**Empiricism and After**

**Jim Bogen[[1]](#footnote-2)** <7298 words>

 **Abstrac**t

 Familiar versions of empiricism overemphasize and misconstrue the importance of perceptual experience. I discuss their main shortcomings and sketch an alternative framework for thinking about how human sensory systems contribute to scientific knowledge.

 **i. Introduction**. Science is an empirical enterprise, and most present day philosophies of science derive from the work of thinkers classified as empiricists. Hence the ‘empiricism’ in my title. ‘And after’ is meant to reflect a growing awareness of how little light empiricism sheds on scientific practice.

 A scientific claim is credible just in case it is significantly more reasonable to accept it than not to accept it. An influential empiricist tradition promulgated most effectively by 20th century logical empiricists portrays a claim’s credibility as depending on whether it stands in a formally definable confirmation relation to perceptual evidence. Philosophers in this tradition pursued its analysis as a main task. §vii below suggests that a better approach would be to look case by case at what I’ll call epistemic pathways connecting the credibility of a claim in different ways to different epistemically significant factors. Perceptual evidence is one such factor, but so is evidence from experimental equipment, along with computer generated virtual data, and more. Sometimes perceptual evidence is crucial. Often it is not. Sometimes it contributes to credibility in something like the way an empiricist might expect. Often it does not.

 **ii. Empiricism is not a natural kind**. Zvi Biener and Eric Schleisser

observe that ‘empiricism’ refers not to a single view but rather, to

…an untidy heterogeneity of empiricist philosophical positions. There is no body of doctrine in early modernity that was “empiricism” and no set of thinkers who self identified as ‘empiricists’…[Nowadays] ‘empiricism’ refers to a congeries of ideas that privilege experience in different ways.(Biener and Schleisser 2014. p.2)

 The term comes from an ancient use of ‘empeiria’—usually translated ‘experience’—to mean something like what we’d mean in saying that Pete Seeger had a lot of experience with banjos. Physicians who treated patients by trial and error without recourse to systematic medical theories were called ‘empirical’.(Sextus Empiricus 1961 pp.145-6). Aristotle used ‘empeiria’ in connection with what can be learned from informal observations as opposed to scientific knowledge of the natures of things.(Aristotle, 1984 pp. 1552-3) Neither usage has much to do with ideas about the cognitive importance of perceptual experience we now associate with empiricism.

 Although Francis Bacon is often called a father of empiricism, he accused what he called the empirical school of inductive recklessness: Their ‘…premature and excessive hurry’ to reach general principles from ‘…the narrow and obscure foundation of only a few experiments’ leads them to embrace even worse ideas than rationalists who develop ‘monstrous and deformed’ ideas about how the world works by relying ‘chiefly on the powers of the mind’ unconstrained by observation and experiment. (Bacon, 1994 p.70) Bacon concludes that just as the bee must use its powers to transform the pollen it gathers into food, scientists must use their powers of reasoning to interpret and regiment experiential evidence if they are to extract knowledge from it.(ibid, p105)[[2]](#footnote-3)  Ironically, most recent thinkers we call empiricists would agree.

 Lacking space to take up questions Bacon raises about induction, this paper limits itself to other questions about experience as a source of knowledge. Rather than looking for a continuity of empiricisms running from Aristotle through British and logical empiricisms to the present, I’ll enumerate some main empiricist ideas, criticize them, and suggest alternatives.

 **iii. Anthropocentrism and Perceptual Ultimacy**. Empiricists tend to agree with many of their opponents in assuming

 **1. Epistemic Anthropocentrism**. Human rational and perceptual faculties

 are the only possible sources of scientific knowledge.[[3]](#footnote-4)

 William Herschel argued that no matter how many consequences can be inferred from basic principles that are immune to empirical refutation, it’s impossible to infer from them such contingent facts as what happens to a lump of sugar if you immerse it in water or what visual experience one gets by looking at a mixture of yellow and blue.(Herschel 1966 p.76). Given 1., this suggests:

 2. **Perceptual Ultimacy**. Everything we know about the external world

 comes to us from…our senses, the sense of sight, hearing, and touch,

 and to a lesser degree, those of taste and smell. (Campbell,1952. p.16)[[4]](#footnote-5)

 One version of Perceptual Ultimacy derives from the Lockeian view that our minds begin their cognitive careers as empty cabinets, or blank pieces of paper, and all of our concepts of things in the world, and the meanings of the words we use to talk about them must derive from sensory experiences. (Locke, 1988. pp.55,104-5)

