Accident, Luck, and Serendipity in Historical Research

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. . . le hazard ne favorise que les esprits préparés.
—Louis Pasteur (1854)

ACCIDENT, LUCK, AND SERENDIPITY play an unrecognized part in shaping the works that historians produce. All kinds of seemingly extraneous factors impinge on the research process but remain invisible in the books and articles written by historians. Furthermore, once a piece of historical writing is finished and published, accidental factors are necessarily lost from view and, on that account, are not generally acknowledged in the profession. My goal in this paper is to rescue them for historiography.

When we think of historiography, we usually think of “the literature.” We are interested in debates and conflicting interpretations that historians argue among themselves, research questions they pursue, sources they tap, and methodologies and approaches they deploy to address issues in the literature. Historians have not, to my knowledge, recognized that their works have been shaped in addition by factors such as accident, luck, and serendipity, nor have they included such particulars in historiographical accounts of their works. Yet, once pointed out, the influence of fortuitous elements on individual pieces of work seems almost self-evident. In what follows I want to explore the implications of these accidental circumstances for historiography and historical research.

1 This paper is a revised version of a talk delivered at the Autumn General Meeting on 14 November 2003. An interim version of this paper was given before the “Humanities Forum” at Stevens Institute of Technology on 11 February 2004. The author thanks members of the audience on both occasions for critical comments and suggestions. I owe a particular debt to Susan Babbitt of the Society’s Office of Publications, and to my colleague, Professor Harold Dorn, who burnished a late draft of this paper and whose critical comments clarified many points. I thank Jackie McClellan, whose reading resulted in many improvements.
As an entrée to this topic, I would like to illustrate how accidental factors played out in one particular case I know well, a monograph of mine, *Specialist Control: The Publications Committee of the Académie Royale des Sciences (Paris), 1700–1793.* This work won the 2003 John Frederick Lewis Award from the American Philosophical Society. The Lewis Award prompted an invitation to give a paper to the APS, which led me to begin inquiring into these matters.

Before turning to the hidden story underlying the monograph in question, consider first what a reader is presented with in perusing the published work. In a word, I was fortunate to identify that historical turning point when scientists—the producers of knowledge—first gained control over the publication of the results of their research. Chronologically, that point was at the turn of the eighteenth century, and the locus of this historical union of production and publication was the Paris Academy of Sciences and more particularly its Publications Committee, the Comité de Librairie.

Prior to the institutionalization of specialist control over the scientific press in the eighteenth century, the activities of making knowledge and making knowledge public were largely separate and not governed by the scientific community itself. That Galileo’s *Il Saggiatore*—The Assayer—was published in 1623 by a scientific society, the Accademia dei Lincei, indicates that publication patterns for science were already changing in the seventeenth century. But *The Assayer* still had to be approved by Church authorities, and we know the difficulties Galileo had in securing an imprimatur for his subsequent *Dialogues on the Two Chief World Systems* in 1632 and the fate of that book and its author at the hands of the Inquisition.

By contrast, in the world of science today expert specialists tightly control the publication of research papers. *Science* magazine, for example, publishes only 10 percent of the reports and articles submitted to it. In preliminary reviews a board of reviewing editors composed of 104 eminent scientists from around the world rejects about 65 percent of all papers sent to *Science*. A paper that clears that hurdle then has to be refereed by specialized peer-experts and, if successful, is usually subject to revision; if the paper involves statistics, a separate review by statisticians is required. Nothing in *Science* magazine or science in general today gets into print without being vetted by the community of scientific practitioners. I am sure that each of us in our respective scientific fields

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3 On these points see *Science* 303 (2 January 2004): 102–03; and *Science*’s Web site http://www.sciencemag.org.
social science, or humanistic disciplines has experienced the reality of refereeing and peer review.

The development of specialized control over the scientific press is indicative of the shift from early-modern to modern science that brought into being the ethos of modern scientific norms and conduct: to wit, the advent of institutional sponsorship, refereeing, peer-review, certification of authorship, insistence on originality of results, institutional guarantees of priority, rejection of secrecy, and imposition of norms such as citing prior work and replicating experiments.

How this new kind of specialist control played out in the confines of the Old-Regime French Academy of Sciences is a fascinating story, or at least I think so. The way the Comité de Librairie stifled publication of La Condamine’s work on inoculation is one juicy episode; the way it subtly shifted the theoretical orientation of P.-J. Malouin’s study of diseases in the Paris region is another; the way it suppressed Dr. Ruë’s report of an abortifacient from the Indian Ocean is yet another, and there are many more. If this story is of any interest, I have no shame, because there are no royalties involved, in suggesting that readers order a copy of the monograph from the APS.

