Authority of Experience

Experience enjoys a special privilege in its capacity to inform us of contingent propositions in science. That privilege does not derive from any special capacity of human sense organs or of human mental processes. Rather, as has already been noted in Chapter 9, its privilege derives from its constitution as a continuous physical process that connects us with the systems of interest. This foundation in a direct access to the system of interest gives its deliverances an unmatched priority in any dispute.

There are many, familiar examples of the exercising of this authority. It was doubted, as we saw in Chapter 9, that biology could admit such an anomalous animal as a platypus, a duck-billed mammal. Once a specimen was secured, examined and found not to be a hoax, experience had established these doubts cannot be sustained. In the late nineteenth century, two views of

[141] See my *Material Theory of Induction* (2021, Ch.6), for an account of how productive simplicity requirements are really veiled references to support from background facts. Small-e empiricism asserts that these background facts are ultimately supported solely by experience.

electrodynamic action persisted. In one view, the electromagnetic effects arose through unmediated action at a distance but came retarded in time. In the other view, the action propagated as waves as required by Maxwell's electromagnetic theory. Heinrich Hertz (1893, Ch. VII) presented powerful evidence for Maxwell's wave view by using a spark detector to map out the distribution of the nodes and peaks of standing electromagnetic waves in space and then, similarly, to reveal further their properties such as their finite speed of propagation. Hertz's spark gap detector probed and revealed the waves by connecting directly with them.

4.2 The Power of Inductive Inference

Experience enters the scientific literature in the form of propositions that describe various stages of the experiential processes. That these propositions correctly reflect the properties of the system of interest is established by relations of inductive support. That a proposition describing the system of interest does so correctly is supported inductively by propositions describing nearby stages in the experiential process. These last propositions are supported inductively by those at farther stages; and so on.

Hubble's determination of the recession of the galaxies, as described in Chapter 10, illustrates these relations of inductive support. First, consider the stages of the experiential processes. Hubble (1929, p. 169) reported the velocities of recession of some galaxies as "available." They are made available through continuous physical processes that begin with the emission of light from these galaxies. This light is modulated into distinct frequency lines that are characteristic of the luminous material in the galaxy. Those frequency lines are shifted toward the red as the light propagates towards us through expanding space. The light, arriving at a terrestrial observatory, is split into its component frequencies and recorded in a spectrogram. Further optical instrumentation then measures the magnitude of the shift in the frequencies of the spectral lines.

Each of these stages is given propositional expression, such as in the paragraph above. Inductive support proceeds in the direction opposite to that of the light propagation. The propositions describing stages farther from the system of interest, the remote galaxies, provide inductive support for those closer to the galaxies, until support is provided for the propositions that the galaxies are receding at the velocities indicated. For example, the spectrogram is an image of lines on a photographic plate. The proposition recording the spacing of the lines supports inductively the corresponding distribution of frequencies of the light received. That an entire suite of known lines is found shifted by the same amount in the spectrogram of some specific galaxy inductively supports the proposition that the light as emitted carried the unshifted lines. That there was uniform shift then supports the proposition that the galaxy receded with a velocity suitable to produce the shift.

Once support for propositions describing the system of interest has been secured, those propositions can then provide inductive support for further propositions in the wider science. For example, the propositions asserting a velocity of recession of the observed galaxies inductively supports the proposition of a velocity of recession of the remaining galaxies for which red shifts were not measured.

That inductive inferences play such an important role in connecting experience with the further results of science means that the connections are necessarily fallible. The fallibility is controlled by the process of "winding back," described in Chapter 10. It allows us to reaffirm the correctness of an inductively supported result or to correct erroneous judgments of support. Such corrections were needed in the case of Hubble's assessments of distances to the galaxies. Winding back though his experiential processes, it was found that his erroneous distance estimates arose from mistaken assumptions about Cepheid variable stars.

Inductive inference has the capacity to provide strong inductive support for the contingent propositions of science on the evidence of experience. Whether such support can be provided depends on the strength of the evidence experience provides and, perhaps, our persistence in pursuing suitable experiences. It turns out, as is well-known, that this capacity for very strong support has been realized multiply in our science, both in sciences past and in our present sciences. For science after science, it is a routine exercise to display the empirical evidence that supports it. It is not that the science is, in a loose, general sense, supported as a whole. Rather, that overall support consists in the fact that each individual proposition of central importance to the science is individually well-supported by experience.

4.3 The small-e empiricists' Ontology

Experience has a privileged place in small-e empiricism. It provides inductive support for further and deeper truths of the world. These deeper truths that reach well beyond experience specify the small-e empiricists' ontology. The physical processes of experience are themselves a part of the ontology of science and subject to affirmation by empirical investigation. This gives small-e empiricism a self-supporting character. It allows experience to inform us of the contingent propositions of science; and those contingent truths in turn support the efficacy of experience in informing us of these contingent propositions.

This ontology reaffirms the privilege and authority of experience. Consider experiences derived from human sense organs. Galileo's observation of patterns of light and dark on the moon can be taken as veridical since there really are such patterns on the moon's surface and their configuration is imprinted on the light that propagates from the moon through Galileo's telescope to his eye. The ears of listeners in Mauritius in 1883 really did hear the eruption of Krakatoa. The sound was carried by real acoustic sound waves from an event nearly two thousand miles away. When Marsh reported a garlic-like smell in the context of his test for arsenic, molecules of arsine were really present in the arsenic coated glass of his apparatus and were really carried by diffusion to his nose.

The same consideration applies to instrumental experiential processes. Radar detection succeeds because there really are electromagnetic waves propagating from the target to the radar receiver. X-ray crystallography can reveal the structure of DNA since there really are X-rays diffracted by the molecule's crystalline structure. Gravitational waves detected by LIGO can inform us of distant gravitational cataclysms since there really are such waves. Chromatographic analysis of the components of some sample works because, within the device, the different components really do diffuse at different rates, allowing their sequential detection.

5. Inductive Hesitations

Inductive inference is an essential part of the argument for the adequacy of experience within small-e empiricism. There is a long-standing tradition of skepticism about inductive inference. This skepticism is, in my view, untenable. My texts (2021, 2024) have sought to provide a sound foundation for inductive inferences. They argue that inductive inferences are warranted by background facts, where the warrant resides in the meaning of these facts. Someone can simply refuse to accept a well-warranted inductive inference. They are free to do so, but to do so is simply to be irrational. For to be rational is just to conform with the dictates of an applicable logic.

In the example in my (2021, Ch.1), Curie's inductive inference to the crystallographic properties of Radium Chloride were warranted by a fact, Haüy's principle. The meaning of the principle is what justifies her inference. To deny the inference is tantamount to denying Haüy's principle.

Further discussion of the rationality of inductive inference is given in the following chapter. Below I respond to some hesitations about inductive inference that are more precise than a mere mistaken sense of discomfort and malaise with inductive inference.

5.1 Is Induction from Experience Sufficient?

This hesitation doubts that experience alone can provide sufficient evidence to secure inductively our mature sciences. The supposition is that empirical considerations alone provide only a part of what secures our mature sciences. The remainder, it might be supposed, is some sort of non-empirical metaphysics. My earlier work on inductive inference has been concerned precisely with this worry. I argue that mature sciences are supported entirely inductively by experience, without the need for further non-inductive supplements. The character of the support lies in a massive structure of mutually supporting propositions spread throughout a science. That is, if we identify any proposition in a mature science, we can trace its inductive support to other propositions; and the support for those to still others until the supporting network has spread over large parts of the science. Norton (2024, Ch.2, §§5-7) summarizes the case for this conclusion, whose elaboration is found in later chapters of the 2024 work.

5.2 Are Non-Inductively Founded Concepts Essential?

This is a more precise version of the last concern. It identifies the missing, non-empirical metaphysics with concepts that are supposed essential to the formation of a science. The best-known version of this idea is Kant's eighteenth-century proposal concerning concepts like space, time and causality. They are not, he asserted, inherent in things-in-themselves. They are provided by our means of perceiving the world. This general Kantian orientation has been very influential and has reappeared repeatedly in the writing of later thinkers.

The great appeal of Kant's proposal and later versions that followed is that they eliminate inductive risk. Kant feared that mere experience cannot assure us of the irrefutable truth of Euclid's geometry. He sought to secure its inerrancy by removing central propositions of Euclid's geometry from the realm of facts in the world. The problem with Kant's proposal is that his assured certainties fail. Geometry, as we soon learned in the nineteenth century, need not be Euclidean; and, as we learned in the twentieth century, is not exactly Euclidean. Kant's assurances about the certainties of causation have failed similarly.[142] Our subsequent history of securing factual, contingent truths prior to experience has fared no better. We saw in Chapter 3 that the idea that there are no synthetic, a priori truths was a core commitment of the logical positivist movement.

In their place, small-e empiricism asks that factual generalizations of a type appealing to by Kant and Kantians should be introduced tentatively as hypotheses that are to be vindicated by further empirical analysis. Accordingly, Kant's pronouncements on the necessary geometric properties of triangles have been replaced by empirically supported, non-Euclidean results in general relativity.

Versions of Kant's proposal became popular in the nineteenth century as an objection to a bare form of enumerative induction. In his *Philosophy of the Inductive Sciences*, Whewell (1847, p. 36) introduced the notion of the "Colligation of Facts" as essential to the forming of generalizations. According to it, as he put it, "Facts are bound together by the aid of suitable Conceptions." These concepts are not merely induced from experience, but arise (again in Whewell's formulation) "[when by] an act of the intellect, we establish a precise connexion among the phenomena which are presented to our senses." Chapter 2, §13, above reported how similar sensibilities supported a skepticism over the power of inductive inference alone. Henry Sidgwick, we saw, averred that "I think it impossible to establish the general truths of the accepted sciences by processes of cogent inference on the basis of merely particular premises."

These concerns are sustainable in so far as they impugn a bare notion of enumerative induction. A collection of facts that merely affirms many instances of "this *A* is *B*" is insufficient to sustain the generality "all *A*'s are *B*." Fortunately, actual scientific practice does not use this unsustainable, bare form of enumerative induction. Inferences of this form are narrowly circumscribed by whether there is a specific warranting fact available. That fact in turn must be supported, ultimately, empirically, thereby ensuring that the reasoning draws only on experience. As an example, Curie's 1903 generalization was supported by Haüy's principle, which was in turn supported empirically by lengthy investigations in the nineteenth century.

[142] See Norton (2003)

5.3 What about THE problem of induction?

A more serious form of inductive skepticism is provided by Hume's problem of induction. In its modern guise, it asserts that no general rule for inductive inference can be justified. Such efforts at justification are either circular or trigger an infinite regress. The problem cannot be formulated within the material theory of induction since that theory employs no general rules for inductive inference. Norton (2024, Ch. 6) argues for the failure of efforts to recreate such vicious circularities or harmful regresses within the material theory of induction.

