On Method in the Humanities

Grant Wythoff

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From October 1975 to August 1977, Bruno Latour conducted two years of ethnographic fieldwork in a scientific laboratory within the Salk Institute for Biological Studies. Latour and his collaborator Steve Woolgar were interested in two main questions: first, how are "facts constructed in the laboratory," and second, "how can a sociologist account for this construction?"[1] Starkly departing from conventional histories of scientific discovery in which insight flashes up in moments of inspiration or serendipity, Latour painstakingly catalogued the everyday minutiae of laboratory life to portray the processual nature of science, from researchers perusing copies of *Nature* over lunch, to colleagues yelling down the hall with questions about solvents, to the transcription and mailing of the day's lab notes.

> *5 mins.* John enters and goes into his office. He says something very quickly about having made a bad mistake. He had sent the review of a paper. . . . The rest of the sentence is inaudible.
>
> *5 mins. 30 secs.* Barbara enters. She asks Spencer what kind of solvent to put on the column. Spencer answers from his office. Barbara leaves and goes to the bench.
>
> *5 mins. 35 secs.* Jane comes in and asks Spencer: "When you prepare for I.V. with morphine, is it in saline or in water?" Spencer, apparently writing at his desk, answers from his office. Jane leaves.

The challenge of this project for Latour and Woolgar was one of triangulation: while as sociologists they were busy staking their claims relative to competing models and arguments within their own discipline, the biologists they observed similarly positioned their claims relative to the evolving models produced by others in *their* field. They write:

> The observer has to base his analysis on shifting ground. He is faced with the task of producing an ordered version of observations and

[1] Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts*, (Beverly Hills: Sage Publications, 1979), 40.

> utterances when each of his readings of observations and utterances can be counterbalanced with an alternative. [...] we argue that both scientists and observers are routinely confronted by a seething mass of alternative interpretations. Despite participants' well-ordered reconstructions and rationalisations, actual scientific practice entails the confrontation and negotiation of utter confusion. (35, 36)

These complexities in mind, Latour and Woolgar attempted to draw upon what they called "socially available procedures for constructing an ordered account out of the apparent chaos of available perceptions." (33)

Part of what we've seen over the past couple days in this wonderful cluster of panels on lab culture in the humanities is that we have a similar set of moving targets in play. But if it proved a challenge for Latour and Woolgar to pin down the parallax movement of competing claims within this encounter of sociology and biology, then the multiplicity of approaches and assumptions represented by *humanities* labs presents an even more dizzying array of cultures to study. *Method* in the humanities spans quantitative and qualitative models, inductive and deductive approaches, collaborative and solitary forms of inquiry. We have mixed methods practiced by interdisciplinary scholars, each of whom constellate the variety of their influences in unique ways. We have artists and tinkerers who value entirely different outcomes from a particular inquiry, than, say, a historian or a linguist might.

Latour and Woolgar's seminal work highlights some of the key questions I want to keep in view during my talk today. If we begin with their two original questions as starting points, we quickly begin to see how they open out onto new issues for us as humanists. First, the question of how *facts* are constructed leads us to wonder, do we in the humanities have any special investment in the value of facticity itself, with all of its corollaries, like discovery, corroboration, and replicability? And, second, the question of how to produce an account of this construction of fact leads us to wonder how good are humanists themselves at studying the workflows of other humanists? Ironically, Latour and Woolgar use what they call "literary description" to describe lab practices (45). But I'm left wondering whether it's possible to produce an account of a representative instance of humanistic inquiry, and how the recent emergence of laboratories for humanities research extends, critiques, or comments upon what we've always already been doing in the humanities.

I'm coming at these questions as one of the co-founders of the Group for Experimental Methods in the Humanities at Columbia University. (Unfortunately we don't have any catchy acronym—we were tempted to shorten our name to Meth Lab but we figured the administration wouldn't approve.)

Every Friday, a regular cohort of grad students, and some undergrads, meet to incubate new projects while faculty conduct workshops on programming languages, collaborative research tools, and open-source authorship. Overall, we're interested in the question of what it means to "experiment" in the study of history, literature, and philosophy. As the tagline on our website has it, the Group is dedicated to the rapid prototyping of speculative ideas. One of the ways this works out in practice is to invite faculty from across campus to discuss their work – many have no experience with digital methods but arrive with a specific theoretical or research problem that they'd like to play around with. We take these research questions and use them as a prompt to collect, curate, and publish intermediary artifacts of scholarship: drafts, notes, graphics, Twitter bots, web protocols, "situations," tools, and tutorials that take us some way from unstructured thought to some form of accepted knowledge. The group's emphasis on "speculative ideas" serves as a bridge between the expertise of technologists and subject specialists in producing scholarly artifacts that require both.

