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A Telescope for the Mind?

WILLARD MCCARTY

As to those for whom to work hard, to begin and begin again, to attempt and be mistaken, to go back and rework everything from top to bottom, and still find reason to hesitate from one step to the next—as to those, in short, for whom to work in the midst of uncertainty and apprehension is tantamount to failure, all I can say is that clearly we are not from the same planet.

—Michel Foucault, *History of Sexuality*

The phrase in my title is Margaret Masterman's; the question mark is mine. Writing in 1962 for Freeing the Mind, a series in the *Times Literary Supplement,*[1] she used the phrase to suggest computing's potential to transform our conception of the human world just as in the seventeenth century the optical telescope set in motion a fundamental rethink of our relation to the physical one. The question mark denotes my own and others' anxious interrogation of research in the digital humanities for signs that her vision, or something like it, is being realized or that demonstrable progress has been made. This interrogation is actually nothing new; it began in the professional literature during the 1960s and then became a sporadic feature of our discourse that persists to this day. I will return to present worries shortly. First allow me to rehearse a few of its early expressions. Then, following the clues these yield, I will turn to the debate that I am not at all sure we are having but which, if we did, could translate the neurotic search for justification into questions worth asking. The debate I think we should be having is, to provoke it with a question, What is this machine of ours for? Or, to make it personal, What are *we* for?

"Analogy is an identity of relationships" (Weil, 85), not of things. Thus the computer could now be to the mind, Masterman was saying, as the telescope was to seventeenth-century observers, enlarging "the whole range of what its possessors could see and do [so] that, in the end, it was a factor in changing their whole picture of the world." ("The Intellect's New Eye," 38) She suggests that by thus extending our perceptual scope and reach, computing does not simply bring formerly

unknown things into view but also forces a crisis of understanding from which a new, more adequate cosmology arises. (I will return to this crisis later.) She was not alone in thinking that the computer would make a great difference to all fields of study, but she seems to have been one of the very few who argued for qualitative rather than quantitative change—different ideas rather than simply more evidence, obtained faster and more easily in greater abundance, to support ideas we already have in ways we already understand. Masterman was a linguist and philosopher; pioneer in computational linguistics; one-time student of Ludwig Wittgenstein; playwright and novelist; founder and director of the Cambridge Language Research Unit; adventurous and imaginative experimenter with computing, for example in composing haikus and arguing for the significance of such work against sometimes ferocious opposition; and part of a community of people genuinely, intelligently excited about the possibilities, however implausible, that the computer was then opening up before hype muddied the waters.[2]

Masterman begins her contribution to Freeing the Mind by distancing herself from her predecessors' evident notion that the digital computer is "a purely menial tool": "in fact . . . a kind of intellectual spade. This, it has been shown, can indeed assist a human scholar . . . by performing for him a series of irksome repetitive tasks . . . that the scholar, unaided, just cannot get through. . . . They take too long, they are backbreaking, they are eye-wearing, they strain too far human capacity for maintaining accuracy: in fact, they are both physically and intellectually crushing" (38). She had (can we have?) quite other ideas. Nevertheless the complaint pointed to a very real problem—that is, very real drudgery that at various times the demands of maritime navigation, the bureaucratic state, warfare, and scientific research inflicted on those who were professionally adept at calculation. Thus Gottfried Wilhelm Leibniz complained about enslavement to "dull but simple tasks" in the seventeenth century, Charles Babbage in the nineteenth, and Herman Goldstine in the twentieth (Goldstine, 8–12; Pratt, 20–44). All three responded by devising computational machinery. We certainly cannot and should not deny the crippling effects of the mathematical drudgery about which they all complained. But, Masterman insisted, these spadework uses, however welcome the time and effort they liberate, "provoke no new theoretic vision" ("The Intellect's New Eye", 38). Relief of others' drudgery is a noble undertaking, but to slip from laudable service of that practical need to the notion that the computer is *for* drudgery is a profound error. It is an error that became an occupational hazard among early practitioners of humanities computing.

In 1978, literary scholar Susan Wittig paused to take stock of accomplishments in computing for her field. Quoting Masterman via an article promoting content analysis for literary study (Ellis and Favat), Wittig argued that Masterman's call for more than spadework had come to naught. Although the computer "*has* added immeasurably to the ability of literary analysis to perform better and more efficiently the same tasks that they have performed for many years" (her emphasis),

Wittig wrote, it has not "enlarged our range of vision or radically changed for us the shape of the universe of esthetic discourse" (211). The problem she identified was not the machinery; as Thomas Rommel has pointed out, the basic technical requirements for making a real difference had been met at least a decade before Wittig wrote (93). The problem she identified was the "limited conceptual framework" of the then dominant but ageing literary-critical theory, New Criticism, which along with structuralist-formalist grammar held, "first, the notion that the text is a linear entity; second, the idea that the text is a one-time, completed work, firmly confined to its graphic representation, the printed page; and third, the belief that the text is autonomously independent of any other entity, that it is meaningful in and of itself" (Wittig, 211–12). The force of these theoretical assumptions was to foreshorten the horizon of possibilities to what computers could then most easily do.

