Making It Up

The recent academic “scandal” prompted by the efforts of a trio of scholars to discredit “grievance studies” (their word) by submitting twenty fraudulent articles (seven were accepted) to a range of journals in such “subdomains of thought” as “gender studies, masculinities studies, queer studies, sexuality studies, psychoanalysis, critical race theory, critical whiteness theory, fat studies, sociology, and educational philosophy” has set me to thinking: Would the American Historical Review be able to spot such a fraud? Or are we similarly vulnerable to fake scholarship playing to our preconceived ideas and designed to reveal the bankruptcy of our peer review process, not to mention our entire intellectual endeavor?1

My first reaction was that this could never happen at the AHR, or indeed at any history journal. One thing that has baffled me about this entire affair, for instance, is why the editors of the duped journals sent the submissions out for review in the first place, given that their provenance should not have passed an initial smell test. For instance, a number of the articles—most notoriously the ones on dog parks and on sex toys—were represented as the products of a fictitious research outfit called “the Portland Ungendering Research Initiative.” 2 Moreover, the authors supposedly attached to this institution were similarly entirely fabricated. Don’t the editors of Gender, Place and Culture or Sexuality and Culture use Google? Don’t they check the affiliations and bona fides of those submitting articles before they send them out for review? If not, this takes “blind” peer review to an absurd level, constituting an appalling willingness to waste the time of external reviewers. In a few other instances, the fraudsters prevailed upon a retired history professor at Gulf State College to allow them to attach his name to the articles (an ethically questionable practice, to be sure, since he “submitted” articles under his name that he did not write himself). But here again, a quick online search makes the real Richard Baldwin an unlikely author of “An Ethnography of Breastaurant Masculinity: Themes of Objectification, Sexual


2 “Helen Wilson,” “Human Reactions to Rape Culture and Queer Performativity at Urban Dog Parks in Portland, Oregon,” Gender, Place and Culture [retracted]; “M. Smith,” “Going In through the Back Door: Challenging Straight Male Homohysteria, Transhysteria, and Transphobia through Receptive Penetrative Sex Toy Use,” Sexuality and Culture 22, no. 4 (2018) [retracted]. Ironically, by citing these “articles,” I contribute to their potential credibility, boosting their citation index—even though they are now labeled by the journals as “retracted,” at least in the online versions. Obviously, the print versions will remain forever unchanged, perhaps to be read by unsuspecting scholars or students decades from now.
Before submissions are sent out for review, it tends to be a good idea to make sure the authors’ purported areas of expertise match up with the research they claim to have done. Nevertheless, the presented methodology in all three of these accepted articles appears reasonable, even if the identity of the researchers does not.

These strike me as basic matters of due diligence for any editorial team; the editors’ neglect seems well out of the norm for any history journal I can think of, let alone the AHR. But, to give them the benefit of the doubt, I have a sneaking suspicion that such sloopy practices may derive from a combination of electronic submissions systems, a lack of sufficient editorial attention (Gender, Place and Culture comes out twelve times a year), and perhaps an overly insistent policy on making reviews “blind,” so that even the editors do not see (or pay attention to) an author’s name and affiliation until the final stages of production. To my mind, this serves more as an indictment of the growing “assembly line” attitude of some academic publishers and scholarly journals, made ever more possible by consolidated journal publishing and editorial management systems that divert manuscripts to the cloud, than a skewering of the peccadillos of any particular “subdomain” of study. “Editors” need only read an abstract or title, and then click on a button that matches the submission with a potential reviewer. I was struck, for instance, by the fact that one of the fake submissions was sent to a graduate student as a peer reviewer. (To his credit, while he took it seriously, he did ultimately reject it.) There is much to recommend these time-saving instruments—the AHR will adopt some of them in the coming years—but not at the expense of the hands-on editorial attention necessary to sniff out fraudulent material.

