Extracted from earlier chapter, edited, September 8, 2025
Edited September 12, 26, 2025

Chapter 11

The Terminal Obsession[1]

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.

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 experience 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. Second, theoretical terms will appear in propositions describing the stages of the experiential process. The operation of winding back described in the previous chapter allows us to eliminate any nominated theoretical term from the descriptions of stages of the experiential process. Whereas their complete elimination is probably unattainable, their presence and role is controllable and thus can be rendered benign.

Big-E Empiricism is preoccupied with the idea of the terminus of the process as uniquely efficacious, epistemically; and its critics have correspondingly been preoccupied with trying to establish that such a terminus is not identifiable. From the perspective of small-e empiricism, the sound and fury of the ensuing debates is misplaced. There is no such terminus and a viable empiricism has no need of one. Thus, I call its pursuit the "terminal obsession."

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[1] I thank Nora Boyd for helpful discussion on an earlier draft of this chapter.

The earlier Sections 1 to 6 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. Section 4 recounts how small-e empiricism escapes the problem through its conception of experience as a physically continuous process. The escape applies even if we adopt the mental state of some human observer as a provisional terminus. The sciences of psychology and physiology allow us to continue to trace the experiential process beyond them and to control and correct the reports. Sections 5 and 6 give two examples in the personal equation from astronomy and the psychological analysis of dubious UFO reports.

Sections 7 to 12 address the thesis of the theory ladenness of observation. Section 7 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 development. They depend, as I argue in Sections 8 and 11, 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 and psychological gestalt switches 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 9 and 10 argue that his case fails in both the philosophy and its history. It exaggerates the malign import of the ineliminability of theoretic 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 allowing 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. Experience is a continuous physical process that connects with the system of interest. There is no distinct terminus. What matters for small-e empiricism is not the terminus, but the whole process. If we do identify a stage as a terminus, it is provisional and can be revoked if the needs of the analysis require consideration of stages still closer to the system of interest. This provisional identification presents no foundational problems beyond those arising in the identifying the other stages of the process.

Might we, nonetheless, be a definite terminus if we identify it with the system of interest itself? That system does mark a point of initiation of the experiential process. To answer, I do not conceive it as a terminus. The system itself is a boundary to the experiential process. It is not a part of that process. It is what initiates the process. In other terms, the event itself is not the experience of it. Since the process is continuous, as a matter of abstract mathematical definition, there is no closest stage to the initiating boundary. For each close stage there is one closer.

Consider, for example, the 1883 volcanic eruption of Krakatoa. The sound waves propagating from it and their detection at various distant locations comprise a compounded experiential process with many points of detections. For any location that detects the sound, there is, in principle, one closer. The mathematics of the continuity of the propagation ensure it. As a practical matter, of course, the resulting infinity of possible locations of detection will not be realized. However, if one doubts the report of a detection at some designated location, one could in principle check that the sound was also detected at the right time at an intermediate location on its way to the designated location.

Debates over the terminus are commonly formulated in terms of human sensory experience. The terminus arises in some suitable combination of an excited human sense organ and a corresponding mental state. The continuity that precludes a definitive terminus is most apparent if we consider experiential processes that are fully implemented instrumentally, without human sense organs playing any essential role. For then there is no distinct sense organ and mental state that might serve as an irreducible terminus.

Consider the examples of Chapter 9. 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 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 is displayed visually in the superimposed plots in 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.

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 provisional terminus is a mental state. Psychology and physiology are the sciences that allow us to proceed past it to the further 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. The two sections that follow provide two examples of how such sciences allow us to trace beyond and to correct some human sensory report.

5. The Personal Equation in Astronomy

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 exactly 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.

6. The Psychology of UFO Reporting

There is a curious element in many of the reports of flying saucers, UFOs or UAPs:[2] 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.[3] 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 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)

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[2] See Chapter 10 for more details of UFO and UAP investigations.

[3] 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."

> 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, arc, 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.

7. 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 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" 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 an earlier 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. Whereas we may never be able to eliminate theoretical terms entirely from descriptions of the stages of an experiential process, winding back allows 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. Whereas 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.

8. 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. 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.

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 suggest (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.

9. Paul Feyerabend and the Failure of Meaning Invariance

The most philosophically rigorous development of these themes are 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 are expressed in theoretical terms: (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.[4] (pp. 168-169)

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 will employ theoretical terms. However, for any specified decision between two competing theories, if the records of observation and experiment have been kept, it

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[4] 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 properties assumed for what the theory designates as mass are sufficiently error tolerant as to preserve the uniqueness of the referent.

is possible to wind back through the experiential processes to a stage whose description is free from the theories in competition. Then the observations can serve to adjudicate among the theories without circularity. The illustrations earlier of winding back should have already made this obvious. In the next section, it will be illustrated for the case of special relativity.

10. 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 uses it and offers an ambitious, general argument against the possibility of observations providing an objective basis for deciding between them:[5] (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.

This analysis is quite at variance with what actually happened. The observational and experimental evidence that Einstein found decisive was originally discovered in the context of theories that differed from special relativity. Yet 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

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[5] I will not labor the failure of Feyerabend's frivolous objection that evidence that is not Lorentz covariant cannot "lead to" a Lorentz covariant theory. For precisely this is what happened. 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.

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 the velocity of 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.[6] Whereas it is unlikely that Einstein consulted Bradley's paper directly, he could have done so with no ill effect. Bradley's result, as used by Einstein, applies to an Earth moving at a velocity much smaller than that of

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[6] 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 states. 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.

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 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.[7]

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 to the principle of relativity, he would arrive directly at the relativity of simultaneity, without any

[7] 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 $v << c$.

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.

11. Thomas Kuhn on a Neutral Observation Language

Thomas Kuhn's (1996) *Structure of Scientific Revolutions* and related writing 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 of the impossibility of a neutral observation language 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 on p. 113).

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 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.

There is a second failing of Kuhn's argument. It depends essentially on exporting results in the psychology of our sense experiences to restrictions on how the results of observation and experiment can be expressed. Since Kuhn's time, as was noted in an earlier chapter, human sense experience has ceased to be an essential element in the fully instrumental, experiential processes that now permeate much of science.

12. 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.[8] 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, although 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 Pythagoreans. There is no common 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 such theories and thus cannot rationally decide between them. Since the discovery of the incommensurability in geometry was catastrophic for the Pythagoreans,

[8] 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.

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[9] 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 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 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

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[9] 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."

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: [10] (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.

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 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.

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."

[10] 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.

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.[11]

11. Conclusion

Big-E Empiricism has long been troubled by its attachment to an elusive notion of pure experience. Its proponents have long sought unsuccessfully to demarcate the experiential and non-experiential content of science. Critics have long claimed that such demarcation is impossible because observation, experience and theory are irremediably intermingled.

The concept of experience of small-e empiricism as a continuous process resolves both problems. Because of its physical continuity, the experiential process harbors no distinct terminus. The identification of any terminus is provisional and, if necessary, further investigation can delineate stages closer to the system of interest. Similarly, this 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 wind back to stages closer to the system of interest whose description no longer employs the troublesome predicate.

[11] 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.

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