 A second version maintains that the credibility of a scientific claim depends on how well it agrees with the deliverances of the senses. In keeping with this and the logical empiricist program of modeling scientific thinking in terms of inferential relations among sentences or propositions,[[5]](#footnote-6) Carnap’s *Unity of Science* (UOS) characterizes science as

…a system of statements based on direct experience, and controlled by experimental verification…based upon ‘protocol statements’…[which record] a scientist’s (say a physicist’s or a psychologist’s) experience….(Carnap,1995, p.42-3)

 Accordingly, terms like ‘gene’, and ‘electron’, which do not refer to perceptual experiences must get their meanings from rules that incorporate perceptual experiences into the truth conditions of sentences that contain them. Absent such rules, sentences containing theoretical terms could not be tested against perceptual evidence and would therefore be no more scientific than sentences in fiction that don’t refer to anything. (Schaffner, 1993 p. 131-2) For scientific purposes, theoretical terms that render sentences untestable might just as well be meaningless. This brings the Lockeian and the Carnapian UOS versions of Perceptual Ultimacy together.

 The widespread and inescapable need for data from experimental equipment renders both 1. and 2. indefensible.[[6]](#footnote-7) Indeed, scientists have relied on measuring and other experimental equipment for so long that it’s hard to see why philosophers of science ever put so much emphasis on the senses. Consider for example Gilbert’s 16th century use of balance beams and magnetic compasses to measure imperceptible magnetic forces. (Gilbert 1991, pp.167-8)[[7]](#footnote-8)

 Experimental equipment is used to detect and measure perceptibles as well as imperceptibles, partly because it can often deliver more precise, more accurate, and better resolved evidence than the senses. Thus although human observers can feel heat and cold, they aren’t very good at fine grained quantitative discriminations or descriptions of experienced, let alone actual temperatures. As Humphreys says,

[o]nce the superior accuracy, precision, and resolution of many instruments has been admitted, the reconstruction of science on the basis of sensory experience is clearly a misguided enterprise. (Humphreys 2004, p.47)

 A second reason to prefer data from equipment is that investigators must be able to understand one another’s evidence reports. The difficulty of reaching agreement on the meanings of some descriptions of perceptual experience led Otto Neurath to propose that protocol sentences should contain no terms for subjective experiences accessible only to introspection. Ignoring details, he thought a protocol sentence should mention little more than the observer, and the words that occurred to her as a description of what she perceived when she made her observation. (Neurath, 1983, pp. 93ff) But there are better ways to promote mutual understanding. One main way is to use operational definitions[[8]](#footnote-9) mentioning specific (or ranges of) instrument readings as conditions for the acceptability of evidence reports. For example, it's much easier to understand reports of morbid obesity by reference to quantitative measuring tape or weighing scale measurements than descriptions of what morbidly obese subjects look like.

 In addition to understanding what is meant by the term ‘morbidly obese’, qualified investigators should be able to decide whether it applies to the individuals an investigator has used it to describe. Thus in addition to intelligibility, scientific practice requires public decidability: It should be possible for qualified investigators to reach agreement over whether evidence reports are accurate enough for use in the evaluation of the claims they are used to evaluate.[[9]](#footnote-10) Readings from experimental equipment can often meet this condition better than descriptions of perceptual experience.

 The best way to accommodate empiricism to all of this would be to think of outputs of experimental equipment as analogous to reports of perceptual experiences. I’ll call the view that knowledge about the world can be acquired from instrumental as well as sensory evidence **liberal empiricism,**[[10]](#footnote-11) and I’ll use the term ‘**empirical evidence’** for evidence from both sources.

 Both liberal empiricism and anthropocentrism fail to take account of the fact that scientists must sometimes rely on computationally generated virtual data for information about things beyond the reach of their senses and their equipment. For example, weather scientists who cannot position their instruments to record temperatures, pressures, or wind flows inside evolving thunderstorms may

…examine the results of high-resolution simulations to see what they suggest about that evolution; in practice, such simulations have played an important role in developing explanations of features of storm behavior…(Parker, 2010 p.41)[[11]](#footnote-12).

