

“How do we know what we know?” is both a generic philosophical question, and an empirical question that ought to be raised every time we attain to new knowledge—especially if the knowledge is spoon-fed to us. Yet it is a question seldom asked, perhaps because answering it seems like too much work. I have found, though, that to habitually ask this question is both intellectually liberating, and the necessary precondition to the attainment of real knowledge and understanding. And a consideration of the generic philosophical issues leads naturally into a study of the more specialized epistemologies of science, and of the law—the two essential human methodologies for pursuing, and validating, the truth. Both are, like all human endeavors, subject to breaches of integrity, and as institutions to deep corruption. But both also incorporate self-corrective mechanisms that help prevent them from straying too far off course.

The Epistemology of Science

I follow a number of specific sciences, and scientific theories, in a casual way (I’ve been a subscriber to the reliable and concise *Science News* for almost 40 years), occasionally dipping down into the specifics of published books and studies in various fields. Most recently, I’ve gotten quite involved with the application of genetics (via DNA testing) to the unraveling of family and human population history. One of the principal takeaway from all this reading is that science tends to bite off much more than it can chew, and that most of the findings that are breathlessly reported in the latest journals and magazines are strictly provisional, and likely to be overturned, or at least seriously questioned, in the next edition.

But the very provisionality of science is also one of its great strengths. As Popper argues in *The Logic of Scientific Discovery* (1934), and its sequel, *Conjectures and Refutations: The Growth of Scientific Knowledge* (1963), nothing is ever proved in science: all hypotheses remain perpetually open to revision. Indeed, it is the chief task of the scientist to devise tests, not to validate, but to falsify his hypotheses, because only through falsification (or more usually partial falsification), is progress made— by revising the faulty hypothesis then testing again. Only the facts, says Popper, are known for sure. Harold I. Brown in *Perception, Theory, and Commitment: The New Philosophy of Science* (1977) argues, convincingly I think, that even facts are hypotheses.

But what about scientific laws? Aren’t they necessarily true? Without getting in subtles of Kantian analysis, in a word, no. Scientific laws are such well-established hypotheses that we may reasonably predicate even our most important choices in life on their truth, but they yet fall short of the necessary truths of mathematics and pure logic. Science, in a certain narrow sense may be considered a system of mathematical reasoning based on certain accepted postulates, but its broader value to us as human beings trying to make our way in the world derives from its applicability to the universe as we know it, and in fact our knowledge of that universe may be considered at present a wild extrapolation from a bare minimum of data. Even the most fundamental physical constants, like Planck’s Constant, or such fundamental principles as entropy need not hold everywhere in this universe, and over infinite time, and for all we know there are other universes where things work entirely differently.

The Sociology of Science

Meanwhile, Thomas Kuhn, in *The Structure of Scientific Revolutions, 2nd edition* (1962) brings into consideration the social factors affecting scientific opinion: Kuhn argues that science at any given moment in history, is a matter of consensus, and that scientific knowledge grows, not through an orderly but unending cycle of hypothesize (experiment—revise—experiment—revise...), but through a succession of unpredictable revolutions in the scientific consensus.

Kuhn’s work has been deservedly influential, and has been rightly applied to many other kinds of intellectual communities. Indeed, it has perhaps inspired Randall Collins, in *The Sociology of Philosophies: A Global Theory of Intellectual Change* (1998) to extend it to all intellectual (including religious and artistic) communities. But Kuhn’s, and Collins’s theories are sociological, not epistemological in

character. Scientific epistemology stands on its own two feet, and is thoroughly captured by the formulations of Karl Popper and his successors. All thought is in the end individual, and if there were only one scientist, we would still recognize him as such by the fact that he practiced the scientific method regardless of anyone else's opinion of his work.

That noted, there's something to be said for Kuhn's concept of the scientific paradigm. Science does seem to progress occasionally by leaps, rather than by steady accretion, and the sociological factors introduce an adversarial element, as in the law, which may on balance accelerate the persistent search for the truth; or it may, on the other hand, produce mostly noise and distraction. I have no strong opinion on that score.

