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Attaining the Stable Movement of Knowledge Objects through the Swedish Criminal Justice System: Thinking with Infrastructure

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Abstract

This article thinks with infrastructure about the stable movement of knowledge objects such as crime scene reports, traces, and order forms through the Swedish criminal justice system. Infrastructures span different communities and borders; the criminal justice system is made up of necessarily disparate epistemic cultures. Thus, they share a central concern: Both aim for stable movement from one context to another. Thinking with infrastructure, the article argues, makes it possible to widen analytical focus and capture the structures and the continuous work that resolve the tension between different sites and thus enable the stable movement of knowledge objects. Using sensibilities from infrastructure studies– for the resolution of tensions, for continuous maintenance, and for inequalities – the article argues that the criminal justice system *enacts* the knowledge objects' stability across epistemic cultures. In other words, the stable movement of evidence-to-be through the Swedish criminal justice system is the result of infrastructur*ing*, that is, of its continuous creating of conditions that facilitate movement and create and re-create stability. This perspective may be useful for studying the movement of knowledge also in other contexts.

Keywords: epistemic cultures, infrastructure, stable movement of knowledge, knowledge objects, standards, alignment work, inequalities, forensic evidence, criminal justice

Introduction

This article brings sensibilities from infrastructure studies to analyzing the movement of knowledge objects related to forensic evidence through the Swedish criminal justice system. There, the stability of the evidence-to-be as it is moved through a collaboration of disparate epistemic cultures (Knorr Cetina, 1999; see also Kruse, 2016) is a central concern: forensic evidence is only understood as legally secure if it can be perceived as unaltered from the crime scene to the courtroom. However, the epistemic differences implicit in the collaboration of the different professions make it difficult for knowledge objects (such as expert statements or crime scene reports) to carry meanings stably. The epistemic differences between authors and recipients mean that knowledge objects are not always read in the way they were intended to. Thinking with infrastructure about the criminal

justice system's movement of knowledge objects makes it possible to see the continuous work which undergirds this movement and through which the criminal justice system enacts the stability of the knowledge objects being moved through its epistemic cultures.

In this way, this article aims for a new and deeper understanding of how the criminal justice system moves knowledge. Its framework and understanding may also be helpful in analyzing the movement of knowledge in other contexts; thus, the article in extension also aims to contribute to the development of STS theory.

Infrastructuring across epistemic cultures

This article's analytical point of departure is understanding the different professions in the Swedish criminal justice system as different epistemic cultures (Knorr Cetina, 1999) and paying attention to how epistemic differences between them make the stable movement of knowledge difficult.

Epistemic cultures produce and maintain specific understandings of what valid knowledge is and how it should be produced and understood. This also implies that knowledge produced in one context or epistemic culture is not necessarily understood in the same way in another. This makes it difficult to move knowledge stably between them. While *knowledge objects* travel, they may be understood quite differently by producers and recipients, and thus the *knowledge* they are meant to move does not remain stable. Such mutability is, of course, not necessarily problematic. Quite the opposite, mutability or flexibility – in de Laet and Mol's (2000) term, 'fluidity' – may be a prerequisite for mobility. In the criminal justice system, however, mutability *is* problematic. There, epistemic differences, while necessary in other ways, are an obstacle to the stable movement of knowledge.

In order to think about how the Swedish criminal justice system makes possible and enacts the stability of knowledge objects that travel through its different epistemic cultures, this article thinks in terms of infrastructure (e.g. Larkin, 2013; Star and Ruhleder, 1996) and infrastructuring (Karasti and Blomberg, 2018). STS-informed scholarship on infrastructures has underlined their relationality. As Star and Ruhleder put it, infrastructures resolve *"the tension between local and global"* (1996: 114; italics in original) – or, perhaps, rather the tension between different kinds of local. They famously propose asking "*when* – not *what* is an infrastructure" (Star and Ruhleder, 1996: 113; italics in original), pointing out that an infrastructure becomes one through being embedded in relationships and practices. As infrastructures facilitate the movement of things or people across distance, often spanning different communities and borders, they share the criminal justice system's central concern: Both aim for stable movement from one (local) context to another.

Infrastructure studies, then, offer sensibilities that can be fruitful for thinking about the mobility of knowledge, especially the work that the facilitation of movement requires: Infrastructures such as roads (Harvey and Knox, 2015) and canals (Carse, 2012), must be set up and maintained, which requires concerted efforts across several communities and thus necessitates relationships. Establishing a large sociotechnical system like an infrastructure means establishing a new set of relationships (Carse, 2012) between a number of human and non-human actants – for example through standards (Star, 2010). In addition, infrastructures are vulnerable to both tampering and decay (Öhman, 2016), thus requiring constant attention.

Linking infrastructures with knowledge is not a new concept. Implicitly, they figure in quite a few STS notions dealing with the movement of knowledge, for example the trading zone (Galison, 1997), the boundary object (Star and Griesemer, 1989), or the network that Actor Network Theory (ANT) describes as being co-constructed with the immutable mobiles that are moved through it (Latour, 1983: 155).

Other scholars have paid attention to the role of infrastructures in the production of knowledge – recently in a series of theme issues of *Science & Technology Studies*[1]. In the context of collaborations, scholars have also spoken of data or information infrastructures, especially when discussing the sharing of data (e.g. Borgman et al, 2013; Edwards, 2010; Edwards et al, 2007; Parmiggiani and Monteiro, 2016; Plantin et al, 2018), and often in terms of how such infrastructures

consist of more than physical structures. Monteiro et al (2013: 576), for example, have, drawing on Hanseth et al (1996), pointed out that technologies and artefacts in dispersed collaborations are embedded in and sustained by standards, norms, and practices – i.e. information infrastructures – that enable them to function as tools for collaboration. Thus, this work strongly resonates with Bowker's infrastructural inversion (Bowker et al, 2009: 99), underlining the importance of establishing and ordering relationships between different actants. One of the central points in this scholarship is that "our knowledge of nature is inextricably entangled with the infrastructure that we use to gather data about nature" (Parmiggiani and Monteiro, 2016: 32).

I will use the figure of the infrastructure to think about the structures and relationships and the work associated with them that support the (stable) movement of knowledge. Specifically, I want to bring three sensibilities from infrastructure studies to my analysis of the stable movement of knowledge objects through the criminal justice system.

The first sensibility is attention to the resolution of tension, that is, to how the relationships that are so crucial for an infrastructure's functioning are constituted and organized. As Star and Ruhleder (1996: 114) point out, infrastructures can successfully span different places and communities because they resolve the tension between them. In this resolution, standards play a central role. With Bowker and Star, standards are "a set of agreed-upon rules for the production of (textual and material) objects" that "spans more than one community of practice (or site of activity)"[2] and "persists over time" (Bowker and Star, 1999: 13). As such, standards are an organizing principle that can resolve at least some tensions between different sites, for example by establishing shared understandings; as I will discuss, they can also contribute to the stable movement of knowledge.

This sensibility enables analytically capturing and acknowledging different understandings, interests, and involvements in a shared endeavor and the bridging of such differences. In other words, it is a sensibility that can speak also to differences and tensions rooted in disparate epistemic cultures.

The second sensibility I will bring to the analysis is for the continuous – and often invisible – work that sustains the resolution of tension. Infrastructures are vulnerable and thus require continuous maintenance to remain functional. Dams for example are vulnerable in a number of ways: the intractability of large bodies of water, dangers like earthquakes and flooding, and the peril of sabotage all make them susceptible in different ways (Öhman, 2016). Thus, they are in constant need of supervision and stabilization; the tension between different kinds of local is not resolved once and for all but must be resolved continuously.

Similarly, I will maintain here, do the relations and standards that resolve tension between epistemic cultures require continuous work to remain functional. In addition, not only do standards need maintenance; standards also cannot be built to be applicable to every possible case and thus have limitations. As Star famously points out, "there are always misfits between *standardized* or *conventional* technological systems and the needs of individuals" (Star, 1990: 36; italics in original).

These inevitable misfits mean that the tensions between different kinds of local are not and cannot always be completely resolved through standards alone. Resolving them requires additional work. This does not necessarily need to be copious or conspicuous work; Star (1990: 36ff) for example discusses how scraping onions off one's hamburger in a fast food restaurant addresses the misfit of standards with individual needs – in her case, an allergy to onions. Fast food, she explains (Star, 1990: 36ff), is prepared according to standards that make it possible for the restaurant to serve guests quickly and for the chain's guests all over the world to know beforehand what their order will get them. But for someone who is allergic to onions – a standard ingredient in hamburgers – obtaining a hamburger without onions can be time-consuming and thus the opposite of fast food. Ordering a standard meal and scraping off the onions, then, is a comparably simple resolution of the clash – but it does involve work.

Such work, I argue, supports the standards, resolving the tension the standards alone

cannot resolve – and at the same time keeping the standards functional. In other words, like physical infrastructures require maintenance, so do standards require work to ensure that they can continue to resolve the tension between sites.

In paying attention to this kind of work, I will take inspiration from Strauss's 'articulation work' (Strauss et al., 1985: chapter 7; Star, 1991: 275) that highlights the often invisible or unnoticed work that makes the work perceived as the "real" work possible. An example are nurses preparing patients for examination by a doctor (Strauss et al, 1985: 156). I will combine his notion with Vertesi's work on producing "moments of alignment" (Vertesi, 2014: 268). She draws attention to the "seams" between infrastructures with different standards and "how actors skillfully produce moments of alignment between and across systems" (Vertesi, 2014: 268) to "produce a shared experience of seamlessness" (Vertesi, 2014: 277) in environments that rely on multiple, overlapping infrastructures. That is, she argues that seamlessness is a fleeting state that must be repeatedly produced.

Her "seams" are comparable to Star's misfits (Star, 1990: 36); in both, the resolution of tension causes new tensions and in both, this new tension must be dealt with for the "real" activity to be able to continue. Like articulation work, this is work that, I will show, is not always noticed or acknowledged as part of the primary work. I will call such work of supporting standards *alignment work*, since this work (temporarily) aligns different epistemic cultures, creating an experience of seamlessness between different sites in the criminal justice system.

With a sensibility for this work, I want to draw attention to the continuous work that is performed around interprofessional standards in the criminal justice system, supporting and complementing them. In other words, this is an analytic sensitivity to the constant and sometimes unnoticed work that nevertheless is essential for maintaining the resolution of tension between epistemic cultures and thus for the movement of knowledge between them.

Connected to these two sensibilities is a third one, one for power and power relationships. Infrastructure studies discuss how the relationships manifested in and through infrastructures need not be symmetric or equal – indeed, infrastructures can materialize existing inequalities (e.g. Hoag and Öhman, 2008; see also Larkin, 2013); in addition, infrastructures have often been a part of domination in colonial processes (e.g. Bear, 1994; Chikowero 2007; Öhman, 2016; von Schnitzler, 2018).

Infrastructures are intertwined with power and inequalities in other ways, as well: who has the power to draw together the resources and relationships required (Harvey and Knox, 2015), to whose benefit and at whose cost infrastructures are established (Öhman, 2016; Carse, 2012; see also Edwards et al, 2007), who is given access (Anand, 2012; Appel, 2012), and who does the maintenance work and to whom this work is (in)visible. Infrastructure studies have also drawn attention to how the "same" structure can do radically different things for and be understood radically differently by different people that (have to) engage with it (e.g. Harvey and Knox, 2015; Öhman, 2016).

Öhman discusses such inequalities on an existential level, showing how reservoirs can be a very useful part of the national electricity infrastructure at the same time as their weak ice covers in winter pose a lethal danger to the local (and indigenous) people (Öhman, 2016: 67ff). This resonates – albeit on a much larger scale – with Star's discussion of the "misfits" (Star, 1990: 36) between standardized systems and individuals: the same standards that are meant to make people's lives easier and more efficient make life considerably more complicated and difficult for those considered non-standard. In other words, standards exclude at the same time as they expedite, and whom they exclude (and benefit) is a political question.

As knowledge and power are intertwined (e.g. Foucault, 1977) – not least when it comes to what is accepted as valid knowledge (e.g. Harding, 1998; Shapin and Shaffer, 1985; Verran, 2001) – paying attention to inequalities associated with its movement seems only prudent. However, I certainly do not mean to say that there are inequalities of a level comparable to those discussed in connection with physical infrastructures between the different professions of the Swedish criminal justice system – to do so would be to trivialize suffering. What I want to do, instead, is to use this sensitivity for power to trace

how different professions engage with the resolution of tension between different sites.

Star's work is particularly helpful here: Maintaining standards despite inevitable misfits, she explains, "often involves the private suffering of those who are not standard" (Star, 1990: 43). With this, she draws attention to not only the marginalization implicit in standardization but also to the mainly invisible work that the engagement with standards requires of some people in case of misfits, for example the rather mundane scraping of onions off hamburgers or other ways of closely monitoring one's food when eating out. Similarly, co-operations that depend on standards often rest on the invisible work of only some people and not others (Star, 1990: 36-44).

Which work is visible and which is (made) invisible is also a question of power relationships. Invisible work, for example the articulation work discussed by Strauss et al (1985: 151ff), is typically performed by occupational groups lower down in the hierarchy, whereas visible work is typically performed by higher-status professions. In medicine, for example, coordination of work or giving patients emotional support (Bowker and Star, 1999: 229ff), both largely invisible yet essential for health care, are performed by nurses, whereas the visible work is performed by doctors. In other words, the issue of (in)visibility and, consequently, recognition is intertwined with status and power.

Thus, the sensibility for power and power relationships can draw attention to invisible versus visible (alignment) work and control over one's work. This is a sensibility that, I will argue, draws attention to inequalities embedded into the stable movement of knowledge across even relatively equal epistemic cultures.

These three sensibilities are of course not the only possible ones to take away from infrastructure studies. I have chosen to focus on them because they offer a way of thinking about facilitating movement across contexts – ideally, without losses or changes – and about work that is performed away from but still essential for this movement. This, I will show, is helpful in analyzing how stability is enacted in the movement of knowledge across the epistemic cultures of the criminal justice system.

Methods

My empirical material comes from two different but related ethnographic studies in the Swedish criminal justice system. Between 2008 and 2012, I have studied how the criminal justice system's different professions collaboratively produce and use forensic evidence (see Kruse, 2016), conducting ethnographic fieldwork at a public prosecution's office, a criminal investigation division, a crime scene division, and three units of the NFC (the National Forensics Centre, Sweden's state-run and only forensic laboratory; then called the Swedish National Laboratory of Forensic Science). I also observed a number of trials in district court and conducted formal interviews with forensic scientists, prosecutors, district court judges, defense lawyers, and a crime scene technician.

In my analysis, I took inspiration from Grounded Theory (Glaser and Strauss 1967), looking for patterns as well as for contradictions. My analytic focus was on understanding what was important to practitioners in the different parts of the criminal justice system and how their perspectives related to each other and their collaboration. This made it possible to trace the production of forensic evidence through the criminal justice system as a whole, from the crime scene to the verdict, and how the different professions with their different competences contribute in different ways to this production (Kruse, 2016). It also brought out the occasional frictions between professions in the collaboration – and made these frictions understandable.

The question that stayed with me was how the criminal justice system organizes for and makes possible this collaboration. Thus, the second study focused on crime scene technician training at the NFC, a site where two of the criminal justice system's professions – forensic scientists and crime scene technicians – meet for an extended period of time. The training prepares the crime scene technicians not only for examining crime scenes but also – and as importantly – for mediating between the police and prosecution on the one hand and the forensic science laboratory on the other: They will commission analyses from the laboratory (on the investigation leader's orders) and participate in formal meetings and informal

conversations with police investigators, prosecutors, and forensic scientists (in varying constellations) to help align different requirements and understandings into a working cooperation. In other words, the training, a course spread out over a year and alternated with the students' regular work, is one way of setting up and supporting cooperation across professions.[3]

My fieldwork there took place with the class of 2013, consisting of ten men and ten women. With few exceptions, I observed all of the lectures, exercises, and crime scene examinations, listened to and participated in discussions over coffee and lunch, and conducted informal interviews with both teachers and students.

In the analysis of this material, I have, again, looked for both patterns and contradictions. Elsewhere, I have written about the crime scene technicians' professionalization and role in the criminal justice system (Kruse, 2020a) and their professional socialization. For this article, I have analyzed the material with a focus on how the students were prepared for their mediating role and how different understandings and perspectives were (or were not) addressed and negotiated. I chose this focus in an attempt to understand the collaboration of different professions in the criminal justice system in spite of different understandings and perspectives.

Epistemic cultures in the Swedish criminal justice system

The different professions or epistemic cultures (Knorr Cetina, 1999) in the Swedish criminal justice system share a rhetoric of willing cooperation as well as the goal of solving crimes in a legally secure manner – i.e. of working towards that the right person is convicted for the right crime – but they contribute to that cooperation and goal in different ways and at different times. Crime scene technicians examine the crime scene; forensic scientists perform laboratory analyses on the traces the crime scene technicians have recovered; police investigators set the analysis results into a wider context through for example interviewing, suspects, and witnesses; prosecutors assemble the whole of the evidence into a court case; and judges and lay assessors evaluate the whole of the evidence put before them (see also Kruse, 2016).

They also have very different backgrounds and competences: Crime scene technicians with their police backgrounds and specialization in forensics have broad knowledge and skills in forensic technologies as applied to crime scenes. Their competence is in finding and recovering a wide range of traces and analyzing crime scenes; their concerns are about contributing to investigations as a whole and about protecting society. Forensic scientists with their science backgrounds are specialized in one or a few forensic laboratory analyses. Their competence is in analyzing single traces and evaluating the result; their main concern is the correctness of their expert statements. Police investigators with their policing backgrounds work predominantly with people, not crime scenes or traces. Their competence is in assembling evidence, especially through interrogation, thus providing a context to the forensic evidence; as they meet the people involved in investigations, their concern is with them as well as with society as a whole. Prosecutors are specialized in the law; their competence is in bringing together the specifics of a case with legal rules and requirements. Their concerns are about assessing and assembling all of the evidence into a convincing whole. Finally, judges are, like prosecutors, specialized in the law, but their competence is in assessing the evidence as a whole, and their concerns revolve around legal security (Kruse, 2016).

These epistemic differences are highlighted by occasional frictions. For example, crime scene technicians (and police investigators or prosecutors in their capacity as investigation leaders) may want traces analyzed at the NFC that forensic scientists deem unnecessary. Conversely, forensic scientists sometimes express dissatisfaction with the quality of the material they have been sent, when in at least some of these cases, the crime scene technician in question probably has done the best they could under the circumstances. In addition, where forensic scientists speak about traces and their analysis in terms of assessing hypotheses, police investigators and crime scene technicians also talk about the value of laboratory analysis turning up the unexpected – e.g. a cold hit – that may not be of immediate use but can lead to useful evidence or information (e.g. through

interrogating someone who may not otherwise have drawn the police's attention). That is, while both the forensic scientists' and the crime scene technicians' expertise is in forensics in a wider sense, not only is the forensic scientists' expertise deeply specialized whereas the crime scene technicians' is general, the different professions also have different experience – of the ordered laboratory versus the disorderly crime scene – and different roots. These differences manifest for example in different views on both crime scenes (see also Kruse, 2020a, 2020b) and specific traces and may lead to frictions.

Furthermore, prosecutors and police investigators are much more interested in how a laboratory result fits into the whole of a case than they are in the intricacies of the probabilistic reasoning with which the forensic scientists evaluate it – nor do they have the training that makes these intricacies easily accessible to them. Thus, there is sometimes friction between forensic scientists and prosecutors about what a piece of evidence means (Kruse, 2013).

Even though such frictions were not a regular part of everyday work, they were still common enough that everyone had stories of them. Thus, they may not paint a representative picture of the Swedish criminal justice system's cooperation, but they do illuminate the complexity of the collaboration of disparate epistemic cultures: their different contributions to the production of forensic evidence are all essential, but their epistemic differences also make for mis- and differing understandings. In other words, it is both necessary and difficult to move knowledge between the criminal justice system's different professions.

Knowledge objects

The criminal justice system's collaborative production of forensic evidence takes a form that could be likened to a relay – while all of its epistemic cultures contribute to the collaboration, they do so successively, not simultaneously. The 'batons' in this relay, then, are the knowledge objects that are moved from epistemic culture to epistemic culture and are expected to transport the evidence-to-be from the crime scene, through the forensic laboratory and the criminal investigation division to the prosecution and finally the court.

These knowledge objects are, for example, forensic expert statements,[4] crime scene reports, traces, and order forms. To my interlocutors, it was of utmost concern that recipients and producers understand them in exactly the same way – in other words, that the knowledge objects remain stable.

Consider the reports Swedish crime scene technicians write after their examination to convey their results to police investigators, prosecutors, defense lawyers, and judges. These reports contain a description of the crime scene, the traces the technicians have found and recovered, and, ideally, the results of the analyses the NFC has performed on the traces. The reports conclude with the technician's assessment of the crime scene as a whole, explaining the technician's conclusions (and on which traces and analyses these conclusions are based) to the reader.

When forensic scientists teach the writing of these reports, they emphasize the importance of readers' understanding why the crime scene technician made the assessment they made and how strong the conclusions are. Their concern is twofold: The first one is the reports' durability. If the technicians convey exactly what their conclusions are based on and how they arrived at them, new information – sometimes brought to the investigation long after the crime scene examination – will not render the report obsolete. Even if the conclusions should be refuted, the descriptions and explanations in the report will make it possible to reassess the technicians' findings in the light of the new information. Thus, the report will remain useful even if the case should turn into a cold case.

The second concern is with legal security. Misunderstanding the reports – including over- or underestimating the strength of the conclusions – may affect the outcome of the investigation and subsequently the verdict.

Accordingly, standardized expressions of "value", based on Bayesian reasoning, are meant to harmonize the writing and the reading of the text. The NFC has developed and teaches the technicians to use a scale of "value," that is, of how strong they consider their conclusions.[5] The expressions range from the results of the crime scene examination "showing" that something has happened

– the strongest expression – to giving "strong support", giving "support" and speaking "neither for nor against." The results can also show or give support to something not having happened. For example, a complete lack of pertinent traces at the scene of an alleged burglary might be concluded to give support or perhaps even strong support that the burglary did not happen.

The rationale behind such uniform expressions is to make it easier for readers – mainly conceptualized as prosecutors[6] – to understand the reports. Instead of technicians making up their individual systems of expressing their conclusions and readers having to figure out how strong the technician intended the conclusions to be, there is only one standardized set of expressions that always means the same. This reasoning resonates with the laboratory's own evaluation and communication practices; the NFC expresses the strength of their laboratory results on a graded numerical scale paired with similar expressions (cf. Nordgaard et al., 2012; Kruse, 2013). In other words, the knowledge object is templated in a way that is meant to transport meanings intact.

Another type of knowledge object in the criminal justice system are the traces that crime scene technicians recover from crime scenes, even though they might not appear as self-evident knowledge objects. Since they are tangible objects – for instance fingerprints captured with powder and tape, hairs in envelopes, glass fragments in plastic jars – their movement from the crime scene to the laboratory may seem like merely a practical matter.

However, as soon as a trace is being abstracted from the crime scene (cf. Latour, 1999: 48 ff; see also Kruse, 2016: chapter 5), it is the product of the technicians' work and understandings and thus decidedly material-semiotic (cf. M'charek, 2016: 16). And although traces are not documents (but they certainly are part of a bureaucracy) and are not expected to be as easily read by a wide range of recipients as a document, they still are expected to carry decipherable meaning from the crime scene to the laboratory, albeit decipherable only through (successful) analysis.

This decipherability depends on how the trace is recovered. For example, as crime scene technicians learn during training, bodily fluids decompose under certain conditions, which hampers DNA profiling; thus, they are taught to mop up such fluids with forensic cotton swabs and then seal the swabs into special paper bags. Fibre analysis requires care to avoid cross-contamination or loose fibres falling off and being lost; accordingly, crime scene technicians are taught how to turn a garment and brown paper into a "Swiss roll" that keeps the disturbance of fibres to a minimum, how to examine clothes from different persons in different rooms and by different technicians, and how not to cross-contaminate these rooms.

These methods aim to stabilize the traces across contexts and over time. Their application, albeit not always as straightforward as it seems in the classroom (see Kruse, 2020a: 72ff), is what makes the traces' movement from one epistemic culture to another seem "merely" a practical matter. In other words, even though traces are not written documents, they – and their decipherability – are still shaped by their "authors."

The last knowledge object I will discuss here are the electronic order forms that accompany each trace to the forensic laboratory. Like the crime scene reports, these forms are documents, but their projected lifespan is much shorter. Besides specifying which analyses (from a catalog) the trace is to be submitted to and giving contact information, order forms are meant to convey relevant parts of the larger investigation's context to the laboratory.

The latter has to do with the laboratory's evaluation practices: After the forensic scientists have established a match between a trace from a crime scene and a comparison sample, they evaluate how "strong" the conclusions are that can be drawn from it. To do that, they might, depending on the type of analysis, need quite a bit of information; a fibre analyst, for example, may need to know under which circumstances and when (in relation to the presumed crime) the clothes she is analyzing have been seized. In other evaluations, the forensic scientist may not need to know details.