A dozen years earlier, literary scholar Louis Milic, also noting the great assistance provided to the old ways, had bemoaned the failing that Masterman indicated and that, we might say, lies behind the problem Wittig complained of: "Satisfaction with such limited objectives denotes a real shortage of imagination among us. We are still not thinking of the computer as anything but a myriad of clerks or assistants in one convenient console. Most of the results . . . could have been accomplished with the available means of half a century ago. We do not yet understand the true nature of the computer. And we have not yet begun to think in ways appropriate to the nature of this machine" (4). Fourteen years later, the situation had still not changed much. Summing up his experience and observations in research that had begun almost two decades earlier, Father Roberto Busa wrote with evident impatience (evincing the prevalence of the error) that the computer was not primarily a labor-saving device to be used to free scholars from drudgery but a means to illumine ignorance by provoking us to reconsider what we think we know (Busa, "The Annals of Humanities Computing"). Four years before that in "Why Can a Computer Do So Little?" he had surveyed the "explosion" of activities in "processing non-numerical, literary information" during the previous quarter century but noted the "rather poor performance" of computing as then conceived (1). Like Wittig and much like Jerome McGann at the beginning of the twenty-first century, Busa argued that this disappointing performance pointed to our ignorance of the focal subject—in this case, language: "what is in our mouth at every moment, the mysterious world of our words" ("Why Can a Computer Do So Little?," 3). Back to the theoretical drawing board (which was by then already filling up with very different ideas).

Masterman's vision of computing—her "telescope of the mind"—was not the only one nor the most ambitious. Best known is Herbert Simon's and Allen Newell's in 1958, phrased as a mixture of exuberant claims and startling predictions of what computers would, they said, be capable of doing within the following decade (Simon and Newell, "Heuristic Problem Solving"; cf. Simon and Newell, "Reply: Heuristic Problem Solving"). The gist of these Simon gave in a lecture in November

of the previous year, preceding and following them with these confident statements as they appear in his lecture note:

> IV. As of A.D. 1957 (even 1956) the essential steps have been taken to understand and simulate human judgmental heuristic activity. [. . .] Put it bluntly (hard now to shock)—Machines think! *Learn! Create!*
>
> V. What are the implications of this[3]

In *Alchemy and Artificial Intelligence* (1965) the philosopher Hubert Dreyfus famously took Simon and Newell to task for their pronouncements. But whatever our view of either, it is clear that by the mid-1960s signs of trouble for early visions were beginning to surface. The next year the Automatic Language Processing Advisory Committee of the U.S. National Research Council published *Language and Machines: Computers in Translation and Linguistics* (1966), a.k.a. the "black book" on machine translation, which effectively ended the lavish funding for the project (Wilks, *Grammar, Meaning and the Machine Analysis of Language*, 3–4). At the same time, however, the committee (much like Busa) recommended that efforts be redirected to research in the new field of computational linguistics "and should not be judged by any immediate or foreseeable contribution to practical translation" (ALPAC, 34). Machine translation was, they said, a research question, not a practical goal.

The like did not happen in the humanities, despite efforts such as John B. Smith's, for example, in "Computer Criticism" (1978, the year Wittig measured current achievements against Masterman's vision). More than ten years later Rosanne Potter, in her preface to a collection of papers that included a reprint of Smith's "Computer Criticism," wrote laconically that literary computing had "not been rejected, but rather neglected" by the profession (*Literary Computing and Literary Criticism: Theoretical and Practical Essays on Theme and Rhetoric,* xvi). Two years later, in her bibliographic survey of the first twenty-four years of *Computers and the Humanities*, she identified nine essays that, she wrote, "have attempted to reflect on what we are doing and why, where we are going and whether we want to go there" ("Statistical Analysis of Literature: A Retrospective on *Computers and the Humanities,* 1966–1990," 402). All of them, she noted, "warn against the same danger, seduction away from what we want to do by what the computer can do, call for the same remedy, more theory to guide empirical studies, and end with perorations about moving from the easy (data gathering) to the more creative (building new, more complex conceptual models)" ("Statistical Analysis of Literature: A Retrospective on *Computers and the Humanities*, 1966–1990," 402–3). She concluded that this was "as much self-reflection as the field was capable" ("Statistical Analysis of Literature: A Retrospective on *Computers and the Humanities,* 1966–1990," 403). And now?

In August of that year the World Wide Web was released to the public; and, as many have noted, everything changed for computing in the humanities, though

slowly at first. Also that year, Mark Olsen, presiding over the development of tools for one of the early large corpora, the *Trésor de la Langue Française*, at the American and French Research on the Treasury of the French Language project (ARTFL), shocked and even outraged many of those most closely involved with the field by arguing in an Modern Language Association (MLA) paper for what Franco Moretti has more recently called "distant reading." A special issue of *Computers and the Humanities*, centered on a revised version of that paper, was published two years later (*Computers and the Humanities* 27.5–6). In it, Olsen sounded the familiar sentence: "Computer-aided literature studies have failed to have a significant impact on the field as a whole" ("Signs, Symbols and Discourses: A New Direction for Computer-Aided Literature Studies," 309). Again, but as Yaacov Choueka said in somewhat different terms in 1988, "The tools are here, what about results?"[4]

