

## Our Knowledge of the Past: Interview with Aviezer Tucker:

Aviezer Tucker was a research associate at the department of Philosophy and Law of the [Australian National University](#) in Canberra (at the time of interview) but has previously worked in America, Europe and Asia and was then a lecturer in the School of Politics, International Studies and Philosophy at Queen's University in Belfast. He resigned from Queen's after being subjected to blackmail and refusing to inform on colleagues; he recounted his experiences via *The Prague Post* in his articles [Informing as a state of mind](#) and [Race to the bottom](#) and readers are advised to avoid Queen's and particularly Cynthia MacDonald and Shane O'Neill. He is currently enjoying fatherhood and working on a book on political philosophy. He aims to obtain appointments in Africa and Antarctica to be able to brag that he has worked on all the continents. He is the author of [Our Knowledge of the Past](#) (also available from [Cambridge University Press](#)). I was able to ask him some questions about this book and its implications.

**Interviewed by Paul Newall (2006)** [Extracted from <http://www.galilean-library.org/manuscript.php?postid=43809>].

**PN:** How do you understand the difference between History, Historiography and the Philosophy of History?

**AT:** History is composed of past events and processes. Historiography attempts to describe, explain and theorize parts of history. Most of history is unknown and unknowable because it did not generate information-preserving effects that survived to the present. Historians who write historiography infer propositions about the past from present evidence.

The philosophy of history attempts to gain knowledge of history that does not depend on empirical evidence, it attempts to by-pass historiography. Philosophies of history typically rely on idealistic epistemology, on intuitions of the essence of history. These intuitions may appear in the guise of "historical self-consciousness." Philosophers of history like Vico or Hegel believed they could gain knowledge of the historical process that resembles self-knowledge. They believed that as we have intuitive self-consciousness of ourselves, philosophers of history could gain knowledge of the past by becoming the self-consciousness of history. Since we can understand a living process only at its end, philosophers of history from Hegel through Marx to Fukuyama believe themselves to be living at the end of history. However, all existing philosophies of history have reflected the conflicting historical consciousness of their age, how they perceived the historical process. Yet, the historical process always goes on towards ends we cannot possibly fathom. The end of history will always happen tomorrow and be accompanied by yet another philosophy of history....

**PN:** The subtitle of your book is "A Philosophy of Historiography". How do you conceive of marrying the two?

**AT:** The philosophy of historiography, unlike the philosophy of history, is a branch of epistemology that examines our knowledge of the past. As much as the philosophy of science asks how do scientists gain knowledge of nature by examining the relations between scientific theories and the evidence, the philosophy of historiography examines

our knowledge of the past by examining the relations between historiography and historiographic evidence. As much as the philosophy of science begins by examining the successful paradigms of Galileo, Newton, Einstein etc., why and how they replaced previous paradigms, the philosophy of historiography examines the history of historiography to discover how and why new paradigms in textual criticism, comparative historical linguistics, historiography and evolutionary biology were established.

**PN:** In your book you distinguish between two alternative approaches to our knowledge of history that you call "historical skepticism" and "historical esotericism"? What do you mean by these?

**AT:** Skepticism argues that there is no knowledge of the past. Whatever consistency we find between historiographies is the result of factors external to historiography, such as political or economic interests. Esotericism claims that there is knowledge of the past, but it is impossible to know how successful historians obtain it. Historiography then is a practice like cooking that cannot be reduced to a set of instructions. I argue that skepticism and esotericism are implausible because they cannot explain the history and sociology of historiography as well as the kind of internal account I develop. Skepticism cannot explain the uniquely heterogeneous broad consensus on parts of historiography. The totalitarian reduction of all opinions to political or economic interests cannot explain why historians of widely different, indeed conflicting, interests and identities agree on so many historiographic propositions. I develop an alternative account that explains better this consensus by shared theories and methodologies concerning the transmission of information in time that may seem trivial yet are incredibly fruitful. I do not like the undemocratic and elitistic implications of esotericism. If knowledge of history is the preserve of an obscure elite that benefits from long apprenticeships with older masters, they cannot be criticized by the unwashed masses who may not have been touched by expensive genius, but can examine the evidence just as well. I think that it can be shown that esotericism is false by presenting explicitly the actual theories and methods that historians use and teach, as I do.