The objective summary just presented is belied by the ways in which the coming into being of my monograph was shaped by accident, luck, and serendipity. I use the terms “accident, luck, and serendipity” as technical terms to mean three different things. Consider first that the monograph crowned by the APS is wholly the result of an accident, meaning something arising from contingent circumstances, happening by chance and without any forewarning or expectation. The tiniest trigger was ultimately responsible for my undertaking this investigation of specialist control in science in the first instance. The work did not emerge as part of a research program. Before Wednesday, 5 April 1995, I had no inkling that there was even a subject to be explored, and the whole project might easily not have happened. That afternoon Mr. Daniel Roche asked me a question. The scene was his seminar at the École Normale Supérieure in Paris. I was presenting a quantitative study of the several thousand scientific papers that appeared in the Mémoires of the Paris Academy from 1699 through 1790. At the very end of the seminar session Roche asked me how it was that these papers came to be published, that is, through what editorial processes they saw their way into print. Had Roche not asked his question—had he, like the hero of Camus’s The Stranger, been distracted by this or that, the seminar would have ended, and I would have gone my merry way without ever having stumbled onto the topic of specialist control over the scientific press.
From one perspective everything that brings a historian to a particular study is the result of accident largely considered: the circumstances of birth and background; his or her training and brain chemistry; that some committee decided favorably or unfavorably on a fellowship, and so on and so forth. From this point of view accident means everything and nothing, and the concept is vacuous historiographically. Conversely, we say that, although it was an accident or luck that I won the lottery, surely someone was to win, and therefore, although accident may be important to me, the concept lacks historiographical significance in that the prize would be claimed by the discipline whether or not I won the laurels. The case at hand, however, undermines both views of possible roles of accident in historiography: the accident in question was something very specific, not some vague effect of fate or fortune; similarly, Specialist Control would not have been written had Roche not asked his question. In the world of science we might say that simultaneous or independent discovery uncovers the “same thing” in nature, but that is hardly the case in historical research. Another historian might have similarly stumbled onto the same topic as I did, but the results would not have been the same, and in fact they might have diverged.

In any event I was embarrassed not to have a good answer to Roche’s question, and the next day I hurried over to the archives of the Academy of Sciences on the Quai de Conti. Here luck, in the sense of good fortune, enters the story. With help from the Academy’s documentaliste and my good friend, Claudine Pouret, I was lucky to stumble across the two bound volumes of minutes of the meetings of the Publications Committee from 1749 through 1780. (These registers are stored in plain sight on a bottom shelf on the left just as one enters the present archival storage room.) Historians of the Academy have always known about the Comité de Librairie and had logged the existence of its volumes of minutes. As lucky as I was to find the registers for myself, I was perhaps even more fortunate that no historian had looked at these documents for what they might tell us about the history of the scientific press. I set out to do so in order to answer Roche’s seemingly simple question.


But I get ahead of myself, because for the next couple of years I did not know what the registers had to tell me. As I worked my way line by line through the 266 handwritten pages of minutes, I began to uncover and flesh out the stories to be found there. But at this stage, I confess, I just thought the material was “cool.” For example, I found it hilarious that the Comité granted servants permission to pass through the Academy’s apartments in the Louvre only if they were there to serve or to carry their masters. The picture of the Academy’s secretary rushing off to the print shop to see to last minute corrections was vivid, as was the plea for a raise by the poor Haussard sisters who engraved plates for illustrations. But I had no idea what these and like vignettes meant or added up to. I was just someone enjoying the existential and voyeuristic pleasures of digging around in dusty old archives.

In the fairy tale *The Three Princes of Serendip*, time after time the heroes discover something they need but were not actually looking for, to rescue them from one dire situation or another, and so I invoke serendipity as my last principle to account for the coming-into-being of the published work. What I inadvertently discovered, and what I needed without quite knowing that I needed it, was some sense of what was important and worthy of reporting from all of the material I had surveyed. I needed a story to tell. And, in at last realizing that the story concerned the advent of specialist control over the scientific press, I had serendipitously hit on a key and heretofore unrecognized turning point in the history of science, a topic worthy of writing up and bringing to public attention.

As long as we are being frank about these matters, however, I would not want to take too much credit even for coming up with a finding of historical interest. In fact, the “story” emerged only through interactions with fellow scholars and with referees as I began to write up and refine my narrative. In retrospect, my first efforts amounted to another embarrassment. The distinguished historian of science and longtime APS member Professor Charles C. Gillispie read an early draft, and was kind enough to suggest diplomatically that I limit my enthusiasm for all the detail I had uncovered. The work in question then went through three rounds of refereeing before it assumed the final form that