When filling out the order forms, the crime scene technicians are to convey what they consider the relevant parts of the investigation to the forensic scientists: They convert the question

the investigation leader wants the trace to answer – for example whose fingerprint they found – into an analysis code from a catalog, and they organize and convey information from the investigation in a way that is relevant for the forensic scientists and their analysis (see also Kruse, 2020a: 72ff). In other words, these order forms contribute to making the traces decipherable in a way relevant to the investigation.

All of these knowledge objects are created in order to move decipherable evidence-to-be between the criminal justice system's epistemic cultures. My interlocutors talked about their concerns in realtion to this movement in terms of traces deteriorating or getting damaged, of vital information getting lost, and of readers misunderstanding – all of which, they explained, would affect legal security. From an STS perspective, my interlocutors' foremost concern thus is the *stability* or, to speak with Morgan, the *integrity* (Morgan, 2011: 12) of the knowledge they are moving. That is, they want the knowledge objects to travel as a "stable configuration" (Law and Mol, 2001: 611) that is understood in exactly the same way everywhere in the criminal justice system.

But it seems that such stability is elusive, and not only because of epistemic differences. Due to the relay character of the collaboration, my interlocutors also often did not know how their knowledge objects were received. Reception takes place in the producers' absence, and it is unusual for members of the criminal justice system to be able to track what happens to a case to which they have contributed. The frictions that sometimes occur around knowledge objects in the criminal justice system when producers and recipients do meet – for example in meetings, phone calls, informal conversations, or when witnessing in court, all of which are typically rooted in recipients' questions (cf. Kruse, 2016: 112ff) – contribute further to their uncertainty; they made them wonder how other knowledge objects were received. Forensic scientists repeatedly talked about wondering how often prosecutors close cases instead of taking them to court because they mistakenly think that the evidence is too weak. In other words, in the cases in which the producers of knowledge objects are not involved in the objects' further career – which is the majority – it can be very difficult to know for practitioners whether they have achieved stability or whether they only (and perhaps mistakenly) believe so.

Analytically, the stability of knowledge is equally elusive. How can one tell whether a knowledge object has traveled stably? And how stable is stable enough?

What is also difficult to capture analytically is how such elusive stability can be achieved. My interlocutors appeared to focus on the knowledge objects themselves, spending time and care on both designing templates and crafting individual knowledge objects. I argue, however, that these knowledge objects (and their templates), as important as they are, do not tell the whole story of how knowledge is moved stably. I will draw attention to the work that undergirds the movement of knowledge through the criminal justice system and that, like physical infrastructures, forms the substrate for this movement. The sensibilities from infrastructure studies will make it possible to trace and analyze how the stability of knowledge objects is enacted in the criminal justice system.

Resolving tensions: Interprofessional standards

A sensibility for standards and how they resolve tension between different sites makes it possible to think about how epistemic differences in the criminal justice system are bridged. Practices such as the templating of crime scene reports and the teaching of how to recover different traces can, with this sensibility, be seen as interprofessional standards that (aim to) resolve epistemic tensions and to facilitate the movement of knowledge objects.

Consider the traces – fingerprints, hairs, glass fragments, etc – that crime scene technicians recover at the crime scene and send to the laboratory: These traces must be recovered, packaged, and transported in a way that makes successful laboratory analysis possible; in other words, the crime scene must be harmonized with the laboratory. When crime scene technicians are taught how to recover traces so that the subsequent analyses do not suffer or become impossible,

the teachers in effect disseminate standards that are meant to resolve the tension between the laboratory and the crime scene. These interprofessional standards are exactly what *makes* their movement, and their stable movement at that, a merely practical matter. That is, just like plumbing standards are not at the forefront of thought for someone opening a faucet, interprofessional standards that are firmly in place may become virtually invisible.

The importance of interprofessional standards for resolving epistemic tensions becomes easier to see where they are not fully in place, as for example the expressions of value to be used in the crime scene reports. While the crime scene technicians seemed to welcome the standardization as a way of establishing shared practices within their profession, the prosecutor invited to give a guest lecture as part of the crime scene technician training was hesitant when students asked about these expressions. They might be understood quite differently by different people, she said, so it was chancy to rely on them alone. What was important, however, she went on, was that the crime scene technicians explained their process of thought.

Her concern that the expressions of value could be understood differently by different readers suggests that she did not perceive them to be as easily readable as they were intended. Of course, one prosecutor is not representative, but as this was a prosecutor who had been specifically invited to the course to talk about crime scene reports, her hesitation might still be indicative. It also ties into the hesitation I had heard in my earlier study from both prosecutors, judges, and defense lawyers about the NFC's scale (which then had been in place only a comparatively short time). Then, there was sometimes quite some uncertainty about what a particular grade meant or how many grades there were (see also Kruse, 2013).

Such hesitation and uncertainty indicate that, to function as an interprofessional standard, the scale and the expressions of value would have to be established across the criminal justice system's epistemic cultures as holding fixed and shared meaning. If they are not, the crime scene technicians' crime scene reports may not always travel stably – or "with integrity" (Morgan, 2011: 12) – to their recipients.

In other words, applying a sensibility for standards and how they can resolve tensions between sites to the movement of knowledge objects through the criminal justice system makes it possible to see the necessity of establishing shared understandings across epistemic cultures for the stable movement of knowledge objects: Without them, knowledge objects cannot travel stably or with integrity through the criminal justice system; like a boundary object (Star and Griesemer, 1989), their intended and received meanings might differ quite considerably. However, interprofessional standards alone do not guarantee the stability of the knowledge objects in the criminal justice system, nor do they make stability less elusive – if standards are not well-established or standardized expressions are misunderstood, this may pass unnoticed.

In addition, as the next section will discuss more closely, standards always and inevitably have limitations. Thus, in order for the tension between epistemic cultures to be resolved, standards must be supported and complemented.

Alignment work

With a sensibility for the – often invisible – work that is necessary to keep the undergirdings of movement in working order, I want to draw attention to the alignment work that supports and complements interprofessional standards and, at least temporarily, resolves the tension between sites or epistemic cultures in the criminal justice system. It aligns, for example, standards and specific circumstances or different understandings with each other.

The interprofessional standards involved in moving traces from the crime scene to the laboratory, for example, cannot be applied as effortlessly in every situation as it may seem at first glance or in the NFC's classroom. The stories students told each other about particular crime scenes they had encountered were an illustration that there are misfits (cf. Star, 1990: 36) between (general) standards and (individual) crime scenes and that quite some work may be required to stabilize both knowledge objects and standards in the face of these misfits.

One such story vividly described examining the site of a suspected rape on a beach, where a dog brought in to sniff out semen had duly marked a spot in the sand. Of course, the technicians were well aware of how to handle presumptive body fluids: by mopping them up with forensic swabs, sealing these into their sterile paper bags, and posting them to the NFC. However, the spot the dog had marked was several square meters large – certainly too large to swab – and nothing could entice the animal to be more precise. So, the technicians obtained pizza boxes from a local restaurant, carefully scooped up the sand in question with them, stacked them in the back seat of a car, and drove them across the country to the NFC.

This was evidently a highly entertaining story; when it was told in a corridor during a break, it was met with laughter and questions for details. The entertainment was neither at the expense of the technicians in the case, nor did it seem to be meant as more than mild criticism towards the (absent) teachers. It was easy, the students agreed, to say what one should *not* do – they were quite aware that pizza boxes were not an approach endorsed by the NFC. But what *should* one do, then? Swabbing several square meters of sand clearly would not have worked, nor is a portion of beach a movable object that can be put in the post, which is an option with objects suspected to carry relevant traces. That is, the students neither disputed the general validity of the standards they were being taught nor their necessity, but wrestled with their applicability.[7]

The story – reminiscent of the stories with which photocopier technicians share experiences and reflect about their work (Orr, 1996) – did not report the results of the laboratory analysis, and no one asked about them. Instead, the story and the subsequent discussion highlighted how the crime scene technicians, through inventiveness, professional skill, and dedication managed to save a possibility of forensic evidence: They did so by reconciling the interprofessional standards that make the traces' movements possible with the particular circumstances of a crime scene. In the story, circumstances were unusual and difficult, which made the technicians' work unusual and thus visible, but the principle applies to every crime scene – their work at the crime scene supports the interprofessional standards' resolving or at least decreasing the tension between the crime scene and the laboratory.

Their alignment work is also part of maintaining these standards and keeping them functional. By aligning the standards with each crime scene's particular circumstances, they minimize how much the misfits between standards and individual crime scenes affect the resolution of tension between crime scene and laboratory. Thus, their alignment work prevents the standards from failing to resolve tension and thus from becoming obsolete and perhaps discontinued.

Alignment work does not always maintain and support such standards in the criminal justice system, it may also complement them, for example technicians' witnessing in court. In her lecture on proceedings in court, a lawyer invited to the crime scene technicians' training talked at length about the importance of testimony complementing the written crime scene report. "Reading alone isn't always going to do it," she said, so even a well-written report – one that, in her words, "leads" the reader and "explains" what the crime scene technician has seen and done at the crime scene and concluded afterward and on which premises – might not be sufficient in itself.

If the technicians were summoned to court, she stressed, "that isn't because the report isn't good enough, but because the case is so complicated that the prosecutor thinks it's valuable that you come and explain [your work] and answer questions." She explained, "I don't see the connections you see;" in other words, as her competence was in the law, not in forensic or crime scene work, she did not expect to understand every crime scene report and its implications in exactly the same way as a crime scene technician would. What would be self-evident to a technician could be difficult or even obscure to her. Thus, she felt she – and by implicit association also other lawyers, prosecutors, and the courts – occasionally needed additional explanation and guidance. I have encountered similar reasoning in other voices from the criminal justice system, for example a judge who appreciated forensic scientists' testifying in court in addition to their expert statements, as "it [the evidence] would probably

have been harder to understand from just the written material."

In other words, stability in the form of more homogeneous understandings can be achieved by placing reports and stements in the company of their authors and their "explanations." The crime scene technicians' expert testimony thus constitutes a different kind of alignment work, one that keeps the crime scene reports stable by aligning how they are understood in court with their intended meanings. One could say that summoning crime scene technicians to court compensates for the insufficiency of the interprofessional standards that are meant to harmonize the writing and the reading of the reports. One could also say, however, that their expert testimony is a different way of resolving tensions between the crime scene and the court and of facilitating the reports' stable movement. With both perspectives, however, the reports' stability is achieved not (only) through the object itself, but (also) through work being done repeatedly and in an institutionalized way – namely through the well-established institution of summoning members of the criminal justice system to court as expert witnesses. This kind of alignment work also makes it possible for practitioners to capture the otherwise so elusive stability – the back and forth of questions and answers in court gives both authors and recipients a feeling for whether their understandings are in alignment.

Thinking about alignment work, I argue, also necessitates thinking about by whom and under which circumstances this work is performed – in other words, to bring a sensibility for power and inequalities to the infrastructuring that undergirds the movement of knowledge.

Inequalities

A sensibility for power makes it possible to think about the visibility and invisibility of alignment work in the criminal justice system as well as about practitioners' possibilities to shape their work and the larger collaboration.

The crime scene technicians' alignment work at the crime scene, for example, is quite invisible in the criminal justice system as a whole. Even though its sometimes less than perfect results may be very visible – it is reasonable to assume that the forensic scientist receiving the pizza boxes noticed that they were not forensic swabs in paper bags[8] – both the performance of and the necessity for alignment work are absent from official descriptions of crime scene technicians' work, as well as from the classroom sessions discussing and disseminating interprofessional standards (see also Kruse, 2020a). The technicians' stories about "difficult cases," as they called them, such as the beach case, were told predominantly outside of the classroom, that is, during breaks or transitions between classes.

In other words, like other invisible work – coordinating medical work (Strauss et al., 1985: 151ff) or giving patients emotional support (Bowker and Star, 1999: 229ff) – the work of aligning standards with crime scenes and vice versa is made invisible through not being marked as officially part of the production of forensic evidence. Unlike some other invisible work in other contexts – for example the monitoring required when people with uncommon allergies engage with standardized gastronomy (Star, 1990: 35) – this alignment work did not necessarily always seem to be only a burden to the crime scene technicians, however. Judging from how the story about the beach case and other, similar ones were told and received, the crime scene technicians performed alignment work willingly, with the common goal in sight, and took pride in their dedication and inventiveness. That is, the usefulness and durability of the standards may not come at the price of private suffering (cf Star, 1990: 43), but at the price of the crime scene technicians' skill, inventiveness, and dedication being unseen and unacknowledged outside of their own profession.

When it comes to control over one's (alignment) work, an example may be the electronic order form that crime scene technicians fill out when they send traces to the NFC. This form, designed by the NFC, is, of course, a kind of infrastructure in itself, resolving tension between the investigation and the laboratory by linking a trace with information such as which analysis is being ordered, who is ordering it, whether the case is prioritized (for example because there is a suspect in custody or the case involves minors), and relevant information about the case. As such, it is meant to facili-

tate the traces' smooth and stable movement to the laboratory and subsequently the analysis results' movement to the investigation.

However, the cost for at least a part of this resolution – albeit not a financial or material cost – remains, perhaps unwittingly, with one profession: While including case information appears an unproblematic part of laboratory procedure to the NFC, to the crime scene technicians, providing such information poses a problem. They see the investigation's integrity jeopardized: Both to prevent false confessions and to preserve the evidentiary value of a true revelation, it is crucial to know whether an interviewee talks about a detail because they have been involved in or witnessed the crime or because they have heard or read about it. The fewer people who know details of a case, the easier it is to keep track of who knows what. Thus, crime scene technicians – many of whom have been investigators earlier in their careers – may have been ordered to silence or be reluctant to give details even to forensic scientists. In the words of a crime scene technician student who commented the teachers' instructions for the forms, it was one thing to give details to a medical examiner she knew, but it was a different thing entirely to give these details to "someone at the [NFC] whom I've never even seen and who I don't know who they're married to."

That is, she apparently felt that she and her peers were being asked to trust strangers with classified information – something she was not comfortable with. In addition, the order form may, in accordance with the Swedish principle of public access to official records, eventually become a public document, which may make crime scene technicians even more reluctant to provide information there. This, in turn, affects the stability of the knowledge objects – the traces – being moved.

Refusal to provide information, however, may lead to friction between crime scene technicians and forensic scientists. The NFC's position as the forensic authority in the country, together with the forensic scientists' academic backgrounds as opposed to the police's and thus the crime scene technicians' blue-collar profession, may be a factor in how practitioners experience this friction.

That is, even though there are no tangible sanctions, crime scene technicians may feel that their refusal may lead to their being perceived as troublesome and uncooperative in a collaboration whose willingness is otherwise emphasized. In addition, they may also feel frustrated that their concerns about the quality of the evidence being produced are not being heeded – in other words, they may feel that control over the collaboration is distributed unequally.

Conclusion: Enacting stability

In this article, I have examined the movement of knowledge objects through the Swedish criminal justice system through the lens of infrastructure and infrastructuring. In particular, I have brought to this movement three sensibilities from infrastructure studies – for standards' resolution of tensions, for maintenance, and for inequalities.

This has made it possible to widen analytic focus from the knowledge objects themselves to structures and practices that undergird their movement. Through these structures and their associated practices, I argue, the criminal justice system *enacts* the knowledge objects' stability across epistemic cultures. In other words, the stable movement of evidence-to-be through the Swedish criminal justice system is the result of infrastructur*ing* (Karasti and Blomberg, 2018), of its continuous creating conditions that facilitate movement and create and re-create stability. Not only must standards be developed, they must also be disseminated to the relevant professions – professions who constantly lose and acquire members – and, perhaps most importantly, supported and complemented by alignment work.

Paradoxically, this continuously enacted stability has quite a lot in common with the fluidity de Laet and Mol (2000) underline as the characteristic that makes a particular bush pump mobile. Both the bush pump's mobility and knowledge objects in the criminal justice system are dependent on the involvement of different actors. The bush pump can only work in different places with their different conditions if there is a community around it that sets it up and maintains it, that is, adapts it to these different places and conditions. Likewise, the criminal justice system's

knowledge object's mobility often depends on alignment work that aligns different epistemic cultures.

Looking at the movement of knowledge in the criminal justice system through the lens of infrastructuring thus makes it possible to think about stability as not a quality (of for example a knowledge object) but as a rather fleeting state that requires the work of a community and can only be temporarily attained. That is, the stability that may, for example, be achieved when crime scene technicians, in the words of the lawyer, "explain" their crime scene work in court is part of the court interaction, not only of the crime scene report that is being explained. In other words, knowledge objects can be stabilized as long as there is infrastructuring – for example in the form of functioning standards or of alignment work – to resolve the tension between epistemic cultures. With this perspective, it is not surprising that stability is so elusive – it is momentary and fragile.

The infrastructuring that undergirds the stable movement of knowledge in the criminal justice system is not always visible or noticed. Some alignment work, for example, is very visible – such as acting as an expert witness in court – whereas other alignment work may only be visible within the profession performing it, e.g. a large part of the alignment work performed at the crime scene. Nor is the work of infrastructuring or control over it always distributed equally. Compared to other inequalities, for example those discussed in infrastructure studies (e.g. Öhman, 2016; Anand, 2012; Appel, 2012), these certainly are not grave – yet they affect the mobility of knowledge objects. If interprofessional standards clash with unseen or unheeded local needs – such as the investigation's need for information control – they add instead of resolve tension. Unnoticed or unacknowledged alignment work also means that practitioners may not receive relevant training (see also Kruse, 2020a), which, in turn, may affect the collaboration. Thus, a sensibility for power can be fruitful for thinking about the stable movement of knowledge even through the criminal justice system; in sites less characterized by common goals, this sensibility might be of even more importance.

Thinking with infrastructure and (some of the) sensibilities from infrastructure studies about the movement of knowledge objects through the Swedish criminal justice system has thus made it possible to think differently about how stability – however fleeting – can be attained when moving knowledge across epistemic cultures. It has also made it possible to draw attention to work that may be performed spatially or temporally away from the knowledge objects that are being moved but still is essential for this movement. This perspective, I suggest, may be useful for studying the movement of knowledge also in other contexts.

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Notes

1 Special Issue on knowledge infrastructures, see *Science & Technology Studies* issues 1-3, 2016 (vol. 29).

2 For an example of how convoluted standards in border-spanning large technical systems can be, see Hanseth et al. (1996) on information infrastructures.

3 See Kruse (2015) for more on becoming a crime scene technician in Sweden.

4 These I have discussed elsewhere (Kruse, 2013).

5 For a discussion of this scale, see Kruse (2020b).

6 In the Swedish criminal justice system, prosecutors lead pre-trial investigations of severe crimes or in which there is a suspect.

7 Such stories can, of course, still carry an element of resistance. It is conceivable that at least some crime scene technicians harboured resentment towards the teachers (most of whom, after all, are not crime scene technicians themselves) for telling them how to do their job.

8 In addition, the forensic scientist presumably performed alignment work of their own – that was not visible to the crime scene technicians – when (or if) preparing the trace for analysis.

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-

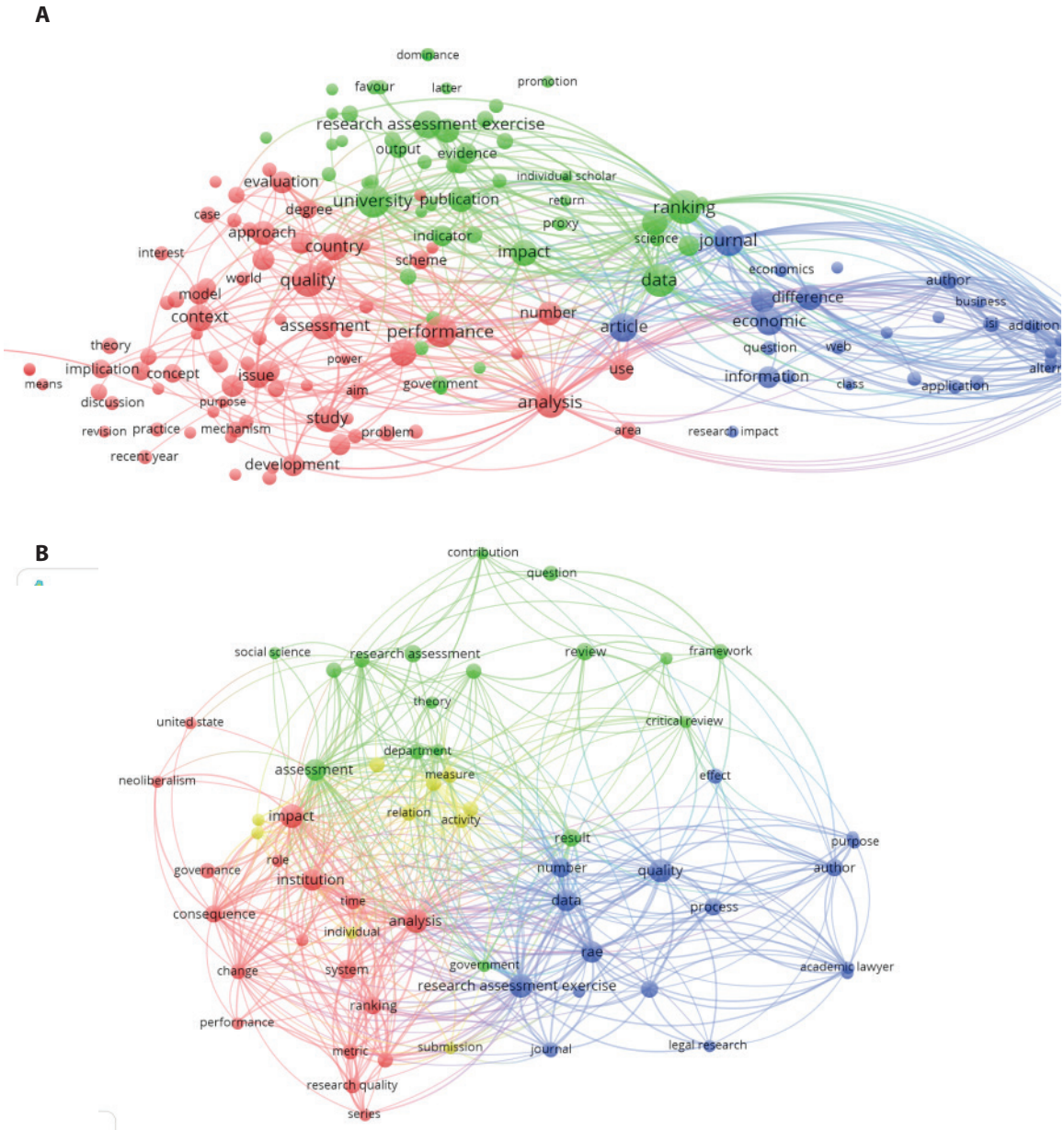

Figure 1. Co-word of article results in economics (above) and sociology (below)

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 ground-

breaking. 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

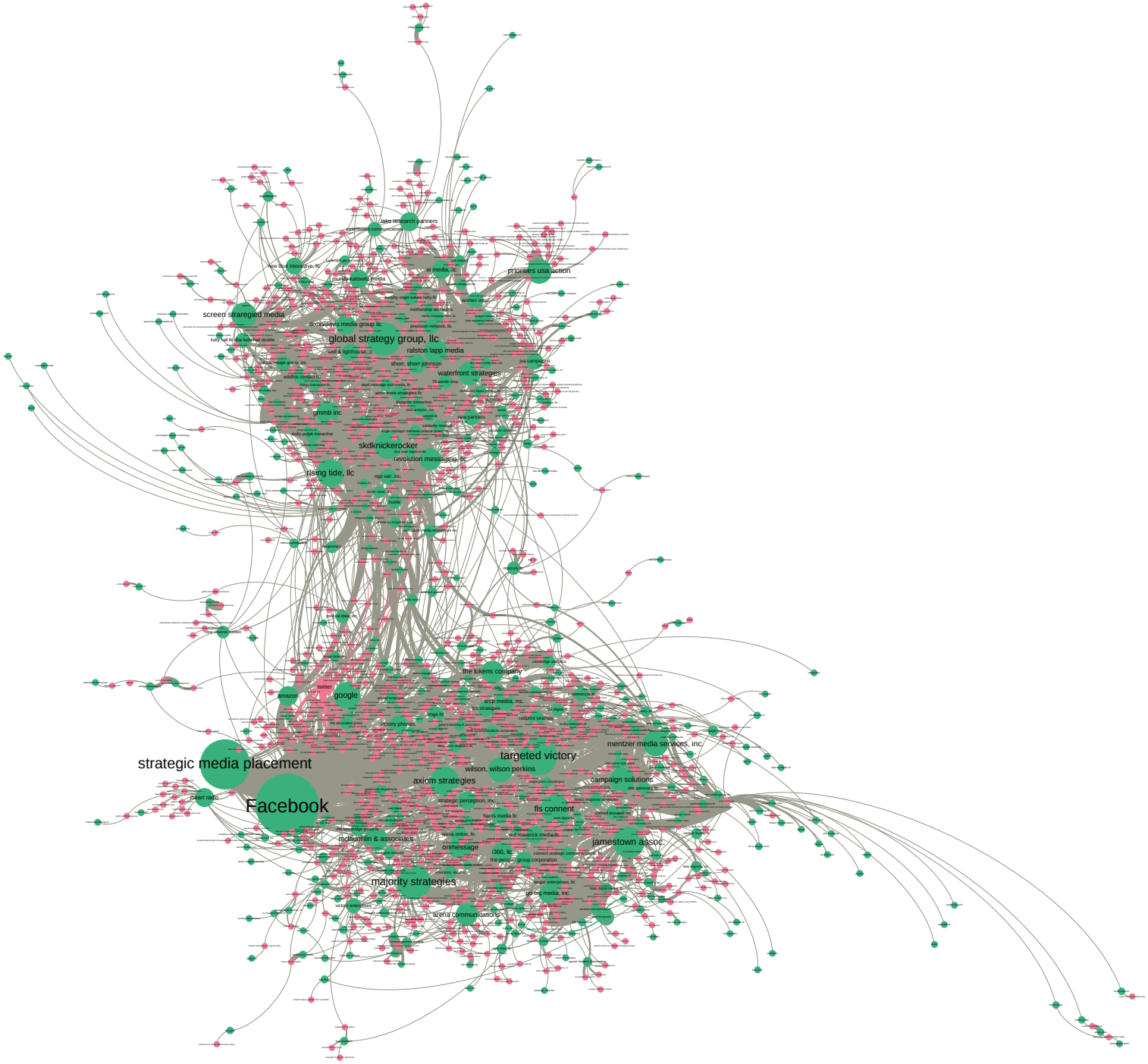

Figure 2. Payers (in red) and Payees (in green) in the UK and US election cycle 2014-6. Payees are sized by number of connections in this version of the graph.