**PN:** In recent years there has been a vigorous debate on the nature of historiography. Why did you consider it necessary to provide a scientific approach to historiography?

**AT:** I argue that a part of historiography is indeed scientific in the sense that it offers highly probable propositions of all kinds (descriptive, explanatory, theoretical) about the past, and indeed has been so for about two centuries, since the breakthroughs and paradigm founding in textual criticism, comparative linguistics and historiography. I also argued that other parts of historiography, marked by sociological fragmentation into schools, are less than scientific.

It seems to me that much of the debate has been marked by over-simplifications and inattention to either historiographic practices or contemporary epistemology and philosophy of science. Both sides overemphasized in my opinion the significance of historiographic texts in comparison with historiographic research and practice. Both sides imported to the philosophy of historiography theories and methods of interpretation that were originally developed for and fitted other disciplines at other times and I do not believe quite fit historiography. I do not think that either side would finish reading *Our Knowledge of the Past* feeling smug or vindicated. I think they need to go back to the drawing board and produce a more sophisticated, complex and above all sensitive to the practice of historiography and its history versions of their original positions. I do hope the debate will

go on. It is an important debate and it can potentially flash out many interesting things about historiography.

**PN:** In the section on the theory of scientific historiography, you rely on Bayesian probability theory to develop a method of comparing explanations. Can you explain briefly what it involves?

**AT:** The three most common lies are: The check is in the mail, if you sleep with me I'll divorce my spouse and, among academics, "I'll be brief." I'll try to be brief, then:

I use an interpretation of Bayesian theory that is closest to the one Elliott Sober developed to account for the inference of common cause, though it differs from Sober in several crucial points. Let me try and explain it informally:

The task of historiography is to offer the best explanation of the evidence. The evidence consists typically of two or more units of evidence like texts, languages, testimonies, species, genomes, or cultural practices that are similar in certain respects. I will use here as an example, two very similar exams that are handed by students to their teacher and deserve a perfect score, to simplify the argument and connect it to everyday experience. I believe that the inference of historiography from evidence proceeds in three stages: First, we ask whether the similarity between the units of evidence is more likely given some common cause, or given separate causes. For example, if two or more students submit very similar exam essays, it is far more likely that there was some common cause than there were separate causes. It is highly unlikely that the students came up independently with exactly the same words in the same order. The same is true of texts in biblical criticism and classical philology or of testimonies in historiography. However, if the exam is in logic or mathematics, two identical, perfect score, exams, are more likely given separate causes, given that the students wrote answered the exams independently of each other because there was a single correct or best answer to each question. Likewise, similar biological traits that convey adaptive advantages such as wings or fins or human behaviors like agriculture, language and fishing may develop independently of each other, from separate causes because they are the best solutions to shared types of problems.

Second, if we are able to establish that the similarity between the units of evidence is more likely given some common cause than given separate causes, we need to distinguish five possible causal nets, and find which is the best explanation of the evidence:

- A single common cause caused the similarity between the units of the evidence; for example, both students copied the same textbook.
- Several common causes affected the evidence; for example, the students copied a textbook, an encyclopedic entry and a Website.
- One or more of the units of evidence affected the others; for example, one student wrote the exam and the others copied his text into their notebooks.
- All the units of the evidence affected each other; for example, all the students co-authored the exam together.
- Combinations of 1 or 2 with 3 or 4.

Distinguishing between these five possibilities, finding which one is most probable, requires further analysis of the evidence and/or the discovery of new evidence. For example, the teacher who is certain there was plagiarism but wishes to discover the culprit(s) may consider whether the language of the similar essays is too grammatically and

syntactically perfect, sophisticated or mature for students, which would decrease the probability of 3 & 4 above, and vice versa. The teacher may consider if the students could communicate during the exam with each other and whether they are friends. If the style of the exam is typical of one of the students and he has been a significantly better student than the others, the third option is most likely etc.

Third, once the historian draws a likely causal map, a genealogic tree of information transmission, it is possible to try to infer the actual properties of the various units on the map. In our example, if stylistic or conceptual discontinuities in the text point to multiple common causes, the teacher may attempt to infer which parts of the exam resemble the style of an encyclopedic entry and what this entry may be, and which resemble more paragraphs in a text book and which text book it may be. If the teacher has access to a good library, she may try to match books and encyclopedias with parts of the text of the exam.

