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Travelling Representations

Introduction

Data Journeys [Leonelli and Tempini 2020] is an edited collection of papers presented at various workshops and conferences organised under the aegis of a European Research Council project. It aims to make two contributions. One is to sociological method and the ambition here is to demonstrate the analytic utility of Mary Morgan's riff on Bruno Latour's notion of mutable mobiles. This riff is encapsulated in the trinity of data, their vehicles, and the journeys they go on referenced in the volume's title. The second contribution is to the social philosophy of research and focuses on the implications for philosophical debates of sociological findings about peregrinating data. What is noticeable about the first contribution is (a) just how patchy the use of the trinity actually is in the presentations and (b) when it is deployed in earnest, just how little novel analytic utility it provides. The surprising thing about the second contribution is that discussion of it is almost entirely confined to the introductory and closing summaries. In the eye of the editorial beholder, philosophical significance, it seems, is pretty much a bookend feature. Moreover, the editorial philosophical exercises are more than a little over-excited, not least because they make claims which are not substantiated by the studies they summarise.

We will take each of the contributions claimed for the volume separately and (we hope in an even-handed way) set out the reasons for our downbeat judgements. We start with the editorial philosophical rune reading and then turn to the sociological content. For the latter, we concentrate mainly on Mary Morgan's contribution and a small number of the others.

Section 1. The Social Philosophy of Data

The social philosophy being promoted by *Data Journeys* is not the application of conventional philosophical arguments to puzzling or contentious social phenomena but the application of sociological arguments to philosophical puzzles.[1] Its general form is a proposition which we could summarise as follows.

> 'The studies reveal how different vehicular designs for transporting data through the research pipeline generate different forms of mutation within their cargoes of data structures. This is because the research environment is a field of "social forces". The normative character of the practices institutionalised in each environment both predisposes these mutations and prevents their extent and character from being disclosed.'

Since Sociology generally assumes normative structures have interest-driven homeostatic functions, it seems fairly straightforward to conclude the scientists think/say/believe they are doing objective science but the studies show they are doing "science-as-politics". With this revelation, we are being invited to see the scientists believing/thinking/assuming the 'data' they use are depictions/representations/translations or scientific distillations of properties of natural objects whereas the studies reveal them to be forms of social construction (that is, social constructions sociologically conceived).

Use of this revelation carries an irony of explanation. Its purpose is to emphasise how bodies of 'data' exemplified in the studies morph so much during their analytical research journey they should no longer be described in terms those who generated them would accept. What no-one seems to have noticed is how closing the reflexive arc on this revelatory argument is achieved. On their own terms, the process of data journeying must apply to sociological accounts too (unless anyone wants to claim they have somehow stepped out of the social worlds of science/research/social life and into some Archimedean realm of higher existence). Across the research transitions from the original experiences with the 'cases' to their final description, the institutionalised vehicular designs of the dominant and subordinate explanatory *sociological* metaphors exert their own transformations. What is first theorised as the "naive fantasy" of "brute, unanalysed givens" (p.391) gradually pupates into a *sociological fantasy* of multiple data

[1] For the rest of this commentary, this is what 'social philosophy' designates.

production and manipulation processes which threaten the preservation of "the integrity and meaning of the data" (p.397).[2]

In pointing out this irony, we are not arguing for or against 'relativism', 'interpretivism', 'constructivism' or any other stance in social philosophy. Instead, we *are* arguing for a greater sensitivity to the parameters of the sociological attitude and hence its character as an analytic *mise en scene*. Having made this point, we also want to argue for the possibility of undertaking studies which do not turn them into exercises in ironicising their subjects and subject matter.

Design Specs

The place to start is at the end, with Helen Longino's summary chapter and, indeed, with its initial paragraph.[3]

> The naive fantasy that data have an immediate relation to the phenomena of the world, that they are "objective" in some strong, ontological, sense of that term, that they are the facts of the world directly speaking to us, should be finally laid to rest by the papers collected in this volume. One might think that "data journeys" catalog the way that raw givens are transformed as they move from their original context to other contexts, whether higher levels of abstraction in the same field or other fields of inquiry. These papers, investigating data journeys in fields from particle physics to urban planning, show that even the primary, original, state of data is not free from researchers' value- and theory-laden selection and organization. [p. 391]

Here, in a nutshell, is the blueprint for the luge onto which the studies in *Data Journeys* are strapped in order for them to make their headlong epistemological Cresta Runs. It comprises two guiding postulates lashed to the flimsiest of sociological frameworks. The postulates are first, no data apperception ("raw given") is pristine. Researchers' experience of the objects of their research is contaminated by theories and values which undercut the objectivity of that experience. Call that the notion of the "tainted percept". Second, as research proceeds through the particular

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[2] Henceforth, all unattributed quotations are to [Leonelli and Tempini 2020]

[3] To be fair to Helen Longino, her position has shifted considerably since she wrote the piece for *Data Journeys*. She now insists the key term in the analysis of social epistemology should be "interaction" and that due weight must be given to interactive practices in, for example, the resolution of disagreement. Puzzlingly, she calls for more attention to be paid to such practices in actual scientific settings without seemingly being aware of the many studies in SSST which do precisely that. However, she still seems to hold to the view the findings of these studies have implications for the conclusions of epistemological argument. See [Longino 2022]. Despite this later, more nuanced argument, we have retained references to Longino's contribution if only because it is as clear a statement as one might wish to find of the chimera we are hunting down.

steps of the relevant discipline's processes (for example, experimental design, data collection, data aggregation and organisation, data analysis, generalisation derivation and finally results presentation), the initial tainted percept is transmogrified by local tailoring of the standard practices deployed at each stage and so becomes the 'constructed reality' of the final outcome. The framework to which these ideas are bound is expressed in the assertion that, wittingly or otherwise, the researchers engaged in the science being studied have been socialised into epistemic cultures whose membership entails conformity to norms and values underpinning the power of extant vested interests. The hope is that this construal of the studies will provide a revelatory presentational descent without the study content, the framework and *in extremis* the guiding ideas parting company disastrously.

