SWANSONG

A Valediction in Four Parts

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Good wits jump; a word to the wise is enough.

Don Quixote.

To friends and colleagues, those who have been on the voyage with us since the start almost 50 years ago and those who have joined at various ports of call on the way. We thank them all for their faith and their persistence. We would particularly like to dedicate this collection to two friends who are no longer with us, Ted Cuff and John Hughes. Their companionship is sorely missed.

Contents

Preface

The pieces presented in this collection were all written over the last decade or so. They comprise essays, reviews, extended notes, and preliminary reports representing what we hope are reasonably accessible summaries of various things we have mused on or argued over during that time. None was written with the express purpose of publication and certainly not with the thought they might make an integrated package. Moreover, they remain in various states of almost-near-completion. Whilst we recognise their unbuffed, ill-fashioned character, it is unlikely much more work will be done on them. That being the case, now seems an appropriate time to release them to the world. Although they are an assortment, we have clustered the pieces into broadly defined topic areas. It may be some will feel individual items have been mislocated. We are relaxed about that because there is no cumulative master narrative being laid out here. Other than the loose topical clustering and a tendency to move focus from the most general and introductory to the more specific and hence specialized viewpoint, there is no organising internal logic at work. One outcome of this, of course, is that many of the essays find us traipsing across the same or very similar terrain. All we can do is beg our readers' indulgence.

Part I is deliberately pedagogical in nature and is aimed at those coming new to Sociology or Ethnomethodology. It assumes no more than a reasonable introductory knowledge of what Sociology is about (or claims to be about). The aim is not to say or show anything novel but to repeat things others, especially Garfinkel and Sacks, have said before. However, it does try to do this in a somewhat different way. Our hope is to encourage those learning Sociology to see *and grasp* the problems of abstraction and formal description for themselves through examination of the detail of actual pieces of sociological reasoning rather than just acquiring a superficial knowledge of those problems and an ability to parrot arguments about their causes. Building on a body of work in the Philosophy of Science, we develop a simple heuristic for tracing how sociological reports transform data collected about aspects of the social and reduce that data to sociological phenomena which can then be analysed. The point is to show novice sociologists how they might sense assemble the practical reasoning which supports any particular sociological report which they happen to be reading. All we want is to provide a guide for those have no experience of doing sociological reasoning themselves and so we hope no more is made of these pieces than what they aim to be, working notes for a possible teaching strategy. We are clear, then. There is not much news in Part I for established conventional professional sociologists. More than likely they already know the problems from the inside. Neither is there any for fellow ethnomethodologists. The message is old hat for them. Beside introducing the heuristic, Part I also illustrates its use with types of sociological analysis other than the formal mathematical modelling used in the initial demonstration.

Part II is devoted to some misunderstandings found in the application of social epistemology to the practise of the natural and social sciences and assumes a little knowledge of the range of pathways that particular domain has followed since the (mostly phony) wars over science in the late 1980s and the related emergence of Feminist Standpoint Theory. These misunderstandings have resulted in overly extravagant claims about what sociological findings might mean for an understanding of the epistemological status of the natural and social sciences. Part III takes an interest in more parochial matters but places them in a similarly broad context. It traces the trajectory of Institutional Ethnography from Dorothy Smith's radical re-interpretation of Sociology's social character to its current realisation as the embodiment of familiar ethnographic techniques found in conventional sociological work.

We expect Part IV will mostly be of interest only to those friends and colleagues who have been wondering what lies behind our occasional public interjections regarding debates over Ethnomethodology's current progress and direction. Here the essays try to pick their way through a number of often counterposed positions in order to uncover the sources of what seem to us to be unnecessary assertions and very likely to be unsuccessful interdisciplinary and cross disciplinary projects related to them. Part IV assumes, then, more than a little familiarity with the current state of Ethnomethodology.

Although the collection presented is a motely, we think some common threads can be discerned. The first is what we have referred to as the sense assembly of the sociological. This is not surprising because it is a concern we have returned to time and again over the years. On each occasion we were motivated by a simple but straightforward question. "How can a particular sociological description be framed as an instance of practical reasoning?" Alas, the answers have always proved less simple and less straightforward to frame than the question and so we have never been able to shape a fully satisfactory response.

The problem manifested itself once again when we took up an interest in applications of forms of mathematics within sociological reasoning, especially attempts to apply mathematical models from Physics to represent problems in sociological theory and analysis.[1] When reading examples of mathematical sociology, we became more and more aware of a disjunction between the monothetic character of the sociological phenomena described by the mathematics and the polythetic nature of the social experience depicted in the data from which the phenomena were derived. This we called "the problem of the abstraction gap" and the strategies for its practical management we described as its "praxeology". Such management strategies were provisions made in the construction of a sociological investigation and its reporting to prevent difficulties arising from the processes of isolation and simplification which generate abstraction gaps together with attempts to ameliorate the situation when they did. We suggested their role was to secure or sustain the plausibility of the reasoning structure developed in the report. The broad metaphor we used to draw these issues out was the conception of writing sociology as a production process and the writer and readers as complementary 'recipient-designed' writer/reader social actors.

It is easy to discern the influence of Harold Garfinkel and Harvey Sacks in this formulation. Their discussions of modes of sociological description and related suggestions about the reconstructed nature of sociological accounts and "the unsatisfied programmatic distinction between and substitutability of objective for indexical expressions" [Garfinkel 1967, p 4] had already pointed to the possible significance of the abstraction gap we had identified. However, they and we lacked a framework by which its features might be made visible and so analysable in terms of their local 'production processes'. This deficiency was remedied when we encountered what were for us at the time two unrelated clusters of ideas, though now we now see them as intimately connected. The first was the claim made by James Bogen and James Woodward [Woodward and Bogen 1988] and later revised and extended by Woodward [Woodward 2009] that the natural sciences do not describe their data. They describe their phenomena. The process of scientific investigation in the natural sciences is a process of data acquisition and

[1] Throughout this collection, we shall reserve capitals to designate disciplinary, sub-disciplinary bodies of practise (Physics, Sociology, Arithmetic, Epistemology etc.) and clearly identified unified bodies of work (Standpoint Theory, Critical Theory, Institutional Ethnography, Newtonian Mechanics, Quantum Mechanics, Ethnomethodology etc.). We use lower case designations (mathematical sociology, applied mathematics, social epistemology, quantitative and qualitative sociology etc.) to particular modalities of practise.

transformation followed by analysis. In the transformation phase, experiential data are turned into theorised phenomena. The second was a remarkable set of studies by Mark Wilson [Wilson 2017; Wilson 2017; Wilson 2019]. In these studies, Wilson reconstructs the practical reasoning of Classical Mechanics. In so doing, he points to a range of practices which he groups under three heads: constructing well posed problems; avoiding difficult or undoable physics; preserving internal coherence by effacing unnecessary or irrelevant features or steps. Professional good practise in Physics consists in deploying these strategies and their tactics in recognised ways and under normatively regulated conditions.

For Bogen and Woodward as well as Wilson, data reduction is key. To be processed by analytic methods, the manifold of experience captured in and as data must be rendered down. This rendering is a response to the challenge of choosing among the problematic possibilities of description when assembling everyday experience. The term the mathematical sciences use for this resolution is 'characterisation'. Characterised data are data turned into phenomena. Characterisation involves the selection of a minimal set of parameters to be used in the description of an object or process and the stipulation of the variables and measures applied to those parameters. Analysis is the generalisation over equivalence classes constructed from characterised data.

Related to the first thread is a second; the epistemic status of sociological descriptions and the metaphysical claims which can be made on their basis. Whilst visible elsewhere, this thread actually provides the tonal palette for Part II. In previous work [Anderson and Sharrock 2019], we have argued for the treatment of sociological representations as "keyed descriptions" with their defining conceptualisations being "convenient fictions" organised by tropes. The need to reiterate the advantages of this instrumental treatment was brought home to us recently by a couple of comments made by David Albert. He prefaces these comments with a little story in which he imagines he is teaching Classical Mechanics. Having outlined the nature of the 6^N-dimensional phase space and how it can be represented in a single curve, he points out the mathematical advantages of this form of representation. After the lecture, two students approach him. The first exclaims: "I now understand that we *really* live in a 6^N-dimensional physical space—not the 3-dimensional one I had always taken for granted". The second, somewhat more cautiously, says: "I now understand there is simply no *fact of the matter* as to whether our world consists of N particles moving around in a 3-dimensional space or a single particle moving around in a 6^N-dimensional space". Albert makes it clear both students have failed to understand his lecture and the nature of models in Physics. He goes on:

> What I should *say* to these students—what I should *explain* to these students—is that phase space, as it is employed in classical mechanics, is an explicitly and self-consciously "roundabout" way of talking about systems of classical particles. It is useful for all sorts of practical and theoretical and calculational purposes—but it is not meant to be taken as a *direct* or *literal* picture of what is going on. [Albert 2022]

The import of Albert's comment for us lies in the state of affairs it points to. If, in a discipline as rigorous and systematic as Physics, those learning the discipline and going on, no doubt, to have successful careers in it, can simply fail to grasp the constructed nature of their discipline's representations and the reasoning processes behind them, how much more likely is it to be the case in Sociology? [2]

The third thread runs through almost every element but is most easily glimpsed in Part IV. It concerns Ethnomethodology's ambivalence towards conventional Sociology and the tensions generated thereby. Ethnomethodology emerged from conventional Sociology as a response to dissatisfaction with the central tenets of that discipline's methodological stance. As a result, setting up a well-known sociological position on some phenomenon as the foil against which an ethnomethodological treatment of what appears to be the same issue can be laid out, has become a standard opening move for investigations. Usually, this is couched as an allegation about features of "the setting" being deployed as resources rather than taken as topics. Although Garfinkel is often more guarded, he is nonetheless very clear. For him, Ethnomethodology is an alternate sociology not an optional investigative technique within conventional Sociology. It has been formed and framed to do things conventional Sociology does not want to do and could not do even if it wanted to. The two are incommensurable in some sense; a position which led to him defining Ethnomethodology's attitude to conventional Sociology as "indifference".

Given the principle of indifference and its foundation in Ethnomethodology's incommensurability with conventional Sociology, it makes no sense to berate some instance of Sociology for not being ethnomethodological. Yet, this is what the use of the foil often amounts to. One way to resolve this tension is to treat sociological practise ethnomethodologically; that is, as cases of practical reasoning. In doing so, we are enjoined to set aside the claims professional Sociology makes about its theories, methods and findings and look solely at what it takes to get it

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[2] This is not to say all physicists are in this position or that the nature of the discipline's representations has not been understood. However, for many the implications are both puzzling and distasteful. Unlike Albert, their reactions when queried on them is more likely to be that summarised by David Mernin's sardonic "If I were forced to sum up in one sentence what the Copenhagen Interpretation [of Quantum Mechanics] says to me, it would be "Shut up and calculate!" [Mernin 1989]

done as the practical, routine, generally accepted and acceptable going concern it is. Points can be marked where the practice of the discipline departs from the claims made about it, but such marking is simply a note about the practical, not an opportunity for excoriation and critique. Given the principle of indifference, it could not be.

It would seem obvious that Ethnomethodology can adopt precisely the same ethnomethodological attitude to instances of ethnomethodological reasoning itself and so treat them under the same principle of indifference. Indeed, we ourselves have occasionally attempted this. However, and this is a crucial point, all species of Ethnomethodology should exhibit adherence to *the same* fundamental principles, a condition which does not apply vis a vis Ethnomethodology and Sociology. Whilst it is clear that it is not a requirement of ethnomethodological analysis of ethnomethodological reasoning, commentary can draw out when and where adherence to those principles seems to have lapsed. A just balance must be struck, however, between Ethnomethodology's aspiration to analytic indifference and its desire for the achievement of disciplinary probativeness.

A further thread is the difficult question of Ethnomethodology's potential therapeutic function with regard to the domains in which it carries out investigations. Although it seems pretty clear Ethnomethodology might be able to play that role for Sociology, it can only fulfil the necessary diagnostic and remediation requirements by setting aside its critical stance and with it the oft adopted insistence Sociology should re-specify itself in line with ethnomethodological principles. No doubt such a change in tack would present challenges, obligations and constraints but they are likely to be vastly different to and more straightforward to accommodate than will be the case with many other arenas of disciplinary and professional practise and, in particular, the natural sciences.

Our final thread is possibly the most elusive but perhaps the most important. Its shadow presence can be felt in our repeated use of words like "depiction" and "construal" in relation to sociological descriptions and their construction. These terms are functions of our attempts to operate a systematic suspension (though, having been shooed out of the front door, these beasties have a way of creeping back in a rear window when you aren't looking) of epistemological commitments such as empiricism's myth of the given, the Kantian fudge of the synthetic apriori and Phenomenology's vision of a transcendental Ego grasping noumenal essences. This suspension leaves our sociologising oriented towards a space of descriptions (to paraphrase Wilfred Sellars) with no place for naturalistic observation, analytic realism or eidetic reduction. Such a sociology has no truck with 'fundamental', 'foundational', 'grounding' or 'necessary' or any other epistemologically privileged representations except as social objects subjected to its analyses. Instead it accepts the imposition of an overriding concern with the praxeological character (the 'how') of the organisation of description and the methods deployed to manage and resolve the problematic possibilities of adequate description under whatever canons of adequacy may be in play on the occasions being examined. This radical equality of sense assembly is, of course, reflexive. It could not be otherwise. For this form of sociology, since the only way to determine the adequacy of accounts is from of the context of their deployment and the determinations made by those engaged in the courses of action unfolding therein, the adequacy of its own descriptions is given by the consistency and coherence of the application of its adopted structuring by keys. This is not the whirligig of relativism nor the regress of scepticism but a commitment to the probativeness *within* modes of analysis. Given a mode and its keys, things can be settled. But only for that mode.

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Part I

Sociology and Sociological Worlds

Introduction

We think a good way for careful readers to approach reports of sociological investigations is to treat them as narratives or stories about goings on in some segment of a social world. By this, we do *not* mean we think sociological reporting is full of falsehoods and inventions, though some may well contain not a few convenient fictions and rely a little too heavily on what might be called 'sociological urban myths'. Rather, it is that investigative reports have conventional narrative structures and generally deploy the same narrative components. The objective of the essays in this Part is to provide an extended introduction to and illustrations of a suggested heuristic, a guidance note, for understanding sociological reports as constructed narratives. The structure of the heuristic is derived from studies in the Philosophy of Science whilst its core principle is a cornerstone of Ethnomethodology's conception of the nature of sociological descriptions. The essay on the heuristic uses examples from formal mathematical analyses of the social. However, other essays in this Part broaden the scope to include more and different styles of sociological analysis.

In relation to all these illustrations, there are two things we need to state very clearly right at the start. Pointing to the formal and informal devices which sociological reasoning deploys does not mean we intend a sharply critical stance. Just a careful one. The heuristic proposes a re-presentation of the materials contained in a research report and a way of constructing their interrelationships. If the reader wishes to, it could also provide a basis for adopting a more quizzical attitude. Having grasped the contents of the report, the reader can move on to a conventional probing of the degree of adherence to the self-imposed disciplines of method consequent upon adopting the investigative strategy chosen by the researcher. Such a probing could be placed alongside an exploration of the implications of any variation in that adherence for the security and plausibility of the findings laid out. Alternatively, it could be used as an "aid to a sluggish imagination" (to quote Garfinkel) for a much less conventional endeavour; an ethnomethodological consideration of the report as the display of practical reasoning occasioned by the necessity to *manage* method when mounting an investigation. Where these two options differ is in their premises. For the first, choices over modes of implementation of an investigation, whilst conventional within the discipline, are assumed to be open. The researcher chooses a method and follows its 'recipe' as a feely adopted solution to options such as selection of a system of data collection and with it, data analytic techniques and forms of analysis. The presumption here is that, in most cases, adherence to the rules of the method is sufficient to provide re-assurance about the plausibility of the case being made. For the second form of analysis, the proposition is that no abstract depiction of method suffices to remove all methodological decisions. It is the "how" of this decision making which is front and centre now, under a recognition the contingencies of any investigation mean the detailed application of any chosen method must be worked out "first time through" each time, every time. Such contingency managing decisions are the practical reasoning on which the recognisability of following the method rests. In this second option, an axiom is being explored, namely that there is gap in the texts which set out the method. Successfully utilising the method entails resolving the gap as part and parcel of applying the method here and now so that the plausibility of the findings can be sustained. Many studies in this Collection display elements of both these forms of analysis.

Although use of our heuristic might be complemented by an appraisal of the findings demonstrated and the claims made about them, appraisal is not its purpose. Nonetheless, a probative discipline (which is, we assume, what Sociology aims to be) should welcome such summaries. In our illustrations of the application of the heuristic, we focus on forms of mathematical sociology and suggest they rest heavily on a limited range of operations applied to $\mathbb{R}^N$, the real number system. This reliance has several consequences. Saying so is an observation not a disparaging comment. Neither, and this should be an obvious truism, is it a prediction of the inevitable failure of any mathematical structures or any formal methods, including formal languages, to be of value for sociological reasoning.

With regard to the ethnomethodological analysis of practical reasoning which might be prompted by the use of the heuristic, the hope is investigations might lead to greater clarity about relationship between Sociology's generally endorsed methodological and disciplinary objectives and the actual epistemic virtues it is oriented to in practice. We see provision of such clarity as a positive contribution to Sociology. Rather than promoting the annihilation of formal analysis (as is sometimes assumed must be Ethnomethodology's mission), analyses could well lead to the remediation of identified shortcomings. Since all enquiry is a human activity, we should expect any sociologising to display at least a few errors or maladroit moves. We should be careful here, though. Our approach cannot determine which modes of investigation are most suitable for which specific research topics and objectives nor how any identified shortcomings should be addressed.

It could not. Instead, somewhat akin to the way work by Michael Lynch and colleagues in the Sociology of Science prompted the philosophers James Bogen and James Woodward [Woodward and Bogen 1988] to rethink the fundamentals of the Philosophy of Science, the approach we recommend could encourage researchers to reflect on how data are transformed into phenomena. Most important of all, though, for us the approach is of interest in itself. But we hope others, such as those learning to be practitioners, might find it to be of some use as well.

Finally, as we emphasised in the Preface, we are very aware we provide a *reading* of the work of sociologising. Necessarily, it is just one among many possible descriptions. Given our commitment to the sense assembled character of any working sociology, it would be perverse of us to think otherwise.[1]

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[1] Our old friend Martyn Hammersley [2022] once chided us for professing this ecumenical sentiment and then immediately following it with a claim that what we did was a 'First Sociology'. Alas Martyn had not noticed what we were talking about was not the order of importance of the work undertaken but the logical ordering of the objects we took an interest in when viewed from the point of view of the sociality of the processes of reasoning we were examining. They come *before* the reasoning gets underway, and hence are *principia* rather than *theoria* introduced within that reasoning.

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Reading Sociologically

Introduction

This is a journeyman piece. It makes no big claims for its proposals and tries to make as plain a case as it can for them. The objective is to outline an approach to the reading of sociological research reports which treats such reading as the sense assembly of plausible accounts of the phenomena they depict. The depictions given are sociological objects and the phenomena they represent are social ones. We will suggest the process of moving from the one to the other is a matter of data reduction, a process which thereby creates an abstraction gap. In assembling their sense or understanding of a sociological report, we propose readers should seek the extent to which the span of the abstraction gap reduces or increases the plausibility of the account being given. We will summarise our approach in a reasonably self-evident heuristic and illustrate it by reference to a few studies. The studies are drawn from the sub-domain of mathematical modelling in Sociology. There is a reason for this. Mathematical modelling in Sociology deliberately couches phenomena in terms of well-attested formalisms usually derived from the physical sciences. These are abstractions. Sociology's data have their origins in the concrete accounts of their experience of social phenomena which ordinary members of society provide as input to sociological research processes.[1] Making this stipulation means, for the moment at any rate, we can avoid having to

[1] This input can take many forms and be more or less distant from the experience described. Personal accounts, documents and reports, survey responses, participant observation, experimental set ups and the analysis of official statistics all require some form of coding before being 'mathematised' in an analytical model.

address issues of realism, naturalism, subjectivity, faithfulness to the phenomena and so on which would otherwise complicate our task. Mathematical models are just that: models. No-one wants to nor should confuse the model with that which is modelled. There is another advantage to be had. The source of our heuristic lies in philosophical thinking about the epistemology of Physics. Since it borrows so much else from it, we can make the reasonable assumption mathematical modelling in Sociology is committed to the same epistemological virtues as Physics.

Section 1. What is a Well Posed Problem?

Stephan Körner once observed Pure Mathematics has parted company with perception [Körner 1968]. We think he meant it had parted company with the Natural Attitude. To speak loosely for a moment, the world of pure mathematical objects (e.g., infinitely small points, converging parallel lines, denumerably infinite numbers, trans-finite numbers, multiple systems of numbers including countable numbers, negative numbers and a number for the absence of number, etc.) is not derived unmediated from the object world of daily life. The divergence, however, is of no matter until we seek to apply Pure Mathematics back onto the world of common sense. When deciding how much concrete will be needed to fill a post hole, work out how much carpet to order for a room or the time it will take to make a journey by car, we find ways of bringing the abstract objects and associated manipulative techniques of Pure Mathematics into alignment with the activities of daily life. We do so by relaxing rigour and definitional rigidity to allow approximation and relative goodness of fit. Körner suggests this involves substituting inexact for exact concepts.

As we will see, with Sociology at least, what is going on is a bit more complicated. Study after study has demonstrated the sociological attitude involves the use of sociological concepts which depend on elements of the Natural Attitude. In its actual practise, the sociological attitude is a comingling or confluence of technical sociological conceptualising and the Natural Attitude of common-sense life. Doing sociological investigations could well be described as naturalistic sociologising. Applied mathematical modelling in Sociology comprises two orders of transformation therefore: from common sense modes of understanding of the social into a naturalistic sociology's modes of understanding and from there into a mathematised naturalistic sociology's mode of understanding. These transformations are not incommensurable. Much is preserved, though some things are set aside or, as we will say, effaced. As we have said, our interest is in the character of these transformations and their implications for the sense assembled plausibility of the analyses they sustain.

Somewhere on the edge of the novice sociologist's learning strategy should sit two questions. The first is: What is it about the social that makes a 'sociology' possible?[2] In other words, what is it about the metaphysical characteristics of social phenomena which mean they are amenable to being corralled and subjected to systematic investigation? The second is: What are the ways in which the social are made investigable by the kinds of sociology which are generally practised? Although related, these two are not the same. The first is *philosophical* and its hinterlands are issues of perception, cognition, ontology and, of course, epistemology. The second is *socio-methodological*. It has as its hinterlands the socially organised character of the practices by which investigations of the social are carried out. Our starting assumption concerning issues of practise and metaphysics is that for most sociologists most of the time, solutions are culturally given within the standard forms of the discipline they adhere to. They come in conventional packages and are simply how 'sociologists like us do what we do'.

The focus here is on the institutionalised choices made regarding mathematical modelling in Sociology. We are agnostic with regard to the relative 'value' (on whatever dimension you want to construct it) of the examples we review. Rather, borrowing a term of Brian Smith's [Smith 1996], we are interested in them as particular modes or methods for "registering" the social which have been adopted within the discipline. Inevitably, such registrations are abstract and generalised. Our questions are about the constraints or requirements the use of these descriptions might impose on investigations. Clearly this does not mean we should demand all accounts be formed in the same way. They are, after all, different kinds of sociology and address different kinds of perfectly proper sociological objectives. But, since they are classes of generalised description, it is reasonable to ask how they stand as representations of the phenomena they encapsulate.

We will lump the tactics used to produce these renderings under the label "strategies of effacement", a notion we have taken from Mark Wilson [Wilson, 2017a; Wilson, M., 2017b]. Strategies of effacement are practices by which phenomena of interest are distilled and fixed so they can be subjected to investigative scrutiny. What we are looking at are the practices enabling the mathematical formulation of sociological problems evident in the instances of mathematical sociology we examine. The notion of 'practice' is a term of art in ethnomethodological accounts of professional work. We are not intending a fully formed ethnomethodological analysis. All we are doing is illustrating one way in which ethnomethodological concerns with sociological description

[2] The term 'sociology' in this sentence does not designate an existing body of disciplinary practice nor some extant sub-domain. Rather, it means *any* sociology which might be made possible. The sociologies we have are just a (small) subset of the sociologies we might have.

as a mundane activity (its recognisably plausible reasoning) might be reflected back into the body of professional Sociology as a positive contribution to Sociology's own self-understanding.

A Position and Some Terminology

Guides to methods of mathematical analysis usually address problems by citing examples rather than their characterisation. To pursue the issues we have set out, we draw on some of Mathematical Physics' terms and concepts, albeit with the recognition that they will have to be adapted to be serviceable across the range of endeavours we have set our sights on. One particular resource is Jacques Hadamard's [Hadamard 1923] reflections on what constitutes a "well posed problem" in the mathematical analysis of physical systems. The domain he looked at was differential equation modelling and what is known as "Cauchy's problem". However, before we can turn to the details of analysis, we must consider the way the 'small world for analysis' is constituted. As already intimated, we call this the process of *registration*.

Each discipline has its own ways of constructing its processes for collecting data, transforming them into phenomena, and then presenting analytic results. All these ways construct a world-for-analysis. When turning to the social world as target for investigation, sociologists 'bracket' their normal, everyday understandings of how social life operates and construe the social processes they are interested in sociologically. The term "registration" is Brian Smith's [Smith 1996] and refers to the experience of directing one's attention toward objects rather than (passively) perceiving them.

> To register the world....is to do or be oriented towards the world *in such a way that it presents or arranges or constitutes itself as a world.* [Smith 1996 pp. 194-5 italics in original]

Smith goes on to say that registration not only refers to attending to individual objects and subjects but includes cultures, language communities and any other collectivity or social institution. What makes 'registration' particularly apt for our use are some of its connotations. Registration is an intentional act in the Brentano sense and hence someone whose registration is under discussion has a role in its selectivity. Registration does not just happen to you. Second, registration is not content free. Something is registered *as* a particular individual or type of object or process. Third, registration also carries the notion of alignment. To use a broad mathematical term, there is some mapping between the object and the mode of registration of the object. All these connotations testify to the premise Smith's use carries. The world-for-analysis is configured in large measure by the disciplinary and other cultural machinery we bring to it. It is important though not to see registration as purely or even primarily conceptual, not just the application of a schema or framework. As embodied beings, our awareness context is shaped both by how we think about things and by how comport ourselves towards them. *Mutatis mutandis*, this extends to sociological registration on those occasions where an important feature of a particular course of social action is the arrangement of or physical engagement with objects or other persons. Being clear about registration, allows us to move on to look at definitions of the domain and descriptions of objects in it. This we will call *characterisation*. Hadamard's analysis of Cauchy's problem provides a perfect guide here.

Cauchy's problem is this. Differential equations track change across two or more variables. When characterising a phenomenon in order to analyse it, one or more parameters of the distributions (what are called the 'beta' values) must be unknown. Empirically (under sets of specific real-world conditions), a particular set of values may provide a solution to the problem posed by the unknowns in the equations. However, our arriving at one solution does not mean we have exhausted the possibilities. In principle, an infinity of other solutions could be found along the curves described by the functions we have used simply because, by definition, these paths are infinitely divisible (the co-ordinates are given in the real number system $\mathbb{R}^N$). Cauchy's question was how to bound problems rigorously and precisely so that we can fix on a single solution. Clearly the *idea* that investigative problems should be well-posed is a general one. We confine ourselves here to what it might mean when applied mathematics is used to model in Sociology.

The essential feature of a well posed problem is to be found in the setting up of that problem (the 'form' the problem given for our analysis takes and the 'data' applied to it). This should be defined sufficiently clearly and precisely to allow a clean 'cut' or disengagement of our designated investigative objects and properties from the plenum of actualities encountered in the real-world situations from which we have drawn that problem. The set of arrangements (S) being examined must be sufficiently well extracted from surrounding environment (E) for S to be subjected to controlled manipulation and analysis.[3] Part of this cutting involves the "effacement" of detail. In discussions of their work, all researchers refer to this process as "simplification", "condensation", "idealisation", "abstraction" or in some similar way. These terms are perfectly

[3] Although this sounds like a version of the bench experiment, in fact it is the other way around. The bench experiment is just one way in which the well-regulated cut required for a well posed problem can be achieved.

serviceable but they lack the precision Hadamard demands. A well-posed problem demonstrates just what simplification, idealisation and abstraction *inter alia* should come to.

Hadamard suggests a well posed problem has the following characteristics.

1. A solution to the problem exists. That is, there is a known form or family of forms for representing the relationships depicted in the problem under a range of freely varied initial conditions.
2. The solution is unique. That is, the solution form does not lead to multiple possibly stable conditions (the potentially infinite solutions mentioned above) with no basis for making choices between them.
3. The solution responds in robust and stable ways to the range of data applied to it. The term Hadamard uses here is "continuously". By this he means there are no envisageable combinations of conditions among possible outcomes for the effaced system which generate 'chaotic' divergence thereby causing the problem to become "ill-posed".

For Hadamard, three further features or factors are required for a problem to be "well posed".

1. A set of boundary conditions marking the effaced transition of S from the plenum of E.

   Mark Wilson defines boundary conditions as:

   > Boundary conditions, roughly characterized, represent claims about how a certain portion of the universe interacts with its surroundings along their mutual boundary. [WILSON, M., 1990, p.566]

2. A set of initial conditions under which the effaced S begins to operate.
3. A well substantiated closed set of functions (for Hadamard these were always equations) describe the behaviour of S. By 'closed' he means there should be no gaps, elisions or uncontrolled collapses of phenomena in the account of S given under these functions. These closed sets are often referred to as "system laws".

Finally, general guidance is offered for the composition of the elements.

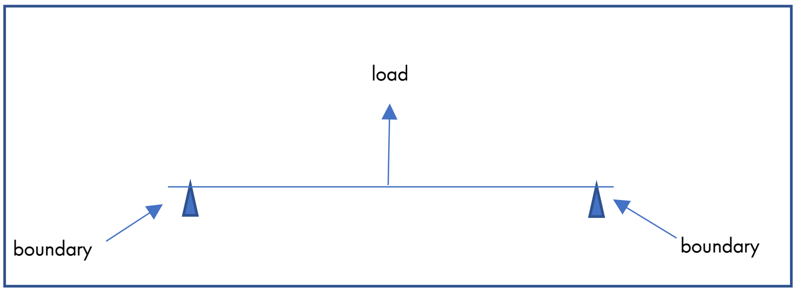
1. The set of boundary and initial conditions (**W**) should be sufficiently restricted to allow at least one possible solution to exist.
2. The boundary and initial conditions should not be so restricted that multiple possible solutions exist.
3. The form of the function used and the nature of the conditions selected should be matched otherwise the requirement of continuity may be undermined.

At first sight, it might seem Hadamard is trying to reduce applied mathematics to recipe following and rules of thumb, but this is far from the case. As he makes clear in his book *The Psychology of Invention* [Hadamard 1954], what we have just described is an orderly framework within which mathematical imagination, inspiration and perspiration can be channelled. Unfortunately, his views on the sources of this imagination and inspiration are very much of their time. Such reservations, though, should not undermine the value of his description of the formal conditions for 'well posed problems'.