If we look at the history of textual criticism, comparative linguistics and evolutionary biology, we will see that they follow these three successive stages of development, though some do not have sufficient evidence to advance to the next stage. For example, comparative linguistics is capable of proving that the similarities between the Indo-European languages are too numerous and detailed to be the result of separate causes. The Indo-European languages are connected in a causal net. However, there is insufficient evidence to favor either the hypothesis that there was a single proto-Indo-European language from which all other Indo-European languages descended, or that there never was such a language, only a group of unrelated languages, spoken by peoples who lived in close proximity to each other and therefore progressively influenced each other's languages until they became quite similar, as in the "wave theory of language." In historiography, however, typically stage one is quite easy, and often stage three is achieved as well.

I consider this part of *Our Knowledge of the Past* to be the most significant, in proving that the philosophically significant disciplinary distinction is not according to the subject matters of the disciplines, between human and natural sciences, or according to their way of describing the world, between nomothetic and ideographic sciences, but between the sciences that infer common causes tokens and the sciences that confirm hypotheses about types of causes. The sciences that infer common causes tokens are the historical sciences, historiography, both of humans and of nature, comparative linguistics, textual criticism evolutionary biology, and probably parts of geology and archaeology as well.

**PN:** One of the main criticisms of Bayesian approaches is the difficulty in assigning prior and conditional probabilities. What problems do these pose for your thinking and for historiography in general?

**AT:** Though historians, unlike biologists who infer phylogenies, do not use explicitly Bayesian formulae, I think that the Bayesian model I developed above is the best explanation of the history and sociology of historiography, of the actual practices of historians.

Though historians do not plug in precise quantitative values to the Bayesian variables, they do make comparative more/less estimates of the values of the Bayesian variables: In the present stage of historiography, when there is a broad network of well-corroborated beliefs about the past, it is fairly easy to estimate the priors of many hypotheses, according to whether they fit everything else we already know about the past. Historians typically

estimate the likelihoods of the evidence given competing hypotheses by examining the information chains that should connect past events with present evidence. I argue that this examination of information chains is the main professional activity of historians. The theories that historians typically develop and use are about the voluntary and involuntary mutation and decay of information in time.

Since I argue that accepted historiography must offer a better explanation of a broad scope of evidence than existing alternatives, it is not necessary to prove any absolute quantitative probabilities, merely that one historiographic hypothesis is considerably more probable than an alternative one.

**PN:** Throughout your book you are keen to emphasise the similarity between your scientific historiography and the methodology used in evolutionary biology. Can you explain the parallels?

**AT:** Natural and human historiographies attempt to infer descriptions of events and processes from similarities between units of information preserving evidence. Units of evidence may be species in evolutionary biology or documents, testimonies and material artifices in human historiography. The inference of common causes in historiography and evolutionary biology follow the same three stages outlined in my answer to question 5. The difference is that in evolutionary biology the historical evolution of the system is identical with the transmission of information over time. When evolutionary biologists make a phylogenetic inference, they trace the evolution of species from present similarities among genome sequences through fossil evidence to an ancient ancestor. Historians of society also infer sections of the evolution of society from information preserving evidence in the present. But the information preserving causal links they study are not identical with the evolution of society; historians are interested in causal-information chains that generated documents or material objects, not the present state of society.

Historically, whether or not Darwin knew about and was influenced by comparative historical linguistics before introducing the theory of evolution (Darwin's cousin and brother in law was a philologist), the British educated public had known and had accepted Darwin's methods of inference of natural history before they read his books. The educated British public had already accepted scientific genealogical trees of languages. The imperial encounter with the languages of the Indian sub-continent induced interest in the Indo-European hypothesis. Those who accepted that inference had already accepted Darwin's method before he even introduced it. Darwin used comparative linguistics as a heuristic analogy several times in his writings to explain his new ideas to an audience that had already been familiar with the new achievements of comparative linguistics.

**PN:** In the conclusion you remark that historiography attempts to provide an analysis of the past via the best explanation of the available evidence. You add that "[t]he most that historiography can aspire for is increasing plausibility, never absolute truth." How does this understanding of historiography differ from what we typically think of history?