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6 Horace Walpole coined the word “serendipity” in 1754 from a nominally Persian fairy tale, *The Three Princes of Serendip*, that first appeared in Italian in 1557. See the OED entry and the in-depth study of the history of the word by Robert K. Merton and Elinor Barber, *The Travels and Adventures of Serendipity: A Study in Sociological Semantics and the Sociology of Science* (Princeton: Princeton University Press, 2004), here 14–18. Merton and Barber’s book appeared after this piece was first crafted, but, as will be evident, it has proven an invaluable resource in framing this published version.
was published by the APS. One anonymous referee hated the piece and effectively scuttled its publication as an article in *ISIS*, the journal of the History of Science Society. Had he or she felt less negatively, the work would have appeared in a different forum and in a different form than it ultimately did. The opportunity to develop the argument and presentation more thoroughly in a monograph format was ultimately a blessing, albeit an unexpected and at first a decidedly unwelcome one. Other referees, however, had very substantial and critically constructive things to say about the manuscript, and these “outside” contributions are very much responsible for the analytical framework that ended up structuring the piece in question. In particular, at more than one juncture in the preparation of the work, the interventions of Professor Alice Stroup of Bard College and Professor David J. Sturdy of the University of Ulster at Coleraine proved crucial in this regard. (The irony of a piece about refereeing being refereed did not escape general notice.) I have long said that authors get too much credit for their works, and the point seems especially true in this instance.

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I have not entered into these details in order to draw attention to myself or to this as an idiosyncratic case. True, a smarter or more experienced historian would doubtless have been more clairvoyant and effective in approaching this same material. Nonetheless, I believe that every piece of historical writing shares an analogous set of accidental factors shaping its genesis. Most historian colleagues with whom I have broached these matters at once agreed that accidental factors are indeed at play. More than once, raising this point has elicited an “Oh, yeah, . . . when I was working on X-project, this and that intervened in this and that way to change the course of my research.” The best example I have encountered is a colleague who specializes in American foreign relations and who, in the bowels of the library at the University of Alabama, came across an uncatalogued six-hundred-page manuscript about a massacre of missionaries in China in 1895. Not only did he publish a study of the manuscript, but it set the course for his own work, and has in turn influenced his field in general.7

In some cases, then, accident plays an essential role in the creation of new knowledge in the discipline of history. Not every piece of historical writing need be quite as contingent as this last case or my own, however. For example, many, if not most, historians have some a priori

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commitments—such as Marxism or feminism—that drive their work. Similarly, those engaged in formal programs of research, such as Annales school cliometricians or social constructivists, know in advance in a general way what they are looking for in their investigations. Nevertheless, contingencies inevitably enter the picture here, too, and I suggest that the more personal and idiosyncratic accidents highlighted in this paper inevitably influence the course of even this sort of theory-driven research as it moves from the general to the particular. Those accidents are undeniably there and need to be identified in each individual instance. Obviously, the more interesting and noteworthy cases are those where accident plays a more essential role, but there needs to exist a continuity of accident, so to speak, and accidental factors must impact even the most apparently straightforward piece of historical research. The general historiographical importance of accident remains, even if it proves useful to distinguish degrees of adventitious influence in particular cases.

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Granting that accident, luck, and serendipity do play a role in historical research, the question becomes what differences accidental factors make. As a first step toward saying something about this question, I went looking at the literature on historiography to see what might have been written on the topic. I examined the full run of the key journal, History and Theory, since its inception in 1960, and most of another important publication, Storia della storiografia. Using various electronic databases, I surveyed the titles and abstracts of two thousand “hits” of articles tagged with the keyword “historiography,” and a thousand books similarly classified. I also consulted numerous annotated bibliographies and manuals for pursuing historical research. To my knowledge, the considerations evoked here regarding accident in historical research simply do not figure in the historiographical literature.

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8 I owe this consideration to Professor Londa Schiebinger, presently of Stanford University.

Historiographical attention seems devoted primarily to contested areas, as in, for example, conflicting literatures dealing with the Holocaust, slaves and slavery, religion (notably early Christianity), colonialism, gender, and so forth. Most explicitly historiographical studies deal with narrow subsets of the literature. “The Historiography of Crazy Horse,” “Ten Years of Post-Communist Historiography in Albania,” and “Fernand Braudel on Historiography and Its Implications for Marketing” are recent examples. There is a lot of serious and high-quality work out there, but historiographical studies do seem to lack the element sometimes labeled “reflexivity,” that is, an awareness that their historiographical perspectives apply to them also. The history of history writing is the essence of historiography, of course, and the subject figures prominently in more general accounts. Notable examples from the first half of the twentieth century include G. P. Gooch, *History and Historians in the Nineteenth Century* (1913), Herbert Butterfield, *The Whig Interpretation of History* (1931), E. H. Barnes, *A History of Historical Writing* (1937), and Marc Bloch, *The Historian’s Craft* (1953). Similar sorts of useful narratives continue to be written today. But, tellingly, nothing seems to have been written on this metatopic of what we might call the historiography of historiography. A flurry of historiographical debates has unfolded over the last three decades concerning postmodernism in the writing of history, but this literature is only an apparent exception proving the rule, in that nothing of the themes raised here regarding accident in research enters into the debates over postmodernism. Furthermore, even in these esoteric discussions, as elsewhere in the literature, the starting point for historiographical inquiry is the polished body of historical writing found in published books and articles, not the circumstances that led to their creation.