6. The Exclusivity of Experience

Section 4 above argued for the adequacy of empirical investigations to secure the evidence for our science. This adequacy is compatible with a weaker form of empiricism in which experience suffices to establish the contingent facts of a science, but leaves open the possibility of other modes of establishment. In this weaker version, we might accept that some specific, contingent result can be established empirically by arduous observational or experimental work. But we might also allow that the same result could be secured with less effort by, for example, a thought experiment whose formulation does not already tacitly include the result. Might there be some other, as yet undiscovered mode that equals or betters the performance of empiricism, we are left to ask.

The second clause of the statement of the core doctrine of small-e empiricism precludes this weaker form of empiricism. It asserts

> "… and the inductive support of experience is the only means of providing this support."

Establishing this exclusivity presents a greater challenge than merely establishing the adequacy of experience. For the latter is already apparent in the millennia of successes of empirical investigations in science. To establish this exclusivity of experience, we must now consider not just occurrent modes of investigation but speculation about the possibility of other modes; and then argue for their failure.

This section and the next summarizes the case for this exclusivity of experience. The first element of the case is the simple fact of history that no other successful mode has been found. Their enduring failure has been masked by the fact that their successes are retrospective. That is, they succeed only with results that have already been established empirically. Otherwise, they turn out to be repeatedly and reliably in error.

The second element is the opacity of these other modes. We are to suppose that they succeed in informing us of the world while having at best a thin account of how they do so. This contrasts with empiricism's success. It provides a detailed and well-supported account of the experiential processes that give us access to the world. Small-e empiricism's ontology has no place for the channels required by the other modes. Their failure is expected. This opacity means that these other modes have no internal resources to correct failed predictions. They can only say that their Platonic vision was clouded or that they had misunderstood how a metaphysics of simplicity is implemented in the world. These other modes must repeatedly defer to empirical affirmation of their result and, if there is a conflict, the empirical result will be favored.

6.1 The Failure of Known Alternatives: Successes are *post hoc*

A simple historical fact already gives us good reason to think that no other mode can succeed. We have tried over millennia to find such a mode, but without success. This is, I believe, the result of a survey of other modes. All of them that I have been able to identify have failed. These alternatives can be grouped loosely into three types, with some overlap between them. They are:

Platonic insight

Innate or intuitive ideas

Oracular revelation

Recounting the details of the failure of the individual modes requires a lengthier exposition and, for this reason, has been postponed to the following section.[143] Here, however, the main conclusion is easily stated. None of these alternative modes has been successful.

We can summarize here the way in which we find these other modes to fail. The failures may not be apparent immediately since the proponents of these other modes can claim successes. Their successes do not vindicate the modes, since they have been retrospective or *post hoc*. That is, they are able to assert, subsequent to empirical discoveries, that their modes affirm the

[143] Blum (2021) provides an analysis of John Wheeler's 1950s non-empirical physics. Blum (p. 219) diagnoses its failure as deriving from its "lack of empirical input," in conformity with the analysis given here.

discoveries. When we already know the answer, it is all too easy to convince ourselves that this is just what our favored non-empirical mode would tell us. When these modes offer results that go beyond what has been established empirically, their failures are frequent and sustained.

6.2 Empiricism Only Has the Capacity to Self-correct

Perhaps this dismissal of these other modes as merely *post hoc* is too hasty. Their proponents can concede that their modes have failed sometimes. However, they might say, this is no fatal error since the same is true of empirical methods. We have many cases of sciences that enjoyed strong empirical support but later proved to fail. The most important examples arise when try to apply a science in domains beyond those in which their evidential support is found. The familiar example is Newtonian mechanics. It fails when we consider motions with speeds close to that of light (special relativity); extremely intense gravitational fields (general relativity); large cosmic distances (relativistic cosmology); and the very small (quantum theory). All modes, including the empirical, fail sometimes.

There is a difference, however, that distinguishes empirical methods as singularly successful. Empirical methods have within them the means to correct their errors. When an experiential result errs, if enough information on the experiential process is available, we can locate precisely how the error arose through the "winding back" recounted in Chapter 10. In 1610 and the years following, Galileo misidentified the rings of Saturn. He reported that the planet has lobes or even "ears."[144] We now know that the misinterpretation resulted from the inability of Galileo's early telescope to resolve Saturn's rings.

The corrections are possible on a larger scale, when there are revolutionary changes in a science. Empirical methods tell us what is best supported by the evidence then available. That qualification allows that further evidence may require revisions to what is best supported. That sort of revision is possible and perhaps even expected when new evidence is drawn from new domains remote from those already explored. The familiar example is again the failure of Newtonian mechanics. It was revealed by empirical investigations that probed extreme domains, as sketched above.

The other, non-empirical modes do not have the means internally to provide for and explain the failures. They rely on empirical investigations to alert them to the problem. When an

[144] As recounted in Shea (1998, pp. 223-24).

error is found, they can only say that their Platonic vision was clouded or their intuitions misread. They must just concede the error. Perhaps they can then find *post hoc* how their methods might vindicate the new theories found empirically. They are intrinsically unreliable.

6.3. The small-e empiricists' Ontology Precludes Other Modes

From the perspective of small-e empiricism, the unreliability of these other modes is quite expected. The ontology of small-e empiricism circumscribes the ways that we can access systems of interest in the world. They are restricted to those that are identified as experiential processes here. Claims of other modes are unsupported by this ontology. Their successes, it follows, are fortuitous or covertly guided by what experience has already affirmed.

We are used to the ontology of science refuting claims of alternative modes of access to the world. A version of astrology asserts that the specific positions of celestial objects at the moment of a person's birth correlates in profound ways with that person's future. Astrophysics has identified the repertoire of interactions between these celestial objects and occurrences on earth. There are some connections. The connection between the positions of the Moon and Sun and the Earth's ocean tides is well-known. However, that repertoire is so sparse that it precludes the correlations asserted by this version of astrology.

Sortilege is a form of divination that seeks signs of future events in the outcome of simple physical processes like cards dealt from a shuffled deck (Tarot), the configuration of cast sticks (I Ching) or the results of die casts (divination in the ancient Greek world). Modern mechanics affirms that all these processes are effectively random. To someone with a modest knowledge of mechanics, the belief that the outcomes of these processes correlate with future events requires a willful credulity. It is especially so given that the full details of the mechanical process that produces the results are so fully exposed as to assure their effective randomness.[145]

For our purposes, the main result in the empirical study of sense perception is that its ontology powerfully restricts the range of sense experiences we humans can have of the world. The ontology precludes a parapsychology that proposes further modes of sense experience. These further modes include telepathy, which proposes an addition channel of communication between minds, and clairvoyance, through which agents have the ability to "see clearly" in way

[145] Norton (2022, §4.3, §5.2) provides more details of these forms of divination within an analysis of notions of chance outside probability theory.

that goes beyond the five senses. To someone who accepts the ontology of senses supplied by mainstream psychology, the notion of "ESP," extra-sensory perception, is an oxymoron.

6.4 The Priority of Experience

In the light of the considerations so far make, it surely is futile to harbor the hope that some as yet undiscovered alternative mode may appear. Were such a mode to be found, we have reason to believe that it would be subordinate to empiricism.

The reason is that empiricism enjoys a priority over all other modes in that the results of any other mode is overruled if they contradict experience. No matter how erudite a mode's assurance that there can be no duck-billed platypus, its failure is assured when a specimen emerges. Such overruling has been a frequent occurrence in the history of science. A few examples, in addition to those in earlier sections, illustrate it.

Johannes Kepler, in his 1596 *Mysterium Cosmographicum*, described a geometric construction in which the five Platonic solids were nestled between six spheres. The sizes of these spheres matched the orbits of the six planets then known. This Platonic recovery of the orbital size and number of planets fared poorly under the subsequent empirical discoveries of more planets and the existence of an asteroid belt between the orbits of Mars and Jupiter.

Modern physics supplies many examples of how intuitions have been overruled by new, empirical discoveries. Einstein's 1905 special theory of relativity conflicted with long-standing presumptions about time. It entailed that whether two spatially separated events are simultaneous depends on the inertial frame of reference in which their timing is judged; that moving clocks are slowed; and that observers in relative motion will judge each other's clocks to have slowed. These results are now treated as elementary facts to be learned by any student of physics. When Einstein proposed them and for many to the present day, they so contradict intuitive expectations as to make them inadmissible.[146]

Quantum theory provides a more extreme example. According to its standard interpretation, atomic processes are indeterministic: the full specification of the present state allows only a probabilistic determination of future states. Particles may interact non-locally, that is, with processes whose speed of propagation exceeds that of light; and atomic states can be

[146] See Marder (1974) for a survey of just a small portion of the immense literature that unsuccessfully challenges special relativity.

non-separable, that is, they need not be fixed by properties assigned to a single event or even small neighborhood in spacetime. Einstein himself was repelled by these features and remarked:[147]

> It seems hard to sneak a look at God's cards. But that he plays dice and uses 'telepathic' methods (as the present quantum theory requires of him) is something that I cannot believe for a single moment.

There is no converse priority. Other modes of investigation cannot overrule experience. This asymmetry is so familiar that it has been encoded in Thomas Huxley's much-repeated riposte of 1870. It concerned the empirical refutation of Buffon and Needham's theory of "Xenogenesis" (as Huxley called it) for the creation of life (1870, p. 402):

> But the great tragedy of Science—the slaying of a beautiful hypothesis by an ugly fact—which is so constantly being enacted under the eyes of philosophers, was played, almost immediately, for the benefit of Buffon and Needham.

In more recent times, no matter how greatly our intuitions rebel against the odd results of quantum theory, that rebellion alone is no match for empirical support. If these odd results are to be overturned, it will be through further empirical investigations.

7. Illustrations of the Failure of non-Empirical Modes of Investigation.

7.1 Platonism

The history of Platonism in science is one of retrospective successes and prospective failures. This is already evident in the earliest applications of Platonic ideas. A matter theory plays an enduring role in the cosmology of Plato's *Timaeus*. Plato, speaking through his character Timaeus, supposes that the elements of matter are each made up of particles too small to be seen by us (Plato, 1888, p. 201). Plato is then able to recover the four elements, earth, air, fire and water, that were then supposed to form the basis of all terrestrial matter. He achieved this by finding: (p. 191)

> …the four fairest bodies that could be created, unlike one another, but capable, some of them, of being generated out of each other by their dissolution …

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[147] In a letter to Cornelius Lanczos, March 21, 1942, Einstein Archive, 15-294; As translated in Dukas and Hoffmann (1979, p. 68).

