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The Practitioner of Science: Everyone her Own Historian

MARY P. WINSOR
I.H.P.S.T.
Victoria College
73 Queen's Park Crescent East
Toronto, Ontario M5S 1K7
Canada
E-mail: mwinsor@chass.utoronto.ca

Abstract. Carl Becker's classic 1931 address "Everyman his own historian" holds lessons for historians of science today. Like the professional historians he spoke to, we are content to display the Ivory-Tower Syndrome, writing scholarly treatises only for one another, disdaining both the general reader and our natural readership, scientists. Following his rhetoric, I argue that scientists are well aware of their own historicity, and would be interested in lively and balanced histories of science. It is ironic that the very professionalism that ought to equip us to write such histories has imposed on us a powerful taboo that renders us unable to do so.

We who count ourselves sophisticated in describing the effects of social forces upon past scientists have been remarkably unconscious of the ways our own practices are being shaped by our need (and perhaps more importantly, the needs of our teachers' teachers) to distinguish ourselves from scientists who write history. Our fear of presentism in general and Whig history in particular is really a taboo, that is, an excessive avoidance enforced by social pressure. It succeeds at making our work distinct from histories written by scientists, but at the awful cost of blotting out the great fact of scientific progress. Scientists may be misguided in expecting us to celebrate great men, but they are right to demand from historians an analysis of the process of testing and improvement that is central to science. If progress in general is a problematic term, we could label the process "emendation."

Keywords: Carl L. Becker, emendation, historiography, presentism, progress, Stillman Drake, Whig history

Stillman Drake was an amateur. The history of science was his hobby. His only earned degree was a B. A. in philosophy. After he fell in love with the writings of Galileo, he decided to publish a new English translation, so he taught himself Italian during his daily commute across San Francisco Bay on the ferryboat, on his way to his job in the investment business.[1] His amateur status ended only after his reputation as a scholar was well established, when

[1] Stillman Drake's widow, Florence Drake, confirms my recollection of the ferry anecdote, adding however that he was not entirely self-taught in Italian, but employed a tutor. I am grateful to her for conversations and encouragement.

his old friend John Abrams offered him the post of full professor at the new Institute in Toronto.[2]

That was in 1967. Two years later John Abrams hired me, on the strength of my undergraduate specialty in history of science at Harvard and my unfinished doctoral dissertation in the history of science from Yale.[3] Thus my own status as a professional was already secure, notwithstanding that I was an utter neophyte. The discipline in which I was trained was very new; shortly before I began my studies, the people who did history of science were people whose PhDs were in philosophy, or history, or science, united only by their love of the topic, welcoming into their fellowship anyone who shared that love, including non-academics like Drake. Yet how few of us professionals, for all our training and numbers, will leave as great a mark as he did on the world of understanding. Drake's *Discoveries and Opinions of Galileo* has never been out of print, his views still need to be reckoned with, and his careful scholarship will never be out of style.

Possibly some of us may feel a twinge of envy when reminded of Drake's stature. Perhaps we solace ourselves, in private and unworthy thoughts, by remarking that his work was marred, to some extent, by the same failing we notice in the work of scientists when they indulge in amateur history: various forms of presentism, including a naive willingness to bring to the past whatever modern science claims to know about the laws of nature. Drake never hesitated to applaud when Galileo exposed the errors of his enemies and discovered a scientific truth. He was frankly motivated by an admiration of empiricism and a distrust of metaphysics, in whatever century these may be found. When Victorian hero-worshipping biographers of Galileo were accused of Whiggishness, Drake defended them.[4] That reaction exposed his amateur roots, for we young professionals would go to any length to distance ourselves from the historiographic sin ridiculed in Butterfield's little book of 1931, *The Whig Interpretation of History*.

The distinction between professional and amateur is, I believe, an important one, indeed essential for understanding the history of science. When I teach the history of biology, I try to explain the professionalization of science. I show my students the difference between the isolated genius of Hooke and Leeuwenhoek, who founded no research tradition because it was not their job to train students, and the microscopists of the 19th

[2] Levere, 1999, pp. xi–xiii.