The paradigm idea raises other questions. An intellectual paradigm in Kuhn's sense is analogous to the set of postulates that define a branch of mathematics—such as the Euclidean postulates at the foundation of classical geometry. Science in general is presumed to share the same set of postulates. How then, do we account for the many instances noted in Kuhn and in science in general, in which scientists seem to be talking past each other—not even inhabiting the same universe of discourse?

I think the answer to this question is compound. First, considering the paradigm shifts in the most fundamental science, physics, over the last two hundred years, it is evident that occasionally postulates themselves are subject to change. A key to the acceptance of Einstein's theories of relativity was the overthrow of the postulate of "the ether". This was a rather tenuous postulate to begin with, and not one of longstanding, but it still took something like a scientific revolution to overthrow it. However, it is a characteristic feature of science at any given point, that the present paradigms are fully capable of accounting for the old ones in purely scientific terms. Thus, the shift from the Newtonian perspective to the Einsteinian one was merely a generalization of the old science to a new higher level, not an overthrow of it.

Metaphysical vs. Scientific Hypotheses

However science at any given time is capable of harboring other more insidious kinds of mutually incomprehensible paradigms. A case in point is the theory of evolution, or rather, the theories of evolution—for they are many. For an account of the rich history of the theories of evolution, I recommend *Darwinism Evolving: Systems Dynamics and the Genealogy of Natural Selection* (1997), by David J. Depew, and Bruce H. Weber. In the Creationist vs. Evolutionist debate the Creationists are quite right in pointing out that some, or even most forms of evolutionary theory aren't scientific at all. Of course the Evolutionists in this debate are right too, when they allege the same thing about Creationist theories.

Karl Popper has anticipated this situation too in his distinction between *scientific hypotheses* that are designed to be subject to falsification, and *metaphysical hypotheses* that are not even in principle subject to falsification. Hypotheses about God are of this character, even the argument from design, which, as Hume pointed out, is essentially an empirical argument derived from our apparent predilection to view the world in terms of cause and effect; but how can one infer a necessary metaphysical being from a chain of causes that reaches back far beyond the scope of our possible knowledge? the argument from design, and thus Creationism, is hamstrung in exactly the same way as most evolutionary hypotheses, and while we are at it Global Warming: given the gross inadequacy of the data, they aren't falsifiable even in principle. Christianity, Evolution, and Global Warming, are all just so many religions, which, I hasten to add, I do not mean to disparage as such, as long as they don't overstep their bounds and claim to be grounded in science.

The fact is, the propositions of history can never be scientific because they aren't subject to falsification, except in infinitesimal part. We might, in certain cases, be able to show that particular events did or did not occur in history by inferences from their residue, but we can never, even in principle, reconstruct the complete historical context sufficiently to permit historical hypotheses like

evolution by natural selection, or global warming, to be subject to robust falsification. If we narrow those hypotheses sufficiently, and expand our data exponentially, some of them may legitimately be allowed to creep in under the scientific tent as long as they behave themselves. For example, some of the Galapagos studies, and other more recent ones, have demonstrated the operation of natural selection in the wild over a mere matter of decades, but we can't reasonably extrapolate from that, that natural selection has been the principal mechanism for evolution over millions of years.

Global Warming: a Textbook Case of a Metaphysical Hypothesis

Similarly, Global Warming is dismissable out of hand as a scientific theory, merely from the observation that we barely have sufficient data today to account even for the distribution of temperatures across the globe, and the scant scientific data we have to represent the past goes back just a few decades. True, we have the tree rings, which go back a number of centuries (centuries out of millions of years!), but it's ludicrous to think that we can make strong inferences about global temperatures just from a sampling of well-preserved trees of just a few species in a few isolated areas. And of course the actual tree ring data can just as well be used to argue for the metaphysical hypothesis of Global Cooling, or more modestly for episodes of global cooling, like the Maunder Minimum (1645-1715) that at least has some independent support in the historical record.