Plato reassures us that this is no idle play, but a serious attempt at finding the true theory of matter. He continued the above quote with[148]

> … for if we succeed in this, we have come at the truth concerning earth and fire and the intermediate proportionals.

The "fairest" bodies are the five Platonic solids, four of which are then associated with the terrestrial elements as (p. 199):

> fire—tetrahedron
> air—octahedron
> water—icosahedron
> earth—cube

With this association, Plato had recovered retrospectively why there were then thought to be just four terrestrial elements. The awkwardness that there are actually five Platonic solids is escaped by associating the remaining solid, the dodecahedron, with the cosmos as a whole (p. 197).

That this was a serious theory of matter is affirmed further by Plato's extensive use of the theory to accommodate, retrospectively, known properties of matter. The narrative recovers at some length, page after page, many of the familiar properties of terrestrial matter. These properties include how matter behaves under melting, flowing, cooling and solidification (p.213) and how the matter theory accommodates the interactions of matter with living organisms, such as the origin of the sensations of colors (pp. 247-53). The accommodations are easily formulated and, to an adherent of Plato's theory, likely plausible. For example (p. 291):

> Fire is composed of finer particles than any other element, whence it penetrates through water and earth and air and whatever is composed of them, and nothing can keep it out.

So much for the retrospective successes. They just return properties of matter already evident in experience. The account fares poorly prospectively, that is predictively. In assigning the fifth Platonic solid, the dodecahedron, to the cosmos, it mistakenly predicts that the matter of the heavens is unlike terrestrial matter. Plato's narrative includes a rudimentary theory of elementary transformations. Fire, air and water are intertransformable since their primary shapes are composed of triangles. No such transformation is possible for earth since its primary shape,

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[148] This appears to be an early instance of inference to the best explanation.

the cube, is composed of squares. For example (pp. 203-205), if the particles of water are divided by fire or air, they may recombine into various combinations fire and air. Earth, divided by fire, can only return to earth (p. 203).

It is hard to know what to make of these predictions, since there is considerable ambiguity in associating the four elements with substances we now know. The carbon of a diamond, the hardest of all minerals, if set upon by fire and air, resolves into something entirely gaseous, carbon dioxide. Is this a transformation forbidden by Plato's theory of earth to air? In all this ambiguity, one thing is certain: Plato's analysis failed to make definite predictions that conform with later empirical results.

It is tempting to dismiss these failures as inevitable at such an early stage of the development of science. Is it not just unkind but anachronistic to expect anything better? My point, however, is that this pattern persists to the present. For their entire history, attempts at Platonic insights succeed retrospectively and fail prospectively. Einstein provides a more modern example. His early, successful work in physics conformed with empiricist expectations. Its successes depended on an astute sense of the import of experimental results. After his completion of the general theory of relativity in the mid 1910s, he shifted towards a mathematical Platonism that found its fullest expression in his Herbert Spencer Lecture, delivered at Oxford on June 10, 1933.[149] There he proclaimed (1933, p. 274):

> Our experience hitherto justifies us in believing that nature is the realization of the simplest conceivable mathematical ideas. I am convinced that we can discover by means of purely mathematical constructions the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena.

Even more striking is Einstein's explicit acknowledgment (p. 274):

> In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed.

Just as Plato sought the fairest shapes for his elements, Einstein now sought the mathematically simplest expressions for the fundamental laws of physics. The retrospective successes were

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[149] For further historical details, see Norton (2000) and van Dongen (2010, Ch. 2).

immediate. Einstein could claim that versions of Maxwell's electrodynamics and his own general theory of relativity conformed with this Platonism (p. 274):

> The physical world is represented as a four-dimensional continuum. If I assume a Riemannian metric in it and ask what are the simplest laws which such a metric can satisfy, I arrive at the relativistic theory of gravitation in empty space. If in that space I assume a vector-field or an anti-symmetrical tensor-field which can be derived from it, and ask what are the simplest laws which such a field can satisfy, I arrive at Maxwell's equations for empty space.

This new outlook was then supposed to prove itself prospectively as the secure guide to his unified field theory. The program proved sterile prospectively. Einstein toiled in vain for over two more decades in the search for his unified field theory up to his death 1955.

These are only two episodes within a long-standing tradition that ascribes a mathematical structure to the world. It encourages the idea that mathematical explorations are a fertile means of learning about the world, in advance of experience. As a general matter, the successes of these means are retrospective. They provide no basis for concluding that mathematical investigations are a means of discovery of facts of the world, *prior to experience*. For further discussion, see Norton (2000, Appendix D; 2024, Ch7, §7.4).

One further example is a striking illustration of the purely retrospective success of a priori reasoning. In the 1930s, the eminent physicist, astronomer and cosmologist, Arthur Eddington, created an expansive theory intended to unify cosmological and quantum-scale phenomena. It was developed as a purely deductive theory. Eddington (1936, p.5) introduced it as "I think it will be found that the theory is purely deductive, being based on epistemological principles and not on physical hypotheses." Eddington argued that, by considering large number coincidences and other factors, he could deduce the Hubble constant, which measures the rate of recession of the galaxies. He wrote (1933, p. 94):

> According to the argument here developed we can calculate by pure theory what ought to be the speed of recession of the spiral nebulae. … Since certain small factors in the formulae are at present left in suspense, there is a temporary indefiniteness; but we can say provisionally that the result is between 500 and 1000 km. per sec. per megaparsec.

Eddington then emphasized how this recovery of a result up to then only found by laborious astronomical measurements was a striking confirmation of his theoretical analysis.

> No astronomical observations of any kind are used in this calculation, all the data being found in the laboratory. Therefore when we turn our telescopes and spectroscopes on the distant nebulae and find them to be receding at a speed within these limits the confirmation is striking.

Where Eddington found this a striking confirmation of the powers of his purely theoretical reasoning, it is apparent to us that it is yet another illustration of how a priori reasoning can only recover results retrospectively, but not prospectively. For Eddington's value of the Hubble constant of "between 500 and 1000 km. per sec. per megaparsec" just happens to agree with the value that Hubble had found observationally in Hubble's 1929 work. We saw in Chapter 10, Section 7, that Hubble's value for the constant was roughly an order of magnitude too large. Eddington's analysis was only as successful as the retrospective results to which he was, perhaps unwittingly, adapting his reasoning.

To preclude any misunderstanding, I have no doubts about the value and even necessity of mathematical descriptions of physical facts and of mathematical formulations of physical theories, once their factual content has been discovered empirically.

7.2 Innate or Intuitive Ideas

That we know facts in the world through some prior, innate or intuitive faculty of the mind has, traditionally, been denied by empiricists. This denial forms the starting point and guiding theme of Locke's (1689) *Essay Concerning Human Understanding*. Chapter II is entitled "No Innate Principles in the Mind." Locke's denial is more expansive than that needed for a scientific empiricism, for Locke's concern extends beyond mind independent facts in the world to mental states such as the experience of colors.[150]

That we have some innate or intuitive sense of mind-independent truths of nature has an enduring presence. The most familiar instance is Euclid's foundation for geometry. He based it

[150] Here Locke's empiricism intersects with issues that are properly subject to empirical investigation in psychology. Whether we are born with an innate capacity to recognize faces remains, I believe, a subject of debate in modern psychology.

on a short list of postulates and axioms, whose truth were supposed self-evident. Here is how the founding postulates of Euclid's geometry were introduced in the first English edition of Euclid's *Elements*, Billingsley (1570).[151] They are…

> … certain general sentences, so plain, & so perspicuous, that they are perceived to be true as soone as they are uttered, & no man that hath but common sence, can, nor will deny them.

The axioms—"common sentences" for Billingsley--were introduced with similar assurances:

> … certaine general propositiós, commonly known of all men, of themselves most manifest & cleare, & therfore are called also dignities not able to be denied of any.

These postulates and axioms then provided sufficient basis for the deductive recovery of the geometry found by practical measurement. In so far as these postulates and axioms are self-evident, prior to experience, they implement an alternative to empiricism.

As we all know, the self-evidence of Euclid's founding principles proved fragile. In the 19th century, alternative geometries were discovered as physical possibilities, in contradiction with Euclid's postulates. In the 20th century, Einstein's general theory of relativity established that our actual geometry deviated from that of Euclid. The axioms proved similarly fragile. The cardinal measures in Cantor's transfinite set theory required a denial, or at least a refinement, of Euclid's axiom "Every whole is greater than his part."

The pattern set by Euclid has been replicated throughout the history of our science. Once a new science is established from experience, its foundations are scrutinized to find its simplest and most appealing formulation. The result is often presented as establishing the necessity of the particular science. The foundation is judged to be so natural as to be self-evident and thus knowable prior to experience, if only we had taken the trouble to conceive it.

Olivier Darrigol's, *Physics and Necessity* (2014) is a capacious inventory of such attempts, drawn from work over the last five centuries. Its 390 pages recounts examples from mechanics, prior to Newton, in Newton's time and in the later development of classical mechanics, in geometry, in spacetime theories, in numbers and mathematics, in classical field theories and in quantum mechanics. There are so many examples that I assert only with slight

[151] Billingsley's edition is unpaginated. The quoted passage follows the statement of the first postulate, called by Billingsley "peticions or requestes."

hesitation that *all* of them are *post hoc*. Empirical investigations have done the work and they are now wise after the fact.

Kant's epistemology is perhaps the most familiar example of this approach in philosophical writing. He successfully recovered retrospectively key physical elements of the new Newtonian science. Most prominently, he gave an absolute assurance of the eternal security of Euclid's geometry. That was a lesser challenge when that eternal security was already the default expectation. His plumbing of intuition through the postulation of the synthetic a priori proved to have meager prospective powers when developments in geometry and physics in the 19th and 20th centuries overturned Euclid's geometry empirically.

7.3 Conceivability

The modern incarnation of innate or intuitive ideas in the philosophical literature is conceivability. It is vested with great powers. Most prominently, what is conceivable reveals to us what is metaphysically possible. Whether conceivability truly has such powers remains a matter of great debate in the philosophical literature. In their introduction to a collection of papers on the topic, the editors, Tamar Gendler and John Hawthorne, after some preliminaries, lay out a challenge to be pursued in in the volume (Gendler and Hawthorne, 2002, p. 6):

> While these clarifications dispel a certain amount of confusion, they do little to resolve an obvious puzzle: on the face of it, the idea that conceivability is a guide to metaphysical possibility is extremely problematic. According to current orthodoxy, metaphysical possibility can neither be reduced to, nor eliminated in favour of, linguistic rules and conventions; it constitutes a fundamental, mind-independent subject-matter for thought and talk. Given this picture, it is rather baffling what sort of explanation there could be for conceiving's ability to reveal its character. It seems clear that the causal explanation for the reliability of perception is unsuitable here- and it is profoundly difficult to see what to put in its place.

