Rethinking the 'Great Divide': Approaching Interdisciplinary Collaborations Around Digital Data with Humour and Irony

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Abstract

It is often claimed that the rise of so called 'big data' and computationally advanced methods may exacerbate tensions between disciplines like data science and anthropology. This paper is an attempt to reflect on these possible tensions and their resolution, empirically. It contributes to a growing body of literature which observes interdisciplinary collabrations around new methods and digital infrastructures in practice but argues that many existing arrangements for interdisciplinary collaboration enforce a separation between disciplines in which identities are not really put at risk. In order to disrupt these standard roles and routines we put on a series of workshops in which mainly self-identified qualitative or non-technical researchers were encouraged to use digital tools (scrapers, automated text analysis and data visualisations). The paper focuses on three empirical examples from the workshops in which tensions, both between disciplines and between methods, flared up and how they were ultimately managed or settled. In order to characterise both these tensions and negotiating strategies I draw on Woolgar and Stengers' use of the concepts humour and irony to describe how disciplines relate to each others' truth claims. I conclude that while there is great potential in more open-ended collaborative settings, qualitative social scientists may need to confront some of their own disciplinary baggage in order for better dialogue and more radical mixings between disciplines to occur.

Keywords: digital data, interdisciplinarity, mixed methods, quant/qual, data visualizations

"Why don't we just focus on things we *can* quantify?" asks a computer scientist. It's day two of a three-day 'data sprint' workshop and we're in the middle of a feedback session. The three teams, each of which are composed of 4-6 researchers from medicine, anthropology, computer science and science and technology studies (STS), have just been reporting back to the larger group on the progress of their mini-projects. It is not going very well. The teams seem frustrated with the tools, or their colleagues, or perhaps me, the organiser and facilitator. I ask if anyone has any advice to give to any of the other groups. A heavy silence hangs in the air, mercifully ended by the computer scientist's provocation.

This rhetorical question seems to imply that we have been spending too much time on things which we *cannot* quantify. In this case, she is probably referring to the long, messy and poorly formatted textual accounts we have been mired in over the previous days. She suggests that we could overcome the current impasse by focusing on the data which is more amenable to representation as numbers (time stamps, rankings and categorical data). On some level, I know that she is right. If the goal of a workshop like this is to mock up some data analysis tool or data visualisation in a very short amount of time or to gain some cursory insight into a difficult data set, then it would make sense to focus on what is ready-to-hand and feasible.

However, in these workshops, I have been actively trying to resist this sort of understanding of the objectives, defined instrumentally in terms of tools or results. I was interested in how one could conduct research with digital data from online platforms without falling into standard routines and divisions of labour between, say, quantitative and qualitative or technical and non-technical researchers. In this particular workshop, I had been encouraging the researchers, including the more technical ones, to close-read the data. This had yielded all sort of interesting insights about the substantive topic, but it seemed to produce (for many of the participants) a skepticism towards the automated tools, resulting in a palpable slump in the room and a lack of direction within the teams.

Introduction

It is often claimed that the increasing availability of digital data (from online platforms, tracking devices, and open government portals) and the prominence of semi-automated forms of data analysis (like data visualisations, machine learning and neural networks) may exacerbate already existing tensions between 'quantitative' and 'qualitative' research or between different disciplines, like computer science and anthropology (Burrows and Savage, 2014; Marres, 2012; Wouters et al., 2013). At the same time, others proclaim that certain methods (particularly network graphs) used in combination with these new data sources finally allow the reconciliation of macro and micro approaches (Venturini and Latour, 2010) and enable new types of collaborations and contributions (Blok et al., 2017; Neff et al., 2017; Vinkhuyzen and Cefkin, 2016).

Encounters like the one detailed above, however, suggest that things are more complicated. It suggests that frictions between disciplines and between methods are still very much present: that computer scientists might misunderstand the value of qualitative analysis or that self-identified qualitative researchers might have their own resistances to these tools. It also suggests that these divisions are not fundamental but play out in situated, practical negotiations over, for example, what sort of data to use and in what ways. How might we characterize these tensions and how could they be navigated?

This paper contributes to a body of literature which analyses interdisciplinary encounters around new forms of digital research and data infrastructures *empirically* (Blok et al., 2017; Kaltenbrunner, 2014; Neff et al., 2017). This work is increasingly vital as governments and funding bodies frequently demand interdisciplinarity but often only understand the term through abstract pronouncements. These empirical studies contribute to our understanding of interdisciplinarity as a practical and situated activity by observing novel approaches to data analysis and detailing messy interactions between different sorts of researchers and disciplines. However, I argue that existing roles and routines in these settings may be strong enough to paper-over many potential sources of tension and even *prevent* more radical mixings, and that these disciplinary tensions (and mixings) may require more active cultivation or interventions in order to be drawn out.

This paper also contributes to work within STS from researchers who have adopted quantitative tools but employed them largely in the service of qualitative research (Callon et al., 1986; Latour et al., 1992; Rogers, 2013; Rogers and Marres, 2000). These experiments, arguably, go further than many established forms of interdisciplinary collaboration or mixed methods approaches because they have incorporated interpretivist critiques of data and computational methods into the practical application of using such tools. These researchers have also been highly reflexive about their own practices – how these tools incline them in certain directions as opposed to others. However, I ague that more could be done to observe how these idiosyncratic approaches fare in the wider landscape of established disciplines and frameworks.

In this paper, I will examine disciplinary and methodological tensions as they played out in three workshops which were set up to expose mainly self-identified 'qualitative', 'non-technical' researchers to simple digital tools (such as scrapers, automated textual analysis, data visualisations). I will discuss three examples in which tensions flared up and how they were ultimately managed or settled. I will propose that these tensions and the various responses to them can be understood in terms of 'irony' and 'humour' as understood by Woolgar (1983) and Stengers (2000) respectively. Woolgar (1983) characterised constructivist sociologists of science as 'ironists' because they 'reveal' natural science accounts of reality to be constructed without subjecting their own science (ethnography) to the same criteria. It was in reference to Woolgar's paper that Isabelle Stengers advocated that analysts of science approach their subjects and interlocutors, with 'humour', that is, with the understanding that their fates are intertwined with those they observe (2000: 65). At the end of the paper, I will draw on Katie Vann's (2010) more recent analysis of these concepts in order to question how we might interpret these orientations in terms of interdisciplinary encounters. My objective is not to offer some definitive account of interdisciplinary interactions, but to expand the lexicon for talking about these tensions as well as the arsenal of tactics for moving past them.

The current settlement

As we are repeatedly told: the last several years have seen governments and private companies amass unprecedented amounts of data, housed in 'data warehouses', dumped in 'data lakes'. These masses of found or 'transactional data' from online platforms and open government repositories, often positioned in contrast to survey data (Burrows and Savage, 2014), are seen by many as naturally amenable to much-hyped techniques like machine learning, Artificial Intelligence (AI) and data visualisations. While it is important to be sceptical towards these narratives about the newness of these data sources and the power of these computational methods, these performative claims are nonetheless reshaping industries and academic disciplines. New approaches like data science (Schutt and O'Neil, 2013), computational social science (Lazer et al., 2009) and digital humanities (Berry, 2012) are moving into traditional social science and humanities territory, given that much of this newly amassed data is nominally 'social' in character. These developments might necessitate closer collaboration between social scientists and computer scientists but they also might involve computationally advanced methods supplanting what one might call, for lack of a better word, 'qualitative' or 'interpretivist' forms of knowledge (Marres, 2012).[1]

In this section I will discuss different reactions to the state of affairs engendered by the rise of digital social data from 'qualitative' social scientists. These reactions range from outright critique to calls for convivial but, as I will suggest, rather safe collaborations. What I want to argue is that much of both the critical and convivial relationships represent a settlement in which there is not much at stake and there is little chance of either party being changed in the process. At worst, this takes the form of an 'ironic' distance, as I will explain, and at best this results in siloed modes of working. To unthink this settlement I argue that we need more studies of interdisciplinarity in practice, which see both tensions and negotiations as not given but as accomplishments of situated practice. However, we also need studies that do not presume from the onset that we know what disciplines are composed of.