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11 For more current expositions of historiography and the history of history, see sources cited above at n. 9 and in next note.

A related set of discussions about rhetoric as a feature of history writing is blind to unanticipated inputs into the polished outputs of historians.\textsuperscript{13} The same can be said about that corner of philosophy, the philosophy of history, that concerns itself primarily with philosophical issues regarding the past, such as how we can know things about the past, and with the truth status of what historians have said about the past. Here again, as interesting as it is, the literature of the philosophy of history takes off from the finished corpus of work that presents rational reconstructions of the past, and it seems not to concern itself with the course of historical research itself.\textsuperscript{14}

Thus far, I have found nothing substantial in the historiographical literature that deals directly with the messy realities of historians \textit{in medias res}.\textsuperscript{15} The closest I have come to a historiographical piece that touches on the issues of concern in this paper is the set of erudite essays published by Carlo Ginzburg in 1999, \textit{History, Rhetoric, and Proof}.\textsuperscript{16} In addressing current historiographical debates over rhetoric and the status of truth claims in history, Ginzburg enunciates his commitments to “the working methods of historians . . . the concrete work of historians . . . and actual historiographical practice.” He likewise expresses his desire “to articulate the point of view of those who work in contact with documents.”\textsuperscript{17} His primary purpose, however, is to salvage a sophisticated notion of rhetoric from postmodern skeptics “who insist on separating historical narratives from the research on which they are based.”\textsuperscript{18} As a result, Ginzburg unfortunately does not pursue this theme

\textsuperscript{15}Christine Mason Sutherland contributes to this line of inquiry in reminding us of the importance of place for historians engaged with their subjects; that is, she recommends the methodological virtues of visiting and experiencing with all our senses locales and remnants relevant to historical actors and to the subjects of historical inquiry; Sutherland (cited n. 13), 113–14.
\textsuperscript{16}Ginzburg (cited n. 13).
\textsuperscript{17}Ibid., 1–2.
\textsuperscript{18}Ibid., 102.}
of the “working methods of historians” in any detail, and instead concludes only with the following programmatic and still largely abstract statement:

I find the current approach to historical narratives highly simplistic, since it usually focuses on the final literary product, disregarding the research (archival, philological, statistical, and so forth) that made it possible. Our attention should shift instead from the end result to the preparatory stages, in order to explore the mutual interaction between empirical data and narrative constraints within the process of research itself [original italics].

Ginzburg’s perspective opens the door to considering the nominally extraneous factors that shape historical writing, but he does not lead his readers across the threshold.

It might have been otherwise. As Robert K. Merton and Elinor Barber reveal in their recently published study, *The Travels and Adventures of Serendipity*, in the nineteenth century various British antiquarians, bibliophiles, collectors, lexicographers, and littérateurs adopted the term serendipity to describe the often accidental and inadvertent character of their finds and activities. Unfortunately, note Merton and Barber, “these amateur scholars—whose avocational interests kept them on the fringes of established academic disciplines—did not come across the word serendipity, or use it, in the course of professional scholarship,” and it seems that more reputable humanistic scholars, if historians be such, did not pick up the term. As a result, the significance and connotations of serendipity, historiographical and otherwise, did not enter the professional consciousness or methodological considerations of practicing historians or historiographical commentators.

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Why should this be the case? Why should these seemingly fundamental, if difficult to characterize, factors of accident, luck, and serendipity be so ignored by the very practitioners most affected by them? Part of an answer may go back to the old question whether history is an art or a science. If history is an art, then its products are the works of artists, and the generally received, romantic notion of the artist as creative genius leaves little space for the play of chance in the productions of genius. From this point of view what matters is how inputs, fortuitous or not, are refracted through the prism of the creative mind, not how or from what sources the artist composes his working palette. Conversely, if history is a science, it obviously lacks the authority of

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19 Ibid., 101.
20 See Merton and Barber (cited n. 6), 48–60 and 123–40.
the natural sciences even in its Marxist or cliometric guises; and historians hardly promote the cause of history—even as social science—by admitting that the vagaries of chance and accident are essential elements of its character and practice. Culturally, we prefer explanations that show us in charge as historical actors and as chroniclers, and it is upsetting and destructive to the historical enterprise to think otherwise. In the final analysis our narratives cannot seem to be the result of accident.21

An old distinction in the philosophy of science may help to further explain the paradox of the important, yet invisible, accidental factors affecting the craft of history. The distinction is between the “context of discovery” and the “context of justification” in the sciences. As the philosopher of science Hans Reichenbach expressed the notion in 1951,

The scientist who discovers a theory is usually guided to his discovery by guesses; he cannot name a method by means of which he found the theory and can only say that it appeared plausible to him, that he had the right hunch, or that he saw intuitively which assumption would fit the facts. Some philosophers have misunderstood this psychological description of discovery as proving that there exists no logical relation leading from the facts to the theory. . . . These philosophers do not see that the same scientist who discovered his theory through guessing presents it to others only after he sees that his guess is justified by the facts. It is this claim of justification in which the scientist performs an inductive inference. . . .