Empiricism ignores the striking fact that computer models can produce informative virtual data without receiving or responding to the kinds of causal inputs that sensory systems and experimental equipment use to generate their data. Virtual data production can be calibrated by running the model to produce virtual data from things that experimental equipment can access, comparing the results to non-virtual data, and adjusting the model to reduce discrepancies. Although empirical evidence is essential for such calibration, virtual data are not produced in response to inputs from things in the world. Even so, virtual data needn’t be inferior to empirical data. Computers can be programed to produce virtual measures of brain activity that are epistemically superior to non-virtual data because virtual data

…can be interpreted without the need to account for many of the potential confounds found in experimental data such as physiological noise, [and] imaging artifacts…(Sporns 2011, p.164)

By contrast, Sherri Roush argues that virtual data can be less informative than empirical data because experimental equipment can be sensitive to epistemically significant factors that a computer simulation doesn’t take into account. (Roush, forthcoming). But even so, computer models sometimes do avoid enough noise and represent the real system of interest well enough to provide better data than experimental equipment or human observers.

 **iii. Epistemic Purity**.[[12]](#footnote-13)  Friends of Perceptual Ultimacy tend to assume that

 **3.** In order to be an acceptable piece of scientific evidence, a report

 must be pure in the sense that none of its content derives from ‘judgments and conclusions imposed on it by [the investigator]’. (Neurath 1983, p. 103)[[13]](#footnote-14)

 This condition allows investigators to reason about perceptual evidence as needed to learn from it as long as their reasoning does not to influence their experiences or the content of their observation reports. A liberal empiricist might impose the same requirement on data from experimental equipment. One reason to take data from well functioning sensory systems and measuring instruments seriously is that they report relatively direct responses to causal inputs from the very things they are used to measure or detect. Assuming that this allows empirical data to convey reliable information about its objects, purity might seem necessary to shield it from errors reasoning is prone to. (Cp. Herschel 1966, p.83) But Mill could have told the proponents of purity that this requirement is too strong.

One can’t report what one perceives without incorporating into it at least as many conclusions as one must draw to classify or identify it. (Mill 1967, p.421)

Furthermore, impure empirical evidence often tells us more about the world than it could have if it were pure. Consider Santiago Ramòn y Cajal’s drawings of thin slices of stained brain tissue viewed through a light microscope.(DeFelipe and Jones, 1988) The neurons he drew didn’t lie flat enough to see in their entirety at any one focal length or, in many cases, on just one slide. What Cajal could see at one focal length included loose blobs of stain and bits of neurons he wasn’t interested in. Furthermore, the best available stains worked too erratically to cover all of what he wanted to see. This made impurity a necessity. If Cajal’s drawings hadn’t incorporated his judgments about what to ignore, what to include, and what to portray as connected, they couldn’t have helped with the anatomical questions he was trying to answer. (ibid, pp.557--621)

 Functional magnetic resonance imaging (fMRI) illustrates the need for impurity in equipment data. fMRI data are images of brains decorated with colors to indicate locations and degrees of neuronal activity. They are constructed from radio signals emitted from the brain in response to changes in a magnetic field surrounding the subject’s head. The signals vary with local changes in the level of oxygen carried by small blood vessels indicative of magnitudes and changes in electrical activity in nearby neurons or synapses. Records of captured radio signals are processed to guide assignments of colors to locations on a standard brain atlas. To this end investigators must correct for errors, estimate levels of oxygenated blood or neuronal activity, and assign colors to the atlas. Computer processing featuring all sorts of calculations from a number of theoretical principles is thus an epistemically indispensable part of the production, not just the interpretation, of fMRI data.[[14]](#footnote-15),

 Some data exhibit impurity because *virtual* data influence their production. Experimenters who used proton-proton collisions in CERN‘s large hadron collider (LHC) to investigate the Higgs boson had to calibrate their equipment to deal with such difficulties as that only relatively few collision products could be expected to indicate the presence of Higgs bosons, and the products of multiple collisions can mimic Higgs indicators if they overlap during a single recording. To make matters worse, on average close to a million collisions could be expected every second, each one producing far too much information for the equipment to store.(van Mulders, 2010 p.22, 29ff.). Accordingly investigators had to make and implement decisions about when and how often to initiate collisions, and which collision results to calibrate the equipment to record and store. Before implementing proposed calibrations and experimental procedures, experimenters had to evaluate them. To that end, they ran computer models incorporating them, and tested the results against real world experimental outcomes. Where technical or financial limitations prevented them from producing enough empirical data, they had to use virtual data. (Morrison 2015 pp. 292ff) Margaret Morrison argues that virtual data and other indispensible computer simulation results influenced data production heavily enough to ‘cast…doubt on the very distinction between experiment and simulation’(ibid, p.289) LHC experiment outputs depend not just on what happens when particles collide, but also on how often experimenters produce collisions and how they calibrate the equipment. Reasoning from theoretical assumptions and background knowledge, together with computations involving virtual data exerts enough influence on all of this to render the data triply impure.[[15]](#footnote-16) The moral of this story is that whatever makes data informative, it can’t depend on reasoning having no influence on the inputs from which data is generated, the processes through which it is generated, or the resulting data.