Somewhere between a lab experiment and experimental art, we try to open a space for process-based scholarship, in the words of John Cage: "to be judged not on its success or failure, but simply as an act the outcome of which is unknown."

`Our projects can be found at <http://xpmethod.plaintext.in/research.html>`

One of the other things we've been trying to do at the Group for Experimental Methods is to think about all of these activities in a historical frame.

So over the last academic year, we hosted a visiting lecture series that attempted to provide some historical perspective on projects like these by examining the range of methods, theoretical and practical, used by humanities scholars and critics, past and present. In preparing for the series, these are some of the questions we posed to our participants: what are the overarching techniques—what John Unsworth calls our "scholarly primitives"—and epistemologies inherent to humanities research? How are the technological challenges and opportunities provided by new research methods and organizational structures tethered to epistemological shifts as well? Following Thomas Kuhn, can we outline paradigms of humanistic inquiry? Does it make sense to define "method" in the context of the humanities and if so, what are the varieties that method has taken on? What are the national specificities of these methods, and of descriptions of the humanities itself?

So while much time has been spent theorizing the "digital" in digital humanities, we sought with this series to gain a greater understanding of the heritage and future of humanities methods in general, while contextualizing more precisely the contributions of computational approaches in the process.

Personally, I started to become interested in how laboratories in the humanities are usually seen as synonymous with the growth of digital humanities as a. . . whatever you want to call it—field, discourse, mania. Humanities labs are established when some kind of connective tissue has formed between a particular institution's departments and offices, and the opening of a new lab space is a signal that DH of some kind has officially taken root on the campus. But there is a much longer history of collaborative research spaces that sparked debates over *method* in the humanities that isn't reducible to the introduction of computation.

Thinking about this history of method in the humanities has provided us with a useful heuristic for thinking about the discourse surrounding DH today.

This seems like a relatively simple point that we'd all agree on—that in order to understand the present we need to look to the lessons of the past. But unlike scientific research, which has a robust academic discipline devoted to its history—the history of science—we don't really have anything like a field called the "history of the humanities" (although a new society was recently founded on just this topic – they have a yearly conference and a very interesting new journal that's just getting off the ground). So it wasn't exactly anyone's natural conclusion that classical philology would be the place to negotiate issues in machine learning, for instance, but that's kind of what happened.

And so one of the things I've been up to lately is hunting around for more instances of historical labs. For today, I've prepared a series of exhibits—three from the work of other scholars and two that I'm working on—that stage particular questions about the role of method in the humanities.

Exhibit 1: Chad Wellmon has written on what he calls the "collective empiricism" of 19th-century German philologists, and in particular an effort led by Theodor Mommsen and a team of hundreds of philologists over decades to collect and catalogue every Latin inscription ever found. Within this massive research project, individual scholars located the dignity and value of their work not in their particular object of study, but in their contributions to a massive, collective undertaking requiring so much labor that the project would necessarily outlive them all. Wellmon argues that the history of such collaborative research projects should throw into relief new work in DH and the way we value and credit the contributions of librarians, programmers, designers, and students doing data entry.[2]

Jerome McGann in a piece for *Critical Inquiry* describes how Jean-François Lyotard's book *The Postmodern Condition* actually began as a 1979 report that was commissioned by the Counsil of Universities of Quebec to examine the impact of modern technology on knowledge production and research institutions. In this report, Lyotard called for a "postmodern science" that would de-emphasize instrumental reason and change "the meaning of the word knowledge, while expressing how such a change can take place. It is producing not the known, but the unknown." McGann remarks that "though [Lyotard's project was] based in a critique of instrumentalism, Lyotard's report was itself instrumentalist. It projected an institutional reorganization as the means for promoting the practice of postmodern science."[3]

If you *really* like labs and want more labs in your labs, there's recent work

[2]Chad Wellmon, "Sacred Reading: From Augustine to the Digital Humanists," *The Hedgehog Review*, 17, no. 3, (2015), http://www.iasc-culture.org/THR/THR_article_2015_Fall_Wellmon.php