[3] I attended Radcliffe College at Harvard University from 1961 to 1965; my field of concentration was "History and Science" which included tutorials in history of science and a senior thesis. My doctoral work was done at the Department of the History of Science and Medicine of Yale University from 1965 to 1969.

[4] Drake, 1978, p. xxiii.

century, who multiplied like rabbits.[5] I like Coleman's slant in discussing Claude Bernard, that the discipline of physiology consisted of more than the topic, more than the journals and educational structures, it included the methodological principles upon which adherents agreed.[6] The categories "amateur" and "professional" give only a rough outline of the issue, because the very definition of layperson and expert change over time field by field, and science has never met the strict criteria of the model professions like medicine and law.[7] What is nonetheless clear is that we should distinguish between an intellectual topic, that is, some potentially coherent body of subject matter, and a discipline, which although based upon study of a topic, has a social structure. Eli Gerson explains that the sociologists' concept of a "social world" applies to scientific disciplines, whose members are linked together in networks of communication, rivalries, common goals, and agreed-upon norms as to what methods and explanations are legitimate.[8] Amateur activities like birdwatching or playing volleyball can also form more or less distinct social worlds, but professional disciplines always function this way.

A good sociologist would have no trouble describing the forces that were abroad in the 1960s when the history of science, which as a topic of study was quite old, became a profession, in other words, when a social world of full-time historians of science came into existence. The first college courses devoted to the history of science, or the publication of the first volume of *Isis* in 1913, or even the founding of the History of Science Society in 1924, did not constitute the establishment of our discipline, in spite of George Sarton's fervent wishes; those events belong to its "pre-history," when there were only "proto-historians of science," because until a decade or so after World War II, there were no jobs.[9] This good sociologist would doubtless also explain that during the creation of a new discipline, boundary-work must be done, that is, successful institutional establishment depends on the members claiming superiority for their own skill and approach over whoever may already be cultivating the same subject. We sophisticated historians are quite

[5] My first teacher in history of science, Everett Mendelsohn, introduced me to this and other issues in the social structure of science as long ago as 1961; one of our first assigned readings was Boris Hessen's "The social and economic roots of Newton's *Principia*" (1931). At Yale I took Derek J. de Solla Price's seminar on the social context of science. I mention this because nowadays it may be imagined that the history of science used to be internalist, the social dimensions of scientific knowledge being given their due only recently. Those two categories did tend to be treated, however, as separate realms.

[6] Coleman, 1985, pp. 49–70.

[7] Winsor, 1991, pp. 164–197.

[8] Gerson, 1983, pp. 356–377.

[9] Thackray, 1972, 1975, 1980a, 1980b.

capable of recognizing and describing that kind of scenario when biochemistry first distinguished itself from organic chemistry, or molecular biology from biochemistry. Even though embryology was a logical part of the age-old question of how inheritance works (for the way genes work is by regulating development), Bateson and Morgan's formula for a powerful new discipline included limiting their focus to transmission genetics, where they had techniques that could get results. Erecting intellectual boundaries allowed these young disciplines to concentrate their energy by ruling inside their walls. In our own history, exactly because the subject matter of history of science had already attracted a cluster of fans, the first generation of professionals felt the need to differentiate their approach from that of their intellectual forebears. The norms they developed were coherent and convincing, and have lain secure in the fabric of the mainstream of the discipline ever since. Chief among these were the rejection of presentism and, a subset of that, rejection of "Whig history".[10]

Agreed-upon norms are the coalbox and fire that drive the engine of a discipline's accomplishments, productively guiding the actions of members of a community to produce outcomes their fellows can recognize and build upon.[11] For historians of science, however, there are two negative consequences of our discipline's norms, and after I point them out, I shall go on to suggest that everyone could benefit if our norms were modified. I do not think this means that setting them up was a mistake when our discipline was new. Rien – je ne regret rien.

The norms of a discipline – the set of beliefs about the method, essential knowledge, and boundaries proper to it – may be credited with much of whatever the discipline does well. However, to make an omelet ya gotta break some eggs, and there are some negative aspects to the story. Adherence to norms means that some ideas are ruled out of order unexamined, and the people who hold them are branded unworthy of attention.