The first thing to say about this design is that the key friction reducing property of the vehicle (we'll stop this image now!) is an applied philosophy of perception.

1. A philosophy of perception does not describe how you see the cat is on the mat or hear the phone ringing downstairs. It does not try to provide a (causal or any other kind of) description of 'physical', 'mental' or whatever cognitive goings on are occurring when you and we do these things. Rather, on the basis of certain stipulations, it describes what assumptions (implications or entailments) we will have to accept if we want *to defend the argument* we can make 'perceptual judgements' such as 'The phone is ringing'. What, given the way we want to argue, must be the forms of 'physical' and/or 'mental' conditions which make our seeing and hearing things possible?

2. Most contemporary philosophy is framed by and struggles with Kant's answer to this question: an answer usually rendered in the slogan "no percept without concept". Sociology, being the progeny of the post-Kantian Enlightenment, takes Kant's precept for granted. Its application (hence our calling it an *applied* philosophy of perception) involves adding social conditions to the list of entailments (more usually as replacements for the physical and 'mental' ones). The list of social conditions Sociology identifies as drivers of perception is extensive. In Longino's summary, they are lumped as theories and values.

3. Any working sociology, then, starts from the presumption perception requires a conceptual apparatus. This is because of an anterior axiom it also insists on. The social actor is an interpretive actor. No matter how it works through this premise, every

sociology starts from these two. Social action is based intersubjective interpretations rooted in motivated compliance with the common norms and values of a shared culture. Naturally, the sociologies differ is in what they take "rooted" to mean.

4. So, what Longino presents as a conclusion derived (arrived at by induction/deduction/abduction) from the studies is actually the QED of a demonstration how to make just one kind of sociological argument from the given postulates. In other words, in her telling, each of the studies is an instance of pretty much the same story about scientific research; a story which starts with the pre-supposed philosophy of perception.

The second thing to say concerns the origin of the notion of an 'untainted percept' itself and its status as a myth. In particular, who exactly is supposed to believe this myth? Longino is happy (as is Leonelli in her introduction) to promulgate the view that the idea of an untainted percept is a myth and to imply some groups in society (scientists and non-critical sociologists presumably) are convinced by it. However, they offer no evidence either direct or indirect for this claim. There are no direct quotations from scientists clearly showing they hold to the view nor are illustrations taken either from the studies presented or elsewhere to substantiate it.

Why is this? Well, it could be argumentative laziness. They just haven't bothered to collect such data. On the other hand, it could be their version of the dog that didn't bark. Although myth-eaten talk abounds in the laboratories and elsewhere, unfortunately they never happened on any. There again, and alas we think is the most likely, it could be the evidence is not supplied because they didn't think they had to. Everybody (well, everybody who does the social philosophy they do) knows that 'hard-core' natural scientists and philosophers of science believe the myth. But do they? Does it actually function as a *weltanschaung* sustaining belief for them? Or is the presumption that it does a conviction which provides motivation for the whole genre of this kind of analysis, itself a myth (a kind socio-philosophical legend, if you want)? And if so, what are its origins?

Go back to the philosophy of perception. Like all philosophies, philosophies of perception are about constructing arguments. One standard (rhetorical) strategy by which philosophical arguments get their traction is through the construction of a for-the-sake-of-the-argument contrast. The tainted/untainted percept contrast rests upon a much broader for-the-sake-of the-argument contrast between 'appearance' and 'reality' which we owe to Parmenides. Parmenides was worried about the nature of existing (or *Being*, as we like to say these days). In particular, he was exercised by the possibility there might be two modes of existing; the continuous existing of the eternal realm and the temporary existing of the material, empirical world. His worry was how to relate or unify them. He proposed to resolve the worry (we hurry along a little) by arguing 'real' existence (*Being*) is eternal and empirical, material existence only has the 'appearance' of reality. Since we are (mere material and empirical) mortals, we cannot grasp pure reality but only its tainted or degraded appearance.

The point is not whether Parmenides believed in the difference and, if so, how he organised his philosophical and non-philosophical life on its basis. It is, rather, the career the idea has taken in Philosophy, and in the philosophy of perception in particular. In essence, this has been to license a convenient philosophical fiction from which to construct chains of argument. It operates in much the same way as the ideas of $\sqrt{-1}$ or the infinite denumerability of the set of real numbers $\mathbb{R}^N$ act as convenient fictions in Mathematics thereby providing the grounds on which to motivate particular chains of mathematical reasoning. Because mathematicians doing their mathematics talk about $\sqrt{-1}$, transfinite cardinals, converging parallel lines and even quaternions, does not mean that when they are not talking mathematics, they believe there are such things lurking in the world somewhere; objects which will somehow prevent them from tiling the bathroom wall or dividing up the strawberries among the grandchildren. Equally, that philosophers talk about the conceptual separation of percept from concept does not mean when selecting fruit in the greengrocers or counting the spoons in the cutlery drawer, they are transfixed by the possibility they may not see what they see. (David Hume's remedy for that temptation was to play billiards).