So given the catalog of failings and disappointments that emerges from the complaints of practitioners, I ask the same question that architectural designer John Hamilton Frazer recently asked of once adventurous British computer art: "What went wrong?" (Brown et al., 50). This is not an idle question, for the digital humanities especially in regard of its strong tendency to define itself as serving client disciplines, which tend to initiate collaborations, set the agenda for the research and take academic credit for the result. As the popular metaphor of "text-mining," the focus on large infrastructural projects, and the preoccupation with standards suggest, anticipation of service to be rendered moves the field toward an industrial model, in which curiosity-motivated research is subordinated to large-scale production, better to facilitate research that happens elsewhere by other means. Big Science is cited as a precedent without anyone asking about the historically documented and prominently attested consequences for the affected sciences. But to answer this historical question properly for the disciplines most affected—those for which interpretation of cultural artefacts is the central activity—would require more than any of the surveys of the last three or more decades. I am convinced, but cannot yet demonstrate, that an adequate historical account could be written and that a genuine history of the digital humanities in its first half century would greatly help us turn pitiful laments and dull facts into the stimulating questions we should be asking now. To write such an account, however, an historian would have to locate practitioners' minority concerns within the broad cultural landscape of the time and then describe the complex pattern of confluence and divergence of numerous interrelated developments.[5] These practitioners were not working in a vacuum; it is trivial to demonstrate that they were well aware of what was going on elsewhere. Why did they react (or not) as they did?

My intention here is much more modest. I want to talk about what we can do meanwhile, reflectively, to address our own predicaments beyond simply recognizing them.

A start may be made with the manner in which we now express our worries. No doubt in response to the demands for accountability from funding agencies, we

have in recent years picked up the trendy phrase "evidence of value," thus asking how we might prove that money has been well spent.[6] We have, that is, shifted from the older argument for justification based on acceptance by our mainstream peers to a new one. What can we learn from it?

Roughly speaking the phrase "evidence of value" has migrated from legal disputes over property and the like to modern debates, for example, over the worth of public health care schemes (where it has become a buzzword and branded label). The question of value the phrase raises is a very old and persistent one that begins formally with ethics in the ancient world and continues today in philosophical arguments about whether affective states, such as feeling good or being excited about something, have anything to do with the value of that thing or whether a focus on evidence proves a dangerous trap. The eminently practical question of whether effort should continue to be spent in a particular way is sensible enough. There is nothing whatever wrong with it in the context of the purest, most wicked or curiosity-motivated research, for which you might say its constant presence is a necessary (though not sufficient) condition. But what do we accept as evidence for the worth or worthlessness of the effort, and who decides?

If funding agencies ask the question of whether research is worthwhile and judge the answer, then the effort is measured in funds spent, and evidence is defined as the "impact" of the research, in turn measured by citations to published work. For example, the rapporteur's report for a recent event at Cambridge, "Evidence of Value: ICT in the Arts and Humanities," begins thus: "With large sums of public money being channelled into this area, how is the 'value' of this investment assessed, what exactly are we assessing and for whom?"[7] Argument for qualitative as much as quantitative evidence was made, but what qualitative evidence might be other than claims supported by anecdote isn't clear. We can imagine a proper social scientific study of claimants' claims—how, for example, computing has changed their whole way of thinking—but would the results, however numerically expressed, be persuasive? Is any measure of "impact" critically persuasive for the humanities? To push the matter deeper, or further, are we not being naive to think that measurement simply establishes how things are in the world? Thomas Kuhn put paid to that notion for physics quite a long time ago (1961, the year before Masterman's visionary analogy).

In other words, it begins to look like the old philosophical argument, made by the consequentialists, carries the day: a preoccupation with evidence is mistaken; what matters, they say, are the consequences. We should ask, then, not where is the evidence of value. We should ask, instead, is computing fruitful for the humanities? What kinds of computing have been especially fruitful? In areas where it has not been, what's the problem? How can we fix it?

There is, of course, the practical concern with how to continue the research that we do (I don't ask whether) in the face of demands for evidence of value that often simply cannot be supplied without perverting it. If funding is contingent on providing this evidence, then the question becomes, what can we do without funding?

If funding is cut anyhow, as it has been for the humanities in the UK, then only the possibility of compromise is removed. What kinds of work can be done under the circumstances in which we find ourselves? Here is a debate we should be having, but it is not the debate I regard as most insistent, since what we can do on our own (which is really what we're left with primarily) is a matter for individual scholars to decide and find the cleverness to implement.

What lies beyond the let's-get-on-with-it scenario (where "it" has become one's own research made procedurally modest but as intellectually adventurous as can be) is the longer term question of how to improve the social circumstances of humanistic research. The question was debated briefly on the *Humanist* listserv from late October to early December 2010.[8] Here I return to a remark I reported there from the current UK science minister, David Willetts. Justifying the protected funding for the sciences, he noted that "the scientific community has assembled very powerful evidence such as in that Royal Society report, *The Scientific Century,* about what the benefits are for scientific research. Now you can argue that it's all worthwhile in its own rights, but the fact that it clearly contributes to the performance of the economy and the well-being of citizens—that's really strong evidence, and we deployed it."[9] Arguing for economic benefits is a long reach for the humanities, but "the well-being of citizens" is not. What can the digital humanities do for the humanities as a whole that helps these disciplines improve the well-being of us all?