[3]Jerome McGann, "Philology in a New Key," *Critical Inquiry*, 39, no. 2, (January 2013): 327–346, doi:10.1086/668528

in archaeology on the reconstruction of alchemical techniques using electron microscopes and X-ray fluorescence imaging to study the equipment of a 16th century alchemical laboratory and learn more about the specific techniques "natural philosophers" had in "the quest for the philosophers' stone and the transmutation of base metals into gold." This collaboration between archaeologists and historians of science is referred to by the study's authors as "a sixteenth century lab in a twenty-first century lab."[4]

One historical "lab" that lately I've been very interested in revolves around Marshall McLuhan's 1953 application to the Ford Foundation for a grant—titled "Changing Patterns of Language and Behavior and the New Media of Communication"—to support a research group that would "observe and describe the nature and effect of the new kind of language which has come into existence as a result of the meeting of the new media with the old." This was before McLuhan had made his career shift from literary scholar to guru of the electronic age, and the money from Ford established a group with which he began thinking about the unique ways that the humanities could contribute to the study of new media that the social sciences couldn't. The group would later become the Centre for Culture and Technology at the University of Toronto, which at the time was basically McLuhan's office, but it's a center that still runs today.

What's so interesting to me about this proposal—a formative moment in the history of my discipline, media studies—is how it plays off of McLuhan's long-standing debate with his colleague and "nemesis" at the University of Toronto, Northrop Frye, a scholar who couldn't be more different. McLuhan was a convert to Catholicism, while Frye was an ordained minister of the protestant United Church of Canada. McLuhan went to Cambridge and was trained by the founders of the New Criticism and its "close reading" practices (I.A. Richards; F.R. Leavis), while Frye went to Oxford and formulated a unique, systematic approach to literally all of literature that would later inspire and echo the enthusiasms of structuralist theorists in the 1960s.

Their debate was fundamentally one that centered on the question of method: McLuhan's New Critical approach attempted to isolate the sensory structure of a single aesthetic experience, while Northrop Frye was a critic who attempted to formulate "a total systematic understanding of the fundamental laws governing all of literature."[5] I don't know if anyone has ever read *The Anatomy of Criticism* but it's an absolutely wacky book—and worth the read.

While today, we might associate Frye's macro-level, or "distant" reading approach—distilling the entirety of literary production down to a finite number of forms and mythemes—with collaborative research labs in the humanities, it

[4]Marcos Martinón-Torres, Thilo Rehren, and Sigrid von Osten, "A 16th-Century Lab in a 21st-Century Lab: Archaeometric Study of the Laboratory Remains from Oberstockstall, Austria," *Antiquity*, 77, no. 298, (2003)

[5]Alan Bewell, Neil ten Kortenaar, and Germaine Warkentin, eds., *Educating the Imagination: Northrop Frye, Past, Present, and Future*, (Montreal & Kingston: McGill-Queen's University Press, 2015), 3

was McLuhan's emphasis on close reading media that spawned a new humanities lab with many participants. Frye preferred to work alone, viewing the work of the critic as a strangely solitary form of "science":

He'd write: "If criticism exists, it must be an examination of literature in terms of a conceptual framework derivable from an inductive survey of the literary field. The word 'inductive' suggests some sort of scientific procedure. What if criticism is a science as well as an art?"

Adding. . .

"However, if there are any readers for whom the word 'scientific' conveys emotional overtones of unimaginative barbarism, they may substitute 'systematic' or 'progressive' instead."

My final "exhibit" is one in which macro-level arguments were made in the humanities by strangely deductive, rather than the supposedly inductive means cited by Frye.

This one revolves around the highly controversial publication in 1974 of Robert Fogel and Stanley Engerman's *Time on the Cross,* a book that at once heralded the introduction of computationally-assisted, quantitative methods into the discipline of history *and* used them so irresponsibly that these methods wouldn't return to mainstream historiography in any form for decades.

Robert Fogel was an economic historian at the forefront of a movement founded at the end of the 1950s in the U.S. known as the "new economic history" or "cliometrics," taking its name from the Greek muse of history, Clio. Fogel in particular was interested in counterfactual questions, challenging generally accepted notions by removing certain variables in a causal chain of events to see if historical change could have proceeded any differently. In his research on nineteenth century railroads and economic expansion, for instance, he challenged the idea that rail was "an important factor in the economic development of the United States," so he built a model that allowed him to "test" "what the American economy would have been like in 1890 without the railroads."[6]

Fogel took what is a quintessentially inductive discipline—history gathers details, evidence, and documents in order to look for patterns that serve as the basis for broader narratives of actors, events, and change—and transformed it into a form of deduction: beginning from an experimental assertion and working backward to show a certain sequence of events in which this assertion, mathematically speaking, has to be true.