The same holds for scientists. They don't see themselves doing philosophy even if social philosophers like Leonelli, and Longino will insist the scientist must be a (social) philosopher *malgré lui* simply because their own analysis is premised in a (social) philosophy of perception. To someone with a rubber hammer, everything is a knee. When scientists talk about their science, we suspect it is mostly in terms of the practicalities of organising the research pipeline, planning and managing for the possibility of instrumentational artefacts and working within the parameters of the conceptual space in which their research is constructed. The appearance/reality, tainted/untainted distinctions are philosophical points made by philosophers and by sociologists during their philosophical excursions. There is no good reason to foist them onto scientists. Yet, without such foisting, the myth driving this kind of sociology of science cannot be maintained. Drop the myth and all the talk of no longer…

> .....under- estimating the politics and power of data, which so many contributors to the emerging field of critical data studies have so

> effectively highlighted, we seek to document how.....politics is embedded, reified and/or revised in the technical and epistemic work that structures everyday research practices [p.6]

drops out too. As a result, the attributional wrapper around the accounts offered for the studies in *Data Journeys* evaporates and the detail offered in each study can be examined for what it says about the actualities of scientific life.

ROAD WORTHINESS

So much for the over-excited conclusions. Now let's turn to the character of the theories and values held to be governing the selection and organisation of data. Earlier we said that for Sociology, it is a given the social actor (its homunculus) is interpretive. What Longino calls the lading of theories and values are just the resources, to use as broad a term as possible, on which these interpretations rely. They are the relevant ethical, practical, economic, organisational, disciplinary and whatever else (yes, sometimes political too) considerations actors use, deliberately or otherwise, to guide their judgements when deciding what they should do in any set of circumstances. None of these considerations are contextually omni-relevant and the ordering of their influence is not fixed. This is as true for any account we might want to give of the normative structures involved in choosing a birthday card for one's wife or selecting to which journal a research paper should be submitted. That is precisely what is meant by calling such actions 'interpretive'. It is only a particular sociological 'theory' which determines the 'political' must be the master consideration and choices of card or journal must always be ultimately determined by it.[4] The sociological finding 'it all comes down to politics' has a second explanatory irony, the substitution of a master sociological conceptual apparatus for the contextual particulars of the conceptual apparatuses which social actors (the scientists in this case) use.

Finally, let's circle back to import of all this. At the end of her Introduction, Leonelli tells us the studies detail how the various issues they take up....

> ....are deeply political and have significant implications for ongoing debates around, for example, the trustworthiness of Big Data as source of evidence and the potential for inequality and exploitation underpinning open data policies.[P.11]

We don't doubt she is convinced of this. Equally, we don't doubt the scientists whom she and her colleagues have been studying would hardly say so. They would point to the arrays of disciplinary,

[4] As a consequence, we should note causal terms like 'shaped' and 'informed' are weasel versions of 'determined'.

practical and other virtues promoted by the practices they use. Finding themselves in this kind of stand-off puts the social philosopher in a bind. Either they have to contend research subjects are irredeemably credulous (if not cretinous) in hanging on to what is a demonstrably false description of science or they have to allege scientists are somehow delinquent in not advertising their agreement with the socio-philosophical claims. Since allegations of delinquency are potentially libelous, it is not surprising the myth of the myth of an untainted percept prevails.

In the end, those making such arguments find themselves in this position because of their commitment to their own myth and its ironic consequences. This is the myth that the concept of the social construction of reality requires adherence to the proposition that our conceptions of what is real, factual, true, proven and no longer to be argued about *can only be seen* as ultimately driven by social forces and every other account is an instance of false consciousness. Social philosophers assert scientists make false judgements about the objects of their research simply because of the theories and values they hold as scientists and members of society. But such assertions rely on the theories and values which these social philosophers hold as social philosophers and members of society. The end result is analytic description vying with and displacing scientific description of scientific research. How ironic is that?

In the way they have set up the contributions they have gathered, the editorial overviews and summaries given for the studies in *Data Journeys* seem to have set an elephant trap for their contributors.

Section 2. Data and Its Travels

Do the contributors avoid the trap? Funnily enough, they do. This is because none of them, not even the Editors in contributory mode, seem to think the major lesson to be learnt from their own cases has anything to do with the myth of the objectivity scientific data and findings. What the contributors to *Data Journeys* do talk a lot about are various forms of data generated in a range of natural, social and policy sciences and the ways they are processed and managed. In this, they are picking up the pointer provided by Theordore Porter in the Introduction to *Trust in Numbers* [Porter 1995] where he describes his approach to the study of quantificational methods in science and the professions as follows.