And so I come to the debate I think we should be having.

We who have been working in the field know that the digital humanities can provide better resources for scholarship and better access to them. We know that in the process of designing and constructing these resources our collaborators often undergo significant growth in their understanding of digital tools and methods and that this sometimes, perhaps even in a significant majority of cases, fosters insight into the originating scholarly questions. Sometimes secular metanoia is not too strong a term to describe the experience. All this has for decades been the experience of those who guided collaborating scholars or were guided as scholars themselves through a gradual questioning of the original provocation to research, seeing it change as the struggle to render it computationally tractable progressed. In a sense, there is nothing new here to anyone who has ever attempted to get to the bottom of anything complex and ended up with, as Busa said, a mystery, something tacit, something that escapes the net. So not only is evidence of value to our collaborating colleagues thick on the ground, but it is also to be expected as a normal part of scholarship. But what about the argument? By definition evidence is information that backs up an argument. In other words, no argument, no evidence, only raw, uncommitted information.

The problem we have and must debate, then, is the argument or set of arguments that will convert decades of experience into (I believe, from a quarter century of it) incontrovertible evidence of *intellectual* value. We've seen and, I hope, are by now convinced that all computing in the humanities is not *for* drudgery even as it

becomes more and more difficult, through ever-multiplying layers of software powered by ever-better hardware, to see what goes on behind the friendly service our devices provide. Some computing is designed to relieve us of drudgery. But to go back to Turing's scheme for indefinitely many forms of computing, whose number is limited only by the human imagination, what is computing in and of the humanities *for*? Are we for drudgery? If not, with regards to the humanities, what are we *for*?

NOTES

1. Freeing the Mind was first published as a series of essays in the *Times Literary Supplement* from March 23 to May 4, 1962, then republished as a slim volume together with selected letters to the editor later that year. It provides an excellent snapshot of nontechnical reflection on and about computing, as was characteristic of the *Times Literary Supplement* during the 1960s and 1970s.

2. As Yorick Wilks says in his biographical tribute to her, Masterman was "ahead of her time by some twenty years . . . never able to lay adequate claim to [ideas now in the common stock of artificial intelligence and machine translation] because they were unacceptable when she published them," making efforts "to tackle fundamental problems with computers . . . that had the capacity of a modern digital wristwatch," producing and inspiring numerous publications that today seem "curiously modern" (Wilks, *Language, Cohesion and Form*, 1, 4). For her work with haiku, see Masterman and McKinnon Wood, and Masterman "Computerized Haiku"; for vitriolic opposition to it see Leavis. For an idea of the diverse company with which her work associated her, see the table of contents in Reichardt's *Cybernetics, Art and Ideas*. Art critic Jasia Reichardt was responsible for the landmark *Cybernetic Serendipity* exhibition in London, August to October, 1968 (Reichardt, *Cybernetic Serendipity*). Among the exhibitors was "mechanic philosopher" and inventor of visionary "maverick machines" Gordon Pask, who was a long-time friend and research partner of Robert McKinnon Wood, Masterman's colleague at Cambridge; for more on Pask, see Bird and Di Paolo.

3. An image of the original manuscript upon which this transcription was based may be found at http://www.mccarty.org.uk/essays/McCarty,%20Telescope.pdf.

4. At the 1988 Association for Literary and Linguistic Computing Conference in Jerusalem, Choueka assigned me to the panel "Literary and Linguistic Computing: The Tools Are Here, What about Results?" The title was his. See http.sigir.org/sigirlist/issues/1988/88-4-28.

5. My historiography owes a great deal to the late Michael S. Mahoney; see the collection of his papers and the editor Thomas Haigh's discussion in Mahoney; cf. McCarty.

6. For "evidence of value" in the digital humanities, see subsequent sections in this chapter and www.crassh.cam.ac.uk/events/196/. The AHRC ICT Methods Network, under which "evidence of value" was the subject of an expert seminar, has concluded its work. Otherwise, a search of the web will turn up thousands of examples of its use in other contexts.

7. Wilson; see also www.crassh.cam.ac.uk/events/196/, and Hughes.

8. See *Humanist* 24.427–8, 431, 436 (http://www.digitalhumanities.org/humanist/, with reference to a British Academy lecture by Martha Nussbaum), 440, 445, 448, 453, 455, 464, 469, 479, 481, 483, 485, 504, 511, 515, 527, 541. As is typical with online discussions, a particular thread remains distinct for a time then begins to unravel into related matters. This one remained coherent for quite some time.

9. "The Material World," BBC Radio 4, October 21, 2010, my transcription. For the Royal Society report, see royalsociety.org/the-scientific-century/.

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Sunset for Ideology, Sunrise for Methodology?

TOM SCHEINFELDT

Sometimes friends in other disciplines ask me, "So what are the big ideas in history these days?" I then proceed to fumble around for a few minutes trying to put my finger on some new ism or competing isms to describe and define today's historical discourse. Invariably, I come up short.