One of the dominant responses to the proliferation of data and computational methods has been largely *critical*. Anthropologists and qualitative social scientists have long raised concerns about data-driven techniques on epistemological, ethical and political grounds (Iliadis and Russo, 2016; Manovich, 2012), arguing that they fail to capture the nuance of situated practice (boyd and Crawford, 2012) or lead us toward simplistic research questions (Uprichard, 2013), that they exacerbate existing asymmetries of access and visibility (Benjamin, 2019) and that they remain largely unaccountable (Pasquale, 2015) to the people whose lives they affect. STS scholars in particular have examined how algorithms and data analytics achieve their performed neutrality, commensurability of different types of data (Slota et al., 2020) and gloss over gaps and silences (Coopmans, 2014; Leonelli et al., 2017; Lippert and Verran, 2018; Neyland, 2016).

While these critiques draw much-needed attention to the politics of automated, data-driven approaches, particularly to *the effects* of these systems, it is not self-evident that the more methodological or epistemological critiques of these systems have resonated with the data scientists who design them (see Moats and Seaver 2019). One reason for this might be because these critiques often seem to judge data science or data visualisations implicitly *vis a vis* ethnography or other qualitative methods – that they are reductive or simplistic when compared to qualitative methods or 'small' curated datasets (Abreu and Acker, 2013; boyd and Crawford, 2012). And while asserting the value of qualitative methods in relation to computational methods is an important task, such criticisms risk unfairly framing data science as a failure to capture nuance and complexity when the potential value of computational methods may lie in simplicity and abstraction.

These claims are also potentially in danger of falling into the ironic fallacy described by Woolgar (1983): they purport to show the limits, social determinants and constructedness of data and data science, while the methods used to demonstrate this fact (often ethnography) are seen to represent reality faithfully. Of course, ethnographers are first to admit the constructedness and partiality of their own accounts, but Woolgar's point is they often slip into an implicit correspondence – or in his words 'reflective' (1983: 243) – theory of truth in order for their account of 'social factors' or 'politics' to be believed by the reader. This reliance on a conventional report of what-was-witnessed is in some sense unavoidable (Woolgar, 1983: 244), Woolgar notes, but when social scientists temporarily exempt themselves from this fundamental problem, they sidestep important questions about what makes an account of some reality adequate for this or that audience, which are arguably central to interdisciplinary relations.

While I do not wish to return to long-dormant debates about constructivism, and this argument mainly relates to the written accounts of ethnographers and scientists: I think this is a useful way of thinking more generally about how disciplines relate to one another and think about the status of each other's truth claims. Do they dismiss each other's methods and facts out of hand or see knowledge production as a shared and ongoing problem? Stengers starts *The Invention of Modern Science* (2000) by asking why scientists have not responded well to social science analyses of their work. She argues that social scientists should approach the sciences not with irony but with 'humour', by which she means "…the capacity to recognize oneself as a product of the history whose construction one is trying to follow" (Stengers, 2000: 65), to put their own identities *at risk*. So, while these critiques of the new data science are important ones, I wonder if the separation effected between them and their object of study makes it unlikely that computer scientists will adopt these critiques from outside or that qualitative social scientists will propose viable alternatives.

The other dominant reaction to this situation is to call for more and better collaborations between interpretive social scientists and computational researchers. There is a long tradition in STS but also anthropology, sociology and human computer interaction (HCI) of productive collaborations with computational disciplines in the academy and in industry. Vertesi and others' (2016) contribution to the STS Handbook describes four modes of engagement with computational researchers ranging from 'corporate' and 'critical' to 'inventive' and, most radically, 'inquiry'.[2]

However, for every apparently 'successful' collaboration (as the authors note, one of STS's main contributions to these fields is to ask 'success for whom?' (Vertesi et al., 2016: 176), there are many other more fraught encounters, where ethnographers complain about being misunderstood (Dourish, 2006) or shut out of the process, or where computer scientists relate to social scientists in what Barry, Born and Weszkalnys (2008) might call a 'subordination-service' mode. In any case, most ethnographers or micro-sociologists in these projects would probably admit that their influence on the proceedings is often limited and circumscribed: they are often relegated to attending to so-called 'social factors', ethics and effects of technical systems, rather than their technological design and implementation.

But why is this so often the case? One possible reason has to do with roles which ethnographers and social scientists take on, or which are assigned to them. These include: detached observers watching from the side-lines; token ethicists; experts in science communications; reluctant spokespeople for end users (Woolgar, 1990) or for publics. Researchers have occasionally been able to assert different priorities within these programs (Neyland, 2016) or argue for one set of technique as opposed to another (Adams, 2016; Vinkhuyzen and Cefkin, 2016), but in general, many of these roles assume that qualitative social scientists will not dirty their hands with statistics and algorithms or visual representations of data.

Of course, there have been many attempts to address this longstanding 'siloing' of disciplines. Discussions around mixed methods (Denzin, 2010) have long provided models for practically combing different methods and philosophical paradigms (Tashakkori and Teddlie, 2010) in the same study, in more productive ways than the above roles might allow.[3] Grounded theory (Glaser and Strauss, 1967), in its many forms, proposes that qualitative insights can be built up inductively into theories (through achieving 'saturation') which can then be tested or modelled quantitatively. While these frameworks are widely accepted, even beyond the academy, central debates about validity (Clavarino et al., 1995), reliability and triangulation (Denzin, 2012; Silverman, 1985) suggest that these disciplinary or methodological tensions are by no means settled, only sublimated.[4] More recently, Blok, Pedersen and collaborators (Blok et al., 2017; Blok and Pedersen, 2014) have proposed a 'complementarity' between ethnography and data science: that both sets of methods are mutually exclusive yet mutually necessary.

But while mixed methods, grounded theory and complementarity may be very effective strategies for managing collaboration, even if (or precisely because) they do not resolve philosophical tensions, because these frameworks tend to keep researchers at a distance, separating them into different phases of the project or in different parallel tracks with intermittent contact, they do not allow for the possibility that these roles might be transformed in the interaction (Stengers, 2000), that anthropologists might take up quantitative tools in a *different* way or that computational disciplines might integrate social science criticisms of their approaches (as mentioned above) into their tools. In these frameworks, (potential) tensions might be hidden from view and *alternative* configurations of researchers, disciplines and methods might never emerge. So while critiques and collaborations seem like contradictory responses, they both result in what I will call a 'settlement' in which disciplines are kept separate and there is little chance of radical mixing happening.

Now some might argue that such a settlement is inevitable: that most anthropologists and qualitative sociologists do not have the technical literacy to take up these tools in different ways, though as I will discuss later there are plenty of researchers working between different traditions (e.g. Murthy, 2008). Others might claim that these relations are underwritten by historical distinctions between quantitative and qualitative methods, scientific and humanistic disciplines (Gould, 2011; Snow, 1998), objective and subjective epistemology (Daston and Galison, 2007), variable or process orientations (Maxwell, 2010) or research which is communicated in terms of "stories" or "numbers" (Smith-Morris, 2016).

Much important work has been done to question these divides (Hammersley, 1992), to trace alternative genealogies in which, for example, anthropologists have engaged with techniques of counting, calculating and mapping (Munk and Jensen, 2015; Seaver, 2015). Quantitative sociologists have also made overtures to qualitative researchers by taking into account traditionally interpretivist concepts like meaning-making (Mohr, 1998), narratives and emergent phenomena (Abbott, 2016). But even if such divisions are not inevitable or hard-wired, they cannot so easily be wished away. We know that digital technologies are not parachuted in out of nowhere, they must take root in the existing, evolving infrastructures (Edwards, 2010; Wouters et al., 2013) which often are maintained by and within disciplines (Kaltenbrunner, 2015).

While these alternative histories offer important inspiration, the point is that neither the tensions, nor the successful negotiations are natural or given, but are rather accomplishments of situated practices. These divisions and relations are enacted in everyday interactions and entrenched routines and even instances of boundary work (Gieryn, 1983) – invocations of charged pejoratives like 'positivist' and 'relativist'. And likewise alternative configurations of researchers are fragile, modest and extremely hard won. So one important place to look for alternative possibilities is in detailed *empirical* studies of collaborations between different sorts of researchers – because they give us hints as to exactly what tensions and negotiations are made of.