The mystical interpretation of the hypothetico-deductive method as an irrational guessing springs from a confusion of context of discovery and context of justification. The act of discovery escapes logical analysis; there are no logical rules in terms of which a “discovery machine” could be constructed that would take over the creative function of the genius. But it is not the logician’s task to account for scientific discoveries. . . . logic is concerned only with the context of justification.22

The route a scientist takes in making a discovery is thus different, often quite different, from the presentation he or she makes to persuade others of the correctness of a claim. Friedrich August Kekulé’s 1865 discovery of the hexagonal structure of benzene is the paradigmatic

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21 I owe this point to my colleague in the English department at Stevens, Professor Maria Plochocki-Williams.

example, with Kekulé coming up with the ring structure of benzene by hallucinating a snake eating its own tail.\textsuperscript{23} Needless to say, the published paper that announced his discovery gives no indication of this route to discovery.\textsuperscript{24} The context of justification, in other words, always involves rational reconstructions and necessarily obscures the coming-into-being of a scientific discovery. Moreover, the recognition of the difference between the context of discovery and the context of justification in science has been the starting point for inquiry in the history of science and science studies since the 1960s.\textsuperscript{25} That is not to say that the claims of science are necessarily false or that the context of discovery represents the whole story, but indisputably our understanding of science and its history has been significantly enriched by acknowledging these “extraneous” factors.

It is astonishing to me that this “discovery-justification” commonplace has not been assimilated into thinking about the work of historians. History, the study of the past, is not as socially significant or as useful as the enterprise of science, the study of nature, especially in the latter’s applied guise of science as technology. For that reason, one suspects, history simply has not attracted critical attention similar to that given to science. But looking at the “context of discovery” rather than at ex post facto rational reconstructions reveals the shaping influence of accident, luck, and serendipity in the writing of history. To recur to my own monograph, except that I do incorporate a bit of this material into it, no one would know how contingent the final piece actually was. Rather, it reads as if its author possessed a rational mind and was, in a straightforward and logical way, making a contribution to an equally coherent body of literature, something like the account that I presented at the outset of this paper. Recall the sentence I used to describe my findings: “I was fortunate to identify that historical turning point when scientists—the producers of knowledge—first gained control over the publication of the results of their research.” This first sentence explaining my work is actually the last sentence I came up with in trying to figure out my subject. The sentence sounds pretty good,

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but the confused reality behind it is a far cry from the (I hope) clear picture presented in the text. Inevitably, then—and this is the main point—once a rational narrative falls into place, the accidental factors that were essential to its creation disappear from the account. The often haphazard scaffolding required to build a work is dismantled once the edifice is complete. And thus, because the discipline of history strives to provide rational accounts and explanations of change over time and because historiography is the rational study of these rational accounts, we see why accidental factors become effaced and have not received their due attention in the history of historiography.

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What might we say, then, about the enterprise of historical research if we bring accidental factors into focus? On the one hand, we might dismiss the role of accidental factors as trivial or self-evident. The philosopher of science Sir Karl Popper rejected any consideration of the “context of discovery” in science because he thought that there was simply nothing interesting to be said about it. As he put it at some length and with no little passion:

The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question of how it happens that a new idea occurs to a man [sic]—whether it is a musical theme, a dramatic conflict, or a scientific theory—may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge. . . . Accordingly I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. . . . Some might object that it would be more to the purpose to regard it as the business of epistemology to produce what has been called a “rational reconstruction” of the steps that have led the scientists to a discovery—to the finding of some new truth. But the question is what, precisely, do we want to reconstruct? If it is the processes involved in the stimulation and release of an inspiration which are to be reconstructed, then I should refuse to take it as the task of the logic of knowledge. Such processes are the concern of empirical psychology, but hardly of logic. . . . My view of the matter, for what it is worth, is that there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process.26

26 Karl R. Popper, The Logic of Scientific Discovery (New York: Harper Torchbooks, 1968), 31–32; original emphases. Popper first published this work in German in 1934; it was translated into English in 1959. See also Helge Kragh, An Introduction to the Historiography of Science (Cambridge: Cambridge University Press, 1987), 56. It should be pointed out that in recent years philosophers of science have returned to investigating the logic of discovery; see Baruch A. Brody and Richard E. Grandy, Readings in the Philosophy of Science, 2nd ed. (Englewood Cliffs, N.J.: Prentice Hall, 1989), 398ff.
Following Popper, perhaps we should say the same about the role of accident in historical research. We might admit that, yes, accident plays a part, but intrinsically—just because it is accident—there is nothing of analytical significance to be had in pursuing a study of accident in our works.