 **iv. Scope empiricism**. The empiricist assumptions I’ve been sketching attract some philosophers to what I’ll call **Scope Empiricism**:

 **4.** Apart from logic and mathematics, the most that

 scientists should claim to know about are patterns of perceptual

 experiences. The ultimate goal of science is to describe, systematize,

 predict, and explain the deliverances of our senses.

 Bas Van Fraassen supported Scope Empiricism by arguing that it is ‘epistemically imprudent’ for scientists to commit themselves to the existence of anything that cannot in principle be directly perceived--even if claims about it can be inferred from empirical evidence. Working scientists don’t hesitate to embrace claims about things that neither they nor their equipment can perceive, but scope empiricists can dig in their heels and respond that even so, such commitments are so risky that scientists shouldn’t make them. Supporting empiricism this way is objectionable because it disparages what are generally regarded to be important scientific achievements, especially if Scope Empiricism prohibits commitments to claims that cannot be supported without appeal to impure data.

 Van Fraassen knows that scientists make general claims whose truth depends upon how well they fit unobserved as well as observed past, present and future happenings. (Van Fraassen [1980] p.69) Thus Snell’s law purports to describe changes in the direction of light rays that pass from one medium into another—not just the relatively few that have been or will be measured. And similarly for laws and lesser generalizations invoked to explain why Snell’s law holds (to the extent that it does). Van Fraassen argues that although scientists can’t avoid the epistemic risk of commitment to generalizations over unobserved perceptibles, they should avoid the greater risk of commitment to the existence of imperceptibles. To the contrary, it’s epistemically imprudent to commit oneself to a generalization without good reason to think that unexamined instances conform to known instances, and as I’ve argued elsewhere, the best reasons to accept a generalization over unobserved perceptibles sometimes include claims about unobservable activities, entities, processes, etc. that conspire to maintain the regularity it describes.(Bogen 2011 pp. 16--19) Apart from that, it’s epistemically imprudent to infer regularities from data unless it’s reasonable to believe in whatever the data represent. Anyone who draws conclusions about the brain from Cajal’s drawings had better believe in the existence and the relevant features of the neurons he drew. But we’ve seen that the evidential value of Cajal’s drawings depends on details he had to fill in on the basis of his own reasoned judgments. By the same token, the use of LHC data commits one to imperceptible collision products. Here and elsewhere, disallowing commitment to anything beyond what registers on the senses or on experimental equipment undercuts evidence for regularities scientists try to explain and generalizations they rely on to explain them.

 **vi. “Points of contact with reality”.** As Neurath paraphrased him, Schlick maintained that

…perceptual experiences are the ‘absolutely fixed, unshakable points of contact between knowledge and reality’. (Neurath 1983 .p. 103.)

Perceptual data might seem to be the best candidates for what Schlick had in mind. But the realities that scientists typically try to describe, predict, and explain are **phenomena** which Jim Woodward and I have argued are importantly different from data. Phenomena are

…,events, regularities, processes, etc. whose instances, are uniform and uncomplicated enough to make them susceptible to systematic prediction and explanation (Bogen and Woodward 1988, 317).

The melting point of lead and the periods and orbital paths of planets are examples. (ibid, pp.319–326) By contrast, **data** (think data points or raw data) correspond to what I’ve been calling empirical evidence. They are records of what registers on perceptual systems or experimental equipment in response to worldly inputs. They are cleaned up, corrected for error, analyzed, and interpreted to obtain information about phenomena.