Human nature being what it is, norms sink into the subconscious, becoming practically invisible to the people who hold them, and in the process growing more powerful. A vivid example of this phenomenon was explored by an an undergraduate student of mine, Ms. Anjum Choudhry, who wrote a paper for me surveying the career of zoologist Donald Griffin. She described how he happened upon the riddle of bat navigation early in his career, and then spent decades of experimentation showing that they fly by sonar. Though Griffin was not trained in psychology himself, he absorbed

[10] Hall, 1983.

[11] I think my use of "norm" for shared commitment to particular methods and modes of explanation is close to what is meant by "paradigm" as Kuhn originally used it (1962) but I avoid his word because people still tend to equate "paradigm" with theory.

and adopted the norms of specialists in animal behaviour, norms that forbade all anthropomorphism. What jolted Griffin out of what he now regards as a terribly limited range of inquiry was a short article by the philosopher Thomas Nagel entitled "What is it like to be a bat?" Although the article answered, in effect, that we can never know, it was a radical move even to pose the question, a move probably impossible for a professional scientist. Griffin has now written several books on animal consciousness. This issue is still contentious, and I have no idea whether Griffin's views will remain on the fringe or will win over animal psychologists. What interests me here is Griffin's argument that the experts had put blinkers on their minds, refusing even to notice the evidence for self-awareness. He chooses the word "taboo" in his description of the situation. The field of animal behavior early in the Twentieth Century doubtless had a great need to erect this particular taboo, to distinguish proper scientists from other writers on the same topic like Henri Fabre or Ernest Thompson Seton. Note that the word "taboo" connotes an irrational belief forbidding people to touch certain objects, or in this case entertain certain thoughts, and it suggests a belief sustained by social enforcement. One of Griffin's supporters described how "if one raised the subject of consciousness in cognitive discussions, it was generally regarded as a form of bad taste, and graduate students, who are always attuned to the social mores of their disciplines, would roll their eyes at the ceiling and assume expressions of mild disgust."[12]

So one of the drawbacks of the cognitive structure of disciplines is that specialists can be prevented by prejudice from considering the full range of their natural subject matter. I call this the Taboo Problem. Another of the costs of professionalism is its members' intellectual isolation.[13] The pressure of duty to one's colleagues and students turns experts away from the considering anyone's needs but their own; even to communicate to other professionals takes an effort. Let us call this the Ivory-Tower Syndrome. We should perhaps distinguish two aspects of the syndrome: isolation from members of other specialties and isolation from the general public. Scientists notoriously exemplify the Ivory-Tower Syndrome; they can rarely spare the time to keep up with developments outside their own narrow field, much less to read more widely, and much of what they write can only be understood by their fellow experts. Whatever may be its main cause, the Ivory-Tower Syndrome is worsened by that feature of human nature that makes members of a group scorn deviant individuals. Stephen Jay Gould is greatly loved by

[12] Searle, 1990, p. 585, cited in Choudhry, 1997.

[13] A few historians of science have noticed and regretted the link between our professionalism and our tendency to write only for our peers, particularly Russell, 1984.

many readers, but it is notorious that his popular essays actually undermine his status among his peers in paleontology.

We who are watchers of scientists are familiar with their Ivory-Tower Syndrome, but I think we rarely reflect upon the fact that the same kind of forces press in upon us from our own discipline. The clearest evidence of this is that we keep writing things no one wants to read. Most of us are lucky if our books sell in the hundreds instead of by dozens, yet the fault cannot lie with our subject matter. Journalists like Nicholas Wade or Dava Sobel can use our material to hit the best-seller list, and so indeed can we, as Desmond and Moore's *Darwin* book proved, if only we care to try.[14] A loss of audience is easy to explain if a specialty adopts an arcane jargon, as some of our sister fields seem to be doing lately, but writing style cannot be blamed for the limited readership of most history of science.

