CHAPTER THIRTY-FIVE

The Place of Computation in the Study of Culture

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HERMENEUTIC AND EMPIRICAL CULTURES OF KNOWLEDGE PRODUCTION

In the course of a somewhat weary trek through more departments and disciplines than I ever intended, it gradually dawned on me that, while there isn't really any difference between that which is generally called "Social Science" and that which is sometimes called "the Humanities" and sometimes "the Arts," there is a very real distinction to be drawn between two cultures of knowledge production, each of which is to be found in both of the aforementioned. There is a place for computation—that is, for mathematical calculations, executed with or without the assistance of a machine—in one of these cultures, but in the other, there is not—and I submit that wider acknowledgment of this point might be of help to that which refers to itself as "the Digital Humanities," as its relationship to the rest of the academy continues to develop and evolve. In order to draw a line between my argument and that of C. P. Snow (1959), I shall refer to the two cultures that I have in mind as the "empirical" and the "hermeneutic."

Under the empirical culture of knowledge production, one answers research questions that are fundamentally *about the world*. Some (although not all) such questions are susceptible to approach through the use of what are conventionally known as "quantitative methods"—which is to say, through the use of methods wherein the analytic stage involves computation. However, under the hermeneutic culture of knowledge production, one answers research questions that are, by contrast, fundamentally about *meaning*. Although there have been attempts to recast hermeneutic questions in apparently objective terms—most famously, the intentionalist argument that the only meaning that can validly be ascribed to a text is the meaning that its author demonstrably intended (see especially Hirsch 1967), and the structuralist argument that the meaning of a text is a function of its relationship to a system that can potentially be described in its entirety (see above all Culler 1975)—it remains the case that the only measure of success for an interpretation of *Hamlet*, the *Theses on Feuerbach*, or the Sermon on the Mount is its intersubjective acceptability for an audience of the interpreter's peers. And computation has nothing to contribute under such a paradigm—unless we mean those computations which go on, unnoticed, in the background, incessantly, so that emails can take the place of the postal service and a word processor can take the place of a typewriter. But almost nobody cares about those.

That's just the way it is, and I'm not going to waste anybody's time by trying to change it. I'm not talking about a revolution.

QUANTIFICATION AND THE HERMENEUTIC CULTURE OF KNOWLEDGE PRODUCTION

Both cultures are to be found across the contemporary university. However, there are some disciplines—such as literature, philosophy, and theology—which are *essentially* hermeneutic. These may have adjunct empirical disciplines, such as the history of books, the history of concepts, and the history of religions, and the august figures who rule over the hermeneutic parent disciplines may sometimes allow representatives of these quasi-legitimate offspring to hold posts in their departments, but, at their core, those departments nonetheless continue to operate by rules alien to the empirical culture of knowledge production. This is to say that, by design, the critic, philosopher, or theologian addresses questions which are not susceptible to computation; moreover, he or she addresses them in relation to objects few enough in number to make computation redundant. If the scope of one's research is Sonnet 116 or a fragment of *On Nature* or a hadith, then qualitative methods are sufficient because each of those objects is unique. Hermeneutic scholars may write of relatively large classes of objects, such as "Elizabethan poetry," or "pre-Socratic philosophy," or "the sayings of the Prophet." But one of the key things taught to undergraduate students of literature, philosophy, and theology is to avoid making generalizations that would be impossible to support through careful deployment of quotations from irreducibly unique individual texts.

All this is fine. But the value and interest of quantitative methods lies in their potential to generate evidence to support—or, depending on the outcome, to *refute*—precisely those kinds of generalizations that students of essentially hermeneutic disciplines are trained to avoid. Quantification is inherently a form of generalization. In order to count something, one must first adopt the assumption that all examples of that thing can be treated as equivalent—at least from the point of view of the count. This involves a sort of anonymization of experience, treating everything that is to be counted as an instance of a type.