 The reason scientists seldom try to develop general explanatory theories about data is that their production usually involves a number of factors in elaborate and shifting combinations that are idiosyncratic to specific laboratory or natural settings. For example, the data Bernard Katz used to study neuronal signaling were tracings generated from neuronal electrical activity and influenced by extraneous factors peculiar to the operation of his galvanometers sensitive to trial to trial variations in positions of the stimulating and recording electrodes he inserted into nerves, and physiological effects of their insertion, changes in the condition of nerves as they deteriorated during experiments, and error sources as random as vibrations that shook the equipment in response to the heavy tread of Katz’s teacher, A.V. Hill walking up and down the stairs outside of the laboratory. Katz wasn’t trying to develop a theory about his tracings. He wanted a general theory about postsynaptic electrical responses to presynaptic spikes. No theory of neuronal signaling has the resources to predict or explain effects produced as many mutually independent influences as Katz’s tracings. Katz’s data put him into much more direct epistemic contact with states of his equipment than to the neuronal phenomena he used it to investigate.

 Sensory and equipment generated data make contact with reality in the sense that they are produced from causal inputs from things that interact with equipment or the senses. But causal contact is not the same thing as epistemic contact. Recall that virtual storm data help bring investigators into epistemic contact with temperatures in storm regions that do not causally interact with the computers that generate them. More to the point, investigators often use data from things that do interact causally with their equipment or their senses to help answer questions about phenomena that do not. Thus Millikan used data from falling oil drops he could see to investigate something he could not see--the charge on the electron.

 **vii. Tracking epistemic pathways as an alternative to received accounts of confirmation**. The last empiricist notion I want to consider is an idea about confirmation:

 **5.** **Two term Confirmation**. A scientific claim is credible only if it is

 confirmed by perceptual evidence, where confirmation is a special two

 term relation between claim and evidence--the same relation in every

 case.

 One major trouble with this is that as we’ve seen, many scientific claims owe their credibility to non-perceptual evidence from experimental equipment or computer simulations. Now I want to take up a second difficulty which can be appreciated by considering how two different sorts of evidence were used to support the credibility of General Relativity Theory (GRT). The first consisted of photographic plates, some of which were exposed to starlight at night, and others, during a solar eclipse. The second consisted of records of telescope sightings of Mercury on its way around the sun. Although both kinds of evidence were used to evaluate one and the same theory, the support they provided for it cannot be informatively represented as instantiating one and the same confirmation relation, let alone a two term one. The starlight photographs were used to compare GRT to Newtonian predictions about the path of light passing near the sun. Their interpretation required measurements of distances between spots on the photographic plates, calculations of the deflection of starlight passing near the sun from differences between relative positions of spots on eclipse and nighttime plates, and comparisons of deflection estimates to predicted values. The telescope data was used to evaluate GRT and Newtonian predictions about the perihelion of Mercury. In both cases the evidence supported GRT by favoring its predictions. In addition to different calculations involving different background assumptions and mathematical techniques, different pieces of equipment were needed, and different physical adjustments and manipulations were required to promote the reliability of their data.[[16]](#footnote-17) These data bear on the credibility of GRT by way of different, indirect connections. The natures and the heterogeneity of such connections are ignored by hypothetico-deductive and other received general accounts of confirmation. And similarly for a great many other cases.

 The pursuit of a two term relation of confirmation whose analysis will explain for every case what it is for evidence to make a claim credible tends to distract epistemologists from issues analogous to those that are obscured by the pursuit of a single relation whose analysis can distinguish causal from non-causal co-occurances of events. Among these are questions about intermediate links between causal influences and their effects, differences in strengths and degrees of importance of causal factors in cases where two or more factors contribute to the same effect, and the ways in which some effects can be produced in the absence of one or more of their normal causes. Such questions arise in connection with a great many scientific investigations. To illustrate, shingles presents itself as a collection of symptoms including, most notably, a painful band of rash, They are caused by the chicken pox virus, varicella zoster. The virus typically produces them only after remaining dormant in neural tissue for years after the original bout of chicken pox subsides. Instead of closing the books on shingles once they had identified the virus as its cause, investigators looked for intermediate and supplementary causal factors to explain how it survives while dormant, what inhibits, and what promotes its activation, and where in the causal process physicians could intervene to control it.[[17]](#footnote-18) This research raises questions about whether or how factors that are present in the virus from the outset help explain its survival, activation, and rash production, and whether the same account of causality (e.g., interventionist, mechanistic, pragmatic law, etc.) illuminates the explanation of every stage in the process. Such questions are obscured in standard philosophical literature on causality by the analog of idea 5 above.