Time on the Cross brought this style of thinking to bear on the history of American slavery. The undertaking was massive: Fogel teamed with Engerman to lead a team of five other cliometricians, each with their own specialized skills, in collecting data from census records, sales invoices from the New Orleans

[6]Francesco Boldizzoni, *The Poverty of Clio: Resurrecting Economic History*, (Princeton, N.J: Princeton University Press, 2011), 13

slave market, and planation records culled from local historical societies. Eleven "resident graduate students" at the University of Chicago assisted on the project, and as many as fifteen additional grad students at other institutions headed up larger "data collection teams." In addition, over a hundred readers reviewed drafts of the manuscript at various stages.[7]

All of this data was collated and printed on punch cards which were then fed into computers that ran various mathematical models on the data. As Fogel told a reporter interviewing him about the book – and the book made a huge splash – the authors appeared on national television and were featured in *Time* magazine: "This project couldn't have been done in the pre-computer age. We were able to do in five minutes for $40 what would have taken 2,000 man days of work before."[8]

The problem with the book was that the conclusions Fogel and Engermann drew from these numbers was that slavery wasn't as bad as we think it was. Some of Fogel and Engermann's more ridiculous assertions included the idea that only " '2 percent of the value of income produced by slaves was expropriated by their masters,' and that this falls well within modern rates of expropriation" by wage labor. Or that "most slave sales were either of whole families or of individuals who were at an age when it would have been normal for them to have left the family." Or that "slave diets were extremely varied" and "nutritionally adequate." Or that "the typical enslaved person received less than one whipping per year (0.7, to be exact)."

Not only did Fogel and Engermann consult none of the existing scholarship in the field, but they presented these numbers as though they could speak to some truth independent of slavery's abject horror. Historians who reviewed the book didn't have a problem with computational methods per se. Instead, it was the rhetoric of computational thinking that bothered them. One of the book's reviewers, David Brion Davis, noted that Fogel and Engerman "speak casually of their legions of research assistants, of their mobile SAM computers, of their electronic weaponry, of their occupation of every hidden site. We are told that we are encircled, cut off, and cannot fight unless we have weapon-systems equal to those of the Cliometricians." C. Vann Woodward in another review found that "the rattle of electronic equipment is heard off stage and the reader is coerced by references to . . . inconceivable mountains of statistical data."

If history was to incorporate statistical methods, it was clear from the reception of this book that those methods had to supplement already existing work, rather than just from some kind of rhetorical move.

At first glance, it may seem across all of these exhibits somewhat reductive to discuss method in the humanities, an array of interlocking disciplines in which—to invoke the philosopher of science Paul Feyerabend's famous dictum from *Against*

[7] Herbert G. Gutman, "The World Two Cliometricians Made: A Review of F - E = T/C," *The Journal of Negro History*, 60, no. 1, (1975): 53–57, doi:10.2307/2716794

[8] Peter Gwynne, "Slavery Controversy," *New Scientist*, (November 1974): 512–513

Method—"anything goes."[9] Returning to Latour and Woolgar, the humanities also offer innumerable "procedures for constructing an ordered account out of the apparent chaos of available perceptions." But I'm not suggesting here that we confine ourselves to ex post facto reconstructions of brilliant arguments or the prescription of deadening, procedural checklists. Like the anarchic intellectual inquiry championed by Feyerabend, the humanities are no doubt "much more 'sloppy' and 'irrational' than [their] methodological image."[10]

But if I can risk the generalization, we tend to operate from an impoverished sense of the history of the humanities, and what side of that history we're on and draw on when it comes to micro vs. macro approaches, individual vs. collaborative research, analog vs. computationally inflected projects. This tends to lead us into making over-simplified and often incorrect assumptions about "factions" when describing the work we do in DH and humanities laboratories.

If there's one thing I can distill from this kaleidoscopic series of "exhibits" I've given you, it's this: the question of method is interesting in the humanities not because it makes possible replicability and corroboration as it does in the sciences, but because it allows us to produce useful "images" of the work we do: our assumptions, our tools, and the assumptions behind our tools.

[9] Paul Feyerabend, *Against Method*, (London; New York: Verso, 1993)
[10] ibid., , 160