A Toy Example

Suppose we are interested in the behaviour of a harp string. The string is connected at each end to fixed pegs and we want to describe how it vibrates when we pluck or tap it. This is a standard introductory example in applied mathematical modelling and, indeed, one Hadamard himself uses.[4] The system we are describing can be depicted like this.

Figure 1



The tapping or plucking of the string exerts a load causing it to deform. Once the load is released, over time the string returns to its original position.

What have we effaced in this model? The short answer is everything except the length of the string, the position of the pegs and the force of the loading. Other factors (well known to affect

[4] The version we are using is Wilson's. It is a favourite of his and is used in several places. The most technical is [Wilson 2006] whilst a more general recent introduction is [Wilson 2022]

real strings on real instruments) such as air pressure, humidity, the type of material of the frame and the relative condition and composition of the string have been severed off. We'll come back to the accommodation of these factors later.

What are the boundary conditions laid down for the problem? They are, first, that the string be fixed in position. Plucking or tapping it does not cause the pegs to move or the tension to be reduced. The system stays stable. This is known as a "Dirichlet condition" since it determines the values of the system at its boundary. At the pegs, movement of the string is zero. We do not have to try to work out just how much the pegs resonate as the travelling wave of restorative force hits them once the load is removed nor what that instantaneous deceleration does to the shape of the travelling wave. The second condition is the set of the precise co-ordinates of the load's position and the load's magnitude. Where the load is placed sets the length of the string each side of the pluck point and thus the pattern of wave interference as the waves meet each other on their return from the pegs. Third, the movement of the string in response to the pluck or the tap is vertical to its original position. In other words, the string does not warp. Such warping would require an analysis of the dynamics of the forces along the interior of the string. These two conditions are also Dirichlet with the latter allowing what Wilson calls "physics avoidance" of tricky, perhaps unmanageable, conditions. The clearly stipulated features of our well posed problem leave us with a set of conditions (**W**) which are precisely defined and bounded.

What, then, are our initial conditions? They are (a) the position and length of the string (its coordinates); (b) the position and magnitude of the pluck or tap; and (c) its precise timing. Obviously, what we are tracking are the dynamics of the string's movement over time (in physics-speak, the trajectory of its states from t_0 to t_n).

Finally, what system laws can we use to track the string's trajectory? These are the standard wave equation, Newton's basic F = ma 'law' and Hooke's 'law of restoration'; all three being used together under the stipulation of no warping. The wave equation has the following general form, the components of which can be filled out by plugging in measurements processed by the above laws.

$$\frac{dy^2}{dt^2} = \frac{c^2dy^2}{dx^2}$$

In providing for the curvature of the string and the restorative force of the loading, the combination of system laws achieves the required equational closure. We can use it to project forward the states of the system over the relevant time frame in which the boundary conditions remain in place.

This gives us what we can call "descriptive sufficiency" for the problem we have posed over the time frame we have defined. The description is sufficient to allow an appropriate solution to our well posed problem.

What happens if we want to alter the boundary conditions, say by introducing a violin-like 'bridge' over which the string passes? This introduces a new set of states (**P**) into the model. The challenge is to define a new set of Dirichlet conditions which ensure the integrity of the system is maintained under **W** + **P**. This will require any descriptions we give of **P** to be consistent with our descriptions of **W**. In our toy case, we will require further characterisations in the terms given by the system equations. The demand for the maintenance of structural integrity in **S** means we cannot simply make our effacing cut anywhere. The choice we make must respect the demand for consistency.

In all of this, as Hadamard and practising bench scientists are well aware, the nature of the measured data is critical since the well posed character of the problem depends upon continuity with that data. It is here that the tricky topic of measurement error enters the discussion. Even small errors in measurement can lead to large divergences as differentiation unfolds. To combat this outcome "regularisation" adjustments are introduced. We will not discuss such technicalities here.

Finally, although there are constraints on the placing of the cut, it is important to note the modalities of effacement (what is severed off and what is retained) are interest driven. The process is not a boiling down to essences but a selection of focus. Given the starting point is the plenum of the world, how those interests are defined is open—though as we have already suggested, in large measure the process is usually institutionally or culturally given.

So, what have we got here? We have a general strategy for selecting the constituents of a "small possible world", the system S, to which we can address our interests. This selection consists of an orderly filtering or effacing of phenomena in which we are not interested or cannot handle and then the precise definition of the boundary conditions under which the features of interest can be presented. In dynamic systems, the initial conditions for analysis are set as well. Finally, we must have a sufficiently complete analytic apparatus (what we called "system laws") to cover any possible state the system might attain given the boundary and initial conditions set and the functional form of the descriptive apparatus.

The issues in constructing well posed problems are about walling off complexities we either don't want or can't manage. These complexities may be scope-related (the entanglements of the environment such as those we mentioned) or they may be scale-related (the effects of the vibration along the whole length of the string rather than the small-scale effects of molecule realignment in the cross sections of the string at points of warping).

THE HEURISTIC

From the above discussion, we can extract the following key components of a well posed problem.

1 Problem Statement.
   a. Registration and characterisation of phenomena.
   b. Notation or descriptive formulation.
2 Problem Specification.
   a. System laws and relevant generalisations assumed to be in operation.
   b. Boundary conditions fixing the scope of 'the world under investigation'.
   c. Strategies of effacement filtering out irrelevant or irresolvable issues.
3 Analytical Protocols.
   a. Analytic procedures applied to effaced descriptions.
   b. Analytically derived results interpreting over the results of those procedures.

Although this might seem like an orderly framework for a coherent narrative concerning a problem and its solution, in practice the components are often assembled in different sequences. However, using this check list as a guide, a careful reader can reconstruct the contents of a sociological report's propositions, descriptions and summaries to sense assemble the sociological significance (both meaning and import) of the material which is presented. This will be not just be in terms of the integration and coherence of the piece but also its relative alignment with other similar research reports whose contents may have also been sense assembled in this way. In following this approach, a newcomer would soon have a good grounding in how to reason sociologically and, in time, what the state of the field in any domain might be. If adopted broadly, it might even encourage more effective calibration of sociological reports and thus be a practical contribution towards the discipline's achieving a degree of probativeness. Something it does not currently possess.

Section 2. Some Illustrations of the Heuristic

We will now take our heuristic and see how it fares when applied to a small selection of ways social phenomena are registered and analysed under various forms of mathematical sociology. The cases we look at are taken from some of the major domains of mathematical modelling in Sociology. We will present the cases in relatively brief vignettes focusing only on how they are worked through. We will not provide their disciplinary background, consideration of their strengths and weaknesses as solutions to the problems taken up nor review subsequent developments in the relevant fields.

ABM and Racial Segregation

Our first example is probably the most abstract and certainly the most controversial. It is Thomas Schelling's description (what these days is called an 'agent-based model') of the unintended generation of residential segregation by race resulting from members of a community acting on their preferences for whom they would like as neighbours. Whilst the 1969 paper [Schelling 1969] is perhaps the more well known, the developed account of his simulation is to be found in the 1971 version [Schelling 1971]. In both papers, Schelling presents his results as three scenarios. Using the terminology we have adopted, we will say these scenarios represent related sets of registered and characterised small worlds and their boundary conditions, though the third is really a different problem. In each case, the scenario shows how segregated patterns of residential distribution emerge given a range of initial conditions applied to given boundary conditions. Central to the array of scenarios is the nature of the space within which the distributions are considered. The argument Schelling presents moves from 1 dimensional, to bounded 2-dimensional and unbounded 2-dimensional spaces.[5] The simulation is adjusted as we move through the types of space.

We start by scoping how Schelling registers the small world. First, he posits a formal analogy between the social processes of racial segregation and the emergence of unanticipated and hence unintended structures (so-called "hidden hand" consequences) in market systems when buyers have well defined and ordered preference for goods and services together with free choice over how to exercise those preferences. When it emerges, this kind of segregation is "unorganised". By calling the analogy 'formal', we mean the same abstraction can be used to

[5] Just to be clear, "space" in this and related discussions does not necessarily refer to our ordinary 3-dimensional sense of space. Instead, it refers simply to a set of objects and the defined relationships among them.

represent the general principles at work. In choosing racial segregation as the social phenomenon for his model, Schelling defines the second of his boundary conditions. The criteria for segregation are said to be binary (white and non-white (black)), exhaustive (everyone is either white or non-white), and immediately recognisable (expressed as skin colour). In his first two scenarios, Schelling adds an important third initial condition. No-one in the community has a preference for the overall mix in the neighbourhood even though they do have preference for the mix of their own nearest neighbours.

One Dimensional Bounded Space

The following further boundary conditions are laid down for this scenario.

1. The space or neighbourhood is linear and can be continuously (i.e., infinitely) partitioned (operates like the number system). With appropriate expansion, space can always be found between two contiguous areas.
2. Each member of the neighbourhood has a residency preference or tolerance for the proportion of the category opposite to themselves living in their immediate environment. If that preference is met, they will remain in their current position. If it is not, they will move within the community to where that preference does hold.
3. Movement is frictionless and unconstrained (in other words, costless and always possible because of the infinite divisibility of the space).
4. The number of residents in the neighbourhood is fixed.
5. Each triggered movement is to the nearest 'space' which satisfies the member's tolerance level. This is achieved by assuming movement is facilitated by everyone 'budging along' to allow the incomer to be inserted and the space the incomer left thereby to be filled.

In this scenario, we have a fixed number of members of binary categories arrayed linearly with 'motivating' conditions under which they will stay in place or move. Schelling now imposes two "system laws".

1. Given the binary nature of the categorisation, both categories cannot be in the majority.
2. The tolerance schedules are functions holding the ratios of the categories' preferences. Once an individual's neighbourhood reaches the upper threshold, that

individual's movement is triggered. Schelling left these functions defined informally. Haw and Horgan [2018] have recently formalised them as:

$$\frac{Y}{X} = XR_x\,(X) \quad [X's\, preference\, ratio]$$

$$\frac{X}{Y} = YR_y\,(Y) \quad [Y's\, preference\, ratio]$$

3. The dynamics of the process can, therefore, be expressed as a pair of differential equations.

$$\frac{dX}{dt} = X[XR_x(X) - Y$$
$$\frac{dY}{dt} = Y(YR_x(Y) - X$$

It is important to note the schedules comprise two different types of function. One is the paired preference orders just described. The other is an *if...then...* decision rule. It is the decision rule which triggers the action if the threshold is passed. The conjunction of the two functions drives the simulation.

There are two Dirichlet conditions. The first is the definition of the actor/agent/member of the community as a finite state automaton with 4 (paired) states: satisfied/unsatisfied; move/don't move. This is the severing from the ordinary conception of a social actor acting in a social environment. The other is the stipulation that the automaton's "psychology" be limited to just one component, the preference rule. This is a necessary scaling effacement and "avoids" the complexities of having to derive a "sociology of preference coordination" which would otherwise be required. It is equivalent to our setting aside the 'interior' of the string in our toy example.

The initial conditions Schelling uses are:

1. The ratio of the categories is 1:1.
2. The placing of the categories along the space is random.
3. The preference ratio is 50:50 for a local neighbourhood of 8 nearest neighbours. For any member of the neighbourhood to be satisfied the run length of nearest neighbours in their own category must be 5 out of 9.

Having posed the problem, Schelling iterates over the distributions shifting the unsatisfied members of the community according to the rules. Each set of moves constitutes a new configuration of the racial composition of the neighbourhood and hence a new state. Over the iterations, the nearest neighbour run length proportions drive the degree of racial clustering of the community, that is, the extent of unintended racial segregation.

Three things are worth noting at this point.

1. Even this highly simplified model has far more elaborate boundary and initial conditions than our 'toy' model. This may speak to the difficulty of easily severing off parts of the social world.
2. The general structure of the model is very little different to Hotelling's famous model of the market for retail space in town centres in terms of the operation of supply and demand in unrestricted 'free' markets such as stock exchanges [Hotelling 1929]. Although the natures of the driving functions are different, segregation, the distribution of retail space and stock markets all rely on preference orders.
3. Apart from the tautology about majorities, the algorithmic system laws are nowhere near as empirically well secured as those of those covering the toy model. The best one could say of them is that they are convenient fictions constructed for the simulation.

Two-Dimensional Bounded Space

For this scenario, Schelling redefines the boundary conditions and thereby significantly shifts the cut. The space becomes delimited and 2-dimensional. Schelling uses a 13 x 16 checkerboard as a working definition of the space he is imagining. As we saw in our initial discussion of well posed problems, any relaxation of the austerity of the definitional severing raises questions concerning the maintenance of the integrity of the system under view. Adding a second dimension violates the boundary conditions of the first scenario simply because it is no longer a 1-dimensional space. Consequently, Schelling has to adjust the boundary conditions as follows:

1. The working concept of space changes from a relative one (who is next to whom) to an absolute one (everyone sits on a space defined by bounded integer co-ordinates (1:13, 1:16)).

2. A fixed number of vacant spaces is always available for occupation. When a member moves to occupy such a space, a vacancy occurs in the space left behind.
3. Movement is limited to the nearest vertical or horizontal vacant space.

Under these set-up conditions, the system laws remain the same.

The initial conditions proposed are:

1. The nearest neighbour ratio is 50:50 membership of the two categories.
2. The run length of nearest neighbours is defined by the 8 surrounding neighbours.
3. The initial distribution of members of the categories is random.
4. An ordering of moves and what counts as their 'periodicity' is defined. For example: a turn might consist in making all the moves for one category of dissatisfied members whilst the next turn moves dissatisfied members the other category. Alternatively, moves might be made in a top-down, left-right or similar fashion. Or again, the ordering could be random.

As with the first scenario, in different runs of the simulation Schelling varies initial conditions such as the tolerance ratios and whether they are balanced, the numbers of members in the community and the mix of the categories. Each run produces a different unintended configurational state or zonation resulting from the shifting positions of members of the population as they scoot around the board. The properties of the zonation define the extent of the segregation created.

Clearly, this scenario is an extension of the first and its dynamics are very similar. For both scenarios, the ultimate stable state is a racially segregated neighbourhood.

Two-Dimensional Unbounded Space

At this point, Schelling changes the problem entirely. Instead of patterns of zonation, he focuses on the structure of the population under conditions of competition for inclusion. This makes the tolerance schedules even more like the utility functions of economic theory (willingness to tolerate being the analogy of willingness to pay) and the allocation process even more like a market for residence. What Schelling is interested in are the dynamics of demand for the products in such a market; that, is, the final category distributions within 'black' and 'white' residential areas.

The new boundary conditions are:

1. The space (i.e., neighbourhood) is wholly abstract and so the spatial constraints can be entirely dropped. As many as are willing to live there, can live there. The property of "nextness" in the 1- and 2-dimensional sense is an irrelevance as is the question of absolute numbers. Now all that is at issue is the mix or ratios.
2. Both categories are defined by frequency distributions for tolerance. These distributions may not be the same for both categories. In other words, preference orders vary across the members of each category (even if only marginally). There is no upper bound on the ratio of their own category they will tolerate but there is an upper bound on the ratio for the opposing category. That upper bound will be a position along the tolerance function (willingness to tolerate). When someone perceives their neighbourhood has reached that point, they move. It follows that while members of each category could well wish to live in neighbourhoods made up of different ratios including one exclusive to their own category, no member of either category will remain when they are surrounded only by the other category. Incremental shifts in the ratios generate movements in and out of mixed category clusters as individuals perceive the neighbourhood to be more or less attractive. At some point at less than a 99% mix there are no members of a category willing to tolerate the mix of the other (being the only black among whites or vice versa).
3. Everyone knows the precise ratio of the categories in the neighbourhood, but they do not know the tolerance functions of other individuals. This means there can be no 'futures market' for residential access. This is the analogue of the idealisation of perfect information in a free market.

The first and the third of these conditions are clearly effacements. The first sets aside issues of space constraints, transfer frictions and so on. The third sets aside uncertainty and hence the related (social) psychological attributes which otherwise would have to be modelled. In other words, the finite automata have been re-specified.

Under these new boundary conditions, the initial conditions required are more limited. They are:

1. For each run of the simulation, the ratio of the categories for any individual is a re-defined constant (k).
2. The tolerance functions are linear and the upper bound are defined.

Because of the initial conditions, the system laws are amended slightly. Instead of

$$\boldsymbol{Y} = \boldsymbol{X}\boldsymbol{R}_x(\boldsymbol{X})$$
$$\boldsymbol{X} = \boldsymbol{Y}\boldsymbol{R}_y(\boldsymbol{Y})$$

we have

$$\boldsymbol{Y} = a(\boldsymbol{X}(1 - k\boldsymbol{X}))$$
$$\boldsymbol{X} = b((1 - k\boldsymbol{Y}))$$

Plugging defined ratios into these equations produces parabolas which act as a 'pay-off' functions or a 'return on tolerance' curves. Tracing each function on a two-dimensional space produces distributions such as the following:

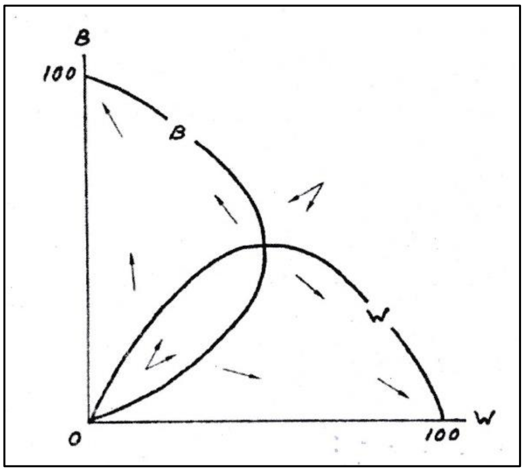


Figure 2 From Schelling (1962, p.492)

The intersection of the parabolas is a zone of 'static viability' whilst outside the intersection are zones of 'dynamic movement'. Note there are points on the mapping where the values are 0:100 and 100:0 indicating states of total segregation. As the parameters of the initial conditions are altered, the shapes of and interactions between the parabolas change. In this scenario, Schelling's analysis is a working through of alterations to such parameters as limits on the size of each population, variations in the tolerance functions, limitation of the ratios of the categories (essentially rationing membership for either category) together with possible perturbations. Each run of the model produces a different mapping of the zone of intersection and hence different patterns of segregation.

Discussion

The absence of direct empirical reference in Schelling's model means we would be justified in describing it as an exercise in pure mathematical sociologising rather than an applied mathematical modelling. It uses social or sociological labels for mathematical objects (e.g., racial mix preferences as functions). Because it is such an exercise, it is relatively straightforward to re-cast the analyses within the well-posed problem framework. The mathematics used is not derived from the sociology but self-standing and the sociology is plugged in to it. The processes of effacement in the definition of boundary conditions and the specification of initial conditions shape the social phenomena so that the mathematics can be applied to them. Of course, this is equally true of our own toy example but with two significant differences. First, the mathematics applied in the toy case was developed to solve problems of the kind to which it was applied. Second, the processes of effacement were 'structure preserving'. What was carried over in the severing were the defining conditions of the vibrating string as experienced pre-analytically. Schelling, on the other hand, re-characterises the pre-analytical problem with each scenario. This is not a point about the 'realism' of his analyses or their 'goodness of fit' to the experience of residential segregation but about the requirement to achieve a degree of conformity to a mathematical framework. It seems that ensuring the well posed character of the mathematical formulation of a sociological problem might come at the possible cost of weakening the preservation of structure within the sociological construal of the social. This is an issue to be held in mind as we turn to further examples.

SEMs and Career Patterns

The universal availability of high-powered personal computers and their statistical programming environments has made the mechanics of structural equation modelling (SEM) trivial—some would say too trivial. Here, for instance, is one knowledgeable observer.

> Modern SEM software makes it possible to crank out results for complex, networked models that are based on only the vaguest of intuition, and then to test these same intuitions with data that does not directly measure them—and to do it all without understanding the statistics. In the wrong hands, this is surely a recipe for bad science. [Westland, 2015, p.5]

We are not going to rummage around in all the issues Westland raises, but concentrate on just one class, those of problem set-up.

SEMs are systems of simultaneous equations describing the relationships between underlying and hence not directly observable (latent) structures. The relationships used are measured covariances among observed variables. They are often accompanied by illustrative, so-called 'path' or 'network', diagrams which order the relationships described. There are numerous flavours of SEM but a core two step technique is common. Measures of covariance are computed across a range of observed 'indicator variables' assumed to bear on a target problem. Principal Components Analysis (PCA) is then applied to that covariance matrix to derive factors or latent variables. From the latent variables so derived, coefficients for the associations are extracted and used to define a model for the (causal) 'structure' of the effects the latent variables have on each other. At their simplest, SEMs contain one more complicating step in their effacement strategy than ABM simulations. The selected indicator variables are severed from the entanglements of real-world experience and given mathematical form. That is step one. They are then subjected to processes of variable reduction and transformation as they are shaped into the model. That is step two.

Although the terminology varies among the different techniques, a common set of concepts are used:

1. A distinction is drawn between an 'inner' structural set of modelled relationships among the latent variables and an 'outer' theorised collection of indicator or measurement variables.
2. The inner/outer structure sustains a distinction between the exogenous and endogenous variables which comprise the total system of relationships. Exogenous variables are assumed (or known to be) statistically independent of the endogenous ones.

3. A decision procedure identifies factors either based *a priori* on what can be called "informed subjectivity" (termed 'reflective latent variables') or from the measures of association across the set ('formative latent variables'). As Westland implies in the quotation above, inexperienced use of reflective variables is a recipe for ill-posed problems.

The distinctions point to what is known in SEMs as the "identification problem". How that problem is resolved is the topic we focus on. Other interesting questions abound in the entrails of the machinery of modelling. We will not address them.

Here is the standard SEM diagram.

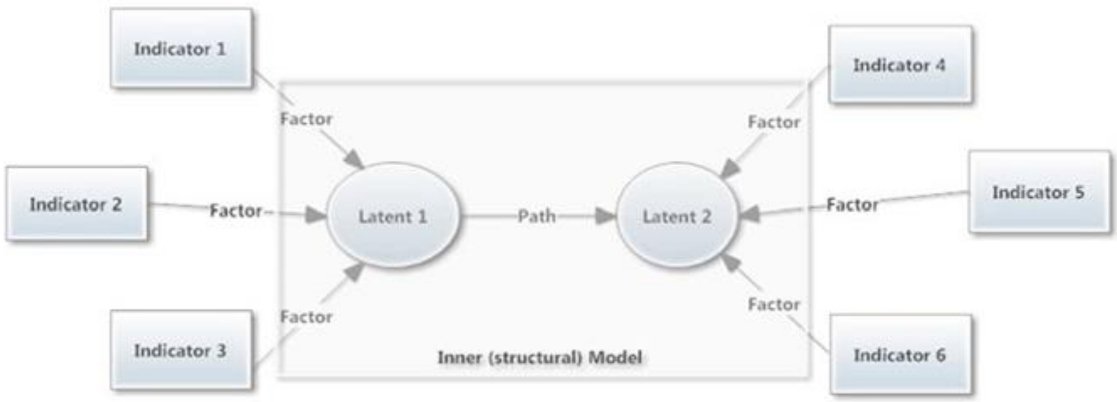


Figure 3 From Westland p.11

The indicators are the measured variables. The latent variables are the outputs from the PCA. The paths in the model are given by this notation:

$$Lat1 = f(Ind1 + Ind2 + Ind3)$$
$$Lat2 = f(Lat1 + Ind4 + Ind5 + Ind6)$$

The diagram brings out the first boundary condition very well. The relationships are acyclic. There are no feedback paths among the latent variables nor from them to the indicators. We can understand why this must be so by looking at what happens if we violate the condition.

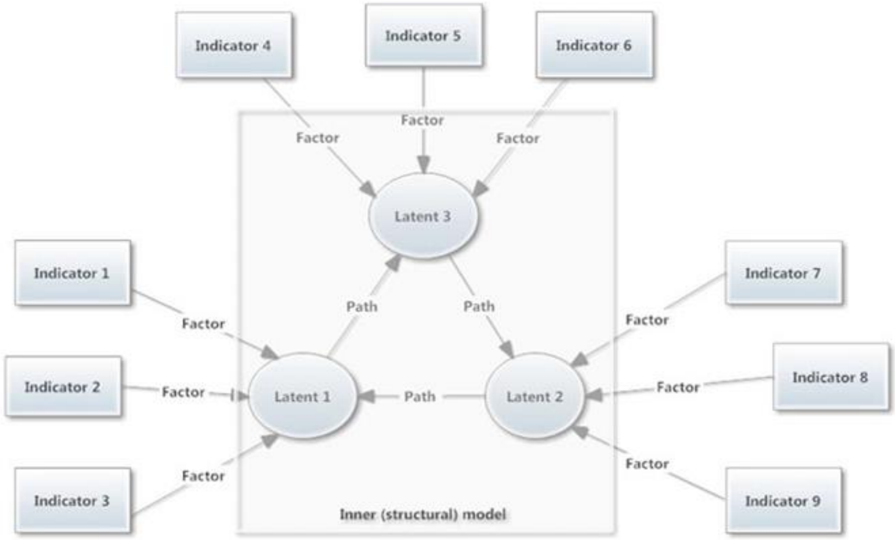


Figure 4 (Westland p 13)

Labelling the latent variables L1.....L3 and following the paths, we see L2 is influenced by a path from L1 via L3. In turn L2 influences L1. Thus L1 = *f*(L2+ L3 + I1 +I2 + I3). We can solve for the indicators because they have a non-recursive relationship to the latents. However, the relationships among the latents create what is known in computing as a 'race condition'. The initial value of the system can only be set by solving the system of latent variables for L1 but to do that we have first to compute L2 and L3. We can only break the circle by stipulating an initial value or using some other 'regularisation' technique. Alternatively, we can re-construct the model (in other words, start again).

We can see the second boundary condition by looking at the equations. Suppose we reduce the system (replacing L with the more familiar X and I with Z to ensure clarity). We end up with familiar simultaneous equations.

$$X_1 = b_1\, Z_1 + u_1$$
$$X_2 = b_2\, Z_2 + \; b_1\, Z_1 \; + u_2$$
$$X_3 = b_3 Z_3 + \; b_2\, Z_2 + \; b_1\, Z_1 \; + u_3$$

The $u_{1...3}$ are the residual or 'error' terms and refer to any unknown variance left after the variables are defined. If any of this variance is generated by one of indicator variables, then the exogenous/endogenous distinction is vitiated because the error term will not be statistically independent of that exogenous variable. This means the relevant indicator will have to be added to the endogenous set of variables. It is also usually assumed that the residual values are uncorrelated with each other (i.e., random). They are i.i.d: independent and identically distributed. The different forms such distributions can take is one of the ways various SEM techniques can be distinguished. The i.i.d condition is Dirichlet since it fixes the (lack of) relation between some parameters within the model (the latents) and the influence from the external environment transmitted through the indicators. Notice this is the only formal condition on the selection of indicators. Finally, and this is similar to the issue of race conditions, we must be able to construct the system of reduced equations so that at least one of them is distinguishable from the others (that is, not composed of exactly the same variables as the others).

To illustrate the setting of the initial conditions, we'll use one of Westland's examples. The data is invented but that has the advantage of facilitating clarity while not being part of its strategy of effacement. Suppose we are interested in the extent to which a person's health influences their success in a professional career. We might hypothesise a model like the one below where we have a set of health factors and a number of factors which influence career directly.

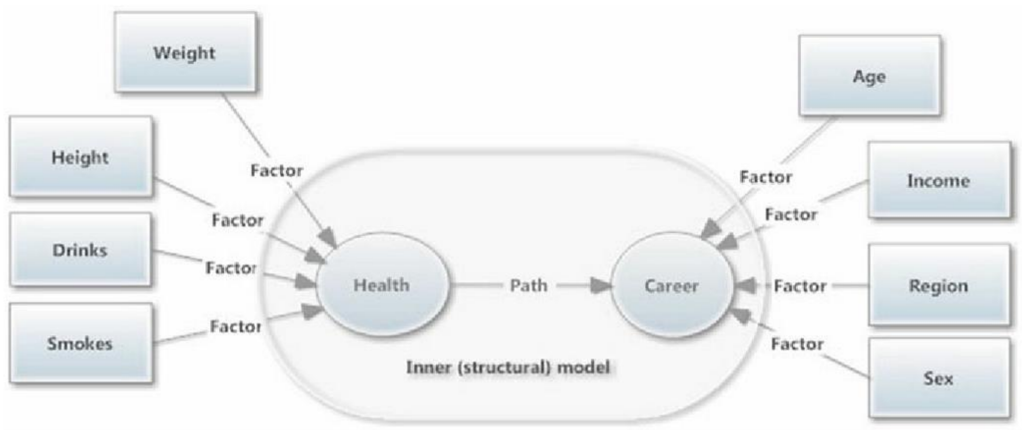


Figure 5 Westland p.68

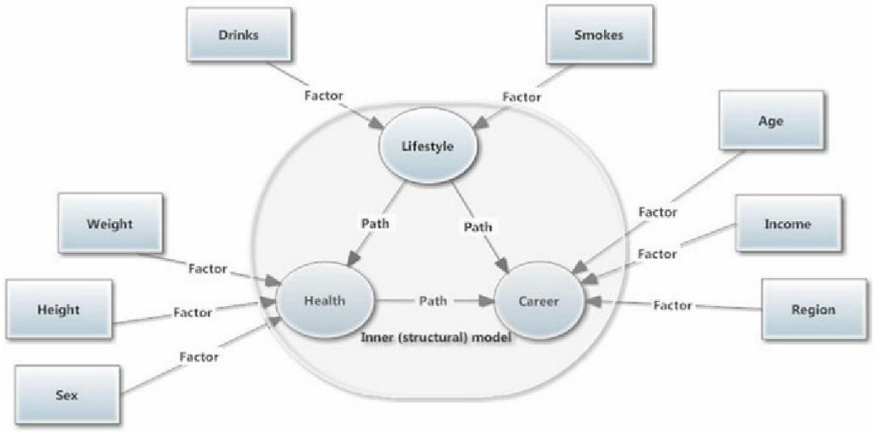
We operationalise our factors into measurable variables (sex being measured as male = 1 and female = 0) and survey a range of career professionals. At first sight, we might think the initial conditions ought to be the measures on the variables obtained by the investigative technique. In fact, the initial conditions for the model are the transformations of these measures constructed by computing pairwise variances across the indicators and deriving factor loadings for them. These factors are reduced variable vectors in the space defined by the indicators and are the principal components or latent variables. Each component reduces the variances of the pairs of measures to a set of single values (a vector of eigenvalues or eigenvector) which are the factor scores.

Thus far the transformation is 'mechanical'. PCA will produce as many eigenvectors as there are variables. So, how do we decide how many we need? The factor cut off is usually placed where the component variances of the vectors sum to 1.0 (known as the 'Kaiser criterion'). This is an informal guideline. Having made the cut, we examine our filtered components to see if they show clusters and/or whether any such high order cluster can stand for our hypothesised variables. Our analysis now depends on whether we have sufficient imagination or insight to discern and name an appropriate cluster to act as a latent variable or alternatively have the good luck to find indicator variables closely associated with hypothesised latent variables. We are looking for relatively higher order factor values (say a loading of 0.10 and above). In Westland's example, the PCA extracts 8 eigenvectors or principal components (L1.......L8) with the majority of the overall variance being carried by just 3. Looking at the composite indicators for the latent variables, we label the three 'Health' and 'Career' as before and the third 'Life Style'. Fixing of the initial conditions, then, is accomplished by mechanical computation, analytic 'nous' and culturally given norms (or rules of thumb).