Growing up in the second half of the twentieth century, we are prone to think about our world in terms of ideologies and our work in terms of theories. Late twentieth-century historical discourse was dominated by a succession of ideas and theoretical frameworks. This mirrored the broader cultural and political discourse in which our work was set. For most of the last seventy-five years of the twentieth century, socialism, fascism, existentialism, structuralism, poststructuralism, conservatism, and other ideologies vied with one another broadly in our politics and narrowly at our academic conferences.

But it wasn't always so. Late nineteenth- and early twentieth-century scholarship was dominated not by big ideas but by methodological refinement and disciplinary consolidation. Denigrated in the later twentieth century as unworthy of serious attention by scholars, the nineteenth and early twentieth century, by contrast, took activities like philology, lexicology, and especially bibliography very seriously. Serious scholarship was concerned as much with organizing knowledge as it was with framing knowledge in a theoretical or ideological construct.

Take my subdiscipline, the history of science, as an example. Whereas the last few decades of research have been dominated by a debate over the relative merits of constructivism (the idea, in Jan Golinski's succinct definition, "that scientific knowledge is a human creation, made with available material and cultural resources, rather than simply the revelation of a natural order that is pre-given and independent of human action"), the history of science was in fact founded in an outpouring of bibliography.[1] The life work of the first great American historian of science, George Sarton, was not an idea but a journal (*Isis*), a professional society (the History of

Science Society), a department (Harvard's), a primer (his *Introduction to the History of Science*), and especially a bibliography (the *Isis Cumulative Bibliography*).[2] Tellingly, the great work of his greatest pupil, Robert K. Merton, was an idea: the younger Merton's "Science, Technology and Society in Seventeenth Century England" defined history of technology as social history for a generation.[3] By the time Merton was writing in the 1930s, the cultural climate had changed and the consolidating and methodological activities of the teacher were giving way to the theoretical activities of the student.

I believe we are at a similar moment of change right now that we are entering a new phase of scholarship that will be dominated not by ideas but once again by organizing activities, in terms of both organizing knowledge and organizing ourselves and our work. My difficulty in answering the question, "What's the big idea in history right now?" stems from the fact that, as a digital historian, I traffic much less in new theories than in new methods. The new technology of the Internet has shifted the work of a rapidly growing number of scholars away from thinking big thoughts to forging new tools, methods, materials, techniques, and modes or work that will enable us to harness the still unwieldy, but obviously game-changing, information technologies now sitting on our desktops and in our pockets. These concerns touch all scholars. The Center for History and New Media's Zotero research management tool is used by more than a million people, all of them grappling with the problem of information overload. And although much of the discussion remains informal, it's no accident that Wikipedia is right now one of the hottest topics for debate among scholars.

Perhaps most telling is the excitement that now (or, really, once again) surrounds the library. The buzz among librarians these days dwarfs anything I have seen in my entire career among historians. The terms "library geek" and "sexy librarian" have gained new currency as everyone begins to recognize the potential of exciting library-centered projects like Google Books.

All these things—collaborative encyclopedism, tool building, librarianship—fit uneasily into the standards of scholarship forged in the second half of the twentieth century. Most committees for promotion and tenure, for example, must value single authorship and the big idea more highly than collaborative work and methodological or disciplinary contribution. Even historians find it hard to internalize the fact that their own norms and values have and will again change over time. But change they must. In the days of George Sarton, a thorough bibliography was an achievement worthy of great respect and an office closer to the reference desk in the library an occasion for great celebration. (Sarton's small suite in study 189 of Harvard's Widener Library was the epicenter of history of science in America for more than a quarter century.) As we tumble deeper into the Internet age, I suspect it will be again.

NOTES

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1. Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science* (Cambridge, UK: Cambridge University Press, 1998), 6.

2. George Sarton, *Introduction to the History of Science* (Washington, D.C.: Carnegie Institution of Washington, 1962); *History of Science Society, Isis Cumulative Bibliography: A Bibliography of the History of Science Formed from Isis Critical Bibliographies 1–90, 1913–65* (London: Mansell, 1971).

3. Robert K. Merton, "Science, Technology and Society in Seventeenth Century England," *Osiris* 4 (January 1, 1938): 360–632.

Has Critical Theory Run Out of Time for Data-Driven Scholarship?

GARY HALL

Certainly, something that is particularly noticeable about many instances of this turn to data-driven scholarship—especially after decades when the humanities have been heavily marked by a variety of critical theories (Marxist, psychoanalytic, postcolonialist, post-Marxist)—is just how difficult they find it to understand computing and the digital as much *more* than tools, techniques, and resources and thus how naive and lacking in meaningful critique they often are (Liu; Higgen). Of course, this (at times explicit) repudiation of criticality could be viewed as part of what makes certain aspects of the digital humanities so intriguing at the moment. From this perspective, exponents of the computational turn are precisely *not* making what I have elsewhere characterized as the antipolitical gesture of conforming to accepted (and often moralistic) conceptions of politics that have been decided in advance, including those that see it only in terms of power, ideology, race, gender, class, sexuality, ecology, affect, and so forth (Hall, *Digitize*). Refusing to "go through the motions of a critical avant-garde," to borrow the words of Bruno Latour, they are responding to what is perceived as a fundamentally new cultural situation and the challenge it represents to our traditional methods of studying culture by avoiding such conventional gestures and experimenting with the development of fresh methods and approaches for the humanities instead.[1]