Much the same can be said of an older discipline, history. My colleague Michael Bliss, who teaches at the University of Toronto and is also active as a radio and newspaper commentator, lectured his colleagues a few years ago for not producing readable histories of Canada. During political discussions about the question of the separation of the province of Quebec, there was, he said, "a remarkable demand on the part of Canadians, amounting to a kind of hunger, for help in understanding where we came from, who we are, and where we might be going. In my view we have a duty as scholars, university, teachers, and citizens to do all that we can to meet that demand."[15]

Bliss's appeal belongs to a fine old tradition. In 1931 Carl L. Becker, as president of the American Historical Association, delivered an address called "Everyman his Own Historian," warning his colleagues that writing history that no one will read is a vain and pointless business. Becker said,

> Berate him as we will for not reading our books, Mr. Everyman is stronger than we are, and sooner or later we must adapt our knowledge to his necessities. Otherwise he will leave us to our own devices, leave us it may be to cultivate a species of dry professional arrogance growing out of the thin soil of antiquarian research. Such research, valuable not in itself but for some ulterior purpose, will be of little import except in so far as it is transmuted into common knowledge. The history that lies inert in unread books does no work in the world. The history that does work in the world, the history that influences the course of history, is living history ....[16]

If Becker had had the benefit of my little review of the dynamics of disciplinary identities, he might have predicted what kind of reception his audience

[14] Broad and Wade, 1982; Sobel, 1995; Desmond and Moore, 1992; Moore, 1996.

[15] Bliss, 1991, p. 15.

[16] (Becker, 1935), pp. 252–253.

would give his message. After all, at the very core of any professional's training, what justifies the late nights of study and long years of apprenticeship, is the belief that the expertise one is mastering is vastly superior to that of the rank amateur. Maybe indeed the Ivory-Tower Syndrome results as much from such professional pride as from pressures of time. Becker's fellow historians did close their ears to his plea.[17] In rejecting his message, they elaborated instead a renewed commitment to the quite fantastic idea that their proper mission was to study the past for its own sake. Becker "had taken it for granted that 'if we are interested in, let us say, the fact of the Magna Carta, we are interested in it for our own sake and not for its sake.' "[18] But his colleagues immediately denied exactly this. They were in the grip, as many historians still are, of an extreme horror of presentism.[19] After all, the founder of modern historical scholarship, Leopold von Ranke, had declared that unlike those who thought that "history ought to judge the past and to instruct the contemporary world as to the future" his goal was to "merely tell how it really was [wie es eigentlich gewesen]."[20]

I call the notion of history for its own sake fantastic because writing history is an action that can only be undertaken by a living person, so I would think that any competent epistemologist could prove that some degree of presentism would be impossible to avoid.[21] I also imagine that any competent ethicist should be able to prove that we should not want to avoid it even if we could, but I think it is important to recognize that this is quite a separate issue. The source of historians' confusion is that our ideal of objectivity enjoins us not to distort the past (the ideal of objectivity of course applies with equal force whether or not the writer hopes to judge the past and shine light toward the future). We think we minimize distortion by behaving as though our goal were knowledge of the past for its own sake, and once we adopt the pretense, we then forget that it is only a methodological fiction.

Ranke's dictum has taken such deep root in both history and the history of science that, even though philosophers of history have thoroughly exploded it, it is still rarely challenged.[22] Let me illustrate my objection to it with a parable. What would we think of a paleontologist who said she studies fossils for the sake of the dinosaurs? What she means if she says that, is that she

[17] I may be here indulging in hyperbole. Many of Becker's colleagues congratulated him, according to Hingham 1965, p. 123, and Novick, 1988.

[18] Novick, 1988, p. 272.

[19] Graham (1981, p. 4) gives a nice description of indoctrination into anti-presentist historiography.

[20] This most famous quotation, included in Bartlett's *Familiar Quotations*, dates from 1821, the sentiment being repeated in letters of 1831 (see Krieger, 1977).

[21] Carr, 1961; Danto, 1985.