**AT:** Parts of historiography have such a high level of probability that ordinary people frequently consider them as facts, rather than well-confirmed hypotheses or theories. For pragmatic purposes of orientation in the world this works just as well. However epistemically, George Washington is an extremely well confirmed and useful hypothesis that explains a wide scope of evidence, not a fact. The Renaissance is a very useful theoretical concept that was introduced in the late nineteenth century by Burckhardt.

**PN:** You are critical of both traditional historiography and the arguments of skeptics. In what ways are the two inadequate?

**AT:** The skeptics cannot explain, cannot make sense of, the uncoerced, large, and uniquely heterogeneous consensus of historians on the Rankean paradigm, its methods and results. The skeptics are better positioned to explain what I call the traditionalist part of historiography, where historiographic schools interpret inconsistently vague large scope theories *ad hoc* to explain a narrow range of evidence. Still, though traditionalist historiography, associated sociologically with historiographic schools, cannot claim a scientific status, it is neither reducible to political or economic interests, nor indeterminate. Though the evidence is insufficient in some cases for discriminating between several competing traditionalist historiographic interpretations of school theories, the evidence is sufficient for rejecting quite a lot of hypothesis that do not fit it. Consequently, I argue that traditionalist historiography is neither determined, nor indeterminate, but underdetermined.

**PN:** Why do you claim that in some respects "the philosophy of historiography is a philosophy of liberation from the tyranny of the present"?

**AT:** Though the philosophy of historiography is a sub-field of epistemology and is not political, it has some political implications. I only hinted at them at the end of the book. Many social and political conflicts have been perpetuated by non-scientific historiographic interpretations. Typically, these historiographies tell a story of group victimization by another group, imply the responsibility of the other group for whatever misery has befallen the favored group since the victimization and the obligation of the current generation to right the historical wrongs and vindicate their ancestors. These are the kind of historiographic stories that both sides to conflicts in places like the former Yugoslavia, the Middle East, Northern Ireland, etc, tell their children. These stories perpetuate these conflicts by teaching young people to hate and repeat the mistakes of their ancestors. If one holds, as the skeptics do, that all historiography is fiction, there is nothing that can be said to prove such stories false. At most one can preach to both sides to listen and understand each other's narratives and be tolerant and respectful of them, hardly an effective means for ending such conflicts. But if we recognize that parts of historiography offer probable knowledge of scientific quality, it is possible to say that some historiographic narratives are plain false or quite unlikely in comparison with their scientific alternatives. It is possible then to tell young people that the historiographic narratives their elders taught them are false or improbable and offer no basis for them to kill or be killed.

For example, Israelis and Palestinians fight over the "graves of the patriarchs" in the town of Hebron. The evidence for the presence of these graves in Hebron is from a verse in the book of Genesis and a tradition that originated in the Roman period. If we examine the evidence from Genesis in its linguistic and textual contexts, we may conclude that the best explanation of this verse is a dispute in the fourth century BCE over the exact borders of the Jewish province in the Persian Empire. To establish a claim to the then disputed town of Hebron, the editors of the then young bible probably added this verse. Further consideration of the architecture of the graves in Hebron suggests that the best explanation of the oldest part of that structure is that "the graves of the patriarchs" are the graves of Edomite sheikhs from the first few centuries BCE. Scientific historiography can then tell both sides that they are not fighting over the graves of their patriarchs, but over old Edomite bones.... Further, if Abraham and Jacob existed, it is highly unlikely that they were buried together, since the Abraham stories all take place in the territory of what would be

the kingdom of Judea, while the Jacob stories all take place in the territories that would become the kingdom of Israel. Abraham's god is Jehovah; Jacob's God is Elohim. The best explanation of these differences is that Abraham and Jacob were originally the mythical ancestor fathers of different tribes. After the kingdom of Israel was destroyed in the eighth century BCE and the Judean kingdom assumed the claim to the Israelite territory and heritage, the two narratives were combined to unite the mythical ancestors of both ethnic groups as the patriarchs of a united nation. If Jacob existed, he was probably buried somewhere much north of Hebron.

Just as false are some narratives of irresponsibility. Scientific historiography can tell neo-Nazi Holocaust deniers that their narrative is not just ugly and vicious, but also plain false. The weight of the evidence is that the best explanation of the material remains in East Europe, the Nazi documents, and the testimonies of survivors is that there was indeed a Holocaust. The Nazis were responsible for the Holocaust.