In a series of posts on his *Found History* blog, Tom Scheinfeldt, managing director at the Center for History and New Media at George Mason University, positions such scholarship very much in terms of a shift from a concern with theory and ideology to a concern with methodology:

> I believe . . . we are entering a new phase of scholarship that will be dominated not by ideas, but once again by organizing activities, both in terms of organizing knowledge and organizing ourselves and our work . . . as a digital historian, I traffic much less in new theories than in new methods. The new technology of

> the Internet has shifted the work of a rapidly growing number of scholars away from thinking big thoughts to forging new tools, methods, materials, techniques, and modes or work which will enable us to harness the still unwieldy, but obviously game-changing, information technologies now sitting on our desktops and in our pockets. (Scheinfeldt, "Sunset")

In this respect there may well be a degree of "relief in having escaped the culture wars of the 1980s"—for those in the United States especially—as a result of this move "into the space of methodological work" (Higgen) and what Scheinfeldt reportedly dubs "the post-theoretical age" (cited in Cohen, "Digital Keys"). The problem is, though, without such reflexive critical thinking and theories many of those whose work forms part of this computational turn find it difficult to articulate exactly what the point of what they are doing is, as Scheinfeldt readily acknowledges ("Where's the Beef?").

Witness one of the projects I mentioned earlier: the attempt by Dan Cohen and Fred Gibbs to text mine all the books published in English in the Victorian age (or at least those digitized by Google).[2] Among other things, this allows Cohen and Gibbs to show that use of the word "revolution" in book titles of the period spiked around "the French Revolution and the revolutions of 1848" (Cohen, "Searching"). But what argument is it that they are trying to make with this? How exactly is the number of times a word does or does not occur significant? What is it we are able to learn as a result of this use of computational power on their part that we didn't know already and couldn't have discovered without it (Scheinfeldt, "Where's the Beef")?

Elsewhere, in an explicit response to Cohen and Gibbs's project, Scheinfeldt suggests that the problem of theory, or the lack of it, may actually be a matter of scale and timing:

> It expects something of the scale of humanities scholarship which I'm not sure is true anymore: that a single scholar—nay, every scholar—working alone will, over the course of his or her lifetime . . . make a fundamental theoretical advance to the field.
>
> Increasingly, this expectation is something peculiar to the humanities. . . . it required the work of a generation of mathematicians and observational astronomers, gainfully employed, to enable the eventual "discovery" of Neptune . . . Since the scientific revolution, most theoretical advances play out over generations, not single careers. We don't expect all of our physics graduate students to make fundamental theoretical breakthroughs or claims about the nature of quantum mechanics, for example. There is just too much lab work to be done and data to analyzed for each person to be pointed at the end point. That work is valued for the incremental contribution to the generational research agenda that it is. (Scheinfeldt, "Response")

Yet notice how theory is again being marginalized in favour of an emphasis on STEM subjects and the adoption of expectations and approaches associated with mathematicians and astronomers in particular.

This is not to deny the importance of experimenting with the new kinds of knowledge, tools, methods, materials, and modes of working and thinking that digital media technologies create and make possible, including those drawn from computer science, in order to bring new forms of Foucauldian *dispositifs*, or what Bernard Stiegler calls *hypomnemata* (i.e., mnemonics, what Plato referred to as *pharmaka*, both poisons and cures), or what I am trying to think in terms of media gifts into play.[3] And I would potentially include in this process of experimentation techniques and methodologies drawn from computer science and other related fields such as information visualization, data mining, and so forth. Yes, of course, it is quite possible that as Daniel W. Stowell, director of the Papers of Abraham Lincoln project at the Illinois Historic Preservation Society puts it, in the future "people will use this data in ways we can't even imagine yet," both singularly and collaboratively (cited in Cohen, "Digital Keys"). Still, there is something intriguing about the way in which many defenders of the turn toward computational tools and methods in the humanities evoke a sense of time in relation to theory.

Take the argument—one I have heard put forward at a number of different events now—that critical and self-reflexive theoretical questions about the use of digital tools and data-led methodologies should be deferred for the time being, lest they have the effect of strangling at birth what could turn out to be a very different form of humanities research before it has had a chance to properly develop and take shape. Viewed in isolation, it can be difficult, if not impossible, to decide whether this particular form of "limitless" postponement (Deleuze, 5) is serving as an alibi for a naive and rather superficial form of scholarship (Meeks) or whether it is indeed acting as a responsible, political or ethical opening to the (heterogeneity and incalculability of the) future, including the future of the humanities. After all, the suggestion is that now is *not the right time* to be making any such decision or judgment, since we cannot *yet* know how humanists will *eventually* come to use these tools and data and thus what data-driven scholarship may or may not turn out to be capable of critically, politically, theoretically.