In my view, the challenge is devastating and unanswerable and the remainder of the volume struggles to meet it. The problems have already been rehearsed in Gendler and Hawthorne (2002) and elsewhere (Norton, 2022a)), so only brief mention of them is needed here.

The supposed relation of conceivability to metaphysical possibility is fatally troubled on both sides. First, take conceivability. It is untroubling, in so far it merely affirms logical compatibility or restates something well founded in experience. If it seeks to do more, however, the notion is too inchoate to serve the weighty role attributed to it. The well-known difficulty is that it is often as easy to conceive some result as to conceive its negation. The relation would then tell us that the result is both metaphysically possible and metaphysically impossible. We can also conceive results known assuredly to be false.[152] The optimistic response is to undertake a convoluted quest for just the right, restricted form of conceivability that would escape the plethora of troubling counterexamples. The more realistic response—my view—is to accept what should have been obvious from the start. Conceivability is just too inchoate a notion to serve the weighty ends sought. There is even a tinge of magical thinking to it: if I truly believe it so in just the right way, then it is so.

Second, the notion of "metaphysical possibility" is similarly troubled. It is defined poorly by suggestive metaphors, commonly concerning limitations on the powers of fictional gods. Metaphysicians struggle to find any cogent instances. Or so I have argued in Norton (2022a, §12). It seems to me to be a fiction invented by metaphysicians. The literature on metaphysical possibility looks to me like tomes on the biology of unicorns. There might well be many, learned treatises debating whether unicorns are ruminants. They must come to nothing since no amount of learned debate can wrestle anything factual from such an invention of fiction.

Thought experiments have provided a convenient vehicle for proponents of the three modes above: Platonism; innate or intuitive ideas; and conceivability. Superficially, through a thought experiment, we seem able to learn contingent facts by mere introspection without empirical investigations. That is just what each of these modes purports to provide. In my view, thought experiments only provide the illusion of non-empirical access to contingent facts. A closer analysis—one that I have defended for roughly four decades—shows that thought experiments are merely picturesque arguments. They can only licitly deliver contingent truths if those truths are the conclusions of cogent reasoning in the thought experiment from suitably rich, contingent premises that may or may not be explicit in its set up.

7.4 Oracular Revelation

In this last mode, deep truths of the world are supposed to be revealed through the pronouncements of an enlightened person or the writings within a sacred text. The veracity of the

[152] In my mind's eye, I see a neo-Pythagorean who, after millennia and a computer's assistance, has finally found a ratio of enormous whole numbers equal to the square root of two. Eureka!

revelations is supported by a narrative that may draw on divine interventions or by some other mystical authority. Since I doubt that these sorts of revelations are now taken seriously as modes of discovery in science, only a few brief remarks are needed here, lest I advance invidious critiques. The content delivered by these modes, once again, conforms with what was retrospectively already accepted ideas at the time of their formulation. They provide little prospectively. Creation narratives, for example, typically conform with what are plausibly the best guesses of then contemporary peoples. If the narratives provide medical advice, they are again reflections of common ideas and often notable for their omission of what are now rudimentary elements of public health policy.[153]

7.5 "Non-empirical theory assessment"

Richard Dawid has written about (2013, *passim*) "non-empirical theory assessment" and (2022, p. 61) "non-empirical confirmation."[154] One could be forgiven for assuming that these terms connote a challenge to empiricism.[155] Dawid's account turns out to be the reverse. It is a resounding endorsement of the strength and reach of empirical evidence. This is evident from Dawid's initial account in his paper. His concern (2006, §2) there was to dispute what he called the "scientific underdetermination principle," which asserts a limitation on the reach of evidence by assuming that there are always competing scientific theories, adequate to the empirical data. Using the example of string theory, Dawid argued that the existing empirical evidence in particle physics was *already* powerful enough to determine string theory without the need for novel empirical evidence that addresses string theory specifically. This followed from further considerations, such as the lack of viable alternatives to string theory, a parallel to the development historically of the standard model of particle physics and the internal coherence of string theory. In characterizing his account as identifying "purely theoretical theory

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[153] A popular riposte is that ancient health guides would have done well to advise "boil water!"

[154] Dawid (2022, p. 61) then replaced this last term with one he deemed more specific: "meta-empirical confirmation."

[155] Chapter 6 recounts the strong negative reaction that arose after publication of Dawid (2013) and was expressed in the workshop, "Why Trust a Theory? Reconsidering Scientific Methodology in Light of Modern Physics," held in Munich in December 2015.

confirmation," Dawid (2006, p. 319) was careful to qualify the claim with a footnote recalling the importance of observational support:

> This, of course, includes indirect empirical support, that is, those observations that have led to the prior set of theories from which the theoretical scheme in question has been construed.

Similar qualifications appear in Dawid (2013, p. 3):

> While these arguments can be called non-empirical in a certain sense, they are nevertheless rooted in observation and may be understood in terms of an extension of the conventional horizon of observational input.

And again (2013, p. 38):

> The term "non-empirical" clearly does not imply that no observation or no empirical data has entered the argument.

Rather, empirical evidence is distinguished as data that can be predicted by the theory, whereas non-empirical evidence provides support without being predicted by the theory.

8. Science is Empirical

8.1 Preclusion of Empirically Inaccessible Propositions

One of the notable and most familiar consequences of traditional empiricism can be summarized in the slogan used above, "Science is empirical." More expansively, empiricism asserts a blanket exclusion from science of contingent propositions that, by their construction, can never be inductively supported by experience.

Small-e empiricism retains this exclusion. A form of it, congenial to small-e empiricism, arises through the following question. Are there sciences of such a peculiar nature that they elude empirical support, that is, they elude inductive support by experience? Small-e empiricism entails that such bodies of propositions do not constitute a science in the sense of being a well-systemized and well-supported collection of contingent facts.[156] It is easy to formulate empirically inaccessible, contingent propositions that at least superficially appear to belong within a science. Such propositions preclude, by supposition, inductive support by experience,

[156] Mathematics is sometimes described as a science to reflect its rigor and precision. It is not a science in the sense indicated here since its propositions are not contingent facts.

which is the *only* means through which they could be supported, according to small-e empiricism. Hence, they must remain an unknowable speculation, to put it kindly, or a fanciful hunch, if we are less kind.

For example, in a highly speculative cosmology, we might posit alternative universes that make no physical contact with our universe and whose physics is stipulated to be so different from ours that we cannot apply results recovered in our universe to these other universes. The contingent propositions of such a cosmology are excluded from science in virtue of their lacking the possibility of empirical or any other form of support.

8.2 The Logical Positivist's Meaninglessness is Too Strong

In a severe formulation, such empirically inaccessible propositions were the favorite target of the logical positivists. In his "The Elimination of Metaphysics Through Logical Analysis," Carnap (1931, p. 237) offered the example of propositions concerning Hans Driesch's neo-vitalist "entelechy." It is an animating spirit whose understanding cannot be provided in physical terms. Logical positivists, such as Carnap, used their verifiability principle to denounce such propositions as meaningless metaphysics. Their elimination was a major goal of the logical positivist movement and a defining characteristic of the empiricism of the Vienna Circle.

Small-e empiricism also deprecates such metaphysics, but not by the Draconian resort of judging them to be meaningless. That reduces them to something comparable to an uncommunicative grunt or drool. Propositions about alternative universes and life spirits are not completely meaningless. Although they may not meet the standard of a refined philosophical account of meaning, they have a meaning at the level of informal discourse. Ordinary language users understand more from such propositions than is conveyed by a grunt or a drool, even if they may be unable to articulate fully the state of the world portrayed. Small-e empiricism simply remains silent on such propositions. They do not lie within the scope of the sciences.

8.3 No Demarcation Criterion

This preclusion of empirically inaccessible propositions from science is not intended to provide a demarcation criterion for science, such as promoted by Karl Popper with his criterion of falsifiability. The problem with such criteria is that they replace delicate matters of degree with oversimplified, absolute declarations. This body of work, a criterion will proclaim, is science; that body is not. Familiar declarations of this type are detrimental since they are too cheap and too open to misuse. A dubious enterprise can concoct scenarios, even if fanciful and remote from realization, in which its propositions could accrue empirical support or empirical falsification. The enterprise is thereby ennobled as "science." This is so even if its propositions are irresponsibly speculative; its empirical support is meagre; or if its propositions are falsified.

What matters is not whether some body of propositions is labeled "science" or not, according to some philosopher's definition. What matters is the strength of empirical support for the body. We prize those that enjoy the strongest support for the fact of this support. For then we have good reason to accept the propositions; and, if we are rational, we should do so, no matter how the label "science" has been distributed. For more discussion, see Norton (2026).

8.4 Laws of Nature

There is a traditional three-part division of propositions in science into mere contingent truths, logical necessities and laws of nature. These last propositions are not truths of logic or merely accidental truths but have an intermediate status. They acquire their truth from some overarching necessity in nature itself.

Small-e empiricism has no place for laws so understood.[157] Nothing empirical distinguishes such a law of nature from a mere generalization of very broad scope. The inclusion of laws as a third class of truths is an unwanted intrusion into empiricism of a non-empirically grounded metaphysics. It contradicts small-e empiricism, which only admits two classes of truths: the contingent and the logically necessary.

The closest small-e empiricism has to laws are contingent propositions of very wide scope that are strongly supported by empirical evidence. This view is similar to but weaker than a traditional regularity view of law.[158] Generic regularity accounts commonly require some form of universality, such as no restrictions in the laws to particular places and times and to proper names. They are to be exceptionless.

That there are such unrestricted universal truths is an enticing supposition and it may well be the case. We have yet to find any. Although we have found many regularities of broad scope,

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[157] I thank Howard Sankey for pressing me on the need for this discussion of laws.

[158] Ayer (1956, p. 154) "… a proposition expresses a law of nature when it states what invariably happens. … The 'necessity' of a law consists, on this view, simply in the fact that there are no exceptions to it."

none of them are exceptionless. Our present quantum theory fails for strongly gravitating systems. Our best theory of gravity, general relativity, fails for processes in a quantum regime. Our chemical laws are restricted to matter in domains of sufficiently low energy that the familiar chemical species can form. Our best laws of biology are restricted to life forms that happen to manifest on our planet Earth. The lack of universality grows more apparent as we proceed through the sciences. Psychology is restricted to humans specifically or, at its more expansive, to other sentient life forms on our planet Earth. Sociology is restricted to the collective organization of Earth-borne humans. In short, that there are universal truths of unlimited scope that can serve as laws is a speculation so far not borne out by experience.[159]

Efforts to repair a regularity account of laws while keeping within the spirit of empiricism have not fared well. The best known of these accounts is David Lewis' "best systems" account. It distinguishes laws as those regularities that achieve the best balance of two requirements in tension: simplicity and strength. Lewis's compact formulation is: (1986, p. xi)

> Few would deny that laws of nature, whatever else they may be, are at least exceptionless regularities. … I suggest that the laws are the ones that buy into those systems of truths that achieve an unexcelled combination of simplicity and strength. That serves the Humean cause.