There is a long tradition of such empirical studies (Wouters et al., 2013). For example in a companion piece to their paper about complementarity, Blok, Carlsen and colleagues (2017) discuss an interdisciplinary project in Copenhagen pairing data obtained from Facebook with ethnographic observations. They give rich, situated accounts of how the ethnographic fieldnotes were used to raise questions about data science findings and vice versa. Other studies, however, suggest more messy encounters. Kaltenbrunner (2014), in his study of collaboration between computer scientists and humanities scholars, describes how different researchers working with a common dataset fail to agree on the project goals because their approaches have different 'hinterlands' (Law, 2004) and disciplinary ways of phrasing research questions. Collaboration cannot proceed, he argues, until they 'decompose' the process, placing these different ways of doing research on the table. Neff and colleagues (2017) examine several instances of anthropologists and data scientists experiencing problems with data, finding that their data science colleagues exhibit the sort of reflexivity and critical attention to data provenance normally attributed to qualitative researchers.

These studies offer invaluable glimpses of interdisciplinarity in practice: both how tensions might flare up and how they can be resolved. However, these studies are at their best when they do not take for granted, from the onset, that we know what, say, ethnographers and data scientists *do*, when as Kaltenbrunner's account shows, *what they do* must be examined and rethought. As suggested above, when observing mixed-methods style projects, it becomes very difficult to see past these inherited divisions of labour. For this reason, I think the most interesting studies seem to focus, not on successes, but on tensions, problems and failures and attempts to surmount them.

Another place we might look such alternative disciplinary configurations is in a longstanding movement within STS and related disciplines in which largely qualitative researchers have been adopting and adapting quantitative tools to their own ends (Callon et al., 1986; Latour et al., 1992; Rogers and Marres, 2000). In doing so, they incorporate STS understandings of methods as performative (Law, 2004) and social science critiques of quantitative research into their own practices (Marres, 2017). These researchers are also highly reflexive about their struggles and negotiations with these tools (Birkbak, 2016; Jensen, Forthcoming; Munk et al., 2019; Pantzar et al., 2017), though some of the most interesting moves remain tacit, not always explicated outside the community. For example, they tend to use graphs not as demonstrations of findings but rather as exploratory maps to locate cases to investigate qualitatively (Rogers and Marres, 2000). They deploy these techniques in order to document the partiality and constructedness of the tools (Venturini et al., 2014), or of the underlying data and devices behind them (Gerlitz and Helmond, 2013; Rogers, 2013) and the normative commitments they smuggle in (Madsen and Munk, 2019). They also prefer to only use categories or dataset demarcations (Marres and Moats, 2015) which arise empirically, rather than impose their own assumptions onto the proceedings (Uprichard, 2011).

These are interesting tactics which fold some of the criticisms of interpretivist social science researchers about computational data analysis into the practice of data analysis itself, in a way which starts to repair the 'ironic' distance mentioned above – raising, rather than settling, questions about the status of knowledge claims.

However, these observations are largely circulated within homogeneous teams of STS researchers and have rarely been tested in the wider academic community where expectations of what constitutes 'quantitative' and 'qualitative' methods abound and roles are more entrenched.

In this section, I have argued that both ironic critique and convivial collaborations amount to a settlement which I think may prevent both productive dialogue and alternative configurations of disciplines from emerging. I suggested that in order to move past this impasse, we need to study interdisciplinary interactions in practice, particularly ones in which tensions manifest themselves. The aim of this paper is to add to these empirical studies of tensions and negotiations between different approaches around digital data. But how can we observe situations in which disciplinary identities are put at risk, which allow for both disciplinary tensions and more radical mixing to unfold?

Three workshops

In thinking about this problem of how to shake up disciplinary routines, I have been inspired by recent calls for 'situated interventions', in which researchers take concrete actions in the social settings they are embedded in, both with the aim of making a difference and learning about how actors respond when pressed in various ways (Zuiderent-Jerak, 2015).[5] For example, Zuiderent-Jerak, as a participant observer embedded in a hospital, tested some of his ideas by translating them into forms more amenable to his informants like flow charts and economic models. Analysing reactions to these interventions allowed Zuiderent-Jerak to reflect on the different normativities at play in particular settings but also make visible and challenge some his own (Stengers, 2000). For example, Zuiderent-Jerak was able to, among other things, rethink his hard-wired disciplinary resistance to practices of standardisation.

So, what sort of intervention would put both anthropological and computer science identities at risk?[6] There are several established settings in which qualitative researchers and programmers already collaborate. Hackathons (Irani, 2015) and Data Sprints (Munk et al., 2016) are events where participants collaborate on small projects over two to three days. Normally the participants are split into sub-groups based around shared interests, data-sets, methods or problems. In these interactions, the horizon of possibilities is often set by the more technically-capable participants (Ruppert et al., 2015), while qualitative researchers and anthropologists become 'topic experts' who relinquish responsibility for the analysis or using the tools. It seemed clear that these encounters would need to be modified in order to avoid participants falling back into established roles and routines.

A group of us at Linköping University decided to put on a series of workshops, each one focusing on a particular area of social life which was being transformed by the rise of digital data. These were based on hackathons and data sprints but tweaked in various ways to unsettle these knee-jerk roles and ways of working. Firstly, we involved mostly participants who self-identified as 'non-technical' including researchers from a variety of disciplines including STS, medical sociology, medicine, media studies and anthropology. The idea was that this would encourage these participants to get their hands dirty with the tools, rather than have a technical expert do it for them. I was also curious what these ostensibly sympathetic disciplines would make of recent STS experiments with data and digital tools. The workshops also included more technically capable researchers from information systems, computer science and library sciences; however, we tried to shake them out of established routines by using different sorts of data than they were used to. Secondly, we encouraged the participants to spend more time on 'problem definitions' – we discussed particular social and intellectual problems related to the topic before we made any mention of possible digital tools and data sets. This was because much research about computational techniques shows how readily available tools and data may incline us to focus on what is easy to analyse (Uprichard, 2011) rather than what is important to analyse, as the opening vignette of this paper also eludes to.

Thirdly, we focused on producing simple data visualisations, mostly network graphs. Visualisations are interesting because, while they necessarily involve algorithms and metrics, they foreground the role of (equipped) human interpretation in the process (Card et al., 1999). They also, it is claimed, can open the research process to a wider array of less technically-minded participants and, as has already been noted, anthropologists in particular have a long history of employing mapping approaches (Munk and Jensen, 2015).[7] We provided slides of several unconventional visualisations which we felt were more compatible with anthropological or micro-sociological approaches because they addressed some of the criticisms from these fields: for example, they were seen to lend themselves to exploratory analysis and to avoid aggregation and researcher-defined categories where possible (see discussion in previous section). Finally, as workshop organiser, I actively intervened in various groups' projects. Sometimes I helped with suggesting data sources and tools of analysis while at other times deliberately detached myself to allow a group to find their own way. Sometimes I took on the role of technical expert, offering computational solutions or demonstrating tools, while other times I became more like a curmudgeonly anthropologist, slowing things down and raising annoying questions about computational practices. As someone who is part of the STS community experimenting with computational tools, this was not a huge leap as, I often find myself caught between these roles anyway. But as the opening vignette suggests, I was not always in control of the proceedings, or my place in them.

It should also be said that these workshops were primarily set up to cultivate networks of researchers and foster new approaches to important empirical topics, but they also offered occasions to reflect on interdisciplinary relations (something which I made clear to all the participants). In what follows, which is based on my fieldnotes made at the time, I will discuss three moments in which disciplinary or methodological tensions manifested themselves and how they were navigated. I will discuss one vignette from each of the workshops because each of them involved different configurations of researchers, which may have impacted how these interactions played out. I will first discuss a more conventional disciplinary situation, followed by one which exemplifies the more reflexive STS work and finally a less common interaction which was both more fraught and, arguably, more radical in character. I hope, given the discussion thus far, that it goes without saying that my accounts of these workshops are partial and interested, as are my strategic choice of vignettes. My purpose here is not to convince you, the reader, that the workshops played out in exactly this way, or that they are perfectly typical of interdisciplinary relations. However, through the positioning of these vignettes I hope that qualitative social scientists might reconsider the ways in which they conceptualise their ways of knowing in relation to those of their disciplinary 'others'.