Types do not have to be reified, and may of course be adopted only provisionally, for the purposes of a single research question. But, despite this, it remains true that to think in such terms goes against every instinct of the hermeneutic scholar. A major part of disciplinary training in literary studies, art history, film studies, media studies, cultural studies, and multiple forms of qualitative social science consists in learning to see everything as unique, and in learning to focus most closely on that which distinguishes any given object or occurrence from all else that might be compared to it. The same word may have very different connotations in two different sentences. The same sentence may have very different implications in two different conversations, and even in two different parts of the same conversation. But if everything is unique, then everything is its own category, and every category has already been counted, and the total for every category is 1.

This is not, I must emphasize, a matter of the humanities versus the social sciences. The following, for example, appears in a sociology professor's response to an article promoting certain forms of quantitative analysis:

> Pierre Bourdieu in *Distinction* inadvertently confirms how quantification relaxes testing … As we know, Bourdieu compares the frequency with which persons agree that a sunset over the sea is suitable material for a beautiful photograph …. Persons with low occupational and educational rankings are more likely to affirm this subject matter as promising than are persons

> with high occupational and educational rankings. Supposedly this result confirms that persons with more education deploy a distinctive habitus by which they distance themselves from the trite and commonplace. The numbers confirm a differential, to be sure, but … [b]y formalising people's aesthetic appreciation into countable agree/disagree results, Bourdieu made it facile to confirm his hypothesis.
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> (Biernacki 2015, 343)

It's important to recognize that the above is not simply a critique of Bourdieu, whose theories are controversial even among those who do not reject his methods. It is, rather, a dismissal of the very idea of quantification, and it forms part of an assertion of the superiority of hermeneutics over empiricism: elsewhere in the same essay, it is asserted that "[h]umanistic method preserves arcane detail because none of it in a text is automatically privileged, which is to say, [because] almost any of it can become a cornerstone thanks to shifting the frame of reference" and that "[t]o maximise the potential of 'surprise' for theorising … we must preserve the minutia as pivots of interpretation and therefore as source points for competing guesses" (2015, 342). Although made in this case by a sociologist, the same points could have been made in the same way by many a literary scholar, outraged at some of the stronger claims which have been made for the Digital Humanities. And those points are, moreover, completely valid: every cultural production and every experience *is* unique, if you only look at it closely enough, and so is every historical event, and much is lost along with the "arcane detail" that must be generalized away before computation can begin.

And yet to admit empiricism to a discipline is to open up a space within it for quantification.

QUANTIFICATION AND THE EMPIRICAL CULTURE OF KNOWLEDGE PRODUCTION

To quantify means to establish the numerical quantity of—which is to say, to measure or to count, or simply to express as a number. Quantification, then, involves temporarily laying aside the knowledge that everything is unique, and instead provisionally adopting the contrary assumption that it may be—for particular purposes—useful to treat particular things as *generic*. This is the opposite of what one learns to do when one learns to read poetry and scripture, and also of what one learns to do when one learns to read movies and videogames as if they were poetry and scripture. By analogy with close reading, quantitative methods have sometimes been sold to humanities scholars as "distant reading" (e.g., Moretti 2013)—which is a clever piece of marketing, but ultimately rather misleading, because quantification is not reading at all. Reading involves grasping the particularities of some text as a whole, and this is the case even when one does *not* read closely—for example, when leafing through a tedious novel to find out how it ends, or when skimming an academic article in order to pick out the major findings or to discern the overall argument.

By contrast, as Stephen Stigler (2016) has argued, statistics—the science of numerical analysis—is effectively founded on the realization that the informativeness of data is increased through aggregation, even though (and in fact, *because*) this means discarding a particular kind of information that might on the face of it seem rather valuable, i.e., information relating to the unique circumstances and characteristics of each observation or phenomenon. This is what happens

whenever one takes the mean of a series of numbers (for example, describing a group of texts, or people) and henceforth uses the resulting number to stand for the whole series, often for purposes of comparison with some other series (for example, describing another group of people or texts).