This argument would be more convincing as a responsible political or ethical call to leave the question of the use of digital tools and data-led methodologies in the humanities open if it were the only sense in which time was evoked in relation to theory in this context. Significantly, it is not. As we have seen, advocates for the computational turn do so in a number of other and often competing senses, too. These include the following:

1. That the time *of* theory is over, in the sense a particular historical period or moment has now ended (e.g., that of the culture wars of the 1980s)

2. That the time *for* theory is over, in the sense it is now the time for methodology
3. That the time to return to theory, or for theory to (re-)emerge in some new, unpredictable form that represents a fundamental breakthrough or advance, although possibly on its way, has not arrived yet and cannot necessarily be expected to do so for some time given that "most theoretical advances play out over generations" (Scheinfeldt, "Response")

All of this gives a very different inflection to the view of theoretical critique as being at best inappropriate and at worst harmful to data-driven scholarship. Even a brief glance at the history of theory's reception in the English-speaking world is sometimes enough to reveal that those who announce its time has not yet come, or is already over, that theory is in decline or even dead and that we now live in a posttheoretical world, are more often than not endeavoring to keep it at a temporal distance. Positioning their own work as being either pre- or posttheory in this way in effect gives them permission to continue with their preferred techniques and methodologies for studying culture relatively uncontested (rather than having to ask rigorous, critical and self-reflexive questions about their practices and their justifications for them). Placed in this wider context, far from helping to keep the question concerning the use of digital tools and data-led methodologies in the humanities open (or having anything particularly interesting to say about theory), the rejection of critical-theoretical ideas as untimely can be seen as both moralizing and conservative.

In saying this I am reiterating an argument initially made by Wendy Brown in the sphere of political theory. Yet can a similar case not be made with regard to the computational turn in the humanities to the effect that the "rebuff of critical theory as untimely provides the core matter for the affirmative case for it"?[4] Theory is vital from this point of view, not for conforming to accepted conceptions of political critique that see it primarily in terms of power, ideology, race, gender, class, sexuality, ecology, affect, and so forth or for sustaining conventional methods of studying culture that may no longer be appropriate to the networked nature of twenty-first century postindustrial society. Theory is vital "to contest the very sense of time invoked to declare critique untimely" (Brown, 4).

NOTES

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1. This is one explanation as to why many exponents of the computational turn appear to display such little awareness of the research of "critical media scholars (like Matthew Fuller, Wendy Chun, McKenzie Wark, and many others) and hacker activists of the

past decade; research that has shown again and again how these very formalisms [i.e. 'the "quantitative" formalisms of databases and programming'] are 'qualitative,' i.e. designed by human groups and shaped by cultural, economical and political interests through and through" (Cramer). Liu encapsulates the situation as follows: "In the digital humanities, cultural criticism—in both its interpretive and advocacy modes—has been noticeably absent by comparison with the mainstream humanities or, even more strikingly, with 'new media studies' (populated as the latter is by net critics, tactical media critics, hacktivists, and so on). We digital humanists develop tools, data, metadata, and archives critically; and we have also developed critical positions on the nature of such resources (e.g., disputing whether computational methods are best used for truth-finding or, as Lisa Samuels and Jerome McGann put it, 'deformation'). But rarely do we extend the issues involved into the register of society, economics, politics, or culture in the vintage manner, for instance, of the Computer Professionals for Social Responsibility (CPSR). How the digital humanities advance, channel, or resist the great postindustrial, neoliberal, corporatist, and globalist flows of information-cum-capital, for instance, is a question rarely heard in the digital humanities associations, conferences, journals, and projects with which I am familiar. Not even the clichéd forms of such issues—e.g., 'the digital divide,' 'privacy,' 'copyright,' and so on—get much play."

2. See http://victorianbooks.org.

3. See http://garyhall.info.

4. Lest this aspect of my analysis appear somewhat unfair, I should stress that the ongoing discussion over how the digital humanities are to be defined and understood does feature a number of critics of the turn toward techniques and methodologies derived from computer science who have made a case for the continuing importance of the traditional, theoretically informed humanities. See, in their different ways, not just Higgen and Liu as referenced above but also Drucker and Fitzpatrick. For an analysis that draws attention to some of the elements of misrecognition that are in turn to be found in such a traditional, theoretically informed humanism, see my "On the Limits of Openness: Cultural Analytics and the Computational Turn in the Digital Humanities" (unpublished manuscript), especially the conclusion, and also Hall, "The Digital Humanities Beyond Computing: A Postscript."

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There Are No Digital Humanities

GARY HALL

Building on the work of Jean-François Lyotard and Gilles Deleuze in *The Postmodern Condition* and "Postscript on Societies of Control," respectively, let us pursue a little further the hypothesis that the externalization of knowledge onto computers, databases, and more recently mobile media environments, networked servers, and the cloud is involved in the constitution of a different form of society and human subject. To what extent do such developments cast the so-called computational turn in the humanities in a rather different light to the celebratory data fetishism that has come to dominate this rapidly emerging field? Is the direct, practical use of techniques and methodologies drawn from computer science and various fields related to it here, too, helping to produce a major alteration in the status and nature of knowledge and indeed the human subject? I'm thinking not just of the use of tools such as Anthologize, Delicious, Juxta, Mendeley, Pliny, Prezi, and Zotero to structure and disseminate scholarship and learning in the humanities. I also have in mind the generation of dynamic maps of large humanities data sets and employment of algorithmic techniques to search for and identify patterns in literary, cultural, and filmic texts as well as the way in which the interactive nature of much digital technology is enabling user data regarding people's creative activities with this media to be captured, mined, and analyzed by humanities scholars.