 Genetic research is another example. Geneticists who know which DNA codons are especially important to the synthesis of a specific protein look for connections between intermediate causal factors at various steps of protein production including influences of promoter molecules that initiate or encourage, and repressors that block or dampen specific sub-processes.[[18]](#footnote-19)

 The best way for philosophers to do justice to such research is to construe the effect of interest as the end point of a causal pathway whose steps involve different factors interacting in different ways to contribute to its production.[[19]](#footnote-20) By analogy, I think philosophers interested in questions that empiricists tried to answer should attend to what I’ll call **epistemic pathways**.

An epistemic pathway leads *from any epistemically significant factor you choose*, e.g., a parcel of empirical or virtual evidence, *to the credibility of a scientific claim.*

 Depending as it does on what it’s reasonable to believe, credibility is relative to people, places and times. What’s credible relative to evidence, methods of interpretation, background knowledge, etc. available now need not be credible relative to what was available 50 years ago. What’s not credible now may become credible later on. What it’s reasonable for one group of people to accept may not be credible for their contemporaries who work with different evidence, background knowledge, methods of analysis, etc. For many philosophical purposes, the simplest way to think about credibility is to treat it as a function of logical, probabilistic, and other connections among epistemically relevant factors available at the same time to typical or ideal human cognizers who are proficient in using all the inferential and computational resources we know of.[[20]](#footnote-21)

 An epistemic pathway might be represented by a node and edge diagram a little like the ones Judea Pearl recommended for use in programing machines to draw reasonable conclusions from uncertain premises. For example, if p is uncertain, but has a probability close enough to 1, you might want a machine to infer q from p and (p & p⊃q) even though the probabilities do not rule out ¬q. Furthermore, p can change the probability, and hence the credibility, of q and to different degrees when the premises are taken together with other propositions of different probabilities. Nodes in Pearl graphs are made to correspond to probabilities of propositions, and edges, to relations between values of the nodes they connect. If there are exactly two nodes, X, and Y, connected by a single directed edge running from the former to the latter, then, assuming agreement over how high a probability to require, Bayesian probability theory can decide whether the assignment of a value to X requires the machine to assign a high enough value to Y to warrant its inference. If X and Y are connected indirectly

by nodes and edges in a linear chain, the value of each node can be calculated conditional on its immediate predecessor until one gets to Y. In more complicated (hence more realistic) cases, one or more of the nodes will be connected directly or indirectly to other nodes in a branching network with or without feedback loops. In all of these cases, conditional probabilities decide what it would be reasonable for a machine to infer.

 Unfortunately, epistemic pathways are not susceptible to any such uniform treatment. For one thing not all of the factors along typical epistemic pathways are propositions, and not all of them bear on credibility in the same way. To illustrate this, along with my reasons for thinking that node and edge epistemic pathway diagrams can be philosophically useful, here is a very rough description of part of an epistemic pathway leading from experimental results to the credibility of a Higgs boson existence claim, **C.**

 Although a crucial pathway segment runs from particle collision data, to C, the relevance and reliability of the data depend in part on the use of computer models, and mathematical techniques to time collisions, calibrate detectors, and select data for storage. Rather than determining probabilities of propositions along the pathway, these factors help decide what data will be produced, the errors it is liable to, and the extent of its reliability. The same holds for physical facts about the environment and the equipment. For example, the equipment must be buried deep enough to shield it from confounding solar radiation; the detectors must be physically such as to make them sensitive to the relevant particles; and the equipment must be in good working order. The contributions of factors like these is causal rather than inferential or computational. It is better to think of their epistemic significance on analogy to promoters and inhibitors in causal pathways than in terms of probabilistic relations among propositions in a Pearl network.

Theories, models, and background knowledge contribute in too many ways to too many kinds of inference and derivation to capture in a node and edge diagram where every edge is interpreted in the same way.