There is a lot of truth in that position, but it is not the whole truth, and the ways in which it falls short highlight the historiographical importance of accident. Long-standing and fundamental principles of historiography say that no source can be accepted at face value, that every published work has to be placed in its own historiographical context: who the author was, when he or she published, and under what circumstances and with what individual prejudices and points of view, etc. (Nota bene: These considerations apply to historiographers as well as historians face-à-face with their sources.) And so, without recognizing and acknowledging the additional factors I have raised here, our sense of the full historiographical context in which any and all works are to be placed and evaluated is necessarily limited, incomplete, and therefore flawed. We may be able to intuit some of the unstated reasons a published piece sees the light of day—someone needs tenure, someone is a committed Marxist—but, unless we recognize the role of accident, we will not and cannot know the full story behind a work, and, moreover, on that account we are unlikely to admit the contingent character of works—that they might begin on a particular afternoon with a question, or that they might not have been written at all. It would be preferable and a simple matter, it seems to me, if an author provided an account of how a work came to be along with the work itself. That is not asking a lot. A few words in a preface would suffice. Adding such perspectives would enrich and, indeed, complete historiographical understandings and historiographical accounts that have almost universally been lacking to date. We are in a better position to evaluate a work if we know better how it came to be.

What has just been said about accident affecting individual pieces of historical research can be usefully projected onto historiography as a whole. Plainly, the sum of the idiosyncratic forces behind individual works adds up to influences shaping the whole body of the literature. And then, accidents can affect historiography directly. Consider, for example, the untimely death in September 1980 of the thirty-three-year-old historian of Darwinism and evolution Dov Ospovat. Stephen Jay Gould labeled Ospovat’s posthumously published book, The Development of Darwin’s Theory, “a landmark in Darwinian studies.”

\[\text{27 The Development of Darwin’s Theory: Natural History, Natural Theology, and Natural Selection, 1838–1859 (Cambridge: Cambridge University Press, 1981), blurb.}\]
his *éloge* of Ospovat, Frederic L. Holmes said that he, Ospovat, was “one of the most promising future leaders of the field. . . . We had every reason to expect that he was on his way toward an outstandingly productive and significant scholarly career.”

It is not “what if” history to acknowledge that the contingency of the death of this talented young historian of science at the outset of his career affected the Darwin industry and, by what he did not accomplish, changed the course of research over the last quarter century in the history of evolutionary theory. Therefore—and this is the point here—we need a new paradigm for historiography as a whole, a new frame of reference that sets historiography in an enlarged context incorporating contingent factors that affect the course of its development. And, if we do that, we must admit that historiography is not—and should not be presented as—entirely the rational reconstruction of the history of history. We need to recognize that its sources and historiography itself are also buffeted by the winds of chance. Greek Clio consorting with Roman Fortuna. In other words, even if we perhaps cannot do that much analytically with the notion of accident, acknowledging that accidental factors play a role in historiography is essential, nevertheless, and must transform our views of the literature of history, the nature of historical research, and historiographical reportage. In this way accident, luck, and serendipity take on a genuine theoretical importance for historiography and the discipline of history.

Beyond an intuitive sense of the importance of accident for the historical literature as a whole, what might we say in particular about the context of discovery in historical work and the play of accidental factors for historical research? One can certainly draw a practical, pedagogical lesson. Clearly, graduate students would be well served by at least some acknowledgment that accident plays a real role in historical research. They would be better prepared if their sensitivities were trained to be attentive to the possibilities of the unexpected in historical research. They would be better prepared if their sensitivities were trained to be attentive to the possibilities of the unexpected in historical research. And, if we do that, we must admit that historiography is not—and should not be presented as—entirely the rational reconstruction of the history of history. We need to recognize that its sources and historiography itself are also buffeted by the winds of chance. Greek Clio consorting with Roman Fortuna. In other words, even if we perhaps cannot do that much analytically with the notion of accident, acknowledging that accidental factors play a role in historiography is essential, nevertheless, and must transform our views of the literature of history, the nature of historical research, and historiographical reportage. In this way accident, luck, and serendipity take on a genuine theoretical importance for historiography and the discipline of history.

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Historians should be more explicit about the large contingencies affecting their work. One obvious category of contingency concerns the archival or other scholarly resources available to the historian. What

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29 Mr. Ron Lorton of Stevens Institute of Technology suggested this point to me.
we can write about in large measure depends on the material available to us, and obviously we do not have access to all possible resources. The destruction of the library at Alexandria, selective access to the Vatican archives, and the opening of archives in the former Soviet Union have impacted what historians can and cannot say and do. Acknowledging what we do not possess better frames what we do.30

Another category of accidents worthy of further attention is discoveries or findings not made! That is, in addition to discovering how accident, luck, and serendipity can lead to new knowledge, we need to recognize that these contingencies may equally well prevent discoveries.31 The question not asked, the lead not pursued, the rejection that disheartens and consigns unpublished work to the dead file—these nullifying twists and turns are no less real and noteworthy than the fortuitous ones that prove fruitful. Here, of course, we are lured into the whirlpool of “what if” history, but we need not succumb to the enticements of that siren in order to recognize things-not-discovered as the complement to things discovered.