The system laws are, of course, the simultaneous equations. Their form, but not their content, is given in advance. Each equation is a linear function (f(y) = a +bx). We have as many of them in the system as there are retained latent variables. The latent variables for our model are:

Life Style	-0.707drinks + 0.707sex
Career	0.327age – 0.6income + 0.729region
Health	0.436sex +0.722weight -0.538height

We can now reconstruct our model using the latent variables we started with and adding the third (discovered or 'formative') one.



We have two paths in the system: life style via health to career and life style directly to career. We can write the causal flow as

$$career \sim health + life\ style\ \ [career\ given\ \ life\ style\ and\ health]$$
$$career \sim life\ style\ [career\ given\ life\ style]$$

and calculate the relevant path coefficients (we'll ignore the mechanical technicalities of this step).

Path	Path Coefficient
Life Style ~ Health	-2.6
Health ~ Career	-4.92
Life Style ~ Career	-210.13

DISCUSSION

Unlike our first example, SEMs are not a demonstration of specific theorems or pre-defined functions using sociological terms as designators. Rather, a statistical device is used as a computational machinery to transform observed data of social phenomena into a system of simultaneous equations whose functions are derived from the transformation. A standard format, the path diagram, is used to represent the results. In that sense, it is applied mathematical sociology. As a result, the analysis is far more responsive to the character of the observed data than the Schelling example. However, when we look at how the well-posed problem structure fits the set-up, we can see there is a fair amount of openness in the effacement process, especially in the selection of the initial conditions. Its plausibility structure rests on judgements about the fit of indicator variables to latent variables, assumptions about the i.i.d. status of error terms and informed judgements about the clustering of the factor loadings. These interpretations and judgements must be carried out skilfully and carefully if they are to bolster the plausibility of the 'causal narrative' concerning the relationships among the latent variables.

FQCA AND SOCIAL WELFARE

Qualitative Configurational Analysis (QCA) and its later variants break with mainstream quantitative social science. Rather than analysing variance across large numbers of instances of chosen variables (what are known as 'large Ns), it analyses qualitative and quantitative differences in those variables across a small range of cases. As its proponents put it, QCA offers a formal approach to the comparative analysis of small Ns. For its early developers, one of the key motivations was to find a way to deal with large scale social, political and economic phenomena for which replication under controlled conditions is morally, logically or practically impossible (either because history cannot be re-run, or because we can't use statistical control variables since individual observations are not i.i.d., or because controlled replication would violate social norms). Instead of solving differential and simultaneous equations, the formalism uses Boolean logic formatted as truth tables. As we will see, that description is something of a misnomer. If variable analysis is built around the arithmetical procedures of simultaneous and differential equations, QCA is built around the arithmetic of set theory. QCA's claims to being both quantitative and qualitative have made it attractive as a 'mixed method' spanning both approaches in Sociology. Because it focuses on and responds to the details of a small number of cases, it is often felt to be superior to the "blunt instrument" of variable analysis.

The general steps in analysis are easy to summarise.

1. Using your preferred theory, select a social phenomenon or process of interest (e.g., the conditions for stable democracy, the conditions for developed market economies, the conditions for sound financial decision making in Corporate Banks....).

2. Define the ideal type features of the phenomenon. Choose a title (like those just given) which allows flexibility in determining inclusion in the set of social objects defined by the ideal type.
3. Select instances/cases/ 'data points' which broadly satisfy the properties defined by the ideal type.
4. Define the 'scales' by which these properties will be 'assessed' and carry out the assessment for each case.
5. Translate these assessments into nominal categories (T: F or 0:1) for 'crisp' sets or scalars (in the range 0:1) for 'fuzzy' ones to produce a 'truth table' for the cases in terms of possession of the properties.
6. Configure the patterns of the assessments to produce an ordered list of the set members and set aside those which don't have a good fit to the configurations (the 'remainders').
7. From the surviving cases in the property set, extract the (sufficient and necessary) conditions for the ideal type processes defined above.
8. Use the initial theory to shape a narrative which accounts for the pattern of conditions and configurations for the array of cases in the set.

To bring out how a variant of QCA, *f*QCA (*fuzzy*QCA), can be reconstructed in terms of our heuristic for a well posed and analysed problem, we will walk through an example of the above protocol set out in [Ragin 2000].

THE DRIVERS OF WELFARE PROVISION

Various convergence theses suggest welfare provision (its universality and scope) should be increasingly standardised in the advanced capitalist democracies. The usual explanations rely on a claim about increasing homogenisation of political cultures generating two convergence forces: (a) marketisation associated with competitive pricing driving (b) low-cost provision forcing regression to minimally acceptable common standards. And yet the expected trends are not happening. The failure of the convergence is usually explained by traditionally entrenched 'leftist' political cultures, nationally strong unions and highly articulated democratic structures. All of which militate against single party dominance. Multi-party democracies are typically defined by a policy 'battle for the middle ground' where the provision of social welfare has had important electoral traction.

Ragin registers his world-for-analysis by listing examples of advanced capitalist democratic countries (mostly the ones you might expect). His question is about the varying combinations of conditions associated with the range of welfare provision on view. He identifies the following conditions: degree of generosity in welfare provision, existence of strong left parties, existence of strong unions, presence of a corporatist industrial system and sociocultural homogeneity. Advanced capitalist democratic systems with the conditions he lists define his sociological world. The proposition being tested is that the last four conditions produce, or at least shape, the provision of social welfare.

The definition of the conditions and the specification of their (logical) relationship locates where the analytic cut is being placed and the basis on which it is being made. However, the range of countries included is clearly narrow. Essentially, it is confined to states sharing North American and European ('Western') political cultures. Given we are talking about research carried out in the late 1990s, it might not be surprising China doesn't feature nor, perhaps, Russia. But why are South Korea , South Africa and Mexico excluded? Since one of the arguments being tested concerns the degree of sophistication of democratic structures, it could be argued these three should be included alongside the likes of New Zealand, Ireland and Norway to enable comparison on that particular variable. Ragin acknowledges the problem (p.296) by admitting the selection of countries is based on a stipulation—they must have been continuously democratic since 1945. Poland, Latvia, Lithuania and Estonia for example are thereby ruled out. East Germany is included as it is now part of a reunified Germany. It turns out, then, that the equivalence class of cases is not defined simply by the character of their political economy but also by the length of time this political economy has been in place. Although the restricted nature of the choice is acknowledged, its consequences are not explored.[6]

There is a second issue related to the (analytically necessary) severing of entanglements with the complexities of the real world. Most of the data used in the analysis consist either of unprocessed official statistics or are direct derivations from official statistics. Since one of QCA's central tenets is an acceptance that investigators have 'interests' which 'inform' the data they collect and the way they present them (this is part of their claim to being more 'realistic' than variable analysis), presumably Ragin would also accept state administrations have 'interests' which shape the data they collect and the way they present them. By relying so heavily on official statistics, entangled interests of the kind just mentioned are passed through into the analysis in a (statistically) uncontrolled way. Remembering Hadamard's concerns about the character of the

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[6] For a discussion of reliance on overly homogenous equivalence classes, see [Mahoney 2022].

data fed into the system laws, such pass-through must have implications for how well posed the problem might be.

We now turn to the setting up of the boundary conditions. This is done by allocating the cases to ordinal positions on the properties of the ideal type. We'll look first at the set of countries with "strong left parties". As in all QCA listings, it is assumed that in assembling the set of cases with strong left parties, we are also assembling a list of cases which *do not* have strong left parties. In this example, we are not dealing with crisp sets defined by binary membership (yes, no, T: F). It is explicitly being assumed there is a range of 'relative inclusion' and 'relative exclusion' within the two categories. A country which is very definitely a member of one category will, reciprocally, be very definitely *not* a member of the other. Others will be more ambiguous. Generally, *f*QCA defines the set value of 0.5 as the 'crossover point' between being 'more in than out' and 'more out than in'. This median value is the universal peg for category membership. In other sets of course, should they wish to investigators could nominate different 'peg' values indicating where thresholds for full membership and marginality will be located. Such decisions reflect the investigator's judgement about the width of the value band for subset boundaries (for instance, 'almost fully in' and 'nearly fully out'). Set values, then, are ordinals defined as calibrations of set members in terms of what an idealisation for full membership of the property set defining the type would be. Although Ragin denies this is so, the investigator *chooses* where upper and lower bounds should be placed. Simply picking the highest or lowest GDP score or GINI index to determine set values for full inclusion (1) or exclusion (0) as measures of a country's wealth or poverty is not acceptable to fQCA. The consequent play in the ordination generates potential definitional problems, not the least being the difficulty of comparing different *f*QCA studies of what are ostensibly the same or closely related problems. In addition, it may well be impossible to trace the detail of the reasoning which drives the mappings between the input values derived from domain data and the set of membership values on which the sociological analysis rests.

The causal flow of Ragin's analysis has the standard structure. The independent variables (i.e., the explanans) are the distributions of the states according to the ideal type conditions. The dependent variable (the explanadum) is the value for generosity of their welfare provision. Like SEMs, the direction of flow is to the explanadum from the explanans. The necessary and sufficient conditions for variation in social welfare provision are derived from the distributions of the states across the sets. This means the 'assessment' of a state's values reflecting its positioning along the spectrum of 'generosity' in welfare provision is crucial and is the first place to look when discussing how the boundary and initial conditions are set.

Ragin uses two indices developed in Esping-Andersen's [1990] study of welfare and capitalism to define types of social welfare provision. These indices measure (a) the extent to which benefits are means-tested and (b) the separation of the benefits from labour market participation. Ragin transforms both indices into z-scores with the score for means-testing being inverted (means-testing being regarded as ungenerous). The resulting paired values on the indices are then averaged. Lower and upper thresholds are set (Ragin is coy about where, but by inspection we can infer it is at 0.1 and 0.9). With an average below 0.1, we can assume a case is 'fully out' and above 0.9 we can assume it is 'fully in' the 'generous welfare' type. This thresholding results in USA being 'out' of the set and Norway and Sweden being most definitely in. If the thresholds were set at 0.2 and 0.80 Canada and Australia would be drawn into the not generous set and Denmark into the 'definitely generous' set leaving Belgium being borderline to the latter.

The rationale for the z-score transformations is not clear. For statisticians, z-score transformation is a 'de-meaning' strategy which allows comparisons across very different distributions. The formula is

$$z = \frac{(\mu - x_i)}{\sigma}$$

where μ is the mean and σ the standard deviation. This results in a distribution in the range -1 to +1 around a mean of 0, so Ragin's transformation takes the averaged index values and re-arranges them in the range -1 to +1 with a mean of 0. Z-scores are dimensionless numbers (they do not measure physical, social or psychological 'variables') so what is supposed to be meant by 'averaging' them is not clear. Their value resides in the fact that because the variable they are derived from is assumed to be random, the scores can be treated as a probability density and so allow for comparison between distributions. A well-known statistical theorem states that for a Gaussian (normal) distribution, 68% of the distribution can be found within 1 standard deviation (z score of 1) from the mean and 95% with 1.96 standard deviations (z = 1.96) from the mean. Given this, we can compare the shapes (and hence the character) of very differently scaled distributions. Since QCA rejects "variable analysis", it is not clear why Ragin wants to treat welfare generosity as a probability density function. He could just as easily have scaled each case against the overall range of the variable. A scaling such as

$$\left(\frac{(max - x_i)}{max - min}\right)$$

would have been much more obvious.[7] These scales would also be dimensionless, so the issue of averaging them remains.

If we now look at the calibration of socio-cultural homogeneity measured by indices of (a) religious and (b) ethnic and racial homogeneity, similar concerns arise. For each country, the proportion of the population by sub-group of religious affiliation and ethnic and racial self-identification is calculated. Each measure is squared (to avoid the total equalling 1 which would throw an error later in the calculation). The totals for each index are then summed and z-scored. Again, the z-scores are averaged. Setting aside the z-score oddity, we have a potential confounding issue here. In all the states listed, there is a strong collinearity between religious affiliation and ethnicity/race. Demonstrating these two are i.i.d would be quite a challenge, a fact which might lead us to think we are measuring the same 'latent' cultural variable twice.

So far, we have been raising issues of scoping, characterisation, severing and effacement with regard to just three of the properties or dimensions of Ragin analyses. It is clear the same concerns apply to the calibration of most of the others. One stands out not just for these issues but for the plausibility of the proposed evaluations. It is the condition of having strong unions. Three Nordic countries score highly (Sweden, Denmark and Finland). Italy, Germany and France are more marginal as is the UK. It is hard to understand how these latter four could be co-classified as a marginal subset. Union power in the UK has been in decline since the 1980s. In Germany, such power is institutionalised in socio-political and economic structures through arrangements such as Board membership—thereby making Germany much more like the Nordic countries. Italy and France have strong unions, to be sure, (they have recently been significant movers in the prevention of major state initiated social re-organisations of pensions and retirement age), but their power is exercised outside the political institutions. The term 'union power' seems to be too coarse to really be a useful indicator variable.

[7] What would be lost would be the allocation of ordinals in the -1:0 range for the 'not S' class. But, as indicated above, that could be remedied (fixed up) by choosing a threshold somewhere in the low positive scalars (eg 0.25) to act as the 'peg'.

Table 10.6
Fuzzy Membership Scores for Analysis of Countries with "generous welfare states" (18 AIDCs)

Country	Generous Welfare States	Strong Left Parties	Strong Unions	Corporatist Industrial System	Sociocultural Homogeneity
Australia	.26	.25	.40	.17	.25
Austria	.72	.70	.64	.83	.67
Belgium	.79	.54	.84	.83	.29
Canada	.26	.00	.06	.05	.10
Denmark	.86	.85	.81	.83	.86
Finland	.76	.56	.86	.83	.72
France	.57	.12	.10	.33	.31
Germany	.68	.43	.20	.67	.30
Ireland	.67	.11	.63	.67	.84
Italy	.64	.10	.39	.50	.55
Japan	.52	.00	.04	.33	.95
Netherlands	.69	.33	.17	.83	.27
New Zealand	.56	.40	.54	.17	.15
Norway	.95	.95	.53	.83	.95
Sweden	.98	.98	1.00	.95	.70
Switzerland	.53	.34	.13	.67	.10
United Kingdom	.63	.61	.34	.50	.15
United States	.09	.00	.04	.05	.05

Figure 6 From Fuzzy Set Social Science Ragin 2000 p. 292

Let us now turn to the initial conditions. These are the transformed scores on the property types for the list of selected states. As we just said, having a generous welfare state is the outcome and the property distributions are the set of causal conditions. *f*QCA takes these property distributions and looks for alignments with the outcome property. If the condition score for a case is equal to or greater than the outcome, the outcome is taken to be a subset of the condition and hence necessary for the outcome property for this case. The overall 'necessity' score for the condition is the proportion of the scores where this inference holds. A filter is then applied to the calculated proportions. This is a "probabilistic test", as Ragin (p.295) calls it, which uses a threshold of 0.8 (set by Ragin) to which he allocates a significance level of p = 0.05. In other words, in variable analysis-speak, if the proportion is above 0.8, there is a 95% chance the result is not random and we can accept the proposition (null hypothesis) that the condition is necessary. As one of its boundary conditions, QCA insists that the identification of a set of countries with the condition creates its inverse, the set without the condition. The analysis tests both sets, meaning there are a possible 8 threshold proportions having a significance of p = 0.05 and above. Another way to describe this is to imagine the total set of countries mapped onto a 0:1 Cartesian space defined on the x axis by the relevant condition (say, strong unions) and on the y by the outcome (welfare generosity). What fQCA is looking for are those outcome values above 0.8 located in the lower right-hand triangle of the space where the x value equals or exceeds the y. Ragin talks of distributions like this as "corners".

None of the conditions satisfy this test and so Ragin concludes there are no necessary conditions. He explains this result by pointing to the lack of diversity in the collection of countries selected. They are all 'Western' AIDCs (a point we made right at the beginning). One feature usually associated with being such an AIDC is some level of welfare provision.

The method for identifying sufficient conditions is similar but more convoluted. The conditions are tested singly and in combination to see if they are subsets of the outcome. This is done by checking whether their scores on the property are equal to or less than the outcome (this time we are looking in the upper left triangle of the property space). If the property is a subset of the outcome, it shows it is one of possible ways the outcome might be generated. Once again both positive and negative conditions are tested. In the social welfare example, the combinatorics give us 3^4-1 (80) tests to run. The result is a list of approximately 40 instances of singleton properties or combinations which satisfy the 'test'. To simplify the analysis, *f*QCA looks for common factors in the combinations. If those common factors are themselves in the set, then their combinations are deleted because they are held to be logically redundant. The principle is: if condition A is sufficient, the combination of A with any other condition will also be sufficient. This is the application of sociology avoidance. Using the 'rule' allows Ragin to avoid the complications of providing sociological reasons why particular combination should be deleted. Truncating the properties in this way leaves the following as sufficient conditions for generous welfare provision: strong left parties; the intersection of strong unions and sociocultural homogeneity; the intersection of {the intersection of strong unions and corporatist industrial system} and absence of sociocultural homogeneity.

With all this in place, the completed analysis consists in identifying and describing patterns across the cases (which are close to which in regard of what conditions) and providing a

Table 10.7
Fuzzy Membership of Countries in Causal Expressions Passing Sufficiency

Country	Strong Left Parties	Strong Unions• Corporatist• ~Homogeneous	Strong Unions• Homogeneous	Corporatist• Homogeneous	Maximum of Causal Expression	Generous Welfare States
Australia	.25	.17	.25	.17	.25	.26
Austria	.70	.33	.64	.67	.70	.72
Belgium	.54	.71	.29	.29	.71	.79
Canada	.00	.05	.06	.05	.06	.26
Denmark	.85	.14	.81	.83	.85	.86
Finland	.56	.28	.72	.72	.72	.76
France	.12	.10	.10	.31	.31	.57
Germany	.43	.20	.20	.30	.43	.68
Ireland	.11	.16	.63	.67	.67	.67
Italy	.10	.39	.39	.50	.50	.64
Japan	.00	.04	.04	.33	.33	.52
Netherlands	.33	.17	.17	.27	.33	.69
New Zealand	.40	.17	.15	.15	.40	.56
Norway	.95	.05	.53	.83	.95	.95
Sweden	.98	.30	.70	.70	.98	.98

Figure 7 Ragin 2000 p. 298

socio-political historical narrative for why this might be the case. But this only takes us back to where the analysis started, namely the presumed similarities between clusters of the countries chosen; the Scandinavian countries; EU countries and Australia, Canada and USA. Japan and Switzerland remain the outliers.

DISCUSSION

We have provided far more detail on *f*QCA than the other examples we have looked at with good reason. Compared to them, and certainly compared to our idealised toy example, problem set-up and delineation of boundary and initial conditions involve a great deal more numerical 'processing' and 'transformation'. This is not simply because of the complications of the cases and the necessity of calibration. It is a direct result of an inability to ensure a clean severing at the problem's boundary.[8] The confounding of definitions, the informal basis for determining membership values, the imprecise characterisation of the objects in the property spaces as well as the odd ways the vectors of 'causal' conditions are computed, all contribute to a weakening of the account's plausibility structure. The system laws *f*QCA uses are contained in the arithmetic of fuzzy logic used to create and analyse the final lists of conditions given in the table above. We have said nothing about how well Ragin's usage conforms to the usual requirements of that mode of analysis. Since fuzzy logic is a well-defined domain in Mathematics, we assume the difficulties we are presented with are produced by the way it is used not the mathematics itself.

Section 3. Conclusion

The objective of this discussion was to offer a method for reading sociological reports which would allow an understanding of the extent to which the structure of the sociological rendering preserves the structure of the social objects it is attempting to describe. Our shorthand for this condition was a report's "plausibility". The method we suggested focuses on three key elements: the posing of the analytic problem, the procedures for effacement of non-relevant detail and the analytic procedures deployed. We have applied the method to a small number of different types of mathematical model building. Using the method to guide our reading, we identified several potential difficulties which those who apply mathematical methods within sociological analysis will have to overcome if their reports are to be structure preserving. Most, but not all, are consequences of the dependence on arithmetic of the mathematics being applied and its grounding in $\mathbb{R}^N$, the real number system. These problems lie both in the degree to which the problems can be 'well-posed' and the extent to

[8] This may sound harsh, but it is not as harsh as some well-informed commentators have been about Ragin's method. See [Lieberson 2004].

which a 'clean cut' can be achieved between the 'world-for-analysis' and 'the social world-for-investigation'.

The advantage of the heuristic we offer is that it focuses attention on key aspects of a report's construction. These aspects underpin its plausibility as a sociological account of the social phenomena being examined. As such, we think it might be a helpful addition to a student's analytic armoury when first encountering sociological reports for themselves (other than when summarised in texts, say). That is the spirit in which we recommend it. If it has further values (as we think it may well have), hopefully they will be demonstrated in the companion pieces included within this Part and later in this collection.

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2

Metaphysics and Arithmetic

Introduction

This short essay is an adjunct to *Reading Sociologically* and should be read alongside it. In that essay, our objective was to introduce a heuristic *aide memoire* for reading sociological reports and then illustrate its use through some examples. In doing so, we pointed to various issues and challenges which writers of research reports face in managing the necessary transit across the abstraction gap between the phenomenology of the social data collected and the sociological phenomena research reports describe. Central to our discussion was the task of turning description of social objects gathered as data into descriptions of sociological objects cast in mathematical terms. At various points, we alluded to an allied difficulty which might also be present. This was the robustness of assuming an isomorphism between the logical grammars of our commonsense concepts of the social and that of the mathematics of the real number system $\mathbb{R}^N$ which is the basis of the arithmetic methods used in the analyses. Examining this isomorphism in the cases we were discussing would have taken us a long way off our intended path and further complicated what was already turning out to be a sufficiently complicated story.

This essay returns to that question. It looks beyond the immediate analysis of well posed problems to offer an initial exploration of the assumptions underlying the isomorphism. In doing so, it introduces a number of considerations which are relevant not just to mathematical sociology and the disciplinary metaphysics of the social sciences in general. These might be summarised in a blunt question: Why are we so sure the metaphysics of the social maps onto the structure of the Real Number system (or indeed either of its siblings, the Cardinal and Ordinal systems)?

Section 1. KF Structures

A common account of the difficulties in applying mathematics in Sociology rests on the potential for tension (or worse) between satisfying the mathematical requirements (for well-posed problems or whatever) and the sociological requirements for capturing the sociality of a phenomenon. But that simply describes the issue, it does not explicate it. To understand what it is about the mathematics and the sociology which generate the problem, we need to look at why applying mathematics works as well as it does in Physics[1] and why it so often doesn't in Sociology. Resorting to an obvious explanation by invoking a distinction between conceptual and empirical disciplines won't serve since both Physics and Sociology are empirical. We must look elsewhere and the most natural place to start is the conceptual structures (metaphysics) of the worlds organised by their descriptions and the logical grammars of the concepts deployed in them. Since the fit between the mathematics used and the phenomena it studies seems to be so tight in Physics, we build the 'base case' from that.

Our guide in this exercise is Penelope Maddy and her analyses of the metaphysics of Arithmetic, Logic and Physics. The approach Maddy adopts is what she calls "second philosophy".[2] It starts with what we ordinarily know about the world and, using the basic method of trial and error, works back through what various kinds of mathematics might have to say about that world. She partitions Mathematics into 'pure' or 'standard' and 'applied' forms. We will concentrate on her analysis of Arithmetic since it is at the base of most of forms of mathematics which Sociology has borrowed from the natural sciences. By 'applied' Maddy intends something like 'has a direct or untranslated application to the phenomena of the commonsense world'.

Maddy's narrative consists of commentary on the thinking of an idealised enquirer examining some philosophical topic or question. The idealisation, though, is post-Quinean. It is not the enquirer's ambition to argue from appearances to the apodicities underpinning grounded knowledge. Rather she starts from the assumption any well founded science tells us what there is in its domain and the task is to work out the grounds of such knowledge from what that science says

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1 The phrase "as well" is open to an awful lot of debate which we will not enter into here. For our purposes, no matter how well or badly philosophers (and some physicists) think it works, it certainly appears to be a better fit for reasoning in Physics than it is for reasoning in Sociology.

2 Maddy provides innumerable sketches of this approach [see Maddy 2000; 2007; 2011]. Whilst they are more or less the same, they do differ slightly in their details, a feature which has given her critics considerable ammunition. [Santos 2016]

and does. As we have said, her approach is one of probing, questioning and trial and error together with assaying the consistency and security of the answers she arrives at. As Maddy puts it:

> Very roughly, the thought is that the ground of a stretch of discourse is something extra-linguistic that guides and constrains what counts as proper or correct in that discourse, something extra-linguistic to which the discourse is responsive and responsible. [Maddy 2014, p. 223]

The second philosopher begins with the everyday world of material objects. A world in which there are shoes and ships, sealing wax, cabbages, kings and a whole lot more. She also starts with our science of that macro-world.[3] This world, she confirms, is one in which there are objects with properties. These objects stand in relations. This does not mean everything has the same properties or that all the distinctions between objects, properties and relations can be nailed down. She calls the underlying logic of these objects, properties and relations a 'KF-structure' because it articulates the conceptual integration of transcendental idealism, empirical investigation and formal logic laid down by Kant and Frege. The nub of her argument is a correspondence thesis. The metaphysics underpinning our common sense, scientific and mathematical understandings of the physical world are members of the set 'KF metaphysical structures'. This co-membership means scientific, mathematical and common-sense ways of understanding and describing the objects of the natural world correspond or stand in a mapping relationship to one another regarding the set defining properties.

Maddy's argumentational premise is that the world must have a KF-structure. As a result, our primitive cognitive mechanisms have evolved (or whatever developmental theory you want) to "detect and represent" objects which have these features [Maddy 2007 p 226]. It follows humans are cognitively configured (that is, think the way they do) because they live in such a world. Since science has its foundations in natural observation and engagement with the world, its descriptions evidence that world's KF-structure. Maddy is well aware this argument is an inference over non-philosophical evidence but that is how second philosophy operates. She is also well aware it would not satisfy the radical sceptic, but she dismisses radical scepticism on two grounds. First, she thinks scepticism is an exercise in nihilistic premise denial which, given her 'set-up conditions', she

[3] An important qualification. As we have already hinted, once you get beyond the macro-world, things get decidedly tricky and ugly. See Schapiro and Reeder [Shapiro 2009] for a commentary on Maddy and an examination of some of the implications of Quantum Physics for her argument.

cannot engage with. And anyway, *nothing* she could say would satisfy what scepticism is asking for.

With the KF structure of the world in place, Maddy turns to the other half of the practice underpinning science's success, the mathematical structure of Arithmetic. She finds its logic also shares a KF structure. Its operations look to be more or less complicated exercises using the number system to "track" (an important verb for Maddy) the manipulation of variously sized bundles of things. The trouble is the Real Number system ($\mathbb{R}^N$), for which Arithmetic is the operational mathematics, has one obviously problematic feature: infinite extensibility. There is no largest or smallest number. We are faced with what has been called the 'Benacerrafian challenge' [Benacerraff 1978]. How do we, a finite being operating with finite objects, ground the realism of the 'transfinite'? How do we justify the realism of adding objects together so they aggregate beyond the finite number of objects there are in our finite world? The general case of this recursion Maddy calls the '.....' problem. In a way, it is the nub of what Wittgenstein called the problem of "knowing how to go on".

Once again, Maddy doesn't offer philosophical arguments but psychological ones; the demonstration in Psychology of ontogenic mechanisms (or primitive number systems) for tracking small groups (less than 4) and large groups (more than 4). It is the first of these which allows us to 'track' individuals and is implicated in basic arithmetic. The second, called the "analog system", allows us to track larger groups but is somewhat imprecise. Studies have shown the same systems can be found among animals. Maddy takes this to mean there must be an evolutionary origin. But what humans have and animals lack is an elaborated language. Recursion is associated with one pertinent aspect of elaborated languages, the sequence of number words. Psychology has shown children learn recursion in learning number words. Here is Maddy's summary.

> .....it seems fair to say that by the time we master something like the decimal system, we have come to think that despite the limits of paper, pencil and human breath, there is always, at least in principle, another numeral.......... Much as our primitive cognitive architecture, designed to detect KF-structure, produces our firm conviction in simple cases of rudimentary logic, our human language-learning device produces a comparably unwavering confidence in this potentially infinite pattern. [2014 p. 234].

We don't grasp infinity and then develop competence in the number system. We develop competence in the number system and eventually have what Jessica Carter [Carter 2019] calls an "epiphany" with regard to the possibility of infinity. It is this 'intuition' which gradually morphs into the mathematicians' and philosophers' theorems about infinite sequence and is formalised in Peano's axioms for standard (infinitary as opposed to finitary) arithmetic.[4]

So here we have the story. Basic (finitary and applied) arithmetic and standard arithmetic are based upon (a) the common logic of KF-structures; and (b) the development of recursion as a linguistic competence. Standard (infinitary) arithmetic is the formal idealisation of the finitary form. Its idealisation is no more than abstract modelling which introduces necessary falsehoods, approximations and torsions. These can be justified only if they are benign and result in the provision of "mathematical depth" accessed and assessed in the usual ways that mathematicians do. In sum, for arithmetic

> ....firm groundings run throughout, from the world's KF-structures, to the recursive element of the language-learning devise, to the more esoteric facts of mathematical depth.[2014 p. 248]

As a form of mathematics, then, finitary applied arithmetic is secure. But what about the use science puts it to? Here Maddy does have some qualifications.

> When we represent a cannon ball as a perfect sphere, the lengths, times, angles and forces involved as real numbers, the local surface of the earth as flat, and so on, in order to determine where a given ball, fired with a given force, will land, we have a fairly good idea of at least some of our departures from literal truth and why they are admissible. When we represent spacetime as a continuous manifold, we aren't entirely sure whether or not this constitutes a literal truth, though our well-informed hunch is that even if it is an idealization, it's a good one–much as Euclidean geometry is a good approximation to the truth in most ordinary cases. But the fact remains that the mathematics has been peeled away from the science; the actual claim the scientist makes about the world is that it is probably, at least approximately, similar in structure to the mathematical model in certain respects, and that the idealizations involved are beneficial and benign for the purposes at hand.......
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> ...(O)ur best mathematical accounts of physical phenomena aren't the literal truths Newton took them for, but free-standing abstract models that resemble the world in ways that are complex and sometimes not fully understood. [2011, pp26-7]

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4 In a discussion of structural theories of Mathematics, Brice Halimi [Halimi 2019], offers a fascinating example. Initial students of the theory of permutations usually are happy to accept that $\binom{abc}{bac}$ and $\binom{abc}{cba}$ are different permutations but the epiphany to see the same is true of $\binom{123}{213}$ and $\binom{123}{321}$ wholly eludes them. Clearly there is a lot we don't know about the psychology of numbers as representations.

The "peeling away" of the mathematics from the science opens a potential gap in the relationships. The mapping is now not correspondence but approximation. As a result, idealising across the gap must at least be non-distorting and should be structure preserving.

Section 2. The Elements of KF Structures

Any structure is an organisation of elements. KF-structures are no different. Their elements are objects, properties and relations, dependencies and their determinacy. We'll take a brief look at each in turn.