Lewis' starting point is quite amenable to empiricism.[160] It is Lewis' "Humean mosaic." The world just consists of a huge collection of specific facts. His formulation is: (Lewis,1986, p. ix)

> [Humean supervenience] is the doctrine that all there is to the world is a vast mosaic of local matters of particular fact, just one little thing and then another.)

We can, if we are so minded, ask which generalizations over these facts best balance simplicity and strength. Justifying our answers has proved to be notoriously difficulty since we have no precise, serviceable account of simplicity and no correspondingly precise account of the balancing.

[159] We can use conditionals to produce an illusion of universality with antecedents such as "For systems outside the quantum regime, …" or "For lifeforms on Earth …" They simply affirm the lack of universality by conceding the exceptions.

[160] See my reservations in Chapter 2, Section 0.

The real difficulty, however, from an empiricist perspective, is to discern what is achieved by the identification. I see no foundational gain. If the facts of the world are exhausted by the Humean mosaic, then all such laws do is to provide a summary of them. Some summaries are better than others, but there is no added discovery of foundational importance in identifying the best of them. Such summaries have no special value other than as conveniences to we humans struggling to comprehend the complications of the mosaic. Alternatively, we might conceive the project as seeking a formula that picks out the regularities that scientists happen to designate as laws. It provides an interesting technical challenge, but has no special value without an independent analysis that shows why these selections have foundational import.

9. Empirical Discovery

9.1 Discovery and Justification are Intertwined

The doctrine of small-e empiricism, as articulated and defended so far, applies to the justification of the contingent propositions of a science. It makes no assertion directly about the process of discovery. If, however, we pay attention to how successful scientific discoveries are actually made, we find that a close engagement with experience is an essential element in successful discovery processes. Science is empirical in both discovery and justification.

This follows from fact that discoveries in science are not made in one momentous act of insight in which a profound truth of science springs forth fully formed in an instant. Rather discovery in science occurs through a gradual exploration that has many intermediate steps and many intermediate results. To succeed, the exploration must be controlled repeatedly by checks on whether it is heading in the right direction. Whether these intermediates are doing so is a question of justification that requires a reliable mode of justification. It follows that discovery and justification are intimately intertwined. The prospects for successful discovery are greatly enhanced if the mode of justification employed is reliable, that is, is empirical. The prospects are poor for a discovery process whose checks are guided by a mistaken mode of justification.

We have seen an example in Section 7 above of how a poor mode of justification can compromise a discovery process. Einstein's quest for a unified field theory was guided by the mistaken supposition that the truth of a physical theory is revealed by its great simplicity. This supposition replaced what had been the distinctive characteristic of Einstein's earlier successes:

he was guided by his extraordinary ability to see the deeper import of experimental results. When that guide was replaced by the guide of simplicity, Einstein's search ceased to be productive.

That the incremental character of scientific discovery is not generally recognized results at least in part from a practical need for commentators. It is to provide terse and simple accounts of discoveries in science. There are, to be sure, moments of extraordinary discovery in science that do leave us in awe of a scientist's intellectual prowess. However, I fear that the creative powers of the scientist's pure intellect are commonly exaggerated. An abbreviated history of discovery is easier to narrate if the many, messy steps of the real events are collapsed into a heroic tale of inscrutable genius. My sense from my work in history of science is that our *unfettered* imagination is rarely productive in science. Rather, what appears in summary as great leaps of the intellect are, in reality, the slow accretion of many small advances; and, crucially, those small advances are powerfully constrained by experience. Here are some examples.

9.2 Examples of Discovery Processes

Einstein's discovery of special relativity is justly celebrated as a premier instance of extraordinary intellectual creativity in science. It is precisely because of my admiration for his work that I have studied closely how he came to his discovery. The core conceptual innovation was the relativity of simultaneity. If we compare Newton's Absolute Time with Einstein's relativity of simultaneity in a single step, it is a breathtaking leap. Einstein, however, struggled for seven or more years with problems in electrodynamics before he hit upon the idea. As I have reconstructed in some detail in my Norton (2004), Einstein had become convinced on empirical grounds of the principle of relativity and sought a way to implement it in a modified electrodynamics. After all his efforts had failed, he realized, in an historic moment of insight, that the relativity of simultaneity solved the problem without a modification to electrodynamics. Here Lorentz' theorem of corresponding states provided the mathematical expression of the result needed. Einstein merely had to connect the spatially dependent time coordinate of the theorem with the true time measured by clocks. Lorentz' theorem was in turn a consequence of Maxwell's empirically well-founded electrodynamics.

Other celebrated examples in modern physics follow this pattern. In the mid 1920s, quantum mechanics emerged in a roughly modern form. Its core innovation was the notion of a quantum state, be it represented by Heisenberg and Born's matrices or Schroedinger's waves. That this state would be the core of the new theory did not arise in the physicists' minds in an instant. It was the accumulation, in the quarter century preceding, of many small steps, each being driven further from classical conceptions by empirical consideration. Heisenberg's pivotal "*Umdeutung*" paper (1925) introduced his matrices explicitly as a summary of the empirically determined amplitudes for transitions in electron energy states in a hydrogen atom. These amplitudes were in turn read from empirically measured emission spectra.[161]

10. Connection to Belief

Small-e empiricism differs from much of the literature on empiricism in making no essential use of notions of belief or knowledge. Where a more traditional empiricism might consider whether we should believe some proposition in science, small-e empiricism asks only after the inductive support provided for the proposition by experience. Its burden is only to specify the nature and extent of evidential support from experience for the contingent propositions of a science and how the requirement of inductive support from experience circumscribes the scope of such propositions. Here the analysis conforms with the primacy afforded to inductive inference over belief in my earlier works (2021, 2024) on the material theory of induction.

There is, however, an important connection to belief formation that lies outside the strict formulation of small-e empiricism. It comes *after* relations of inductive support have been established. That some contingent proposition is well supported inductively must have some further import or else the declaration is a mere curiosity of inductive logic. That further import concerns our beliefs and is provided by Hume's (1894, p. 110) celebrated maxim:

> A wise man, therefore, proportions his belief to the evidence.

In offering this maxim, I ignore Hume's inductive skepticism and suggest that the maxim be read independently of it. I read the maxim as directing us to accord belief in propositions according to the strength of their inductive support from the evidence of experience. There is no need for a scientific empiricism to elaborate. For that would enmesh scientific empiricism in debates that

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[161] Einstein's discovery of general relativity is, with justice, taken as a supreme instance of a scientist's creativity. Even in this most extreme case, careful historical scrutiny shows how Einstein took many small steps towards the final theory, with each being powerfully controlled by empirical considerations. The story is too hard to summarize in a short paragraph. Some details are in Norton (2020).

are best left to epistemologists. How should our beliefs be structured? Should they be organized as probabilities? How should evidential support be incorporated in them? Just when do those beliefs rise to the level of knowledge?

These are questions worthy of philosophical analysis. However, they have engendered vast, tangled and, in my view, inconclusive literatures. There is no need to encumber a scientific empiricism with these unresolved problems, when precise statements of small-e empiricism can be formulated without them.

The deepest reason for eschewing the analysis of beliefs, their interrelations and their modes of formation is this. Since beliefs are mental states, we should not expect armchair philosophical reflection to discern their natures. States of mind are properly the province of empirical psychology. Armchair speculation has proven over millennia to be incapable of anticipating the results of empirical investigations. This limitation is the reason, I believe, for the unsatisfactory character of much of present-day epistemology. It seeks results in an empirical science by relying merely on anecdotes and introspection, as opposed to the systematic empirical investigations that psychology requires if it is to be a science.[162]

11. The Rationalism-Empiricism Debate

11.1 The Seventeenth Century Debate

The issues raised in this chapter intersect with those of a classic debate in philosophy between empiricism and rationalism.[163] Conceptions of rationalism, prevalent in the seventeenth century, combine three modes discussed above: Platonic insight, innate or intuitive ideas and oracular revelation. Nelson (2005a, p.4) provides this characterization:

> Traditional rationalisms identify the intellect, the mind, or the rational part of the soul (or even the State) as of primary importance in receiving and holding knowledge. The corresponding objects of knowledge are then nonsensory, general, and unchanging or eternal.

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[162] However, a later chapter will explore the *import* of small-e empiricism for these debates over the nature of knowledge.

[163] Some sense of the reach of the debate from the rationalist perspective is supplied by the articles collected in Nelson (2005b).

The third, oracular revelation, arises through the theological entanglements of seventeenth century rationalism.

A more complete analysis of the exclusivity of experience may well require a full engagement with this debate. However, the literature on this debate is immense and well beyond what can be accommodated here. My feeling, however, is that much of this classic debate lies outside the issues directly relevant to the present concerns of scientific empiricism. The debate is largely—though not exclusively—focused on disputes arising among rationalists and empiricists in the seventeenth century. The debate readily mixes concerns we would now identify as scientific with others in theology that lie outside modern science. A major focus is the mental life of agents over such issues as whether and how certain notions can come to be conceived in the mind. These concerns are antecedent to those of scientific empiricism, whose starting point is experience once it has been formulated in mind-independent propositions.

11.2 The Example of Infinity

The differing views on infinity in the seventeenth century provide a mere glimpse of the concerns of this venerable debate. René Descartes (1641, pp. 25-37) famously employed the notion of infinity in his third meditation as part of his proof of the existence of God. He argued that we finite beings cannot independently form a concept of infinity. Our concept must be caused within us by something external. Descartes then invoked a causal principle: "…that there must be at least as much reality in the total and efficient cause as in its effect." It followed, Descartes argued, that our concept of infinity was imprinted upon we finite beings by an infinite being. Elsewhere, in his *Principles of Philosophy* (1644, Prop. 26, 27; pp. 13-14), Descartes reserved the term "infinite" solely for God. We are instead to describe quantities that always admit of extension as "indefinite."