Global Warming is also dismissable out of hand as a scientific hypothesis by the common sense observation that with all of today's scientific observations, weather expertise, and supercomputers, we can barely predict the weather two weeks hence, let alone two years, two hundred years, two thousand years, or two million years.

The Global Warming hypothesis is actually a congeries of separate hypotheses that all have to be valid for the pattern to hold, and to take just one of these subordinate hypotheses, the "greenhouse effect", one could in principle, concoct a lab experiment that demonstrated the postulated mechanism underlying that effect (or one of the possible mechanisms), but unless all of the actual climactic conditions that have been operating over the last 100,000 or so years can be recovered, and unless the climactic conditions extending far into the future can be predicted, simplistic lab models prove exactly nothing about what actually has, or will happen to the globe. I picked 100,000 years as the relevant period, because any macro theory of climactic change also needs to be able to factor in the ice age cycle (if it is a cycle), but the ice age theory is itself appears at present to be a metaphysical, and not a scientific hypothesis.

The Epistemology of the Law

Unlike science, in which all truths are provisional and subject to falsification by testing, in the courtroom truth is determined by a consensus of human judgements, and it is determined once and for all (allowing for the appeal process to run its course).

There are occasions where scientific evidence, and the scientific method are allowed into the courtroom, but unfortunately few jurors or jurists understand science or the scientific method sufficiently to take proper account of the meaning and weight of such evidence. In fact, jurors who might have this ability tend to be systematically excluded from the judicial process, and witnesses with scientific knowledge and acumen are subject to the same sorts of personally discrediting cross-examinations as any other kind of witness, and the *ad hominem* bullshit tends to outweigh the scientific evidence they present. Not that cross-examination of such people is inappropriate, because witnesses with scientific credentials often have only their unscientific opinions to offer about key features of the evidence, which is after all mostly historical in character and not subject to falsification, and it is their credentials and authoritative pronouncements that are meant to persuade, not their scientific knowledge.

Truth in the courtroom is arrived at by an adversarial process in which competing theories, or narratives that account for all the facts of the case, are retailed to the judge or jury by the two adversarial parties. In principle, either judge or jury could reject both stories in favor of a third story of their own devising, but I think that this must rarely happen, because it's hard enough to come up with any narrative that explains all the facts, and chances are that the two attorneys, if they are competent, will already have latched onto the most plausible ones.

The adversarial aspect of the proceedings pretty much guarantees that each story will be subject to severe criticism, and in principle this will stimulate the judge and jury to think actively and critically about the facts and thereby arrive at some reasonable approximation to the truth. This is a good theory and a good system, but like any other human institution, the court system is subject to deep corruption, and ours has long since arrived at that state. In America, most important verdicts, criminal and civil, are rendered by juries—supposedly a jury of peers of the parties at trial—but by the time the jury selection process has run its course, the outcome of the trial has largely been determined (although it may still be unknown) even before the facts and theories of the case have been presented. Knowledgeable and independent-thinking jurors aren't wanted, and will seldom survive *voir dire*. The typical courtroom proceeding is as much an exercise in jury manipulation, as in trial by presentation of competing narratives.

Nonetheless, I believe that the epistemology of the law provides us with the best model for the epistemology of history, and that scrupulous historians, in fact, have a much better chance of arriving at the truth by applying this model, than the participants in courtroom circuses.

The Epistemology of History

History, like the law, is an inquiry into past events, whose reconstruction isn't subject to the scientific method, because the events are not repeatable.

One of the great strengths of the courtroom paradigm is its constant focus on epistemology. Hearsay testimony is generally excluded, and even the most credible witnesses may be discredited by showing that their testimony wasn't, or couldn't have been, based on personal knowledge. With respect to documentary or physical evidence, only the original, or, under certain circumstances, the best extant substitute, will do.

Unfortunately, historians are at a disadvantage with respect to these stringent rules of evidence. Most witnesses are dead and not subject to cross-examination; they can be brought into the courtroom only by documentary means, like deponents. And even so, the contents of most historical documents would still have been hearsay even if their recorder were present to testify in court. For example, even a prime piece of genealogical evidence, such as a birth record entered contemporaneously by a town clerk, is only a transcription (and possibly an erroneous one) of what the clerk was told. The one rule of evidence historians can, and should, insist on is recourse to the best documentary source. Otherwise these inherent evidential deficiencies must be overcome to the extent they can by constant application of the epistemological question ("How did this person know that what he said was true?"), and by careful weighing of the evidence in its broad circumstantial context.