Objects

The material world consists of numerous (perhaps infinitely numerous) individually identifiable things. The paradigm for what is meant by 'thing' are the constituents of the macro-world in which we live. These are the medium sized physical objects around us such as trees, buses, jogging shoes, salt crystals and lumps of rock. These things have a physical unity, boundaries, positions in space and relative persistence over time. Each can be picked out from a collection of other things of the same type and from its surroundings. Most importantly, they can be counted and, if we have the appropriate methods, some of their properties can be scaled and measured. Many of their features fit the real number system and manipulations of those features by methods derived for the Real Number system reveal more.

As we move away from the paradigm ontological type, this characterisation begins to break down. In the domains of particle physics and the lower plants, objects do not necessarily comport themselves in the ways we expect. Particles such as photons don't seem to occupy a predictable, defined position and can appear to be smeared out in space. Under some circumstances slime moulds behave rather like plants and under others more like yeasts. The analysis of particles has led some to the conclusion that other forms of mathematics to those based in Arithmetic are needed for their description, whilst consideration of the lower plants and animals often results in ontological re-calibration and engineering. However, these kinds of adjustments do not lead anyone to propose the re-consideration of the paradigm cases.

Properties and Relations

One of the neighbours has a black and white dog which yaps incessantly. Another's has a morose look and is almost silent. Buses accommodate different numbers of passengers and are often painted different colours. Some properties are definitive of membership of a category. The black and white dog is male because it has X and Y chromosomes. Sharing a single party wall defines a house as 'semi-detached'. Other properties while not definitive are intrinsic. Being rigid doesn't uniquely identify levers. Lots of other things are rigid too. But a rubber lever is not much use to anyone. Yet again, some properties are just contingent. It just so happens the blackbird's nest is in the rhododendron by the gate. It could have nested somewhere else.

Some of the properties an object has can affect other objects. Put your hand into a boiling saucepan and you will be scalded. Roll a marble down a slope and it will accelerate. These relationships can be direct as in the cases cited or indirect. Changing day length and increases in ambient temperature induce bio-chemical changes in frogs which in turn cause them to seek to spawn. Leaving the kitchen door open when baking bread causes the central heating thermostat in the hall to switch off as heat is transferred out of the kitchen. As a result, the bedrooms cool down. Interestingly, these indirect relationships are all mediated by local direct relationships (also known as 'mechanisms') thereby preserving the KF-structure of the relationships. As ever, move away from the domains of barking dogs, boiling saucepans and baking bread and the notions of distinctive properties and determinate relations become more and more unglued. Here we find talk of light waves/particles choosing their route through a slit screen or of causes happening after their effects have been manifest.

Dependence

As already indicated, one way in which objects are related is through the dependence of properties. For the kettle to boil, the switch on the wall has to be on. The carrots in the veg. patch won't survive unless rabbits from the neighbouring fields are kept out. Ivy looks a bit like her mother because she shares her mother's genetic makeup. Some of these relationships are binary. Left in the dark, green leaved plants die. Other dependences, though, are more like preferences. The foxgloves will reluctantly grow in the flower beds but spread like wildfire in the gravel pathways. Some of these dependences are visible and well understood. Eggs break (usually) when you drop them on the floor. Some are not, though if needed we have effective ways of figuring out what will happen (for example, the trial and error of tests and experiments). There again, others do escape understanding altogether. Why, for instance, does any loose length of string or any collection of clothes hangers get tangled up when you aren't looking at them?

Indeterminacy

We have talked of objects being bounded and denumerable and for many, perhaps most, this is so. But for some it is not, or on occasion is not. Looking out of the window, the sky is grey but not uniformly so. Lighter and darker patches merge but where they merge is hard if not impossible to delineate. The field edges are defined by 'rough' grasses where the standard rye grass savanna has not been sown. Yet the boundary between the field edge and the 'improved' pasture is not a line but an imperceptible merging. While no-one has an official (or even unofficial) definition of the number of stones that make a way-marking cairn, when out on the hills we can usually tell the difference between a random pile of stones and a (constructed) cairn. 'More' and 'less', 'not enough' and 'sufficient' are essential concepts for us. But just how short does a piece of string have to be before it won't tangle?

Our ability to deal with indeterminacy is one of the commonsense ways we cope with the findings of the natural sciences. On one classic description, the desk under the keyboard is a solid, stable object. It was here yesterday and, as long as nothing untoward occurs, it will be here tomorrow much as it is now, covered in books, papers and other bits and pieces. At the same time, the table is just a lattice of forces and only has the appearance of solidity. Even the appearance of permanence is misleading. The desk is actually entropic. Given enough time, the physical structure will become disorganised. We can treat each of these descriptions as 'true' without worrying about at just what point we should switch from the adoption of one 'justified true belief' to another. Since, the home range of our concepts is common sense, we adopt the reasonable position of giving common sense usage a working level of primeordiality. This does not deny vagueness but embraces it. Vagueness is a sometimes feature of the world, a feature we don't have to give up on unless other ways of dealing with vague phenomena show benefit for managing our world. After all, does it help when you are looking for the pen you just put down to think of the table as a lattice of forces or imperceptibly sliding into a chaotic force field?

This is not a campaign for wholesale conversion to Maddy's proposed metaphysics. All we want to do is use her arguments to open up a novel line of discussion with regard to mathematical sociology. Maddy's argument turns on the confirmation by science that natural phenomena have KF structures of the kind common sense attributes to them. These structures are shared with Arithmetic. Our ordinary concepts about the natural world (and hence those of non-Quantum science) are KF constructions and so can be represented by appropriate methods of finitary arithmetic. Arithmetical procedures work in science because both the mathematics used and science share a common logic. Common sense understanding of the natural world also has that logic. The explanations of mathematised science convince us because our two ways of thinking about the natural world are alike.

This conclusion leads to the obvious question. Can the same thing be said for thinking about the social world?

Section 3. KF Structures and the Social

What Maddy does is align the logical grammars of three distinct conceptual structures; common sense understanding of the world, pre-Quantum natural science (Physics) and basic arithmetic. Her claim is all three deploy KF-structures. This is an unsurprising but contingent consequence of the facts that:

1. Arithmetic and Physics are predicated in our common sense understanding of the world. They start with that but rapidly move beyond it.
2. Arithmetic and Physics evolved together as the applied mathematics of the natural world.

The 'ground' of all three forms of metaphysics is what some have called a 'mechanical' view of the world. The ultimate parts/units composing the world are 'unitary', 'solid', 'denumerable' 'objects' standing in 'causal' relations. All objects are either ultimate unitary objects of this kind (call them particles if you want) or made up from them. The KF-structure is the logic of these objects.[5] The question we are pursuing now is whether some or all our commonsense metaphysics of the social world *also* has a KF-structure. If it does, then importing of basic and standard arithmetic as part of the mathematical sociology may make perfect sense. If it doesn't, that move might amount to little more than a basic category mistake and lead to some of the difficulties we discuss in the companion essays to this. Of course, it could be some parts of the social do conform and some parts don't, with all the complications which would follow from that!

We came to our question as a result of looking at how well-posed investigative problems in Sociology might be. It is natural, then, to look at the process of constituting problems for investigation and analysis to see we can find any pointers to an answer. One location worth examining is what is known in methodology texts as "operationalisation" or what we called the use

[5] Whether there are 'looping' relationships between common sense understandings and the understandings of science as that science is popularised/bowdlerised is not germane for us right now. The basic grounding goes one way only.

of "analytical procedures": the translation of abstractions such as the sociological concepts of preference, healthy living and socially generous political cultures into systematically measurable indicator variables. From what we have said so far, it will be important to see if these indicator variables display reasonable adherence to K-F structures whilst maintaining their conceptual character as representations of the social objects for which they stand. If they do, mapping mathematical procedures onto them is structure preserving.[6]

This immediately brings us face to face with two important issues which we will have find ways to manage. The first is the fact our common sense and scientific categorisations of the natural world are of no importance to the objects we so categorise. Birds and bees do not care if we put them in the same or different ontological categories. As far as we know, not even our closest primate cousins, the Benobos, worry about what we think of their mental life or our assumption they act solely on the basis of biological 'drives'. The Ash trees in the garden are indifferent to our theories of how they measure time marked by the seasonal round. On the other hand, when Sociology talks about social categories and processes, sometimes what we say definitely does matter to those whose categories and processes they are.[7] The social worlds containing such phenomena are imbued with meaning for those who live in them. And these meanings count. What role such "meanings" should play in relating our sociological descriptions and their "understanding" of their ways of life is far from clear. Even after 150 years, we still don't know what to say about the sociological import of 'the actor's point of view', except that it seems to be important that it has one and it is important not to misrepresent it. At various times, we have talked about overcoming this challenge as "faithfulness to the phenomenon" where the phenomenon in question is the lived experience of those undertaking the courses of action under investigation. As we will see in Part II, questions of representation and misrepresentation of social actors' experience of the social are sometimes raised by the substitution of sociological for social characterisations. Closely related to these concerns are those relating to the inclusion of the observer/analyst within the analytical frame of reference. This is more than simply the reflexive extension to the practices of Sociology of ethnomethodological interest in practical reasoning. It involves the constitution of the

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[6] It is important to note that this is not a question of the logical validity of any imputed relationships between the indicator variables and the phenomenon. That is a very relevant question for the robustness of the explanations being given but it is not the question we are asking.

[7] The 'science wars' in and over SSST are more than sufficient evidence for this.

observer's objective analytic point of view in relation to the actor's subjective interpretive point of view. How do we frame the constructs of the analysis to carry both?

The second issue is how to treat what has recently become the highly popular sport of 'social ontology'.[8] Social ontology is a kind of sociological exercise. The home ranges of the categories of objects chosen for analysis are sociological and the accounts given of them (their 'grounding and 'anchoring' in Epstein's [Epstein 2015] case) are sociological.[9] This means what Epstein and others are up to should be labelled "sociological ontology" or "sociological metaphysics" both of which are perfectly proper things to be doing but they are not what we are after.[10] Mike Lynch [Lynch 2013] has called what we have in mind "ontography" , one justification for which was beautifully captured in a famous remark by John Austin.

> ...our common stock of words embodies all the distinctions men have thought worth drawing, and the connexions they have found worth making, in the lifetimes of many generations: these surely are likely to be more numerous, more sound, since they have stood up to the long test of the survival of the fittest, and more subtle, at least in all ordinary and reasonably practical matters, than any you or I are likely to think up in our arm-chairs of an afternoon—the most favoured alternative method. [Austin 1961] p.182]

What follows is nothing like a fully-fledged exercise in the ontography of the sociological mathematics of the ordinary common sense social world, nor even a sketch of what it might be. It is more of a snatch, a glimpse, a doodle for what could be involved; a highly preliminary exercise carried out using as its stalking horse some of the examples we have already discussed. But weak as it is, it suffices for the purposes we have for it. It raises questions about the goodness of fit (relative "inexactness" in Stephen Körner's terms) between our common-sense concepts and the formulations of them in mathematical sociology.

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8 As Brian Epstein [Epstein 2016] admits, the term is a misnomer. It should be 'social metaphysics'.

9 For an illustration of this, look no further than Epstein's refutation of 'ontological individualism' by reprising Merton's famous run-on-the-bank thought experiment using the imagined insolvency of Starbucks.

10 This throws up a separate and but very important corollary to our argument which we do not have space to examine. In Philosophy, conventional metaphysics is the conceptual analysis of how we think about the natural and social worlds. It mostly uses the results of science as its data and reference points. This is justified on the basis of the claim to realism in science (which as we have just seen, in Maddy's eyes rests on shared logical structures). Social ontology is also a 'meta' exercise, this time on Sociology. If there is a divergence of logical structures between Sociology and common sense (which is the thesis we are examining), what does that do to any claim Sociology might make to be structure preserving and hence to the viability of social ontology?

NEIGHBOURHOODS AND PREFERENCES

The two notions which Schelling has to operationalise are 'residential preference' as in 'the character of where I want to live' and 'social or community neighbourhood' as in 'the place where I actually live'. The way he does this is to define neighbourhood as a 'field' of 8 nearest neighbours. We use the notion of 'field' to cover the linear and 2-dimensional spaces he locates neighbourhoods in. Preference is defined as a binary value (+/-) determined by some stipulated tipping point in the (racial) composition of the field.[11]

At first blush it would seem we are on sure ground. After all, neighbourhoods are locales and neighbours live in them. Spatial co-ordinates and counts are paradigm applications of the real number system.[12] Andrew and Lisa live next door and they are our neighbours. Robert and Alison live over the road and they are our neighbours too. But what about Simon and Julie who live a kilometre away? Are they also our neighbours? True there are just two houses between us but calling them neighbours in the same sense Andrew and Alison are doesn't quite work. The reason, of course, has to do with the cluster nature of our concept of neighbour. Sometimes it is used to mean just those who live 'next-door' (to some reasonable numerical value of "next") and sometimes it is used to mean some numerically vague or indeterminate 'living in the general vicinity'. Our village and the next village are indeed neighbouring villages but we don't think of the people living in Warslow as neighbours in anything like the sense Robert and Alison (or even Simon and Julie) might be.

But it gets even trickier. Draw a line around the village. Is the delimited area "our neighbourhood"? Well, for some purposes it is (after all we share a 'neighbourhood shop', a village school and church and other 'neighbourhood services') but for some it isn't. One of the things that plays into this distinction is the expectations we might have of people who live in the neighbourhood. Being 'neighbourly' does not mean greeting everyone we meet on the street and passing the time of day with them. It does not mean feeling obliged to help everyone with clearing snow from the pavement outside their house. For some we do and others we don't. These expectations are not ordered by proximity but are bound up with the multitude of other ties we might have with them. For example, we share friends, they are on the school committee, they collect leaves and petals from our garden for the Well Dressing. Sociologists have bundled all

[11] Most critiques of Schelling focus on the fact the segregation is about race. That is true but not our interest.

[12] Perhaps we ought to add here that the KF-structuring of the basic categories of space and time has been strenuously challenged from Bergson onwards. See [Bergson 2001]

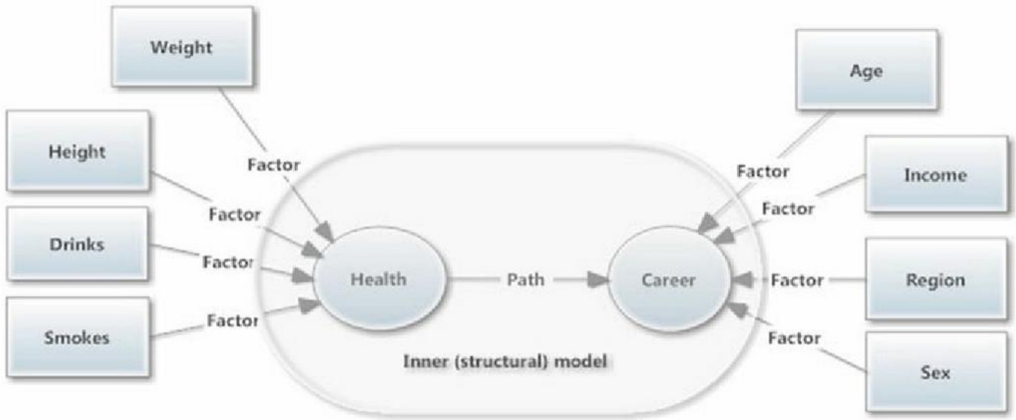
these ways of talking about the people who live near us into the concept of "role". The role "neighbour" is a plastic one. We recognise it, use it, understand it without needing an abstract, formalised descriptive rule for it. Of course, sometimes that usage has an approximate KF in structure as when we talk about the people "over the road" and "next door" So does the designation "neighbouring village". But not all the connotations captured by the sociological role of 'neighbour' are fixed, individuated and denumerable in the same way. And this matters because how we think about the general character of our neighbourhood is a manifold. It depends on what we talking about and to whom we are talking. And that has implications for what might be said about our preferences for living there.

Preference also seems on safe ground. After all, preferences are orderings. You can list your favourite beers and which supermarket in the nearby town you'd rather use. You can give advice on the easiest way to find your house and what determines the choice of routes. But that word 'choice' hides the issues with treating a preference as a fixed, distinct, denumerable 'thing'. You might say the easiest route is to go one way but if traffic is thick, then go another. When traffic is thick, what was the second or third choice is *now* your first preference. Route choosing preferences are circumstance driven. Then think about choosing wine in a restaurant. Someone's preferences might well be fixed: white with fish and only New Zealand. But others might well be open. What do they *feel* like drinking? What do their dinner companions *like*? What are they eating? Choosing now becomes a matter of balancing, weighing, interpreting over many different considerations. To be sure a choice is made, but to say that choice is the *preference* is to stretch that term beyond its limits. Sometimes preferences are KF in form and sometimes they are not just more nuanced but almost the inverse of KF-structures. They are fluid, unstable and have no abstractly discriminable boundaries.

If we want to use the concepts of neighbourhood and preference in a sociological account such as Schelling's, we have at least three choices. We could try to describe how the actions or behaviours we are interested in are shaped by the (perceived) conditions and circumstances in which those who undertake them find themselves. Central to these circumstances is the classic "What the Devil do they think they are up to?" question posed by Geertz. And then the question of 'mattering' we raised earlier becomes important. Alternatively, we could provide arguments for why among the array of analysed usages which make up the cluster our concept conveys, just the KF-structures (or a single KF-structure) are *the relevant ones for this investigation of this phenomenon*. Or third, and this is Schelling's choice, we could decide by fiat to use the term in a way which naturally fits a KF-structure and proceed from there. Schelling's hand was forced by the need to make the mathematics he wanted to use work. Neither first nor the second option would sustain the kind of analysis he wanted to provide simply because his analysis required arithmetical manipulations.

Healthy Living and Professional Success

Like Schelling, the operationalising Westland undertakes is most easily seen in his visualisations, the path diagrams. In them we can find precisely what is taken to define living healthily and predispose (or, at any rate, mark) having a successful professional career.[13]



The trouble is both these target notions are socially and sociologically complex. For example, how is professional career success made socially and sociologically recognisable? It involves personal attainment, to be sure, signified by promotions and other career markers like salary progression, increased responsibility and authority all wrapped up in increased organisational power. But it also includes peer and even public esteem represented by reputation and acknowledgement of successes in spheres relevant to the domains an individual works within. In many cases, peer and public respect does not track the most obvious markers of 'professional progress'. We had an example before us not so long ago. Those who work in what is called "the front line" of the Covid pandemic were rightly praised for their professionalism, dedication and self-sacrifice. They were celebrated by the media, politicians and the public. But very few of them had the obvious trappings of professional success. They had no insignia (offices, cars, secretaries) and

[13] We are not going to make anything of the difference between these two sets of relationships nor why some indicators have been placed where they are (e.g., why are age and sex not related to health?). Neither will we discuss the fact that the explanation depends upon finding high degrees of association between the indicator variables. The example is an illustrative one and uses standard diagnostic categories from the Health Sciences and Sociology.

were not well paid. The same holds for professionals such as teachers, priests, administrators, authors, musicians and many, many more. In these domains personal and public recognition of success is not marked by easily measurable properties like salary and offices but by expertise, the value others place on the work they do and, most importantly perhaps, by the value they themselves place on it. For these professionals, success is as much intrinsic as extrinsic. Intrinsic (social) characteristics are not easily subjected to standardised measurement. Of course, we don't want to get too starry eyed here. University professors, probation officers, journalists, doctors, nurses and others no doubt do appreciate extrinsic 'reward'. But they are just as sensitive to what their work means to them (well, maybe not always journalists!).

To be fair to Westland, we should not claim he ignores all this. His example is an explication not an actual research report so we cannot say precisely how he would characterise professional success. The indicators he lists are standard demographic variables and all he can produce from them is a description of how whatever he takes success to be varies on them. What we can say, though, is that if he wants to select a sample population on which to measure those demographic variables, the basis on which he chooses who is and who is not successful will face the challenges we outline.

You can run almost the same arguments regarding living healthily. Of course, we understand relative healthiness tends to be associated with various behaviours (non-smoking, moderate alcohol intake, weight control) and height and weight are standard demographic variables. But living a healthy life is as much a personal state of mind as it is the possession of some standard attributes. It has to do with a general sense of personal wellbeing, positive attitude and freedom from anxiety (to pick a few of the many features of our lives we might value). Once again, choosing those who are living healthily cannot be fixed simply by extrinsic, instrumented measures.

Generosity and the State

Much of what we had to say about Ragin's analysis in *Reading Sociologically* bore upon the topics we are now dealing with. His whole analysis turns on an ordering of countries according to the generosity of their welfare provision. That order falls within a range from 0 to 1 with Sweden, Norway, Denmark being the most generous and USA, Australia and Canada the least. This ranking is the result of comparing the countries with regard to the extent of means testing and the independence of benefits from labour market factors such as earnings, years of work and so on. Labour market considerations and available financial resources are held to be used as throttles on the level of benefits to which a claimant might be entitled. They are mechanisms for rationing allocations. Countries which have less stringent rationing are more generous than those where the rationing is stricter. Call this the 'open handed conception of generosity'. Now compare it to what we might call the 'Widow's Mite' conception.

> 41 And Jesus sat over against the treasury and beheld how the people cast money into the treasury: and many that were rich cast in much.
>
> 42 And there came a certain poor widow, and she threw in two mites, which make a farthing.
>
> 43 And he called unto him his disciples, and saith unto them, Verily I say unto you, that this poor widow hath cast more in, than all they which have cast into the treasury:
>
> 44 For all they did cast in of their abundance; but she of her want did cast in all that she had, even all her living.

Here, generosity reflects not the characteristics of the receiver but those of the giver. In many ways, this carries more of the commonsense interpretation of generosity than the rationing conception which Ragin's concept carries. This is because the attribution of generosity comes with moral overtones.[14] The widow is deemed to be virtuous because of the proportion of her own wealth given not because of how she determined who should and who should not receive it. No-one would say the widow is less generous because she gives less than others. In fact, the biblical lesson is that she is more generous because she has less to give. One way Ragin might have captured this sense of generosity could be to use the proportion of GDP or of Government spending allocated to welfare provision. The point we are making is that Ragin appears not to have thought what he should mean by generosity, how it should be represented and how whatever representation he uses catches the moral dimension our cultural sense of generosity has. Instead, he taken an off-the-shelf set of 'measures', recast them by means of his ordering mechanism and used them in his analysis.

Section 4. Conclusion

We do not want to overegg the claims made on the basis of the above limited and undoubtedly threadbare analysis. All we would say is that Maddy's analysis of the metaphysics of Physics and Arithmetic might lead us to be diffident about operationalising analytical protocols for sociological concepts around straightforward lifts of arithmetic methods from the natural sciences. There appear

14 Barbara Kiviat [Kiviat 2023] has recently pointed to the moral implications of using different "narratives" as the basis for constituting social actors as "cases" so they can be subjected to algorithmic-driven processes.

to be cases when simply applying standard arithmetic to counts of some phenomena's occurrence seems to fail to secure a successful closure of the abstraction gap between a piece of sociological analysis and the character of the social experience which it renders. At that point, the social and sociological worlds appear not to align, let alone map.

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3

The Intricacies of the Ordinary

Introduction

In this study, we extend the use of our heuristic for sense assembling sociological reasoning to one of the major modes of sociological empirical research, longitudinal case analysis. The investigation we examine is Laura Hamilton and Elizabeth Armstrong's *Partners and Professions* [Hamilton and Armstrong 2021]. We have chosen this study for its very ordinariness. It has no pretentions and is as routine a piece of sociological analysis as you could wish to find. Its very routineness is what makes it attractive as an illustration.

Section 1. Problem Statement

The report presents a twelve-year study of the life histories of a single cohort of 45 University-educated white women. It tracks them through college and into early maturity. In so doing, it touches on various aspects of these women's lives but its central theme is social mobility. In the diagram below, the results are summarised as pathways which the cohort of women took through a social space defined by class structure.[1] This is the sociological world (domain) being described.

[1] There is a whole seam of analysis to be worked out regarding sociological reasoning through diagrams. We will not pursue it here.

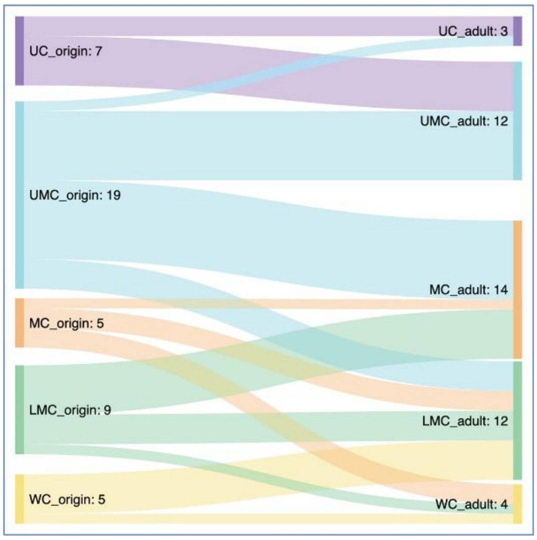

Figure 1 From Hamilton & Armstrong, p. 117

Hamilton and Armstrong summarise this world in the following way:

> (The figure) offers a visual representation of flows from social class of origin into adult class locations at age 30. The figure vividly illustrates the stickiness ofsocial class. Among adult white women, the upper class and upper-middleclass are composed entirely of those who started in privileged families. Fifty-eight percent of women who started in privileged social class positions remained so as adults. Women from privileged families typically did not fall further than the middle class, and none landed in the working class. The four women who experienced the steepest downward mobility were still relatively protected in the lower-middle class. [p. 116]

Registration

The trajectories in Figure 1 are a set of sociological objects constructed from the initial and terminal locations of the social actors (the investigation's subjects) in the defined social space. The pattern those trajectories form is the outcome or consequence of the women concerned having followed different "class strategies" or "projects" as they moved through college and into early maturity. These projects motivate "bundles" of decisions taken over time in pursuit of a desired set of future personal circumstances. Three of the projects are profiled in the report:

1. Gender Complementarity: the matching of female skills in the domestic and social spheres to men's skills in the economic sphere.

2. Professional Partnership: the maximising of success in both the labour and marital spheres.
3. Self-Reliance: an ambition to achieve economic independence within or outside of marriage.

Projects are sociological objects constructed from the data Hamilton and Armstrong collected. A third set of objects presented are sociologically defined "social classes", namely the components of the structure of the social space through which Hamilton and Armstrong trace the women's mobility. The research formulates the detail of individual women's life histories in their social worlds as being within this or that class whilst pursuing one or other strategy. As a result, the women "trace a trajectory" through the class-defined social space. Membership of these equivalence classes defines the women as sociological objects and the social space of their trajectories defines the sociological world for analysis. Hamilton and Armstrong's account is designed to ensure the evidential realism of the social objects can be carried over to the sociological objects thereby securing the account's plausibility. In turn, this conjunction allows Hamilton and Armstrong to frame causal connexions from projects to trajectories as strategically adopted responses to the impact of contingencies in the life histories of the women.

Characterisation

The property set attributed to the constituents of Hamilton and Armstrong's configured world is very sparse. It consists of location in the social space based on family of origin class and point-mass. This set is filled out by the stipulation of other 'facts' which act as premises for the analysis. For Hamilton and Armstrong, these latter facts are the specifications of class. This is a list of characteristics by means of which class is 'identified':

(a) Parents' Education
(b) Woman's Occupational Role
(c) Partner's Occupational Role
(d) Household Income ($k)
(e) Family Wealth
(f) Residential Location
(g) Family Structure

This list serves to erase all other social characteristics for the subject group. The respondents become point-masses loaded with what might be thought of as a class charge displayed as quantitative and qualitative values against the stipulated categories. As a result, the operation of the target system and the conditions it can display are constrained to just those associated with these features. Only data relevant to them will be relevant to the analysis.

Stepping away from the detail for a moment, we could suggest the core of Hamilton and Armstrong's approach is a solution to a *qualitative* differential equation. Changes of class position over time result from the pushes and pulls consequent upon changes in class categorial values. The mobility trace for a respondent is a 'nominal' (up, down, flat line) as opposed to 'numerical' solution to this 'equation'. These solutions are then grouped as the trajectories depicted in *Figure 1*.

NOTATION

Figure 1 provides a good example of configuring a target world-for-analysis. The slice of social world being investigated is set out as a 'social geometry' whose dimensions are class and time. [2] Each trajectory represents a combination of individual traces through that social space. As just described, under this conception individuals are 'social point-masses' moving over time through the class structure. Although Hamilton and Armstrong are not explicit about it, the way their investigation is set up suggests they are using an inertial frame of reference where the 'null hypothesis' (to use that term) is:

> At t_0, social point-masses occupy a class position of origin derived from their parents. Once in motion, each point-mass stays in its class of origin unless impacted by sufficient social forces to give it 'exit velocity'.

What Hamilton and Armstrong investigate is the range of social forces at work on their subjects' actions and what impacts they have on their inertial paths. The trajectories generalise those impacts. In presenting their results, Hamilton and Armstrong provide a discursive 'integration' of the relative effects of such forces on the trajectories set out.

Section 2. Problem Specification

The relationship between sociological phenomena and social data is mediation not isomorphism. Such mediation often makes use of standard tropes to shape the character of the processes at work in the target social world. The master trope in Sociology is the contrast between appearance and reality. For Sociology, 'reality' is 'real-as-construed-within-sociological-theory'. This contrast is

[2] Bourdieu's conception of social space is a two-dimensional force field. Although they draw on Bourdieu in a number of ways, Hamilton and Armstrong depart from him here. Time is not a force. Whether this vitiates the claimed "Bourdieusian" character of their analysis we leave for others to decide.

usually organised within a second order trope, that of 'levels', where surface level sociological constructs of daily social experience stand in some proposed (causal, functional, statistical, or other associative) relationship to deeper structures or where meso- and macro-levels emerge from micro-level interaction in daily life. A complex trope such as this is clearly shaping Hamilton and Armstrong's basic inertial frame. Its influence is clearest in their description of their own investigative intent and in their explanation of the notion of "class position". First, their investigative intent.

> In this article, we compare the class position of white millennial college-going women's parents (captured when women began college) to women's own adult class positions at age 30. Class position is assessed using education, occupation, income, and wealth. We focus on intergenerational mobility patterns—seeking to explain why and how some white college-going women reproduce parental class location, while others experience upward or downward mobility relative to their parents. Our data are uniquely designed to uncover these mechanisms. [p. 103]

What they take such "mechanisms" to be becomes clearer when they describe what makes up a "class position".

> Class positions are characterized by not only differential access to economic resources but also distinct tastes, habits, and dispositions—what Bourdieu (1984) describes as "habitus." Habitus includes, for example, knowledge and comfortwith higher education, socialization patterns (e.g., wine drinking in Napa vs.beer drinking in the back of a pickup), material consumption (e.g., designer clothing vs. big box store clothing), and mode, duration, and location of travel(e.g., regular international vacations vs. rare travel). [p. 109].

Although this summary simply associates various activities with different class positions, the theorised association carries the (causal) nexus associated with the notion of "mechanism". The women made the various choices they made for the reasons they did, little realising they were being 'processed' by the mechanisms of social class.

The depiction just described is re-worked by Hamilton and Armstrong within a further global trope: strategic decision making and implementation. This trope provides an important rationalising device for rendering the undoubted mix of thought-through, ad hoc, impulsive and Hobson's choices the women made before, during and after their college education. These are treated as co-ordinated "bundles" and used to work out instrumental socio-structural logics.