Does this all sound rather reductive? Naturally it does! The ultimate point is a) to end up with a yes or a no, such that the generalization in question can be more or less confidently added to humanity's store of things that are or are not considered to be true, where this confidence comes from the fact that b) one arrived at the yes or at the no through a truth-determining procedure so anonymous that anyone could theoretically have followed it and arrived at a similar result. Quantification is reductive *by design*; it is reductiveness raised to a virtue. The idea of treating cultural artifacts in such a way may appear scandalous, because it is precisely the opposite of what critical interpretation entails. But—paradoxical as it may seem today—such resistance to aggregation was once commonplace even in a discipline such as economics:

> When, in the 1860s, William Stanley Jevons proposed measuring changes in price level by an index number that was essentially an average of the percent changes in different commodities, critics considered it absurd to average data on pig iron and pepper. And once the discourse shifted to individual commodities, those investigators with detailed historical knowledge were tempted to think they could 'explain' every movement, every fluctuation, with some story of why that particular event had gone the way it did. … It was not that the stories [they] told about the data were false; it was that [those stories] (and the individual peculiarities in the separate observations) had to be pushed into the background. If general tendencies were to be revealed, the observations must be taken as a set; they must be combined.
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> (Stigler 2016, 15)

As with commodity prices, so with poetry, and music, and everything else: taking things in aggregate prevents us from explaining their individuality through stories founded on our detailed knowledge of the particularities of the things themselves and their histories. What it enables us to do instead is to observe general tendencies, and then to test our explanations of those general tendencies. It's never going to work quite as well when the generalizations being tested relate to social or cultural objects of knowledge, because the objects themselves are messier than physical objects of knowledge such as molecules or bacteria and are substantially more difficult (sometimes impossible) to consider in isolation. But that doesn't mean that we have to give up, nor even that it would be morally justifiable to do so. It only means that, if we are going to apply quantitative methods to such objects, we will have to proceed with exceptional caution and care.

This is unlikely to generate much excitement from scholars committed to the hermeneutic culture of knowledge production, because it isn't what they went into academia to do. On the contrary, they came to gain detailed knowledge of particularities and then to use it to tell enlightening stories. Which is fine: it is; honestly, it is. Better a single erudite monograph than a thousand badly designed experiments. But if we can design our quantitative studies carefully and well, and carry them out scrupulously, and demonstrate the contribution that the ensuing calculations make to existing knowledge—however small, however provisional, however apparently banal—then perhaps we may slowly begin to demonstrate a different kind of value, as those contributions accumulate.

THE TEMPTATIONS OF PSEUDOSCIENCE

But *will* those contributions accumulate? There is no guarantee. In a famous 1974 lecture, the Nobel prizewinning physicist Richard Feynman observed that much research in the social sciences was analogous to the ritual practices of "cargo cults," as they were then understood by Western anthropologists. The term "cargo cult," which covers "a range of millenarian ideas, cults, and movements that originated [in Melanesia] in the wake of Western colonisation and, more often than not, involved a strong concern with the acquisition of Western goods" (Otto 2009, 82–3), is now widely recognized as problematic: Lamont Lindstrom (1993) argues that it refers essentially to a Western myth, and Elfriede Hermann observes that residents of areas in which supposed "cargo cults" have been observed came to recognize the term "cargo cult" as "a catchphrase" used by colonial authorities and Melanesian elites alike to "degrade their culture" (1992, 56, 68). But if we understand what Feynman termed "Cargo Cult Science" as a metaphor whose vehicle is not an actual Melanesian religious practice but a Western myth or fable of "people at the margins of capitalised society who are conspicuously obsessed with getting hold of industrial products but are using the wrong means to do so" (Otto 2009, 93), it provides a useful means by which to understand the equally Western practice of *pseudoscience*: the natural sciences yield useful and reliable findings, like cargo planes coming in to land; not understanding the vast infrastructure necessary to produce this end result, some researchers from outside the natural sciences build runways and control towers of their own, and vainly hope for a drop-off. The runways and control towers are methods of numerical data collection and statistical analysis whose power is that "[w]hen [they] are employed correctly, they usually serve as a self-correcting system of checks and balances" (Cokley and Awad 2013, 27). But very often, they are not employed correctly—in part, because it is simply harder to employ them with regard to the more nebulous objects of humanities and social science scholarship, and in part, because of the lack of a culture in which "[o]ther experimenters will repeat your experiment and find out whether you were wrong or right" (Feynman 1974, 11). As a result, "the planes don't land," even though "all the apparent precepts and forms of scientific investigation" seemed to have been followed (Feynman 1974, 11). The fundamental problem is that all the statistical procedures in the world can't make up for the absence of the *collective* practice of formulating hypotheses and then using empirical evidence to test those hypotheses to destruction.