[22] Hull, 1979; Pickstone, 1995; Hardcastle, 1991.

studies them simply because she finds them fascinating; in other words, she finds her own private world enriched by visiting these beasts in her imagination. But if she does say to herself that she does it for their sake, this is because she wants to be open to whatever surprises may lie in store for her in the rocks, so she pretends that the dinosaurs themselves would complain if she gets them wrong. I do concede that she may not study the fossils for our sake, though her salary indirectly depends on other people sharing her enjoyment; we who support her do so for our sake. Yet I insist that none of us, whether producers or consumers of paleontology, are really in it for the sake of the dinosaurs; surely they, frolicking about in the fields of eternity, want nothing from us.[23]

Many people remember only the second half of the statement I quoted from Ranke, and agree that we should tell it like it was. In quoting him I was reminding you of its context, namely Ranke's distancing himself from earlier historians who rushed to judge the past and draw morals from it. Scholars may argue whether Ranke actually abandoned that role or merely played it surreptitiously, hiding his judgments in his interpretations. Nevertheless, the choice of following Ranke in fleeing presentism or heeding Becker and Bliss in caring about living history is still a moral choice open to each historian. I apologize if I am belaboring the obvious, but until recently I did not see how separable the two issues are. There is no logic linking the two parts, no logic that says you have to study a subject for its own sake or else you will get it wrong. We can admit or even embrace the fact that we study dinosaurs for our sake and still apply our scientific skills to recreating them as they actually were.

Exactly what makes the dinosaur so fascinating, even though our knowledge is terribly fragmentary and will always be open to correction, is that we are convinced that tyrannosaurs and velociraptors (considerably smaller than Spielberg's rendering) really did once roam the Earth. People also enjoy thinking about centaurs and dragons, but at some crucial point in childhood we learn the difference between fantasy and reality. Even partial knowledge of an animal that really existed satisfies us in a manner a thousand mythical creatures cannot match. If you agree with me that we study history for our sake, this does not open the floodgates of relativism. Yet that conclusion is often drawn. Becker's audience misunderstood him to mean what he certainly did not mean, that false history is just as good as true history. On the contrary, recognizing the value of living history actually strengthens the second part of Ranke. If we hope our knowledge of history will light our way to the future, then we certainly want the least distorted picture of what really happened

[23] Gary McIntyre pointed out to me that the concept of art for art's sake, and truth for its own sake, goes back to Aristotle's Nicomachean Ethics.

we can manage. Whether assessing one's place in the world, or deciding upon action, sane adults prefer reality over fiction. I have never had the least doubt that we are interested in Darwin, Agassiz, and a thousand lesser dead scientists not for their sakes, but for our own edification. I do not believe there is any thoughtful person, which certainly includes scientists, who find mythical Darwins and Agassizs more edifying than the ones uncovered by careful scholarship.[24]

Have I digressed? I was examining the devotion to history for its own sake of Becker's colleagues, suspecting that it was one of the reasons they, and Bliss's colleagues too, cultivate the thin soil of antiquarian research and write history that lies inert in unread books. I hope to convince you that Becker's plea applies with equal force to historians of science. To accomplish that I must first sketch for you a bit more about "Everyone," the Scientific Practitioner of my title. Becker began by offering the following minimalist definition: "History is the memory of things said and done." He includes in memory, of course, not merely the recollections of one's own experience but also ideas acquired by hearsay or reading. From this definition it follows, says Becker,

> that every normal person, Mr. Everyman, knows some history. Of course we do what we can to conceal this invidious truth. Assuming a professional manner, we say that so and so knows no history, when we mean no more than that he failed to pass the examinations set for a higher degree .... Mr. Everyman, as well as you and I, remembers things said and done, and must do so at every waking moment. ... [T]he memory of Mr. Everyman, when he awakens in the morning, reaches out into the country of the past and of distant places and instantaneously recreates his little world of endeavor, pulls together as it were things said and done in his yesterdays, and coördinates them with his present perceptions and with things to be said and done in his to-morrows.
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> Yet ... unaided memory is notoriously fickle; and it may happen that Mr. Everyman, as he drinks his coffee, is uneasily aware of something said or done that he fails now to recall ... a bit of history lies dead and inert in the sources, unable to do any work for Mr. Everyman because his memory refused to bring it alive in consciousness. What then does Mr. Everyman do? He does what any historian would do: he does a bit of historical

[24] I mean "reality" in its ordinary pragmatic sense, not implying perfect knowledge, and I mean "fiction" to include innocent error as well as myths perpetrated deliberately. Some scientists do sometimes falsify history, and other scientists embrace their stories. Frank Sulloway (1992) convicts Sigmund Freud of constructing false tales about his own early career, building a myth which stayed central to psychoanalysis for many years. In this scientists are no better than, but probably no worse than, everyone else.