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

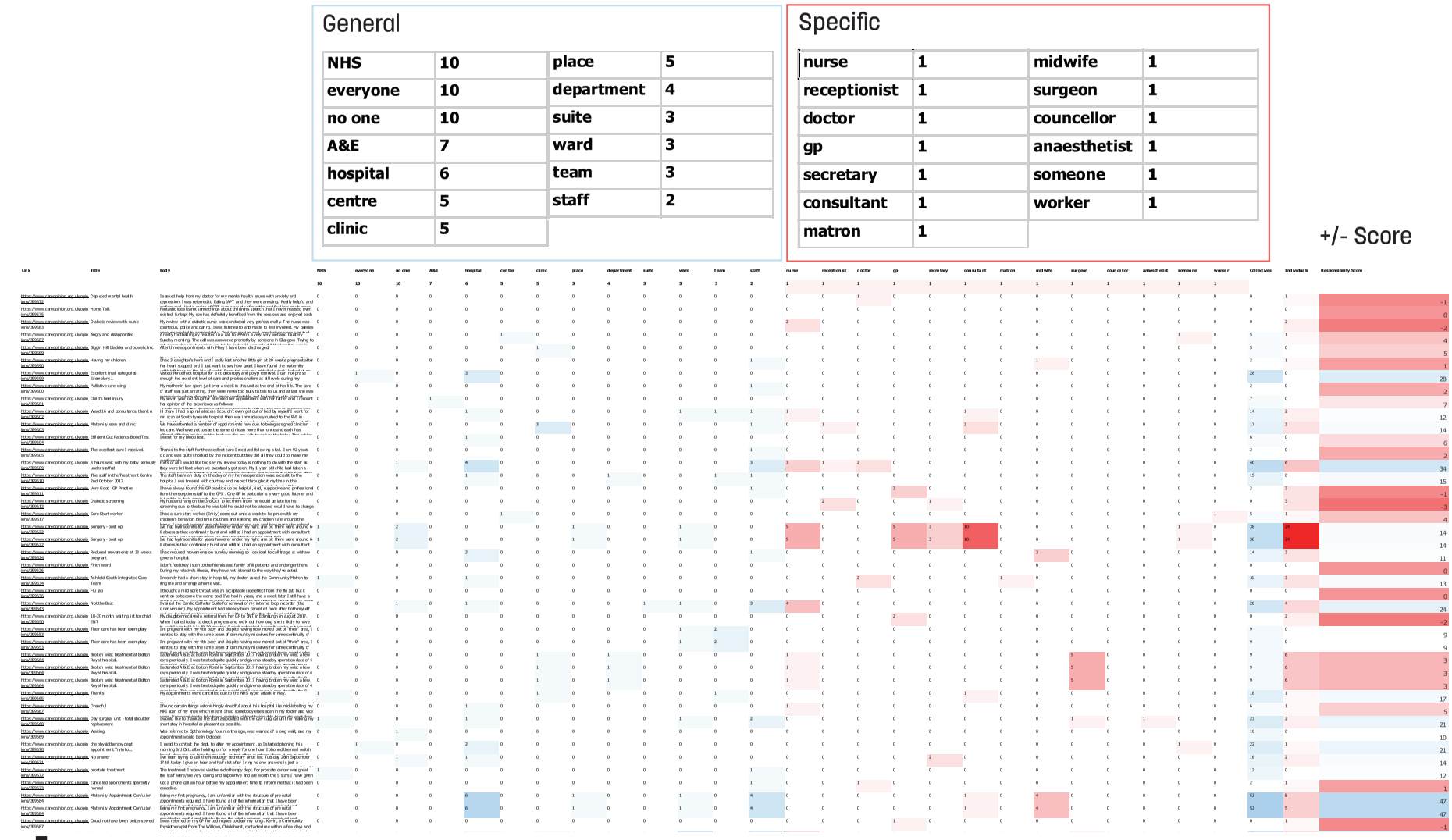

Figure 3. Stories with cumulative generalised responsibility scores

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 "parasites" 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.

Enacting Maasai and Palaeoanthropological Versions of Drought in Oldupai Gorge, Tanzania

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Abstract

While palaeoanthropologists have travelled to Tanzania's renowned human origins site of Oldupai Gorge for over a century, lasting collaboration has not been established with the Maasai pastoralists who live there. This paper uses actor-network-theory and the concept of enactment to compare palaeoanthropological and Maasai livelihoods and to explore why collaboration has been infrequent. Here we show that scientists and locals navigated large political-economic contexts and expertly acquired resources in non-scientific and non-pastoral worlds. As part of these livelihoods, both Maasai peoples and researchers created and multiplied reality and ontologies by enacting composite – yet conflicting – versions of drought. Whereas Maasai peoples faced contemporary drought, palaeoanthropologists needed to focus on producing publishable data about ancient drought during short fieldwork seasons. Such livelihood exigencies have hindered meaningful collaboration between these groups who both dug in the Gorge to address drought. While the legitimisation of scientific ontologies is well-intentioned, Maasai drought unfortunately remains unaddressed.

Keywords: Oldupai, Olduvai, Maasai, palaeoanthropology, drought, ontology

Introduction

Northern Tanzania's Ngorongoro Conservation Area features abundant wild animals and spectacular landscapes that draw safari-bound tourists from around the planet, yet it also contains Oldupai Gorge, one of the world's most important sites for human origins research. As a result, other frequent guests to the region are palaeoanthropologists, who archaeologically excavate the fossils

and stone tools associated with humanity's ancestors. Rarely noted in palaeoanthropological literature is that the lands surrounding Oldupai are home to a pastoralist Maasai society. Even though scientists have, for over a century, sought to illuminate the shared past of our species in what many regard to be a cradle of humankind (Clark, 2001; Leakey, 1978), there has seldom been meaningful collaboration between Maasai peoples and palaeoanthropologists. While a palaeoanthropological research partnership called Stone Tools, Diet, and Sociality is commencing a new chapter at Oldupai by collaborating with locals, many challenging issues remain.

We conducted fourteen weeks of ethnographic fieldwork in the region, during which we stayed at a paleoanthropological research camp located at the centre of Oldupai Gorge, participated in palaeoanthropological excavations, joined Maasai peoples in their pastoralist livelihood practices, and conducted interviews and focus groups with both communities. Throughout our research, we explored two primary research questions: how do Maasai and palaeoanthropological practices at Oldupai compare? Why have the groups rarely collaborated? This paper uses actor-network-theory to elucidate the *practicalities* behind the surprising lack of association between Maasai peoples and palaeoanthropologists. Members of each group engaged in practices that required navigating larger political-economic contexts, and both the Maasai and researchers tactfully obtained essential resources in non-pastoral and non-scientific worlds in order to support their valued pastoral and scientific livelihoods (Latour, 1987: 45-162). As part of these livelihoods, members of both groups shared in the practice of digging within the Gorge to address drought. While researchers possessed the financial and logistical means to conduct archeological excavations and thereby model an ancient drought and support their scientific livelihoods through writing publications, Maasai residents have been facing a multifaceted and livelihood-compromising drought that has gone unheeded, a drought that necessitates digging in the same Gorge for buried (and dirty) water. Each group created and proliferated reality and ontologies by enacting composite, yet conflicting, versions (Mol, 2002) of drought; one in the deep past and another in the present. These parallel enactments of drought demonstrate that palaeoanthropological and Maasai subsistence exigencies – respectively a publish-or-perish academic climate and the necessity to find water – have been highly pressing matters during the relatively brief time in which palaeoanthropologists conduct fieldwork that have deferred and impeded collaboration in Oldupai Gorge.

Background

Maasai peoples practice, and are often associated with, pastoralism (Spear, 1993). This livestock-tending livelihood prevails in locations where fickle rains make agriculture problematic. As pastoralists, the Maasai seasonally move livestock to dispersed water and forage, share resources and terrain with family and adjacent communities, and subsist on their animals' meat, milk, and blood (Fratkin, 2001: 3-4; Nelson, 2012: 4). During dry seasons, Maasai pastoralists who live within the Ngorongoro Conservation Area (NCA) often migrate livestock to resources located in highland areas, and when the rains arrive, they move back to lowlands. This transhumant livelihood promotes the regeneration of forage (Århem, 1985: 189-194; Galvin et al., 2008: 261-264). Rather than damaging ecosystems (Coughenour et al., 1985), pastoral practices have actually been a key factor in the genesis of contemporary savannah ecosystems that tourists flock to East Africa to experience. Within these landscapes, iconic safari wildlife, domesticated animals, and Maasai populations live in congruence. Pastoralists such as the Maasai protect and sustainably utilise areas where forage is available during dry periods, thereby sheltering wildlife whose migrations take them beyond the arbitrary borders of national parks (Nelson, 2012).

While Maasai peoples are often associated with pastoralism, Spear (1993) explores how Maasai livelihood practices have long been more fluid than such ascriptions suggest. Spear (1993) writes that it was sometime before 1000 AD that the contemporary Maasai's ancestors, who engaged in both agriculture and pastoralism, began moving southward from North Africa to what is now Tanzania and Kenya and began to

more intensively practice pastoralism in the plains of the Great Rift Valley. They eventually became part of an emerging ethnic division of livelihood labour, forming around the eighteenth century, in which Maa-speaking people claimed the mantle of pastoralists while Okiek peoples and Bantu peoples respectively identified as hunter-gatherers and agriculturalists. However, Maasai pastoralists have long depended on exchanges of services, goods, and *people* with these agriculturalists and hunter-gatherers they defined themselves against, meaning that the 'pure' pastoralism that emerged was on the one hand a way to retain jurisdiction of pastoral resources yet on the other has been linked to modes of production outside the pastoral economy (Spear, 1993). Maasai peoples often espouse a 'pastoral ideal', that being a livelihood dedicated to tending livestock (Igoe, 2006), and Spear (1993) explains that they cemented and revitalised their pastoral ideal in the twentieth century when they forged alliances with colonists against other ethnic groups, and when colonists spatially divided pastoralists, hunter-gatherers, and agriculturalists in the Rift Valley.

The nation now known as Tanzania was colonised by Germany in 1885 and Britain in 1918 (Ngowi, 2009: 262), a period in which foreign actors took control of land, often by establishing farms or national parks (Bushozi, 2017: 8-9). The Tanzanian Maasai started to lose access to rangelands during this colonial era. The British administration instituted Serengeti National Park (SNP) in 1940, and upon the 1959 separation of a section of SNP that became the NCA, the British Government removed the Maasai from SNP. Unfortunately, SNP houses key Maasai water sources, and the Colonial Government did not bring many development projects to Maasai rangelands. While the 'Game Parks Laws Miscellaneous Amendments Act of 1975' resulted in the creation of the Ngorongoro Conservation Area Authority; an organisation that actively nourishes Maasai, wildlife, and tourism interests in a region deemed a 'multiple land use area'; Maasai peoples have faced some challenges following Tanzania's 1961 independence. The World Bank and the International Monetary Fund endorsed neoliberal policies that ended up diminishing spending on education and healthcare, policies that also saw the conversion of Maasai pastoral rangelands in ways that *seemingly* more productive and 'modern' economic industries could be implemented. Lowland wildebeest populations in the NCA have flourished due to conservation projects, yet these animals bring diseases to Maasai livestock. Since tourist infrastructure claims a lot of the water remaining within the NCA, the Maasai's highland resource caches are becoming congested, which exacerbates livestock disease conditions. Combined with increases in human populations, established forms of pastoralism in the region are no longer fully viable, and cultivation restrictions and limited access to participation in the tourism industry impede the Maasai's livelihood diversification options within the NCA (Galvin et al., 2008; Hodgson, 2011: 64-75).

Highlighting the fluidity of Maasai livelihoods *and* the espousal of the pastoral ideal, Maasai peoples have left for work outside the NCA as a means to support their pastoral lives within. Whereas the 1992 lifting of a cultivation ban in the NCA inspired most Maasai residents to start tending crops on a small scale, 2012 saw the reintroduction of the ban, and McCabe et al. (2014) write that their young Maasai interlocutors from the NCA have migrated to urban areas in search of employment as guards or in factories. These migrants often kept working until they were able to purchase livestock and thereafter return to their homes (McCabe et al., 2014), the NCA lands so popular with tourists.

Safari tourism in 2007 brought $1.6 billion to Tanzania (Nelson, 2012), money that infrequently reaches Maasai communities. While the Maasai's pastoral livelihood contributes to the vitality of Tanzania's renowned ecosystems and thus its tourism industry, an entrenched yet misguided conviction that pastoralists overgraze pastures has guided evictions of the Maasai from rangelands (Nelson, 2012). Parties have sought to turn Maasai peoples away from pastoralism, which many have portrayed as obsolete. Concurrently, remunerative tourist mementos depict Maasai as archaic icons of primeval African landscapes (Hodgson, 2011: 64-70). Explaining that such representations embody and convey expectations that Maasai peoples cannot 'modernise' and that pastoralism

harms environments, Galaty (2002) suggests that these ideas lead some decision makers to regulate the Maasai, which downplays actual Maasai interests and views.

Besides tourists, other visitors to the NCA are scientists. In 1911, a German butterfly aficionado named Wilhelm Kattwinkel came upon Oldupai's scientific resources (Leakey, 1978: 151). After Louis Leakey maintained that humanity's origins could be uncovered in East Africa, Mary Leakey excavated the legendary skull of *Zinjanthropus boisei* at the Gorge. This 1959 discovery catapulted Oldupai into public consciousness and cemented it as a prime location for palaeoanthropological research, especially since the Gorge's renowned stratigraphy provides an exemplary window into human origins (Clark, 2001). Through interdisciplinary means, palaeoanthropologists unearth and investigate the bones and stone tools associated with extinct hominins, the ancestors of contemporary humans. Palaeoanthropologists explore the life histories, diets, and sociality of hominins by reconstructing the climatic shifts and environmental contexts that impacted on our prehistoric ancestors (Keenleyside and Lazenby, 2011: 189-193).

All varieties of archaeologist – including palaeoanthropologists – engage in the practice of excavating, and in doing so, selectively remove and thereby produce specific kinds of evidence that they can use to reveal conceptions of the past. Thus, archaeology can make possible, stabilise, extend, and naturalise political discourse, interests, and truths (Abu El-Haj, 2001: 1-21). Despite representations of science as detached from society (Latour, 1987, 1993a) the field-based practice of archaeology overtly occurs in specific temporal and spatial landscapes. Archaeology can never be removed from the social, political, and institutional contexts in which it exists and reconstitutes, and other groups often make claims to the landscapes and traces that archaeologists seek (Abu El-Haj, 2001: 1-21).

A palaeoanthropological project that recognises such issues is *Stone Tools, Diet, and Sociality* (SDS), a palaeoanthropological partnership between environmental specialists, biologists, geologists, social scientists, and palaeoanthropologists from institutions located in numerous countries. SDS's interdisciplinary collaborations are producing myriad new insights that are vital for understanding humanity's emergence (Favreau 2019; Mercader et al., 2018a; Mercader et al., 2018b; Mercader et al., 2019; Soto et al., 2019; Soto et al., 2020a; Soto et al., 2020b; Tucker 2018; Tucker et al., 2020). Furthermore, SDS aspires to curb the history of neocolonial research at Oldupai by collaborating with the local Maasai, and Maasai peoples shared in such desires for collaboration.

We used actor-network-theory (ANT) to investigate and try to explain the various social groupings and associations at Oldupai. With ANT, an ethnographer documents social interactions, relationships, and various groups as temporary associative networks between 'actants' - diverse entities that include objects, facts, humans, nonhumans, ideas, and abilities. The ANT framework posits that groups form to accomplish specific objectives. This formation results from authoritative spokespeople establishing associations between actants, thereby creating some ensembles while also invalidating other ensembles. If one of these ensembles or actor-networks becomes 'potent', it may inspire action in others. When an association between actants withstands disassociation, it then becomes a discrete actant itself and is available to be brought into other associations (Finlay, 2014; Latour, 1993a: 158-236, 2005; Smart and Smart, 2017: 29-65).

Latour and Woolgar (1986: 105-179) demonstrate the above process by documenting how a group of scientific researchers agreed that a certain grouping of actants, including multiple scientific ideas, laboratory technologies, and experimental results, conclusively determined the chemical composition of Thyroptropin Releasing Factor. Even though Thyroptropin Releasing Factor might not consist of the particular variety of amino acids that scientists agreed upon, it became a new ontological entity that researchers across disciplines could use as a tool – another actant – in other associations. Latour (1987: 93-214) explains that while many feel that an understanding of an ever-present nature is why scientific debates resolve, ANT shows how settled debates actually produce nature. Moreover, with ANT, society is also a product of various associations; and the variety of connections between

society, science, and technology illustrate that there are no absolute distinctions between social and scientific realms. For example, to support their laboratories and the science within them, scientists acquire resources/actants in the variegated social worlds beyond laboratory walls.

Thus, ANT sheds light on scientific understandings of nature, yet it can also be applied to myriad domains (Latour, 1993a: 158-236, 2005: 87-120). Ontology is the "branch of metaphysics that concerns itself with what exists" (Blackburn, 2008: 260), and ANT underscores how through practice, reality and ontologies can become multiple (Goldman et al., 2016: 28-32). In an ANT-aligned ethnographic study of disease, Mol (2002) introduces 'enactment': within two separate Dutch hospital departments, diverse practices enact multiple objects that share the same name, which subsequently proliferates reality and ontologies. In the outpatient clinic, doctors temporarily enact 'atherosclerosis' through diagnostic interviews and physical examinations with patients; while in the pathology ward, the same disease is 'done' through dissection and microscopy of tissue. Rather than being two subjective perspectives on the same objective disease within bodies, these exclusive practices give rise to different entities that are both called 'atherosclerosis'. Since there are many variations in the way this disease is enacted, even within outpatient clinics, there are nearly limitless multiplicities of reality (Mol, 2002: 1-51).

Mol (2002: 44-164) explains how various modes of coordination unite the multiple versions of enacted atherosclerosis. When diagnoses coincide, they can enact atherosclerosis in partnership, yet when techniques diverge in diagnoses, the single object can be maintained by finding faults in one of these methods. Moreover, rather than abandoning the incongruent results of diagnostic techniques, physicians can compile the different versions of atherosclerosis that emerge, producing a 'composite' disease. On the other hand, potentially conflicting enactments of atherosclerosis can simultaneously exist by remaining isolated within their respective sites of diagnosis. These 'distributions' of reality permit patient-specific treatments, and the word 'atherosclerosis' helps to coordinate communication between these place-specific enactments, preventing complete fragmentation. Entities (which in ANT terminology would be 'actants') such as knives, patient records, and corpses all play key roles in the myriad practical enactments of disease, ailments that then become new entities ('actants'). If others attribute facticity to the enacted disease, treatment follows. Assorted types of medical professionals then bring the enacted disease into new practical associations: treatments in surgery wards that 'counteract' the malady (Latour, 1987; Mol, 2002: 44-164).

In an application of Mol's (2002) enactment concept, Goldman et al. (2016) eschew the epistemological/perspective-based approach, which would begin with the assumption that there is a lone reality that members of different groups gaze upon and that scientists have unclouded and exclusive access to it (Goldman et al., 2016; Latour, 1993b: 96-112). Instead, Goldman et al. (2016) investigate how scientific and Maasai practices establish what a drought, ontologically speaking, *is* for members of each group. Both climate researchers and Maasai peoples astutely surveyed different arrays of culturally relevant actants, which resulted in discord between the groups regarding the year in which drought occurred. Unfortunately, Maasai peoples residing in areas not officially designated as zones needing relief can miss out on assistance. The authors accentuate how paying attention to ontology legitimises non-Western methods of knowing the world, an approach that flattens seemingly axiomatic ontological hierarchies that influence policy makers to only consider the supposed supremacy of scientific models when formulating decisions.

Goldman et al. (2016) demonstrate how Maasai pastoralists navigated political-economic forces, such as enduring colonial-era border impositions and the implications of top-down decisions. Rather than assuming the existence of stable and pre-ordained social contexts and forces, an ANT framework has the analyst instead regard 'society' and 'the social' as transient associations between exceedingly diverse types of actants (Latour, 2005) that may be mobilised on vast scales (Latour, 1993b: 96-124). In addition to the aforementioned political-economic forces that the Oldupai Maasai face – such as lucrative safari tourism that contrib-

utes to eviction from lands, along with neoliberal reductions in social service expenditures – a potent force is the erroneous and influential view of Maasai pastoralists as archaic (Galaty, 2002; Galvin et al., 2008; Hodgson, 2011; Nelson, 2012). When promoters, writers, and artists depict Maasai peoples as timeless, Oldupai's locals appear premodern (Latour, 1993b).

Latour (1993b) writes that "We Have Never Been Modern": while members of so-called 'modern' societies visualise massive permanent shifts from archaisms, such as the seemingly anachronistic 'premodern' practice of making associations between 'nature' and 'society', all societies build 'hybrid' networks. Members of 'modern' cultures, especially scientists, strive to progressively sort out immutable nature from the polluting subjectivity of society. This practice fuels assumed breaks from the past and downplays the construction, within 'modern' societies, of hybrid networks that are comprised of myriad actants that 'moderns' may define as variably contemporary or primitive, natural or social. For Latour (1993b), 'symmetrical anthropology' entails investigating and comparing hybrid networks – which consist of ostensibly 'natural' and 'social' elements that are themselves both natural and social – wherever they manifest. Latour (1993a: 230-236, 1993b: 13-112) declares that while many accord scientists sole authoritative access to 'nature', we must refrain from placing knowledge systems in hierarchies, as we all make knowledge by establishing sundry associations.

To debunk narratives that depict Maasai peoples as anachronistic, narratives that on many occasions work to inform policy decisions, Galaty (2002) advocates constructing normalising counter-narratives that acknowledge Maasai pastoralists as humans who have human yearnings and necessities, demonstrate that the Maasai practice their customs because they are proud of them, and recognise that the dynamic Maasai are denied access to the boons of 'modernity' that they often seek. We hope that our symmetrical approach (Latour, 1993b) throughout the following pages paints a humanising picture of life for Maasai peoples and for the researchers who make their living at Oldupai Gorge. While the Maasai participants in this study espoused great pride in their pastoral identity, they also drew on a long history of livelihood fluidity (Spear, 1993; McCabe et al., 2014) as they diversified their livelihoods and ventured into non-pastoral worlds. This paper will demonstrate that such ventures were aimed at supplementing and realising the Maasai 'pastoral ideal': a livelihood centered around livestock (Igoe, 2006).