Unfortunately, much Digital Humanities advocacy reads as a plea to be allowed to establish a pseudoscience in peace. For example, the following argument from a leading Digital Humanities scholar was widely circulated in a number of different forums:

> Eventually, digital humanities must make arguments. It has to answer questions. But yet? Like eighteenth-century natural philosophers confronted with a deluge of strange new tools like microscopes, air pumps, and electrical machines, maybe we need time to articulate our digital apparatus, to produce new phenomena that we can neither anticipate, nor explain immediately.
> (Scheinfeldt 2013, 58–9)

The fact that some scientific progress was made before the formalization of the scientific method as understood today does not mean that it would be advantageous, or even ethically acceptable, to suspend that method's usual requirements for a discipline that is "young." (It should also be emphasized that the Digital Humanities are not so young as all that: the term "Digital Humanities"

has been around since the turn of the Millennium, and the Department of Digital Humanities in which I work is much older than that, albeit that it originally did business under another name.) If a comparison may be drawn between Digital Humanities and Computational Biology, the origins of the latter field are usually traced back to the publication of Alan Turing's article, "The chemical basis of morphogenesis" (1952), which, far from aiming to "produce new phenomena" that cannot immediately be explained, instead proposed an explanation of known facts in terms of established physical laws. One wonders whether computational biology would have reached the prominence that it enjoys today had its founders given themselves a pass on answering research questions on the grounds that a new discipline needs time to find its feet and, after all, "[t]he eighteenth-century electrical machine was a parlour trick" (Scheinfeldt 2013, 57).

I don't mean, of course, to imply that Digital Humanities is inherently pseudoscientific, nor that Digital Humanities research has added nothing to the store of human knowledge. For a contrary example to these strawman positions, one only has to think of the stupendous contribution to scholarship which has already been made by the incomplete, fully digital third edition of *The Oxford English Dictionary*; if one wishes for something more numerical, there are many impressive examples, such as Ted Underwood and Jordan Sellers's (2016) analysis of diction as a predictor of whether or not a given work of nineteenth-century literature would be reviewed in a prestige venue. But these examples have nothing in common with parlor tricks: the former evolved directly from existing lexicographic practice, iteratively extended through application and development of forms of technology with which the institution responsible had been working for many years, while the latter was designed in order to test a specific hypothesis emerging from existing, non-digital literary scholarship (i.e., "that a widely discussed 'great divide' between elite literary culture and the rest of the literary field started to open in the late nineteenth century," Underwood and Sellers 2016, 323), and as such explicitly adopted the most straightforward form of what is known as the scientific method. In other words, where Digital Humanities research succeeds most impressively, it is by adopting the standards expected of researchers outside the Digital Humanities, and by attempting to contribute to knowledge in a very clearly defined way.

This may explain why non-computational traditions of Digital Humanities research have often achieved more convincing results than the computational traditions on which this chapter's critical eye is primarily focused. When textual critics turned to digital technology in hopes of extending their existing scholarly practice beyond what was possible given the limitations of print as a medium in which to embody it, they knew what they were about (see Bode 2017, 94–102). By contrast, computational methods in the Digital Humanities have too often been justified either as works-in-progress that may eventually become rigorous modes of scientific enquiry at some unspecified point in the future, or otherwise as unpredictable procedures for the generation of ideas which may or may not prove fruitful as starting points for conventional (and often hermeneutic) scholarship. An example of the former is provided by the following piece on topic modeling, written for a Digital Humanities audience by one of the computer scientists who had pioneered the technique a decade previously:

> Here is the rosy vision. A humanist imagines the kind of hidden structure that she wants to discover and embeds it in a model that generates her archive. The form of the structure is influenced by her theories and knowledge — time and geography, linguistic theory, literary theory, gender, author, politics, culture, history. With the model and the archive in place, she