> research in the sources. From his little Private Record Office (I mean his vest pocket) he takes a book in MS ... and there he reads "December 29, pay Smith's coal bill ...."[25]

Everyman has a vivid recollection of "the precious coal sliding dustily through the cellar window," but when Smith informs him that it was Brown who supplied it, Everyman does further historical research, that is, he looks in his papers and uncovers Brown's invoice. Becker teases his audience, "If Mr. Everyman had undertaken these researches in order to write a book instead of to pay a bill, no one would think of denying that he was an historian."[26] From this Becker builds toward the assertion I quoted from a moment ago. "The history that does work in the world, the history that influences the course of history, is living history, that pattern of remembered events, whether true or false, that enlarges and enriches the collective specious present, the specious present of Mr. Everyman."

What he means by "specious present" is something larger than the mere instant of time between past and future; it is the wider range of awareness in which a person actually functions. "Of all the creatures," Becker says, "man alone has a specious present that may be deliberately and purposefully enlarged and diversified and enriched. The extent to which the specious present may thus be enlarged and enriched will depend upon knowledge, the artificial extension of memory, the memory of things said and done in the past and distant places."[27]

And it is this enriched memory, says Becker "running hand in hand with the anticipation of things to be said and done, [that] enables us, each to the extent of his knowledge and imagination, to be intelligent ...."[28] Becker leaves it to his hearers to conclude that if they choose to research matters Everyman can find relevant, they contribute to human well-being.

I ask you please to repeat in your imagination this simple exercise, as Becker outlined it, replacing now for Everyman a scientist. Even those of us who have not played ethnographer in someone's laboratory can call up a picture of Dr. Anyone, thinking as she drinks her coffee of a bill for glassware she must pay, and wondering what experiment it would be most fruitful to do next. We may like to say, "Oh, like most scientists she cares nothing for history," when what we really mean is that she would fail the exam in our own undergraduate course. You may be sure she knows what was said and done in her little corner of science in the recent past, and when memory fails her she

25 Becker, 1935, pp. 235–237.
26 Becker, 1935, p. 239.
27 Becker, 1935, p. 241.
28 Becker, 1935, p. 242.

knows how to seek out the record, even if her methods involve shortcuts like asking her colleagues what they have been up to. The practitioner of science must always be operating in a moving, sparkling space hovering between the latest results, which point in certain directions but never with certainty, and tomorrow's hoped-for discovery.

This picture of the working scientist is essentially the same as other scholars are emphasizing, for example, Pickering's *Mangle of Practice* (1995). After many decades when the logic of scientific reasoning seemed to float timelessly, we are now hearing about the historicity of science. Sometimes, though not always, Dr. Anyone is allowed to be aware, as Becker assumed Mr. Everyman was, of her own time-embededness.[29] Following Becker I notice that the specious present of scientists must include, besides yesterday's results, whatever thoughts they hold about last year and the past decade and lessons from the more distant past. Exactly as Becker did, I want to insist that every scientist is her own historian, whether we professionals like it or not.

Maybe you are tempted to regard all this as a false deduction from Becker's trick definition of history.[30] You do not doubt that scientists are keenly interested in what happened down the hall last week and in the laboratories of their peers last year, and also that they may even find amusement in their mentors' tales of what their field was like a generation ago, but these things are not what we mean when we use the word history. Yet as for written history, you admit that they do read each other's review articles, anniversary speeches, and obituaries. But you still assert that most scientists are not really interested in the history of science properly defined. I disagree. I think we have been misled by the fact that scientists are not interested in the history of science that we professionals choose to write. And why should they be? By buying into the fiction of history for its own sake, we let ourselves ignore the needs of potential readers.