Methods

Inspired by science and technology studies scholarship that highlights how practice creates reality for people (Goldman et al., 2016; Latour, 1987; Mol, 2002), our research at Oldupai was guided by two overarching research questions: how do the day-to-day practices of palaeoanthropologists and Maasai pastoralists, at a flagship scientific research locality, compare and contrast? Why have Maasai peoples and palaeoanthropologists, for over a century, rarely collaborated? Endorsed by the University of Calgary's Conjoint Faculties Research Ethics Board and aided by Mol's (1995) Maa language learning handbook, we secured Tanzanian permits and began exploring these questions by conducting ethnographic research amongst Oldupai Maasai communities. Samson Koromo – a Maasai scholar – assisted in translating the study's research protocol and interviews, and Samson Koromo and Patrick Lee regularly discussed how the project was and should be progressing. We collaborated with key informants whom we came to know during a 2015 pilot study, and we used snowball sampling to find others interested in contributing, meaning that the interests of community members directed the course of inquiry (Cohen, 1984: 223-225; Hammersley and Atkinson, 2007: 104-105).

Maasai homesteads consist of one or more family units (McCabe et al., 2010: 323-324) whose integration is a primary value and foundation for collective autonomy (Sharif and Bugo, 2015: 631-633). Thus, we employed focus groups (Buzinde et al., 2014: 26) at Maasai homesteads to explore how members of families view their lives at Oldupai. We engaged in 9 focus groups that featured females and males from all of the age-sets within which Maasai societies are organised (Spencer, 2003: 15-37). We also conducted 15

semi-structured interviews with warriors, elders, youth, age-set leaders, women, mothers, and Maasai pastoralists employed by researchers. Maasai community members welcomed our participant observation in their villages, at community events, and as they practiced their pastoral livelihoods, which let us gain a contextual understanding of daily life (Camfield et al., 2009: 11). Interlocutors at Oldupai conveyed all of the direct quotes that are presented in this paper.

Ethnographic researchers can explore the standpoints of both non-scientists *and* scientists as a means to thoroughly comprehend the impacts that science makes on other groups (TallBear, 2013: 23-25). Thus, to holistically examine Oldupai and to understand how the discipline of palaeoanthropology affects the Gorge and the Maasai, we chose to 'study up' (Nader, 1969; TallBear, 2013: 17-18) and conduct ethnographic research amongst palaeoanthropologists. We used participant observation at palaeoanthropological dig sites and at scientific research camps. We spent our nights at these camps and had daily personal conversations and group discussions with researchers that were nearly identical in form to our interviews and focus groups amongst the Maasai.

Beyond anthropological symmetry, which entails examining all actor-networks equally (Latour, 1993b: 90-129), Callon (1984) calls for 'generalized symmetry': utilising a singular etic vocabulary to describe all entities. Thus, in the following text, we use the term 'actants' to refer to the myriad entities that were relevant to palaeoanthropological and to Maasai actor-networks. Nonetheless, rather than expunging insightful and distinctive cultural lexicons (Miettinen, 1999: 190-192), we strive to strike a balance between the emic and the etic by analysing the discussions in which our informants engaged *and* by making use of common ANT terminology. In this undertaking, our goal is to avoid contributing to any misguided and unjustified distinctions between scientists and locals (Latour, 1993b: 96-112).

Results

Palaeoanthropological drought

Palaeoanthropological fieldwork was typified by digging. Each sunrise, the SDS team climbed down Oldupai's steep walls in order to excavate at various sites that the team's prior surveys of the landscape, along with previously published research, had indicated would be bountiful in traces of the past. However, it was not only the highly tangible remnants of hominins – such as fossils and stone tools – that SDS excavated, as some team members dug into the stratigraphy that comprised the Gorge's walls. By digging at a series of vertical points and collecting samples of sediments that were progressively laid down through time over the past two million years (Mollel and Swisher, 2012), the researchers collected sediment samples that contained entities such as biomarkers and phytoliths (discussed below). SDS thus collected remnants of numerous ancient ecosystems and environments that hominins had lived in. Researchers immediately placed the sediment into sample bags to prevent the influx of other substances that may complicate analyses of the samples, and later transported the bags back to a contaminant-free clean laboratory in Canada where sought-after traces of the past, such as phytoliths, would be delicately extracted from the sampled sediment.

SDS researchers also constructed a *mobile* clean laboratory over their archaeological excavations while at Oldupai, a structure that prevented contemporary residues from contaminating the stone tools that the team unearthed from a large three metre by three metre excavation (see Lee et al., 2019). Confident that the residues adhering to lithics were ancient, SDS later analysed the stones at the clean laboratory in Canada as a means to understand how hominins used tools for processing food. However, this 'pure' science, which had a foundation of digging in numerous spots around Oldupai Gorge, also depended on the support of associations with countless types of extra-laboratory actants/resources.

Scientists must diplomatically acquire resources in the 'non-scientific' world beyond laboratory walls (Latour, 1987: 45-162), where they must also navigate cultural, economic, and

political contexts that are themselves assemblages of actants (Latour, 2005). Surrounded by vast tracts of seasonally-desiccated backcountry, SDS needed to bring a wide variety of items with them in order to conduct the archaeological activities inside the mobile laboratory. Implements included trowels, screwdrivers, hammers, permanent markers, sieving screens, GPS devices, tape measures, a power generator, a HEPA air filter, air conditioners, starch-free clothing, nails, rope, pins, tape, thermometers, computers, cameras, notebooks, and sample bags. With no access to Wi-Fi nor to a library, palaeoanthropologists relied on previously-obtained excavation skills and theoretical knowledge. Researchers required assistance from an exceedingly wide variety of items and people, as archaeological assistants used generator-powered jackhammers, shovels, and wheelbarrows to remove large chunks of the ground. Once they had exposed sought-after layers, researchers could then begin to systematically search for bones and stone tools.

Archaeology is field-based; a science that always occurs in unique spatial and temporal contexts (Abu El-Haj, 2001: 20-21). To effectively respond to Oldupai's harsh landscape, palaeoanthropologists constructed a camp that featured tents, a communal meal pavilion, an outdoor cooking pit, latrines, and generator-induced electricity during select morning and dusk hours. The camp harboured bottles of water, food, plates and cutlery, and generator fuel. Prior to each evening meal, researchers, students, and assistants shared freshly-popped corn while attentively listening to a daily-rotating cast of lecturers deliver scholarly presentations with the assistance of a laptop and projector. The camp's hired attendants prepared meals while researchers and their assistants were away at excavation sites, swept the sand that the ever-present warm breeze constantly blew into the meal pavilion, and guarded the area from potential intruders. Nonhumans assisted in this latter task, as dogs protected the camp from hyenas and ostriches during the twilight hours, seemingly in exchange for table scraps.

We participated in a supply run to a settlement located 71 kilometres east of the camp. With a local driver, and assisted by research permits that allowed us to travel through the Ngorongoro gates, we traversed rutted dirt roads and arrived in Karatu. With a list of scientific and subsistence items in hand, we hopped between roadside kiosks, gas stations, a gated water outlet, and a central market. At this chaotic marketplace, we wandered through countless rows of kiosks, each featuring a vendor offering food items for which one had to negotiate a price. Not knowing how to do this, we fortunately found a friend of the camp's chef, who borrowed our shopping list and gathered each item on our behalf. Saving the team countless hours, her expertise was an irreplaceable asset to conducting research at Oldupai.

In an ANT framework (Callon, 1984; Latour, 1987, 1993a, 2005; Smart and Smart, 2017: 37), the distinguished leaders of palaeoanthropological teams coordinated networks of associations between multifarious actants, including humans, nonhumans, ideas, theories, objects, and abilities. Such networks were transient, as scientists conducted fieldwork during their institutions' summer months, a period that coincided with Tanzania's dry season. The associations facilitated specific action: enabling scientific work in a remote and grueling location. As the SDS project continues, its spokespeople will create stability by periodically re-coordinating these associations and assemblages of actants.

As a science that takes place in the field, archaeology occurs in – and reconstitutes – specific social and political contexts (Abu El-Haj, 2001: 20-21). Many of the actants that supported the 'pure' science within the Gorge were quite costly. In a world characterised by capitalism, neoliberalism, and reduced public expenditures on higher education (Blim, 2000: 27-31; Crewe and Axelby, 2013: 89-90, 159; Ylijoki, 2003: 307-310), palaeoanthropologists had to engage in a cycle common to academic disciplines (Finlay, 2014: 145-181; Latour and Woolgar, 1986: 187-230; Rabinow, 1996: 19-31): obtaining funding, conducting research, and producing credential-bestowing publications that secure continued funds. During the summer, researchers were able to escape the rigours of academic funding cycles that prevailed in their home institutions (Rabinow, 1996: 19-31) – along with the frustrations of top-down administrative decisions and policies – and used funding to acquire items such as research permits. These

documents allowed palaeoanthropologists to conduct excavations at specified sites in the Gorge, which is located in the *seemingly* pristine NCA. Like its neighbouring national parks – which harbour important dry season water sources that Maasai peoples can no longer access – the NCA is not fully 'natural'. Rather, the NCA is a *created* piece of wilderness that attracts tourists and income in the same capitalistic world economy that researchers had to navigate. The NCA is a social-natural hybrid, and is the landscape that both palaeoanthropologists and Maasai peoples pursued their livelihoods in (Blim, 2000: 27-31; Crewe and Axelby, 2013: 89-90, 159; Cronon, 1995; Galvin et al., 2008; Hodgson, 2011: 64-75; Latour, 1993b; Nelson, 2012).

Researchers had a relatively short timeframe in which to conduct their livelihood-securing field research. Combined with the significant costs of archaeological supplies, camp items, research permits, and transportation across the planet; scientists focused on reconstructing the past as a means to publish academic publications, a key component of palaeoanthropological subsistence. On most teams, female and male researchers equally carried out excavations and wrote papers. One of SDS's mandates was to address a disproportionate quantity of African scholars excavating and publishing in the field of palaeoanthropology, a discipline largely controlled by outsiders to the continent (Shepherd, 2002: 205; Wadley, 2014: 209). Palaeoanthropologists listed as a co-author every researcher who had contributed to a project, and each contributor then shared in the all-important credentials that the act of publication conferred (Finlay, 2014: 145-181; Latour and Woolgar, 1986: 187-230). These papers also immortalised and extended some of the temporary associations – the associations between scientists in complementary fields, in particular – that the spokespeople of research groups forged while in Oldupai.

SDS researchers sought to illuminate the forces that drove humanity's emergence, and their baseline was modelling a period of fluctuating – and generally increasing – aridity that transpired in the Oldupai region 1.8 to 1.3 million years ago. Palaeoanthropologists were seeking to understand connections between this long-term drought and how ancient hominins began producing a variety of stone tools called the Acheulean, how hominins used such tools for processing food, and how carnivore-free patches of vegetation and water within a gradually opening and formerly forested landscape might have inspired new hominin social arrangements and behaviours. A researcher eloquently emphasised to us the centrality of this drought and the importance of not solely analysing fossils and stone tools: "It is not just archaeology that we are interested in, but the total environmental change".

Engaging in what palaeoanthropologists called a 'multi-proxy approach', researchers examined an amalgamation of various traces of past environments that they had excavated – such as biomarkers and phytoliths – in order to clarify the environmental conditions that may have influenced hominins to change their patterns of behaviour and to develop the Acheulean stone tool industry roughly a million and a half years ago. Lipid 'biomarkers', such as normal (*n*-) alkanes, are chemically inert organic compounds that resist biodegradation throughout extremely large stretches of time, and analysing the isotopic values of biomarkers that are found in the sediments that surround buried stone tools can reveal changes in past plant composition and availability of water (Eglinton and Eglinton, 2008; Patalano, 2019). Biomarker research that SDS scientists conducted confirmed a drying and warming trend at an Oldupai Gorge site in the deep past, and demonstrated that the site was likely a riverine forest surrounded by a mosaic-like landscape that featured both heavily treed areas and more open environments (Patalano et al., 2017). Another trace of the past are 'phytoliths', which begin to form when live plants absorb silica and the silica subsequently fills cell spaces. While plants eventually die, these silica casts of plant cells can preserve for millennia, allowing researchers to compare the shape of such phytoliths to those from reference collections of contemporary vegetation in order to understand the types of plants and environments that existed in the past (Gallagher et al., 2015: 1-2; Itambu, 2020; Zhang et al., 2011). SDS researchers analysed phytoliths that they collected at three other Oldupai sites and revealed that they indicated a diverse, gradually opening,

and progressively drying landscape (Itambu et al., 2017).

This long-term drought became the ancient environmental context in which SDS explored a variety of connections and associations between tool types, tool use, and hominin diet and social behaviours. Through various practices, SDS was enacting (Mol, 2002) a drought that occurred in the deep past. In an ANT framework, the researchers harnessing biomarkers and the scientists using phytoliths each made particular associations between heterogeneous actants – such as theories, methodologies, new data, laboratory techniques, and the countless supplies and relationships that made possible excavations at particular Oldupai Gorge sites – to enact this drought. Illustrating how actants need not be material, researchers established and corroborated the drought's existence and subsequently mobilised the *idea* of this ancient arid period into new associations. Such associations included those that allowed SDS to establish new scientific facts about prehistoric hominins through writing publications, publications that also support academic livelihoods (Finlay, 2014: 145-181; Latour, 1987, 1993a: 158-236, 2005; Latour and Woolgar, 1986: 187-230; Mol, 2002: 44-164; Mortensen, 2016: 61-62; Rabinow, 1996: 19-31; Smart and Smart, 2017: 37).

Thus, abiding by the 'multi-proxy' approach, researchers enacted (Mol, 2002: 1-51) drought by establishing different arrays of associations, such as those associated with either biomarkers or phytoliths. Rather than discovering a singular nature, researchers used these particular practices to enact two different versions of the same bygone drought, multiplying reality and ontologies. However, much like the composite disease diagnoses that Mol (2002: 53-117) outlines, SDS researchers could compile these site-specific practical enactments, thereby turning the composite drought into a foundation in which to explore other as-of-yet unclarified connections between stone tools, the landscape, and ancient diets. Researchers referred to this drying period as a singular event, preventing complete fragmentation (Mol, 2002: 108-117) of the enacted drought(s).

The drought had profound effects on humanity's shared past in Oldupai yet allowed contemporary scientists to continue making palaeoanthropological discoveries. Due to the relative brevity of field seasons, the costs of field research in a place distant from their homes, and the need to produce credibility-granting publications based on their research in a flagship research locality; palaeoanthropologists spent their energies exploring bygone times, such as the implications of the ancient drought. Navigating such exigencies, along with possessing the financial and logistical means to bring water to the research camp, meant that a contemporary drought – one affecting the Oldupai of today and the people who live there – usually remained peripheral to enacting the past.

Maasai drought

One afternoon, we were walking with some researchers and a Maasai warrior to a palaeoanthropological dig site. Our route through the sharp acacias took us down the Gorge's walls and through its dried-out basalt base. Upon reaching this parched bedrock, the warrior showed us a hole. A metre deep, two metres wide, and surrounded by a fence of barbed acacia tree branches that kept wild animals at bay, the hole harboured traces of water. A few days later, we were walking with a Maasai elder. We eventually found ourselves meandering through the bottom of the Gorge, and came across two Maasai girls who were crouched beside a hole in the ground. The girls had a donkey with them, along with two large containers. Further on, we passed a pool of water that was surfacing before our eyes. A girl emerged from the acacias and allowed her goats to drink from the pool. The elder explained that this saline water was not ideal for human consumption, yet using a shovel to dig a hole, he proclaimed that buried water was more drinkable. Thus, it was not only palaeoanthropologists whose livelihoods centred around digging in Oldupai Gorge. As the following pages will show, these digs were also a means to address desiccation, yet this reduction in water was much different from that being reconstructed by researchers.

The Maasai's established brand of pastoralism depends on unobstructed access to communal

resources (Galvin et al., 2008: 274) and making associations with a wide variety of actants. We joined Maasai pastoralists as they left their villages in the morning and herded livestock to forage, and as they acquired water for their own consumption, for their livestock, and for medicinal soups. These soups contained numerous medically-active plants (Chapman et al., 1997), and interlocutors explained that their ethnomedical resources were absolutely vital since biomedical facilities were located nowhere near Oldupai. Maasai pastoralists placed the blood and meat of livestock into soups, and at other times drank their animals' milk.

To support their livelihoods in Oldupai and at home, palaeoanthropologists navigated large forces – such as the consequences of global capitalism and neoliberalism – and forged a wide variety of associations. Comparably, political-economic forces that are also tied to the spread of capitalism and neoliberalism have carved out natural/social hybrid landscapes – lucrative and ostensibly 'natural' spaces such as Serengeti National Park that Maasai peoples have been evicted from – and have compromised the above brand of established pastoralism in the NCA by fragmenting rangelands and restricting the Maasai's access to dry season water sources (Blim, 2000: 27-31; Crewe and Axelby, 2013: 89-90, 159; Cronon, 1995; Galvin et al., 2008; Hodgson, 2011: 64-75; Latour, 1993b; Nelson, 2012).

While conceptions of 'nature' vary between knowledge systems (Satsuka, 2015: 6-7), a wide variety of analyses have explored the creation of protected 'natural' spaces across Africa, where the proliferation of such spaces has engendered countless disputes (Neumann, 1998). Conservation frameworks tend to envision separations between 'social' and 'natural' realms (Saj et al., 2006), and Brockington (2002) defines the widespread practice of evicting local people whose livelihoods depend on engaging with environments to be 'fortress conservation', which can be seen in various points in Tanzania's history. European imperialism and colonialism were accompanied by a mindset that rigidly separated human and natural/animal worlds, exemplified by the eviction of Maasai peoples from Serengeti National Park (McCabe et al., 1992; Walley, 2004: 170-174). Following the nation's 1961 independence, conservation projects have almost always omitted the knowledge of rural people while relying on that of 'experts' and scientists, even if those projects incorporate local populations (Walley, 2004). Conservation actors have been more recently focusing on the spaces *surrounding* Tanzanian parks that wildlife migrate into (Goldman, 2009: 342), and the expansion of neoliberal frameworks in Tanzania has seen the promotion of Wildlife Management Areas (WMAs), spaces beyond park boundaries where local communities are encouraged to forge partnerships with foreign investors to develop wildlife-based businesses (Igoe and Croucher, 2007). However, it would seem that fortress conservation (Brockington, 2002) lives on, as Igoe and Croucher's (2007) research revealed that countless members of local communities had not been informed of – nor consented to – WMAs being implemented and felt that WMAs precluded access to natural resources. Explaining that those communities who are adjacent to parks and who can partake in WMAs are usually conceptualised merely as tools for the agendas implemented by conservation agencies, Goldman (2003) suggests that two shifts in conservation frameworks must materialise in order for projects such as WMAs to be effective and to truly move on from fortress-style exclusionary management. The first shift is now frequently realised and entails regarding local people as people who can be partnered with, yet the second and much more elusive shift is the act of legitimising – instead of excluding – the knowledge of local communities. As this paper will show, we join calls for comparable shifts (Ichumbaki et al., 2019) to begin characterising palaeoanthropological research *within* protected 'natural' spaces such as the NCA.

Another large and related force that Maasai peoples face is the lingering idea – despite evidence demonstrating how pastoralism is ecologically beneficial – that Maasai pastoral livelihoods are damaging to Tanzania's famed protected spaces that tourists come to see (Nelson, 2012). This notion that downplays Maasai perspectives and concerns (Galaty, 2002) is a powerful actant that Maasai peoples must navigate, and is made of other actants such as tourist memorabilia that paints the Maasai as

archaic and timeless (Hodgson, 2011: 64-70). While modernity does not exist according to the qualifications established by 'modern' societies, in which humans completely separate 'natural' entities from the 'social' (Latour, 1993b: 67-90), the creation of 'natural' spaces and the eviction of 'premodern' people from them highlight that the *idea* of modernity remains a powerful and influential force.

These various top-down forces – assemblages of actants that are making established forms of pastoralism nonviable (Galvin et al. 2008) – have dissolved what were relatively stable (Smart and Smart, 2017: 54) Maasai actor-networks. For example, actants essential to pastoralism, such as crucial dry season water sources, lay in national parks that are next to the NCA and inaccessible to Maasai peoples. Comparable to scientists acquiring key resources in non-scientific or 'social' realms to support their scientific livelihoods and 'pure' science within laboratories (Latour, 1987: 45-162), Oldupai Maasai made ventures out into non-pastoral worlds, forging novel associations and obtaining heterogeneous resources/actants that could support 'pure' pastoralism. Social structures do not perpetually reappear generation after generation (Rosaldo, 1980: 1-28); Maasai peoples had to renegotiate the existence of their pastoral actor-network. They are not a 'timeless' society.

Maasai interlocutors often explained that they were staying in the challenging NCA region because it was their home, yet also because goats and sheep – unlike cattle – could thrive in extremely arid conditions: "we like it here because of the goodness of our pastoralism, especially for our goats and sheep. The plain is good for the sheep and goats". We learned that these small stock animals would die from diseases and frigid temperatures in highlands, yet also that since it was the ever-lengthening dry season, numerous warriors had left the permanent Oldupai villages to take cattle to mountainous regions. In one of many types of modifications to established forms of 'pure' pastoralism, Oldupai Maasai forged new associations and acquired key resources by making adjustments to prevailing gendered social roles (Wangui, 2008: 369-370): whenever there were no males around to herd remaining livestock to forage and water, women and girls stepped in to fill this role.

Like researchers, Maasai revealed frustrations over navigating top-down decisions and forces. They also emphasised that their isolated rural homeland was lacking social services such as water, hospitals, and schools. While the Maasai placed great value on ethnobotanical medicines, they also desired hospitals. If Maasai peoples needed hospital attention, they had to sell livestock to raise funds for a journey to medical facilities. The closest of such facilities were in Endulen, over 60 kilometres from Oldupai. Maasai residents repeatedly stated that various groups have promised services and infrastructure, yet have never delivered. Many Maasai also felt as though they were not permitted to have what Maasai peoples deemed 'modern' developments because tourists arrive in the region looking for wilderness. These travellers bring large quantities of capital into Tanzania's economy (Nelson, 2012), yet Maasai communities have largely been unable to access tourism revenue (Galvin et al., 2008: 260-261). Thus, Maasai were seeking to acquire resources – actants that were not components of established or 'pure' pastoral practices – to support their animal husbandry.

Oldupai Maasai used to acquire food by trading with agriculturalists in the NCA, but a cultivation ban within the Conservation Area has made it difficult for Maasai to access items such as maize. Pursuing elusive cash, Maasai have begun selling their cattle or have migrated to unfamiliar urban areas. In Zanzibar, we talked to Maasai warriors who had migrated there to sell beaded goods to tourists or to perform dances in luxury hotels, hoping to acquire money to re-purchase liquidated cattle stocks. These migrants were also likely capitalising on the widespread representation of Maasai peoples as timeless and exotic (Galaty, 2002; Hodgson, 2011: 66-68; May and Ikayo, 2007). In Dar es Salaam, we observed Maasai women vending beaded jewelry, along with warriors selling medicinal plants that had originated in rural areas. These Maasai were making new associations to continue their pastoral ways, yet males who had attended a certain coming-of-age ceremony were not to interact with those who had not. Demonstrating how all societies are dynamic

and adjust to historical happenings (Rosaldo, 1980: 1-28), Oldupai Maasai have begun initiating youth at a younger age than in the past. As a result, when these youth temporarily migrated to cities, they were able to interact and share resources with older Maasai, which strengthened support networks while away from the NCA.

The top-down partitioning of formerly open landscapes and a lack of access to dry season water caches was just one facet of the increasingly desperate water situation for Oldupai Maasai peoples. Parallel to palaeoanthropologists, Maasai pastoralists were enacting (Mol, 2002) drought. Similar to how multi-faceted conditions of scarcity within their actor-networks in 2009 led the Maasai peoples whom Goldman et al. (2016) worked with to declare and enact drought and subsequently move to other regions, despite influential climate scientists using simplistic rainfall measurements to instead declare 2010 to be the more severe drought year, various different members of Oldupai Maasai communities gauged the conditions of multiple factors and actants in order to announce the arrival of what they frequently called drought.

A decrease in rainfall was part of the enactment in Oldupai. The Gorge is a river that flows with bountiful water during the rainy season, yet it unfortunately dries up completely as the arid season annually sets in. In recent years, the wet season has become significantly shorter. Many Maasai pastoralists attributed this seasonal metamorphosis to global climate change, which will continue making unpredictable impacts on East African ecosystems (Galvin et al., 2008: 273-274). However, this drought was more complex than just reduced rainfall.

Diminished water availability made negative impacts on Maasai nutrition and health: the diminution of forage that came with an extended dry season worked to emaciate and kill livestock, thereby diminishing the meat and milk available for human consumption. Relatedly, rangeland fragmentation – such as what happens when Maasai are denied access to places that contain vital water sources – can create congestion of livestock and a subsequent proliferation of cattle disease (Galvin et al., 2008), and because of a higher prevalence of livestock diseases, many Oldupai Maasai are now apprehensive of consuming their animals' milk and blood. We spoke to multiple interlocutors who had gone over two days without consuming food, and when we asked a focus group if Maasai peoples ever stray from their famed diet of milk, blood, and meat, a Maasai man stated that "yes… the only way to take care of our children. It has come the time with no milk and meat, we have to buy [agricultural foods] for children". Such products were produced outside of the Conservation Area and could be purchased in settlements that were distant to Oldupai. Transport to these places was exceedingly challenging, yet agricultural goods could also be acquired from temporary Maasai markets and small trading kiosks located throughout the NCA.