> Class projects are strategies of action—specifically, patterned actions inflected by normative beliefs of what a desirable and attainable economic existence looks like, and how to achieve it, for people "like me" ..... Most class projects involve the desire for some improvement of existing circumstances or—in cases

> of privilege—reproduction of advantage. However, not all class projects aim for the top of the class hierarchy, especially when doing so runs counter to a moral codeor entails actions that are viewed as undesirable, even repugnant.....
>
> Class projects bundle what might otherwise appear to be discrete choices, linking them through an underlying logic. For instance, ideas about whether college is seen as realistic or necessary, the amount and type of parental aid that should be offered for college, what kind of career is appropriate, when and whom to marry, and how far into the life course parents should provide support are often packaged together .... Class projects often feel obvious to individuals—steps through which one is almost invisibly propelled. [p. 106]

The last trope we will pick out from Hamilton and Armstrong's work is so natural it is easily overlooked and its contribution to the shaping of their account thereby missed. This is the notion of *lifetime transitioning*. The use of transitions to organise sociological descriptions is almost as definitive of the discipline as the master trope pairings of appearance/reality and surface/deep levels. Where would its dominant theories be without the presumption of "The Great Transition" between feudalism and capitalism and the associated characteristics of that process, or explanations of the development of contemporary forms of economic and social organisation without the idea of a transition from 'value rationality' to 'instrumental rationality'? Transitions can be deployed to configure events at other 'levels' than the societal or organisational. In ordinary talk, the organisation of personal lives is often conceived as an evolution involving intense inflection points. Hamilton and Armstrong take a version of this commonplace, namely that education and especially Higher Education is, can or should be an important inflection point in someone's life and construct their research project around it. They are looking for patterns in the lifetimes of a sample of college educated women as they pass through the transition from late adolescence to early adult maturity. What clusters of resources, advantages, disadvantages, opportunities, barriers and enablers are associated with the each before they enter college and how do these clusters contribute to *shaping* the outcomes achieved by the women after they leave?

We have described the broad features of Hamilton ad Alexander's world-for-analysis and their description of the sociological objects in it. This world is configured to provide the conditions under which the patterns of mobility in that world can be displayed and analysed. Instead of choices over what is to count as data on the phenomena of interest, how that data is to be collected, codified, sorted and profiled and all the other aspects of the investigative reasoning process, representational choices can be seen as selections concerning the orderliness, appropriate sequencing of relationships and critical levels of their operation. And, just as the problematic possibilities of method are often resolved by using standard recipes, so too the presentation of results often adopts a standard approach and with it a whole array of taken for granted tactics for constructing the course of analytic reasoning. In this process, registration and characterisation achieve the analytic form required for sociological investigation. They do so by formulating the problem as the 'idealisation' required.

SYSTEM LAWS

One key technique for producing disciplinary registration is category substitution. Here theoretical formulations of the detail of social objects and their properties are substituted for common sense ones.[3] Hamilton and Armstrong are explicit about their substitutions. For example, having briefly outlined the history of their project, they tell us:

> Our data stretch across 12 years of the life course and provide great detail on the evolving class locations of women from a wide range of backgrounds—including women whose fathers held leadership positions in Fortune 500 companies and women whose parents struggled to afford household essentials.
>
> By age 30, the women were still in vastly different, and in some cases precarious, economic positions. How did this occur? [p.103]

Note the switch. The women are no longer 'persons' in the ordinary sense but condensates (our 'point-masses') of their family's class location. This is an important analytic move.

They then go on to explain their investigative strategy:

> Building on a Bourdieusian framework, we argue that uncovering mechanisms shaping reproduction and mobility requires understanding the *class projects* in which families are engaged. [ibid]

The women and their families can tell their own stories of how they arrived at the station in life they were currently occupying and just what brought them to do the things they did in the ways they did to get there. Hamilton and Armstrong set aside all these individual reasons, motives, causes, forced choices and happenstances and replace them with a pair of sociological constructs: "reproduction" (the inertial state) and "mobility" (change in state). The women's life histories become exemplifications of social processes as portrayed by sociological theory. Such substitution

[3] To head one objection off well before it gets anywhere near the pass. We are well aware our data/phenomena categories represent just such a substitution.

is even more stark just a few lines later where, in discussing the women's choices of marriage or life partner and emotional commitments made therein, Hamilton and Armstrong bleakly observe:

> How individuals work to achieve a future class position is shaped not just by their familial social class location but also by their location in gender and race ..... For example, the returns to college for women flow, inpart, through access to college-educated men as marital partners.

We assume that if asked, most of the women would say they married for love or some other culturally endorsed value. We doubt any of them would justify their choices in terms of a discounted return on their investment in college education. The personal experience of marriage has been replaced by a sociologically framed calculus.

The important thing to remember about conceptual substitution as a mode of generalisation is the informality of rules applied. Such informality allows researchers flexibility to adjust the goodness of fit of their data to its conceptualisation in response to the nature of research materials and objectives in hand. Later we will look at how Hamilton and Armstrong are able to take advantage this kind of conceptual looseness to construct their phenomena.

BOUNDARY AND INITIAL CONDITIONS

Boundary conditions mark the points in the configured social space where the target system and its internal workings are severed from the surrounding environment. In Hamilton and Armstrong's study, the single boundary condition is the ascribed family of origin class 'loadings' attributed to each point-mass. These conditions are fixed. Nothing which goes on in the world-for-analysis can alter them, though any individual could traverse the space and end in a different achieved social class of their own. In the ideal case, boundary conditions ensure no active forces pass through to the target world once events in that world have been set in motion. As we will see, this ideal is hard to achieve, especially for the social sciences, and a relaxation of the firmness of the boundary conditions is often required to enable the construction of a workable account.

The system regularities attributed to the target system are key to providing this solution.[4] In the ideal case, such regularities contain enough inferential power to fill out the causal texture of the pathway followed. The inertial principle discussed above might appear to provide such a regularity; accounts of "reproduction" and "mobility" being analogies of the First and Second Laws

[4] We use the term 'regularity' here rather than 'law' because we do not wish to be misinterpreted nor drawn into the philosophical mire that is the debate over determinism, interpretivism, instrumentalism, realism and all the rest.

of Motion.[5] Actually, rather than being regularities, the first of these functions as a framing convention and the second is either a premise or an axiom. It is here sociology encounters much of its difficulty in providing end to end integration within its explanations. The required detail is either missing, unobtainable or has yet to be defined. The regularities which are generally accepted (e.g., 'The Iron Law of Oligarchy', 'the Pareto Rule', 'The Self-Fulfilling Prophecy', 'The Labour Theory of Commodity Value', 'The homeostatic character of social structures') are pitched at such a level of abstraction, they can only allow generalised glosses. Their operation cannot be followed in a close ordered fashion.[6] Later, we will see some of the ways this formidable challenge is managed.

The initial condition which sets the target system in motion is the respondents' taking up of a "class project". As we saw, Hamilton and Armstrong identify three types: Gender Complementarity; Professional Partnership; Self Reliance. These "projects" define a decision logic which drives the pursuit of the chosen strategy for attaining personal lifestyle objectives. This logic is the integrative function for an individual's mobility trace.

Operating Conditions

The primary interest of the investigators is the identification of the range of demands and challenges faced by each respondent and the actions taken to resolve them as they move along their trace. These are coupled with the resources available to the respondents and consequently their capacity to continue to implement their chosen strategy. They are the 'contact' and 'at a distance' forces which either reinforce the original class inertia or cause shifts (upwards and well as down) in a mobility trace. Illustrative detail from individual respondents is used to document these operative conditions. We will return to the use of this detail later but, for now, here are two examples. The first is of 'upper class gender complementarity'.

> Ongoing parental support also bought women the luxury of marrying someone who did not quite fit the bill. For example, Hannah's father urged her to "just go for the money" and date the investment bankers she met. Yet, Hannah rejected these men. Her father, a CFO of a Fortune 500 company, was able to use his ties to secure her a job in the sports media industry. Hannah would marry a coworker, who (at $105,000) earned just slightly more than she did. His family was affluent but not as wealthy as hers. She recognized that she would never "be at the level that my parents are at in terms of

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5 We ignore the Third Law because it requires the identification and measurement of forces and the vector changes they induce (degree of attraction or repulsion). The kind of sociology pursued by Hamilton and Armstrong does not have the instruments to determine these properties (or their surrogates) for objects in a social space. As a result, if they are invoked, such properties are set by fiat.

6 By "close order", we mean no gaps, no jumps and no resorting to magic wands, incantations and conjuring tricks.

> making money." Yet, it did not matter. Her family's continued subsidy ensured that Hannah and her husband remained in a privileged class location. They lived in a $3,500 a month apartment only a 10-minute jog from Central Park in New York City. [p. 121]

The second is an instance of 'upper class professional partnership'.

> Bridge funding after college made geographic mobility to thriving labor and marital markets possible. Both Lydia and Erica's parents helped their daughters get on their feet after graduation, funding moves, paying deposits, providing furniture, and offering a car. These parents typically stopped support as soon as they believed their daughters were able to produce a comfortable life on their own. Thus, as Erica's father indicated after she graduated, they would only need to give Erica a car and pay a few bills because"she'll be making pretty good money working." [p. 127]

STRATEGIES OF EFFACEMENT

The description of problem formulation and registration given above is very much couched as 'the ideal case'. No research report ever conforms to the ideal case. Inevitably, adjustments, modifications, re-workings and relaxations of guiding rubrics are needed to manage the contingencies associated with the problem in hand, the fit of its data to the sociological phenomena invoked and the details of the case being made about them. In this section, we describe various coping strategies Hamilton and Armstrong use when faced with these sorts of practical problems as they configured their world-for-analysis. Later sections will address similar of problems relating to the presentation of results and the construction of the case being made about them.

Ontological Elision

When we look at *Figure 1*, we do not see the trajectories of every individual. We are looking at a summative depiction. Misleadingly, sociologists often refer to summaries like this as 'ideal types', which they are not. They are not theorised abstractions over data but informally computed elisions. Each individual trace is the outcome of a multitude of contingent factors. The process of moving from initial state to terminal state is stochastic.[7] What, in effect, Hamilton and Armstrong do is sum over the probability distributions for the members of each social mobility equivalence class using

[7] Each path represents a complex of probability distributions whose integration Sociology at present has no hope of specifying, let alone calculating.

the discursive equivalent of modal regression.[8] Each individual track is treated as describing a path broadly following the central tendency of the category to which the individual is allocated adjusted for the 'error' generated by the contingent factors relevant to them.[9] Such 'error terms' are dropped from the account by a tactic of 'descriptive lifting' when respondents are 'lumped together' along the central tendency of the total category to which they have been allocated.

There is real advantage in descriptive lifting, but it comes with a rider. The advantage is simplicity. Using the central tendency reduces the volume and complexity of the differentiating detail which would otherwise have to be provided. The 'lifting' Hamilton and Armstrong employ prevents them from having to work out how to enumerate and weight the contingent factors contributing an individual 'error term'. The rider is that in so doing they efface the detail of the social forces generating the paths which the respondents follow. This detail is what their investigation is aimed at. Difficult sociology is avoided at the price of generalised imprecision and causal relaxation. Effacing in this way achieves ontological reduction (a kind of behind-the-scenes Occam's Razor) which reduces the range of types of social actor in the space and hence the overhead of detailing and justifying the characteristics provided for every sub-type. Here is an example of this regression lifting at work and the effacing it achieves. It is another from the section on 'gender complementarity' as a means of achieving the project of privilege reproduction.

> Among the nine women who reproduced privilege,seven were socialites on the "party pathway" during college. Their wealthyfamilies, who benefited from both class and racial privilege, had deep pockets and dense ties to other affluent families. The modal way this group reproduced privilege was to pass economic resources across generations. These families offered ongoing support, or continuous economic and material sup-port during and long after college, that positioned women to marry men fromprivileged families who were making very substantial salaries (see table 3).These parents did not assume that women would contribute sizable economic resources to the project.

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8 An alternative metaphor might be the 'mean field approximation' technique used in Thermodynamics. A swarm of 'energised' particles take vectors which are summarised in a single measure of the 'state' of the field. See [Bahr and Passerini 1998a; Bahr and Passerini 1998b].

9 We do not intend 'central tendency' to be interpreted in a quantified sense, but as an informal majority-approximating description. Hamilton and Armstrong talk of it as "modal" (hence our use of that term) but this must be an informal sense since no measures of a run of co-ordinates in the social space are offered nor tables of measures of variables for the counts of respondents allocated to the sub-paths making up classes such as the set 'reproducing privilege'. This is not surprising since not even Bourdieu has tried to be that specific in his analyses of trajectories across social spaces. We do, on occasion though, get some enumerations of some categories.

> Parents encouraged their daughters to focus on cultivating femininity and building elite networks. For example, Tara's mother urged her daughter to gain entrance to a sorority with "very exclusive and beautiful girls, all blonds, the best" because she calculated that marriage was the most certain route to class privilege: "I don't want Tara to be a career woman. I wouldn't want Tara to be a doctor. I wouldn't even care (for her) to ever be a lawyer. I would love (for) her to meet someone like that. (Besides) she wants to be a cookie-baking mom." [p. 119]

The individual traces of Tara and the "socialites" are reduced to a single track. The women are co-classed and the strategy sub-type substantiated.

Time Compression

Transitions involve re-locations in social space over time. For Hamilton and Armstrong's women, this is between their entering College and their 30th year. The trajectories track their locations in social space over that time frame. Except, of course, they don't. Discursive modal regression produces an interpolated path from position at t_0 when an individual entered college and t_n, their 30th year. Between those two points, moments from individual's life histories (jobs they took up, marriages they made and so forth) are described but a detailed description of the shape of the actual trace followed is not provided. No doubt, some women did follow a smooth glide across the space while others had equally monotonic ascents or descents all the way to the end point. However, given the stochastic character of the processes at work, for some (perhaps even many), the vectors will have exhibited a degree of volatility which would itself have to have been managed as part of the class project. This detail has been effaced from both the descriptions given and the summary depiction provided. What is avoided is the need to provide answers to the difficult (and unresolved) problem of specifying the point-by-point co-ordinates in the social space with sufficient precision to enable the identification of intervening locations and the weighting the forces at work in them. However, these formidable tasks would only yield fruitful analytic insight if at the same time we could solve the even more difficult task of defining the temporal granularity at which they were to be done.[10]

Permeable Boundary Conditions

Boundary conditions are controls on the cut with the surrounding social environment and thus what is and what is not contained in the world under analysis. As we saw, the boundary conditions in Hamilton and Armstrong's study are ascribed class of origin and achieved class at 30 years of

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[10] For those who like these kinds of allusions. It is often said Sociology of the Hamilton and Armstrong type is awaiting its Newton, and this might be so. But even if it gets him/her, it is also going to need its D'Alembert and Euler to provide recipes for ontological reduction and time compression.

age. In the ideal case, the determinants of we called the 'charge' or 'loading' of class each side of the boundary cannot be effective within the world-for-analysis. This requirement gives a neatly demarcated frame for the social space. However, in Hamilton and Armstrong's analysis legacy effects and anticipated consequences feature as active forces. Take Taylor for example. Taylor was a professional high achiever for whom professional successes justified the life decisions made. This justificatory shaping is extended to anticipated successes as well as those already achieved. Hamilton and Armstrong follow Taylor in her logic by using anticipated successes as part of allocating her to her equivalence class.

> At the time of the age 30 interview, Taylor was a practicing dentist, making a salary of $130,000, and she was considering purchasing the dental practice of a retiring dentist. We confirmed that she successfully opened her own practice, boosting her income to $200,000. [p.126]

Melanie, on the other hand, relied on pre-existing networks to acquire a potential marriage partner from among her college peers.

> Melanie met Ben her first year in college.Ben had grown up in a Chicago suburb 10 minutes from her home. Melanieknew people from his high school. They even went to the same summer camp as children. At college, overlapping peer networks, developed througha largely affluent and white residential Greek system, drew the two together. [p.120]

The point is not that people shouldn't use their family or social networks to help them in their early careers but that the use of such networks as a social force determining class location is being included twice, once to determine class of origin and again to determine achieved class.

Both these examples speak to permeable boundary conditions and hence relatively 'loosely posed' problems. Others work in more subtle ways. To use the example mentioned earlier, the whole notion of the "reproduction of privilege" ties a child's success to the lifetime success of her parents. "Reproduction" is, of course, Bourdieu's term. In living their lives, social actors conduct their activities within culturally set patterns. They do not innovate on the pattern but improvise within in it. On such a Bordieuisan view, the relative lack of social mobility of a child which reproduces the social class of origin is no more than the continuous occupation of the class of origin derived from their parents.

Similar boundary permeability is to be found in discussions of the social resources available to each respondent for the pursuit of their strategy. Although the criterial definition of the space is class mobility and the features evincing such mobility are marriage partner, professional achievement and lifestyle, other sources are appealed to as well. We have already mentioned parental resources accessed by both marriage partners and the use of pre-existing social networks. In addition, there are references to sorority choices, choices of post-graduate institution, choices over residential location, occupational background of parents and even the profession of one's spouse.

> For example, Sophie, whose extended family included many doctors, wantedto be a nurse who married a doctor. She spent her age 30 interview narrating her attempts. One doctor was too much of a "gunner" (i.e., someone who was overly ambitious at the cost of their relationship). She coaxed the second into dating, moving in, proposing, and marrying. Sophie explained, "Iwas more into him at first. He wasn't so sure (laughter). I think I was the more aggressive one. And, of course, he was still in med school. But then he ended up liking me as much as I liked him." With a specialty in pediatric anesthesiology, her chosen mate was also a good financial prospect. Because Sophie had low earning potential and lacked a wealthy family that could compensate, marrying a high-earning man transformed her class trajectory. [p. 122]

The net effect of permeable boundary conditions is increased lack of clarity of problem definition. The 45 respondents in Hamilton and Armstrong's study are not the only social actors in the system. For some, the actions of parents are vital since they participate in the making of life choices and strategy selection as well as resourcing its implementation. For others, it is husbands and partners, social networks and even Governments and Corporate actors like universities. Of course, in ordinary life we know relatives, friends, acquaintances, employers and institutions matter in various ways and our common-sense explanations of how individual lives play out often lay emphasis on the influence they have. The current state of knowledge and methodological expertise in Sociology does not allow Hamilton and Armstrong to introduce ancillary 'objects' and the social forces they exert in a systematic and principled way (for example, by weighting personal v parental effects on College choices for different categories or by estimating net barrier v net enabler effect of presence or absence of parental wealth, or College experience). It is therefore impossible to modulate their effects for the context of any individual's case. Such complications and challenges are avoided by introducing the objects and their properties as part of illustrating a particular life history rather than a teasing apart the causal texture of the problem set.

Merging Initial and Operational Conditions

The initial condition for the system is the choice of class project or strategy. These are defined in broad brush terms. Operational conditions, the conditions applying in the social space and which shape the eventual position respondents occupy at age 30 are the contingencies and exigencies faced as they pursue their strategy. The analytic task is evaluation of how effective the strategies are for achieving the original lifestyle objectives. As we have already suggested, in the accounts given for many of Hamilton and Armstrong's respondents, boundary conditions also function as initial and operative conditions. Choices, their adopted strategies and capacity to implement them are "overdetermined" (p. 124) by class position. What is called "class reproduction" (the base case) turns out to be ascribed class continuation. This outcome is what Hamilton and Armstrong dub the "stickeyness" of class, an observation they cite as one of the major findings of their study. In the case of class reproduction, the configuring trope of a transition from ascribed condition to achieved condition appears to misrepresent what is going on. What the study reveals is the radiation of the boundary condition through the operational conditions. This is a consequence of the inability to specify the detail of the initial and operative conditions. As a result, Hamilton and Armstrong have no choice but to 'reproduce' the boundary condition—the social class position of the respondent's parents—because the intermediate 'mechanisms' are beyond analytic reach.

Section 3. Analytical Protocols

ANALYTICAL PROCEDURES

The conclusion we draw from the list of 'troubles' we have discussed in relation to managing the practicalities of configuring the world-for-analysis in sociological reports is that reliance on the solution set, boundary and initial conditions cannot sustain the transformation of data into phenomena. Something more is needed. The solution Hamilton and Armstrong use is 'instanced reasoning'. We have just seen how members of the categories of social class are 'instanced' by members of the equivalence class data. But this kind of referencing to data is insufficient. What is required is reasoning over them; the provision of a set of logical steps which secures the plausibility of the case being made. Hamilton and Armstrong need a logical path from categories of data to instanced categories of phenomena. The term for the provision of paths such as this is 'transitivity'.

It will be easier to appreciate the challenge and how well Hamilton and Armstrong do if we introduce a smidgen of formality. We have said the study utilises two 'global' equivalence classes, data and phenomena. The data class is composed of the detailed ethnographic, survey and other material concerning each respondent gathered into "class projects". The phenomena class is composed of sociological specifications of social class and social mobility trajectories. Each member of the phenomena class is further partitioned as set out earlier.

At the global level (the-world-for-analysis), we have a set *S* which consists of all the members of the data and phenomena classes. This allows us to say:

a. 'Gender Complementarity' is a member of *S* or 'Upper Middle Class' is a member of *S*. In general, let's write that as 'For all x, x ϵ S'.

b. Gender Complementarity is a class project. We can call the equivalency relation 'class project' ([a]). 'Professional Partnership' and 'Self Reliance' also have the equivalence relationship [a]. Let's write that as {x ϵ S : x ˜ a}

c. Let's define the equivalence relation of being a 'social class' [b] and being a mobility trajectory [c]. We now have a set of possible relationships.

    i. a ˜ a for all *a ϵ S*. This is the reflexivity relation among the members of [a]
    ii. a ˜ b implies b ˜ a for all a, b *ϵ S*. This is the symmetry relation of a and b
    iii. If a ˜ b, and b ˜ c, then a ˜ c for all a, b, c *ϵ S*. This is the transitivity relation among a, b, c.

What the steps in i.–iii. give us is a pyramid of equivalences. If Hamilton and Armstrong can establish instances of class projects as equivalent to categories of social class and categories of social class as equivalent to mobility trajectories, they will have secured a logical pathway through the pyramid. Whether they will have also secured a causal pathway without the specific detail we noted above is another matter. At this point, it is important to remember equivalence is not an identity relation. The chosen "class projects" are equivalent not identical. So are the social classes and mobility trajectories. It follows the pathway is not saying 'Gender Complementarity' is identical to 'Upper Middle Class' or 'Gender Complementarity' is identical to 'flat-line mobility trajectory'. What Hamilton and Armstrong do is set up broad associations among these classes. That is what 'instancing' does.

Analytical Results

Possibly the best example of how Hamilton and Armstrong use this presentational strategy of instanced transitivity to display the phenomena of 'social classes' and 'mobility trajectories' is the story of Melanie. We quote it in full so its effectiveness can be appreciated.

> Melanie's story offers a rich illustration of how gender complementarity works. She was from an upper-class Chicago-area family—a socialite who was "worry-free in college. My parents (we)re fully supporting me, so I didn't even think twice about anything." Melanie met Ben her first year in college. Ben had grown up in a Chicago suburb 10 minutes from her home. Melanie knew people from his high school. They even went to the same summer

> camp as children. At college, overlapping peer networks, developed through a largely affluent and white residential Greek system, drew the two together. Upon graduation Melanie and Ben moved to Chicago with friends from Greek life. Melanie's life in Chicago was underwritten by her parents, who provided the funds to purchase a condo in the city and an interior designer to decorate it. Melanie was a teacher in a charter school and then attended graduate school to get her MSW, funded by her grandmother.
>
> During the same period, Ben went to law school. Despite the fact Ben's family was wealthy, like hers, Melanie was not ready to marry him until he completed his degree and took a position in his father's debt collection company. As she clarified after graduating, she needed to marry someone whowas "definitely motivated. I don't really like the real lazy type." Ben was promising because he was "so motivated." But he would not be a sure bet for another four years.
>
> Given the homogamy between Melanie and Ben, connections between their families were close. After marriage, all holidays were jointly celebrated. Melanie was happy that her husband and father consulted on the new family's finances. As she explained, the two men "have a good relationship sothey talk about that together because I don't really understand all of it. It works out really nicely." At the time of the interview, Melanie was six months pregnant. Her husband assured her that she need not return to workif she did not want to, as "no matter what, we'll be financially okay." As Melanie noted, "That's a huge burden off my shoulders." Both sets of parents continued to offer support. Melanie's father had a savings account thatshe could access for her spending. When the couple purchased a more spacious condo after marrying, his parents furnished it as a wedding gift. Herparents were creating a well-appointed nursery for their grandchild.
>
> Melanie's project required family wealth to provide her with a consistently lavish lifestyle. This lifestyle was necessary to maintain ties with her advantaged peers, through which she met Ben. Ben was one of many affluent, highly educated men in her social world; research suggests that this was a benefit of racial, as well as class, privilege .... Women like Melanie never intended their own careers to be a source of economic security. As Melanie noted before marriage, "My dream would be not to have to work,because my mom didn't have to work, and it made such a difference in mylife because she was always around." [p. 121]

Section 4. Conclusion

Hamilton and Armstrong use their instancing strategy to "save the phenomena". This tactic is often needed in sociological reporting because of the combination of rendering processes used to configure the world-for-analysis and Sociology's inability to detail those configurations. Sociological objects such as 'social class' and 'mobility trajectory' are generalised abstractions from and over the detail of each respondent's life history. Every one of those lives consists in a plenitude of possibly relevant detail which has been packaged up and summarised by Hamilton and Armstrong in the responses to the interview questions, materials collected, commentaries made by families and friends etc., they collected. The resulting corpus of data is reconstructed to bring out the 'underlying' structures which it represents. Effacement is necessary to strip data of detail so that analysis can focus only on that which is deemed relevant or to avoid the problems set by the inability to amass the detailed data required. What demarcates the possibly relevant from the irrelevant are the boundary conditions laid down for the problem being analysed. What ensures access to the required detail is the state of sociological method. Clear and effective boundary conditions make for well set analytic problems. Well set analytic problems are constituted by the 'sociological phenomena of interest', in our case here classes and mobility trajectories. These are sociological abstractions and generalisations. Abstractions and generalisations don't have friends and families, get married, take up employment, vote for political parties or join country clubs. People (Hamilton and Armstrong's respondents) do.

Abstractions and generalisations stand in structural relations to one another. Sociological abstractions and generalisations stand in sociological relations to one another which in turn stand in representational relationships to the social phenomena they delineate. Hamilton and Armstrong started with a sample of the endless major and minor detail of a set of people's lives and reduced it to a set of sociological objects standing in structural relations. To sustain the plausibility of the designated structural relationships, Hamilton and Armstrong had to save their phenomena by hooking them back to the detail from which they were constructed so that the plausibility of the whole exercise could be secured. This they did in thoroughly conventional ways; ways they were at home with and could manipulate with analytic dexterity.

Partners and Professionals is a very familiar, very conventional and quite ordinary piece of sociological reporting. Because it is these things, other sociologists can follow it without trouble and understand the case it makes pretty much at a first reading. Such easy accessibility belies the work which goes into managing the practicalities of presenting these data and making a case about them. When looked at in this way, the intricacies of Hamilton and Alexander's reporting become visible. They reveal an organisation available for analysis and reflection.

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4

Two Puzzles about Mathematical Sociology

INTRODUCTION

The first of our puzzles is a technical issue in the application of mathematics in the sciences. The second is more a professional matter. We have some thoughts about the technical issue and these will be the focus of this essay. We don't know what to think about the professional matter and so, rather than venturing opinions, we'll just flag it up.

The puzzles are these. A central thread in much of the debate in the Philosophy of Science concerns the realism of mathematical structures. It is central because almost everyone agrees the application of Mathematics in the natural sciences is both a reason for their undoubted success and one of their distinguishing features. The trouble is, and this is the issue, no-one knows how to justify the claim that the mathematics used captures aspects of 'the reality' of whatever is being investigated. That is to say, no-one has so far formulated a good *philosophical* argument for how or why purely formal abstractions are able to represent material goings-on.[1]

Though perhaps not quite the scandal the failure of Philosophy to answer Hume was to Kant, nonetheless the situation has left a lot of philosophers more than a little uncomfortable. The results of the applied sciences seem to show the mathematics must work somehow. After all, to cite

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[1] We sometimes talk of material goings on as substantive and mean by this spatio-temporal. This is fine for the natural sciences but poses problems for the social and psychological. For example, in what way are the institutions of Government or Justice spatio-temporal? As for thoughts and the processes of thinking.............

the favourite examples, science has put people on the moon, developed vaccines using gene editing and is building quantum computers. Since the probabilities of the mathematics deployed just happening to fit the uses to which they are put are so astronomically small, invoking them as justification would make the successes akin to a cosmic miracle. Alternatively, no-one wants to rely on luck for an argument. These days, somewhat reluctantly, the consensus seems to be to default to what is called an inference to the best explanation. The mathematics just works, even if we don't know why. [2]

The professional aspect of this is something of a dog that didn't bark. We would not claim to be professional philosophers of social science. Neither are we members of the branch of Sociology called Social Studies of Science and Technology. But we are interested outsiders and do follow the debates in both domains reasonably closely. The thing is, in all the discussion over the foundations of modes of Sociology, their metaphysics and ontologies, scientific status, the nature of their scientific practise and so on, we have not come across any consideration of the philosophical puzzle about the applicability of mathematics and what that might mean for the practise of Sociology. We have seen lots of other—usually critical—discussions, but not that one. Two of the implications, or so it seems to us, are pretty obvious. The first concerns the use of mathematical methods and structures to organise sociological descriptions. The second has to do with how in practice the mathematics is put to work in Sociology as well as in the (paragon) sciences. The former is, of course, just the extension to Sociology of the philosophical puzzle. The latter points to a whole research domain which, paraphrasing Andrew Pickering [1995], we could summarise as 'the mangling of mathematics'.[3]

While there are no generally agreed positive philosophical arguments to resolve the mystery, there are some technical mathematical ones. We will use one as our point of departure. Employing it, we will offer a sociological account of the philosophical puzzle by suggesting one way of solving the puzzle could turn on sociality. That is to say, we will sociologically construe the mathematical practise the solution represents and propose therein lies a way out of the quandary. We show how this might be done by looking at a fairly routine example of the application of mathematics to social phenomena. Our argument goes like this. When applied to the social, at

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2 The list of the philosophers who sign up for this inference to the best explanation is long and the status of many of them impressive. These are not people who don't know a good bone to gnaw on when they see one. And yet they don't. Perhaps that's why their sociological counterparts seem to be asleep at the switch.

3 Just to be clear. Eric Livingston has already led the way here. See [Livingston, 1986; 1999]

least, what the philosophical puzzle references is the possibility of an abstraction gap between the mathematical formulation of a sociological phenomenon and the phenomenology or lived experience of some part of social life for which that phenomenon is a sociological reconstruction. When looked at sociologically, the task or work of bridging the abstraction gap is part and parcel of doing scientific or sociological description in a professional and competent manner. This competence, we will assert, involves double fitting the mathematics and the phenomenology to one another.[4]

Section 1. Mapping Mathematical Structures

Of course, the realism of Mathematics is a sub-issue in the general concern with what doing mathematics actually involves and what its foundations are. There are numerous offerings here, some of which go back to the Pre-Socratics. One relatively recent one, Structuralism, is the version we want to focus on. At its simplest (and this is very, very simple!) structures are classes of mathematical sets. Mathematics is about is the "mapping" of such sets. Here is how Stewart Shapiro, one of its modern proponents, explains the idea.