> My approach here is to regard numbers, graphs, and formulas first of all as strategies of communication. They are intimately bound up with forms of community, and hence with the social identity of the researchers. To argue this way does not imply that they have no validity in relation to the objects they describe, or that science could do just as

> well without them. The first assertion is plainly wrong, while the latter is absurd, or meaningless...........
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> ......(Q)uantification is a technology of distance....Since the rules for collecting and manipulating numbers are widely shared, they can easily be transported across oceans and continents and used to coordinate activities or settle disputes. Perhaps most crucially, reliance on numbers and quantitative manipulation minimises the need for intimate knowledge and personal trust. Quantification is well suited for communication which goes beyond the boundaries of locality and community. A highly disciplined discourse helps to produce knowledge independent of the particular people who make it. [Porter 1995, pp. viii-ix]

Through histories of census compiling, accountancy practice, cost-benefit analysis and others, Porter draws attention to the range of theoretical, organisation practical and, yes, ideological circumstances which need to be managed before a standardised working set of calculative procedures can be rolled out and stabilised. As a historian, he highlights what he takes to be the historical significance of this: success is awfully contingent and hence awfully underdetermined. This underdetermination is the phenomenon which the studies in *Data Journeys* seek to examine. The keystone is Mary Morgan's analysis of National Income Accounting and so we will begin with that.

PURITY AND DATA

Mary Morgan 'sociologises' Porter's strategy by recasting the procedures he describes as socially institutionalised "measurement instruments". In her contribution, she summarises what this means as follows.

> I use the term kind of data to point to the facts that there are different kinds of 'measuring instruments' involved in producing numerical data, a term of usage in this context due to Marcel Boumans. The measuring instruments used in social sciences look rather different from the thermometers, Geiger counters, and so forth, that might be first thought of when considering scientific measuring instruments. In the social field, they are mostly various kinds of counting systems that rely on observation posts spread out across the country in government offices, banks, companies and families who all report aspects of their lives (usually for completely other purposes). The raw data collected from these observation points are numerical, and combined in different ways, according to the frameworks or principles and techniques of the measuring instruments (consisting, as Boumans argues, of models, formulae, rules, conventions, etc) used to turn such raw numbers into measurements of the economy and society. [*p.107*]

These measurement instruments are the vehicles she is interested in. They carry scientific data on their journeys. In the social sciences (her source is Economics, but the point is a general one), the measurement instruments are statistics, accounts, indicators and index numbers. To show how they work, Morgan takes the example of compiling National Income Accounts and how the methods used represent them in different ways.

> Economists have developed two kinds of data to capture social-economic well-being. They are based on two different frameworks of measurement The national income accounts are designed to measure the complete set of income, expenditure and products at the level of the nation. They do so by building up from the subcategories of all these three activities which are understood to be - in the bottom line equivalent (in economic and monetary terms). In contrast, the indicator series may look just as ordered because they are arrayed in connection with bigger targets, but they are in fact held together by no such constraints. [*p. 117-118]*

Morgan's recasting frames the national income data measuring instruments as solutions to the classic sociological problem of social (measurement) order. How does motivated normative compliance on measurement constructs get produced? In her account, the resolution takes place by socially organised synecdoche.

> Any individual datum (or bit) has relations not just with the other data points in their series, but also with those of the group (or whole) data set. For example, the data on population growth of a society consist of individuals, who can be counted in a simple aggregate whole, but for social science purposes will more likely be found in data series divided by occupational classes, or age cohorts, or regional spaces. The bit-whole relations will depend upon the kind of group data involved, for there is variety in bit-whole relations just as in those naturalists' examples suggested above. No doubt these varied kind of datum-to-'group data set' relations can be found in other fields of science with complex wholes such as ecology, physiology, and so forth; it is not necessarily a special feature of social science data. What is important is that different kinds of data sets in the sciences have different bit-whole properties, and that these turn out to be very important for the possibilities and fruitfulness of individual datum journeys. [p. 118]

Note what is built into the way the issue is set up. The observation/datum/bit is imbued with sociality. That does not mean simply that we see it as the product of a social processes, though that is part of it. The observation/datum/bit is given a social life of its own. It sits in a network of social relations interacting with others like itself. These interactions enable the aggregation and indication which Morgan is pointing to.

It turns out the characters of the two sets of relationships are different. Accounting aggregation relationships are rigid, hierarchical and composed of tightly constrained short-range connections. Counts are contained within other counts. Equivalent counts should balance. Indicator relationships are far more happy-go-lucky, flexible and hierarchically louche. They facilitate the possibility of unexpected long-range resonances of loosely associated tendencies. Seen from the outside, what these two instruments give us are 'deep' versus 'broad' representations of the global social structure of national income accounts.

Does anything about this formulation look familiar? (Spoiler: what's the title of this Section?) The only thing missing is a diagram placing of each of the social forms of measurement on Mary Douglas' Grid/Group dimensions parameterised for the representational values built into the measurement instruments.[5] Accounting instruments are strong on Group (the clarity of boundaries and levels) and strong on Grid (the maintenance of consistent internal structures) whilst indicator instruments are weak on both.[6] As with Douglas' analyses of the social troubles flowing from the mismatch of classificatory schemes located at different places in the Grid/Group space, Morgan points to the issues which arise for NGOs, public bodies and Governmental Agencies when they try to apply the measurement instruments in circumstances which their social organisation does not fit. The sociological point here, and this is brought out time and again in Douglas though only hinted at by Morgan, is not only that the social arrangements express a socially organised configuration or collective representation but, by embodying that configuration, they reproduce its normative character. For those who 'do' National Income Accounting using either instrument, the 'right' way is their way.