And here we arrive at the essential epistemological methodology of history. Where courtroom attorneys elicit, and/or construct, the most plausible narratives to account for all the demonstrable facts of the case, historians to be credible must recreate the circumstantial historical contexts within which the events they're reconstructing took place. Most genealogists fall short in this regard, because they have insufficient understanding of the laws and customs of the time and place, and/or of the broader historical contexts, but most of all they tend to focus too narrowly on individuals or individual families, instead of thinking in terms of neighborhoods, of extended families (who may live in other areas), or of "allied families" (including in-laws) within the local communities in which most of our ancestors lived.

The libraries are full of local histories with embedded biographical or genealogically relevant evidence, and these have deservedly been criticized by academically credentialed historians for their lack of scholarly rigor. But these kinds of books can be invaluable not only to the family historian, but also to the academic historian, by providing only slightly retrospective windows into vanished times. Typically books of this sort were published in the mid to late 19th century, and they look back several generations from there, but most are based largely either on the accounts of old-timer participants in the events, or at least of their closely related survivors. Hearsay, in such cases, must simply be ruled admissible, though we should do our best to validate the claims independently, and we may draw the line at double or triple hearsay, since each retailing of a story introduces opportunities for corruption and error.

I mention local histories only by way of example. There are many other kinds of books and evidence available to the historian for recreating the contexts of the past. Arguably the most important to the American family historian is the law itself, meaning the common law, because most surviving public documents are legal documents, and their correct interpretation depends on an understanding of the legal implications of their content, and also on the ways in which an individual document may vary from the norm. One of the best ways of learning about the laws, customs, and general preoccupations of the times is reading ones way through the old court books—not only the order books, which tend to be skimpy on details, but the preserved chancery cases. For the family historian fortunate enough to find a chancery case involving people he is researching, rare personal and biographical information may be recovered therefrom.

But the records aren't enough by themselves. One needs to learn a good deal of the relevant law to interpret them correctly. For example, for those who prospect in the area of colonial and early federal Virginia, [*Hening's Statutes at Large*](#) is indispensable, and there is no excuse not to resort to it, because it is available online, and indexed. An excellent book on the colonial common laws affecting women is Marylynn Salmon, *Women and the Law of Property in Early America* (1986), and there are many similar books and resources covering other aspects of law and custom in the various colonies and states.

Making a Strong Historical Case

Most of the evidence available to the historian is circumstantial, and many people imagine that since circumstantial evidence is inadmissible in court, that it is somehow of lesser weight than the direct evidence that so often seems to be lacking. But both the premise and the conclusion of this argument are wrong. Not only is circumstantial evidence admissible in court, it can be argued that all evidence is circumstantial, because even direct evidence that supports an unequivocal inference depends on an implicit mesh of circumstantial patterns for its interpretation. Thus, even supposed “gold standard” evidence, for example, the testimony of two eyewitnesses, may be discredited in court by showing that certain circumstances render their testimony suspect, or that other circumstances cast doubt on whether the testimony of one wasn't influence by the other.

The strongest case, in law or history, is the one that is replete with integral circumstantial detail, because any narrative constructed in the light of a complex mesh of circumstantial detail must be severely constrained in its possibilities. Where the historical contexts are fully developed, any particular narrative reconstruction of events may be wrong in certain minor details, but is almost guaranteed to be right in the main, because no other substantially variant account can conceivably explain all the known circumstances.

Historical Proof Standards

The chief professional accrediting body for genealogists, the Board for Certification of Genealogists, has adopted a “[Genealogical Proof Standard](#)”, which should be of interest to all historians. I subscribe to the evidential principles itemized in connection with this standard, with minor reservations, but I do

not like the term “proof”, which is appropriate only to the field of mathematics, or in a more specialized and limited sense in the courtroom setting.