Preparing preventative and curative soups was becoming difficult. Maasai interlocutors said that they absolutely required water in order to transform medicinal plants, which they defined as vital in the rural Oldupai region, into effective treatments. One man scoffed and laughed when asked if he could use these ethnobotanical resources without water. Furthermore, along with forage, medicinal plants were also disappearing, and this botanical exodus was tied to drought.

Not unlike the palaeoanthropological 'multi-proxy' approach to modelling an ancient drought, in which various researchers with unique specialisations combined their distinctive enactments, different members of Oldupai Maasai communities brought their own concept of drought into being by making associations between the conditions of multiple different actants throughout the region. To enact drought, Maasai peoples combined knowledge about pastoralism that elders had passed down, the inability to enter neighbouring national parks, a shorter wet season, climate change, livestock diseases, food shortages, and disappearing botanical resources. This drought was also a composite enactment (Mol, 2002: 53-117): the drought consisted of the various aforementioned indicators, each a distinct practical enactment of drought engendered by different members of the community, which created multiple versions. For example, some Maasai peoples enacted the specific version of drought that related to the disappearance of

botanical resources by making astute associations between a wide variety of plant and tree actants and their own theoretical knowledge regarding such resources' conventional viability and availability. The Oldupai Maasai community coordinated and prevented the complete fragmentation of this composite drought, which like a diagnosed disease "appears to be more than one – without being fragmented into many" (Mol, 2002: 151), by collectively referring to it in English as 'drought' and in their native tongue of KiMaasai as *alamei*. This term also translated to 'dry season', an increasingly complex period of thirst that Maasai interlocutors explained had not only been more severe and lengthy than it had been in past years, but was also being exacerbated by being unable to access formerly accessible dry season resources located in Serengeti National Park, which neighbours the NCA. Once this temporally- and community-specific drought was enacted, Maasai pastoralists mobilised the all-encompassing drought into novel associations with other actants, becoming a foundation for further action.

The most common response to this drought entailed unearthing the remnants of water that had previously flowed through the Gorge; a river during rainy months. Maasai men dug holes in Oldupai's base, exposing water that livestock could consume on the spot. Females scooped the water into plastic jugs, loaded these containers onto donkeys, and brought the water home. This water was oftentimes saline and potentially unsafe: livestock frequently walked through the water, and Maasai peoples discussed how the lack of medical facilities and transportation in the area meant that children who had to drink "dead water" were suffering from brucellosis and were unable to acquire treatment in distant hospitals. Furthermore, many Maasai infants were suffering from diarrhea, one of the leading causes of death for Tanzanian youth (Mohamed et al., 2016: 33). A local, reflecting on the salty and dirty water, lamented that "[there is] no way out, we have to drink it". Each excavation only remained viable for two or three days before it ran dry, and as the dry season progressed, this buried water completely disappeared.

Palaeoanthropological enactment of drought facilitated the productive subsistence of researchers. *Prior* to enacting drought, scientists *dug* in the Gorge for myriad traces of the past in order to assemble and corroborate the idea of an ancient period of desiccation. The scientists then turned the concept of the drought into a backdrop – a contemporary actant – in which to engage in the present-day practice of exploring emerging ideas about the past, which they later published. Conversely, once the Maasai enacted drought and corroborated its objective existence, they sought to *counteract* it, actions analogous to the numerous practices that Mol (2002: 87-117) describes in which various types of doctor intervene with and treat an enacted disease. The most common of the remedial practices, *digging* in Oldupai's basalt floor for traces of water that was abundant in the recent past, came *after* the enactment of the drought ravaging present-day Oldupai. Facing such pressing issues and troubles, palaeoanthropological information about the deep past was on many occasions irrelevant to Maasai peoples.

Various members of Maasai and palaeoanthropological communities each enacted multiple versions of drought, thereby multiplying reality and ontologies, yet these numerous enactments hung together in two composite forms of drought. Each group's composite (Mol, 2002) drought was quite different, however: scientists desired bygone drought that was crucial to their subsistence and livelihoods, as drought became a vital part of publications, while the Maasai detested contemporary drought that was compromising their subsistence and livelihoods. Ultimately, these conflicting enactments of drought were actants that coexisted side-by-side via spatial distributions of reality (Mol, 2002: 87-117). Palaeoanthropological practices, such as digging within various specific archaeological sites throughout the Gorge and conducting experiments and research within laboratory settings, gave rise to a composite drought. Concurrent Maasai practices in other spots around the Oldupai region, such as judging and evaluating rainfall patterns, livestock numbers and viability, food and water availability, the implications of borders around protected spaces that restrict access to water and forage, and the condition of medicinal plants, enacted numerous versions of drought that hung together

in a composite form that diverged from that of researchers. A different site-specific practice, digging in the base of the Gorge, was geared towards ameliorating such thirst.

Discussion

Applying an ANT framework at Oldupai demonstrates that Maasai and palaeoanthropological subsistence strategies had to effectively navigate large social, political, and economic contexts that were themselves massive assemblages of actants (Latour, 1993b: 96-124, 2005: 1-93). Locals and researchers both expertly acquired and assembled countless varieties of actants in non-pastoral and non-scientific worlds to support their respective livelihoods (Latour, 1987: 45-162). As part of their subsistence, both Maasai peoples and palaeoanthropologists created and multiplied reality and ontologies by enacting their own composite and group-specific versions of drought that ultimately conflicted (Mol, 2002), as one drought was reviled for its ongoing devastation to contemporary Maasai pastoralism while the other existed millions of years ago and was much sought-after for its capacity to contribute to palaeoanthropological publication-based livelihoods. The exigencies associated with Maasai and palaeoanthropological subsistence have been hindering the emergence of a meaningful interface between the groups, despite the fact that members of both dug in the Gorge to address drought: Maasai peoples have been excavating buried water due to facing devastating long-term water security and water safety issues, issues which may make palaeoanthropological information about the lives of hominins in the deep past seem irrelevant, whereas researchers were excavating traces of an ancient drought to navigate a publish-or-perish academic climate while conducting relatively brief and expensive fieldwork excursions, a research context that could make community engagement seem extraneous.

By using a symmetrical approach (Latour, 1993b) to understand the multiple drought-based livelihood exigencies that have impeded collaboration between Maasai and palaeoanthropological communities at Oldupai, this paper *also* demonstrates that theories and frameworks associated with the field of science and technology studies (STS) can be fruitfully employed to understand the construction of knowledge and ontologies located outside of academy-based science. This assertion is much in line with Nader's (1996) contention that Indigenous knowledges are in fact a component of humanity's rational scientific knowledge, since they are most often based on reasoning and experience. Nader (1996) argues that there are forms of rationality that are not identical with Western rationality, and thus that the West is not the only place where 'scientific' dispositions emerge.

While we take inspiration from Goldman et al. (2016) in striving to flatten knowledge hierarchies in which it is only Western scientific frameworks that guide policy, we are also inspired by Ferguson's (2011) espousal of a politics that strives to experiment and to exercise power provisionally instead of merely engaging in the easy practices of condemnation and critique. Ferguson's (2011) politics are based on Foucault's proclamation that the world could never be *without* power – which entails influencing others' behaviour – along with Foucault's questioning of *how* power should thus be enacted. Likewise, we recognise that we are academy-based scholars whose research carries with it a certain sway, a sense of authority, and a capacity to reach distant audiences (Bourdieu, 1975; Brown, 2009; Goldman et al., 2016; Latour, 1993a; Reay, 2007), and Maasai interlocutors explicitly requested that we make the issues they face known to wider audiences. We suggest that when in a position to conduct research amongst non-academic communities who desire for policy makers to understand and address their realities, STS scholars have a responsibility to write publications regarding the ontologies (and associated knowledge) of those communities who exist outside of academia, thereby working within extant power structures to widely disseminate realities that are usually fairly localised and unheeded.

Archaeological practice can reconstitute the social, political, and economic contexts in which it operates, especially by extending official discourse (Abu El-Haj, 2001: 1-21), a discourse which in the case of Oldupai has been well-intentioned yet may be effacing or hiding Maasai ontologies. By enacting an ancient drought

and by excavating hominin bones and stone tools in order to understand the evolutionary emergence of humanity, palaeoanthropologists were retrieving evidence of a shared human past in the region. This common past is a message that many organisations have promoted to tourists, who make massive contributions to Tanzania's economy. Helping to establish the Gorge as a place to uncover scientifically and economically valuable discoveries – findings that illustrate the commonalities between all humans in an all-too-divisive world – may have inadvertently contributed to shrouding Maasai existence in the Oldupai region (Abu El-Haj, 2001: 1-21; Nelson, 2012; Staniforth, 2009: 167-169). If policy makers assume that scientists have a privileged perspectival access to a singular reality and provide legitimacy solely to scientific ontologies and entities by basing decisions *only* on scientific models, there can be profound consequences for underrepresented communities (Goldman et al., 2016; Latour, 1993a: 215-236, 1993b: 13-112). Numerous organisations –including academic publishing firms, funding agencies, and various governments and universities – legitimise the pursuit of palaeoanthropological drought. While this has been a scientifically significant and well-intentioned endeavour, two salient realities for Maasai peoples – drought and exclusion from palaeoanthropology – persist unrecognised (Galaty, 2002). Thus, not only have palaeoanthropological frameworks taken precedence over Maasai ontologies; palaeoanthropological fieldwork and research into a *shared* human past has also continued to *exclude* Maasai peoples within what is frequently described as a cradle of humanity.

Many members of Maasai communities sought to engage with palaeoanthropology as a means to initiate long-term careers in the discipline and to supplement their valued pastoral livelihoods that are being increasingly challenged within the NCA, a protected space where livelihood diversification options are limited (Galvin et al., 2008). Working with researchers is yet another example of Maasai peoples creatively acquiring resources in unfamiliar realms in order to continue practicing pastoralism (Latour, 1987: 145-162; McCabe et al., 2014). SDS has begun collaborations with Maasai pastoralists, who shatter common and unjust representations of Maasai communities as archaic peoples by possessing profoundly sophisticated knowledge that is equally logical to that of palaeoanthropologists (Lee et al., 2019), and these collaborations have not been extraneous. Rather, they have been mutually beneficial, as Maasai peoples are contributing their valuable knowledge to SDS research projects, SDS palaeoanthropologists and Maasai peoples are working together to co-produce new knowledge about the deep past, and Maasai interlocutors explained to us that the wages that they earned from working with SDS researchers allowed members of Maasai communities to supplement their pastoral practices by purchasing water and food from waystations located throughout the NCA during extremely scarce dry season periods. Palaeoanthropologists can thus help to legitimise and ameliorate Maasai concerns about drought *and* exclusion from research, yet much work remains, and we suggest two primary areas where such work should be concentrated.

Two suggestions by way of conclusion

One suggestion for future work is for STS scholars, as we propose research into the agency that people such as the Maasai practice when scientific research is carried out in their lands. For example, while African peoples have continually refused the top-down imperial and scientific practice of cartography, they have also contributed to map-making projects or appropriated cartographic frameworks to meet their own political goals (MacArthur, 2016). Relatedly, in an analysis geographically distant to this research but thematically affinitive, Mickel (2019) explores how local populations hired as labourers at nineteenth century archaeological excavations in the Middle East – workers who possessed valuable and indispensable skillsets and knowledge – had the agency to hinder *or* promote archaeological knowledge production. Some of these labourers resisted unfavourable working conditions and alienation, while others established their own expertise and indispensability to become trusted experts who oversaw excavations. We thus suggest further long-term ethnographic research amongst such

researcher-local community collaborations at Oldupai or other human origins research sites (on the rare occasions they do arise) as a means to understand not only the reasons why researchers do or do not pursue collaborations, but also the agentive contributions provided by local populations to palaeoanthropology.

The second suggestion for future work is based on our ethnographic research but offered to other people who have vested interests in Oldupai Gorge, and this suggestion is inspired by Goldman's (2003) assertion that conservation projects can only be effective if they entail partnering with local people *and* legitimising their knowledge, along with the call by Ichumbaki et al. (2019) for human origins researchers in the NCA to not only hire Maasai peoples but to recognise their vast knowledge while co-producing knowledge with them. At Oldupai, it is still common for Maasai peoples to be excluded from research projects (Lee et al., 2019; Mehari and Ryano, 2016), meaning that our second suggestion is more fundamental than our first, and one that we hope this paper supports. As a normalising and humanising narrative of the Oldupai Maasai's exclusion (Galaty, 2002) from palaeoanthropology *and* the hardships they face, we hope that this paper can contribute – by becoming a potent actant in the forthcoming associations (Latour, 2005: 121-140) that both palaeoanthropologists and policy makers will forge – to a future in which the Maasai's devastating version(s) of drought becomes like that of palaeoanthropological researchers: firmly in the past.

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Enacting the Pandemic: Analyzing Agency, Opacity, and Power in Algorithmic Assemblages

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Abstract

This article has two objectives: First, the article seeks to make a methodological intervention in the social study of algorithms. Second, the article traces ethnographically how an algorithm was used to enact a pandemic, and how the power to construct this disease outbreak was moved around through an algorithmic assemblage. The article argues that there is a worrying trend to analytically reduce algorithms to coherent and stable objects whose computational logic can be audited for biases to create fairness, accountability, and transparency (FAccT). To counter this reductionist and determinist tendency, the article proposes three methodological rules that allows an analysis of algorithmic power in practice. Empirically, the article traces the assembling of a recent epidemic at the European Centre for Disease Control and Prevention—the Zika outbreak starting in 2015—and shows how an epidemic was put together using an array of computational resources, with very different spaces for intervening. A key argument is that we, as analysts of algorithms, need to attend to how multiple spaces for agency, opacity, and power open and close in different parts of algorithmic assemblages. The crux of the matter is that actors experience different degrees of agency and opacity in different parts of any algorithmic assemblage. Consequently, rather than auditing algorithms for biased logic, the article shows the usefulness of examining algorithmic power as enacted and situated in practice.

Keywords: algorithm, assemblage, situated opacity, power, pandemic

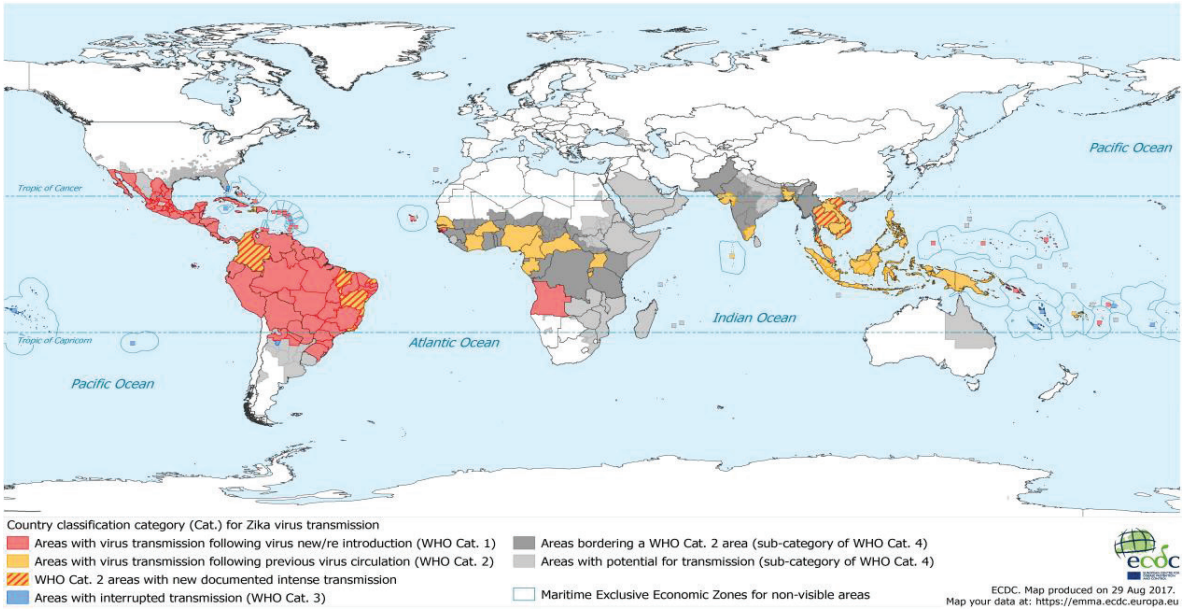

Figure 1. The Current Zika State as published on the website of the European Center for Disease Control and Prevention on 29 Aug 2017

The aim of this article twofold: to make an empirical contribution to our understanding of how pandemics are put together with algorithms, and a methodological intervention in the social analysis of algorithms. As multiple epidemics and pandemics sweep our interconnected and globalized world—not least the COVID-19 pandemic which is holding society in a vice as I write this—it is becoming crucial to understand how a pandemic comes about through various infrastructures, algorithms, models, sensors, practices, and decisions.

The empirical focus of the article is the algorithmic making of the Zika pandemic which emerged in close proximity to the Olympics in Rio de Janeiro in 2015-2016, and started spreading around the world, raising concerns that the disease would spread globally.[1] The article traces the work of assembling a particular version of this epidemic, called the Current Zika State—a map of the spread and intensity of the Zika pandemic (see Figure 1). The Current Zika State was the official version of the Zika pandemic that the European Center for Disease Control and Prevention, the ECDC, published. The Current Zika State was generated by what my informants called the Zika Algorithm.

The empirical aim of the article is to shine light on how this algorithm enacted the Zika pandemic. That is, how the social and natural orders of Zika were assembled at the ECDC and how various quantifications, models and classifications—from faraway times and places—were folded into the Zika Algorithm with consequences for different actors' space for agency, opacity, and power (cf. Lee et al., 2019). For my informants, the Zika Algorithm promised automation, simplicity, and orderliness—an unambiguous and automated map of the Zika pandemic.

This means that we are dealing here with a particular kind of assemblage that my informants and I refer to as an algorithm.[2] In practice, an algorithm is a multifaceted object that can be many different things and is interpreted and used differently in different settings. But, rather than defining it along the lines of computer scientists or health professionals (who call different things algorithms) this article approaches algorithms in an emic manner (cf. Dourish, 2016; Seaver, 2017). This means that I follow my informants in their various makings of what they call the Zika Algorithm. In essence, I follow the work of my informants of assembling, stabilizing, and interpreting the Current Zika State with the Zika Algorithm, and how agency, opacity and power were distributed in this assemblage.[3]

Using this case as a springboard, I argue that much social analysis of algorithms risks falling in

an epistemic trap by importing a stabilized and delineated notion of algorithms from computer science (cf. Muniesa, 2019). I argue that this epistemic trap underpins much of today's algorithmic critique which focuses on the power of the black-boxed algorithm (Pasquale, 2016), different degrees or types of opacity (Burrell, 2016; Diakopoulos, 2020; Larsson and Heintz, 2020), biased search results (Sandvig et al., 2016), algorithmic oppression (Noble, 2018; O'Neil, 2016), or quantitative auditing (Sandvig et al., 2014).

This argument is political to the highest degree. What is at stake is how we understand and analyze power in an algorithmic society. The crux of the matter is that arguments about automated, black-boxed, biased/objective, opaque/transparent algorithms perform a punctualization of agency and politics, which risks transferring the politics of technoscience to the realm of the technical artefact, rather than how power is distributed in assemblages. As one of my interlocutors humorously put it: "Well, isn't it useful to point the finger at the algorithm!"—when we should be having discussions about the use and broader effects that algorithms have on society.[4] Instead of analyzing the racist algorithm we should be looking at racist assemblages—and where the possibility for agency, choice, and power reside in these assemblages.

By following the algorithmic enactment of the Current Zika State, the article demonstrates the usefulness of three methodological rules in the study of algorithms: 1) Don't punctualize agency to the algorithm; *instead attend to how agency and choice are assembled*. 2) Abandon the opaque/transparent binary; *instead attend to multiple and situated translucencies*. 3) And last, dispense with the algorithm as the prime mover; *instead attend to how power clusters and disperses in assemblages*. The argument is that we need to analyze the effects that different algorithmic assemblages have in the *multiple practices* where they are made, tinkered with, and used, rather than focusing on the inherent politics of the black boxed algorithm.[5]

Algorithms, the very idea: on the epistemic trap of the black boxed algorithm

Social studies of algorithms have since the outset acknowledged the fluidity and assembled nature of algorithms, at the same time as the field has lamented the inscrutability and power of the algorithm. Goffey (2008) for instance describes the nature of algorithms as part of long chains of actions upon actions, at the same time as he argues that "Algorithms do things, and their syntax embodies a command structure to enable this to happen" (Goffey, 2008: 17). While Gillespie concludes that the "there may be something, in the end, impenetrable about algorithms." (Gillespie, 2014: 192).[6] Powerful black boxes indeed.

Thus, in debates about the theory and methods of social studies of algorithms, there exists an oscillation—often in the same papers—between on the one hand acknowledging the fluidity, complexity and assembled characteristics of algorithms (Seaver, 2017, 2018), and on the other hand a notion of algorithms as stable and delineated objects, existing out there, that can be unfair, unaccountable, opaque, and biased, and in the need of auditing in order to rectify said biases (Diakopoulos, 2016; Pasquale, 2011; Sandvig et al., 2014).[7]

My argument is that this oscillation in how algorithms are understood analytically—on the one hand as fluid and assembled, and on the other as delineated objects that can be made fair, accountable, and transparent (the famous FAT movement, now FAccT) makes for a precarious analytical vantage point for the social sciences.[8]

One reason for this precariousness is that we social scientists, as Muniesa has argued, have taken over a "vocabulary of information in the analysis of social realities" (Muniesa, 2019: 200). That is, by taking on the vocabulary of our informants—that we indeed analyze an object called *algorithm*—we have fallen into an epistemic trap that delineates, stabilizes, and delimits our objects of study as well as our analytical problems.

My point is that we—along with our informants, computer science, and the media—perform *algorithms* as clearly delineated and pre-existing objects that can be analyzed in themselves. By falling into this epistemic trap we perform algo-

rithms as *punctualized* (Callon, 1991). That is, by treating algorithms as stable objects "out there" we—social scientists—ascribe agency and power to this performed object, instead of paying attention to the assembling of agency in practice and the performative effects that the assemblage has (cf. Callon and Law, 1995).

Consequently, when we talk about the "power of the black boxed algorithm" or "auditing the algorithm," we start thinking about this performed object as being the object for our own studies—with concomitant forms of problematizing algorithms in society. In this way the algorithm becomes a seemingly naturalized object for social analysis, which leads to particular forms of political and analytical action: *Analyze and audit the powerful black boxed algorithm!*[9]

The two cultures of algorithm studies

These different analytical approaches to algorithms—one where they are understood as delineated and stabilized objects and the other as fluid and assembled—create very different understandings of the politics of algorithms. And also very different problematizations of how to analyze the currently unfolding algorithmic society (cf. Lee and Björklund Larsen, 2019).

The stabilized notion of algorithms seems to treat algorithms as having political qualities "under the hood." In this view, the algorithm—as an object for us to audit and investigate—is treated as having stable qualities that shape society in particular ways, which makes the "politics of the artefact" the natural analytical focus (cf. Winner, 1980). Consequently, we take on the epistemic objects of computer science as our own—and become auditors of the stabilized algorithm. (Auditing is of course an important function in a world run by algorithms. No disagreement there!) But, I argue, nonetheless this epistemic trap leads to simplified understandings of how algorithmic power works in society.[10]

In my view, the central problem with the stabilized and punctualized understanding of algorithms is that we take on a reductionist and determinist view of the politics of algorithmic assemblages. For sure, algorithmic assemblages *do have power*. But it is seldom an autonomous power, where algorithms act on their own to oppresses the poor, but rather as Goffey (2008) and others have acknowledged, it is a rhizomatic and capillary power that works by actions upon actions.

This article suggests that to understand the politics of algorithms we need to be wary of importing this punctualized view of the "black boxed algorithm," and keep our awareness of how algorithmic assemblages structure power in multiple and dispersed practices. Don't let social science become the algorithmic auditors that computer science might imagine it needs.

In sum, my argument isn't that algorithmic assemblages are powerless—quite the contrary they are very powerful—but rather that we as social analysts need to be aware of when we are taking over the object definitions of computer scientists, politicians, or auditors as they risk leading to impoverished understandings of how our world is enacted with algorithmic assemblages. Below, I will show the futility of attending to the punctualized version of algorithms and, in conclusion, suggest three methodological rules to break out this epistemic trap.

Let us now return to the issue at hand, the enactment of the Zika Algorithm and the Current Zika State—and how spaces for agency, transparency, and power are configured in these assemblages.