> then runs an algorithm to estimate how the imagined hidden structure is realized in actual texts. Finally, she uses those estimates in subsequent study, trying to confirm her theories, forming new theories, and using the discovered structure as a lens for exploration. She discovers that her model falls short in several ways. She revises and repeats.
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> …
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> The research process described above … will be possible as this field matures.
>
> (Blei 2012)

As an iterative process of hypothesis formation and confirmatory testing, this sounds a lot like the scientific method. But it is, by the author's own admission, only a "rosy vision" which—it is suggested—may become possible to realize at some point in the future, given unspecified technological developments. By contrast, the other mode of presenting a Digital Humanities methodology is evident in the following, written by a Digital Humanities scholar and published in the same special issue of the same Digital Humanities journal:

> How the actual topic modelling programs [work] is determined by mathematics. Many topic modelling articles include equations to explain the mathematics, but I personally cannot parse them.
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> …
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> Topic modelling output is not entirely human readable. One way to understand what the program is telling you is through a visualisation, but be sure that you know how to understand what the visualisation is telling you. Topic modelling tools are fallible, and if the algorithm isn't right, they can return some bizarre results.
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> …
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> Topic modelling is not necessarily useful as evidence but it makes an excellent tool for discovery.
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> …
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> Topic modelling is complicated and potentially messy but useful and even fun. The best way to understand how it works is to try it. Don't be afraid to fail or to get bad results, because those will help you find the settings which give you good results. Plug in some data and see what happens.
>
> (Brett 2012)

Unlike the previously quoted piece, this is a description of a Digital Humanities practice already possible in the here and now. Tellingly, there is no discussion of what might constitute "good results" or "bad results," nor of what features of a topic model might be understood through which kind of visualization, nor of how to recognize "fail[ure]" in topic modeling, nor even of what it means for a method such as topic modeling to be "fallible," nor of what one can really hope to achieve by "[p]lug[ging] in some data"—which data?—to a process whose details one "cannot parse." It is certainly conceivable that a hermeneutic scholar might find inspiration in the output of such a process, much as Surrealist poets found inspiration by cutting up printed texts and rearranging the words at random. Well, fair enough. But, to return for a moment to Feynman's

metaphor (with an acknowledgment, once more, that its vehicle is not a Melanesian reality but a Western myth), no one should be surprised if the planes don't land.

Which of the two visions above provides a better description of topic modeling as it is *generally* taught and practiced in Digital Humanities? In a piece written four years later under the title of "Topic modeling: what humanists actually do with it," one scholar surveyed numerous examples before concluding that topic modeling "allows us to chart new paths through familiar terrain by drawing ideas together in unexpected or challenging ways" that "consistently call us back to close reading" (Roland 2016). Computer as inspiration for hermeneutic scholarship, in other words.

The author of that later piece appears not to have meant his comments as a criticism. But not everyone in the Digital Humanities has been so relaxed. For example, while others were advocating for topic modeling as the next big thing in humanities methodology, Benjamin Schmidt was cautioning that humanities scholars "are too likely to take … [topic models] as 'magic', rather than interpreting [them] as the output of one clustering technique among many," and observing that those humanities scholars who already use topic modeling tend to approach it in a way that "ignores most of the model output" (2012a), employing a research practice which "ensures that they will never see the ways [in which] their assumptions fail them," and in the process "creat[ing] an enormous potential for groundless—or even misleading—'insights'" (2012b). That critiques such as this have emerged within Digital Humanities draws attention once again to the heterogeneity of the field, which is, as I have emphasized above, hospitable not only to under-theorized and often aimless tool use but also to the most rigorous enquiry. But the question must arise: if the former is going to be treated as *good enough*, then why put in the effort to carry out the latter?

There is nothing wrong, of course, with playing with tools as though they were toys, nor with being pleasantly surprised with intriguing "black box" computer output that one cannot fully understand. But Digital Humanities pedagogy is forever at risk of being overwhelmed by the confusion of such entertaining pastimes with the pursuit of knowledge. My personal experience is that, because students must be assumed to come to Digital Humanities with no prior experience of computer programming or of statistics, one must either take the slow approach of building up students' knowledge firstly of statistics and secondarily of programming to the point where they can grasp at least *some* approaches to analysis not as magical incantations but as a form of reasoning—or otherwise teach the *application* of a wide range of techniques without any real exploration of their theoretical and algorithmic underpinnings, thus creating the perfect conditions for pseudoscience to flourish.