Lurking behind such editorial details, however, there is a much larger question: Epistemologically speaking, how can editors of history journals maintain their confidence that the research data underlying article submissions is authentic? Here we are not talking about plagiarism, or political trolling disguised as a book review, or dishonest and unfair reviews—but out-and-out invention of sources with malicious intent aforesought. For that’s what the “grievance studies” fraud or hoax boils down to. The submission of fraudulent research, based on mock field studies never conducted, snared credulous external readers and editors. Some scholars have gleefully taken this as an unmasking of the unwarranted assumptions and confirmation bias of an entire field, exactly as the perpetrators of the hoax intended. But one could also say that once taken in by the initial methodological deception—that such a study was in fact conducted—any responsible external reader would measure the fit between the conclusions and the data, not question the very existence of the data itself. (As in, are you sure you witnessed dogs humping at the dog parks? Which dog parks? What breeds?) Let’s face it, some of the dog park article’s governing questions, such as “How do human companions manage, contribute, and respond to violence in dogs?,” constitute perfectly valid inquiries. Observation of dog park interactions strikes me as a sensible way to study this question, just as ethnographic immersion in a neighborhood is an acceptable method for studying crime and the police. Either ethnography is a valid research method or it isn’t; our faith in the results rests on our trust that a particular study was indeed carried out as described. External readers weigh the

3 Published in Sex Roles, September 2018, and now “retracted.”
conclusions against the data, but are not asked to guess at or comment on the authenticity of the latter. (And yes, I am well aware that there are recent debates about this in ethnographic sociology.) Once an editor is taken in by the idea that Helen Wilson of the Portland Ungendering Research Initiative exists—well, then it is all over. Why distrust the “author’s” assertion that her study “draws on nearly 1000 h of public observations of dogs and their human companions conducted at three dog parks in Southeast Portland, Oregon, beginning on 10 June 2016, and ending on 10 June 2017”? I can think of better ways to spend three hours a day over the course of a year, but who am I to question the dedication of an intrepid researcher willing to examine the genitals of 10,000 canines? Indeed, the research methods are discussed in quite some detail in this article—this, not the “results” ginned up to make a journal look foolish, is the real hoax. I am somewhat surprised that the trio of hoaxers didn’t go the extra mile and create a fake website for the PURI.

So, then, how and why is fake history different from such fraudulent social science—or is it? To be sure, one can think of several notorious cases in which legitimate scholars have been charged with outright falsification—that is, with inventing sources that did not exist. But the truth is, out of the thousands of scholarly articles published in the past three decades, the three significant cases I am thinking of strike me as a very small number (there may be more; I invite readers to call them to my attention). In some instances, the charges of fabrication of research proved to be groundless; in others, the fine line between sloppy research and “intentional fabrication or falsification of research data” proved far more difficult to draw.4 More often than not, questionable methods, poorly documented sources, and egregious misapprehension—“unrelenting carelessness,” as one of those called out for his mistakes put it—of evidence driven by wishful thinking have been mixed in with sound research practices.5 And while (usually self-interested) critics sharply challenged specific findings—that big business abetted the rise of the Nazis, that gun ownership was not so widespread in Revolutionary America, that the “transcript” of the Denmark Vesey trial was a reliable source—these challenges served as much to stimulate vigorous and ongoing debate as they did to refute outright falsifications. In no case did the alleged invention of data throw an entire subfield of the discipline into epistemological question; nor, unlike the recent “grievance studies” hoax, was it designed to. I do believe that history as a field is relatively immune to such malicious practices, in part because of its positivistic reliance on archival data, which can be difficult—though not impossible—to create out of whole cloth. (Oral history, a close cousin of ethnography, might be an exception—although oral historians follow a very strict code of ethical practice and demand archival accountability.) If scholarly journals of history have a vulnerability, it is the professional premium we tend to put on “originality,” which can lead authors, editors, and reviewers alike to embrace apparently unconventional evidence and argument in the interest of advancing “new” findings.6

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4 The phrase comes from Emory University’s investigation of the research of Michael Bellesiles. See “Report of the Investigative Committee in the Matter of Professor Michael Bellesiles,” July 10, 2002, https://www.emory.edu/news/Releases/Final_Report.pdf. The report certainly did not exonerate Bellesiles, finding him guilty of research misconduct for his work Arming America. Still, the investigators proved reluctant to conclude that he had engaged in widespread, comprehensive, willful deception in his research in probate records.