Encounter 1

One of the workshops focused on the use of digital data and digital tools in academia. While the sciences have long produced data about themselves (Wyatt et al., 2017), there are increasing drives to measure and make academic research more accountable, resulting in new approaches like alt-metrics (Costas et al., 2015) and countless rankings of academic output. This workshop was attended by a variety of researchers from STS, anthropology, scientometics and information sciences (12 in total). All of them were sceptical about current, rather simplistic ways of measuring academic output, yet their very attendance at the workshop suggested that they were not against measurement *per se*. Indeed, many of the participants were interested in using computational, automated techniques to demonstrate the existence of phenomena which current metrics and measurement make invisible. Despite this inventive set of goals, because the participants came from relatively mixed departments (scientometrics and information science departments have included quantitative and qualitative researchers for some time) it was perhaps easier for them to slip into existing divisions of labour, as I will explain.

One team of four was interested in whether or not computational tools could be used to detect some of the performative effects (Callon, 1998; MacKenzie et al., 2007) of measurement systems: the ways in which different institutions reacted to or oriented themselves towards being measured. One group member was experienced in both quantitative scientometrics and qualitative STS literature, while the other three had an STS background but varying degrees of experience with digital tools. The group quickly decided that they wanted to experiment with a tool called VOSviewer, developed by the University of Leiden (van Eck and Waltman, 2009). VOSviewer works by scraping the Web of Science database to obtain lists of scientific articles and abstracts as well as metadata like publication date and disciplinary tags. The tool then identifies terms (noun phrases, to be precise) that appear together in the articles: the more abstracts they appear together in, the stronger the connection. These relationships are then represented as a network of words, so that words with more connections are brought closer together into clusters (see also Callon et al., 1986; Danowski, 2009).

Only a couple of the participants had used the tool before and the others were curious to see what it could do. As I had feared, this quickly became a show-and-tell scenario with the scientometric researcher demonstrating the tool to the others on the projector. But the scientometric researcher also slipped into another familiar role of merely implementing the other's ideas (Kalten-

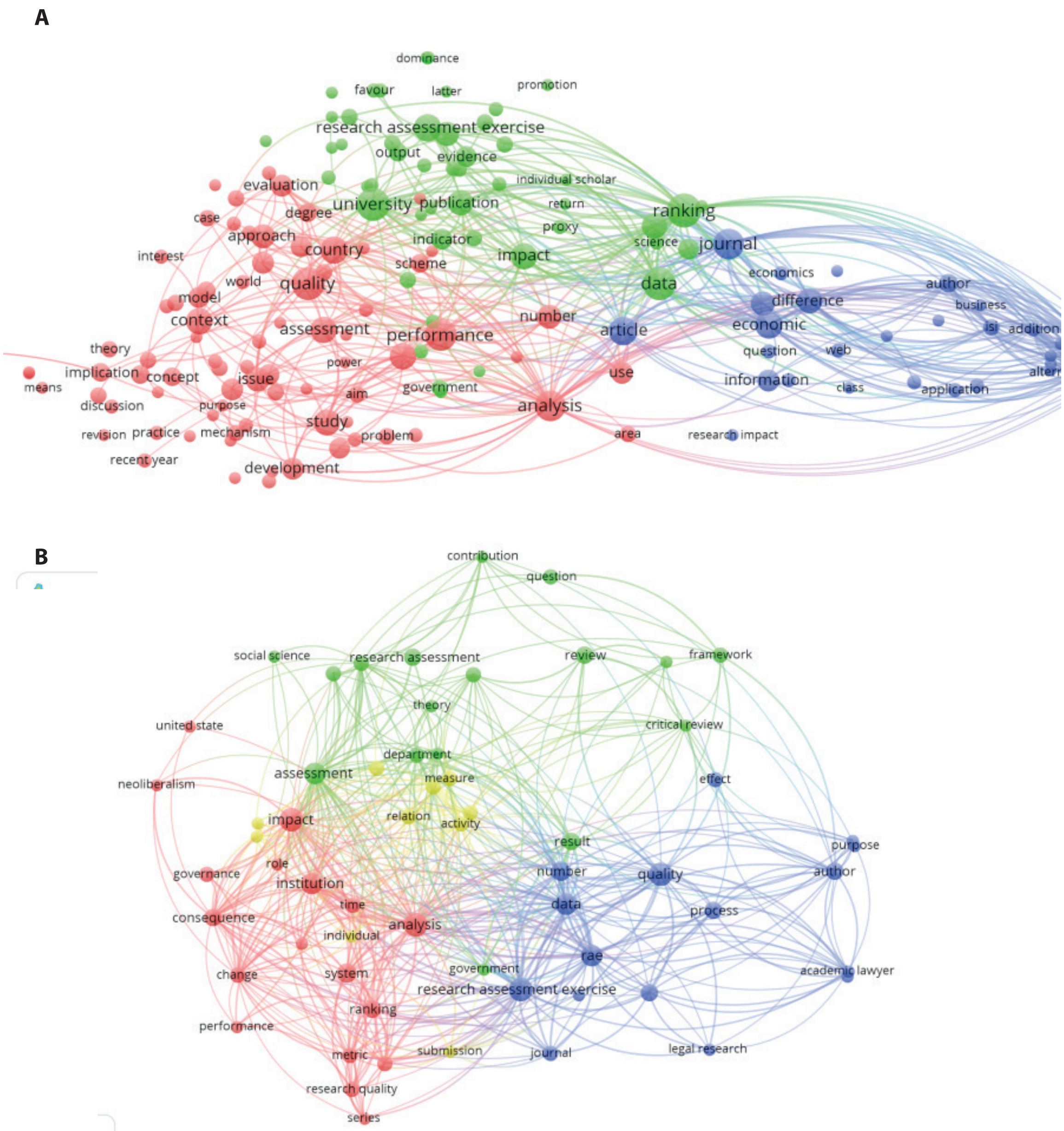

Figure 1. Co-word of article results in economics (above) and sociology (below)

brunner, 2015), acting as a kind of tech support. The other participants asked him to search for the following terms in journals tagged as 'Economics' and 'Sociology' in order to obtain a list of articles explicitly dealing with forms of academic assessment.

TS='academic evaluation*' OR
TS='research excellence framework' OR
TS='Norwegian system' OR
TS='Performance based funding'
TS='research assessment'

The resulting articles were then visualised as two co-word networks, one for the Sociology-tagged articles and one for the Economics articles.

These networks, which showed different configurations of key words used by the different disciplines, seemed to raise more questions than answers. In general, the participants were confused as to what the maps were "saying". They also could not seem to use the maps in an exploratory sense to find interesting papers to read because this way of using co-word did not make visible the articles which contained the key terms. I asked them if this demarcation of economics from sociology made sense because it meant accepting the definitions of economics and sociology provided by Web of Science. It was then proposed that the journal articles from the two disciplines could be pooled and allowed to cluster so that journals which use similar keywords could be brought closer together – the distinction, or lack thereof, between economics and sociology could be interrogated empirically with the graph.

I regretted raising this issue because what happened next was that the scientometric researcher and one of the others continued to work on this alternative graph, hunched over a laptop, while in parallel the traditionally qualitative researchers switched to what they knew best: close reading the texts.[8] Their hypothesis (or hunch) was that economists, who are closer in certain ways to the methods of measuring academia, might articulate the problem in more standardised ways (there would be more alignment in responses from economics and more diversity in sociology). They then read a handful of these articles, trying to pick out particular passages which spoke to the author(s) orientation to ranking and measurement. The group found, perhaps unsurprisingly, that economics framed academic evaluation as a technical problem – the measurements are wrong – while most sociologists treated it more like a threat to academic practice. Both used lots of jargon, but the economic jargon was more technical while the sociological jargon was theoretical. It was only after this exercise that the more interpretive researchers saw traces of their findings in the original maps.