 The main reasons to look for Higgs bosons came from the need to solve difficulties in standard particle theory. (Butterworth 2014 pp.105--132) But according to that very theory, Higgs bosons carry no electrical charge, and decay too rapidly to register on collision detectors. Fortunately, there was good theoretical reason to think their decay should release signature arrays of detectable particles. Having chosen to look for instances of a signature pattern featuring electrons and muons, experimenters set up a magnetic inner tracker to deflect charged particles into telltale curves from which they can be identified. A second layer of detectors was lined with tabs that scintillate in response to hadronic particle jets traveling to them through the inner tracker. Calorimeter responses to heat from the scintillators provided indications of jet momenta. Muons pass through these first two layers to an outer layer of detectors that measure their momenta. Elaborate computations involving both empirical and virtual data along with a variety of mathematical techniques and physics principles convert these detector responses into data. The data are interpreted to reconstruct (i.e., to model) the streams of collision products that first entered the detection equipment. Given the standard theory along with background knowledge, the data raised the probability of a Higgs boson signature event to a degree that made C credible. Clearly the factors along the pathway, their interconnections, and their contributions to the credibility of C are not all of the same kind. Heterogeneity is the rule rather than the exception, not just here, but in many other cases.

 Even so, philosophers can learn a lot by considering individual epistemic pathways. With regard to empiricism, epistemic pathways for the ATLAS example are remarkable for the lack of significant connections between perceptual evidence and the credibility of claims about the Higgs boson it was used to confirm. The epistemic pathway in which perceptual experience figures most prominently leads from displays and reports of ATLAS data and pattern reconstructions to the credibility of perceptual beliefs about the experiment and its results. Apart from helping investigators access the data, and reports of interpretations and conclusions drawn from it, the main epistemic significance of perceptual evidence derives from its use in running, monitoring, and adjusting the equipment, setting up future runs, and so on.

 There are of course other cases in which perceptual evidence comes close to being as important as an empiricist would expect. Cajal’s drawings are examples. Here looking at an epistemic pathway from them to the credibility of his anatomical claims can help us appreciate the significance not only of what he saw, but also, of features of his stains, his microscopes, the tissues he prepared, the methods he used to guide his drawing, and so on. Among other things, this can help us raise and answer questions about errors and their influence, and about how changes in his methods might have affected the credibility of his claims.

 **viii. Conclusion**. Empiricism was quite right to emphasize the importance of experimental results in the development and evaluation of scientific claims. But it was wrong to think that most, let alone all, experimental results are records of perceptual experience. Ignoring equipment and computer generated data as they did, empiricists were also wrong to think, as Herschel put it, that

[experience is]…the great, and indeed only ultimate source of our knowledge of nature and its laws.(Herschel, 1966 p.77)

Claims like this are correct only in the uninteresting sense that we use our senses to access data, and conclusions drawn from it by looking at measurement displays, reading observation reports and published papers, listening to formal and informal verbal presentations, and so on. Finally, it was a mistake to think that scientific credibility can be modeled very informatively in terms of a two term relation that connects every credible scientific claim to evidence that makes it worthy of belief.

 Recognizing the shortcomings of empiricism opens the way to looking for new stories about when and how data and other factors contribute to credibility. I say ‘stories’ because data are produced and in too many different ways to be captured informatively by any single general account. Philosophers who abandon empiricism can confront two major issues concerning the epistemic value of empirical and virtual data. The first has to do with the usefulness of data as indicators of the presence or the magnitude of the item they are used to measure. The second arises from the fact that data about one item are usually produced for use in answering questions about some further item—often one that does not register directly on sensory systems or experimental equipment.

 Recall that fMRI images are often used to measure levels of neuronal activity for use in finding out about anatomical or functional patterns of activity that support specific cognitive functions. Examples of the first issue give rise to questions about how specific features of equipment calibration and use, statistical manipulations that are parts of fMRI data production, environmental factors, etc. bear on the resolution, precision, and accuracy of neuronal activity measures. An instance of the second issue arises from the fact that fMRI equipment, calibration, and use often varies from laboratory to laboratory, and even from trial to trial in the same laboratory. As Jaqueline Sullivan points out, such differences raise vehemently interesting questions about which differences bear on the credibility of specific claims about connections between neuronal and cognitive processes, and how strongly they affect their credibility.[[21]](#footnote-22)  For example, to control for anatomical or functional idiosyncrasies peculiar to specific subjects, neuro-scientists often average fMRI data from a number of resting brains and compare the results to averaged data from the same or other brains engaged in a cognitive task. To allow investigators to compare results from different trials, Individual and averaged results are projected onto a common atlas constructed from images of individual brains. Because various methods are used to construct atlases and project fMRI measures on to them, there can be appreciable discrepancies between their representations of anatomical landmarks or activity locations. (Woods,1996 pp.333-9) It should be obvious that the epistemic significance of any given epistemically significant factor will depend in part on what is being investigated. For some purposes (e.g., studying fine grained differences between right and left brain cognitive processing), discrepancies among averaging techniques, atlases and projection methods can make a big difference. For others, (e.g., studying resting neuronal activity in neuronal populations whose positions and functions don’t vary much over healthy brains) the differences might not matter very much.