Assembling pandemics

In disease surveillance today, the knowledge of disease outbreaks is increasingly produced through an abundance of technical, political, and animal infrastructures.[11] These infrastructures are constantly working in organizations across the globe. In the west, the US Center for Disease Control (CDC), the WHO, and the ECDC are endlessly monitoring their screens, attempting to detect the next big outbreak of disease; in the South and East the Chinese and Brazilian CDCs are important hubs.[12] Currently, these information infrastructures are reshaping our knowledge about epidemics: new disease patterns and outbreaks becoming visible through the development of new infrastructures (Caduff, 2014; Kelly, 2018; Lee, 2020; Mackenzie, 2014; Sanches and Brown, 2018).

At the ECDC outbreaks are continually being assembled, updated, displayed, and debated about. A host of methods are used to classify and value disease intensities, disease threats, and disease risks, which can lead to conflicts between different actors about the understanding of an outbreak (Keck, 2008; Lee, 2020). One visualization, which has been part of disease control for hundreds of years, involves enumerating cases in time and space. Others involve making risk calculations based on environmental models, tracing food stuffs through distribution networks, tracking genetic relations between pathogens, or using social media to find likely places of contagion.

To visualize these classifications and valuations of disease intensities, risks, and predictions there are a number of well-established visualizations that are harnessed in order to establish where in time and space the outbreak is at the moment. For example: There is the infamous epidemic curve, or epicurve, which enacts a disease as the number of cases on a timeline. The epicurve is an iconic part of disease surveillance highlighting the disease intensity over time, producing images of the development of a disease outbreak, COVID-19, different strains of Flu, Ebola, and Zika are all visualized in time series showing the severity of disease (cf. Kelly, 2018; Mackenzie, 2014). There is the contact-tracing chart, which exhibits a network of potential disease pathways between patients.

Last, and most importantly for the assembling of the Current Zika State, there are an abundance of maps produced visualizing where disease risk and disease intensities are highest. The production of maps lies at the heart of disease control. Maps are produced of most outbreaks on different scales and with different purposes. COVID-19 maps, influenza maps, yellow fever maps, Zika maps, Ebola maps. For the surveillance of disease, maps are tools to determine the source of disease, tools for tracking how a disease spreads, or a tool for making recommendations for action.

The visualizations that are produced at the ECDC make political waves. Disease is tied to lock-downs, tourism, food supplies, and work. The presence of disease is a delicate matter: COVID-19 maps reshape our whole lives, Zika created headlines around the world, Salmonella can cause the closure of industrial egg handling facilities, and so on. This puts knowledge production of pandemics in a position where disease, international trade, national economies, international relations, tourism, and national politics can become implicated at any moment.[13]

At the ECDC these visualizations are crucial tools for understanding, discussing, and communicating with disease professionals, decision-making politicians, and the public. Below we follow the assembling of a particular visualization, a map that classifies the world into different intensities of disease, and different areas of disease risk. The Current Zika State is a vehicle for enacting a classification of society and nature. It produces the social and natural orders of a Zika pandemic.[14]

An ethnography of the Zika Algorithm

Empirically, the article draws on fieldwork in a larger project that examines how new information infrastructures shape disease surveillance. The project commenced in 2015 with a preliminary study that inquired into the rise of so-called infodemiology, that is, the harnessing of new types of data in disease surveillance (cf. Fearnley, 2008). These new types of data can for example entail genetic data, web searches, tweets, sales data, or travel information.

The material for this article used a strategy of multi-sited ethnography to follow the assembling of the Current Zika State inside and outside the ECDC (cf. Marcus, 1995). The article draws on a multitude of different materials and strategies of data collection: situated fieldwork, document analysis, and interviews done during 2016, 2017, and 2018. Hence, in this engagement with the Zika Algorithm, I followed the assemblage through a variety of places, situations, and materials.

The fieldwork at the ECDC consisted of three weeks of participant observation in the epidemic intelligence team in early 2017, as well as weekly follow up observations with other teams during the following spring. During the first round of fieldwork I worked in the so-called epidemic intelligence team, where I performed routine disease surveillance. This team is tasked with trawling social media, news media, and a constant flow of email to find and assess new disease threats.

During fieldwork, I attended meetings, participated in staff training, and interviewed my informants formally and informally in the epidemic intelligence team and other teams.

This situated fieldwork served as a springboard for a wider investigation into the making of the Zika Algorithm, where I complemented the initial period of participant observation with interviews and extensive document analysis following where the algorithmic assemblage led. Thus, the current article draws on participant observation, informal conversations, interviews, working documents, flowcharts, official ECDC publications, as well as other scientific publications.

Accordingly, this paper takes its starting point in the observation of a meeting about the Zika Algorithm at the ECDC, and branches out into interviews, observations from other meetings, document studies, and interviews. In short, I have followed the Zika Algorithm to the many sites where it was assembled.

The Zika Algorithm

Me and my informants Thomas, and Bertrand are in the Emergency Operations Centre—a situation room for disease control—at the ECDC after the daily roundtable meeting. At this daily meeting, disease experts from across the ECDC gather to assess today's disease threats against the European population. Thomas and Bertrand are gearing up to have a meeting on an algorithm that produces the Current Zika State.

Bertrand has worked for months to produce this new algorithm for classifying the world into zones of Zika risk. The goal is to construct an algorithm that will help to automate the work of putting together a snapshot of the Zika epidemic. This particular meeting is to start translating the Zika Algorithm (in the form of a logical flowchart) into layers of code, visualizations, and software. The resulting map, titled the Current Zika State, is published online, and included in regularly recurring reports about the state of the Zika epidemic.

At the meeting, the visual focus has become a flowchart (Figure 2) as well as a bewildering array of database tables. They are all projected on the

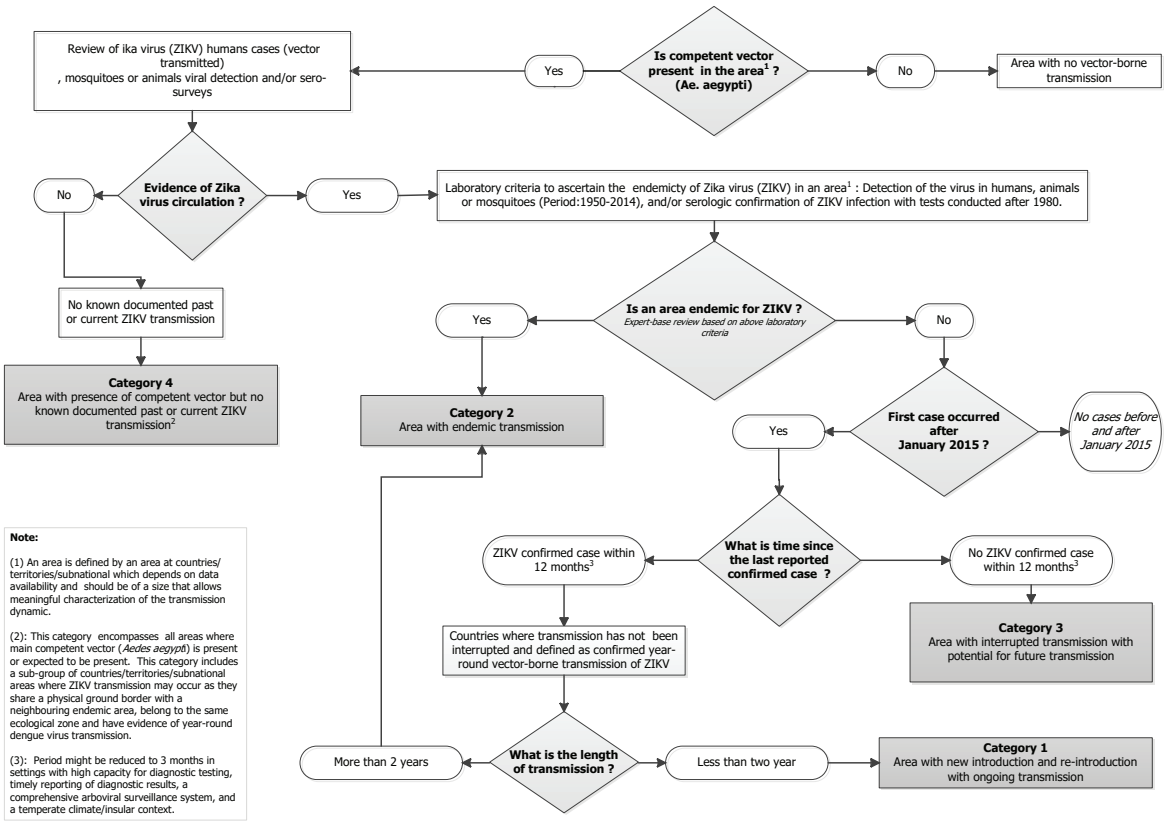

Figure 2. Bertrand's provisional Zika algorithm

huge screen that fills one wall of the Emergency Operations Centre. The flowchart visually outlines the Zika-algorithm that is used to classify a country's Zika risk.

After quickly running through the flowchart version of the algorithm, Bertrand clicks between different columns and datasets in the database to show what data is needed to produce the color-coded Zika map. Bertrand keeps saying that it is easy, and Thomas keeps nodding his head in agreement.

—"The classification is based on dates. Basta!" says Bertrand

The classification of geographical regions according to when the date of the last case was reported is treated as unproblematic. Everyone agrees—the classification based on dates is unproblematic. The question "where is Zika?" has become phrased as "where and when are Zika cases reported?" (Fieldnotes)

At the most basic level, the Zika Algorithm is assembled as a series of questions that aim to produce a Zika classification of the world:

- ◊ Is the mosquito that transmits Zika, *Aedes aegypti*, in the area?
- ◊ Is there evidence of Zika virus circulation?
- ◊ Is an area endemic for the Zika virus?
- ◊ When did the first Zika case occur?
- ◊ What is the time since last confirmed case?
- ◊ What is the temporal length of transmission?

These questions, which are also articulated in the flowchart version of the Zika Algorithm have been translated into computational form by Thomas and Bertrand—layering, as we will see below, several computations, databases and models and transforming them into a series of classifications that are then shown on a world map—the Current Zika State. However, things are not simple and straightforward. As we will see below, Bertrand's statement "The classification is based on dates. Basta!" is full of caveats, nooks, and crannies. At every turn, the production of the algorithm is folded with different datasets, different manners of judgment, and different tools for counting and classifying. And all these folds configure algorithmic power differently (cf. Mackenzie, 2014).

ECDC: situated translucencies | situated agencies

Back to Thomas and Bertrand's meeting: the quality of disease surveillance in different countries has come up. The meeting has paused for a moment. After clicking through a myriad of tabs on the database, Bertrand stops his clicking and highlights a yellow-tinted column of data on the wall-screen of the Emergency Operations Centre. He explains that the database column he has highlighted details the quality of disease surveillance in different countries. Bertrand zooms in on the column and shows that it classifies the surveillance capabilities of different countries as better or worse: "good," "medium," or "bad." Bertrand and Thomas seem to take this classification for granted and nod their heads in agreement. Thomas turns to me and tells me:

—"The Brazilian CDC is very good!" (Fieldnotes)

Thomas' point is simple. Not every country on the globe has the same infrastructure for disease surveillance. And the ECDC wants to take this into account in producing the Current Zika State. What Bertrand is showing on the screen—good, medium, bad—is that the Zika Algorithm is also inscribed with an assessment of the quality of different countries' disease surveillance systems. What my informants are concerned with is: should each Zika case be counted in the same way?[15]

The Zika Algorithm is inscribed with several questions about the reported cases of Zika (see Figure 3): "What is the time since the last reported confirmed case?" If there is a confirmed Zika case reported within the last 12-month period, the algorithm asks about the length of the period where Zika cases have been reported. If the period is less than two years long, the country is classified as "Category 1: Area with new introduction and re-introduction with ongoing transmission." If the period is more than two years long, the country is classified as "Category 2: Area with endemic transmission." If there are no confirmed cases within 12 months the country/area is assigned to "Category 3: Area with interrupted transmission with potential for future transmission."

By asking these questions Bertrand erects several temporal boundaries that define different

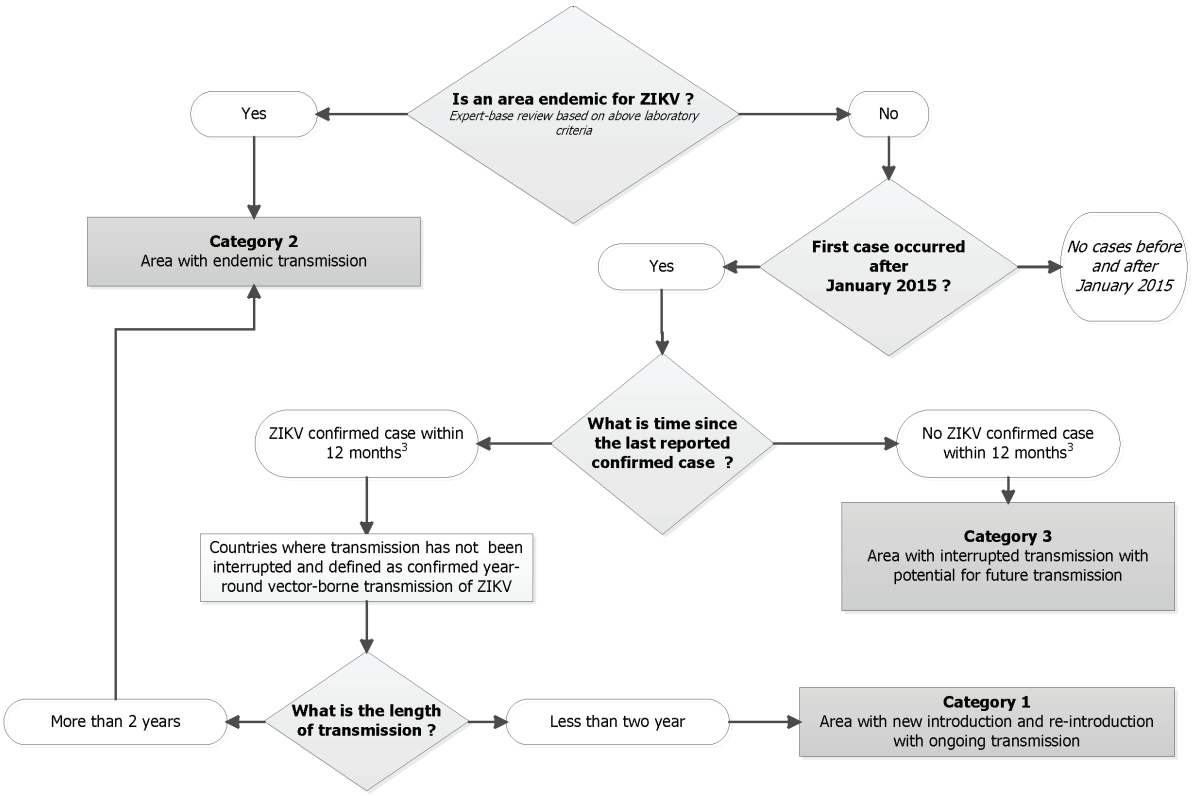

Figure 3. A bifurcation in the Zika algorithm. A zoomed in version of the Zika Algorithm above.

classes of Zika transmission: zero Zika-cases in the last 12 months means interrupted transmission. More than two years of transmission means that the area has endemic transmission. The Zika cases are ordered temporally, counted, and used to construct different classes of Zika risk.

However, in a footnote to the flowchart version of the algorithm, Bertrand's and Thomas' concern for the quality of disease control—"The Brazilian CDC is very good"—and the yellow tinted column of data that Bertrand showed on the wall-screen is brought to the fore. The footnote specifies that the 12-month temporal boundary between different categories should be reduced to 3 months if the quality of the disease surveillance infrastructure of a country is deemed to be good. The Zika Algorithm thus links the counting of cases to the quality of surveillance. The footnote consequently outlines a bifurcation in how reported confirmed Zika cases are counted:

> [The] Period might be reduced to 3 months in settings with high capacity for diagnostic testing, timely reporting of diagnostic results, a comprehensive arboviral surveillance system […] (see Figure 2)

To define the quality of the countries disease surveillance the algorithm asks more questions: Does the country have diagnostic capacity? Timely reporting? A good surveillance system? Which Thomas, at the meeting, shortens to "The Brazilian CDC is very good!" The reported confirmed cases of Zika are counted differently, which has the effect that each Zika case does not carry the same weight on the scale of Zika risk.

A consequence of this bifurcation of the yardsticks for Zika risk is that the boundary between different classes of Zika risk is not equal for all countries. Some groups of countries are judged by certain temporal yardsticks (12 months), while other countries are judged with another yardstick (3 months). But there is no visible trace of this bifurcation of boundaries in the Current Zika State. Clearly apparent categories of disease intensity are made visible on the map—red, orange, grey, white. No intermediate colors or categories. No fuzziness between classes. And no bifurcations of yardsticks. The categories and classifications of the Zika map appear as neatly delineated and unambiguous. The Current Zika State projects an image

of an unambiguous world where Zika presence is clearly visible and bounded.

The Zika Algorithm intertwines quantification, judgment, agency, and opacity in several ways. That is, there are many ways that the algorithm composes power. *This is a matter of the ontological politics of algorithms, of where choices can be made, and where power to effect things clots.*

First, the configuration of choice: The Zika Algorithm was designed to "simply count cases in space and time." But the boundaries between different classes of disease intensity were also intimately intertwined with the judgment of the quality of disease surveillance. The algorithmic logic of the Zika map was not only about quantities, counting cases in space and time, but about qualities as well. Algorithmic quantification and judgment were entwined—but not equally distributed.[16]

Second, the making of situated algorithmic opacities: these struggles point to the importance of attending to the assembling of opacities in practice. For the general public viewing the Current Zika State map online, the map, the algorithm, and the choices made around it, are completely opaque. It seems to be a stable map of the Current Zika State. A classic algorithmic black box if there ever was one. However, the bifurcation of yardsticks was clearly visible, present, and understood by Thomas and Bertrand in the Emergency Operations Centre. *Different degrees of opacity depend on the actors' locations. This is an important methodological and analytical point: opacity is not binary or universal, opacity is situated.*

Third, this points to how the making of algorithmic agency, opacity and power is an achievement in practice. In this particular moment, agency and power to classify countries into different Zika zones was located with a particular set of experts in the Emergency Operations Centre at the ECDC. However, other actors, not present in the Emergency Operations Centre, were excluded from this moment of choice, and this particular moment of power. *The making of opacity and power happens in practice. And different algorithmic assemblages configure opacity, agency, and power in different ways* (cf. Mackenzie, 2014).

Oxford: layered translucencies | dispersed agencies

But there is also a different and parallel mode of sensing Zika at work in producing the Current Zika State, which creates another configuration of agency and power: that of environmental and ecological modeling. In this mode of sensing the Zika Algorithm is assembled to make Zika risk predictions based on computational models instead of on counting cases. For the algorithm, and the disease surveillance team at the ECDC, the attention is switched from counting disease cases in space and time to computing the potential presence of a Zika disease vector. Here, the algorithm moves from classifying a country based on counting reported confirmed cases of Zika to classifying the Zika state of a country based on computer modeling and risk prediction. In this mode of sensing Zika, the algorithm asks several additional questions:

- Is the *Aedes aegypti* mosquito "expected to be present" in the area?
- Does the area "share a physical ground border" with an endemic area?

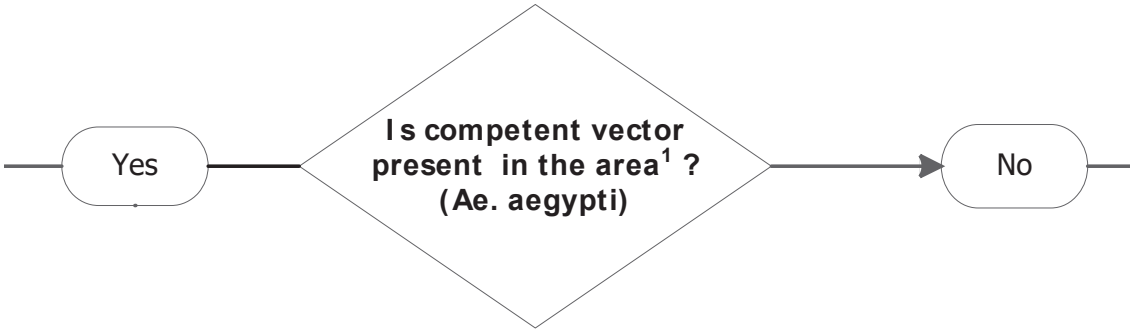

Figure 4. The Zika Algorithm branches out in time and space.

Figure 5. The Aedes aegypti.[19]

- ◊ Are the areas part of the "same ecological zone?"
- ◊ Is there "evidence of year-round dengue virus transmission?"

Each of these questions entangle the Current Zika State with other objects: mosquito ecologies and ecological zones (Is *Aedes aegypti* expected to be present?), physical geographies (Does the area share a physical ground border?), as well as with other diseases (dengue fever is also spread by the *Aedes aegypti*). Due to these questions, several different computer models become entwined with the Current Zika State. The Zika Algorithm is not only assembled to ask "Where is Zika?," but also "Where is the *Aedes aegypti* mosquito?;" "Where is there dengue-fever?;" and "Where is a fitting ecological zone?" To follow one part of this rhizome of algorithmic classification, we shift our attention to how the *Aedes aegypti* mosquito is included in the Zika Algorithm (see Figure 5).[17]

Importantly, for our story, is that the *Aedes aegypti* mosquito is understood to be the most important disease vector for the Zika virus.[18] The actors' argument is that knowledge of where the mosquito roams, will also allow an assessment of the risk of Zika virus transmission. Thus, to know where the *Aedes aegypti* might exist expands the modes of sensing Zika.

To trace how the *Aedes aegypti* becomes included in the assemblage we move our story to a group of ecological modelers at Oxford University, where the computational model that the ECDC uses to predict *Aedes aegypti* presence was produced (see Figure 6). Over several decades, this group has developed a modelling approach that attempted to find covariances between species' habitat and environmental factors:

> […] we concentrate on the use of maps to increase our understanding of the biological and other processes that determine the distribution and abundance of species in space and time. Which are the important variables; how do they act; and how do they differ […]?
> (Rogers, 2007: 3)

The group's computational methodology to trace different species was based on linking known geographical habitats with environmental factors extracted from satellite data or climate databases, such as temperature, rain, elevation, or density of

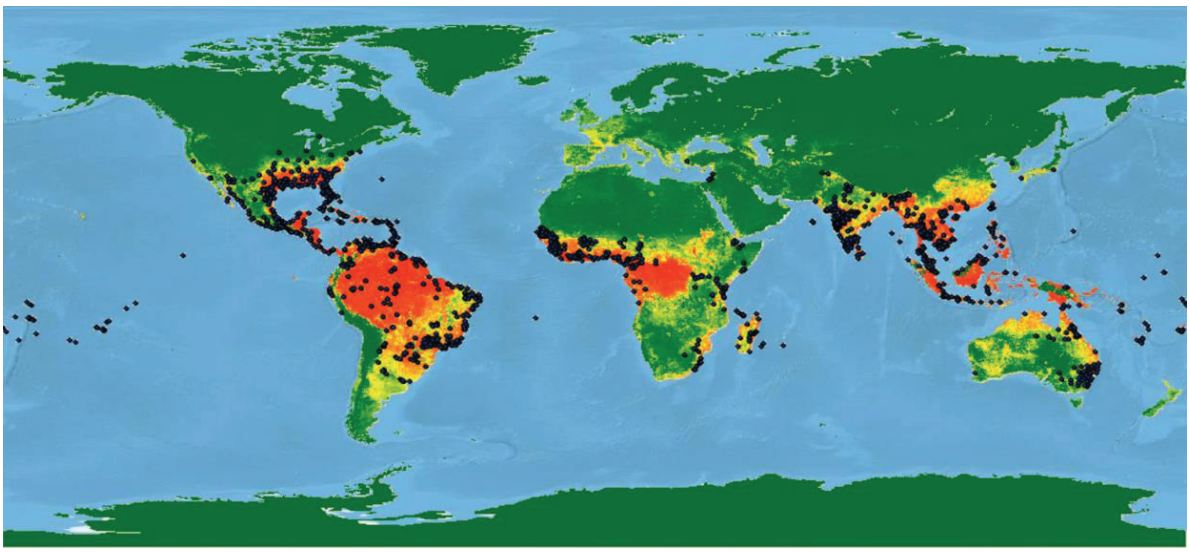

Figure 6. Map of Aedes aegypti risk. Green marks low risk. Yellow and orange denotes increasing risk for presence of Aedes aegypti.[20]

human habitation. The model used at the ECDC matched known locations of the *Aedes aegypti* with environmental characteristics to predict the mosquito risk on a global scale.