At the end of the workshop, the interpretivist researchers had ended up confirming some of their suspicions about economics and sociology, while the other pair of researchers had ended up with an impressive visualisation, in fact an animation, showing the relationship between economics and sociology journals on the topic of research assessment over the past 20 years. Interestingly the animation did not show the disciplines separating into distinct clusters as the teams had suspected, but instead clustered around empirical topics (particular evaluation techniques). The presumed distinction between the fields was not evident, at least to this particular usage of VOSviewer.

This brief account speaks to one fairly common manifestation of disciplinary tensions in the workshops and also one way in which it was managed. The tensions here appear as disappointment, the disappointment that graphs do not show what they are supposed to or that they did not guide the research process. One of the participants after the workshop pointed out in an email that "…the more qualitatively oriented participants were more optimistic regarding the quantitative methods compared to those having more experience in that sort of work." The graphs have farther to fall if one does not know how messy and confusing they can be to work with.

Perhaps for this reason, the groups ended up slipping into a standard mixed-methods division of labour: to work separately but equally and then compare results at the end. They were happy to find some felicitous correspondence between the two processes but the insights came mostly from the qualitative analysis and they were, as the participants admitted, not particularly groundbreaking. It was unfortunate that they ended up reading economics and sociology articles as separate batches, which confirmed some of their suspicions about the *differences* between them because, as suggested by the animated visualisation shown at the end, not presuming the distinction could have allowed them to find more hybrids between the two.

The same could be said about the research process itself: the two approaches were kept largely separate, which inevitably confirmed expectations of what these approaches were *capable* of. Because of this distance, the qualitatively-inclined researchers only projected *instrumental* uses onto the graph but did not imagine a way in which *their* close-reading work could be used instrumentally to help refine the graph. Relations were highly respectful and there was no 'ironic' sense of either scientometrics or qualitative analysis being raised above the other but this was also not 'humorous' because no identities had been put at risk. The participants noted after the fact that their interdisciplinary ambitions were quickly "funnelled" by technical possibilities and time constraints which meant that they were, sadly, kept "in their silos" as they put it.

Encounter 2

Another workshop focused on the use of data analytics in recent political campaigns, particularly the use of machine learning, big data and psychological profiling to target political advertisements to increasingly specific types of voters (Anstead, 2017; Barocas, 2012; Loukissas and Pollock, 2017). The group, composed of 12 participants, was interested in how data-driven political consultancies like Cambridge Analytica positioned what they were doing, how they were involved in redrawing the boundaries between science and politics through their hyperbolic public pronouncements. However, the industry, understandably given recent scandals, proved to be relatively opaque: there were no obvious datasets or materials through which their activities could be observed.

This workshop mostly included participants from the Digital Methods Initiative (Amsterdam) and Techno-Anthropology Lab (Copenhagen), two key centres in which STS-influenced researchers had been experimenting with web scrapers, text analysis and network graphs (Jensen, 2013; Rogers, 2013). While these groups were very adept at using digital tools, and had written extensively about them, most of these methods have been leveraged to analyse social media and other online platforms, which are mostly publicly available and comparatively well-formatted. This topic however entailed that they analyse other sorts of documents and online data, which shifted the research from more anthropological 'how?' questions to simple 'who?' or 'what?' questions: *who* were these political consultants and *what* sorts of data and technologies were they using?

I had suggested that the group could use the electoral registers for the United States and the UK. These are public databases which list expenditures by political campaigns and their proxies in a given election. The larger collective quickly agreed that, if these two lists were combined, they could be represented as a bi-partite network diagram (a network with two types of nodes) connecting payers (political campaigns and proxies) and their payees (various suppliers, consultants and services, including data analysis and targeted advertising). Hopefully this would allow them to identify which types of campaigns made use of social media data for micro-targeting.

One team of two participants (an anthropologist studying data privacy and an STS scholar experienced with digital methods) decided to analyse this dataset. Since the databases placed limits on how many records could be downloaded at one time, they ultimately had to limit the search to individual expenditures over $1000 and disbursements over $10,000 for the US, and a similar level for the UK. They also limited the records to the years 2013-2016 so that they could focus on the 2016 election and EU referendum. The anthropologist started to ask questions like "how long does a campaign work in advance of an election?" or "what size expenditures are most interesting?". The more technical researcher joined in on these speculations. This became another moment of tension, but this time not between the two researchers but between the researchers and the structure of the database they were dealing with.

Combining the UK and US datasets was relatively easy, but participants described the data as "messy", being collated from a host of different organisations and sometimes converted from paper submissions. Through keyword searchers, the group realised that the company "Facebook" was spelled 23 different ways (Facebook, Face Book, Facebook Inc. etc.). Yet making a network diagram inclined them to resolve these alternative spellings into singular entities. The anthropologist asked if it could be assumed that these alternate spellings were the same company: "What about their subsidiaries and proxies?", "Are they also 'the same'?" Her more technically adept partner was also concerned by these questions, but knew that

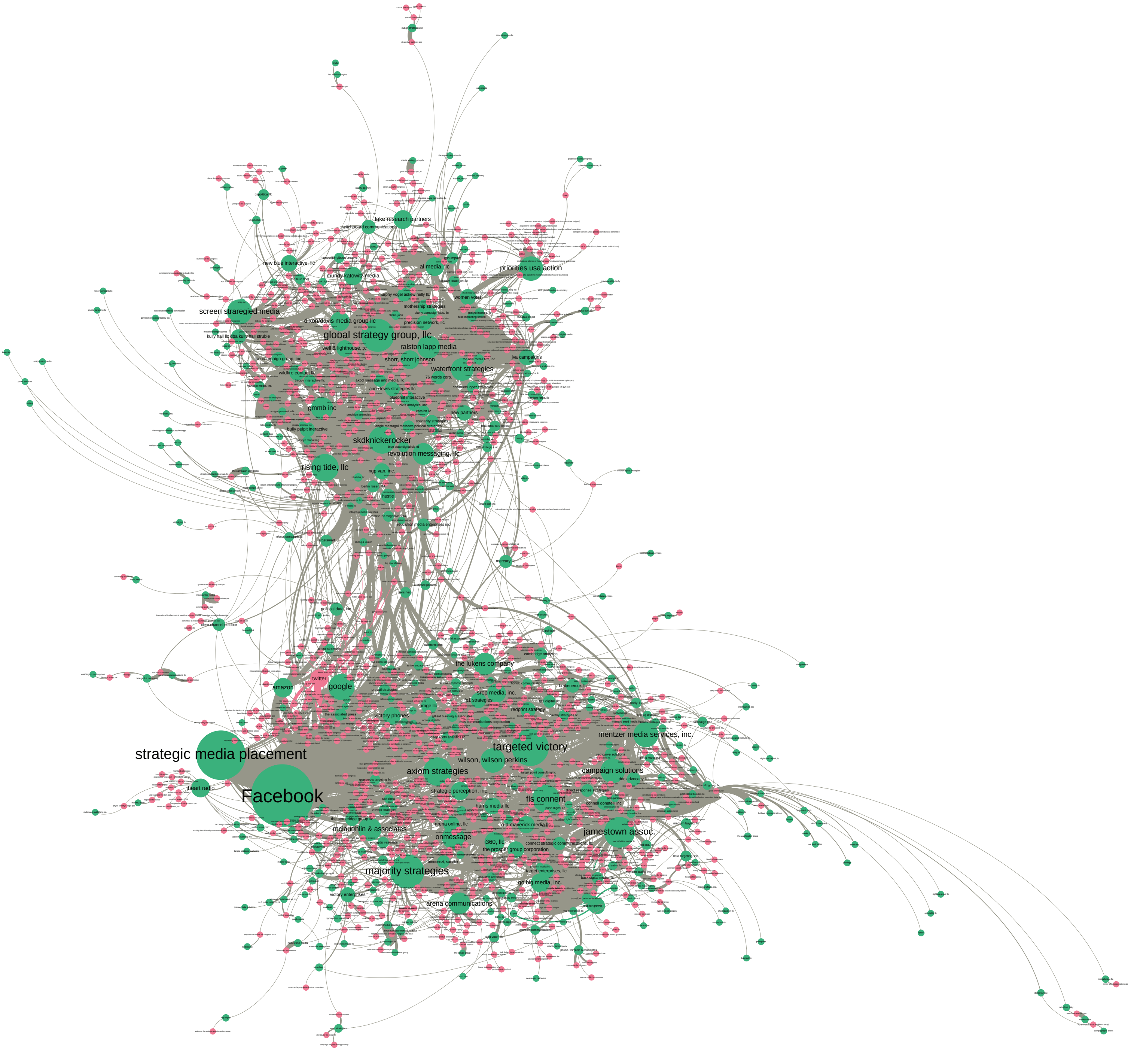

Figure 2: Payers (in red) and Payees (in green) in the UK and US election cycle 2014-6. Payees are sized by number of connections in this version of the graph.

they had to move on in order to have something to present at the end of the workshop.