While the former approach is (in my view) more intellectually defensible, and also happens to furnish students with knowledge and skills more readily transferable to other domains, including those of the workplace—the demand for workers with a little knowledge of Python but none of mathematics has often been exaggerated—it is also more challenging for students, especially given the tight time constraints imposed by a modern modular degree program, which is likely to require all of these things to be taught within a single course no longer nor more intensive than the several other parallel courses on more "theoretical" topics which students are likely to be studying simultaneously. And this is before we have even touched on questions such as data collection and research design: questions which are fundamentally part of both undergraduate and postgraduate teaching in subjects such as psychology or demography, and yet which struggle for space in a typical one-year postgraduate program in the Digital Humanities. In other words, while Andrew Goldstone is right to argue that "[c]ultivating technical facility with computer tools – including

programming languages – should receive less attention [in Digital Humanities pedagogy] than methodologies for analysing quantitative or aggregative evidence" (2019), the reality is not only that it typically receives less but also that there is usually not enough time for *either*. Under such conditions, it is easier to teach students *just enough* technical facility to be able to create an attractive visualization and then display it as an end result—as Goldstone further noted, Digital Humanities as a field "is enchanted with visualisation, to such an extent that many people in the humanities identify the whole of data analysis with the production of visualisations" (Goldstone 2017)—or to create a topic model and then to interpret the individual topics as though they were a sort of haiku.

NURTURING THE DEVELOPMENT OF CULTURES OF QUANTIFICATION

Quantification—and therefore also computation—can only contribute to knowledge within an empirical, as opposed to a hermeneutic, framework, and such a framework does not exist, or exists only at the margins, in many of the disciplines that are regarded as the natural home of research questions concerning cultural artifacts. This means that, realistically, there is scant prospect for the development of quantitative methods in primarily hermeneutic disciplines such as literature, philosophy, or theology. I therefore conclude this chapter by proposing that the most promising future for computation in the study of culture may be through integration into primarily empirical disciplines such as history and anthropology, and into what I have referred to above as the empirical "adjunct disciplines" of a typical Faculty of Arts and Humanities' hermeneutic star attractions: that is, through engagement with disciplines such as the history of books (or concepts, or religions) in a deep enough way to furnish research questions that scholars working within those disciplines find meaningful with answers which those same scholars are likely to find credible, comprehensible, and useful. This is unlikely to mean walking into unknown intellectual territory armed with whizz-bang computational methods whose underlying mathematics almost nobody can understand.

There are three models on which I contend that the sort of engagement that I am arguing for is more likely to be successful. The first is that of immersing oneself completely in a given discipline for a substantial period of time and coming to understand it intimately, so that one eventually becomes able to start from first principles in designing methods and projects that will truly address that discipline's core concerns (both methodological and substantive). The second is that of working in partnership with researchers from a given discipline in order to develop mutually comprehensible approaches to the extension of their existing practice through judicious application of well-understood methods for dealing with numerical information (or at least with information that can be meaningfully represented in numerical form). And the third is that of apprenticing oneself—so to speak—to quantitative currents which may already have emerged within the discipline in question: history, for example, has a vigorous—albeit sadly marginalized—quantitative tradition which predates the Digital Humanities by several decades (see e.g., Termin 1973).

Each of these models represents a radical rejection of the bombast and theoretical naivety which has unfortunately characterized some of the highest-profile examples of Digital Humanities "macroanalysis" (see Bode 2017, 77–94). Moreover, each takes for its starting point the recognition that, because the computational researcher's *existing* skill set will be of uncertain value in relation to

any given scholarly objective, the most important traits for him or her to develop are adaptability and a willingness to learn. Each model can thus be thought of as a different inflection upon a single program.