As philosopher of science, Karl Popper has argued, even scientific propositions aren’t subject to proof—only to falsification, and if decisions at law are final, and thus a case may be said to have been “proved”, that is an epistemological weakness of the legal paradigm. In science, and in history, the case always remains open to the emergence of new evidence, or to a more judicious interpretation of old evidence, perhaps with the aid of newly discovered techniques like DNA testing, or simply new analytical methodologies.

That noted, it is worth considering in some detail the three proof standards that have evolved in the law. The most relaxed is the *preponderance of evidence* standard applicable in most civil cases, which decrees that if the weight of the evidence tips ever so slightly on one side of a proposition, then it shall be considered either proved or disproved. BCG used to wrongheadedly promulgate the preponderance of evidence principle as their proof standard, thus implicitly endorsing a bare minimum criteria that any serious genealogist ought to find inadequate.

A much more rigorous, but at the same time vague and unquantifiable, legal proof standard is the requirement that evidence for a proposition be “clear and convincing”. This has a nice common sense feel to it, but it begs the question: convincing to whom? Convincing to the average judge? The average (dumbed down) juror? The average man in the street? Since this formula antedates the blatant jury manipulation practiced today, let us presume that what is meant is that the evidence, or the case, must be “clear and convincing” to the average juror, who is a true peer of the parties at law. It still leaves us with questions about just how convincing the evidence must be—marginally convincing, as in preponderance of evidence cases—or enough to convict someone of a crime, perhaps with a heavy penalty. It is time now to consider the third and most rigorous legal proof standard: “proof beyond reasonable doubt”.

At first sight, this standard seems subject to the same criticisms as the “clear and convincing” standard, but I think most people would agree that it raises the bar much higher—as it should if it is to be adequate to convict someone of a crime carrying a heavy punishment. But we would still like some quantification to guide us in the interpretation of the term “reasonable” in this formula.

As it happens, there have been some studies that provide quantification for what the average juror considers to be beyond reasonable doubt. In one study^[1] the subject participants were first trained in the quantification of subjective probability assessments, and then asked at what percentage probability

¹ Harry D. Saunders, "[Quantifying Reasonable Doubt: A Proposed Solution to an Equal Protection Problem](#)", in *Beppress Legal Series*, 7Dec2005, Working Paper 881. This study also argues convincingly that the standard should be explicitly quantified by the several state legislatures to reduce the many egregious miscarriages of justice. The results obtained in the study support the author’s argument on this point, as the stated thresholds for conviction ranged from 30-100%, which is to say that one of these prospective jurors would have convicted even though the evidence was heavily on the side of the accused (meaning that the case would have presumably been dismissed summarily by the judge), and in the 100% case the party would have always voted to acquit, no matter what. Since remonstrance failed to budge either of these parties, these extremes might be written off as anomalies, but it’s bad enough that an appreciable proportion of responses clustered in the 80% range, and quite a number insisted on at least 99.9% probability to convict.

It has been pointed out by critics of quantification that a juror’s interpretation of reasonable doubt should ideally be predicated on the balanced utilities of tolerating wrongful convictions, versus letting criminals go free, and that these are, and should be, affected by the degree of the punishment—which jurors are at the present time instructed to disregard. Despite this prohibition, it is argued, jurors nonetheless do take the possible punishments into account, and typically raise the epistemological threshold for capital crimes. It is therefore argued that specifying a particular quantified proof standard for all cases would tend to overbalance the scales in favor of wrongful convictions, but IMO this is a criticism, and a justifiable one, of the justice system, and not of the quantification proposal. Instructing juries that they must disregard the punishment when deciding on guilt or innocence is just another of the many judicial usurpations that have corrupted our original common law jury system.

they would vote to convict “beyond reasonable doubt”. The results were reported in the form of a frequency distribution, although no mean value is provided. The largest cluster, the modal value, was at the 90th %ile, and the other principal clusters were, as one might expect, at the 95th and 99th %iles, in decreasing order of frequency, with the mean lying somewhere around 92%.