I am reading now a book on Czech historical memory (Francoise Mayer, *Les Tcheques et leur communisme* (Paris: Editions de l'ecole des hautes etudes en sciences sociales, 2004)). According to the author, Czechs tell themselves a story of national victimization and irresponsibility: Czechoslovakia was destroyed following the British and French betrayal in Munich in 1938. The Communist take over in 1948 was caused by the Soviets with the cooperation of Czech and Slovak young Communists who blamed the West for 1938. The native liberalization of the Prague Spring in 1968 was again crushed by external forces following the Soviet invasion. This story exempts the Czech from self-examination. Contemporary and former dissident Czech historians argue that it is true that Britain and France betrayed democratic Czechoslovakia in 1938. But the mechanized Czechoslovak military could have attempted to match the German military in case of war, unlike the Polish cavalry that fought the Germans tanks in 1939. Czechoslovakia fell apart before it was occupied following the discriminatory policies of the Czech majority that alienated the Slovak and German minorities. The Communist coup of 1948 was indeed carried by a well-organized Communist minority. But it did not receive substantive Soviet support, and the democratic majority could have resisted it had it been sufficiently united and determined. True, the invasion of August 1968 and the following repression were the faults of the Soviet Union, but from late 1969 to late 1989 all the political repression was carried by Czechs and Slovaks against Czechs and Slovaks. Consequently, scientific historiography can tell Czechs that their elders and ancestors have partial responsibility for their sad half century between 1938 and 1989 and examine critically their political culture, rather than just blame outsiders for all their misfortunes.

In a philosophical context, contemporary historiography can say that the story John Locke told about first appropriation in the second treatise on Government is a fairy tale. Genetic evidence proves that by 10,000 years ago all the continents have already been settled by hunters-gatherers. The last piece of global real estate to be first appropriated was New Zealand, about 1500 years ago. Prior to the last 200 or so years, almost all transfers of real estate involved involuntary appropriations. Therefore, first appropriation historical theories of property rights can justify just about nothing. An article I wrote on this topic entitled "The New Politics of Property Rights" will be published in the last 2004 issue of [Critical Review](#).

Scientific historiography can then liberate us from living the lies that others tell us and even from the lies that we tell ourselves. The liberating power of scientific historiography can be effective on a personal just as much as on a national level. To take a personal example: One

of my great-grandfathers, Nisan-Zvi Gwurtzman, was murdered in the Holocaust, probably, given the established fate of other Jews who lived in his last place of domicile, in the Maidanek extermination camp in eastern Poland.

As I was growing up, I was told stories of his religious piousness, moral uprightness, intellectual vigor and business acumen, all models for me to emulate. My family also inherited his library. It included the kind of religious books that a pious man would have been expected to own. But it included also books about professions and businesses: One book had instructions on how to make fizzy light drinks. Another explained how to produce perfumes. The dates of publication of the books were a few years apart. The latest book, from the late twenties, was about how to perform circumcisions (how to be a *Mohel* in Yiddish)... In the early thirties, after obviously a short career in the penis-chopping business, that great-grandfather of mine emigrated together with my grandfather and his wife to what was British Palestine, so much for the astute businessman. The next obvious question was: If he got out of Europe in the early thirties, how and why did he die in the Holocaust back in Europe? One unit of obvious evidence is the grave of my great-grandmother, his wife, who died in Tel Aviv in 1936. So, obviously in 1936 they still lived in Palestine when my great-grandfather became a widower. Then, I needed more evidence that required pressuring some older members of the family to testify. The historiography that emerged out of their reluctant testimonies was that the lonely widower was offered a match back in Europe, to marry the widowed mother in law of one of his older sons. In the late thirties (sic!) he took a boat from Palestine back to Europe to remarry, leaving his library behind for me to examine four decades later, so much for his sharp intellect. In the process, he abandoned my young grandfather in Palestine, sickly, penniless, and without an education or a profession, so much for the moral uprightness. My abandoned grandfather found then a dominant older woman to care for him, who became my grandmother.