John Locke (1689, p. 67), in his *Essay Concerning Human Understanding*, regarded the mind as a blank sheet of white paper upon which experience writes. He was compelled to doubt that (pp. 62-63) "the infinitely wise God …should be supposed, to print upon the Minds of Men, some Universal Principles…" The notion of infinity was a special concern and discussed at length in Book II, Chapter XVII. His notion of infinity was similar to Descartes' indefinite. Infinities in "Space, Duration [of time], and Number," as he labeled them (p. 167), resided merely in their indefinite extendibility. He denied that we could conceive of an actual infinity of space (p. 171, Locke's emphasis):

> … but to have actually in the Mind the *Idea* of a Space infinite, is to suppose the Mind already passed over, and actually to have a View of all those repeated *Ideas* of Space, which an endless Repetition can never totally represent to it: Which carries in it a plain Contradiction

These considerations are of immense importance in the history of philosophy and merit careful historical analysis. Now, centuries later, and with the benefit of the labors of many subsequent thinkers, the concept of infinity presents no foundational difficulties to the mainstream of present science. Its modern discourses no longer worry about whether a theology might require us to restrict the notion of an actual infinity solely to God. They do not fear that a mind conceiving an actual infinity is committing to a contradiction in requiring the mental act of completing something incompletable.

In present science, the concept of infinity is untroubling. Whether the interactions among bodies proceed infinitely fast is a matter of empirical investigations and is answered negatively by relativity theory. We ask of modern cosmology whether space is infinite in extent and whether time extends infinitely into the past and the future. We expect these questions to be answered empirically without any concern that merely to ask about infinity makes the questions illicit. We no longer fear infinity. Cantor's nineteenth century transfinite set theory has acquainted us with elaborate hierarchies of infinities that would surely have overwhelmed seventeenth century thinkers.

11. Conclusion

This chapter's goal has been to make the case for small-e empiricism. The single most important fact favoring it is that empiricism has an unmatched record of success in informing us about how the world is. Other modes have been tried, but they fail to match this record of success. Rather their successes, when they have them, are retrospective. They are wise after the fact, when empirical investigations have already shown the way. They have no prospective powers. Empiricism has a privileged authority. If some other mode offers a new result that contradicts experience, no matter how confidently, because it contradicts experience, the result is discarded.

Small-e empiricism is distinctive in being able to provide a detailed and well-supported account for why it succeeds where other modes fail. For, through small-e empiricism, we recover an ontology that shows us how the processes of experience connect reliably to the systems of interest in the world. Further, the ontology assures us, they are the only processes that do so. This ontology gives us an unmatched control of our scientific investigations. It allows us to specify in great detail, if we wish, just what supports any particular result in a mature science. If errors arise, we can examine the experiential and inferential processes that led to the error and determine precisely how the error arose and how to correct it. There is no correspondingly rich account for how other modes work. We have no details for processes that are supposed to connect, for example, what our deeper intuitions assure us must be so of the world, in so far as those assertions go beyond what empirical investigations already authorizes. The simple slogan is "Science is empirical."

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13. Why not Big-E Empiricism

Chapter 13

Why not Big-E Empiricism

1. Introduction

The conception of small-e empiricism defended here is optimistic. We can, have and will continue to be informed, by experience, of truths about nature that go well beyond experience and that form the fundamentals of our science. Small-e empiricism renounces the pessimism of Big-E Empiricism, which holds that *all* that we know of the scientific world is *only* that experience. Since Big-E Empiricism has long held a visible position in the empiricist tradition, this chapter collects the reasons given in earlier chapters for why it fails as a scientific empiricism.

First, as recounted in Section 3, to be viable, Big-E Empiricism needs some clean division between propositions expressing experience and those in science that do not. This division is precluded by the relational conception of experience, which in turn is derived from an examination of how experience enters modern science.

Second, as recounted in Section 4 and illustrated in Section 5, Big-E Empiricism elevates experience as the ultimate authority. That authority is routinely belied by the practice of science. No individual experimental result or observation is beyond correction; and a common source of corrections is supplied by the deeper results of scientific theories. Thus, these deeper results have an authority over individual experiences in contradiction with Big-E Empiricism.

Third, as recounted in Section 6, the propositions of experience stand in inductive relations of support with deeper results within the sciences. These are objective relations and that we accept them is a fundamental dictate of rationality. Big-E Empiricism can choose to ignore these relations of support; but it cannot ignore them at the same time as adhering to the dictates of rationality.

Fourth, as recounted in Section 7, a part of the case for small-e empiricism is that its ontology includes experiential processes. We are well-informed by them because they are a real and can convey to us the condition of systems of inference. Big-E Empiricism has to remain uncommitted to this ontology and thus must forgo this foundation for the efficacy of experience.

Section 8 concludes with a small footnote from history. An explicit debate over the issues separating small-e and Big-E Empiricism is not to be expected in routine science. Yet Werner Heisenberg and Albert Einstein undertook just such a debate over the newly emerging quantum theory of the 1920s. Before proceeding to the problems facing Big-E Empiricism, some formulations of Big-E Empiricism are recalled briefly in Section 2.

2. Some Formulations of Big-E Empiricism

Big-E Empiricism shares with all versions of empiricism the defining characteristic of privileging, in some manner, experience in our epistemic enterprises. The distinctive feature is a skepticism about those parts of science that are not directly experiential. A common formulation is that the non-experiential propositions play a purely instrumental role in allowing us to connect experiential propositions. Quine (1951, p.41) in his celebrated "Two Dogmas of Empiricism" expresses it as:

> As an empiricist I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in the light of past experience. Physical objects are conceptually imported into the situation as convenient intermediaries—not by definition in terms of experience, but simply as irreducible posits comparable, epistemologically, to the gods of Homer.

That this role is meager forms the basis of Hempel's classic "theoretician's dilemma" (1958, pp.49-50):

> If the terms and principles of a theory serve their purpose [of "establish[ing] definite connections among observable phenomena"] they are unnecessary, as just pointed out, and if they don't serve their purpose they are surely unnecessary. But given any theory, its terms and principles either serve their purpose or they don't. Hence, the terms and principles of any theory are unnecessary.

Variations on this formulation are possible. We may, following Hempel's dilemma, allow theoretical propositions to have this connective role only. Or we may, following van Fraassen's constructive empiricism, allow theoretical propositions to have a literal meaning, while not assigning a truth value to them.

3. The Relational Conception of Experience

The most immediate and most serious problem for Big-E Empiricism is that it requires a clean division between experience and theory, for experience only can be veridical. The arbitrariness of this division has proven to be an enduring difficulty for Big-E Empiricism. This division is expressed in van Fraassen's constructive empiricism as the division between the observable and the unobservable. We saw in Chapter 4 that the arbitrariness of this division was repeatedly a point of criticism to which van Fraassen struggled to respond.

If we adopt the relational conception of experience of the earlier chapters here, then this difficulty becomes fatal. Big-E Empiricism cannot even be formulated, since a clean division between experience and theory is precluded. There is no monadic property that designates a proposition as purely experiential. Experience enters as a physical process that connects us continuously with the system of interest. In place of Big-E Empiricism's monadic property is a relation over pairs of stages: one may be closer to the system of interest and the other farther from it.

4. The Corrigibility of Experience

In support of Big-E Empiricism, it is tempting to imagine that propositions closer to experience are univocally more secure than those of deeper theory. For experience is the privileged source for empiricists and propositions reporting experiences are closer to this source. The temptation must be resisted because it is not in accord with how reports of observations and experimental results in science are assessed. Such reports, taken individually, are treated as less secure than results of deeper theory.

There are two aspects to this character that speak against Big-E Empiricism and for small-e empiricism. The first is that all reports of experience in science are corrigible and often in need of correction. The second is that a means of correction lies in the deeper results of science that are themselves more remote from experience than the reports they correct. These two aspects indicate that it is a mistake to harbor skepticism over the deeper results of science while exclusively favoring reports of experience. Rather, we should not seek *a priori* to diminish the status of deeper theory, if well-supported by experience. We should assess the totality of the reports of experience and the deeper results of science on their individual merits. We will find, case by case, which reports of experience and which results of deeper theory are well-supported and which are not.

Consider these two aspects in more detail. The first aspect, the corrigibility of all reports of experience, is surely familiar. A night of patient observation of the motion of the stars by ancient astronomers left them in little doubt that they had observed that the stars are affixed to the surface of an enormous, all encompassing, rotating sphere. The observational report is mistaken. There is no such sphere. In another example, we saw in an earlier chapter that there is a near century of confident observational reports of alien spaceships visiting the Earth. Most of these reports are demonstrably mistaken and all are dubious.

That corrections are inevitable follows from the fact that the sum of all reports of experience rarely cohere as a totality. This is a problem even for the gold standard of scientific evidence, the controlled trial. When controlled trials for an effect yield a positive effect, often enough, some efforts at replication fail. These sorts of failures have become so common in psychology and related sciences, that, starting in the 2010s, it came to be known as the "replication crisis."[164]

The problem is not restricted to the human sciences. Since the later 2010s, cosmology has been struggling with the "Hubble tension" and it persists at the time of this writing. It concerns the most basic parameter of big bang cosmology, the Hubble constant. In an account of the state of the tension in 2019, Knox and Millea (2020, p.1) report that the Hubble constant, when estimated from observations of receding supernova, is $H_o = 74.03$ km/s/Mpc. When estimated from measurements of the cosmic background radiation, it is $H_o = 67.27$ km/s/Mpc. These two values, read simply, are logically inconsistent. If we include probabilistic estimates of the uncertainty in both values, they differ by 4.4σ, which makes joint veracity of both reports very improbable.

Inconsistencies and incoherence in these results can be eliminated by "winding back" to stages farther from the sustem of interest. We will eventually arrive at a stage in both that consist only of measurements so localized in space and time that they no longer contradict. The incoherence returns, however, as soon as we seek the significance of the observations by proceeding back along the lines of evidence.

164 For a brief sample of the huge literature the crisis has engendered, see Witkowski (2019).

The second aspect is that propositions deeper within a science are routinely used to correct reports of experience and experiments. The reverse is rare and, when it happens, it is an occasion for revolutionary change in a science. This asymmetry shows that we treat the theoretical propositions as more secure. When we invoke the principle of the conservation of energy to falsify some particular report of a perpetual motion machine (discussed below), we rely more on the conservation of energy than on the particular report, even though the latter is closer to experience. Individual reports of experience are more fragile than the theoretical propositions that we use to correct them.

Big-E Empiricism asks that we remain skeptical about the literal truth of the theory that corrects a report of experience, because the theory is more remote from experience. Prudence dictates that we invert these last attributions. We should be more skeptical of any *particular* report of experience in some domain than we are of a general theoretical statement that governs the domain.

For Big-E Empiricism, the power of a theoretical proposition to correct some particular report of experience results entirely from the theoretical proposition's role as a mediator among reports of experience. The correcting, theoretical proposition merely reflects the accumulated import of many other reports of experience, whose cumulative effect is to discount the particular report of experience.

For small-e empiricism, the assessment is different. It finds it quite revealing that many reports of experience are so in harmony as to have a unified import that can be captured by a theoretical proposition. In this circumstance, it is generally possible to show that these many, conforming reports of experience provide strong inductive support for the truth of the theoretical proposition. To insist on retaining skepticism about the truth of such theoretical propositions is simply to deny the import of evidence. The irrationality of such a denial is a theme that will be examined in greater detail in Section 6 below.