The Oxford group used two sets of data to model *Aedes aegypti* risk: First, a bespoke database of where the *Aedes aegypti* is found. This database was produced by combining known *Aedes aegypti* occurrences harvested from the scientific literature, with an *Aedes aegypti* map produced by the United States CDC (see Figure 7).[21] Second, the group utilized environmental data stemming

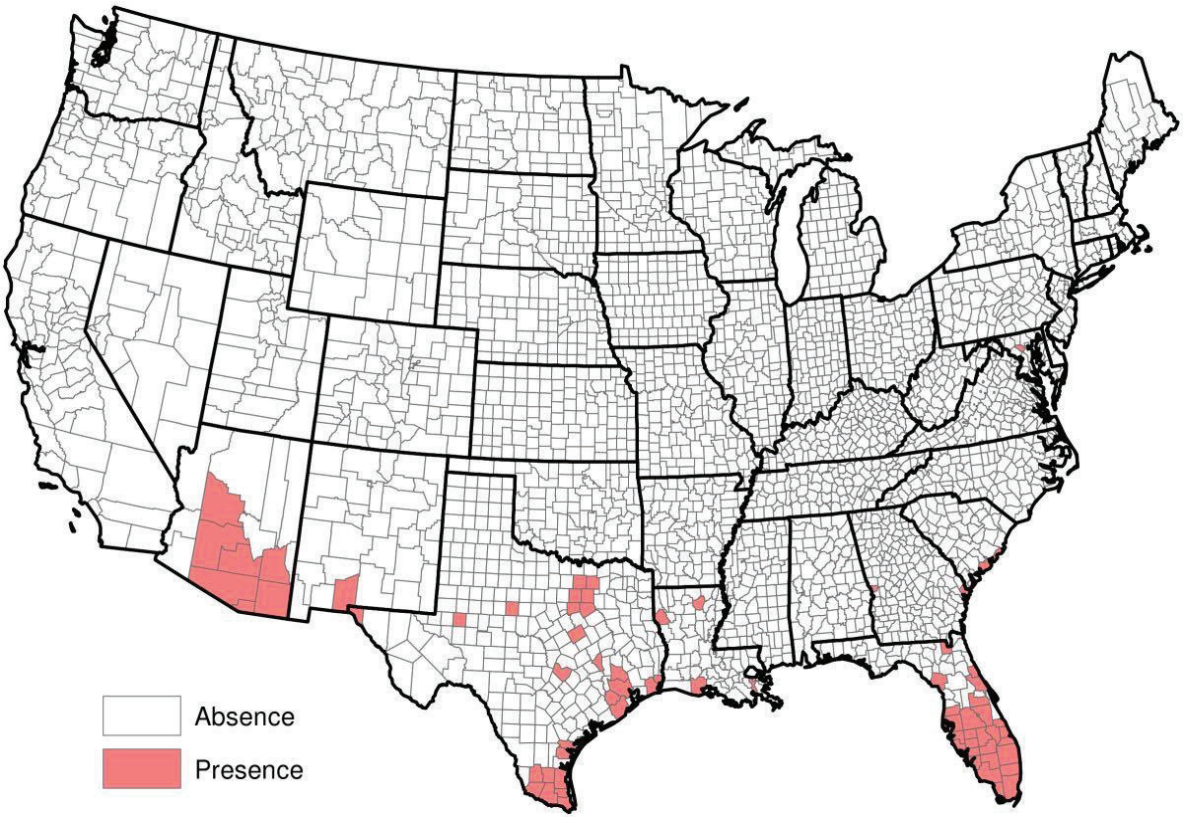

Figure 7. Aedes aegypti map of the USA from 2008.[23]

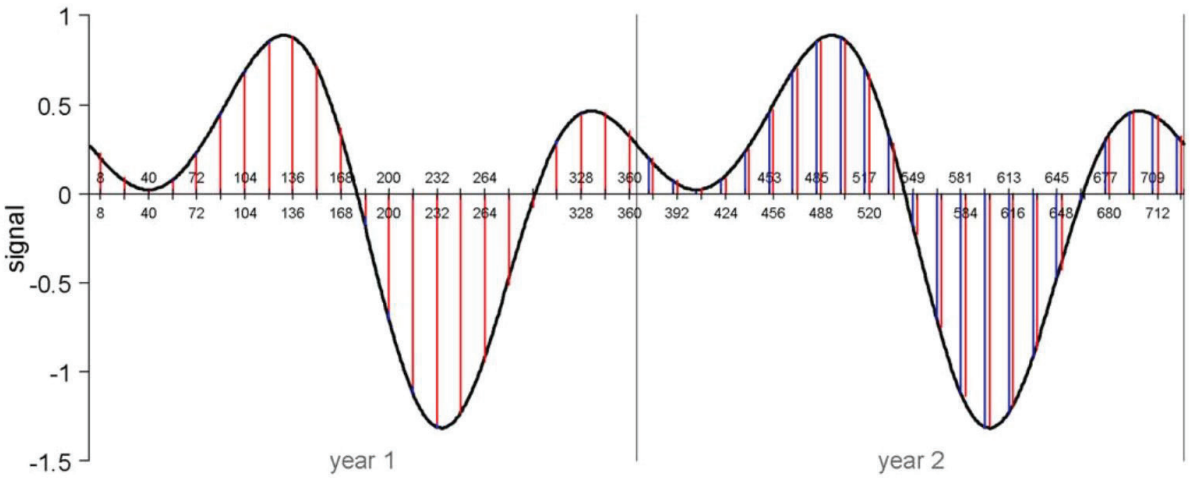

Figure 8. An illustration of the result of a Temporal Fourier analysis of environmental data (Scharlemann et al., 2008: 8).

from satellites and climate databases from a host of different sources. For example, they used "satellite rainfall estimates" stemming from the US National Weather Service (which incorporated data from nine different satellites) as well as data from several different climate models.[22]

This was the basis for their modeling of the *Aedes aegypti*: finding covariances between a bespoke database of mosquito occurrences and several layers of environmental data taken from many different sources.

In order to simplify the global environmental data, the team used a mathematical technique called temporal Fourier analysis, which is used to simplify the representation of different types of signals (see Figure 8). By using this technique the Oxford team transformed the environmental data into a series of mathematical formulas that aimed to describe "information about the seasonal cycles of these indices in terms of their annual, bi-annual, tri-annual etc. cycles (or 'harmonics'), each one described by its phase and amplitude" (Rogers, 2000: 138). These "harmonics" were then used to create several climate zones, that the team sometimes represented by transforming the environmental harmonics into colors on a map (see Figure 9).

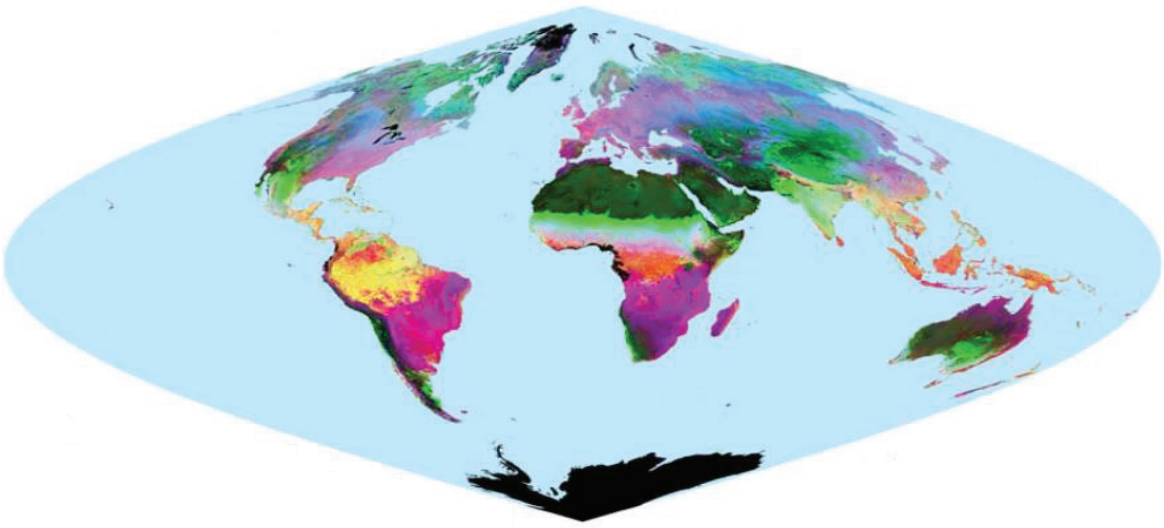

Figure 9. Temporal Fourier analysis of global Enhanced Vegetation Index (EVI) translated into ecological zones on a map (Scharlemann et al., 2008: 7).

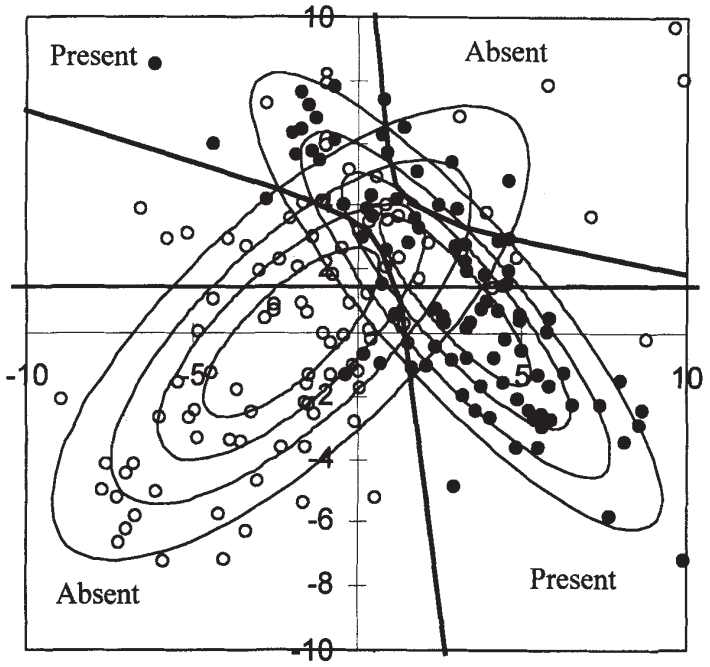

Figure 10. Illustration of Non-linear discriminant analysis (Robinson et al., 1997: 237).

To then find covariances between the environmental data, derived from the temporal Fourier analysis, and the mosquito data, the Oxford group employed a computational technique called non-linear discriminant analysis (see Figure 10). This technique was used to predict the risk of *Aedes aegypti* occurrences on the basis of the many different environmental harmonics outlined above (rainfall, vegetation, etc.).

The mode of sensing described above illustrates how the Current Zika State is built by incorporating many different times, places, people, computations, and efforts. And, as I hinted at the outset of this section, the Current Zika State is not only tied to the *Aedes aegypti* model. The Zika Algorithm also incorporated a dengue fever model produced by the same Oxford team, as well as a climate classification of the world based on the commonly used Köppen-Geiger climate classification of the globe—which was first published in 1884 (see Figure 11 for a recently updated version). The algorithm thus expanded backwards and outwards in both time and space. Through a veritable flood of computational resources, a map of Zika risk was born.

Let us now return to our questions of assembling of agency and opacity. That is, to how the assembling of the Zika Algorithm enacts particular patterns of power. Where is agency to classify located when Zika is sensed by modelling risk of mosquito presence? What types of situated transparencies are made through the Zika algorithm? And where is power located?

In the case of the *Aedes aegypti* risk map, agency is certainly focused in Oxford, with the team of ecological modelers. But agency is also dispersed over a vast array of places and times: it is located at the US CDC in producing the *Aedes aegypti* map of the USA; in entomological expeditions globally trying to determine where the *Aedes aegypti* thrives; in satellite sensors that detect radiation, rainfall or vegetation; and at the ECDC who have commissioned the modelling from the Oxford team and integrated it into their work (cf. Segata 2020, Edwards, 2010).

Agency and power are thus dispersed over time and space, in various parts of the algorithmic assemblage at different locations, with relations to other infrastructures, other scientific teams, and other practices. There is a myriad of classifications made in a myriad of places. There is no center of

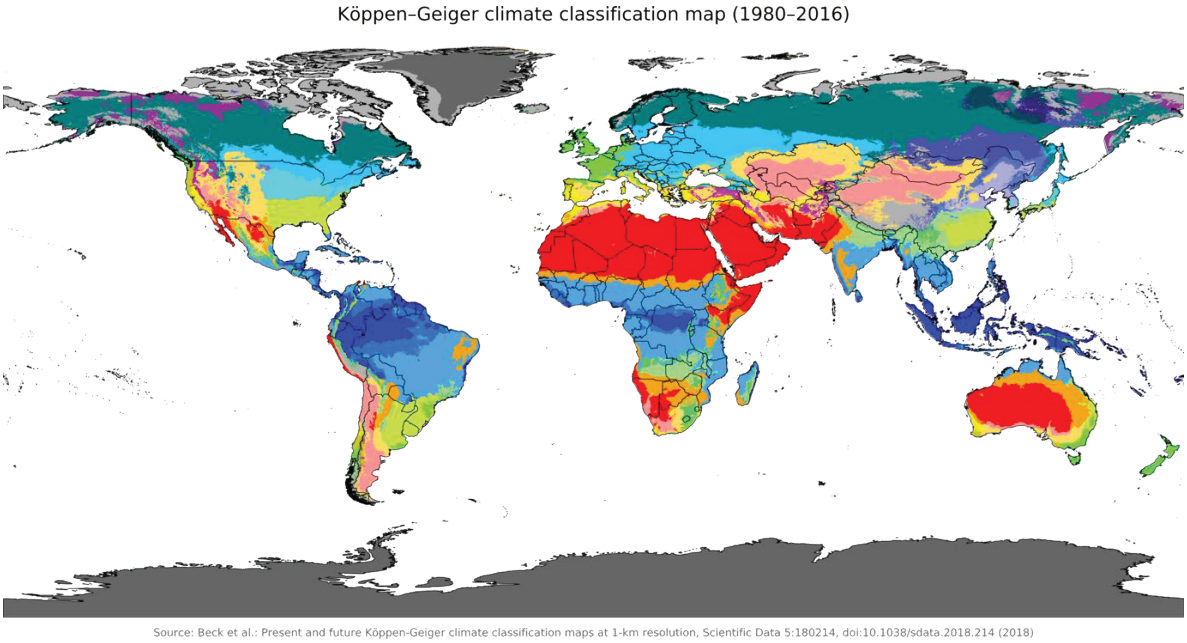

Figure 11. A Köppen-Geiger climate classification map.[24]

calculation, nor a central algorithm, but a heterogeneous assemblage of dispersed calculative efforts (cf. Latour, 1987).

Consequently, in terms of enacting opacity and calculative power, the classification of Zika risk is multilayered, multi-situated and extremely dispersed. For a handful people at the ECDC some layers of computation (the *Aedes aegypti* risk map, the Dengue map, or the Köppen-Geiger map) are visible as computational possibilities. But some parts of the computational assemblage, recede into the fog of the unknown.

Various facets of algorithmic classification and valuation thus drop in and out of visibility depending on the actors' location in the assemblage. The algorithm is never completely opaque, but neither is it completely transparent. My argument is that opacity is multi-situated and dispersed. There is a series of translucencies of varying degrees that spread over different practices (cf. Jordan and Lynch, 1992). In contrast to the counting cases in space and time, this mode of sensing, seems to produce a dispersed calculative power which is not located in a single place.

Sri Lanka and Pakistan: translucency and situatedness

Coming back to Thomas' and Bertrand's meeting, another senior team member, Sergio, has joined the meeting in the Emergency Operations Centre. Talk has moved to the question if a country or area shares "a physical ground border," another question inscribed in the algorithm, and if this can be handled automatically. Sergio brings up Sri Lanka:

"Is Sri Lanka ecologically connected to India?" he asks.

On the scale of a world map, Sri Lanka and Tamil Nadu look quite separate. I don't understand the question. But Sergio points to the series of shoals and small islands called Rama's Bridge (see Figure 12). Does Rama's Bridge connect the two land masses? And more importantly, is it a passage for the Aedes aegypti mosquito? On the map of the Current Zika State, Tamil Nadu in southern India is classified as having Zika transmission, and tinted orange. Should Sri Lanka then be classified as being at risk or not?

Bertrand and Sergio are arguing that there needs to be a "sort of expert-based judgment" on whether a country shares a physical ground border or not.

Figure 12. Rama's bridge between Tamil Nadu and Sri Lanka.[25]

> "There will always be exceptions. We can't automate bordering countries." Bertrand says. (Fieldnotes)

Apparently, the algorithm cannot handle all physical ground borders.

The challenges that Rama's bridge, and cases like it, pose to the mapping of Zika across the globe are monumental. Disentangling and classifying the world's geographical borders in terms of mosquito—geography is an enormous task, needing intimate knowledge about the geographies and ecologies of regions as well as reliable and comprehensive data on the range of the *Aedes aegypti* mosquito (cf. Segata 2020). Thus, the alluring promise of algorithmic classification threatens to fall apart in the face of ecologies and the range of the infamous *Aedes aegypti*. Thomas, Bertrand and Sergio are caught in an algorithmic dilemma. What parts of the Zika Algorithm are possible to automate fully? And what parts need human judgment? Where should agency reside? Ontological politics to the highest degree.

> A few weeks after the meeting I visit Thomas and Bertrand to discuss the Current Zika State again. Thomas tells me that the ECDC and the WHO have had a disagreement about the Zika classification of India. The point of contention was that two cases of Zika had been reported in Pakistan. According to the Zika Algorithm, that event should have reclassified the whole sub-continent of India as having risk for Zika transmission. However, the ECDC argued that the reclassification of the whole Indian sub-continent on the basis of two travel-related cases was absurd. A billion people would have been affected. The WHO on the other hand argued that the algorithm should be followed. The challenge was one of judging the output of the algorithm. What was a good tipping point for reclassifying the whole Indian sub-continent? Were two travel-related cases enough? (Fieldnotes)

The data points of disease surveillance are shifting, ambiguous, and spotty (cf. Gitelman, 2013). They change over time, they come with different definitions. Is this case laboratory confirmed? Does it fit with the current definition of a case? What did the contact-tracing of the case show? Was the case travel-related? The counting of cases takes judgment and choice. How should two travel related cases in Pakistan be counted? And in the case of Rama's Bridge: How should the intertwining of mosquito ecologies and political geographies be handled?

My point is that data—even counting disease cases—is also translucent to different degrees, depending on your location in the assemblage. In the case of Rama's Bridge, Sergio struggled with the qualities of geographical boundaries. The simple algorithmic definition of "sharing a ground border" is judged to be too complex to automate when dealing with mosquito ecology. That is, Sergio grappled with reconciling the qualities of ecological zones, the challenge of judging the possibility of zika transmission over ecological habitats, and how to make this disease risk visible in terms of political geography.

An algorithm for classifying the world necessarily needs to simplify, but in the simplification, we might attend to what is made visible and invisible. What is hidden from view, and for whom? And what are the consequences of doing this? At the ECDC, the space for choosing path—agency and power—was often large. The Zika algorithm and the underlying data was constantly challenged, tweaked, and reconfigured before it was published as the Current Zika State. However, the spaces for choosing path were not equally distributed. Different modes of sensing produced different configurations of power.

Some lessons about the politics of algorithms: punctualization, situated translucency, and power

Above, I have followed how the Current Zika State was assembled, in doing this I attended to how different modes of algorithmic sensing produced different spaces for agency, translucency, and power. But rather than taking as point of departure preconceived notions about the power or effects of the algorithm I have traced how agency and translucency varies depending on the configuration of the assemblage.

What is at stake with this intervention are issues of politics and power. I argue that by analyzing algorithmic assemblages as they branch out in multiple and situated practices, we gain a better understanding of how algorithmic power is structured and works to shape the world. Hopefully, this will allow us to leave the determinist and reductionist logic of "black boxed algorithms that control our future" behind us. The point of this exercise is to prevent that the performed object—algorithm—defines, delimits, and steers our inquiries into the politics of algorithms.

There are several lessons to be learned from tracing the Zika Algorithm, and I therefore suggest three methodological rules to protect us against falling in the epistemic trap of the stabilized algorithm.

Rule 1: Don't punctualize agency | Attend to how agency and choice are assembled

Our first lesson. Do not treat algorithms as stable objects out there for us to apprehend and analyze. Attend to algorithmic assemblages as always already distributed and performed. Algorithmic assemblages are truly fluid, heterogeneous, and dispersed. Not only in the sense that they include agencies of different shapes and forms, but also in how the qualities and characteristics of algorithmic assemblages shift and meander. These are well worn perspectives in STS, but the tendency to ascribe and punctualize agency to the powerful algorithm is tempting—and continues to tempt us.

The consequence of the assembled perspective on algorithms is that agency must be analyzed as being entangled with the objects that we perform as algorithms—not as an inherent property of them. In practice there are almost always struggles about how these assemblages should be structured. Thus, the composition of algorithmic assemblages often becomes the site of political struggles and practical tinkering to assemble these elements in different ways.[26]

For instance, as I have shown above, the space for challenging the Zika classification of a country is large in certain spaces at the ECDC—but the space for challenging the global range of the *Aedes aegypti* is smaller. The expertise and knowledge about the mathematics of mosquito prediction being located with a group of modelers of mosquitos, while expertise about disease classification was located at the ECDC. What we can observe is that the space for agency, action, or judgment is not equally distributed. The spaces for choice and action are redistributed by the different modes of sensing that the algorithm engenders.

Consequently, the power of algorithms is not a stable proposition, where some actors (e.g.

Google's or Facebook's algorithms) have all the power, and others (the technical dope that uncritically clicks on search results or consume their daily Facebook feed) have none (Garfinkel, 1967; Lynch, 2012). That is, algorithms move choice around, and the space for choice is not the same for everyone.

The point is that we can attend to how spaces for agency and choice open up and close in various places of the assemblage—how they are assembled—without succumbing to the epistemic trap of punctualization. This perspective instead raises several questions about the ontological politics of algorithms (Mol, 1999). These are age-old questions about choice and power. Where can choice be located? Who can make choices? Who or what can act? What types of actions or choices are enabled or disabled? Where? When?

Rule 2: Abandon the opaque/transparent binary | Attend to situated translucencies

Our second lesson. Do not treat algorithms as being binarily opaque or transparent. Algorithmic assemblages are always situated and translucent to various degrees (cf. Jordan and Lynch, 1992).[27] For the sociologist of algorithms it has become common to lament the power, inscrutability, and opacity of the algorithm. But the proliferation of articles classifying different types of opacity hints at a marked analytical unease about the binary notion of transparency/opacity in critical studies of algorithms.[28]

As with agency, algorithmic translucencies are assembled in multiple locations, in multiple versions, in relation to multiple people (cf. Mol, 2002). People have situated knowledge about the algorithm (Haraway, 1988). Which means that algorithmic assemblages can be differently understood in different situations and are therefore neither completely opaque, nor completely transparent. Opacity is not only varying in degree or type, but also varies depending on the actor's situatedness.[29]

In the assembling of the Zika Algorithm, choices and judgments were clearly transparent, discussed, and available for scrutiny in certain rooms and moments, like when my informants judged countries' quality of disease surveillance and inscribed their judgments into the algorithm. In other places, choices, computations, and judgments faded away and became an invisible part of an opaque algorithm, as when my informants harnessed the modelling of mosquito presence to make risk assessments about Zika. Translucencies are thus constantly opened and closed, made and remade in practice.

Our lesson is that, in practice, algorithmic opacity is neither binary nor homogeneous. It is situated, gradual, and affects different actors in different ways. We need to *analyze these various translucencies as they are enacted in various and situated practices*. What is made visible? How is it made visible? To whom or to what? Where?

Rule 3: Abandon the algorithm as the prime mover | Attend to how power clusters

As a consequence of these insights, I argue that algorithmic politics should be analyzed by paying attention to how algorithmic assemblages configure agency, translucency, and power in practice—not by auditing a sole algorithm for biases. As we have seen, different modes of sensing the Zika pandemic, makes for very different configurations of agency, translucency, and power. Counting cases and entering them into a geopolitical time and space makes for a particular configuration of agency and translucency, while environmental modelling to predict disease risk makes for a very different configuration (cf. Mackenzie, 2014). *We should* therefore *attend to the performance, locations, and unevenness of power in algorithmic assemblages*.

This is our third lesson, which is about investigating the assembling of power (cf. Callon and Law, 2005; Callon and Muniesa, 2005). The algorithmic production of the Current Zika State is a dispersed assemblage in practice. It is spread over time and space. The algorithm does not produce a center of calculation (Latour, 1987), but a vast *network of calculation* extending outward in time and space (Edwards, 2010). Thus, just as the Arizona stock exchange, shopping lists, prices on shelves, and shopping carts open and close certain spaces for visibility, calculation, and agency, so does the Zika Algorithm open and close spaces for choice and intervention. It opens and closes spaces for algorithmic power.[30]

Thus, algorithmic power accumulates in different places—as a cluster in a multitude, or a

knot in a rhizome—which can include both the mundane and the exotic, the human and the non-human. Where an algorithmic assemblage will concentrate power is not a given but negotiated in multiple situations. We need to understand how this happens to truly understand algorithmic power.

For sociologists trying to understand how algorithms shape society it is not enough to audit the stabilized algorithm for fairness or transparency. As Strathern has pointed out, "audit cannot afford to tolerate loose ends, unpredictability, or disconnections. […] This means that what may be brilliant accounting is bound to be very poor sociology" (Strathern, 2002: 309).