The solution was a rather amusing assembly-line in which the more technical researcher would bring up a list of similarly spelled companies in data processing tool Open Refine, while the anthropologist would Google the names to determine if they were the same and could thus be merged. This took them the better part of the second day (at least 4 or 5 hours), with the repeated chime of "what about these two?" followed by lots of "umming" and "awings" over the din of the other groups quietly typing at their laptops. Once they had satisfactorily "cleaned" the database and created the graph, still more work was required. The graph contained relatively clear clusters but in order to understand what each one represented they had to scan a handful of nodes (payers and payees) residing in each, track down their web pages and quickly get an impression of their political leanings, country of origin, or possible uses of voter data.

When presenting their results, the group first explained their trials and tribulations with cleaning the data. They then showed the above graph (figure 2), explaining that that the two major clusters did not correspond to US and UK, as one might expect. Rather, the top cluster seemed to consist of mostly US Democratic party candidates and organisations and their payees, while the bottom cluster seemed to contain the US Republican party and several of the tech giants (Facebook, Google etc.), as well as most major UK payers. However, as the more technical participant noted: "these are the clusters according to *this* algorithm…", at which point, he clicked through several settings and windows, displaying different configurations of the network, complicating the seemingly clear 'finding'.

In this second encounter, the tension took the form of a wariness on the part of both researchers in relation to their efforts at what they called "data cleaning", interpretations of the graph and the "findings" they presented to their peers. Other datasets might have allowed them to defer their cuts and categorical decisions to actors in the field, but in this case their impulse to be more empirically-grounded clashed with the requirements of the chosen approach. They were required to make somewhat arbitrary choices based on their assumptions about the data, something which they did only reluctantly.

Unlike in the previous encounter, there was less of a clear demarcation of different types of research, despite the technical gulf between the two researchers, possibly because both researchers had STS training. Instead of a clear division of labour, they both actively engaged in counting, cleaning, interpreting and making decisions. So, one might say that the responsibility for producing the graph was shared between them. But how did they reconcile their doubts about their assumptions, and their STS-infused scepticism towards graphs 'revealing' hidden insights, with the seemingly clear findings presented by the graph?

One way in which this tension was resolved is that the researchers performed themselves as "sober and modest", to use Shapin's (1984: 495) phrase, by describing their difficulties and the uncertainty around the 'findings'. They then demonstrated the constructedness and possible arbitrariness of the relatively clear clusters in the graph above by clicking through different settings to show different possible realities they suggested. So, just as many ethnographic STS accounts (perhaps in response to Woolgar's (1983) essay) reflexively draw attention to doubts and ambiguities and poke holes in their own authoritative statements, these researchers did the equivalent for their graph.

However, I think there is something else worth noting about this encounter. Munk, Madsen and Jacomy (2019) argue that visualisations invite people to read into them their own preconceptions of what is in the data. But what is interesting is that often these assumptions would remain unarticulated without the graph. In this particular case, the graph materialised an unspoken assumption, apparently held by many of the participants, that the UK was politically closer to the left of US politics and that a use of political technologies would fall along political lines. So while the researchers were highly sensitised to their assumptions about the data being baked into the "data cleaning" and ultimately informing the graph, they were less attuned to their own assumptions about what they might find. This suggests that instead of graphs being used to definitively demonstrate the existence of some phenomena, they could be used to provoke reactions, to materialise unspoken expectations and assumptions.

Encounter 3

The last workshop I want to discuss concerned online patient feedback and involved 13 participants from a variety of backgrounds: health researchers, former nurses and doctors, medical sociologists and experts on health insurance and digital health. The UK's National Health Service (NHS) among others have been attempting to process mountains of digital patient feedback using machine learning and a technique called sentiment analysis (which I will describe later). But these attempts to automatically extract the topic and (positive or negative) sentiment of the feedback, belied fundamental sociological questions about what patient feedback is for and whether or not patients, doctors and hospital managers understand it in the same way. For example, feedback might be used instrumentally to change policy, as an idle threat or as a cathartic unloading without expectation of a response.

The website Care Opinions (careopinions.org), which collects public and anonymised patient feedback, agreed to give us access to their platform, from which we downloaded one month's worth of anonymised feedback narratives, referred to as "stories", and their metadata. While the researchers in this workshop were closer to the topic compared to the researchers in the other two workshops, they were also, on the whole, less experienced with these sorts of tools. Some of the participants had worked on quantitative surveys and qualitative in-vivo coding, but never with web-based scrapers and network graphs.

One team, composed of three STS inclined ethnographers, was interested to see if there were any automated means of applying Dorothy Smiths' brand of textual analysis to these texts (Smith, 1978). As I explained in the opening vignette, after the experience of other workshops where groups too quickly started experimenting with tools like co-word maps, I purposely slowed things down and forced the group to analyse some of the feedback stories manually. However, I asked them to read while "thinking like a computer" – keeping in mind what aspects of their analysis could be automated.[9] I suggested to them, for example, that one could automatically extract all the nouns, 'the cast of characters', in Smith's language. They then picked a handful of stories and started highlighting the nouns.

The participants quickly encountered problems. One pointed out that it was unclear if the repeated nouns are the same entities each time: "the nurse" and "the paediatrician" could represent multiple people. They also noticed some interesting features of the texts, such as the way the opening sentences provided 'instructions' for how to read what follows: for example, to suggest who the story is addressed to, or who is potentially responsible or culpable for what happened. They also noticed that certain stories convey a moral economy through what Smith (1978) calls 'contrast structures' or the juxtaposition of two statements, implicitly rendering one as 'good' and one as 'bad'.

I had to admit to them that such implicit devices, which have to do with sequences and omissions in the text, are hard to capture automatically with current forms of automated textual analysis. We discussed possible automated approaches, such as creating different corpuses with key-word queries and then using co-word maps or using word trees (Wattenberg and Viégas, 2008) to pull out repeated phrasings. But each of these seemed to be dismissed as they discovered interesting patterns which the tools would necessarily miss. This is understandable: as researchers who study technology, they were accustomed to looking for what different technologies, like these tools of textual analysis, render invisible and leave out. This brings us to the slump I mentioned in the introduction. Mid-way slumps are common in workshops of this sort, and they happened to some extent in the other two workshops, but this one seemed more oppressive, possibly because these researchers were less technically adept overall or because I had forced them to confront the full texts first.