Then, harking back to the epigraph by Pasteur that heads this piece, we need to underscore that, chance and the cliché aside, the prepared mind of the historian must play a role, that there is a necessary and inevitable interaction between the historian and what accident, luck, and serendipity throw into the path of the historian. Yet Pasteur’s notion of the prepared mind needs to be unpacked. There are various levels of preparedness, and we may seem more prepared after the fact than we actually were as we processed some accidental finding or as some new idea or understanding initially dawned on us. When Roche asked me his question, my mind was prepared only to recognize it as an apt question for the discussion at hand, and I was incapable of seeing anything else it might signify or where it might lead. When I stumbled across the registers of the Comité de Librairie, I was prepared only to recognize them as a potentially good source; I was not prepared to say of what. And when I finally recognized the story of specialist control over the scientific press I was prepared only to acknowledge something that had already occurred, the coming into being of a good story that I somehow managed to concoct and present to myself. Crucially,

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31 I owe this insight to my colleague Professor Harold Dorn; see related remarks by Merton and Barber (cited n. 6), 215.
The story and the acknowledgment that I was somehow “prepared” to recognize occurred simultaneously. Only in retrospect does it seem that I was logically and conceptually prepared to uncover all that one finds in *Specialist Control*, whereas in reality the preparedness unfolded over time and was visceral, non-cognitive, and instinctive throughout. Is that what Pasteur meant?

The world of science again provides a path for elucidating this question. In the passages by Reichenbach and Popper quoted above it is noteworthy that, in rejecting the “context of discovery” as a source for understanding science, both philosophers suggest that psychology is the proper discipline for examining scientific creativity and discovery. Perhaps we should look to psychology as a source of insight into how to think about the mind of the historian and the play of accident, luck, and serendipity in historical research. That inquiry, at least as undertaken tentatively for present purposes, proves not entirely unrewarding. The notable gestalt psychologist Wolfgang Köhler examines what happens in a given situation when someone conceives something new. Notably, Köhler makes it clear that the process of discovery and invention is just that, a process that unfolds over time. He similarly emphasizes overcoming habitual ways of thinking, tapping facts not immediately present in the given situation, and discovering new relations among things. Along similar gestalt lines, Howard E. Gruber has made a special study of scientific creativity in the case of Charles Darwin; Gruber emphasizes the point of view of the scientific protagonist and the affective side of creative work and research as additional elements of a theory of creativity. Frank J. Sulloway has likewise made significant inroads into thinking about the psychology and character of scientists in his monumental study of birth order. These sources are enlightening, with the proviso that we keep in mind that they focus on the exceptional in science, on the great theoretical or technical breakthrough in science, and not on more humdrum features of scientific research, where accident, luck, and serendipity are also to be found.

These considerations do provide insight into the mind of a scientist as he or she struggles to make sense of the unexpected and to forge an

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account that others will accept. Our understanding of the role of accident, luck, and serendipity in historical research can obviously be enhanced by incorporating these considerations into thinking about the interaction of the historian and the unexpected. Perhaps we can distinguish authors of truly pathbreaking historical writing from authors whose production is more journeyman in character. Using those distinctions in science, we may be better able to say something about the quality of mind of historians and their capacities to absorb and fruitfully deal with the unexpected in research. Psychology may perhaps tell us that first-born historians are less open to “thinking outside the box,” whereas their younger brothers or sisters are more likely to latch onto that unexpected bit of evidence and turn it into historical novelty. However all that may be, whatever psychology may have to tell us in these regards has not been acknowledged, much less incorporated into historiographical considerations. For better or worse, historiographical psychology is a null set in the historiography of historiography, itself more or less a null set, as we have seen.

The literature of the sociology of science also contributes to thinking about individuals grappling with the unexpected in their research. In particular, as Merton and Barber point out, the topic of serendipity and “accidental” discoveries in science has been actively debated since the 1930s, when physicians and scientists took over the term from antiquarians and began to argue about the role of serendipity and accident in scientific discovery. Some thinkers, notably Walter B. Cannon, professor at the Harvard Medical School, thought accident plays a significant role in scientific discovery, while others defended the high ground of science essentially as rational inquiry. The understandings developed in sociological discussions about science thus would also seem to apply to thinking about the role of serendipity in the scholarly enterprise of history.