Morgan is doing classic sociology. Moreover, she is doing it in the classic sociological way. She bases her analysis on the reports, papers, explanations and justifications offered by those who carry out the processes she describes. These, of course, are contextual reconstructions

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[5] Paradoxically, this central motif framing the sociology of *Data Journeys* is deeply Durkheimian. For all the post-modern editorial marketing, it is actually very conventional. We were reassured in drawing this conclusion by finding Marcel Boumans and one of the Editors, Sabina Leonelli, extensively quoting Mary Douglas from her pre-Grid/Group incarnation in *Purity and Danger* [Douglas 1966] during a comment about data cleaning really being data re-ordering (p.93). What is slightly disconcerting (and worrying given Leonelli is a sociologist) is that they appear to think the point of the "matter out of place" definition of dirt is to point up the disadvantages of fixed pre-conceptions and not a general sociological point about the social structuring of the conceptual frameworks across which systems of classifications are distributed.

[6] We imagine it might be an amusing post-conference pub quiz question to ask where on such a diagram we should place the other categories, such as statistics and index numbers.

of the working practices involved in using indicators or accounting aggregation. This means there is no detail on how these systems of calculation are made to work. One, we are told, is a rigid bottom up, line by line balancing exercise (which is almost all we are told about it). The other is a mash-up of correlates for possible trends and outcomes of interest. We are told they take different modalities for internal relationship construction with the mashed-up entities being far more disengaged from that which they are taken to represent.

None of us should be surprised to hear any of this. This is what we have known for a long time about official statistics and what those of us whose research requires the collection of numerical data know is the ever-present challenge; namely fitting the numbers to the analytic processes we want to run them through. But what Morgan doesn't give us is any detail of how the aggregating and the melding are achieved. To coin her own metaphor, what is the topography like along the different routes taken by the data and how are shared maps (descriptions) of the terrain produced?

Here is an obvious example. Just what are the protocols for valuing the diversity of things-in-the-world which are contained in the NIA lists of activities and products (goods and services) making up a summary number for GDP which ensure those component values are rendered in consistent financial terms? How are these protocols audited to show they do operate consistently and 'reasonably' (along with how the wriggle room such reasonableness provides is then managed)? To take another. How are ratios such as values per head of population for different diseases, road traffic accidents and general health conditions such as diabetes or angina weighted and blended to give an acceptably sturdy 'estimate' of the "wellbeing" of a country? It gets done and how it gets done is critical to the journey the data takes and the 'companions' it travels with. Morgan tells us it is so. But, alas, we are not told what that work actually consists in.

Mary Morgan gives us no insight into detailed social organisation of either of her measurement instruments as they are constructed as part of routinely doing the accounts. No doubt there are problems, mismatches and missing elements. And no doubt, compromises and practicable ad hoc decisions have to be made. How are they taken and how they achieve normative compliance is surely the central sociological question of interest? How do the teams and organisations responsible for producing the numbers actually do social ordering as a coordinated social activity? That what they produce is or is not a constrained compromise is not sociologically interesting. That they have managed to solve the social organisational problems of ordering National Income schedules in the efficient and effective way they have (after all they can do it again and repeatedly do) is the remarkable sociologically pristine phenomenon.

The Editors insist the degree of correspondence between the representation and that which it is said to represent is the issue to be analysed rather than what any cohort of NIA economists has to do to get their activities to mesh so they can claim the representations they build are good enough to work with. Morgan's analysis points to this work and trades on it by insisting the measurement instruments of National Accounting require doing small picture stuff to be able to do big picture stuff. That relational space is where the action is. It is a pity Morgan's investigations don't tell us what routine practical National Income Accounting actually looks like. Though, to be fair, she does tell us it is a (re-)constructive practice.

SPEEDED UP DATING

Alison Wylie tells a fascinating story about the evolution of radiocarbon dating in Archaeology. Both the materials Wylie presents and the account she gives of them are straight forward. Politics (in the large or the small) makes no appearance. Rigour and practicability are everywhere.

Here is the elevator summary. In the 1950s, the attempt to use the tick-tock decay of the carbon isotope ^{14}C to its stable forms (^{12}C and ^{13}C) as a way of measuring the age of archaeological artefacts turned out to be more complicated than was at first thought.[7] Not surprisingly, after the initial introduction of radio carbon dating, it took a little while for the necessary field collection and laboratory processing practises to stabilise and standardise. Although variation across labs carrying out the process was much reduced during this process, other anomalies remained.

> As these challenges were met, a growing number of anomalies were identified in the ^{14}C dates reported for archaeological material that could not be attributed to contamination or processing error. These drew attention to the complexity of the physical processes that radiocarbon dating exploits; much more background knowledge is required to estimate time elapsed since sample death than the "immutable" decay rate of radioactive carbon. [p.290]

Two possible factors influencing results were identified: the huge increase in stable carbon in the atmosphere since the mid-19th century was changing the ratio of stable to unstable isotopes. At the same time, post-WWII thermonuclear tests has significantly increased ambient radiocarbon. What

[7] Wylie calls the original introduction of the method a first 'revolution' in Archaeology and solving the complications it led to a second and then third one—though, to be honest, calling any of these shifts revolutionary is a bit of a stretch since for the most part what it meant to do archaeological field work remained pretty much as it was.

both implied was that the assumption of a stable environment and a fixed 'count rate' of decay might not be sustainable.