On the other hand, maybe you admit our isolation from scientists, our own version of the Ivory-Tower Syndrome, but you do not see this as a problem. You may say, "Oh, it's not news to us that scientists do serve as their own historians. We long ago noticed them using history in their polemics, for purposes of pedagogy or legitimation, and we are quite glad not to subjugate ourselves to their interests, and we want no part of that agenda." Paul Forman says as much in his piece "Independence, not transcendence, for the historian

29 MacIntyre, 1977; Rouse, 1990.

30 I readily admit that Becker's Everyman is a rhetorical ploy. Sungook Hong insightfully points out to me that in the same sense Everyone is her own Scientist, that is, all of us must have some ideas about the natural world. One could therefore construct a parallel complaint about scientists' poor communication to non-specialists.

of science," in which he contends it is high time we historians of science acknowledged our intellectual independence from scientists. We ought, he says, "to be irked by our extreme passivity. . . ."[31] According to Forman, scientists aim at transcendence, that is, at an illusory higher plane of knowledge, and we should not join them in this quest, but rather should dare to pursue "genuine intellectual autonomy."[32]

You may be surprised, if my emphasis on Becker left you with the impression that I think we should kowtow to scientists, that I perfectly agree with Forman too. The reason, though, is simple. I trust that Dr. Anyone is just as full of common sense and healthy self-interest as Becker's Mr. Everyman, who wanted to know who he should pay for his coal, not just who he incorrectly imagined had delivered it. Becker did say the specious present includes false beliefs, but he credited Mr. Everyman with preferring truth. As Becker put it, "One of the first duties of man is not to be duped, to be aware of his world; and to derive the significance of human experience from events that never occurred is surely an enterprise of doubtful value."[33] The virtue of our professionalism is that we have the time, skills and tools enabling us to construct a more accurate picture of the past than part-time untrained people can, and I count intellectual independence among those tools. As for Dr. Anyone or other readers of history, I believe, as the movie says, if we build it, they will come.

The problem is, we have not built it, and we cannot build it. We cannot construct an honest, vigorous, balanced picture of the history of science, neither of science in general nor any field of science that the practitioner of science could recognize as meaningful. Our very professionalism forbids us to try. Our hands are tied, we are wearing blinkers. It is our own version of the Taboo Problem. Our own brand of anti-presentism far exceeds that of general historians, and it affects our subject matter more seriously. We have what amounts to an enormous taboo blacking out the very center of the topic we claim to study. What Butterfield called Whig history, we transformed into what Forman calls the bogey of whiggery.[34] In requiring us to concentrate on context to the exclusion of measuring achievement in relation to subsequent knowledge, our disciplinary taboo has had the automatic effect of banning the concept of progress.

Scientists have always thought, and our own intellectual predecessors thought, that the central theme of the history of science, the big picture, is the progressive increase in knowledge. George Sarton certainly thought so.

31 Forman, 1991, p. 77, n. 20.
32 Forman, 1991, p. 78.
33 Becker, 1935, p. 249.
34 Forman, 1991, p. 82; Wilson and Ashplant, 1988; Ashplant and Wilson, 1988.

Philosophers, although divided over how to define it, acknowledge scientific advance.[35] Yet we professional historians of science talk about social context, we analyze the internal twists and turns of experiment and theories, and we are very good at documenting activity, but we studiously avoid mention of the progressive direction of scientific change. Indeed at any mention of progress, we roll our eyes at the ceiling. And as with other taboos, we think our avoidance is rational rather than imposed on us by social prejudice, when we think about it at all.