5. Illustrations of the Corrigibility of Reports of Experience

Here are a few illustrations of how deeper theory corrects reports of experience and experiment. In 1988, Benveniste et al. (1988) reported an extraordinary result. They took biologically active solutions of an anti-IgE antibody, diluted them massively and sometimes still found the diluted solutions to be biologically active. What made the results extraordinary is that this activity persisted at dilutions of 10^{120}. This outcome contradicted some basic science: that the antibody activity is chemical in nature and that the sizes of the antibody molecules are given by Avogadro's number, $6.02\text{x}10^{23}$, which is almost one hundred orders of magnitude smaller than the dilutions. At such extreme dilutions it is extremely unlikely that even a single antibody molecule remains in the solutions that are still purportedly found to be biologically active.

The conflict with established science was so stark that Benveniste et al.'s paper in *Nature* was accompanied by a note of "Editorial Reservation" that reported the "incredulity of the many referees" and that there is "no physical basis for such an activity." The reported results were so at variance with the deeper science that *Nature* took the extraordinary measure of sending a team to Benveniste's laboratory to check the results. John Maddox, James Randi and Walter W. Steward (1988) concluded the result was spurious. James "the Amazing" Randi was a stage magician who was prominent for his repeated debunking of fraudulent pseudoscience.

In September 2011, the OPERA collaborative reported measurements of neutrinos propagating from CERN in Switzerland to the Gran Sasso Laboratory in Italy that indicated that the neutrinos were propagating faster than the speed of light (OPERA collaborative, 2011). The excess was small if expressed as a fraction of the speed light (mean: 2.48×10^{-5}), but, at 6σ significance, it was not explicable as a random error (p. 22). The result was implausible from the outset and even treated with some skepticism by the OPERA investigators themselves, since it contradicted the prohibition on faster than light propagation of the special theory of relativity. That their observation had breached the special theory of relativity was treated widely with skepticism. The anomalous result was soon explained by various unnoticed sources of error, including a loose fibre-optic cable connection. (See Reich, 2012, for some details.)

Perhaps the most enduring example of deeper theory impugning observational reports concerns perpetual motion machines. These are devices that generate motive power, such as is needed to raise a weight, without depleting a corresponding capacity that is, loosely speaking, the fuel of the device. There have been many proposals for such machines and many reports by observers of their successful operation. All such reports are in conflict with the law of the conservation of energy. On its strength, all these reports are dismissed as instances of self-deception or fraud.

That all attempts at a perpetual motion machine necessarily fail has been a secure result of science. A much-mentioned statement of it was made by the French *Académie Royale des Sciences* in 1775. Its records of works presented to the Academy in 1775 include a decision that the Academy would no longer consider solutions to the three classic, but impossible construction problems in geometry;[165] and also proposals for perpetual motion machines. (Anon, 1778, p. 61). The prohibition on such proposals was justified by the much-quoted assertion (Anon, 1778, p. 65):[166]

> *Le construction d'un mouvement perpétual est absolument impossible.*
>
> The construction of a perpetual motion [machine] is absolutely impossible.

This impossibility was justified by the assertion (p. 65) "*cette [mortice] force ne peut produire qu'un effet égal à sa cause*": "This [motive] force can only produce an effect equal to its cause."

By the middle of nineteenth century, the folly of seeking perpetual motion machines was sufficiently well established that it was the subject of a 400 page monograph. Dircks (1870) catalogued failed attempts at perpetual motion machines spanning seven centuries, from the thirteenth to the nineteenth. Dircks (p. xii) is unequivocal in his historical summary:

> … every mechanical demonstration yet attempted has invariably produced the one ignoble result of palpable failure.

He was correspondingly unequivocal in his scorn for those who persist in their attempts (p. xii):

> The present century is rife in the reproduction of patented blundering, serving only to prove the ignorance and mental imbecility of a certain class of infatuated, would-be inventors, whom no history can teach, no instruction reform,

Dirck's dismal prediction of incorrigibility has been vindicated repeatedly. The US Patent Office granted patent US11,047,359B2 on June 29, 2021, for a "Gravitational Turbine Engine."

The more modern version of these ill-fated proposal is for a "reactionless drive." These are closed systems in which a purported imbalance of internal forces leads the device to accelerate itself. They are proposed as propulsion system for spaceships since they would escape a weakness of common propulsion systems. Such systems require a finite resource on the spaceship, some form of matter, to be expelled to produce a reaction force that accelerates the spaceship. These reactionless proposals all violate the law of conservation of momentum (which relativity theory has merged with the law of conservation of energy.)

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[165] Duplication of the cube, trisection of the angle and squaring the circle.

[166] This statement is much quoted, almost invariably without a source, which I finally located in Anon (1778).

A notable example is the "EM-drive." As reported in Mullins (2006), the drive consists of microwaves enclosed in a chamber. The radiative forces on the walls of the chamber sum, it is suggested, in a way that leaves a tiny, residual force capable of accelerating the chamber. The interesting development is that Harold White and his collaborators at the NASA Johnson Space Center reported experimental measurements of a tiny, residual net force. (See White et al., 2017) Needless to say, the report was met with considerable skepticism, precisely because of its violation of the conservation of momentum.[167]

6. The Irrationality of Inductive Skepticism

An essential component of Big-E Empiricism is some form of an untenable, inductive skepticism concerning the non-experiential content of science. The modern version arises most prominently in van Fraassen's constructive empiricism. As we saw in Chapter 4, constructive empiricism depends on a thorough-going inductive skepticism. In responses to critics of his *Scientific Image*, van Fraassen (1985, p. 247) summarized this skepticism through his (probabilistic) "theory of belief—or, better, of opinion." He wrote: (p. 247)

> [The theory] entails that the empirical adequacy of an empirical theory must always be more credible than its truth. It also entails that a theory as a whole (no matter how explanatory, unified, or consilient, and no matter how many novel predictions it has led to) cannot be more credible than any of its subtheories. … And finally it entails that no virtue which consists in greater informativeness (among which I reckon explanation) can give an additional reason for belief.

There are two foundations for the skepticism indicated here. The first is that the truth of a theory is *always* less credible than its empirical adequacy. This remark depends on the familiar result that a proposition is less credible than any of its logically weaker, deductive consequences. If credibility is understood probabilistically, in generic probability spaces, the proposition is less probable than its logically weaker, deductive consequence.

This remark fails in two ways. First, it fails to support what an inductive skeptic seeks: that the logically weaker, empirical adequacy of a theory should always be chosen *instead of* the logically stronger claim of its truth. For the remark is quite compatible with cases in which the

[167] See for example, Corneliussen (2016).

truth of the theory has been rendered very credible (or of arbitrarily great probability). Then, *both* the truth and the empirical adequacy of the theory should be believed. To refuse belief in the truth of the theory violates Hume's maxim that beliefs should be proportioned to the evidence.

The second problem is that the empirical adequacy of a theory is not generally a deductive consequence of the core principles of a theory. For these core principles generally have no empirical consequences until they are supplemented by auxiliary conditions. Then the empirical adequacy of a theory is a deductive consequence of the conjunction of these core principles and the auxiliary assumptions. If follows that this conjunction can be no more probable than its empirical adequacy. It does *not* follow that the truth of the core principles alone is no more probable that their empirical adequacy. For the truth of the core principles can be more probable than their empirical adequacy, if the auxiliary assumptions are less probable.

This last condition does arise. Consider, for example, the application of the laws of thermodynamics to real gases. The laws, by themselves, make no empirically testable predictions. To make them, the laws must be supplemented by an equation of state of the gas, such as the ideal gas law, the van der Waals equation or one of many others. These equations of state commonly fit imperfectly so that the empirical adequacy of the thermodynamic analysis, at some specified level of accuracy, may be less probable. That can be so, even when the laws of thermodynamics themselves can have far greater probability. Here we can recall Einstein's (1949, p. 33) reflection that "[classical thermodynamics] is the only physical theory of universal content concerning which I am convinced that, within the framework of the applicability of its basic concepts, it will never be overthrown."

This second foundation in van Fraassen's remark is his blanket rejection of inductive inference as an argument form. The remark lists key concepts in popular accounts of inductive inference. They are inference to the best explanation, Whewell's notion of consilience of inductions and inductive support through novel predictive successes. Later in the text, van Fraassen asserted, memorably: [168] (p. 295)

[168] Here I agree with van Fraassen that, in recent decades, the philosophical literature has struggled to supply us with a univocal logic of induction. However, I am allowing myself the

Inductive logic is a make-believe theory; no one has ever written its principles. Attempts to do so have always landed in incoherence or fallen afoul of hilarious counterexamples."

As a counterpoint to these negatives, van Fraassen turned to develop his positive view, which is in accord with his soon to be familiar voluntarism. He redefined the term "rational" opportunistically so that it no longer has its familiar meaning of conforming with reason and logic. He wrote: (p. 248, his emphasis)

> I construe the term *rational*, as applied to opinion here, as a term of' *permission* rather than of *obligation*.

and then:

> … belief in angels or electrons or the truth of theories in molecular biology does not *ipso facto* make one irrational.

For someone like van Fraassen, who is a skeptic about the logic of inductive inference and inductive support, belief in angels and electrons can be treated on a par as permitted choices. For someone who is not an inductive skeptic, the bare fact that someone believes in angels or electrons omits key details needed to determine the rationality of the beliefs. The rationality in each case depends on the extent of their support on the evidence.

My view of all forms of inductive skepticism is that they are indefensible if we are to proceed rationally. To do so requires that we proceed in accord with reason, that is, in accord with what is directed by logic. One can always simply refuse to be guided by logic. But one cannot do so and, at the same time, pretend to be rational. The difficulty is elementary and familiar. It is the substance of Lewis Carroll's (1895) charming parable, "What the Tortoise said to Achilles." It is so well known that I need here only recall the essential point. Achilles offers the tortoise this inference, which I have edited as indicated to make the logical connectives visible (p. 278):

> (*A*) [IF] Things … are equal to the same [THEN they] are equal to each other.
>
> (*B*) The two sides of this Triangle are things that are equal to the same.
>
> (*Z*) [THEREFORE] The two sides of this Triangle are equal to each other.

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conceit that my two books on the material theory of induction (2021, 2024) have provided a univocal account of inductive support, applicable across the sciences.

The tortoise simply refuses to accept that the truth of (*Z*) follows from the truth of (*A*) and (*B*). The diagnosis is direct and obvious. The tortoise is simply irrational. The inference follows from the meaning of the connective "if … then …" It just tells us that, if the antecedent in the hypothetical is true, then we are authorized to infer to the conclusion. To deny the inference one must either be a prankster, such as is Carroll's tortoise, or one must genuinely not understand what "if … then …" means.

