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Chapter 10

Experience as a Process[1]

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 separate body of pure experience. Rather its authority derives from a physical process that connects 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 these processes is described propositionally. These propositions range over content that is close to the system of interest and those that are more

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

remote from it. There is no strict division into experiential and non-experiential propositions. Some parts are closer to the target system; and some are further from it. In place of this strict division is a relational concept: "closer to experience." 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 to 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 describes the process of "winding back" towards the stages of the experiential process that are closer to the system of interest. That winding back is possible makes the results of experiential processes corrigible and thereby ensures a high level of security. The following Sections 6 and 7 give illustrations 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 distance were all of the same type.

The prospects of this process of winding back are limited if there are insufficient records of the stages of the experiential processes. Section 8 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 California 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 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 physical processes that connect 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 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 sensory 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 ranges 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 sensory 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 various stages of experiential processes are given propositional descriptions. The part of these descriptions of importance to empiricism asserts the condition of the system of interest. The farther we are along the experiential process away from the system of interest, the less secure is the representation of the condition of the originating system of interest. There is almost always a subsequent aggregation of propositional results from several processes to yield a single synopsis that is routinely described as an "observation" or "experimental result." Since this final step is almost always a generalization, there is a risk of further reduction of security.

The measure of the extent of that security is inductive. The stages closer to the system of interest condition those farther from it by the mediation of the continuous physical process that connects them. The inductive relations of support employed the scientific analysis proceed in the opposite direction. The propositions describing these farther stages can provide inductive support for the propositions describing the stages closer to the system of interest. If the system of interest is involved 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 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 evidence derived from the experiential processes is 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? 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 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. Many inductive inferences are needed to secure an assurance of success.

The simplest and most familiar inductive inference seeks to establish that that the sample 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 the 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.

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 or presentation requires an assurance that 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 calls for a physical realization of the propositional description of the various stages of the process of sample transport. Overall, the recommendations are designed to secure the integrity of the sampling processes. It is secured inductively in the sense that the fact of adherence to the protocols provides strong inductive support for the integrity.

5. The Corrigibility of Experience: Winding Back

That the evidential processes have this explicit structure is important to preserving the security of empirical evidence and the authority of experience. For this structure gives us the means to assess and correct the results arising from some experiential process. The various stages of the process are accessible through their propositional descriptions and their security derives from the inductive relations of support among them. These inductive relations can in turn be assessed and corrected, if needed. If we work with the material theory of induction, that assessment is conducted by identifying the background facts that warrant the various inductive inferences. This role for background facts gives us the further option of using results from elsewhere to moderate the inductive relations of support.

The process of checking and correcting the results of an experiential processes involves an analysis that passes, stage by stage, back through the experiential process towards the system of interest, checking for cogency at each step. I will call this process "winding back."[2] This sort

[2] 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

of winding back is quite common in the practice of science. In principle, such winding back is always possible. In practice, this might not be so if suitable records have not been kept. The sections following provide illustrations.

The next two sections describe two cases in which significant errors were made. 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 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.[3]

6. Winding Back: Enrico Fermi and the Misidentification of Element 93

6.1 Element 93

In May, 1934, the prestigious journal, *Nature*, published a letter from the Italian researcher in Rome, Enrico Fermi (1934a), that 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

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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.

[3] 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.

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 title indicated Fermi's hesitation "Possible Production of Elements of Atomic Number Higher than 92." The transuranic element produced, if they had 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.[4] It was only later, in 1939, that the core difficulty was discovered. Fermi, Hahn and his collaborators has not included

[4] Otto Hahn's biography (1966, pp. 139-44) provides a synopsis of the collapse of the program.

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.

6.2 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 from the synoptic report of the final result to the descriptions of the results describing earlier stages and assessing the strength of the inductive support provided for them. 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 essential earlier result 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.

6.3 Winding Back

For our purposes, the key inductive inference is from the identification of the half-life of the new element as 13 minutes and 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.

7. Winding Back: Hubble and the Expansion of the Nebulae

7.1 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 their red shifts. 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 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.

The observational basis of all these analyses lay in the detection and measurement of the frequencies and intensity of light from many stars and many galaxes. Each of these measurements involved an experiential process in which light from these astronomical objects propagated to the Earth for measurement by astronomical instruments. These many measurements then needed to be synthesized into the two numbers needed to summarize the recession: the speed of recession and distance to each galaxy. The major errors in Hubble's original determination crept in during this process of synthesis.

7.2 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):[5] "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.

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[5] "$M = -6$." The more negative the magnitude, the brighter the star.

Overall, Hubble's use of the period-luminosity relations 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.

7.3 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 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 soon turned out, as detailed in Baade (1956), that the two types of Cepheid variable stars were 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.[6]

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.

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":[7]

> 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

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[6] 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 $L_2/L_1 = 10^{(1.5/2.5)} = 3.981$.

[7] [log *cz*, *m*] z = redshift. m = apparent magnitude.

> 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.

8. Problems Winding Back

The last two sections have reviewed cases in which winding back succeeded quickly to correct 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 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.

8.1 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.[8] 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

[8] I learned of this curious example from Allan Franklin.

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 in 1929 to 1933[9] in a mile-long, evacuated[10] 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.

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[9] Michelson died in 1931. The new series of measurements were completed by his collaborators.

[10] 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.

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!

The error conjectured of several feet far exceeds Michelson's earlier estimate 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.

8.2 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 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.[11] 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.

9. Conclusion

The authority of experience does not reside in any special powers of human sensory organs or human cognition; or in some elusive but evidentially potent notion of pure experience. Rather it derives from a physical process that connects 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 experiential process to stages closer to 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 for further results; and an anti-empiricist skepticism behind the thesis of the theory ladenness of observation.

[11] Elvis is dead, despite repeated reports of sightings that suggest otherwise.

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.[12]

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[12] For an account of process theory in physics, see Penn (2023).

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