Data collection and management processes now had to accommodate the possibility of variation in the carbon cycle over time (not just in the historically immediate short term but across the whole chunks of the historical record). This was further complicated by the realisation different organisms took up carbon in different quantities and at different rates. For carbon dating to be usable, far more background knowledge of the physical, chemical, biological, geomorphological, cultural and field site contexts had to be taken into account. Appropriately used, this background detail might "warrant" acceptable dating. The originally hoped-for equivalent of the standardised metre bar for object dating had simply proved to be infeasible.

The third shift has been the adoption of so-called "Bayesian" methods. These involve a process of triangulating dating information against a whole range of other relevant information now available. As more concordant information is added to the mix, so the certainty of the target date range increases and the probabilities harden. Hence the nomenclature. Developing the protocols, measurement systems and calibration procedures as well as agreeing standards for the outputs of this new approach is at the heart of current work.

Having presented this history, the conclusion Wylie draws from it is as down to earth as the discipline she describes.

> ......the data that anchor evidential arguments are themselves the terminus of further practical arguments that depend upon their own warrants; as such, their points of origin, and each of the steps involved in capturing and transforming them into useable data are also subject to critical scrutiny, and open to demands for further backing. In the case of archaeology, building these tangles of practical argument is an achievement that depends on a genre of robustness reasoning; it is a matter of enlisting not only the data generated by physical dating techniques but also a wide range of less transportable, context-specific data. The epistemic integrity and credibility of the resulting temporal data is a function of the traceability of these transformations... [p. 299]

While the detail she works through is new and fascinating (at least to us), the general story will resonate with anyone who has any experience of innovation in practical, real world production processes, be they in science or elsewhere. Breakthroughs happen. They cause disruption and, hopefully, improvement. They take time (often considerable time) to bed in, be accepted and acceptable. Along the way they are mangled (to use Andrew Pickering's term), every turn of which has its own effects to be scrutinised and managed. Wylie's discussion is as relentlessly general as

Morgan's and taken up with identifying the sociological significance of the various revolutions in method rather than their actual social substance. What we are missing from both Wylie and Morgan's presentations is the kind of occupational ethnography we find in Latour and Woolgar's *Laboratory Life* [Latour and Woolgar 1979] or Mike Lynch's *Discipline and the Material Form of Images* [Lynch 1985]. In both the detail of what the benchwork consists in and how it is managed is placed at the forefront. Though to be fair, since she is a philosopher, it might be a bit much to expect Wylie to know how to do field ethnography or even to want to do it.

OBJECTIVITY: PROPERTY OR PROCESS?

Suppose a group of us are in a general conversation about climate change and someone comments that although we seem to get fewer really snowy days than we used to, the Cat and Fiddle seems to be closed more often.[8] One of us cuts in and says "Did you know on a clear day you can see seven counties from the front door of the Cat and Fiddle?" Next time you are driving from Macclesfield to Buxton, it happens to be a fine day so you park outside the pub and list the counties you can see. When we are next all together, you remind us of the claim and say you could only see six. An event (seeing the counties) was described and the conditions under which it happened given. Under similar conditions, you tried to replicate the event. Since you only saw six, a discussion ensues with lists compared and the features to look for when identifying counties debated. Let's give what you have done a Sunday morning name and call it *independent intersubjective validation*. We don't think philosophers like Leonelli and Longino think the possibility we are discussing should be treated as a myth and that none of us can believe our senses. At least, we hope that is not what they are saying, for we would all be in trouble if it were true. What they are saying is that under the tainted percept postulate, there can be no guarantees of a mapping between theorised descriptions of phenomena and how the phenomena are *in themselves*. Give that a Sunday morning name of its own and call it the *theorised abstraction gap*. In fact, they are saying a bit more than this. They are saying inevitably there must be a theorised abstraction gap and that gap is obscured by disciplinary practices.

In your taking in the view outside the Cat and Fiddle, you were using common sense mundane practical methods to identify counties in the surrounding landscape. You were not having

[8] Just in case there are those who find this example opaque. The Cat and Fiddle claims to be the second highest pub in England and lies on the A537 between Macclesfield and Buxton. During adverse weather conditions (such as snow or low cloud in the winter months) the police have increasingly taken to closing the road to prevent those whose ambition does not match their preparedness and/or competence from being stranded or driving off the road into the peat bogs on either side.

a philosophical argument with yourself about appearance and reality, truth and objectivity. When scientists attempt to replicate studies, findings and approaches, they are using common sense scientific practical methods to see if they can reproduce the results others have claimed to have found. They are engaged in independent intersubjective validation and do so within the conditions required by the disciplinary culture they share. They are not testing a conjecture about appearance and reality or objectivity and truth. It is perfectly feasible to ground a sociology in the possibility of a theorised abstraction gap in science because such a sociology is a description *of* science not *in* science.[9] As we have said more than once already, it is a description from a particular socio-philosophical theory of perception in which 'politics' closes the abstraction gap. You can adopt that view if you want though, at least for us, it comes with consequences we would rather not have. What you cannot do is undertake science and at the same time doubt your senses. Unlike Leonelli and Longino's sociology, the sciences are not premised in the postulate of the tainted percept but adopt the common-sense attitude that most of the time for most purposes you can trust them. To be sure, there are problems of aberrant data, artifactuality and happenstance. But to address those is precisely the point of independent, intersubjective validation. So, there it is. On the one hand we have descriptions of philosophically inspired processes of epistemological impossibility and on the other we have descriptions of scientifically organised processes of practical agreement. They are not parallel descriptions of the same thing!