Note that the scientific and social science research communities themselves have sought to systematize thinking about serendipity in order to maximize fruitful outcomes in the research process; see, for example, L. Steinman, “Gene Microarrays and Experimental Demyelinating Disease: A Tool to Enhance Serendipity,” *Brain* 10 (2001): 1897–99. Note also similar efforts to tap the concept of serendipity to achieve productivity gains in the business/management world; see, for example, Bruce D. Ario, “Creating Creativity,” *Supervision* 63 (2002): 16–18; David Mechanic, “Lessons from the Unexpected: The Importance of Data Infrastructure, Conceptual Models, and Serendipity in Health Services Research,” *Human Resources Abstracts* 2 (2002): 165–319.
Commentators point to a number of factors said to come into play as a scientist grapples with unanticipated results: the existence of theory, systematic experimentation, quantitative approaches, careful record keeping, unexpected results as defined by expected results, deep grounding in the field, and the strong, community-based nature of the scientific enterprise, not to mention the ability, talent, and personal characteristics of trained specialists. Yet, to whatever extent these factors mitigate the role of sheer accident in scientific discovery and speak to the prepared mind of the scientist, they may be less applicable in helping us understand what is going on as the historian faces his or her sources and the vicissitudes of historical research. Science is about nature, and history is about the past, but the past does not push back in quite the same way or with quite the same force as does the brute world outside our theories or direct perceptions. Merton and Barber go on to list some attributes of historians per se that may prepare them to make the most of accident in their research; these include “persistence, detective skill, fervor, [and] hard work.” Yet Merton and Barber conclude the short section of their book they call “Note on Serendipity in the Humanities” on a pessimistic note: “From the point of view of the literary scholar, unlike that of the scientist, there is, even in principle, no explicit method of distinguishing between strategic data and others: Since there is no coherent body of theory to which new data may be related, only a stock of loosely organized knowledge, the literary scholar’s mind is less well prepared to classify clues as ‘promising’ or otherwise.”

Some earlier antiquarians cited by Merton and Barber appealed to divine or extrasensory powers to explain serendipity in their researches. I doubt that it is useful to evoke such powers in our analysis. But merely to introduce the unexplainable is a form of explanation lacking to date in methodological and historiographical accounts of how historians come to produce their works. Accepting this perspective, historical studies turn out to lack the inevitability and the apparent solidity we tend to impute to them. Accident, luck, and serendipity become something like the “dark matter” of historiography surrounding the luminous patches we recognize. The historiographical trail becomes more tenuous, delicate, and uncertain than we thought. Indeed, the body

37 This signal point was suggested to me by my colleague in science at Stevens Institute of Technology, Professor Bernard Gallois.
38 Merton and Barber, 225–26.
39 Ibid., 227. Note that these words, first published in 2004, were written in 1958.
40 Ibid., 55–58.
of existing historical works by implication defines a complementary realm of possibilities and alternative trajectories for the course of historiography, a world of might-have-been separated from the world that is only by turns of the wheel of fortune.41

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The topic of accident, luck, and serendipity is not exactly the elephant in the room whose presence we need to recognize; perhaps it is more like the staff in the kitchen whose labor we do not ordinarily think about, but who make real contributions to the meals we digest at the high table of intellectual inquiry and discourse.

As a postscript, I cannot conclude without pointing out the obvious irony underlying this presentation. That is, I stumbled onto this

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41 See figure 1. Undated, but third-quarter eighteenth-century German jeton in copper by Johann Wilhelm Schlemm, mintmaster at Clausthal in the Duchy of Hanover (1758–76). Recto: Fortune with her sail facing left atop wheel of fortune with the legend, “fortuna variabilis.” Verso (not shown): Goddess with cornucopia and scales and legend “omnia cum pondere numero & mensura”; signed “I.W.S.” On this jeton see Michael Mitchner, Jetons, Medalets & Tokens: Volume 1: The Medieval Period and Nurenberg (London: Seaby, Ltd., 1988), 604. This theme of the fickle fortune of fate is found in earlier French jetons of the period of Charles IX (1560–74); see Mitchner, 1: 454–55, 519; and Félix Feuardent, Jeton et méreaux depuis Louis IX jusqu’à la fin du Consulat de Bonaparte (1915: reprint, Paris: Maison Platt, 1995), 3: #11711-15 and #11725. The notion of the winds of Fortune was a commonplace from antiquity; see iconography and commentary by Lesley Thomson, “Fortune: ‘All is But Fortune,’” on the Folger Shakespeare Library Web site: http://www.folger.edu/public/exhibit/Fortune/fortune.htm. I thank my brother, Dr. Murray McClellan, for recalling this point to mind and for suppling the URL.
topic by accident as I thought about how my monograph on specialist control came into being. I was fortunate—lucky—that it won the Lewis Award and that I was invited to make a presentation to the American Philosophical Society and to develop the topic a bit.42 And, by serendipity I have apparently come across something of note for the discipline of history that merits critical attention and further development. What a cycle.

42 My wife, Jackie, reminds me that initially I was not going to write out the talk I gave on this subject to the APS. Only when I saw the program and realized the great seriousness of an APS General Meeting, did I discover the need to do more than proceed with the informal oral presentation I had sketched out in my head in the shower. This written version owes its origin to that constellation of affairs.