> Define a *system* to be a collection of objects.......Define a *pattern* or *structure* to be the abstract form of the system highlighting the interrelationships among them, and ignoring any features of them that do not affect how they relate to other objects in the system [Shapiro, 2000 p.259 italics in original].

Structures, then, are abstractions over configurations of the relational positions of objects. Any actual configuration can be patterned in a number of ways. So, the coloured stripes on the cover of a notebook may be seen as an ordered sequence of green, red, black, white and yellow lines or as blocks of cyan, magenta, yellow and black printed dots or as an open representation of emotional states. For the mathematician, it does not matter what the *relata* of the positioning are (ordered stripes, clusters of pixels, emotional responses). The abstract relational structures are what mathematicians are concerned with. And what they want to do is prove theorems about them.

A mapping defines an isomorphic relationship between structures: the objects of one structure can be equally successfully represented by another. To take a straightforward example. The Integer Numbers ($\mathbb{Z}$) 0,1,2,3,4,5 can be mapped into the Rational Numbers ($\mathbb{Q}$), ½, ¼, ¾ etc, by forming ratios of non-zero integers. $\mathbb{Z}$ and $\mathbb{Q}$ are different mathematical structures with a

4 Here lies the implication of this line of thinking for the Sociology of Knowledge in its broadest sense. The double fitting of descriptions might just be a general enough practice to resolve rafts of problems in social epistemology which otherwise seem intractable. See [Baldamus 1971].

mapping. What ensures the success of a mapping is that it is 'structure preserving'. This notion is extended to applied Mathematics by proposing successful mathematical structures preserve the inherent structure of the domain of application. The way this is this done is by finding a minimal set of features or properties which can be used to characterise some physical object or process (see Jessica Wilson [Wilson 2010]). Different colours of the rainbow, for instance, might be reduced to wavelength frequencies. The heat of a kettle and the journey of a car might be reduced to point masses in motion. The purpose of characterisation is to ensure nothing of relevance to the problem's analytic framework is left out and nothing irrelevant included. Effective characterisation is what delivers structure preservation.[5]

Let's take a familiar example from the social sciences and sketch how the idea might apply there. Suppose we are interested in why Governments are reluctant to tax high earners at greater rates than low earners. The answer usually given (and we are not engaging in a debate over this) turns on the economic object 'the consumption function' and the relationships between its major components, autonomous consumption and the marginal propensity to consume. Autonomous consumption is consumption necessary to maintain basic social existence. The marginal propensity to consume is the increase or decrease in consumption as income rises beyond the level needed to maintain autonomous consumption. Economics formulates the consumption function and its components as mathematical objects such as numerical levels, rates and proportions. For families on low income, autonomous consumption requires all or almost all disposable income. As income rises, the proportion spent on autonomous consumption reduces and the residue can be allocated to other sets of goods and services. Eventually (and this turns on the psychology of utility functions—another mathematised concept—but we won't get into that), the very well off begin to run out of things they wish or need to spend money on and so save. Savings are a large element of the proportion of National Income available for investment. Investment is held by most Governments to be the most important driver of economic growth and stability. Hence, they are reluctant to reduce the 'willingness to save' of high earners by reducing their disposable income through taxation.

What is happening with the adoption of the consumption function as the rationale for economic policy is an assumption about a commonality between the structure of people's social and economic choices and that of a mathematical object, the consumption function. Income,

[5] Effective is not the same as exhaustive or perfect. Characterisation of physical processes by reducing them to other physical processes does not provide for an object's category hopping from being spatio-temporal to an abstract one. The end-to-end closure problem remains and hence the mystery.

consumption and saving are measured by translating goods and services accessed and consumed into monetary values. These are expressed in the Real Number system and the consumption function is a function applied to the result. By means of the consumption function, reasoning about how people deploy their incomes within the social system of economic daily life is mapped into reasoning about Real Numbers. The claim is that the structure of the function maps onto and preserves the structure of the spending. But what, exactly, is involved in doing this? That is the mathematico-philosophical question.

In a recent discussion of the debate, James Nguen and Roman Frigg [2022] suggest examples like ours raise three distinct questions.

a. The first has the general form of the philosophical puzzle we asked at the start. How do mathematical structures (a mathematical description) of any kind map? What general conditions are required for them to do so? This is not about prediction. A function will map if it generates mathematically sound results no matter whether they turn out to correlate with the way the social and natural worlds work. Any mapping will have to satisfy appropriate mathematical criteria.
b. How does a particular mapping work? What particular conditions are required for it to work? Is the structure the right one? What is the right form of the function? Are there limitations to its use, and so on?
c. Then there is the fundamental domain question. What enables facts about a mathematical structure expressed as the consumption function (say) to *explain* (if it does) facts about people's spending?

Nguen and Frigg—they are philosophers, after all—are only interested in the first, the philosophical, question. We are interested in the second but will use Nguen and Frigg's analysis as a platform for setting up our interest. The 'explanatory' can with all of its writhing contents will stay closed.

Nguen and Frigg's conclusion is the mapping account doesn't answer the philosophical puzzle. This is because the target of the mapping is usually taken to be a data model of the system of relationships and data models are themselves already mathematical structures. They are mathematical models derived by applying measurement functions on physical and social goings on. So, while the mapping is a perfectly proper mathematical description, we are still left with the original problem, except now it is about data models. How do abstract mathematical sets in the form of data models map onto non-mathematical substantives?[6]

Nguen and Frigg's diagnosis of why the mapping account in this form doesn't work is straightforward. It just doesn't go far enough. They propose an amendment involving the adoption of what they call their "extensional abstraction account" [2022 p. S5953]. When investigators undertake a study, they have to decide which features of 'the world' they wish to describe and how. In sociological jargon, this is the resolution of the problematic possibilities of description. Whilst we might think it is quite natural to see the set of transactions making up the economy as direct and indirect exchanges of valued goods and services facilitated by media of exchange, there are certainly other ways they can be conceived economically (e.g., circulation of surplus labour value). The standard model is just that, a standard model. It is not the only model. The model selects certain orders of objects and the relations they stand in. It sets aside other orders of objects and their relationships. The description given is an instantiation of that selection.

Howsoever we choose to describe it, the description of actual economic activity has to be processed by a step by step climbing of the "abstraction ladder". At every move from riser to riser, more of the directly domain-relevant features of the description are stripped out. Eventually, nothing pertaining to the domain is left. All we have are objects coded in a mathematical structure.[7] But now the question becomes how to move back to the domain when we have the results of the mathematical operations on the structure which we generated. How do we step down the ladder again? This can only be done if we recognise our results depend on the structure generating original (selective) description. The mathematics of the consumption function does not capture essential reality, some uniquely identifying structure of economic life. It is a rendering of economics in a chosen modality. In earlier work, Frigg and Nguen [2019] called such renderings "keyed descriptions".

Nguen and Frigg's solution to the puzzle of the applicability of any mathematics rests on assumptions about the relative openness of selectivity of choice and its institutionalisation in a

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6 This is, of course, simply looking at the relationship in terms of the ability of the mathematics used to respond to the constraints of the physics being described. Noah Stemeroff recently pointed out the constraints that the mathematics places on the descriptions the physics can give are just as important for the viability of the mapping. See [Stemeroff 2021].

7 This stepwise abstraction is a very good explication of what Woodward and Bogen [Woodward and Bogen 1988] were pointing to in their discussion of scientific practise as the transformation of physical data into theoretical phenomena.

professional practise. The latter can be rendered in a standard sociological form. For the sociologist, the patterns of empirical description are institutionalised when they are normatively regulated. They have a normative social character which is open to analysis under an appropriately construed sociology.

Applying the problem as characterised by Nguen and Frigg to the practise of Sociology causes no difficulties for sociological investigators. They just don't view it as a mathetico-philosophical question and so don't talk about it in those terms. For them, it is a practical question; one of operationalisation through coding. Somehow, the collection of reports, records, responses, notes and other impedimenta capturing the investigative experience of any study has to be processed and rendered in some appropriate notation to make it amenable to a chosen mode of analysis. In many cases, just as with the consumption function, this notation is derived from the Real Number system. What the coding produces is the data model to which functions on the Real Numbers can be applied. We used the term "somehow" just now. We did not mean "just anyhow". What the sociological rendering picks out are the disciplinary expectations which normatively regulate research practise and reporting. What these are and how they work are what sociological investigators discuss. Except mostly they don't do it explicitly in the reports they write. Instead, what we have called "the plausibility structure" of reporting (its demonstrable conformity to professional expectations of competent practice) is carried in the reporting itself. For those who know how to read for it, the reported reconstruction provides an "account" of its own production. This is what we meant by suggesting there is a sociological solution to the philosophical problem. Displayed competent practice instantiated in managing the coding problem provides a "for all practical purposes" bridge across the abstraction gap. In the next section, we illustrate one way this can be made visible.[8]

Section 2. Attitudes to Governmental Intervention

In a recent paper, Andersen, Curtis and Brym [2021] model how support for governmental social and economic intervention varies with levels of societal prosperity and income inequality. They

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[8] The "for all practical purposes" qualification here is important. The solutions used are not philosophically inspired nor philosophically justified arguments. They do not answer the question which worries philosophers. What they do is manage the problem by coordinating a practical solution as the recognisable accomplishment of plausible coding. To find a practicable means of avoiding the philosophical impasse, sociologists shape both the data and the structure to get them to have a 'good enough' fit to one another and they do so in recognisably professionally competent ways. Just for completeness sake and to prevent sociologists from incurring too much angst, we should point out that John Bell, one of the foremost theoreticians of Quantum Theory, complained much, if not all, of standard text book accounts of Quantum analysis is of this loosely connected, for all practical purposes kind.

make extensive use of official or semi-official large-scale data sets. The base data used for the study have been collated from cross national surveys of 66 nations carried out by a number of Governments, supra-national Agencies and NGOs. The work required to mould this data to the problem they set themselves is what we are interested in.

PROBLEM STATEMENT

The core of the report consists in the consideration of a number of alternative regression models for the distributions of attitudes. The text defines the problem which the investigators wish to address as the "contradictory" nature of the extant literature on attitude formation towards governmental socio-economic intervention. This contradictoriness arises because of an insufficiently precise focus on the subjective drivers of attitudinal formation. Clarifying these drivers will, or so Andersen et al. propose, facilitate resolution of the contradictions.

Registration

The sought-after clarification is achieved by describing the three-way relationships between attitudes, income inequality and prosperity. Under the current, contradictory approaches

> ....we expect support for government intervention to be high if inequality is high, regardless of the level of prosperity. We also expect support to be high if prosperity is low, regardless of the level of income inequality. On the contrary, support for government intervention should be lowest under high prosperity and low inequality [Andersen et al. 2021 p. 1351]

This expected pattern is illustrated by an associated representation.

TABLE 1 Expected public support for government intervention by level of economic prosperity and income inequality

		Low	*High*
Income inequality	*High*	High	High
	Low	High	Low

Economic prosperity

Figure 1 From Andersen et al p.1351

The intuition is an obvious one. People's attitudes to Governmental socio-economic policies are driven by self-interest and reflect two socio-economic factors: income inequality and economic prosperity. The general level of support can be defined by positions on the nominal co-ordinates of the dimensions. This is the initial conceptualisation of the problem. Attitudes are locations in a Cartesian space of socio-economic dimensions.

Characterisation

'Box diagrams' of this kind are widely used in Sociology to simplify complex relationships and frame empirical and theoretical investigations. Perhaps the most famous are Merton's "Theory of Deviance", Parsons' "GAIL" structure of Functional Imperatives and the 'Prisoner's Choice Paradigm' in Game Theory. Andersen et al.'s use of this heuristic allows the reader to visualise how to step through the intuitive pattern they have hypothesised. In that sense, while *as a statement of the problem space* the illustration adds nothing to the text, it greatly enhances the comprehensibility of interaction in that problem space. Analytically, the text and the table accomplish precisely the same tasks. All the table does is 'concretise' the pattern by stripping out ancillary detail.

The text/picture combination achieves several things:

1. Isolation of the problem space;
2. Reduction of the problem statement;
3. Definition of a first order putative solution to that problem.

We'll take each of these in turn.

1. Attitudes to Government policies have all sorts of social and social psychological sources and supports. Much work has been done trying to articulate the processes by which public and personal opinion are shaped by social context at a personal and societal level. The approach Andersen et al. summarises simply sets that work aside. From the welter of contributing forces, we have a condensate of just two societal levels of income inequality and economic prosperity. These two massively simplify the degrees of variability in the problem space. We have just two dimensions and two levels of measurement.
2. The problem statement is encapsulated in the patterned contents of the boxes: the high/low mappings. This fixes the report's initial explanadum. How are the attitudinal outcomes in the boxes related to the social conditions?
3. The initial explanans is a mechanistic one. Relative position on the dimensions determines the attitudinal outcomes.

To find a place to insert their clarification, Andersen et al modify the explanans. The degree of support for intervention varies with *personal perception* of income inequality. In societies with high inequality and high prosperity, support for intervention is greatest among that segment of society which believe they are relatively poorly off. In other words, once you factor in personal perceptions of one's own position in the income structure, the contradictions are resolved. In this modified explanation, there are three input social factors in play: levels of income inequality and societal prosperity as before and interpretations of 'relative deprivation'. Now we have three dimensions.

Notation

The influence of the input factors on attitudes is the sociological phenomenon they wish to describe. They model this as a series of regression functions. These models will provide instructions for how to read and what to read into the pattern set out in Figure 1. Regressions are partitions of the 'contributions' individual co-variates make to the overall variance of a variable. The clarificatory solution Andersen et al offer is arrived at by applying a regression function on a data model representing perceived and actual income inequality and economic prosperity together with policy attitudes held by the surveyed individuals. The structure of measures of co-variance using a notation in $\mathbb{R}$ is the data model Andersen et al. need to create to allow the regression functions to operate. To get that, they have to operationalise and abstract over the perceptual data.

PROBLEM SPECIFICATION

System Laws

Go back to the original problem statement. The propositions are couched as generalised relationships among theorised socio-economic phenomena. They reference individuals' pre-theoretical phenomenal social experience. Economic prosperity refers to the array of goods and services available for purchase. Income refers to an individual's capability to access those goods and services. Attitude to government intervention refers an individual's view of the role of Government action to redress socio-economic inequality. Finally, a sense of one's own position in the overall distribution of income is quite obviously a personal judgement.

In the problem statement, the two socio-economic drivers determine the sense individuals have of their social experience. This relationship is expressed mathematically as an Ordinary Least Squares (OLS) regression equation in which the value of dependent variable is a linear function of the independent variables. In their presentation of their statistical models, Andersen et al. detail how they teased apart the data by using various control mechanisms to produce different models for mixed effects and country specific effects across the variables. These models are their results.

However, the question we have raised is prior to these considerations and concerns the structure preserving character of the mathematisation itself. Simply put, the question is this. Since the output of the OLS models are linear estimates (technically, they are called "expected values") for the relevant populations, can we be sure the formal statistical assumptions about the structure of the OLS equations map onto reasonable assumptions about the populations of the 66 countries studied and the attitudes their populations might have towards Government intervention? Here we are in precisely the same position Physics is in with regard to its mathematical structures, except Sociology cannot look to a history of successful interventions and implementations on anything like the scale of Physics to justify their use. Instead, what Andersen et al. have to do is to demonstrate professionally recognisable best efforts to shape up their data to reduce any likely mismatch in key areas of their models' assumptions. The possibilities of mismatch fall into three broad clusters: those relating to the form of the relationship among the variables (its linearity); those relating to the statistical independence of the variables; and those relating to the statistical character of the error terms in the regression equations (the 'distance' the values for the attitude of a population actually is from 'expected values' of that population as calculated by the regression function for their country). Since they can know nothing about the error terms, all they can do is make standard assumptions about their form, namely that they are uncorrelated and randomly distributed. And since they know nothing about the actual form of the distribution in the population of the relevant attitudes, they have no grounds on which to prefer a non-linear to a linear form. It follows, the plausibility of Andersen et al.'s analysis has to turn on how they manage what they do know and can affect, their variables. They address this challenge by walking their readers through their analytic procedures.

Analytic Procedures

Operationalisation of the concepts construct a set of relationships, a framework, into which measures on variables can be plugged.

Dependent Variable: Relative support for Government intervention measured by the survey responses.

Independent Variables:

National Level: Level of prosperity measured by GDP per capita; Level of inequality measured by Gini coefficient. These are 'controlled' for cultural factors defined as global region, majority religion, ethnic diversity, level of democracy and communist legacy.

Individual Level: Perception of position in overall income distribution measured by survey response controlled by age, marital status, and religion.

As presented here, the model is a four variable one. For each respondent, the dependent variable is a function of the interaction of independent variables. But each of these independent variables is, in turn, a function of other variables of which there are eight in total. Initially, this gives us 12 degrees of freedom for each respondent. This data space is reduced to 4 by being "controlled". The number respondents included in the analysis is 208,234. The distribution of these respondents in the 4-dimensional Cartesian data space is the data model. We now turn to how this is produced and seen to be produced as managing the potential for an abstraction gap arising out of the interrelationships of the variables. Successfully doing this satisfies the constraint of professional competence.

Strategies of Effacement

When chasing up and down the abstraction ladder, what considerations have to be borne in mind with regard to how 'variables' are managed? There is much professional knowledge and practical experience here and Andersen et al. deploy some salient considerations on the variables they have chosen.

Individual Attitudes

The term "controlled" used with regard to the independent variables is an important signal for a key step that has taken place in the abstraction process. The identified variables are potential "confounders", that is variables which can have interacting effects across a population. Those identified are standard demographic features which are generally recognised as potentially confounding. To remedy this possibility, the data are stratified by the confounding variable (age, marital status etc.) and an analysis of co-variance performed. If there is a statistically significant relationship in the stratified samples of the population, then an adjusting procedure is applied to smooth that out. This is a standard statistical practice and its use displayed simply by reference to the activity of "controlling". Of course, controlling can only be carried out on identified variables. Those listed by Andersen et al, are the ones they take to be material. There can be no guarantee other non-controlled confounding effects remain. One could imagine, for example, educational experience might be relevant and material in the current case whereas height or weight might not. If abstraction is a process of distilling or rendering down, the residues of features not controlled for will remain the in abstracted model. But, once again, this is what every competent mathematical sociologist knows.

In addition to the above possibilities, reliance on 'subjective' measures is known to bring possible complications. Anderson et al. signal their awareness of these by accepting such measures are...

> ....prone to measurement error insofar as respondents may not know their household income or report it accurately. Second, responses to the item depend partly on respondents' perceptions of their countries' household income not just on their actual household income. This circumstance may also induce measurement error because perceptions may be correlated with inequality, with the same absolute income level receiving a higher because perceptions may be correlated with inequality, with the same absolute income level receiving a higher score in a high-inequality country than in a low-inequality country. (Andersen et al, p. 1353)

The problem with such considerations is that although they are well known, there are no satisfactory methods for resolving them. They may be present and material or they may not. There is no way of determining which is which. For the profession, they are well known unknowns.

Prosperity as GDP per capita:

This is defined as the total monetary value of all traded goods and services produced and consumed within a calendar period indexed to a base date. The investigators sum over revenues and costs associated with economic actions. Because of difficulties of comparability, Andersen et al opt to use the UN estimates of national GDP arrived at by pricing a standard basket of goods and services (at PPP or purchasing power parity) indexed to 2005 US dollar values. This index is known to have a number of significant defects. For example, it does not allow for variation in cost structures (e.g., transport and labour costs and sales taxes relevant in different societies) nor the relative cultural value and hence attractiveness or essentialness of some goods and services over others. Both are known to drive price variation (water is regarded as a required public good in most western developed nations and priced as such but not in many developing nations). Not only is GDP not necessarily a measure of experienced prosperity, the use of the PPP method may be prone to an assessable degree of measurement 'slop'.

Income inequality as GINI index:

This index is the difference between two ratios: the normalised cumulative percentage of total income held by a segment of society and the percentage that segment represents of the total population. If 25% of a population receives 75% of the total income, the GINI index is 50%. The absolute limits are 0% and 100%. With 0%, everyone receives the same income. With 100%, only one person receives all the income. As with the GDP measure, there are known pitfalls in using

GINI to make cross national comparisons. For example, there will be clear differences if income is defined simply by earnings rather than including accessible cash as well. Equally, income from the informal sector (the 'Black Economy') is not included. Also excluded from the index are public interventions aimed at reducing inequality such as means tested grants, benefits and subsidies. Finally, there is even the possibility the index will give perverse values. For example, the index may increase indicating increased inequality even though that segment in absolute poverty actually declines.

Political Culture

Clearly, an important factor influencing attitudes to policy implementation is political culture. Andersen et al. include two national level variables to allow investigators to assess its contribution. The first is the existence of democratic institutions. Here the criteria are:

a. Existence of institutions which allow for choice over policy;
b. Institutional constraints on leaders;
c. Constitutional guarantees of civil liberties.

These *pro forma* conditions are taken to index the existence of a democratic culture and coded for any country on a range 0:10. From the discussion, we don't know how the calibration of individual state systems is carried out. Presumably, it is some assessment of the operational effectiveness of the institutions providing the choice, regulating the constraints and guaranteeing the liberties. Since a single value in the range is required for each country, this assessment has to be done by evaluating publicly available and accepted 'objective' distinctions among different liberal Western Democracies and between them and authoritarian regimes of various kinds. This weighing, assessing and judging carries with it similar issues (this time in regard to how the investigators made their evaluations) to those outlined for respondent judgements. As such both are *informal* as opposed to *formal* abstraction processes.

The second variable addresses what might be thought of as the embedding of democratic processes. Here the variable is binary: existence of a communist Government over the last 25 years. This criterion is easy to apply, but what does it represent? Its effect is to act as an index of the history of State Socialism of the Russian, Chinese and related forms. It is not clear, for example, whether the regime in Cuba would count as communist (Cuba is not included in the data) or that of Zambia under Kenneth Kaunda. Equally, no consideration seems to be given to the relative embedding of democratic institutions under non-communist authoritarian regimes such as military juntas and dictatorships.

The indicators used have been developed for large-scale exercises of the kind Andersen et al. are attempting. They summarise their approach to their trade-offs they necessarily have to make as follows.

> If one wants to examine a large number of countries with heterogeneous social characteristics, as we do, these. If one wants to examine a large number of countries with heterogeneous social characteristics, as we do, these issues are unfortunately insurmountable given the current state of cross-national survey research. For example, with the exception of a single case (South Korea in 2018), the WVS lacks an objective measure of income and, with the exception of a single case (South Korea in 2018), the WVS lacks an objective measure of income and, for many countries, lacks other objective measures of economic position, such as social class ...... Faced with a choice between examining a small set of relatively similar countries or a large set of relatively dissimilar countries, we chose the latter because the former has, in our judgment, led to erroneous of relatively dissimilar countries, we chose the latter because the former has, in our judgment, led to erroneous generalizations based on small biased samples. However, the reader should bear in mind the limitations that inhere in our choice. (Andersen et al, p. 1353 reference omitted)

There are, then, well known difficulties with both the 'subjective' and 'objective' dimensions. However, in the absence of a better alternative, to do what they want to do, Andersen et al. have no choice but to use the data they have.

Summary

Andersen and his colleagues provide a thoroughly professional summary of the problem they wish to address and their approach to it. They identify a number of well-known issues with investigations of the kind they have undertaken and outline the decisions they have made in regard to them. In addition, they provide pointers to methods they have used to manage those which they do not discuss. Using these methods, they succeed in reconciling what is initially a somewhat nebulously formulated problem with an amorphous and highly variegated set of data. The problem is given substance and the data is sufficiently simplified and abstract to form a tractable data model.

This success is achieved by a process of property or feature effacement and problem avoidance. However, as we pointed out, techniques of 'confounder control' while professionally applied, are not necessarily exhaustive nor are they necessarily sharply defined. Residues may filter through and relevant properties be inadvertently carved off. Problem avoidance is inevitable in any discipline but especially so in the social sciences where notational ambition quite often outruns methodological instruments. The lack robust methods (and theories) for tracking the specific processes of attitude and opinion formation at an individual level and then aggregating them to the collective level means the account has perforce to pitch its causal story at the summative level. There are no widely accepted structure preserving 'escalators' available for moving from the models of micro personal attitude formation to the models of macro national or collective attitude formation.[9] But, as we say, this order of property effacement and problem avoidance is well known, thoroughly conventional and normatively regulated. It is the stuff of neophyte training and professional off-the-record discussions. Its use constitutes much of the taken for granted background read into research reports by professionally competent readers.

Section 3. Conclusion

Our aim has not been to justify the use of mathematics in the natural and social sciences by providing an answer to the philosophical puzzle with which we started. Neither do we wish to argue for its rejection. What we have tried to do is show how competent disciplinary good practice manages the problem of relating mathematical structures, data structures and phenomenological experience and how it displays that management in its research reporting. We called this the double-fitting of data and phenomena. The degrees of freedom in any such fitting are regulated by the norms of a discipline's own practise. In the natural sciences, these norms impose pretty austere requirements on a description and its notation. In the social sciences, for reasons which we have touched on here and elsewhere, the requirements are more loose-limbed. It is for each discipline to choose the constraints under which it wishes to work and such choices will be made with the need to ensure investigations are "doable" in Leiberson's [1987] sense of not imposing impossible-to-satisfy objectives simply out of a desire to be 'more rigorous' or 'more scientific'. Double fitting phenomena and data is not a defect in either the natural or the social sciences. It is an ineradicable fact of professional life, a "normal natural trouble", to use Garfinkel's phrase, to be handled as best one can in one's investigative reporting. The management of the abstraction gap is not a surreptitious, sub-rosa blemish on investigative disciplines but a routine feature of their own lived experience.

All of which takes us back to Nguen and Frigg's questions. With regards to the second, the justification of a particular mapping, the compromises, carried through residues, workarounds

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[9] Behr and Passerini [Bahr and Passerini 1998a], [Bahr and Passerini 1998b] have suggested Mean Field Approximation methods adapted from Thermodynamics might be suitable. Thus far, their proposals have not really been taken up.

and avoidances Andersen et al. were required to accept to get their investigation to work, seem to suggest to existence of not an insubstantial abstraction gap between their data model and the social phenomenology which is its target. How substantial the gap might be is impossible to assess with current methods. All we can do is turn back to the argument Euler, the paramount mapper of Classical Mechanics, used to justify the extension of the mathematical to the physical, namely the usefulness of the results. But, in our experience, to ask how useful Andersen et al.'s study is or might be, or indeed how useful any application of mathematics to the social might be, is not liable to resolve anything. More likely it will just add confusion and dispute.

As for the first question, given the applications of Mathematics in Sociology almost always use functions on $\mathbb{R}^N$ or some related number system, the security of such mapping rests on the security of the foundations of $\mathbb{R}^N$ or whatever as a system of mathematical objects. The trouble here is there is just as much dispute on this as there is in Sociology over the applicability of its research findings. Today, the foundations of the number systems are sought by looking for a new setting, a geometrical conception of number spaces in which they can all be placed. But achieving that conception requires the invention of new methods and with them new objects, such as imaginary numbers, vanishing points, lines at infinity and so on which make the mathematics work but whose 'reality' many mathematicians balk at. As Mark Wilson [Wilson 2001] put it, the search for new and better theorems and proofs is a process of creation and innovation which is in permanent tension with observance of the primary mathematical virtue of certainty.

As for the third question, how mathematical structures can *explain* the non-mathematical and especially the social, it is probably best to let that sleeping dog lie.

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5

Possible Worlds Historiography

INTRODUCTION

This discussion has two objectives. It applies our heuristic for reading sociologically to the mode of Sociology usually called "qualitative analysis". Second, it explores how this heuristic might act as the framework for ethnomethodological analyses of investigative reports. It takes its departure point from an aside on conditional relevance made by Lynch [2007] in his paper *The Origins of Ethnomethodology* . In that comment, the notion of conditional relevance within chains of action sequences is generalised beyond its traditional habitus, namely the domain of conversational turn taking and related analyses, to all aspects of the organisation of social life. We pick up this loosening of ties (or broadening of application) and use it to outline one way Ethnomethodology (EM) might address Sociology, and sociological texts in particular, without the need to deploy any of its critical armoury nor its usual tactic of topical transposition. We ask about conditional relevance as an endogenous feature of the organisation of sociological writing/reading (the construction of a case, in this instance). In so doing, we treat conditional relevance as a key feature of what we have called "action at a distance". With Lynch, we believe conditional relevance to be one possible general property of action sequences with its provision to be found in all aspects of social life. Providing for and discovering conditional relevance is an essential task in the sense assembly of sociological courses of reasoning. The aim here is to demonstrate how, through his manifest competence in managing the production of such conditional relevance, Max Weber secures one of the cases he makes in Ancient Judaism [Weber 1952]

We have long thought the early papers of Harvey Sacks deserve more attention than they are generally given. Four in particular [Sacks 1997], [Sacks 1972], [Sacks 1963] and [Sacks 1999], although clearly finding their way, sketch an approach to sociological analysis of forms of social life which constitutes them as distinctive finite provinces of meaning. [1] In an inchoate way, Sacks initiates what were later to become standard techniques in EM, namely the presenting of a way of life's "shop floor work" and its character as "instructed action" alongside materials germane to that way of life as displays of what is required for competent performance. Two of the papers focus on the practices of Sociology and seek to open up that discipline's "adoption of the scientific attitude" as an idiosyncratic extension and transformation of the Natural Attitude towards sociality in daily life. As a finite province of meaning rooted in the Natural Attitude (as they all are), Sociology "reconstrues" social objects as data of the social and then transforms them into sociological phenomena.[2]

Here is an example of what we have in mind. Near the beginning of *Ancient Judaism*, Sacks makes the following assertion.

> I am proposing, then, that Weber's is a method for making transformations from documentary materials. The problem I shall address is *how* does Weber produce his transformations? Imagine that one had the Old Testament and its critical exegeses on the one hand and blank paper on the other. What set of instructions would be required in order to produce a document similar to *Ancient Judaism* from these materials? [Sacks 1999, p. 33]

Sacks' answer is that Weber "interrogates" Old Testament materials and treats what he finds as "responses" to his questions.[3] Using these responses as 'evidence', Weber produces a sociological reconstruction of Israelite society. In other words, in order to bring out its structure Sacks analogises Weber's method to standard techniques in police and judicial practice for assembling evidence in the making of a case. Weber has no direct access to Bronze Age Israelite society. His only

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1 Better awareness of this quartet might help put an end to much of the twaddle talked these days about the origins and ambitions of Conversation Analysis as a naturally observable social science. But we digress......