The upshot of this is not that sociological findings could never be material for assessing empirical claims in science. Rather, since the objectivity of scientific data is not an empirical scientific claim, they cannot be germane here. However, they would be if, for example, sociologists wanted to declare light is not bent by gravitational fields, the continents are not subject to the forces of plate tectonics, migrating swallows do not follow Great Circle routes or the structure of DNA is not a double helix. All these reject empirical claims made by sciences and the issue would then turn on just how material any sociological evidence about social forces, interests and institutions might be to them. As its own history shows, SSST has often found walking the path between these material and immaterial debates an extremely tricky thing to bring off.

Götz Hoeppe's contribution to *Data* Journeys stands as an existence proof that objectivity in the sciences is not a myth. It concerns a scientific claim, its assessment by a scientific community and its eventual generally accepted refutation. In 2004, a group of French astronomers

[9] In fact, as this collection shows, we ourselves have made a bit of a habit of doing something similar—though we discuss modes of sociology rather than science and look to sociological practises for achieving closure rather than politics.

announced the discovery of a galaxy at a record distance from earth. Using standard methods, they had assembled digital images from a number of telescopes around the world which they subjected to spectroscopic analysis. For once, we get lots of technical information on what these methods consist in and the particular pieces of evidence the French team thought were significant. As is *de rigeur* in Astronomy, at the same time they announced their finding the team made their data available.

The announcement obviously caused a stir in astronomical circles and almost immediately other teams set out to replicate the French findings. The first challenge came from a team at Imperial College in London who re-analysed the spectroscopic data. Not only were they unable to re-produce the French team's findings, they stumbled on a technical reason for their failure. The French team had not weeded out three "hot pixels" from the images because they had not used (or had not bothered with – our comment not the Imperial group's) the step of frame level comparison. These "hot pixels" contributed to the extreme redshift measures on which the French team had based their case. Thereafter, other attempts to replicate the digital images were carried out. Again, they met with failure. Further examination of the discrepancies between the original data and the tests led to the conclusion that some transient source might well have produced the signal the French team thought was the galaxy. All these attempts at replication made it very clear in their publications that the French team had been very helpful with and supportive of the work which challenged their own findings/discovery. Eventually, the French team published its own assessment of all the research. This contained a major walking back of the initial findings together with some new data and commentary. The story closed when the organisation to which the French team belonged issued a formal statement accepting the original claims were no longer to be considered valid.

The Hoeppe case makes fascinating reading. It is a demonstration that for the practise of science, the problem of objectivity is seen to be a practical problem to be addressed by the collaborative practical solution of experimental replication or data analysis and corroboration of findings. As a process, it has all the pragmatic utility of working for most science most of the time. That is, the process closes in a positive or negative result. As we have mentioned several times, the philosophical conceptual interpretation of this process as a problem has never been closed and its open loop nature remains a justificatory challenge. The myth of objectivity is a particular slant on that challenge. Scientists don't see their challenge as a conceptual problem, but as a practical one. Independent intersubjective validation is their solution to it. It doesn't solve Philosophy's problem, but then it wasn't designed to.

MIDAS AND MEDICINE

In 2007, The Gates Foundation undertook the financial support of the Global Burden of Disease (GBD) data base (i.e., its data collection, structuring, analysing and data distribution functions) and moved the organisation and infrastructure from WHO, The World Bank and Harvard to Seattle. Subsequently, the emphasis laid upon the principle of cost effectiveness as a determinant of the value of health interventions in the GBD's reports and recommendations has been significantly reduced. The leading term for interventions now is public health: the impact on national measures for the numbers of years of life lost to disease (DALY).

Gaudilliere and Gasnier recount the history of the GBD database and its transfer from the perspective of 'political economy'. By this they mean they pick out the policy considerations which impacted by the stresses, strains, tensions, enablers and barriers generated within the partnership of WHO, the World Bank and the hosting University. Each had its own mandates and interests, policy objectives, outlooks and general political interests. All of these surfaced in the way the GBD data base was viewed and used. The issues are the obvious ones. International Agencies like WHO and World Bank are funded by the rich nations of the world. The Governments of these nations have political interests and political ambitions. Inevitably these are fed into the institutions they fund. Universities worry about prestige and association with any activity which might dent that prestige is a risk to be managed. What might appear to be small changes in name (from 'international' to 'global' as a description of a mandate, for example) can signal shifts in the balance of orientation and interest which may be differentially welcomed or resented by supporting organisations. Not only are WHO, the World Bank and the NGOs they support, "working with" (that is, trying to find ways to manage) the local political dynamics of the societies in which they "have a presence", they also necessarily have to surf the inevitable changing priorities of their largest funders. So much is the *realpolitik* of international multi-agency collaboration and the commonplace of broadsheet political journalism. They are the issues Gauldilliere and Gasnier focus on.