After some much needed coffee and pacing around outside the stuffy room, I thought of a new provocation for the group. Instead of trying to approximate close readings in an automated way or scaling up Dorothy Smith's approach, I asked them to start with a computational approach and

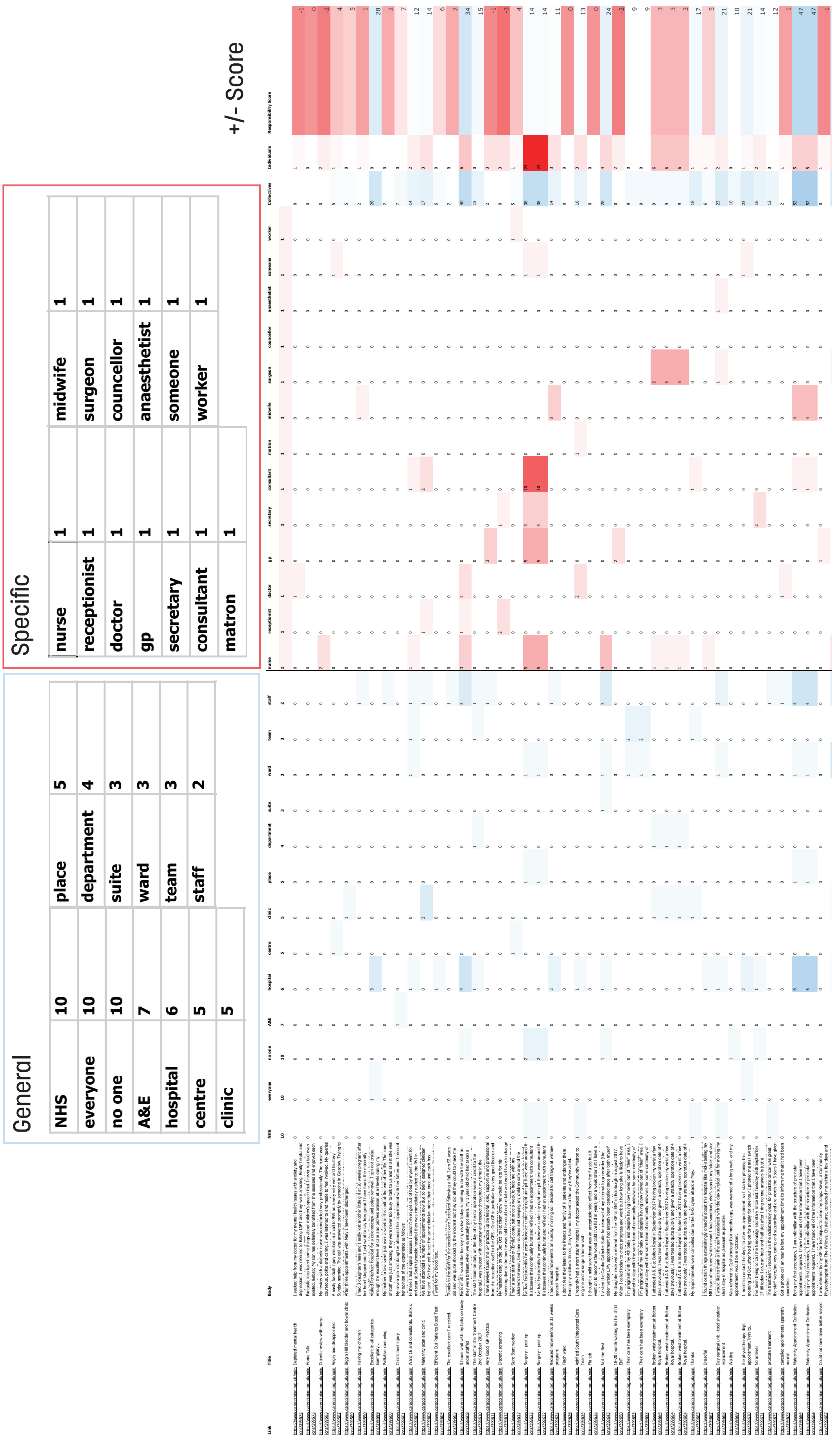

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Figure 3. Stories with cumulative generalised responsibility scores

modify it. I gave them the example of sentiment analysis, mentioned earlier, which Care Opinions already used in the backend of their website. Sentiment analysis, in its simplest form, works using a library of words that are deemed to be inherently positive or negative (ranging from -5 to +5). The words in a sentence are added up to produce a sentence score (taking account of basic modifiers like "not ____" and other rules).[10] The objective, as I put it to them, was then to come up with a system which mirrored sentiment analysis but improved on it by making it either more sensitive, more nuanced or more targeted to the specific problem of analysing patient feedback.

What the group arrived at, after some deliberation, was that entities named in the story could be conceived of on a spectrum of more specific to more general. "A nurse" or "the nurse" was more specific than "the staff", "the hospital", "the NHS" or the practice of medicine in general – and this has very different implications for how responsibility was being distributed in the text. They made a brief library of common nouns and pronouns and then assigned them rankings from 0 – 10. There was much joking about the absurdity of assigning number scores to these words, but the group seemed happier to commit to the process, given that it was undertaken with a sense of play. The words appearing in the text could be added up to determine what we called "generalised responsibility scores" for the story as a whole, and stories could then be colour coded (as in the above image), ranked or graphed in various ways.

Now, obviously ranking the generality of words also relies on faulty assumptions and requires abstracting words from their wider context, but such a provisional metric is still a more compelling or social scientific way of sorting texts than whether they are positive or negative. In addition, such an approach could be used to launch a critique or light parody of sentiment analysis and related techniques, without dismissing such automated techniques all together. For this reason, it could also make an interesting intervention in the field because, while doctors and hospital bureaucrats probably already have their preconceptions about 'positive' and 'negative' feedback, they likely do not have preconceptions of 'generalised responsibility' and may approach the sorting and analysis of stories with a more open mind. What practitioners would make of this metric, however can only be tested through dialogue with them, though both the medical professionals and the computer scientists in the room seemed to be intrigued by the approach.

The tension in this encounter, once again, manifested itself differently than in the other workshops. The group's scepticism towards the tools seemed to be based on a lack of fit between what the graphs could see and what *they* could see as textual analysts. Something was deemed to be 'lost' in the translation of full texts to texts-as-data; between close and 'distant' reading. While this is understandable given the immediate juxtaposition of the two, it puts STS scholars and anthropologists in the odd position of being "realist" about texts as one participant put it after the workshop – believing that textual extracts, or rather manual readings of them, are more real than computational representations of them. Yet, in other situations, the same researchers might protest that these texts are also a very partial, performed account of another reality: the rich social world of interactions in a hospital.[11]

It was also interesting that the participants seemed stuck when starting from qualitative close reading and approaches like ethnomethodology, but by starting with a tool and asking how to modify it, they actually managed to engage in the design of a computational approach. This seemed to satisfy their scepticism but also allowed them to create something legible to computational researchers in the room at the same time.

Discussion

I think these brief vignettes demonstrate that, while there is certainly a genuine will by many researchers to have closer collaborations between disciplines and make use of new digital tools, there are still tensions, disciplinary baggage and resistances which need to be dealt with. What I want to talk about in this section is how to characterise these tensions as well as ways of overcoming them. I described the first tension in terms of disappointment that is a disappointment at the tools not performing as they are supposed to, instrumentally speaking. The second I described as a wariness toward the assumptions imbued in these tools and the third I described as a scepticism toward the reduction of one reality to another.

I think that each of these tensions, interestingly, entail some version of the ironic fallacy as understood by Woolgar (1983): applying certain criteria to one's disciplinary other, without applying the same criteria to one's self. In the first case, the disappointment arose because interpretivist researchers seemed to have instrumental expectations of the graphs but did not imagine an instrumental use of their own methods in the service of making better graphs. In the second, the group were wary of the assumptions required to make the graphs work but then were surprised when confronted by other assumptions they had held which were materialised by the graph. In the third, the researchers critiqued computational representations for failure to live up to another set of qualitative representations which were, at least in the heat of the moment, treated as somehow less constructed.

This is not to criticise these researchers or propose that they did something wrong. Rather, I think that slipping into these positions is an occupational hazard of doing interdisciplinary work, which others working in these ways will hopefully recognise. These insights come from discussions with the participants afterwards who, with the benefit of hindsight, regretted various ways things played out. In fact, one of the main effects of these workshops has been to make the author more aware of his own tendency to fall into this ironic stance.

As for the responses, which purport to address or contain these frictions or tensions, the first example I characterised as 'diplomatic',[12] keeping methods separate but relations respectful, the second was a 'modest' and 'reflexive' response and the third was a playful appropriation of a method by a team who might otherwise have rejected it. The first response should be familiar to many researchers and the second is very common in Digital STS circles, but the third I think is more surprising. How might we make sense of these negotiating strategies? Do they repair or reproduce the ironic distance which seems to have prompted them?