I note that the quantified results of modern scientific experiments are mostly subject to a strikingly similar probability quantification. To be considered of “statistical significance”, and therefore worthy of note in a peer-reviewed scientific journal, a result must show at least a 95th %ile (20-1 odds) of not being due to chance, or, to adopt a more stringent criteria, a 98th %ile (50-1 odds) level.

Historians, including family historians and genealogists, must make the best of their evidence, and if they think that the evidence is preponderant on one side of a proposition, they have an obligation to provide their readers with some idea of just how preponderant the evidence is. This need not be done in a strictly quantitatively way, by affixing numerical percentages, but there should at least be a judicious and consistent use of qualifiers attached to any important historical proposition. All too often the historian’s conclusions are presented *ex cathedra*, without any qualification at all, and served up, to boot, with a godlike pretense of objectivity.

If there are to be “proof standards” for historical or genealogical propositions (but please, let us use a less absolute word than “proof”), let them aim high, and, for clarity, imply a quantified standard. Speaking from experience, I think that a 95% probability is well within reach for many of the kinds of propositions family historians put forward, and I think they are therefore entitled to consider such propositions as established beyond reasonable doubt. As long as the historical case always remains open, as it will and must, I see no reason why historians may not strike a note of confidence with respect to their better grounded propositions, but the work of many (typically the very ones who skimp on the presentation of supporting evidence) would be improved if they served up their less well founded claims with appropriate qualifiers. And a word to editors: editing out qualifiers in historical writing may improve the style, but it weakens the credibility of the matter

There are situations where quantified probabilities can be legitimately introduced into the argument for historical propositions. These are cases where the arguments, though not strictly based on falsifiable scientific hypotheses, may nonetheless appropriately use statistics and other scientific methodologies. Certain conclusions derived from yDNA testing, for example—mostly negative conclusions—are a case in point.

Another obtains in populations for which it can be shown statistically that most adhered to a certain pattern. For example, as I have argued [elsewhere](#), in the Scotch-Irish settlements on the American frontier, and in Scottish families in general, a particular onomastic pattern was generally followed in the naming of children, at least for the first three of each sex, and where the names of all the children and the birth order can be ascertained, it can be concluded beyond reasonable doubt that the pattern was followed in any particular family. The probability of this can even be quantified by reference to a study I have made of the frequency of particular Scottish given names. And in families where the pattern is thereby shown to obtain, strong inferences can be made about the names of otherwise unknown parental ancestors, or children.

Conclusions

In sum, I believe that the epistemology of the law, including the standards of proof and evidence that obtain in the courtroom, can serve as a model, with certain modifications, for validating the propositions of history. The scientific method, *per se*, is rarely applicable to such propositions because history cannot be rerun like a lab experiment. Still, aspects of the scientific methodology may occasionally prove useful, and the Popperian philosophy, that the propositions of science must always remain open, and can never be proved, only falsified, or falsified in part, applies equally to the propositions of history.

A corollary of Popper's characterization of the scientific method not heretofore discussed, is that since hypotheses should be designed to be falsified, the ones that are most easily falsified—namely the boldest ones—are the best, since they speed up the revise-test-revise cycle. This dictum may be also be applied to historical hypotheses (even though they are not, in the scientific sense, falsifiable), and it is one of the reasons why genealogists are advised by such accomplished genealogists as Robert Charles Anderson to make complete family reconstructions, including at least estimated birth dates for all the children, even where the evidence is skimpy to non-existent. Such bold hypothetical frameworks are almost guaranteed to conflict with circumstantial data not otherwise suspected of being relevant, and to thereby provide feedback resulting in improved estimates. The Popperian version of the scientific method, works psychologically something like the Socratic Method, using bold assertion to stimulate argument and creative thought. And as further justification for this top-down, theoretical approach to the pursuit of truth, Popper obliges us, in *The Logic of Scientific Discovery*, with an apposite advisory from the German philosopher Novalis, who died in 1801 at the age of twenty-eight:

Theories are nets; only he who casts will catch.