The situation is no different with inductive logic. The observations of the cosmic background radiation and the motion of the galaxies provides strong inductive support for a primordial big bang. The term "strong inductive support" simply means that we are authorized to infer to the propositions supported and, according to the material theory of induction, this meaning is recovered from the meaning of the propositions that warrant the inductive inference. Since the support is inductive, the truth of the observational propositions does not guarantee the truth of the big bang proposition supported. Since the support is strong, the inference to it can be taken with only a very small inductive risk. One can simply refuse to make the inference. To refuse it is to be a mischievous, inductive tortoise. However, one cannot refuse it, while, at same time, proceeding rationally.

7. The Lack of Ontology

A strength of small-e empiricism is that it accepts the efficacy of inductive inference. Thus, as articulated in Chapter 12, it allows experience to support inductively an ontology that includes the experiential processes themselves. How is it that these processes can inform us? They can do so, small-e empiricism responds, because we have good reason to accept that they are real. The gravitational waves detected by LIGO are real. They really did propagate to Earth for eons of time through vast stretches of space from cataclysmic gravitational collapses that really happened eons ago.

Big-E Empiricism is trapped in its skepticism and cannot draw on a corresponding justification. For it maintains that all we learn about the world from experience is the content of that experience itself. Although a science may offer results that go well beyond experience, those results are to be discounted in some way. Perhaps they are merely instrumentally useful in allowing inferences to further experiences. However they are discounted, they can make no claim to representing the way the world really is. They might figure in an argument for Big-E Empiricism, but they can only serve in an instrumental capacity. Big-E Empiricism is precluded from admitting the reality of experiential processes and thus precluded from using that reality to support the efficacy of experience.

8. Einstein and Heisenberg

As a general matter, the decision between small-e and Big-E Empiricism is one to be taken in the philosophy literature. However, the decision between them in a particular case arose at a decisive moment in the history of physics.

A quantum theory of atomic matter was slowly emerging in the first quarter of the twentieth century. Niels Bohr had proposed that electrons, bound energetically within atoms, could only orbit the atomic nucleus in discretely spaced trajectories. The proposal met with success upon success empirically, even though the discreteness and stability of the orbits contradicted the classical electrodynamics that was also assumed to govern the orbits. By the mid 1920s, however, the soon to be called "old quantum theory" faced sufficiently many anomalies that Werner Heisenberg sought a fresh start. His 1925 "*Umdeutung*" paper, "Quantum Mechanical Reinterpretation [*Umdeutung*] of Kinematical and Mechanical Relations," is taken traditionally as the initiation of the "new quantum theory." His starting point, we can now see, conforms with Big-E Empiricism in favoring observables over any ontological commitment to quantities of deeper theory. The abstract of the paper read in its entirety (Heisenberg, 1925, p. 261):

> The present paper seeks to establish a basis for theoretical quantum mechanics founded exclusively upon relationships between quantities which in principle are observable.

The retreat to observables involved abandoning key parameters of the electron orbits of the old theory. They were then, Heisenberg noted (p. 261), "apparently unobservable in principle, e.g., position and period of revolution of the electron."

The new quantum theory continued to present significant interpretational problems so that this sort of Big-E Empiricism remained an option for at least some physicists. Edwin C. Kemble, a distinguished physicist at Harvard, developed this view in his early, comprehensive 1937 textbook on quantum theory. He formulated it as follows (Kemble, 1937, p. 331):

> We are again led to emphasize the fact that the wave function of a pure-state assemblage is merely a mathematical tool for computing from all previous

observations what the relative probabilities are for different results when we make our next observation.

Anecdotally, this simple sort of instrumentalism still has some adherents today, perhaps when a quick escape from vexing foundational problems is sought.

The small-e empiricism response to Heisenberg's 1925 proposal came from Albert Einstein. In the Spring of 1926, Heisenberg gave a colloquium on his new theory in Berlin. Einstein was in the audience and asked to meet with Heisenberg afterwards. We have a lengthy account in two places in Heisenberg's recollections of what was discussed in that meeting: the longer Heisenberg (1989, Ch. 5) and the shorter Heisenberg (1971, pp. 113-15). Einstein challenged precisely Heisenberg's failure to include electron trajectories in his theory. Heisenberg recalled Einstein's reaction as follows (1989, p. 113):

> He pointed out to me that in my mathematical description the notion of "electron path" did not occur at all, but that in a cloud-chamber the track of the electron can of course be observed directly. It seemed to him absurd to claim that there was indeed an electron path in the cloud-chamber, but none in the interior of the atom. The notion of a path could not be dependent, after all, on the size of the space in which the electron's movements were occurring.

Einstein protested against Heisenberg's idea that (in Einstein's words as recalled by Heisenberg, 1971, p. 63) "none but observable magnitudes must go into a physical theory." Einstein's rejoinder was a version of the idea that one cannot cleanly separate observations from theory. He said, on Heisenberg's recollection (1971, p. 63-64):

> … it may be heuristically useful to keep in mind what one has actually observed. But on principle, it is quite wrong to try founding a theory on observable magnitudes alone. In reality the very opposite happens. It is the theory which decides what we can observe.[169] You must appreciate that observation is a very

[169] [JDN] It is too easy to misread this now much-quoted sentence in isolation as a skeptical reflection that theory so controls observations that they cannot be used to assess novel theoretical proposals objectively. The fuller quote shows that it is not the novel elements of the new theory

complicated process. The phenomenon under observation produces certain events in our measuring apparatus. As a result, further processes take place in the apparatus, which eventually and by complicated paths produce sense impressions and help us to fix the effects in our consciousness. Along this whole path—from the phenomenon to its fixation in our consciousness—we must be able to tell how nature functions, must know the natural laws at least in practical terms, before we can claim to have observed anything at all. Only theory, that is, knowledge of natural laws, enables us to deduce the underlying phenomena from our sense impressions. When we claim that we can observe something new, we ought really to be saying that, although we are about to formulate new natural laws that do not agree with the old ones, we nevertheless assume that the existing laws—covering the whole path from the phenomenon to our consciousness—function in such a way that we can rely upon them and hence speak of "observations."

This exchange occurred in the earliest days of the new quantum theory. Heisenberg's simple instrumentalism persists as just one of the bewildering array of proposals still today for how quantum theory should be understood.[170] Einstein's instinct that electron trajectories are to be preserved persist in what is known as the "Bohmian" account.

The episode does illustrate a useful function for Big-E Empiricism. When a new theory is needed, it can greatly accelerate the arduous work of eliminating old, superfluous elements of theory. Such was the case with Heisenberg's *Umdeutung*. Einstein himself had used the same strategy to eliminate absolute simultaneity from physics in his 1905 work on special relativity. It was clearly an embarrassment for Einstein when Heisenberg pointed this out. Heisenberg (1989, p. 114) recalled Einstein's retort: "Perhaps I did use such philosophy earlier, and also wrote it, but it is nonsense all the same."

Einstein's awkward retraction might reflect what I think is the right assessment. This use of Big-E Empiricism may serve the purposes of the moment. But it is indiscriminately

at issue that controls the observations, but an older body of theory that governs whole process of observation, including the operation of the instruments.

[170] It is mentioned, for example, in Greiner (1989, p.477).

destructive. After its useful work is done, we should retract to the more fertile approach of small-e empiricism. We can affirm more of the world than merely what we observe of it.

9. Conclusion

Big-E Empiricism offers a promise of greater security compared with small-e empiricism. By limiting acceptance to experience and eschewing any further commitment, we might suppose that we are better protected from error. It is a false promise. For the science has no access to experience, pure and simple. It can only judge which results are closer to experience. More seriously, an unqualified reliance on experience is naïve. All experience in science is corrigible and often in need of correction. Those corrections come from deeper theory. That the deeper theory can correct experience shows that the deeper theory is more secure than any individual experience; and it is certainly more secure than the totality of experience, since that totality is inconsistent. To persist in treating this deeper theory as purely instrumental corrections to experience is inductively irrational, for the deeper theory has attained its securer position through the strength of its inductive support on the evidence.

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Afterword

My hope is that readers who share my comfort with empiricism will find the version of empiricism articulated here to be a rather benign elaboration of what a modern empiricism should be. It has kept the most important part of empiricism, that we only learn contingent truths through experience, and has discarded the tendentious skepticism of stricter forms of empiricism. It has kept pace with modern science in employing an expanded notion of experience that conforms with the widespread use of instrumental sensings in science.

Where this version of empiricism may seem untroublesome and uncontroversial, the same is not true of its consequences. Taking small-e empiricism seriously leads to a cascade of corrections to present philosophy of science and epistemology. Here is a brief preview of some of them.

Inductive inferences authorize us to results logically stronger than the premises in experience from which we start. Small-e empiricism requires that this can only arise if this mode of learning is itself secured through experience. The material theory of induction conforms with this requirement, since, according to it, inductive inferences are warranted by facts that are themselves ultimately empirically founded. The same is not true of accounts of inductive inference that rely on universally applicable formal rules, in so far as those rules are not in turn derived from experience.

Since inductive inference is ineliminably fallible, we can never be assured absolutely of results learned from experience. The best we can have is very strong inductive support for our contingent results. There is no independent indicator of truth. It follows that to require truth as a condition of knowledge makes possession of knowledge opaque. Once we discard a separate requirement of truth from our concept of knowledge, the notorious Gettier cases cease to be troublesome. Gettier-like cases turn out frequently to be celebrated as scientific successes in the history of science.

Empiricism has been taken, in recent philosophy of science, to contradict scientific realism. That is only so if we adopt an inductively skeptical, Big-E Empiricism. Since small-e empiricism is inductively optimistic, it conforms with realist sensibilities. It affirms that we can learn core results in science that are remote from experience. Small-e empiricism is itself a version of realism that mediates two untenable extremes. It retracts from the temptation of strict realists to strong theses that science has somewhere, somehow overridden inductive fallibility and found final truths. And it retracts from the inductive skepticism of antirealists who base their skepticism on an exaggeration of the import of inductive fallibilism.

Finally, small-e empiricism maintains empiricism's traditional opposition to metaphysics, in so far as metaphysics seeks to provide us with factual knowledge by means other than the empirical. For small-e empiricism maintains that no such other means work. It follows that a non-empirical metaphysics lacks sufficiently strong ways to settle debates on its major claims. The fortunes of these claims rise and fall with the rhetorical prowess of their proponents. The debates are interminable. The contrast with science is stark. Since science proceeds empirically, it has been able to secure an impressive and ever-growing body of fact.

These consequences of small-e empiricism are developed in a companion volume that is in complete draft at the time of this writing and provisionally entitled *Applications of small-e empiricism*.