Central to that program is recognition that the most superficially impressive methods—the ones which most obviously wear the trappings of *science*—are unlikely to be the most useful ones. The ability to process large volumes of digital text, for example, is likely to be useful only in exceptional circumstances, and tests of statistical significance will often be strictly invalid. On the other hand, the world's archives (now and for the foreseeable future, mostly un-digitized) contain vast stores of commercial and administrative data, ripe for systematic analysis by those who have both the ability and the imagination. Such data may at first sight seem dry, but their potential is immense, as we see from William St Clair's (2004) economic history of the British publishing industry, or from Fabien Accominotti and colleagues' (2018) study of the changing class composition of audiences at the New York Philharmonic. Moreover, as Kevin Cokley and Germine Awad emphasize, there is an urgent need for the development of "empirical constructs which are rooted in the values and realities of … marginalised and/or oppressed population[s]" in order to "help empirical researchers [to] conduct more culturally competent research which will be responsive to the unique needs of … [those] populations" (2013, 30): a task for which scholars trained both in quantitative methods and in cultural critique should be ideally prepared.

Perhaps these ambitions will strike some as dispiritingly modest: rather than learning from and seeking to contribute to other disciplines, should not the Digital Humanities be blazing a trail for them to follow? That was, after all, the promise with which they burst onto the scene and were proclaimed the next big thing. But that was long ago: today, the Digital Humanities are no longer new, and have not been new since before the majority of current undergraduate students were born. And I hope that the Digital Humanities preserve enough institutional memory of their origins in various academic support services—my own department began long ago as a Centre for Computing in the Humanities, for example, and had no students of its own until comparatively recently—to be able to embrace a new service role, this time on an equal footing, with computational methods specialists working in partnership with scholars from across the humanities and social sciences to enrich each of the disciplines that they enter into by nurturing the development within it of a culture of quantification local and specific to the discipline itself. (And also by training their students to do the same—and not only within but also without the academy, where they may contribute to institutions other than disciplines in much the same way.)

A bigger problem, then, is that the ambitions that I have laid out are so difficult to realize: most of us are not yet ready to serve, in effect, as statistical consultants to the empirical humanities. In this connection, there are three observations that I should make. The first is that it's never too late to go back to school. The second is that "going back to school" means learning at the foot not only of statisticians but also of those who have spent their careers applying quantitative methods in empirical humanities disciplines independently of the developments associated with the Digital Humanities. And the third is that, with regard to the sort of program that I have outlined above, it will often be the older and the simpler methods that are most genuinely productive—in large part, because they are so much more straightforward to reason about. Some of those methods would not be taught on a typical statistics course. For example, a typical history project will probably benefit less from—say—spline regression than from an appreciation, grounded both theoretically and in research practice, of—say—the difficulties involved in aggregating quantitative data from different

historical sources. And this is the sort of expertise that a historian with an interest in statistics is as well placed to develop as a statistician with an interest in history. Were it to become the foundation of undergraduate and postgraduate training in the Digital Humanities, I think that we might start to see widespread progress.

Alexander Pope's famous warning still applies: *Drink deep, or taste not the Pierian Spring*. No, a little statistical learning is not "enough," any more than a little historical learning could be. And no—despite years of chipping away at my ignorance—I cannot claim to possess more than a very little of either. But the knowledge is there to be made use of, if only we can make the effort—and there are many, many places where it is needed. Not having yet learnt *enough* is no excuse for not beginning the work, in whatever small way—provided that one can remain forever mindful of one's own limitations, of course. To switch momentarily to a new metaphor: I may not have what it takes to become a doctor, but that would be no reason not to take a course in first aid, and no excuse at all for refraining from applying the knowledge gained on such a course in the event that I found myself in a situation where it could be of benefit.

First aiders to those that don't yet know any relevant statistics, and first aiders to those that do, but don't yet know much about the complexities involved in collecting and working with cultural and historical data: as ambitions go, I've heard worse. Again, I'm not talking about a revolution. I'm only talking about how much good might be done in nurturing local cultures of quantification across the so-called humanities and the so-called social sciences—at least where there exists the potential for such cultures to grow.

Where there doesn't, the smart thing to do is probably to walk on. There's too much work to do anyway.

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