These circumstances also explained a few things about the character of my late mother who grew up in a household with a dominant older mother and a weak, sickly, indeed helpless and dependant, father who died in his mid-fifties. When I pieced this family historiography together a couple of decades ago I found this to be a liberating experience, not because I invented an alternative story to the one I was told during my childhood, but because I had good reason, evidence, to doubt the story I was told and to support an alternative historiography that offers a better explanation of the evidence. This revised narrative liberated me from the myths of my immediate ancestors and allowed me to adopt a more critical approach to people who were more human than saintly.

**PN:** Why do you think practicing historians should be concerned what philosophers of history have to say?

**AT:** I do not think that the actual practices of historians are often affected by what they think or learn of them. Still, I think that historians are interested, like the rest of us, in becoming self-conscious of their practices. Most significantly, historians distinguish, often without reflection, legitimate historiographies from illegitimate ones; for example, Nazi revisionism, Bolshevik fabrications and Nationalist forgeries. I think it is valuable to set clearly the conditions that distinguish historiographies that reflect the strong emotions of those who invent them, from evidence based scientific historiography.

**PN:** How do you answer the objection that philosophers do not understand what goes on in the work of historians?



**AT:** Some philosophers indeed did not and do not; their work indeed should be ignored. In relation to historians, the old New York joke about two psychiatrists who meet on the street and say to each other "You are fine, how am I?" is appropriate. Practitioners, scientists, historians or lawyers, rarely gain through practice the kind of abstract self-consciousness that can lead them to self-knowledge. Outsiders must observe them from without to understand what they actually do and what it means. That is why we need philosophies of science, historiography or law. It is helpful of course if the philosopher-outsider has some practical experience of what practitioners actually do and is familiar with relevant analytical skills. Bad philosophies of historiography, positivist or post-modernist, applied to historiography models that had been developed elsewhere and did not quite fit historiography. That led to the perception of irrelevance of philosophy on the part of historians. But the problem was not with the field, but with the particular approaches that have been dominating the field. The solution I attempted to develop in *Our Knowledge of the Past* is to start with a study of the history of historiography and then construct a philosophy of historiography that attempts to offer the best explanation of the history of historiography.

I believe that the history of historiography is the empirical-evidential ground that should unite and decide between competing philosophies of historiography. I would expect any philosophy of historiography that is better than the one presented in *Our Knowledge of the Past* to offer a better explanation of the history of historiography.

**PN:** How can your work be developed further?

**AT:** In *Our Knowledge of the Past* I discussed only textual criticism, comparative linguistics, historiography and Darwinian biology. I left out the history and philosophy of Geology, Archaeology and Genetics. I suspect that the models I developed will be applicable to these disciplines as well. There is also much more to be written about the history and philosophy of biology and linguistics.

Another implication of the elucidation of the scientific historiographic method is the criticism of philosophies that violated these methods. There are several philosophical debates that originated prior to the introduction or spread of scientific historiography, and somehow managed to maintain their "prehistoric" character to this day. I mentioned earlier my criticism of historical theories of property rights that originated with the prehistoric fairy tales of John Locke about original appropriation. I also applied scientific historiography to criticize the contours of the current debate about Hume's *Of Miracles* as prehistoric in my article "Miracles, Historical Testimonies, and Probabilities" forthcoming in the October 2005 issue of [History and Theory](#). Earlier, I subjected Saul Kripke's theory of proper names to the same criticism in "Kripke and Fixing the Reference of 'God'" in *International Studies in Philosophy*, 34-4, 2002, 155-160. The concept of tradition, as used often by hermeneutic philosophers and social scientists, is very problematic as well. I made a few notes about that at the end of chapter four.

Conversely, historiographies that exceed the limits of evidence or violate the rules of inference of common cause from similar evidence must be criticized as well.

**PN:** What are you working on currently? What will your next projects be?

**AT:** I have been working recently on a couple of projects in political philosophy and theory, one on Democratic theory and practice with my ANU colleagues John Dryzek and Robert Goodin, and one on a theory of post-totalitarianism on my own. I expect to return

to the issues I raised in *Our Knowledge of the Past* next year, once the current projects are complete. I would like to write a philosophical historiography of the historical sciences in several volumes. I plan on writing it "backward" start with the volume on contemporary genetics and end with a volume on Biblical criticism. I'll be forty in the summer, so I probably have enough time to write this grand history of the historical sciences.