Hence, it is crucial to move beyond the trope of auditing the powerful algorithm, and to analyze how algorithmic systems *are designed, tinkered with, and interpreted in practice* (Neyland, 2018; Ziewitz, 2017). Algorithms involve countless situations where quantification and judgment are entwined in different ways.[31] Rather than auditing *the algorithm* (stable singular) for fairness this analytical move opens for description of how spaces for agency and avenues of seeing are made in algorithmic assemblages. Rather than focusing on the binary opacity/transparency of the algorithm, these methodological rules open for an analysis of different actors facing different degrees of agency and transparency.

Rather than punctualizing the inherent oppression or bias to the algorithm, it would open for a situated understanding of multiple power effects. Not as disembodied black boxes—truly *deus ex machina*—that reshape the economy, oppress the poor, or are race/sex/*-ist. But as assemblages of varying degrees of power (that certainly can lead to oppressive outcomes!). *By doing this we could better understand how algorithmic politics works in practice. Which (for sure!) can have effects that are oppressive, racist, and sexist.*

Conclusion: some elements in a sociology of algorithms

As multiple waves of pandemics and epidemics sweep the world—not least in the midst of the current COVID-19 pandemic—the nature and characteristics of each successive pandemic becomes increasingly tied to algorithms, models, and computation. This article has traced how a particular pandemic, of Zika, was assembled with an algorithm. I have shown how algorithms play a crucial role in assembling the intensities, risks, and projections for the future.

A pandemic is made in a web of infrastructures and practices around the world. The state of the Zika pandemic depended on practices of counting (cases and mosquitos—in hospitals, jungles, and elsewhere), computational modelling, decisions in organizations and by algorithms, and so on. In this case, the counting of cases was also folded with climate data and risk modelling the presence of mosquitos. What this shows, is that a pandemic can be composed in many different manners that are often folded together as a seamless whole (cf. Mackenzie, 2014).

But, as we have seen, not least in the current COVID-19 pandemic, constant debates about how to count erupt. What do these numbers mean? How much are we testing? Are these tests reliable? Can we trace disease through sewage? Should we count nationally or regionally? Over time, the manners in which the pandemic is enacted unfolds in different manners. That is, different manners of assembling and visualizing the pandemic are intensely political, and choices are made by different actors, in different times and places. However, it is not a politics of a sole algorithm. It is a myriad of relations that assemble a pandemic.

The article has followed some of the practices of assembling the Zika epidemic with an algorithm. Apart from making this global epidemic in particular ways, the effect of harnessing the algorithm is that it opens and closes particular calculative spaces that move agency and choice, produce particular visibilities, and shape asymmetries of power. Drawing on this analysis I have proposed three methodological rules for the social analyst of algorithms.

Rule 1: Don't punctualize agency, attend to how agency and choice are assembled

Rule 2: Abandon the opaque/transparent binary, attend to multiple and situated translucencies

Rule 3: Abandon the algorithm as the prime mover, attend to how power clusters and disperses

This analytical strategy has allowed me to outline an alternate route to analyzing politics, opacity, and the assembling of power with algorithms. This route emphasizes that algorithms are intertwined with multiple practices, and not set apart from them.

Importantly this approach bypasses the epistemic trap that results in treating algorithms as powerful black boxes that shape our lives and oppress the poor, and opens up a route for an analysis of algorithmic power as it unfolds in practice (cf. Muniesa, 2019). Thus, rather than attending to algorithms as singular and deterministic moments of fairness/bias, or as being binarily opaque/transparent, the article has followed the many ways in which algorithms come to shape agency, visibilities, and power, as well as the making of a threatening pandemic.

These analytical strategies can help focus questions around algorithmic functions beyond the common adages of "algorithms are a modern myth," "algorithms are opaque," and "algorithms shape our world." Asking about how algorithms open and close different spaces for agency and choice, produce particular visibilities, and shape asymmetries of power enables us to tell stories that are sensitive to the fluidity of algorithms, and enables an analysis of their power—as it is assembled in practice.

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Disclaimer

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Notes

1 See Kelly et al. (2020) and Löwy (2017) for a background on the Zika crisis in Brazil.

2 In this, I use the concept of assemblage drawing on Actor-Network sensibilities. That is, I see an assemblage as a network of objects of heterogeneous character whose ongoing relations shape the direction of the whole network (Callon, 2007). Other synonyms for assemblage analysis are: actant-rhizome ontology (Latour, 1999), hybrid collectifs (Callon and Law, 1995), or agencement (Deleuze and Guattari, 1987). The point of such an analysis is to not presume where agency resides, but rather to trace the relations by which it is assembled in particular locations.

3 The computer science definition of an algorithm seems to be the dominant definition in STS. In computer science thick catalogs of optimal algorithms for different tasks are published and updated regularly (Knuth, 1997). In the *The New Oxford American Dictionary* algorithms are defined as "a process or set of rules to be followed in calculations or other problem-solving operations, especially by a computer: a basic algorithm for division." The word itself is said to hark back to the name of the Persian mathematician al-Khwārizmī, who developed a systematic method for solving equations—laying the foundation for modern algebra. In other settings, an algorithm can be a simple (or complex) flowchart for medical treatment. I.e. take a sample for culture from a patient, let sample sit in a petri-dish, if there are bacteria there, give the patient antibiotics, if no bacteria are present tell her that it is probably a virus and that s/he needs to rest.

4 Thanks David Moats for this useful comment on the politics of algorithms.

5 This analytical stance draws on practice-oriented post-ANT sensibilities (Lee et al., 2019; Neyland, 2018; Ziewitz, 2017).

6 As many have pointed out algorithms are often part of a distributed assemblage (Ananny, 2016; Gillespie, 2014; Kitchin, 2016; Seaver, 2018). For instance, Seaver points out that "Presuming that algorithms must have a stable and coherent existence makes it harder, not easier, to grapple with their production and ongoing maintenance." (Seaver, 2017)

7 Sometimes these understandings of algorithms go hand in hand, for instance when complexity and the fearful opacity of algorithms seem to meet (Burrell, 2016).

8 https://facctconference.org/

9 This epistemic trap has led to the unfortunate state of affairs where the black box metaphor in actor-network theory—which was a cry for opening black boxes—has become conflated with lamentations of methodological problems of field access, secrecy, and understanding complexity. The black box metaphor in actor-network theory was never meant to be used to describe a stable state of affairs, but to highlight the human and social tendency to punctualize networks of relations in black boxes. Opening the black box was always a metaphor for uncovering heterogeneous networks of performances, relations, stabilizations of the world. The conflation is understandable, but unfortunate, if we want to actually do social analyses of the unfolding algorithmic society.

10 In the words of Muniesa: "Often qualified as critical, this alternative [analysis of algorithms], is expressed today by an abundant literature that […] is recognized in the rhetoric of the "black box:" that of the computer code, it is that is to say that of the political, economic, cultural encoding that this code serves, an encoding that it is therefore a question of "decrypting" in order to expose the "power of algorithms". This alternative, generally endowed with a more or less emancipatory vision, however, often seeks more to renew the premises cultivated by the computer environment than to free itself from them." (Muniesa, 2019: 205)

11 In the terminology of Lakoff (2010), disease surveillance sits squarely in the "global health security regime" which focuses on preventing disease threats toward the population, in this case the population of the EU. See also Lakoff (Keck and Lakoff, 2013; Lakoff, 2017b).

12 Different parts of the network of disease surveillance are important for different diseases. For instance for the surveillance of the seasonal (or otherwise) flu, disease surveillance in Hong Kong and Indonesia are important centers (Keck, 2014; Lakoff, 2010).

13 For the politics of disease surveillance, see for instance the wide-ranging scholarship centered around studying the politics and expertise of biosecurity (Bingham and Hinchliffe, 2019; Fearnley, 2008; Hinchliffe et al., 2012; Lakoff, 2008, 2014, 2017a; Lakoff and Collier, 2008). On Salmonella politics see Lee (2020).

14 The Zika Algorithm is not a sentinel device in the sense that it serves as an early warning system, but it nevertheless serves to produce a particular charismatic image of the Zika epidemic. It produces the pandemic as an object in society and nature. On sentinel devices, see Keck and Lakoff (2013) on the power of charismatic evidence see Kelly (2018).

15 The ranking of different countries' disease surveillance resonates with MacPhail's (2014: 6) discussion about the assessment of "good" information in US disease surveillance.

16 Compare to the counting of genes and protestors in (Martin and Lynch, 2009).

17 We could drill down in all these risk classifications, but it will suffice to focus on one to make my points.

18 Mosquitos regularly become targets for intervention in disease control and surveillance. Different breeds are adept at spreading different diseases. The Aedes aegypti spreads Chikungunya, Dengue, and Zika. While other breeds spread for instance Yellow fever, West Nile fever, or Malaria. Mosquitos have therefore played a significant role in public health work in tropical climates (Kelly and Lezaun, 2013; cf. Löwy, 2017; Mitchell, 2002; Shapin, 2020). Birds also regularly become the targets of disease surveillance, especially in the wake of the multiple forms of bird flu. See for instance (Keck, 2014, 2020; Porter, 2012, 2013, 2019)

19 Downloaded from: https://en.wikipedia.org/wiki/Aedes_aegypti#/media/File:Aedes_aegypti.jpg.

20 Downloaded from https://e3geoportal.ecdc.europa.eu/SitePages/E3%20Map%20Viewer.aspx#

21 The bespoke database produced by the Oxford team was assembled by searching the scientific literature, in PubMed and Web of Science, for locations where *Aedes aegypti* had been found. The US CDC database of *Aedes aegypti* occurrences was a map of counties in the USA where *Aedes aegypti* existed. This map was originally published in 1965 and augmented in 2008.

22 They used NASA's Terra satellite to obtain spectroradiometer data and elevation. From the Worldclim database they incorporated temperatures and rainfall. They also drew on the HADCM3 climate model, which was produced for the IPCC. Lastly, estimated human population density/km2 was obtained from the Global Rural-Urban Mapping project.

23 US CDC map unavailable at the original location, however it is republished in Wang et al. (2014).

24 See https://en.wikipedia.org/wiki/K%C3%B6ppen_climate_classification#/media/File:K%C3%B6ppen-Geiger_Climate_Classification_Map.png

25 Downloaded from https://earth.esa.int/documents/257246/3374126/Adams-Bridge-Sentinel-2-17112017-full

26 See for instance Cochoy's (2008) analysis of how agency to calculate is configured in shopping practices.

27 Opacity and transparency can be seen as two sides of the same coin. See for instance (Tsoukas, 1997).

28 The literature on algorithms has dealt with the shifting nature of opacity by for instance identifying more typologies for transparency/opacity (Ananny and Crawford, 2018); and by acknowledging that transparency/opacity is not a binary affair, but a gradual continuum between total transparency and total opacity (Diakopoulos, 2020). For instance, Burrel (2016) identifies three types of opacity, while Ananny & Crawford (2018) discuss the challenges of the transparency/opacity ideal. There is also a growing analytical awareness that "The convoluted interrelationships among different technical and human components often complicate and tend to obfuscate accountability for lapses of ethical behavior." (Diakopoulos, 2020). See also de Fine Licht and de Fine Licht (2020)

29 Which Seaver (2017: 5) has observed in terms of the anthropological interest in secrecy. See also (Albu and Flyverbom, 2019) for an organizational argument for analyzing transparency/accountability in practice.

30 For shopping carts, see Cochoy (2008), and for an analysis of the Arizona stock exchange, see Muniesa (2014).

31 Qualculation one might even say (Callon and Law, 2005; Cochoy, 2008).

Blok A, Farías I & Roberts C (eds) (2020) The Routledge Companion to Actor-Network Theory. London: Routledge. 458 pages. ISBN: 9781315111667

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True to its title, this *Companion to Actor-Network Theory* encourages readers to take up ANT as companion in their inquiries; it urges scholars to come along on a common adventure. And indeed, the ANT, or more properly the ANT family, proposed here, is an eminently companionable one, a contrast to some of the rather all-or-nothing ways ANT has been characterised in the past. In the introduction, the editors stress ANT as "an intellectual practice" (p. xxiv) expressed in diverse methods which nevertheless bear family resemblances. A reader gathers that to be accepted as an ANT scholar is merely to avoid betrayal of a certain spirit of analysis by eschewing either too slavish or too wild a form of application. This ANT is proposed as "modes of thinking and speaking near ANT, that is, not simply deploying the existing ANT canon of concepts, research strategies and writing experiments, but keeping them near as a source of questions, problems and inspiration" (p. xxii).

To this end, the purpose of the *Companion* is not to develop a canonical version of contemporary ANT and nor does it aim to provide a collection that is a static snapshot of various members of the family at this point in time. Both those approaches might imply that there is a 'genuine' way to do proper ANT. The ANT inquiries described here go off in many directions, albeit gently harnessed together by the editorial hand. This 'second generation' collection is diverting in the wide range of topics analysed, offering inspiration while maintaining a modest demeanour. It will be welcomed by the large numbers of social scientists who often in their undergraduate studies have been confused by being introduced to a social theory that is not a theory. The essays show how various sorts of situations might be engaged with, rather than explaining either what the steps in using the approach are, or laying the implements out the tool box. The essays on topics drawn from the myriad nooks and crannies of social life by thirty-eight authors, are written in a highly readable, charmingly non-dogmatic English, albeit often slightly idiomatic. The sheer variety of analytic strategies and interpretative forms in the collection reveals the astonishingly wide reach of this way of analytic thinking; ANT can be used to offer telling insight into myriad knotty contemporary socio-cultural issues. The *Companion* feels for, or even elicits, future directions, and steers towards emergent possibilities.

The ANT that is introduced in the low-key prologue is "an intellectual project that is always in beta: it does not construct edifices… [instead] committing to 'ex-titutionalise' ANT… to cultivate…an open-ended experimental becoming" (p. xxiii). The prologue, a quite adequate account of ANT's institutional 'backstory' will no doubt be useful for several generations of scholars-in-the-making, offering eminently quotable quotes for future undergraduate essays. In preparation for entering the text proper the editors end their introductory narrative in mobi-

lising the metaphor of 'mapping'. This is a favourite strategy for editors of handbooks and companions, for example, the map and mapping were much in evidence in introduction of the 1995 *Handbook of Science and Technology Studies*. Although I was surprised by the thought that contemporary ANT is amenable to mapping, so deliciously different were the maps of these two compilations, both in terms of the STS world mapped and the form of the map, that I forgot my hesitation.

The editors deserve a great deal of credit, quite properly their presence is not unobtrusive. They do not lurk in the background pulling strings, their guidance of the project from the beginning being made evident. The generation of the volume's framing, and to some extent individual texts, seem to have been helpfully mediated in initial workshopping. Around two thirds of the contributors are located in European institutions the remaining drawn from all over—an impressive achievement. All contributors are well-placed mid-career academics with disciplinary backgrounds for the most part in anthropology or sociology, yet all are clearly committed to transcending disciplinary boundaries. The fresh younger ANT talent showcased here challenges the old ANT in ways and on topics that likely would never have occurred to the older generation.

The book is divided into six sections, each carefully curated by the editors. Contributions are clustered together into 'families' of between 5 and 7 sibling texts, with vague family likenesses. There are seven short commentaries written in an informative, and consistent editorial voice: an overall introduction, and then six section introductions. Each of the sections is titled by the editors and they suggest that when laid end-to-end as books inevitably do, the sequence of sections is "an attempt to chart out the collective journey of ANT so far, and to elicit pointers for new routes ahead… a tour around ANT… concerned with different aspects or facets of ANT's intellectual practices which one would conceivably want to visit, sooner or later, in the process of drawing near to ANT" (p. xxxi).

The essays in section 1 focus on "modes of weaving ANT's conceptual-empirical inquiries… reflecting on problematising, inquiring, comparing, writing, and criticising with or near ANT" (p. xxi). In section 2 attention is paid to sources of inspiration for ANT, especially silenced, forgotten, or yet to be noticed potential sites of enrichment of ANT studies, in shifting from the past, or unnoticed, in section 3 essays offer soft critique or problematisation of former and current strands of ANT inquiry, and worry at roads not much travelled. Section 4 has authors venturing outside STS to engage with a wide range of knotty puzzles in contemporary collective life, which as things turn out, offer varied conceptual, methodological and ethical challenges for ANT. Section 5 offers new perspectives on scale, an old favourite of the ANT repertoire. The wrapping-up of section 6 showcases some uses of ANT in doing public and professional life: activists who design, designers who consult, and those who run institutions.

So diverse are the topics and voices of the essays, that I found delight and diversion in reading the essays in an orderly way, working my way through a section. But the titles also invite browsing, skipping from one section to another. Many of the individual essays might have found a home in several of these sections. The sections too are discernibly distinct, each has a particular mien or demeanour. The essays in section 1 for example, while diverse seem to be united by a shared ambivalence about ANT ancestors in general, while those in section 2 find common cause in asking what more, or what else, might be drawn from ANT's scholarly roots. Essays in sections 3 and 4 both seem to ask 'But what about…?' with those in 3 focussing on human traits, and 4 on contexts. Section 5, with its focus on scale, asserts solemnly 'This is serious!' while the final group of essays sparkle with exuberance.

I conclude by listing a few more or less random responses to the book.

- The parts are impressive, both the individual texts and their clusters, and yet the whole of this companionable assemblage is still greater than its parts.

- I came away from the book imagining ANT in a quite different way. As a warren of rabbit-holes that readers might choose to fall into or not, but every one of which would unfailingly provide adventure, and where each passage plotted might connect with others, or cheerfully fail to link up.
- I noted that ANT has yet to learn to laugh at itself, perhaps indicating a lingering reluctance to age. Maybe this volume is the beginning of the recognition that nurturing the coming into being of viable successor projects is crucial.
- I wondered what happened to the story of ANT as sociology of semiotically sophisticated translation. To me that still seems to be a useful descriptor of ANT allowing for it as a vague and divergent whole. A friend suggested that translation is actually exactly what this book does.
- I do look forward to ANT learning to take more seriously the work of not taking itself so seriously, it is after-all just another gift of the European Enlightenment albeit that it comes from the Leibnizian end of that remarkably generative time and place.

Pittinsky Todd L (2019) Science, Technology, and Society: New Perspectives and Directions. Cambridge: Cambridge University Press. 270 pages. ISBN 978-1-316-61689-5

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This book aims at bringing together essays on the interplay between science, technology and society (STS). The essays within the book span a diversity of topics, including educational programs, scientific communities, technological decision-making, legal regulation, the role of users and non-users in technological development, and genetic engineering and society. The book draws from a wide range of theoretical perspectives, including psychology, sociology, organisational studies and economics. Overall, it shows a stronger emphasis on technology than on science, although some chapters focus on science or on both, and the two cannot always be disentangled.

Several essays in this book situate current and new directions in STS in relation to the origins of the discipline, providing detailed historical accounts of how prominent themes have developed over time and how they might continue to evolve. Readers who would like to familiarise themselves with some of the more established topics within STS, for instance, may wish to start by looking at these.

Chapter 6 on the social shaping of technology (SST) provides an excellent example of this format. From the 1980s onwards SST has sought to answer questions about how technologies are shaped by socioeconomic, political and material factors. In this chapter, Williams traces the origins of the field and the early studies which constituted it; compares SST with the neighbouring field of actor-network-theory (ANT), in which both humans and objects are conceptualised as actors; surveys the move from SST Mark I to SST Mark II, in which theories of the relationships between technology and society became more complex; and reviews more recent conceptual and methodological directions, which provide evolutionary understandings and combine attention to local context and broader processes of sociotechnical change.

Collins and Evans offer a similarly organised account of the evolution of theories on the role of expertise in technological decision-making in the public domain, another enduring theme within STS. In chapter 4, the authors argue that we have reached a "Third Wave of science studies", which emphasises the need to combine specialist expertise and democracy to make the "best" decisions. Prior theories tended to either promote an excessive reliance on science (first wave) or a suspicion of expertise leading to over-reliance on lay knowledges (second wave). Types of expertise are then categorised on the basis of tacit knowledge and socialisation.

In chapter 3, Godin provides a rich history of how different disciplines have theorised innovation over time, moving from linear models towards system models. In a context of economic, social, organisational and technological change, questions arise about how this change can be accelerated or directed towards desired goals. Historically, different theories on technological

innovation have shared a representation of innovation as a process, and yet Godin argues that more recent theories on innovation as a system, while they still typically address innovation as a process, they tend to emphasise structure over time.

Chapter 5 addresses the multiple relationships between emerging technologies and regulation. According to Brownsword, the most important feature of this regulatory environment is that regulators should maintain the preconditions for human social existence itself. This involves upholding conditions for *human existence*, such as living within planetary boundaries; *human agency*, allowing humans to act in ways which are interactive and purposeful rather than merely defensive; and *moral agency*, to ensure that people have the freedom to choose to do the right thing and not to be compelled to do it via technologies.

Some chapters provide cross-cutting analyses by surveying the field through a particular lens, such as chapter 2, which provides a tour of prominent topics within STS through the lens of comparison. In this chapter, Horn argues that the so called "comparative method" actually covers a wide variety of different projects, but many of these projects have in one way or another been about "what science and technology have in common with other forms of human knowledge and human practice" (p. 52) and what is different, and that these are, at their heart, questions about culture. Comparative studies of science and technology can foreground discontinuities rather than continuity; different ways of knowing beyond the scientific method and rationality; and the roles played by cultural differences.

Simonton's chapter 8 presents a similarly extensive tour of STS theories through the lens of scientific communities. Within scientific communities, scientists typically reach a strong consensus on the questions, theories, methods and techniques that are considered relevant to the community, but research has shown that this consensus is socially constructed. Scientific communities are highly stratified with a small number of elite scientists, and early successes and failures in science cause large differences in the scientific productivity and status of individual scientists. More problematically, there is evidence of direct biases such as gender bias influencing scientific career trajectories. This chapter shows that scientists are embedded in rich social networks, and how this ultimately influences the nature of science.

In chapter 7, Oudshoorn surveys the role of users and non-users in relation to technological development. She shows that users play an active role, from design to implementation to use, and thus act as agents of sociotechnical change in a variety of ways. Additionally, technologies inside the body such as pacemakers have highlighted new forms of vulnerability in relationships between users and technologies because they are irreversible and the risks involved are unavoidable. Historically, non-users have been conceptualised in negative terms, such as laggards. However, non-users can also be rejectors or resistors, expelled or excluded, thus expanding categories of use and non-use. Furthermore, recent research has focused on "the blurry space" between the two.

In chapter 9, Barnes et al focus on the relationship between genetic engineering and society, conceptualising them as coproduced. Genetic engineering can also be described as a technoscience, blurring the boundaries between knowledge production and the production of products. Furthermore, a new technoscience typically emerges in an 'institutional void', and in this context science and technology can "overflow" the boundaries of institutions and laws. Public engagement initiatives have emerged, which open up spaces for the public to participate in the coproduction and democratisation of genetic engineering and governance.

In chapter 10, Mazei connects two streams of research, on sex differences, and on how technology shapes society and individual behaviours, to explore how technology can either enable or reduce sex differences, depending on the context. The chapter also proposes concrete actions to mitigate these effects, such as investing in technologies which facilitate non-traditional divisions of labour between the sexes; portraying women and men in a more balanced way in the media; and standardising workplace procedures such as hiring and promoting employees so that sex is ruled out as an influence.

In the first essay of the book, Granger Morgan and Sicker write about the importance of truly combining technology and public policy education; the difficulty of maintaining a balance between the two; courses which do combine them; the relationship between this kind of education and policy analysis; and the impacts that an education which truly combines technology and public policy can achieve.

Pittinsky's final chapter provides a discussion of what 'technology for society' would look like, acknowledging that society is diverse and multi-layered and so reaching consensus is not easy, but nevertheless arguing that there is a need to better understand how technologies could serve the greater good by promoting one or more of the following: good relationships; economic and material well-being; a health natural and built environment; health; peace and security; culture and leisure; spirituality, religion and ethics; good education; and good governance.

The essays in this book are so diverse and some of them are so "meaty", situating fascinating areas of research in this diverse field within wide-ranging historical and theoretical landscapes, and viewing parts of the field through different lenses to great effect, that it would have benefited from a longer introduction, giving a clearer indication of what the essays within are about, and why these essays in particular were chosen to represent new perspectives and directions in STS. Although it gives a strong take home message, the conclusion could have done more to bring together the essays preceding it. As a result, the reader could be left with an overall impression of STS as a collection of fascinating, but largely siloed topics. However, the book does provide diverse and engaging examples of new perspectives and directions in the field, which is, after all, what it set out to do.

This book is of great interest to students or professional researchers who want to gain a snapshot of some of the many and diverse current themes in STS, the rich history of the ideas behind them, and visions of where they might go next. It may also attract readers with an interest in the complex relationships between science, technology and society, and the myriad of ways in which these relationships are currently studied and understood.

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