To be sure, in what seems to be almost the reverse of the situation Lyotard describes in *The Postmodern Condition*,[1] many of those in the humanities—and this includes some of the field's most radical thinkers—*do* now appear to be looking increasingly to science (*and* technology *and* mathematics), if not necessarily computer science specifically, to provide their research with a degree of legitimacy. Witness Franco "Bifo" Berardi's appeal to "the history of modern chemistry on the one hand, and the most recent cognitive theories on the other" (121) for confirmation of the compositionist philosophical hypothesis in his book *The Soul at Work*: "There is no object, no existent, and no person: only aggregates, temporary atomic compositions, figures that the human eye perceives as stable but that are indeed mutational, transient, frayed and indefinable" (120). It is this hypothesis, derived

from Democritus, that Bifo sees as underpinning the methods of both the schizo-analysis of Deleuze and Guattari and the Italian Autonomist Theory on which his own compositionist philosophy is based. Can this turn toward the sciences (if there has indeed been such a turn, a question that is worthy of further examination) be regarded as a response on the part of the humanities to the perceived lack of credibility, if not obsolescence, of *their* metanarratives of legitimation: the life of the spirit and the Enlightenment but also Marxism, psychoanalysis, and so forth? Indeed, are the sciences today to be regarded as answering many humanities questions more convincingly than the humanities themselves?

While ideas of this kind are perhaps a little bit too neat and symmetrical to be entirely convincing, this "scientific turn" in the humanities *has* been attributed by some to a crisis of confidence. It is a crisis brought about, if not by the lack of credibility of the humanities' metanarratives of legitimation exactly then at least in part by the "imperious attitude" of the sciences. This attitude has led the latter to colonize the humanists' space in the form of biomedicine, neuroscience, theories of cognition, and so on (Kagan, 227).[2] Is the turn toward computing just the latest manifestation of and response to this crisis of confidence in the humanities? Can we go even further and ask, is it evidence that certain parts of the humanities are attempting to increase *their* connection to society[3] and to the instrumentality and functionality of society especially? Can it be merely a coincidence that such a turn toward computing is gaining momentum at a time when the UK government is emphasizing the importance of the STEMs (Science, Technology, Engineering and Mathematics) and withdrawing support and funding for the humanities? Or is one of the reasons all this is happening now due to the fact that the humanities, like the sciences themselves, are under pressure from government, business, management, industry, and increasingly the media to prove they provide value for money in instrumental, functional, performative terms? Is the interest in computing a strategic decision on the part of some of those in the humanities? As Dan Cohen and Fred Gibbs's project to text mine "the 1,681,161 books that were published in English in the UK in the long nineteenth century" shows, one can get funding from the likes of Google (Cohen, "Searching"). In fact, in the summer of 2010 "Google awarded $1 million to professors doing digital humanities research" (Cohen, "Digital Keys"; see also Orwant).

To what extent, then, is the take up of practical techniques and approaches from computing science providing some areas of the humanities with a means of defending (and refreshing) themselves in an era of global economic crisis and severe cuts to higher education, through the transformation of their knowledge and learning into quantities of information—deliverables? Can we even position the computational turn as an event created to justify such a move on the part of certain elements within the humanities (Frabetti)? And does this mean that, if we don't simply want to go along with the current movement *away* from what remains resistant to a general culture of measurement and calculation and *toward* a concern to legitimate power and control by optimizing the system's efficiency, we would be better

off using a different term than "digital humanities"? After all, the idea of a computational turn implies that the humanities, thanks to the development of a new generation of powerful computers and digital tools, have somehow *become* digital, or are in the process of *becoming* digital, or are at least coming to terms with the digital and computing (Frabetti). Yet one of the things I am attempting to show by drawing on the thought of Lyotard, Deleuze, and others is that the digital is not something that can now be *added to* the humanities—for the simple reason that the (supposedly predigital) humanities can be seen to have *already had* an understanding of and engagement with computing and the digital.

NOTES

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1. In *The Postmodern Condition*, Jean-François Lyotard showed how science, lacking the resources to legitimate itself as true, had since its beginnings with Plato relied for its legitimacy on precisely the kind of knowledge it did not even consider to be knowledge: nonscientific narrative knowledge. Specifically, science legitimated itself by producing a discourse called philosophy. It was philosophy's role to generate a discourse of legitimation for science. Lyotard proceeded to define as modern any science that legitimated itself in this way by means of a metadiscourse that explicitly appealed to a grand narrative of some sort: the life of the spirit, the Enlightenment, progress, modernity, the emancipation of humanity, the realization of the Idea.

2. Interestingly, for Kagan, "The scientists' intrusions into the philosophers' territory, which robbed the latter of part of their mission, forced them to find another assignment and many selected analyses of the coherence of the scientists' semantic texts" (Kagan, 228).

3. As Kirschenbaum writes, "Whatever else it might be then, the digital humanities today is about a scholarship (and a pedagogy) that is publicly visible in ways to which we are generally unaccustomed, a scholarship and pedagogy that are bound up with infrastructure in ways that are deeper and more explicit than we are generally accustomed to, a scholarship and pedagogy that are collaborative and depend on networks of people and that live an active 24/7 life online. Isn't that something you want in your English department?"

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