2 This description is ours not Sacks'. It is our interpretation of Garfinkel's discussion of "rendering theorems" [Garfinkel 2002] heavily influenced by the work in the philosophy of science of Woodward and Bogen [1988] and Woodward [2009]. Sacks talks about his approach as 're-construction' which somewhat underplays the essential hermeneutics of all sociological investigation (including that of EM), something which is of particular relevance vis a vis Weber who famously remarked social actors were inevitably suspended in webs of meaning. The implication is that all sociological descriptions are construals and what we offer here is a construal of Weber's own construal. This stance can be giddy making, especially if accompanied by the conviction any attachment to 'ground zero truth' (as some naturalistic philosophers like to call it) and hence empirical bite about a phenomenon, has been lost. Since we are committed to Sociology as a discipline of "keyed descriptions" and its investigative subjects as inquirers (among other things) into the facticity of social phenomena, we see no need for alarm. Those wishing for more than our present insouciance, might like to consult [Sharrock and Anderson 1982] and [Anderson and Sharrock 2019]

3 Schegloff [1999] makes much of Sacks' "construction" of Weber's "interrogation" of his sources as an instance of Sacks' evolving interest in what would come to be called the "formal properties" of practical actions such as sociological description.

witnesses are the texts of the Old Testament and related documents, so he culls what evidence he can from them. Sacks' sociological interest in Weber's *Ancient Judaism* is as a display of the work of interrogation and the compiling of a case on its basis. This construal is Sacks' formulation of Weber's "turning" towards his materials and his adoption of a variant of 'the sociological attitude'. The definition of the finite province of written/read meanings designated by the label "Ancient Judaism" is constituted under this attitude.

Sacks treats this method as a solution to a key problem Weber faces in producing his account, namely the calibration of his description against the required common-sense version of criteria for causal and meaning adequacy. Weber cannot take a trip to ancient Israel to witness (an important word, that) society at work for himself (and neither can we). There can be no intersubjective triangulation of natural descriptions of the direct experience of ancient Israelite life as a criterion of adequacy for what he says. All Weber can do is secure the plausibility and build a case for his hermeneutic reconstruction of Israelite history by relying on his readers to use common sense criteria of recognisability, clarity, consistency, coherence etc. to determine the acceptability of the depiction given. Elsewhere, this is how Weber describes the self-same challenge.

> This rather extensive formulation of a simple matter, which was required for the sake of clearing away ambiguity, shows that the formulation of propositions about historical causal connections not only makes use of both types of abstraction, namely, isolation and generalization; it shows also that the simplest historical judgment concerning the historical ''significance" of a "concrete fact" is far removed from being a simple registration of something "found" in an already finished form. The simplest historical judgment represents not only a categorially formed intellectual construct but it also does not acquire a valid content until we bring to the "given" reality the whole body of our "nomological" empirical knowledge. [Weber, 1949 p. 175]

Of all the possible ways the history of Israel from the Abrahamic peregrinations to the return from Babylon might be told, Weber's task is to convince his readers his interpretation is the most reasonable. Hence the wording of our title.[4]

One thing Weber's text is doing, then, is producing a plausible account of Israelite history. But it is also doing much more. Its plausibility rests on the recipient design of the account's self-explicating character. Call this the interactional problem of textual order. The requirement for achieving meaning adequacy necessitates the assurance of a reciprocity of perspectives between

4 Let us be very clear. The use of 'possible worlds' in our title has nothing in common with the attempt by Lizardo and Strand [2022] to add a "probabilistic Weber" to the Rogues Gallery of Webers assembled by Parsons, Geertz, Giddens and others.

Weber and his readers in order to prevent the work being encountered as "specifically senseless".[5] The motivational structures guiding courses of action must be recognisable and comprehensible to, if not necessarily shared by readers. This is the rhetorical problem of textual order. Wound into both is the documentary problem of textual order. Solutions to the interactional and rhetorical problems of order turn on adequately solving the documentary problem of order. The heart of that problem is conditional relevance, the achieving the integration and coherence of the 'story' he wants to tell about the social structural implications of the evolution of Yawhe worship from a mixed polytheistic-magical belief system to a monotheistic hierocratic religion and its role in the subsequent rationalisation of Israelite society as a "pariah community". The integration and coherence of the story and the achieved conditional relevance of its components as a written/read case provide for the account's recognisability and adequacy as a causal history of the sequences of the events he presents. The events can be found to be properly following *from* and not just *after* one another.

There is a second sense of conditional relevance at work here. This has to do with the multiplicative ordering of elements in the structure and its completeness. Just as hearers presume (i.e., expect and project) internal consistency in the construction of turns and the use of category memberships, so readers presume adequate completeness in a structure of reasoning. Demonstrating such adequate completeness within its course is the work of that reasoning. This sense of adequately constructed conditional relevance provides for putatively 'reasonable' causality without connotations of determinism or combinations of sufficient and necessary conditions. The connections are just the realisations of those possibilities Weber chooses to reconstruct.

Only if *Ancient Judaism* contains solutions to the problems of recipient design is the case made; a case which, from the outset, Weber intends should counter, indeed refute, Werner Sombart's interpretation of the role of Judaism in the development of early modern Capitalism. [6]

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5 The allusion is, of course, to Garfinkel's famous 'experiments with trust' [Garfinkel 1967].

6 The essays appeared in Weber's journal the *Archiv für Sozialwissenschaft und Socialforschung* between 1917 and 1919. They were his response to Sombart's study. A more direct consideration of Sombart's [1913]) argument can be found in Weber [1968 *, vol I part 2 Ch 6 sect. xii]:* also published as Weber [1965].

Section 1. The Taken for Granted Disciplinary Context[7]

In describing how *Ancient Judaism* relies on interrogation to produce a description of the history of Israelite society, Sacks indicates how to construe the text as a reconstruction; that is, how to see it as an instance of the use a standard method of sociological analysis, the interrogation of materials, texts, notes, objects etc, to propose a reconstruction. This approach answers one important sociological question, However, our interest goes further. It asks: What makes Weber's collection of essays *this* reconstruction, the one which Weber intends? That is, how are the results of his procedure and their deployment shaped to produce the text as the self-organising, self-explicating plausible account it was designed to be for the audience for whom it was written? To offer an answer to Sacks' question revised in this way, we need to understand a bit more of the context within which Weber was placing his research; a context which was very much understood and appreciated by those for whom he was writing.[8]

Ancient Judaism was not an innocent project for Weber. For several years, he had been a major participant in the so-called *methodenstreit* over the choice of an appropriate paradigm for social research. The objective of his contribution to the argument against scientism in the human sciences was to demonstrate the superiority of his own preferred typificatory causal explanations over those of more conventional historiographical and hermeneutic approaches. For Weber, such explanations relied on idiosyncratic or individual personal motivation. In addition, within the circle of German scholars with which Weber most clearly identified, a debate was underway over the relative strength of his own account published in 1905 of the origins of Capitalism in the commercial practices and religious ethic of the Protestant sects and Werner Sombart's account (which appeared later) of Capitalism's roots in the adoption of usury as a prominent business practice among the secularised Shephardic Jews of the Venetian and other trading empires. For Weber, the ideal typical outlook of Jewish religiosity after the diaspora was a form of *ressentiment* expressed in a rationalisation of Jewry's pariah status and was inconsistent if not wholly at odds with that of capitalistic practices.[9] The arguments in *Ancient Judaism* are derived deductions from

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7 This section is much longer and more detailed than we would have preferred. This extensiveness was prompted by a concern the frames of reference our readers might bring to *our* account of *Ancient Judaism* would not be congruent with the frames of reference Weber's readers brought to *his*. The latter were the frames which guided his writing. Since the solutions he provides to the interactional, rhetorical and documentary problems of conditional relevance are designed against just those frames, we have included more of the "background detail" than might otherwise have been needed.

8 This is where we part company with Sacks' thematic motif of judicial procedure and move towards our own. See below.

9 In *The Social Psychology of World Religions* [Weber, 1948, p 270 – 72], he warns against a too simplistic adoption of Nietzsche's concept. Rather than 'hostility' or 'jealousy' derived from relative deprivation of whatever kind, Weber attaches it to the (subjective) experience of suffering. The experience of suffering is taken by Jews to represent the condition of "odiousness" in other people's eyes and as a sign of secret guilt. If fortune favours the brave and the good then misfortune is attached to the unworthy (a "theodicy of good fortune"). With Judaism, it is

the materials in the Old Testament pressed to demonstrate the endogenous nature of that inconsistency.

The contents of *Ancient Judaism* do not consist in a single thought developed over the fourteen or so essays in the main part of the book. They are an ensemble of individual pieces presenting an array of symmetric suggestions regarding the relationships between societal, social and economic conditions (including changing political structures) and an evolving ethic which eventually fuses political and religious sentiments into a theodicy based on the distinctive identity and destiny of 'Israel' as the chosen people. For Weber, this relationship is a functional correlate to the kind of "elective affinity" which he identified between the socio-economic experience of the Protestant sects and the ethos of Capitalism. He sets this conception out very clearly and it is worth quoting it at length.

> Now the point is not that the life conditions of the Bedouins and semi-nomads had "produced" an order whose establishment could be considered as something like the "ideological exponent" of its economic conditions. This form of historical materialistic construction is here, as elsewhere, inadequate. The point is, rather, that once such an order was established the life conditions of these strata gave it by far the greater opportunity to survive in the selective struggle for existence against the other, less stable political organizations. The question, however, why such an order emerged at all, was determined by quite concrete religious-historical and often highly personal circumstances and vicissitudes. Once the religious fraternization had proven its efficiency as a political and economic instrument of power and was recognized as such it contributed, of course, tremendously to the diffusion of the pattern. Mohammed's as well as Jonadab ben Rechab's religious promises are not to be "explained" as products of population phenomena or economic conditions, though their content was co-determined thereby. They were, rather, the expression of personal experiences and intentions. However, the intellectual and social means which they utilized and further the great success of creations of this very type are indeed to be understood in terms of such life conditions. The same goes for ancient Israel. (Weber, 1952, pp 79-80)[10]

The factual status of the Old Testament's account of the origin and development of the Jewish nation from Abrahamic times to the setting up of the kingdoms of Israel and Judah is understood not to be the issue. At no point are these 'facts' evaluated. Rather, what is at issue is the understanding post-exilic Jews came to of that history. Weber's readers knew contemporary

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the suffering of the community (in times of challenge and distress) rather than the individual which was expressed as *ressentiment* and channelled into the discounting of pariah status with a theodicy of salvation and the promise of future glory. See also Weber [Weber 1949 p. 190] where the pariah community is held to sublimate its lack of esteem etc in a religiosity of rectification in which 'The Last will be First' or in some future 'messianic salvation'.

10 Hereafter, quotations without name and date references are from Weber's *Ancient Judaism*.

Archaeology and Ancient History generally held the sequence of events described in the post-Abrahamic sections of Genesis to be broadly correct. But he and they also knew there were already doubters even if Jewish Talmudic scholars tended to demur. Available non-Biblical evidence collected from excavations and the few available and relevant documents of contemporaneous cultures did not mention a migration from Mesopotamia to Canaan, an exile of Hebrew people in Egypt, an invading force of liberated exiles from Egypt subduing the tribes in the western Levant, the demolition of the walls of Jericho and so on. Perhaps even worse, no consensus was to be found on who or what made up the Davidic kingdom nor, indeed, whether there was a Davidic kingdom. According to the majority opinion these days, if there was it was likely to be a small set of hilltop towns and fortresses under the control of local warlords among whom David and his son Solomon were preeminent. What was accepted then and remains so now is that Nebuchadnezzar did destroy the Temple of the Jews in 586-7 BCE, enslaved only the Royal, warrior, priestly and administrative classes and carried them off to Babylon.

The emerging dissensus Weber was aware of has coalesced into two possible alternative accounts of the historical origin of the Jewish people as a politico-religious grouping to that presented in the Old Testament. The first is they emerged as the dominant force following the conquest of North Eastern Egypt and Canaan by the Hyksos culture after 1800 BCE, though quite where the Hyksos originated is unclear. The second is that they were the leaders of defensive alliances among some of the tribes in the western Levant in the face of invasions from Egypt, Greece and Mesopotamia (Assyrians, Akkadians and Persians). These alliances or confederations culminated in the formation of the two kingdoms of Israel and Judah in the central Canaan region. The populations of these kingdoms were not genetically homogenous, neither was there a single religious practice. Among the monotheisms, though, the Yahwe cult was of relatively minor importance. There are just two known inscriptions which *could* be references a group called 'Israel' in the 1200 to 1400 BCE period.

Most importantly, it was agreed then and is agreed now the main body of the first 5 books of the Torah was composed in the period immediately after the return from Babylon and the following four a little later. They were written by the Temple elite (the Levites) and (other than by fundamentalist Talmudic scholars) are usually presumed to offer a rationalisation of the need for an authoritarian religious state (a Jewish version of Oriental Despotism) to provide internal stability and external security. The personal warrior god, the covenants, the pariah status and identification with a specific geographical region are all fused in the ideology of the uniqueness of Judaism.

Section 2. The Praxeology of Possible Worlds Historiography

With all this taken for granted background in place, we can now turn to the structuring of Weber's case. Here we turn to our own analytic motif to frame our (re-)construal. We will use the form of a "well-set problem" as that is usually understood in the physical and natural sciences to represent the formal properties of the social object 'the making of a sociological case' as a sub-species of the phenomenon 'reading sociologically'. The latter is, of course, itself a specifically modified application of the universal common-sense two-part problem/solution structure. We will use this analogy as a general guiding framework within which to construe how Weber organises his case.[11] Just to be clear. We are not claiming Weber oriented to this framework. Neither are we claiming Weber was seeking to model his analysis on the procedures of the natural sciences. All we *are* saying is the construction of a written/read course of reasoning requires an organisation and we are using this motif to point to some of the formal features and their interconnections which Weber's organisation displays. It will, if you allow us to offer a mixed metaphor, help us shape a presentation of the architectural anatomy of the sociological case which Weber gives.

PROBLEM SCOPING

Weber states the problem right at the beginning of Chapter 1. The significance of Judaism is to be found in two interconnected processes:

1. Unlike in India, Jewish society developed a caste system of 'in' and 'out' groups in which the religiously privileged in-group was also socially stigmatised. This paradox was rationally reconstructed by the Old Testament as a collective representation of the unfolding of God's will that Jews be a *pariah community*. This stigmatisation is only to be reversed with the arrival of the Messiah or through salvation in the afterlife. The caste system and its collective representation explain the various crises Jewish society passed through in historical and contemporary periods and Judaism's responses to them.
2. The Pauline intervention in the setting up of the early Christian cultic religion adopted a reading of the Old Testament stripped of the rationalisation of the pariah status of adherents. Christianity became a salvation religion without the need for 'this worldly'

[11] In other studies, our use of the analogy has been tighter. For a description of the structure in its home discipline, see Wilson [2017 and 2019]. In our view, Mark Wilson's explicitly philosophical studies of the practical work of constructing and resolving scientific problems stand as a massive untapped resource for ethnomethodological investigations of technical disciplines.

> stigmatisation. This is what underpinned its role as Rome's Imperial religion and later the development of the Medieval Church as a bureaucratised state religion and all which has followed in the West from that process.

Clearly, the question Weber is addressing is not what would subsequent world history have been like *without* the Israelite collective representation of themselves as a pariah community but why did they develop it in the first place? Why did it happen in that society and not in India? Of course, one can endlessly muse on the possibilities of counterfactual history, but for Weber the question is simply: Of all of the possible ways the social structure of that particular group of nomadic tribes at that particular point in Bronze Age Levantine history could go, why did it go that way?

Registration[12]

Alongside the considerable taken for granted evidential background used to frame and address his problem, Weber has to isolate and abstract particular features of the history of Israelite society from the welter of material provided in the extant sources and the Old Testament. Following Wilson (see note 11), we call this process of extraction 'effacement'. By effacing detail irrelevant for his analysis or which he cannot accommodate within it, Weber deliberately constructs a 'small world' whose conditions he can explicate, control and examine.

Under this registration, history is conceived simply as a structure of collectively encountered forces and collectively developed responses. The forces are socio-economic and the responses are modes of rationalisation. These are all that Weber wants to track. Dynastic histories, the histories of Imperial expansions and contractions, technological history, the history of trade and commerce etc., etc., etc. are only relevant if they impinge in a direct, demonstrable way on the sequencing of modes of rationalisation. A very clear example of this is his treatment of the adoption of the war chariot by the leaders of the Confederacy. This technology had secured Egyptian military dominance. However, it could only be afforded by the very wealthy. Not only were its materials expensive to obtain, using chariots required horses to be kept solely for its use. Unlike oxen, horses did not fulfil the function of draught and stock animals. This meant estate holdings had to generate sufficient surplus to allow the costs of the innovation to be covered. As a result, its adoption created a cadre among the leadership class which, over time, developed into the war lordship of the Davidic monarchy. The war chariot was given a place in the religious panoply though symbolic connection to the Ark of the Covenant as it was wheeled into battle at

12 For a full discussion of this term, see [Smith 1996]

the head of the army. Surrounded by his similarly mounted lieutenants, the war god Yawhe drove into battle on his chariot.

Notation

Although he does not talk explicitly in these terms, Weber clearly uses an evolutionary trope as the motor of his analysis. At the level of social structure, history is a sequence of mutations generated by rationalised collective responses to socio-economic causes. This history is a punctuated evolution, though. Within Weber's sociological reconstruction of the structure of the standard historical account, the story of Israelite society can be viewed a history of adaptation to the contingencies of serial migrations and settlement. Where the Old Testament and accepted history saw these events as divine in origin, Weber treats them as endogenous, their divinity being an endowment of their rationalisation. The endogenous history of societal evolution is a kaleidoscope through which Weber wants us to view the events narrated in the Old Testament. Each mutation is yet another turn of his kaleidoscope.

Within the compass of this evolutionary trope are two others which shape the account he is developing. The first is a strategy of conceptual substitution. The sociological epoché of the social attitude is not a method of doubt. It is a mechanism of substitution whereby sociological considerations displace their social counterparts. It is this substitution which underpins the theory of societal evolution. Elsewhere, it can be discerned in the way leading ideas are introduced. Take the core notion of *berith*. This refers to the giving and reciprocal assumption of a commitment to holding to one's 'word'. What you can expect others to do. Who will do what they have committed to and, just as importantly, who can't be trusted to do so Both are mundane matters of concern around which social transactions coalesce. Trust of this kind is not peculiar to the Jews or to the Semitic tribes of the Middle East. Weber introduces it by positioning it as an ethic of governance rather than a matter of everyday practical consideration. In so doing, he lifts its significance to the level of the value rationality of socio-political institutions. Among early pastoralists of what is now Northern Israel, Syria and Eastern Turkey, he tells us, *berith* was owed only to one's kin, one's neighbours and those who were 'guest people' in one's land, the *gerim*. The central purpose of exchanges of *berith* was support in time of war. Even though, after the exile, one still needed to manage the mundane task of stable courses of personal interaction, Weber is clear at that point *berith* was owed only to those who shared commitment to the theodicy of Yahwe as the supreme God of the chosen people. The boundaries of this allegiance circumscribed the obligation of trust. As a consequence, matters of the traffic of daily life have been subsumed into a construal of the socio-historical sequencing of an evolution of religiously validated socio-political institutions. Weber is not unique here. Construals such as this are standard patterning devices in much sociological theory.

A third trope is ad hoc synecdoche. For Weber, this is an important selection device. Using it, he develops a profile of 'significant moments' in the flow of historical events. Such moments have significance because they consist in a nexus of contingent conditions where the evolutionary possibilities could have gone a number of ways. Rather than presenting these conditions in terms of their place in a teleology, Weber reverses the dependency. He looks at the evolutionary history from the point of view of the particularities of an unfolding of series of individual clusters of conditions. Of particular import is the set of social conditions under which the Jewish community lived towards the latter part of the Babylonian exile. (See below.)

What all these tropes turn on is an unstated premise of Weber's design, namely he and his readers share the predisposition to use that form of common-sense reasoning which Hume referred to as a "habit of the mind". This is an inclination to see temporally ordered objects as causally connected. Utilising selective *post hoc propter hoc* sequencing as a principle of conditional relevance allows Weber to organise what otherwise might be seen simply as time separated contingencies into a pattern of significant turning points. This pattern is the solution to the problem he has set himself.

Problem Specification

What we have been talking about so far is Weber's general orientation towards his initial problem. In *Ancient Judaism*, he builds a mosaic of analyses using this orientation. Each takes a dimension of society and traces its evolution by construing them through the kaleidoscope of mutations described above. This has two effects. By elaborating the logic of that account against the particulars of each dimension. it builds explanatory momentum for the description he is constructing. We cannot follow him in his reviews of the physical, economic and social environment at the time of the Patriarchs, the continuous re-definition of the notion of a 'covenant' and its role in the formation of the Confederacy in time of war nor the emerging administrative structure of Yawhe worship and the place of the Prophet. Instead, we will focus solely on the way the emergence of the concept of the Israelites as a pariah community is specified.

Here are Weber's clearest statements of the evolutionary path he has to trace.

> Without the promises of prophecy an increasingly "civic" religious community would never voluntarily have taken to such a pariah situation and gained proselytes

> for sharing it with world-girdling success. It is a stupendous paradox that a god does not only fail to protect his chosen people against its enemies but allows them to fall, or pushes them himself, into ignominy and enslavement, yet is worshipped only the more ardently. This is unexampled in history and is only to be explained by the powerful prestige of the prophetic message. This prestige rested, as we saw, externally on the fulfilment of certain predictions of the prophets, or more correctly, on the construction of certain events as the fulfilment of prophecies. (p.364)

and

> Prophecy together with traditional ritualism of Israel, brought forth the elements that gave Jewry its pariah place in the world (p. 336).

System Laws

The quotation from *Methodology* at the beginning of this discussion contains the phrase "our 'nomological' empirical knowledge". We should approach this notion carefully. By it, Weber does not mean determinate laws of social history such as Michel's famous 'Iron Law of Oligarchy', Marx's universal 'contradictions of capitalism' or Schumpeter's immutable "forces of creative destruction". What he is talking about are social regularities, recurrent features visible over the course of historical events. In *Ancient Judaism* as elsewhere, two especially are emphasised. The first is the increasing rationalisation of individual and collective representations of the natural and social orders. This is his famous "disenchantment of the world". Second there is the imbrication of both instrumental and value rational considerations determining relevances for individual and collective choices. In all spheres of life, both instrumental and value rationality are present and normative. What drives historical evolution is change in socio-economic conditions (largely but not entirely shaped by means-end rationality) together with changes in how those causal forces are collectively represented. As we will see, particular configurations are the outcomes of choices over value rationalities (for example modes of governance) are made on instrumental grounds by those responsible for the management of collective representations (in the case in hand, the prophets and the priests).

Boundary Conditions

These set the limits on forces and events to be considered. They operate at two levels. First there are global boundaries to the socio-historical space Weber is describing. This space has temporal, spatial and related cultural limits. The temporal boundaries set are the Abrahamic migration and the post-Babylonian exile re-settlement. The spatial boundary is that of the western Levant. The cultural boundary at any stage is the configuration of the Israelite community and its *gerim* or metic population of guest workers at that point.

There are two obvious features to this boundary marking. As a cultural agent, Abraham exists as a pastoralist and a Yawhe devotee *and that is all*. He is not a bearer of Mesopotamian culture and religious practices being seeded in Israel. As far as this history of Judaism is concerned, it starts with Abraham. Similarly, under this particular specification, it stops with the settlement and the creation of the state religion. As there is no causal pluperfect history so *there is no causal future perfect history* either. There is no interpretation of the post-exilic regime gleaned by looking forward to the later history of the Jewish state and the regular eruptions of tension between adherents of this worldly and other worldly redemption by which, at various junctures, it was marked.

Initial Conditions

There are only two initial conditions given for the process which culminated in the development of Jewish community's self-identification as a pariah community. The first is the combination of the gradual assimilation of the metic *gerim* into the body of religious adherents and an increasingly rigid drawing of the boundary between the ethnic, territorial and religious identity of followers of Judaism and others.

> In Israel, originally, ritualistic segregation from strangers was totally absent and exclusiveness according to type received its special accent only in connection with the development into a confessional association. This transformation of the Israelite community began, to be sure, under the influence of the Torah and prophecy even before the Exile. Its first expression was the increasing inclusion of the metics *(gerim)* into the ritualistic order. Originally, the *ger,* as we saw, had nothing to do with ritual. Circumcision was not an exclusively Israelite institution. Among Israelites it was obligatory only for the army. The Sabbath was a day of rest diffused, presumably, among full Israelites and perhaps beyond the circles of Yahwe adherents. Gradually it attained the status of a rigid command of the religious ethic. That the *ger* was permitted to be circumcised and then admitted to the Passover meal (Ex. 12:48) was doubtlessly an innovation determined by the pacifistic transformation of the pious circles of Yahwists. This became (Num. 9:14) a duty of the *ger*. The enjoyment of blood (Lev. 17:10) and the Moloch sacrifice (Lev. 20:2) had probably earlier been forbidden to the *gerim* by threat of capital punishment and, above all, he was required to observe the Sabbath. The Deuteronomic and finally priestly doctrine (Num. 9:14; 15:15, 16) destroyed all ritualistic differences between full Israelite and *gerim*. (pp 336-7)

The second is the partitioning of the Jewish community into a left behind 'remainer' population made up of peasants and urban artisans in Judea and nomadic pastoralists and stock breeders in Samaria on the one hand and the population of translocated Royal, administrative, warrior and leading priestly, business and commercial classes exiled in Babylon on the other.

Operating Conditions

These relate to the different paths the two populations followed and the need to deal with their inconsistency once partition ceased.

1. Left largely to themselves by their Persian Suzerains, the remainer population gradually accommodated itself to elements of Persian rule and Persian culture. At the same time, incoming Mesopotamian settlers, especially in the North, adopted a syncretic religious observance of local and Persian practice. The result was the gradual emergence of a relatively tolerant multicultural, polytheistic melting pot. Not surprisingly, the exiles took precisely the opposite path. From the start, they refused to accept or be accepted into the Persian culture to which they had been translocated and maintained a ritually enforced segregation from it.

2. Over time the exiles were less and less constrained by the Persian and Assyrian authorities. They were freer to take up commercial, administrative and public roles as well as to move around Babylon and its environs. This could have resulted in cultural diffusion and eventual assimilation into the surrounding socio-cultural environment. But it did not. Weber condenses the cohesive centripetal familial, cultural, economic, political, religious and other forces at work into three ideal-typical religiously imbued forces:

    a. Residential concentration in the neighbourhoods of kosher butchers had the effect of creating 'Jewish' and 'Jewish-dominated' *arrondisments*;

    b. The exclusionary character of rules about sharing meals or forming marriage contracts meant socially valued ties of kinship and friendship were increasingly community bound;

    c. The observation of the Sabbath as a religiously enforced day of rest placed constraints on performance of activities and obligations during immediately preceding and subsequent days. Managing the consequences of these for business, trade and other commercial relationships led establishments, workshops and similar ventures gradually to segregate into Jewish and non-Jewish types, each with their own 'customer' and 'supplier' bases as well as their own contract rules, conventions and practices.

The inclusivity of the remainer population was directly at odds with the rigid exclusivity which characterised the 'exiled' community on its return. The structures of governance of daily life (the 'Law') could not accommodate both. Writing the Torah provided an opportunity for the returning priestly caste of Levites to re-interpret the boundaries of the Jewish community and with it the expansion of the scope of *berith*. The inclusion of the gerim as participants in the Covenant reinforced the exclusivity of the in-group/out-group distinction.

> Hereafter, one law shall be for the Israelite and the stranger for all time to come. (The obviously late addition Ex. 12:49 agrees with this.) According to Deuteronomy 29:11 the *gerim* belong to the union with Yahwe, and in the Book of Joshua 8:33 this is even incorporated in the Shechemite curse and blessing ceremony. (The late prescription, Deut. 31:12 hence expressly stipulates that the Torah should be publicly read also to the *gerim*.) The driving forces in this process were the demilitarization of the Israelite peasants and town farmers in connection with the interest of the priests in the patronage of the *gerim* among whom such exemplary pious people were to be found as the Yahwistic stock breeders—while the "preeminent," in the account, figure together with the Korahites in the latter's insurrection as opponents of the priests. The politically disqualified or less qualified strata were here, as often elsewhere, an increasingly important field of work for the Levites, and in the Exile, for the priests. (p 337)

3. Increasing importance attributed to the Torah as the collective representation of Jewish history and destiny and the shift in the nature of prophecy's legitimation from charismatic authority to administrative or bureaucratic authority reinforced the new order. While undoubtedly Jewish parents told their children legends and fairy tales and there would have been stories, gossip and 'urban', 'local' and other kinds of 'myths' in circulation, the Torah became the *de facto* and *de jure* account of the origins and distinctiveness of the Jews. It was 'the Word' in terms of which sacred and secular events were to be interpreted.[13]

4. Prophecy was undergoing change during the long cultural moment Weber is focused on. The practice of oracular divination, fortune telling, augur interpretation and associated 'magic' was commodified as a saleable skill and hence made profane. What was being purged, prophecy proper if you like, was the ecstatic practice in which charismatic individuals legitimised their dire prognostications by reference to direction from the hand of Yahwe. Just as is the case with any form of Canon Law, over time circumstances changed, interests shifted and interpretations were re-jigged. The outbursts of the prophets were

[13] To appreciate what this might mean, imagine Henry VIII had insisted Geoffrey of Monmouth's history of King Arthur be the keystone of the Church of England's doctrine. With that as the religiously validated foundation myth for Britain and the British, would the subsequent Stuart, Hanoverian and later settlements have been possible without sparking the outrage of some 17th,18th or19th century Nigel Farrages and their followers?

almost always reactive denunciations or promotions of such re-jigging. With the commodification of prophecy, such interventions became fewer and further between.

The dynamic of the Torah and prophecy structured the Jews' religiously framed outlook on the world. As such, the socio-political and economic dissolving of the boundaries marking off Jews from their guest people was endorsed by both the Torah and the prophets. Since the dynamism of the thematic was wholly concerned with representing the Covenant Yahwe had with the Jews, necessarily what was taken as the identifiers for being a Jew represented a key part of that Covenant. The combination of Torah legitimation and the rationalisation of prophecy is the fulcrum on which the ritual (i.e., sacrosanct) character of the relationship between the Jews and the *gerim* (i.e., the in-group/out-group boundary) turned.

ANALYTIC PROTOCOLS

Analytic Procedures

The inclusion of the *gerim* in the ritual order meant a broadening of the boundaries of religious distinctiveness which had defined what being Jewish was to mean for the Jews, namely being the only Chosen People. Once included in the ritual order, others could be 'chosen' too. The adverb "Hereafter" at the beginning of the opening sentence of the second paragraph we quoted above is critical here. From this point on, the causal flows are in motion and Weber assembles their interrelationships as they move through their pathways towards the hierocratic state of post-exile Judaism and with it the political dominance of the clergy and the rational reconstruction of God's will for the Jews as the fate of being a pariah people.

This interpretation contains elements of both instrumental and value rationality as the relevance structures generating the post-exile mutation in Judaism. Persian occupying forces had a standard approach to the management of defeated peoples. They worked through the religious organisations to encourage acceptance of subjugation. In return, they allowed extant religious practice to continue relatively unchanged. Such preferential treatment offered the priests the opportunity to extend their influence and reduce the role and sway of prophecy.