6 I must thank David Thelen for this important insight.
There is one very important exception I can think of to this rule—the efforts of Holocaust deniers like David Irving to wrap their ideology in the cloak of historical objectivity in order to get dangerous falsehoods accepted as objective “truth” by credulous readers. As Sir Richard Evans has demonstrated, Irving’s work is shot through with “calculated and deliberate . . . distortions, suppressions and manipulations” of the available evidence. Yet, while Irving has published a long shelfful of books, he has never, as far as I can tell, had one of his articles accepted through peer review in a scholarly journal other than the prime outlet of Holocaust denial, the *Journal of Historical Review* (which is not, in fact, peer-reviewed). As Evans points out, the books themselves sometimes pass muster with reputable publishers because few scholars have the time to trace every single citation or quote to its original source; if they did, Evans demonstrates, they would discover deliberate misuse of the historical evidence. “The doctoring of the historical record,” Evans concludes, “was not a small part of Irving’s work, but ran right through it.”

Then again, no less a publication than the *American Historical Review* ran a respectful, if mildly critical, review of Irving’s 1977 work *Hitler’s War*. Though recognizing that Irving “tends to push every bit of evidence to the limits of credibility,” the reviewer nevertheless concluded that despite his concerted efforts to exonerate Hitler from responsibility for the murder of European Jewry, “Irving is neither a closet revisionist nor a covert anti-Semite.” Both of these judgments would be reversed in the notorious unsuccessful libel suit Irving brought against historian Deborah Lipstadt two decades later, who accused him of being just those things.

But I have not answered the original question: Could the *AHR* fall prey to a full-blown hoax aiming to discredit the legitimacy of our peer review process? We did once get a well-argued and highly original submission contending that the only reasonable explanation for Columbus’s ability to reach the Western Hemisphere was that extraterrestrial beings had provided him with a map. The cited research appeared somewhat questionable, however, so I declined to send it out for review. More seriously, though, imagine if you will a submission purporting to show definitively that “birthright citizenship” for immigrants was almost as important to the framers of the Fourteenth Amendment as the protection of the rights of emancipated slaves. Surely that would conform to, indeed flatter, some expectations among the liberal historical professoriate at this particular moment. Imagine further that this submission claimed to be based on a newly discovered cache of letters between Charles Sumner and Frederick Douglass, in which Sumner wrote, “By all means, I believe the Amendment as proposed should apply to all persons born here, whether they be the progeny of those kidnapped into bondage before the late War, brought here under indenture, or surreptitiously arrived of their own free will.” Or that Sumner went on to write, “I truly anticipate a vast horde of coolies [sic] and migrants to make their way to our shores in the coming decades, not least because the former slave masters will seek to replenish their source of labor, now eliminated by the recent [Thir-

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teenth Amendment. Should not the children born of these new arrivals be recognized as citizens of the United States, with all the rights and duties that entails, and be protected by the Federal Govt therein?"  

Would we be taken in by this? I doubt it, not least because our articles editor, Jane Lyle, does take the time to check the existence of archives cited in the text, right down to the box and folder number when digital inventories allow. When feasible, she even checks digital copies of archival documents for accuracy—far easier to manage now, of course, than even a decade ago. Admittedly, this forensic examination comes at the end of the process, after an article has been peer-reviewed and accepted. But this is an important fail-safe, in the unlikely event that a knowledgeable peer reviewer missed this fake reference. Nevertheless, as Jane reminds me, while she takes as little on faith as possible, at the end of the day we do have to trust that our authors are real people citing real sources as accurately as possible. It is that fundamental trust which makes historical debate and exchange possible. And, just as counterfeit money undermines collective confidence in the value of currency, counterfeit scholarship may fatally wound our faith in knowledge and research. Surely that should be unwelcome in these times.

A.C.L.

Charles Sumner to Frederick Douglass, June 12, 1867, and Sumner to Douglass, July 23, 1867, both in Ottlie Assing Papers, Staatsarchiv Hamburg, Korrespondenz, Feld 12, Ordner 6. Lest there be any misunderstanding, I have invented these documents and this collection, although the Staatsarchiv does exist. Assing, Douglass’s mistress, was originally from Hamburg.