A telescope for the mind?

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> 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* 2

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 17th 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 realised, or that demonstrable progress has been made. This interrogation for "evidence of value"--a trendy phrase we seem recently to have picked up--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.[2] 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 17th-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". She suggests that by thus extending our perceptual scope and reach, computing does not simply bring formerly unknown things into view, it 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 *haiku* 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.[3]

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" (38),

> – 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.

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 17th Century, Charles Babbage in the 19th and Herman Goldstine in the 20th (Goldstine 8-12; Pratt 20-44). All three responded by devising computational machinery. We certainly cannot and should not deny the crippling effects to which they all attested. But, Masterman insisted, these spade-work uses, however welcome the time and effort they liberate, "provoke no new theoretic vision" (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 a occupational hazard among early practitioners of humanities computing.

In 1978 literary scholar Susan Wittig stopped 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 spade-work 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", Wittig wrote, it has not "enlarged our range of vision or radically changed for us the shape of the universe of esthetic discourse" (211, her emphasis). 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 (Rommel 93). The problem she identified was the dominant theory of text that had imprinted her literary colleagues: positivistic (and by then senescent) New Criticism.

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 indicated:

> 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, Fr Roberto Busa wrote with evident impatience (evincing the prevalence of the error) that the computer was not primarily a labour-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 1980). 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. Like Wittig, and much like Jerome McGann at the beginning of the 21st 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" (3). Back to the theoretical drawing-board! (which was by then already filling up with very different ideas).

Masterman's vision of computing 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 1958a; cf. 1958b). The gist of these Simon gave in a lecture in November of the previous year, preceding and following them with the confident statements shown below as they appear in his lecture note (Simon 1957):
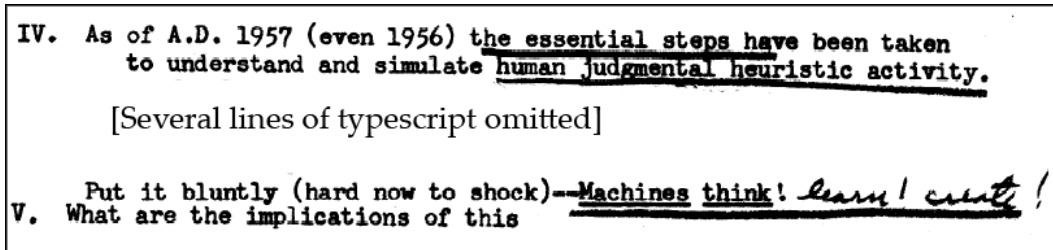
Figure 1

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 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 10 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 (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" (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).

She concluded that this was "as much self-reflection as the field was capable" (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, even outraged many of those most closely involved with the field by arguing in an MLA paper for what Franco Moretti has more recently called "distant reading". A special issue of *Computers and the Humanities*, centred on a revised version of that paper, was published two years later (*Chum* 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" (309). Again, but as Yaacov Choueka said in somewhat different terms in 1988, "The tools are here, what about results?"[4]

So we ask what 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 industrialise its own research better to serve its client disciplines' immediate demands. 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. What is in this search for "evidence of value" in the digital humanities that would help? Why are we asking this particular form of the old question?

Roughly speaking the trendy phrase has migrated from legal disputes over property and the like to modern debates, for example over the worth of public healthcare schemes (where it has become a buzzword and branded label). The question of value it 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 internal 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 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?"[6] 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, 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 (I don't ask whether) in the face of demands for evidence of value. If funding is contingent on providing it, then the question becomes, what can we do without funding? If funding is cut anyhow, as it has for the humanities in the U.K., 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 *Humanist* from late October to early December 2010.[7] Here I return to a remark I reported there from the current U.K.

Science Minister, David Willitts. 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.[8]

Arguing for economic benefits is a long reach for the humanities, but "the well-being of citizens" is not. What can the digital humanities can 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 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. Evidence seems thick on the ground. So, you might ask in honour of Choueka, the evidence of value is here, but what about the argument? The immediate answer is one of definition: evidence is information that backs up an argument. In other words, no argument, no evidence. Only raw, uncommitted information. Just as Kuhn holds for scientific measurement, more often than not the fact or point you're looking to determine profoundly affects what you find to support it. Obviously it would be as much a mistake to argue that argument or theory must come first as to say that information should. But concern for that which you hope to establish, it would seem, needs to be there at the beginning, getting clearer or changing into something better that it has concealed--something that in a sense was there all the way along.

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 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*?

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[1] *Freeing the Mind* was first published as a series of essays in the *Times Literary Supplement* from 23

[2] For "evidence of value" in the digital humanities, see below and www.crassh.cam.ac.uk/events/196/. The AHRC ICT Methods Network, under which it was the subject of an Expert Seminar, as concluded its work; see www.methodsnetwork.ac.uk/activities/core-activities.html (20 April 2011). Otherwise a search of the Web will turn up ca 500K examples of its use in other contexts.

[3] 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" (1, 4). For her work with *haiku*, see Masterman and McKinnon Wood and Masterman 1971; 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. Art critic Reichardt was responsible for the landmark *Cybernetic Serendipity* exhibition in London, August to October 1968 (Reichardt 1969). Among the exhibitors was "mechanic philosopher" and inventor of visionary "maverick machines" Gordon Pask, who was a long-time friend and research partner of McKinnon Wood, Masterman's colleague at Cambridge; for Pask see Bird and Di Paolo.

[4] At the 1988 ALLC 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 www.sigir.org/sigirlist/issues/1988/88-4-28 (9/2/2011).

[5] My historiography owes a great deal to the late Michael S. Mahoney; see the forthcoming collection of papers and the editor Thomas Haigh's discussion (Mahoney 2011); cf. McCarty 2011.

[6] Wilson; see also www.crassh.cam.ac.uk/events/196/ (11 February 2011); Hughes.

[7] See *Humanist* 24.427-8, 431, 436 (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.

[8] "The Material World", BBC Radio 4, 21 October 2010 (my transcription). For the Royal Society report, see royalsociety.org/the-scientific-century/ (9/2/2011).