 A second example: fMRI data typically consist of grids divided into pixels marked by numbers or colors to represent levels of activity in voxels of neuronal tissue (e.g. 1mm3 in volume). Pixels seldom if ever map cleanly onto anatomically or functionally distinct brain regions. The voxels they represent may be large enough to contain neurons doing all sorts of things that aren’t especially relevant to the cognitive function of interest. Alternatively they may be too small to contain a population that supports it. If a cognitive function is supported by neuronal activities distributed over various parts of the brain, there is no reason why those activities should have to be carried out by neuronal populations that fit tidily within the boundaries of any selection of voxels. This raises questions about how to produce fMRI data that are most susceptible to informative agglomeration and interpretation. For instance, what size voxels would best serve the investigators’ interests? How many images should be averaged, and how should the averaging be managed to avoid obscuring anatomically or functionally significant details? And so on. (Glymour and Hanson forthcoming)

 I believe that philosophers who free themselves from the constraints imposed by 1.Epistemic Anthropocentrism, 2. Perceptual Ultimacy, 3. Epistemic Purity, 4. Scope Empricism, and 5. Two Term Confirmation will be in a far better position than empiricists to study and learn from issues like the above. And I believe this can shed new light on philosophically important features of scientific practice.

**Works Cited**

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2. The reliability of induction from perceptual experience was the main issue that separated empiricists from rationalists according to Leibniz (1949, p.44) and Kant (1998, p.138) [↑](#footnote-ref-3)
3. Michael Polanyi’s promotion of emotions and personal attitudes as sources of knowledge (Polanyi 1958 pp.153--169172ff. ) qualifies him as an exception, but his objections to empiricism are not mine. [↑](#footnote-ref-4)
4. This idea is not dead. See Gupta 2008, pp.3ff. [↑](#footnote-ref-5)
5. See Bogen and Woodward 2005, p.233 [↑](#footnote-ref-6)
6. For an early and vivid appreciation of this point, see Feyerabend (1993a) [↑](#footnote-ref-7)
7. Thanks to Joel Smith for this example. [↑](#footnote-ref-8)
8. I understand operational definitions in accordance with Feest 2005. [↑](#footnote-ref-9)
9. For a discussion of this, and an interesting but overly strong set of associated pragmatic conditions for observation reports, see Feyerabend, 1981 pp.18-19. [↑](#footnote-ref-10)
10. Bogen, 2009 p.11 [↑](#footnote-ref-11)
11. Cp. Winsberg 2013. [↑](#footnote-ref-12)
12. A lot of what I have to say about this originated in conversation with Sandy Mitchell and Slobodan Perovic. [↑](#footnote-ref-13)
13. Neurath is paraphrasing Schlick, 1959, p.209—10. [↑](#footnote-ref-14)
14. Cohen 1996, Lazar et al, 2001 [↑](#footnote-ref-15)
15. The fact that different theoretical assumptions would call for the production and storage of different data is not to be confused with Kuhn’s idea that people who are committed to opposing theories have different perceptual experiences in response to the same stimuli**.** (Kuhn,1996 pp. 63ff., 112 ff.) [↑](#footnote-ref-16)
16. Bogen and Woodward 2005, pp. 242--6—234, Earman and Glymour, 1980 [↑](#footnote-ref-17)
17. Online. file:///Users/admin/Desktop/ox%20handbook/Shingles%20-%20NYTimes.com.html. [↑](#footnote-ref-18)
18. See Alon 2007 chpt. 2 for a description of how the process might be represented, and Burian 2004 passim for an indication of its complexity. [↑](#footnote-ref-19)
19. For discussion and examples of this approach see Ross forthcoming. [↑](#footnote-ref-20)
20. This is analogous to studying credibility in what logical empiricists called the context of justification. It would take a time indexed dynamical treatment to study the development of a claim or its credibility or to answer historical questions about sources of knowledge. [↑](#footnote-ref-21)
21. Sullivan 2009 [↑](#footnote-ref-22)