What they spend much less time on are the practicalities of constructing measures of relative health, disease or other related vulnerabilities which will work effectively (that is, serve the goals of the sponsoring agencies). What does it take to get them accepted, stabilised, standardised and then used in a consistent manner, processed in routine ways and understood by user organisations? A little of this is offered here and there, and is interesting. However, alignment of what is reasonable policy history/ investigative journalism to the master trope of Morgan's sociological metaphor is haphazard to say the least and to Leonelli and Longino's social philosophy entirely absent. Early on, we get this:

> In resonance with M. Morgan's analyses in the present book, we could say that the GBD database was first conceived as an accounting logic closely linked with planning: by aggregating epidemiological as well as financial data, the aim was to achieve triage, i.e. balance health budgets and prioritize investments. [p.354]

This idea such a possible underlying logic might be being worked out in the travails of GBD then disappears only to re-appear at the end as

> Morgan stresses the multiplicity of data sets economists have designed emphasizing the importance of their internal logic, i.e., the relationship between bits and whole. She accordingly distinguishes the accounting logic typical of highly integrated data sets like the matrix of national economies and the indicators logic of loosely articulated sets of numbers like those associated with the Millennium Development Goals. The difference resonates with the distinction we make between the uses of the GBD as instrument of economic triage, central to the design of packages, the comparison and optimization of investments on the basis of their cost-effectiveness on the. one hand, and the uses of GBD data in an isolated manner, as measurement of the worth of isolated interventions or projects in order to legitimize choices made on the basis of other metrics and/or criteria be they epidemiological, organizational or social. [p. 366]

Absolutely nothing turns on this hooking of the need to prioritise interventions and the rules for doing so to the construction and use of measurement instruments. It adds nothing to what otherwise is a perfectly interesting but thoroughly predictable story (the only twist in which is the insistence of the Gates Foundation that commercial, 'political' and other non-medical influences should be mitigated) and is perfectly well told without it. Yet again, a case is presented which belies the inflated claims being touted in the volume's bookending summaries.

Section 3. Conclusion

Since we began at the end of *Data Journeys*, it would be as well to end with the start. Here is the Editorial statement about the collection's objective.

> In this volume, we move decisively away from the idea that what counts as data—and in turn, how data are presented, legitimized and used as evidence— can be given (sic) for granted and that finding the correct interpretative framework is all that is required to make data "speak for themselves". We focus instead on the strategies, controversies and investments surrounding decisions around what researchers identify and

> use as data in the first place: in other words, the myriad of techniques, efforts, instruments, infrastructures and institutions used to process and mobilize data so that it can actually serve as evidence. No matter how "big" data are, the road from data to knowledge remains complex and full of obstacles. [p. v]

Several times during our discussion, we have found ourselves wondering for whom this volume has been produced. To be sure, the Preface from which the quotation above is taken reads somewhat like an undergraduate text. But undergraduates are hardly likely to have the background, interest, or motivation to trawl their way through the collection of studies provided nor are they likely to fork out the sum required to access them.

On the other hand, as the quotation itself seems to make it clear, it can't be professional sociologists, especially professional sociologists of science and technology, they have in mind either. 'Problematising' things like data, theories, experimental protocols and results which "speak for themselves" has been the cornerstone of their disciplinary practise since 1938 when Robert Merton first coined the term "the sociology of science" and drew on particular elements of the Puritan values promoted during the Cromwellian interregnum to provide the key to understanding the origins and development of the scientific revolution of the 16th and 17th centuries. Notable scholars within SSST such as Steven Shapin, Andrew Pickering, Harry Collins, Donald Mackenzie and many more have spent their entire careers tracking the strategies, controversies and shifting interests which have influenced how scientific practice has been legitimised and carried out.

Then again, it can't be the scientists, engineers, accountants and software developers whose practices they discuss, to whom our Editors and their contributors are trying to talk. These practitioners know all about the trickiness of fixing their analytic objects in ways that make them amenable to investigation, even if they might not talk about them in quite the way the Editors do. In addition, they have the scars of failed projects, rejected papers and non-replicated and non-replicable results to prove it. Anyway, as we have repeatedly said, the technical detail of the science these people live, breathe and are reliant on is precisely what is missing from almost every study in the book. Mary Morgan's study and its metaphor of data and its journeys is the fulcrum around which the rest of the collection is arranged. And, as we have pointed out, it offers a standard general sociological depiction of National Income Accounting as routinely managed 'epistemic cultures' on all fours with a very conventional sociology of classificatory schemes. In as much as they are sociological, the rest of the studies do the same. Where they lack the sociology, they are tales from the field describing routine problems and the challenges of managing complex administrative environments many of which just happen to be undertaking scientific research.

In the end, all we can conclude is that Sabina Leonelli and Niccolo Tempini have orchestrated a conversation among their authors. A conversation which, as many closed conversations are, was and is fascinating for the participants but leaves outsiders a little cold. Of course, this does not mean there is nothing of value in the studies as studies. Many have interesting stories to tell and novel illustrations which might be potentially useful inclusions in teaching materials and commentaries. This is always providing, of course, such examples can be disentangled from the misleading packaging the Editors insist on wrapping them in.

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