There was of course a good reason, when our discipline was new, to scorn the whiggishness in scientists' histories. Just as Butterfield warned in civil history, a chronicle of contributions leading inexorably towards the modern view gives an utterly misleading picture of how progress is achieved. The problem with Whig history is that it is teleological, in the bad old discredited Aristotelian sense; steps of improvement are chronicled as though they need no cause but their own correctness, that the endpoint somehow pulls events forward, as the oak tree exists in the acorn. Lifting discoveries out of context cannot teach us how they were made, recognized, tested and accumulated. Professional historians of science are excellently qualified to answer all these questions, but we flee the issue, busying ourselves with describing everything else about science except its spectacularly progressive nature. When scientists complain that we throw out the baby and study the bathwater,[36] I used to assume this referred to the internal versus the external aspects of science, but this cannot be right, because even our internal histories are disappointing. My own writing involves plenty of technical detail, but one scientist confessed to me his puzzlement that he could not tell whose side I was on. At the time I did not know what to make of his letter, but now I believe the discarded baby is scientific progress.

Of course I am aware that progress, as the master narrative for civil history, had its heyday and has been sorely battered.[37] I am aware that with respect to biological evolution, where progress once seemed to be a law of nature, the facts do not support it even as a weak trend.[38] Those qualms about other areas of progress only strengthen, it seems to me, a view like George Sarton's, that historians of science are privileged to be describing an exceptional and remarkable saga, a human enterprise that is spectacularly and essentially progressive. I do not worry that this limits us to celebrating the successes claimed by scientists, because besides evaluating scientists'

[35] Laudan, 1977, 1990; Kitcher, 1993.
[36] Smith, 1991, p. x.
[37] But cf. Himmelfarb, 1987.
[38] Ruse, 1996; Gould, 1996.

success ourselves, as Paul Forman urges, we can assign ourselves the even more interesting job of investigating the causes of progress in science.

The word "progress" carries such a burden of associations that I would be glad to abandon it when referring to the increase of scientific knowledge. From my thesaurus I offer in its place the obsolete term "emendation," which means improving by making corrections. I would be comfortable asserting that science is emended, by which of course I do not mean that it magically emends itself, but that facts accumulate and theories improve (reciprocally interconnected) because scientists, working within a social structure that enables this outcome, exert themselves in that direction. I can imagine writing a book entitled *On the Emendation of Systematic Biology*. I'd prefer this to organic metaphors like growth or development, which are teleological, nor would I call it *The Evolution of Systematics*, because I expect to find a prominent role in directing scientific change for intelligence and intention, which neo-Darwinism excludes from biological change.

When scientists complain about our "prig history,"[39] our replies only show how oblivious we are to the ways our taboo has crippled our ability to explain what is most worth explaining about science.[40] It was predictable that we would scoff at the whiggishness of a book called *The Growth of Biological Thought*, but what should we say when its biologist author replies that there is nothing wrong with tracing the genealogy of ideas?[41] We might concoct an abstract answer, but the proper reply ought to be to produce narratives of our own that are more realistic and meaningful than we have hitherto been free to conceive.[42]

Until I began to ask myself recently how the norms of my discipline were shaping my work, I went to great lengths in my own career to skirt around the edges of the central story in the history of systematics. I have looked at how some systematists before Darwin understood their science, and I have looked at the institutional base of the science, that is, natural history museums. What I have never directly confronted was the issue of how a modest number of men and women, a few of them clever or heroic but most of them quite ordinary, and several of them displaying serious weaknesses of character or insight, how this odd collection of people could have combined their efforts in such a way as to construct the enormous body of knowledge about life's diversity

[39] Harrison, 1987.

[40] I was struck by how fully I shared this oblivion when I recently re-read Laudan 1990a. It made no impression on me when it appeared, but it clearly contains most of the ideas I am now enthusiastic about.

[41] Mayr, 1990.

[42] After I explained my thoughts on our disciplinary taboo to Joan Steigerwald, she pointed me to Biagioli's stimulating 1996 article; in several respects he and I seem to be reaching in the same direction. Another article I have found helpful is Brush, 1995.

that is our heritage today. Whatever its present imperfections, systematics has certainly progressed. A chronicle of achievements cannot explain its emendation, but I am confident that historical research directed at the question can expose the process. Released from my apprenticeship in the childhood of a discipline, profiting from the security of a mature discipline that no longer needs taboos, I now give myself permission to concentrate on what I judge significant in the history of science. Emancipated from an outmoded taboo, I can look for the big picture, unafraid to be called whiggish, as Stillman Drake was always unafraid.

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