As Katie Vann (2010) notes, when Stengers invokes a distinction between 'humour' and 'irony' (2000), she is possibly drawing on a distinction between humour and irony made by Deleuze and this use of irony is subtly different from the one Woolgar deploys. She refers to an obscure discussion of Sadism and Masochism in which Deleuze distinguishes the two not as positions in a fetish relationship but in terms of the 'scenography' employed by the two authors de Sade and Masoch in response to modernity – the situation in which 'the law' is no longer grounded on founding principles (Vann, 2010). Sade is 'ironic' because he critiques the law by 'ascent to higher principals', in his case, committing to *Evil*. This exposes the law as comparatively without principles. Masoch is 'humorous' because he subverts the law by 'descent to consequences', adopting the law and the punishment in the absence of any crime. This also exposes the absurdity of the law, by pushing it to its logical conclusion.

This schema does not seem very helpful for the first example because if we consider 'the law' to be something like the rules and routines of a discipline or method then not much subversion was happening, because researchers and methods were kept separate. If we wanted to use the casual metaphor of sexual relationships we might characterise this as a loveless marriage, or at least a marriage without anything 'kinky' going on. Now on first glance we might say that the second example is humorous because it is masochistic, in the sense that it employs a strategy of self-effacement and self-critique. We might then say that the third example is ironic because it takes on the tools of data analysis 'ironically', in the colloquial sense of, 'not how they were intended', or with a distancing wink. But I think Vann's (2010) analysis suggests otherwise. Perhaps the second example is actually ironic because it exposes the laws of computational methods to be absurd by recourse to a higher principle: in this case reflexivity or a belief in the constructedness of all knowledge (a principle which both ethnography and data analysis are both subject to). It also could be the case that the third example is actually humorous because it takes up the laws of computational methods and pushes them to their logical conclusion, to absurdity: if we are going to rank entities, then why not *these*. While Stengers and possibly Deleuze have a normative commitment to humour, which is imminent as opposed to irony which is transcendent, Vann argues that both are possibly part of the same move and both have the potential to "…turn mutual interlocutors into equals" (Vann, 2010: 86).[13] So without advocating one approach over the other, I want to suggest that both may be productive strategies in relation to different sorts of tensions. Both retain the sceptical edge of much critical work on data and computational approaches but, I argue, do so in more productive ways than mixed methods diplomacy because they involve a breaking down or questioning of roles.

However, this remains only a potential in these two later examples because is not clear that the absurdities made present were allowed to cut both ways. In the second example, we did not *quite* allow the graph to trouble our assumptions (only after the fact) and in the third example a computational guise was adopted but not for long enough to produce any defensible knowledge from it. It was still not clear in these instances that these researchers have allowed computational methods and disciplines to inform their own perspectives, or to paraphrase Woolgar (1983, 262), "…make [data] science talk to sociology rather than the other way around."

Conclusion

The purpose of this paper has been to consider empirically some of the possible disciplinary or methodological tensions arising from the rapid proliferation of digital data and computational approaches to analysing it. This is important because, while disciplines like anthropology, STS, sociology and HCI have made many legitimate political and methodological critiques of certain forms of computational analysis, they are not always in a position to influence their development. This may be a result of the ironic distance effected through these critiques, or of the way different existing frameworks for collaboration keep these disciplines in their lanes, muffling dissenting views. I argued that understanding what these tensions are composed of, and thus how they might be overcome, might require some poking and probing to bring them out. I gave three examples of some practical tensions as they played out in a series of workshops and how they were negotiated. By negotiated I do not necessarily mean that these tensions were resolved, but also put 'on the table', 'decomposed' (Kaltenbrunner, 2014) or folded into the eventual outputs.

I should clarify that due to the brief nature of these workshops, these tensions necessarily pertained to the initial, exploratory stages of a study, rather than the consolidation of findings, questions of validity and reliability, which have been the crux of mixed methods debates. More empirical investigations should be undertaken on these later aspects of collaborations in order to understand how the normative commitments of data scientists and computer scientists interact with the concerns of anthropologists and micro-sociologists and particularly how the divergent end-products of research are negotiated between disciplines.

In any case before these more equitable or long term collaborations can proceed, 'interpretivist' researchers might need a stronger sense of *different* roles they can take on in the proceedings and how they could make use of digital tools in ways that address their own wariness or scepticism. What I hope to have accomplished in this paper is to encourage these researchers to examine their own baggage and normative commitments and approach these computational tools with 'humour' or 'irony', as the situation demands. Thus interdisciplinary collaborations could be thought of in terms of what Woolgar (1983) calls 'irony as a project', but this time a collective one, aimed at opening up (but not settling) the problem of what makes accounts of reality adequate and for whom.

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Notes

1 I am reluctant to definitively name the two sides of this supposed conflict because it can take on many forms and both 'sides' of the conflict are not as monolithic as they seem. Throughout this paper I have attempted to stick to the specific discipline, methods or roles being negotiated in particular situations.

2 By corporate they have in mind Suchman's celebrated work for Xerox which helped shape the discipline of HCI (Suchman, 1987; see also Bell, 2011), by critical they give the example of Star's work bringing out the politics of digital infrastructures, by inventive they have in mind forms of 'making and doing' (Downey and Zuiderent-Jerak, 2016) and by inquiry they invoke more speculative, open-ended investigations (Wilkie et al., 2015).

3 As Denzin (2010) notes, mixed methods have, more often than not, involved quantitative researchers employing simplistic, impoverished versions qualitative methods (like interviews and participant observation) in the service of quantitative methods.

4 While there is not space to engage with nearly a half-century of debates about these frameworks, it is important to note than many of the most influential of them were developed in a time defined by different methods (sample surveys, interviews and ethnography) and different disciplinary tensions ("the paradigm wars") and they might require more fundamental rethinking in an age of messy, found data culled from social media platforms, open government databases (Burrows and Savage, 2014) and automated types of analysis like Natural Language Processing, Machine Learning, network graphs and other types of visualisations.

5 There are plenty of approaches within anthropology which offer strategies for rethinking relations between the ethnographer and her informants. Marcus and other have developed the notion of "para-sites" which describe forms of epistemic collaboration with expert communities, however, crucial to this programme is one of deferral to the epistemic expertise of informants (Gilbert, 2015). Co-laboratories (Rabinow et al., 2008) have been involved in questioning the role of extended fieldwork (with a single author) as the primary method of anthropology, but have rarely resulted in anything like the STS examples of anthropologists taking up quantitative tools for their own ends.

6 It is worth pointing out that in Kaltenbrunner's (2014) study, the participants rethinking of their working relationship began when the author shared with them some early STS reflections, which seem to authorise these more radical moves.

7 Visualisations also however raise other sorts of concerns about literacy, and what they make invisible as well as visible (Coopmans, 2014; Kennedy et al., 2016).

8 Interestingly, the terms "qualitative" and "quantitative" were rarely invoked in any of the workshops, except in these moments of switching between approaches.

9 One participant confessed that for several days after the workshop, she continued to think like a computer, constantly looking for things to count.

10 This approach of course ignores relationships between words or between sentences, and, notably, sarcasm. It would certainly need tweaking in relation to medicine where positive outcomes can be related to negative words like “disease”, “death”, “surgery”. “Murder” is taken as an extremely negative word even if the sentence was “I could murder a pizza”.

11 This is why, thinking of these graphs as a bad simplification of close reading is “too simple” to borrow Lynch’s (1988) phrasing. Lynch’s point is that we should not think of scientific representations which are presented as simplifications of other images or phenomena as reductions, but as active transformations into something else.

12 This is not diplomatic in the sense used by Stengers in ‘The Cosmopolitical Proposal’ (2005), which suggests a participant who is themselves put at risk in the scenario.

13 More specifically, Vann suggests, the Sadistic form of Irony and the Masocistic form of humour are both humourous because there are responses to a situation (modernity) in which the law is not grounded.