> The increasing bourgeois rationalism of the people integrated into the relatively pacified world, first of the Persian kingdom, then of the Hellenic, had given the priests the opportunity to suffocate prophecy......(T)he social structure again substantially co-determined the form of piety of the Jewish community, which was then stripped of prophetic charisma. (p. 382)

In return for this opportunity, the priests constructed the Torah around a theodicy which fused elements of traditional prophetic revelation and the centrality of the Covenant with a salvationism which legitimated this worldly suffering. Yawhe was the One High God for whom the Jews were the Chosen People. He had formed a Covenant with them to protect them and ensure their prosperity. In return, he required absolute obedience to his prescriptions. When the Jews transgressed, he used worldly forces to punish them and drive them back to the path of righteousness. In developing the litanies of transgressions denounced by the prophets in the Torah, the priests emphasised exclusivity of marriage arrangements and dietary restrictions as markers of obedience to the requirements of the Covenant. Restrictions on 'connubium' and 'commensalism' reinforced the tendency of Jews being viewed and viewing themselves as a people apart, socially distinct and socially stigmatised. Judaism was a community *against* the world, bound together by *berith* and owing nothing to outsiders. A community, that is, whose view of themselves and their condition is powered by *ressentiment*.

Analytic results

The illustration Weber gives of the way this theodicy is expressed in practical life is as a dualistic ethic of economic relations. This is where he brings this whole discussion back to the issue of the origins of Capitalism. He draws a contrast between attitudes of Jewish and Puritan socio-religious outlooks towards usury and other practices. For the Jews, some had argued the taking of interest from outsiders was a religious commandment. For others, though, while forbidden within the community, with 'strangers' it was harmless and so was perfectly acceptable.

> ….(T)here was no soteriological motive for ethically rationalizing outgroup economic relations. No religious premium existed for it. That had far reaching consequences for the economic behaviour of the Jews. Since Antiquity, Jewish pariah capitalism, like that of the Hindu trader castes, felt at home in the very forms of state and booty capitalism along with pure money usury and trade, precisely what Puritanism abhorred. (p. 345)

A dualism of this kind was entirely missing from the outlook of the Puritan sects of the 17th and 18th centuries and had been from the start. They…

> …pointed with pride to the fact that precisely in economic intercourse with the godless, they had substituted legality, honesty and fairness for falseness, overreaching and unreliability; ……….the godless prefer to patronize their shops, their banks and their workshops before all others. (p. 344)

The traits which characterise the rationality giving rise to modern Capitalism, fairness, honesty and the willingness to forgo apparent opportunities for personal advantage are exactly those which are absent in the economic structures of the Middle East, North Africa and Southern Europe where modern capitalist practices did not arise nor take immediate root.

The relevant forms of business practise are thematised as religiously valued out-group practices performed by two groups both of whom saw the need to maintain a clear moral separation of themselves from "the godless" with whom they were enjoined to interact. In this sense, for Weber they are 'identical'. The same order of motivational causality is in place but the ideal and material interests at play differ. That is, the forms of relationship are legitimised in terms of a common desire for religious benefit, but the conditions for deriving that benefit differed. Judaism had no conception of 'inner-worldly asceticism', the maintenance of which engendered a relatively open range of "with-in" and "with-out"" transactions for the Puritan. The same 'work' could be carried out for both and the same business terms could be set for both. For the Jew, such transactions were strictly limited to those licensed by canonical law, as usury was. Thus, the two "possible worlds" took their own evolutionary paths.

Each modality is a rational choice under the circumstances in which the sets of economic actors were making the decisions they did. The solution to the puzzle of their incongruity is the rendering of economic activities as religiously motivated under different ethical outlooks and different historical circumstances. The actions are impelled by 'the same' general causal motivations but enacted in very different ways. That generic 'solution' is, of course, precisely what Weber claims regarding the transformation of *berith* and the covenant pre- and post-exile and on an even broader canvas, Judaism's *ressentiment* and its realisation in the obligation to accept its nature as a pariah community. Moreover, it is that very nature which prevented it from having the role which Sombart attributed to it.

Section 3. Conclusion

In his intellectual biography, Reinhard Bendix [Bendix 1966] tells us Weber's sociological outlook was fixed by his earliest studies of agricultural labourers and Stock Exchanges. In both, Weber described groups who seemed to act in ways which were at odds with what might have been thought to be their obvious *economically rational* best interests. Agricultural workers east of the Elbe declined to serve as 'indentured' workers on large estates even though such an arrangement would greatly increase their security and standard of living. Instead, they preferred to remain economically precarious day-labourers. Stockbrokers in Manchester, London and Hamburg chose to submit to self-regulation even though this would necessarily limit their ability to engage in trades which would maximise their opportunities and their returns. In both cases, economic activities were suffused with non-instrumental values and choices over the possibilities of action guided by both returns and their meaning. The ideas that labour was just a commodity to be sold to the highest bidder or 'business was business' and shielded from ethical or moral considerations were entirely foreign to them.

As with the early studies so it is with his analysis of the emergence of early modern capitalism.[14] The socio-economic conditions favourable to the development of capitalism were present in many societies at various points in history. These conditions provided for the possibilities of historical development. What determined their actual course was the meaning structure within which those possibilities were evaluated. In this respect, *Ancient Judaism* was to be seen as the complement to (and hence conditionally relevant on) his much more famous *Protestant Ethic*. It is a complement because it is a rebuttal of Werner Sombart's counter study. Sombart argued it was purely the facts first that the Jews were a diaspora community with close familial and kin networks holding to the *berith* norm and second that they were allowed to lend money at interest when late medieval Christians in the trading cities of Italy, France, Germany and England were not which were the precipitating conditions for the emergence of institutionalised banking. And it is true, the institutions of credit, brokerage and their management were required for capitalism and the trust between members of Jewish extended kin groups facilitated their development. But for Weber, these were not what made modern capitalism distinctive. Its distinctiveness was to be found in the moral order of its self-regulated practices.

This moral order was entirely different to that within which the Sephardic communities operated, a moral order which did predispose rational economic action but of a kind which could not have led to the forms which modern capitalism took. The Protestant dissenting sects, like the Jews, saw themselves as a people apart. Like the Jews, they saw worldly success to be a mark of divine favour. And like the Jews, that success necessitated commercial interaction with non-believers. But the ethical stance taken towards the non-believer was different. The same moral obligations were placed on dissenters in their commercial engagements with non-believers as with believers. This was a unified moral outlook. For the Jews, it was entirely different. A dualistic, in-group/out-group moral order was in place which justified practices with non-believers which were forbidden with regard to fellow believers. This dualism had its origins in the construction of a collective representation of the 'Being' of Judaism as a pariah community developed as part the re-

[14] It is important to remember Weber is not talking about modern Corporate Capitalism, State Capitalism and their many modern versions. His interest is in the ethical world of a particular section of the mercantile classes in the 16th and 17th centuries. That the capitalism this outlook engendered developed into the rapaciousness of 19th and 20th century is a proper subject for a Weberian analysis.

writing of the early history of Jewish society and its relationships to Yawheism after the exile to Babylon. Of course, this representation was itself a rationalised history suffused with instrumental and value orientations.

Ancient Judaism traces the path Judaism took from a polytheistic natural religion to a prophetic war cult and eventually to a bureaucratic state religion. At each stage, the theodicy defining the relationship of believers to Yahwe underwent revision and emendation. With the Torah, it became a soteriology of this worldly odiousness and rejection to be compensated for by the realisation of the Jews status as the Chosen People either through a Messiah or at the Final Judgement. This soteriology was encapsulated in the acceptance by the Jews of their inclusion in a pariah community.

Weber's presentation of the Bronze Age history of Israelite society is a paradigm of how to make a sociological case. But it is a case made indirectly and, one might say, in counterpoint. The extreme competence, aptness and efficacy of its design are demonstrated as much in what he does not say as what is included in his text. What he chooses to leave out is that which he can assume his readers know, can see for themselves and most importantly can conclude for themselves. The conditional relevance of this knowing, seeing, interpreting and concluding is given both by the completeness and locally produced logicality of the internal order of the argument made and by the references, notes and pointers he provides. In that respect, *Ancient Judaism* is rather like a mathematical proof or a set of accounts. When it "works", much of what makes it work is not what is set out on the page but what is called up, touched off, resonated with and altered in the heads of its readers. Weber provides just enough guidance, just enough reconstruction, for the train of thought he constructs to be visible and the steps in reasoning to be found plausible and justified. This is Weber's achievement. His is competence *nonpareil* in the production of plausible sense assembly within a structure of sociological reasoning. Weber was not simply writing a history of Judaism. He was building a case which convinces; a demonstration of why the open possibilities of history turned out the way they did. And, as a case of sociological reasoning, it is unsurpassed.

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Part II

Social Epistemology and Sociology

Introduction

The essays in this Part are commentaries on various attempts to adopt the results of sociological investigations as premises for epistemological arguments. Two concern recent debates in the Philosophy of Science and take the form of extended book reviews. The third is a little different. To begin with, we take a reasonably long view. We do so by tracking the rise, evolution and eventual implosion of Standpoint Theory as an epistemology for the feminist movement. In addition, although Critical Theory is often called in to bolster Standpoint Theory, its fundamental precepts lie is a re-purposing of Marx's economic analyses. Since many strands within Sociology avail themselves of precisely the same re-purposing, we feel justified extending our concern to encompass that re-purposing tactic.

That knowledge is socially distributed is the foundational axiom of any Sociology of Knowledge. There is, though, far less consensus over what 'knowledge', 'social' and 'distributed' are to be taken to mean. Based on this axiom, any exercise in the Sociology of Knowledge examines the arrangements, practices and other goings on in some part of some social world and tries to show how those goings on enforce, control, determine, shape, influence or effect (choose your own causal verb) what social actors think, believe, take to be true, are prepared to accept, and so on. The result is a narrative of empirical alignments. Here the pattern of social arrangements. There, the pattern of (social) cognition. Despite what sociologists might sometimes say about their work, these exercises demonstrate how the axiom can be applied to describe social relationships. They do not *prove* the axiom.

That the knowing subject is an individual rationalist (and moreover a radically individual rationalist) confronting the natural, social and psychological worlds in which it lives is the foundational starting point for traditional Epistemology. From this position, Epistemology elaborates chains of logically derivable (i.e., provable by means of logical deduction) arguments for how this rational homunculus might possibly (not 'does') acquire justifiably true beliefs about the worlds it confronts. The result is an analytic framework (an architectonics) of transcendental epistemic arguments.

The Sociology of Knowledge and Epistemology clearly are not horses of the same or even different colours. They are species of entirely different genera and habitats. Any yoking of them is highly likely to have unfortunate consequences. But this is precisely what some philosophers wish to do. They replace the radically individualist Cartesian Knower with the (socially) Situated or Contexted Knower and deploy the demonstrations of the Sociology of Knowledge as the premisses for their own chains of transcendental deductions in order to show traditional Epistemology's premisses to be unwarrantable.

It should not take too much reflection to see this transposition is more than a little hazardous, if for no other reason than it manages to combine a category mistake (the yoking) with premise denial (the substitution). Our commentaries examine some of the unhappy consequences of which flow from this combination. In all three cases, we come to the same conclusion. When applied in earnest with regard to actual settings, the hoped for analytically liberating theory of knowledge and knowing (and, by extension, of social life in general) comes dreadfully unstuck. What results is the ironicising of scientific practise on the one hand and the clamour of warring factions squabbling over Balkanised modes of oppression on the other. In sum: unhappiness all round.

A final note of clarification is in order. At various points in the essays presented in this Part, we refer to 'social epistemology'. We are using the term to designate the premise stipulating move described above. We are not using it to invoke the programme of work proposed and promulgated under the same name by Steve Fuller. See [Fuller 1991; Fuller 2018]. We accept the authors of two of the pieces we discuss might well identify their work with the reflexive aspirations of Fuller's enterprise. However, teasing out the appropriateness of claiming such relationships would require us to undertake a major review of the paths taken and not taken and the rationales for the choices made thereby over the more than a quarter of a century that Fuller has pursued his vision. Such a task would undoubtedly involve plunging into the contentious running debates Fuller has been party to both as instigator and respondent throughout that time. See [Fuller 2012] for one instance. Hearing, as we do, Time's wingéd chariot at our backs, there are more pressing and probably more valuable things for us to attend to.

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6

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

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[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,

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[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

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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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7

A New Economics of Attention?

Introduction

In *Travelling Representations*, we were concerned with attempts to use sociological findings as premises for making epistemological claims about scientific practices. Here, we turn to the scientists themselves and their reactions to the descriptions and diagnoses philosophers and sociologists are promulgating. What we see in their discussions is not a set of disciplines flummoxed by interest driven puzzles about philosophical questions, but practical members of project teams trying to respond to the requirements of a new economics of attention. Attention is always a scarce resource in any walk of life and its allocation has to be budgeted in terms of the opportunity costs of attending to somethings rather than others. A recent colloquium, *The Natures of Data* [Fischer et al. 2020], brings out how the economics of attention may shifting in one corner of the scientific world and how those shifts are being experienced and rationalised. The volume is a reconstructed conversation between a group of biologists, an historian of science and a video-artist interested in scientific practice. The topics cover various aspects of the consequences of the current 'digital revolution' taking place in the biological sciences. Important new research procedures and processes are being developed while at the same time entirely new forms of data and new ways of handling old forms of data are emerging. These new practices are bringing about the shifts mentioned above.

The materials the scientists discuss are fascinating, providing new and different descriptions of actual scientific work. What is less fascinating (dispiriting, even) is the rationalisation which seems to be taking hold. This is couched in a Latourian post-modern idiolect in which the central pillars of what it is to do biological science are held to be under threat, prime among which are said to be the very facticity of flora, fauna, species and other key ontological categories.

Set aside the apocalyptic clamour of the applied sociology and focus on the actual work the scientists discuss and things look somewhat different. True, the scientists are having to 'retool' their research teams, accommodate new specialisms and form new partnerships. As any established researcher knows, these are not comfortable things to have to do. They take time, effort and resources (especially attention, hence our emphasising it), which one might better spend getting on with the science one wants and knows how to do. However, the discombobulation of change is not the same as the collapse of the scientific world as the scientist knows it. Only a hyperbolic sociology would see it as such. When looked at from other more or less conventional sociological viewpoints, what is going on seems entirely predictable and reassuringly normal.

Section 1. An Aside.

The Natures of Data is an output; a project product where a group of scientists (or researchers, if you want to quibble about the scientific credentials of the video artist) have co-constructed a discussion document (literally) about their science. One possible question which could be asked is about its character as a research object. What are its features as a course of biological (or meta-biological, if you will) reasoning in action? Asking this question brings out the very obvious fact that, as data, it is very different to the standard ethnographic material used in studies of science. It is an account the scientists themselves have produced about what they do and what they think it means or might mean. Drawing a parallel with another famous study, Garfinkel's Pulsar Paper [Garfinkel 1981] was about treating a transcript of the work a group of scientists as the collective making visible of their working out of the astronomical meaning of the readings they were getting. *The Natures of Data* could be construed as the reconstructed logic of a similar exercise, the authors making visible their working out (or, at least, attempts to work out) the biological significance of some of the things they are now doing. Adopting this frame of reference forces attention onto the structures this 'research product' might display and how they might be organised. What are the requirements of/the constraints on recipient design placed on the descriptions and on the readings the audience should make as well as who makes up their audience. How is this presentation of their talking to each other shaped as a 'presentation' and for which types of anonymous others? How are these social types discernible from what they say and how they say it? How does the text provide for the self-selection of appropriate audiences and recipients? Most importantly of all, perhaps, how are these to be made sociologically visible and available? What repertoires of formulation and their notations are available to analysts of these discussions and how are the contributions the scientists make to be rendered using them?

Treating the research products as constructed forms of 'instructed reading' might open up all sorts of interesting lines of investigation. Some of them might even bear on the vexed questions of multi- and cross-disciplinary research. Hints of how this might go do appear here and there in the discussion below but undertaking such an analysis is not what we are about.

Section 2. What's Different about the Digital?

The story of Henny Penny amuses because of the universality of the trait it exemplifies. We all know people for whom any large-scale disjunction/disruption/reconfiguration constitutes wholesale revolution portending termination of 'the ways things are'. And, we have to admit, most of us have some of that inclination too. In the past, we've talked about it as the fallacy of the immediate consequential on the shock of the new. What Henny Penny (and the rest of us in our het-up moments) fail to recognise is just how much of what we do and how we live is *not* affected by whatever it is that is disturbing us right now. Covid-19 brought disruption, anxiety and difficulty. But it has not brought the end of the world. We have adjusted. We are adjusting. And while some things might be different for some time, most other things will continue pretty much as they were. Houses will be decorated; children will be brought up; tv will be watched; arguments will be had over who left the light on overnight or the fridge door open.

Precisely the same is true in the development of science. The revolutions in instrumental technologies of the 17th, 18th, 19th and 20th centuries did change the way the sciences were practised. Digital technologies are doing the same today. But what changed the character of the sciences *as sciences* were the ideas not the technologies. And the Kuhnian in all of us should force a recognition that post Galileo and Newton, we have really had only two revolutionary ideas, natural selection and quantum theory. Einstein's special and general relativity weren't really revolutionary since they were attempts to work within the classical conception and the limitations of so doing generated the break-through attributed to him. It is a well-worn story that Einstein could never really reconcile himself to the consequences of his own work.

But even those two 'revolutions' didn't change *everything*. Since the sciences are path dependent, a great deal of how you do practical Biology and practical Physics carried on much as before. Nonetheless, the path had altered and the consequences of these alterations gradually worked their way out. This is where the issues of what is significant and for whom arise. Philosophical and sociological significances have to do with philosophical and sociological questions and puzzles. And these questions do not rest on the particularities of any local forms of scientific practice or theoretical knowledge. Conceptions of ontological differentiation are not overturned because one can describe the behaviour of light in equations written for waves of radiation instead of pulses of particles. Conceptions of causal ordering are not jettisoned because our parameterisation of time yields backward running clocks. And, perhaps most important of all, the metaphysical character of 'an object' is not dissolved simply because the objects we are now discussing can be seen only via complex, highly sophisticated and not always well understood machines and their 'invisible' processes. We can talk about quarks, genes and even "digital fish" (if we must) without becoming unhinged.

The upshot of all of this is that what are posed as all-embracing philosophical issues usually turn out to be much more low-key ones of accommodation and adjustment. How do the things we want to say about the new science fit in? What needs to be flexed, re-thought, re-phrased as well as jettisoned? Revolutionary changes in our philosophical analyses don't come from the outside, from the topics/examples being discussed—particularly those of the sciences, but from within; from philosophers who radically change the questions we are asking, often by pointing out those questions are actually nonsensical.

The implications for sociological analysis are much the same. The sociological conception of human action operational in any mode of analysis is not totally transformed by the contents of the study. Merton would have said the same things about Genomics as he did about 19th century Mechanics (and so will SSST researchers). Some of the participants in *Natures of Data* occasionally lapse into Latourian idiolect, even though ANT itself is little more than the use of network imagery to extend Critical Theory. Insisting the embedded institutionalisation of ways of thought and practice plays out as the preservation of the interests of the powerful is just a tactic for making something quite ordinary and well understood sound mysterious and dangerous. So, more relevantly for the case in hand, is the notion of 'de-materialisation'. Prodded by the 'problematising' strategy of the non-biologists in the group, the scientists themselves seem to have been persuaded (at least for this occasion) to entertain such hyper-inflated notions as the 'de-materialisation' of phenomena, infrastructural 'actors', 'opaque' software, and miscegenated digital monsters.

These terms are a mechanism for dramatising the significance of the issues being discussed. Here are two quotations from the discussion of infrastructure which demonstrate what we mean.[1]

> First:
>
> **Hannes Rickii**: .... The question is: Do these elements contribute at all to shaping research work, and how does this emerge in the context of experimental work? Are infrastructures and nature considered factors of uncertainty, or do these peculiarities get lost in the normality of research work? [p.97]
>
> And then:
>
> **Hans-Jorg Rheinberger** On the whole, however, there is only one kind of materiality involved here, namely the materiality of technology. But there is also the materiality of the object of investigation. It disappears here, it is not present at all, no fish, no nothing at all. No genome. Nothing.
>
> **Hannes Rickii** The fish is there.
>
> **Hans-Jorg Rheinberger** Where is it? Here, in the aquarium?
>
> **Hannes Rickii** Precisely. And that is my motive. I realize that with development toward digital science, much more infrastructure comes between the observer and the object of research. My thesis is that the economy of attention is distributed differently. The materiality of the research enterprise is no longer related exclusively to the investigated organism. Thus, the focus is shifting to what materially makes up the thread between the animal subject and the observer. [pp 101-2]

What is being discussed here is the interpolation of various technologies such as sensors, communications devices (together with image processing, data compression, storage devices and algorithms to run them) between the object being investigated (fish in the Arctic Ocean) and the researchers in their labs. What they are bumping up against are issues of auditing the research activity. Where are the dependencies? Where is what effort expended? What do they have to spend time worrying about? Each aspect of the discussion (data, infrastructure, software, in silico etc) comes at that problem from a slightly different direction and has different consequences for "the economics of attention".

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[1] All unattributed quotations are to [Fischer et al. 2020]

Section 3. Practical Genomics

The best and certainly the longest example of the above tendency is the section labelled *In Silico*. It is worth spending a little time sketching an alternative version of what is described in this part of the book just to show it is possible to give a sociological account of what is being described without losing one's balance. The case is a familiar one to introductory Biology students (or so we are told). In many if not most species, embryos develop through three stages. Species diversification is evident in stages 1 and 3. In stage 2, however, there is a high prevalence of a very small number of forms. While there are two competing 'hypotheses' offered for this pattern, no universally accepted explanation has been provided. The pattern is referred to as 'the hourglass problem'. One potential clue, however, has been spotted in the high correlation of the 'gene expression profiles' in stage 2. One of our scientists, Hans Hofmann, has been trying to solve the hourglass problem using Genomics.[2]

A major industry in Genomics is the identification and tracking species' genes as individuals develop. This is called 'transcriptomics'. The researchers engaged in this work develop large databases of 'gene transcripts' which list the order of the key bases (labelled A, C, G, T) which make up genes for the species they have studied. What Hoffman wants to do is identify the clusters of common genes for the species being investigated ('orthologous genes') in the critical stage 2. He looked at 2,500 genes to see if he could find such clusters. Basically, he threw statistics and simulation at the problem; a strategy which he could not have used before high-speed desk-top computing and large-scale storage were available. (The point about desk-top computing will emerge later).

The first step is to conduct a Principal Components Analysis. What Hoffman has (or will have when he shapes up the data – see below) is a database of 20 samples of 2500 genes x 15 sub-stages of development containing the A, C, G, and T values for each stage for each gene.[3] The matrix of correlations of the order the bases take measures how distinct or 'distant' each pair of species is from each other. This is a high dimension matrix and impossible to scan visually or parse by hand. The aim is to reduce the dimensions to a low (that is, handleable) number. What PCA does is rotate the matrix by zeroing on the centroid of the distribution (and thus re-calibrating the co-ordinates of the genes). Each rotation reduces the dimensionality of the matrix by 1. Clearly,

[2] The scientific detail is presented and its formulation as a testable hypothesis are presented on pp 118-9.

[3] Hofmann doesn't define the matrix but talks of "hundreds of millions of data points". This is our guess.

this process can be iterated until only one dimension is left, but that would squash all the data onto a single line and throw away too much information. Instead, what researchers try to find is smallest number of rotations 'explaining' the highest proportion of the variance in the matrix. The assumption is all 2500 genes contribute to the variance and if you cluster them by clumping them in this way you will separate out grouped contributions to that variance. Following normal practice, Hofmann boils the data down to three components which, using the components as coordinates, allows convenient Cartesian visualisation of the genes. PC1 explains 83% of the variance and PC3 explains 3.1% of the variance. The question is: if these components are driving the variance, what *biological process* are they representing? What do they stand for? Hoffman says PC1 and PC2 differentiate across the species and PC3 gives the variance over time (i.e., the staged development). This makes PC3 really, really interesting. Just a "couple of hundred" of the genes have high loadings on PC3 but these seem to drive the developmental pattern. What the researchers have to do next is match up the patterns of development across the stages for the genes. Elements of this process seem to be handcrafted (part of the reason for the comment about desktop computing).

Once the relevant gene sequences are identified and matched, the second stage in the analysis simulates embryo development with the different ordered sequences. Agent-based Modelling (ABM) is used to do this. Each sequence is modelled 10,000 times (a standard number of runs in that domain) to see what the stable patterns are. For various historical and technical reasons, ABMs are designed to be run on networked workstations. Hence the other reason for using desktop computing. The rules driving the simulation are given by a tailored operationalisation of 'natural selection'. In his account, Hofmann says he has still to do this work. His summary of what it involves is this:

> **Hans Hofmann**: Every individual cell here is an agent ...... There are very simple rules. For example: If the neighbor is so, then so am I. Only there are two, or three or four rules. They are the same as for a biological process. You generate mutations, the mutations generate different phenotypes, and these are affected by the selection acting in the computer. You can define it. On the computer you can play God, if you want to.
> [p. 125]

The important point of the example and why this use of approach of repurposing data bases has implications for the practices of Biology is that the vast majority of the data Hofmann uses is not his own. That is what makes this research strategy different. What makes it sociologically non-significant (not insignificant) is that all the key issues created by its innovativeness are addressed by well understood socially organised processes. Hoffman and his researchers do exactly what we would expect them to do but in ways designed for *this* particular technique in *their* particular setting. Hoffman lists the 'troubles' he has to deal with as follows.

1. **Quality Control:** This is managed by relying on trust and data massaging. Hofmann cannot control the standards by which individual transcripts are produced so he builds his database from the data of colleagues he knows and trusts; people whose work he respects and who have track records for using standard protocols. Nonetheless, there is variation. To ensure operability across the transcripts, the data are tweaked and adjusted to ensure conformance to the requirements of his algorithms. The need for these kinds of routine adjustments is well recognised within the scientific culture to which he belongs.
2. **Standardised Analytic and Simulation Protocols**. The methods of analysis he relies on, PCA and ABM, are well known. The requirements they have and the trade-offs they bring are common knowledge. You don't have to spell them out. This is as true for the standardisation on correlation as a distance measure and the preference for three principal components as it is for the rule set and structure of the simulation 'competition' in the ABM analysis.
3. **Effacement of these choices**: In the presentation of the analytic results, the 'natural history' of the research is rendered using an appropriately chosen standardised format. 'Posters' are a particular 'rhetorical structure' for a certain kind of scientific presentation and Poster Sessions are routinely organised at scientific conferences.

Whilst the ways Hoffman and his team shape their responses might be distinctive (we have no data on this), the general structures of the actions taken are readily recognisable as typical rational patterns of action, be they in science or anywhere else. They consist in a local variant of what in *Travelling Representations* we called independent, intersubjective validation.

It seems a point made by Gilbert Ryle many years ago applies here. No new 'digital genomic world' is being constructed by these experiments in any sense other than a very attenuated and metaphorical one. What is going on is socially organised practical action taking place in the everyday world of innovating science. This routine practical action utilises culturally given forms. Nothing is hidden and the objects of investigation are not shapeshifters. The modalities of the various renderings deployed in the management of the research are visible to but not talked about by those who understand what is going on and can construe the resulting representations. These modalities are seen but never attended to *unless* they need to be.

Section 4. Realism and The Transmogrification of Fish

The conclusions we have just arrived at underlie why digitalisation is not that interesting philosophically or sociologically even though it might look as if it is or should be. We don't want to 'analyse' the following contributions but rather drop in them as illustrations of the kinds of things we have in mind. They are taken from the discussion of data but could just as easily have been drawn from elsewhere.

> **Gabriele Grameisberger** What you two are describing is a fourfold data concept. There are raw data, metadata, and data about the analysis method, and then the data about all of these data. The layers just keep piling up. When you ultimately speak of the datum you have to unravel all of this, because otherwise you have no chance of understanding this datum. (p. 40)
>
> **Hans-Jorg Rheinberger** What also seems important to me—and this may go back to my own experience as a scientist—is that, in the first instance, the step of data collection is decisive. The data have to come from somewhere. During in vitro experimentation I had the experience that the possibilities for processing at one's disposal are usually a magnitude more precise. If you have to pipette microliters or half-microliters in such an experiment, when you've done that ten times, you have an error margin in the whole process. Afterward you insert the sample in the scintillator, in the machine that counts radioactivity, and this scintillator can measure every sample for you to any place after the decimal. But all of this precision is a waste of time somehow, because the upstream imprecision is much greater. For me, the awareness for what actually hap- pens at the point of collection seems to be a prerequisite for everything else. (p. 51)
>
> **Hannes Rickii** It's a situation that is new for me. I haven't actually done such work before. Therefore, it is like a test, to establish a coherence between the data or the object and its form. But this is preceded by a concept: The point is to rematerialize aspects of this data production. I find all of these abstraction processes—from fish to dataset in the computer—extremely interesting, and I want to keep precisely this chain reversible. For me that is an interesting aspect of biology. One proceeds from a concrete organism, which then dematerializes into scientific work. My question as an artist is: How can I make this material basis that lies behind these many steps, these transformations—the infrastructures, energies, matter, etc.—how can I make it tangible? [p. 53]

The central tenet of realism of any kind is a semantic one. Terms refer. In scientific realism, this is expressed by the claim that in our best attested science, scientific terms refer. The major open issue in the discussion of realism in the applied mathematical sciences is about the realism of the mathematical terms used. These terms (i.e., functions and systems of equations etc.) are mathematical descriptions or renderings of physical objects and processes. They seem to work. We have to take them as real. But we have don't have a good account of how the empirical reality claimed can be provided by the wholly abstract devices used.

Under anybody's theory of science, Behavioural Ecology and Computational Biology are among our best attested sciences. From what our discussants are saying, it seems new representations of biological terms (categories of object and associated activities, for example) are being constructed, collected, stored and subjected to analysis but there is no story for how to track the representational reformulations and preserve the realism of the objects introduced at various points in the research 'pipeline' by using some extended logic of resemblance. When fish activity, say, is recorded as a running stream of video, we are inclined to accept the image in the video is 'more or less' the fish in the aquarium. When the image in the video is processed and stored as a set of temporal co-ordinates and an associated metafile, what has happened to resemblance? And, moreover, is it possible to reproduce (work your way back to) the original fish and their activity from these processed deconstructions? The notion of resemblance and the anchoring of it in what is available under the Natural Attitude and how that is now under strain is the central theme of this particular discussion. It is as if the notions of abstraction and simplification are being defined in terms of decomposition and the realism of the whole conceptual apparatus can only be shored up by re-composable resemblance. What is re-composed must somehow "look like" what you started out with. Interestingly, such re-composing (though perhaps not the criteria of resemblance) seems to concern the video artist as well.

The issue of resemblance is tied to the 'lossiness' of the re-description process and lurking beneath that is the problem of 'literal description'. For logicians, literal description is (formal) description in which terms are fully defined. Mathematical descriptions are the paradigm examples. It is always possible to rewrite a mathematical expression in terms of the definitions of its terms. This rests on the re-write structure of mathematical forms. In Physics the definitions of the basic terms (particle, position, sequence, time etc etc) and many derived terms (force, energy, charge etc) are given mathematically, so it is always possible to walk/work back from a derived description of a phenomenon to the basic terms of the science. The trouble is the re-descriptions used by our biologists don't have this property of two-way constitution (or, at least, they seem not to) and that generates the scientists' discomfort. As the re-descriptions multiply, resemblance and re-composition decline and the route back to the basic depiction/description of the fish in the tank is lost.

Section 5. Re-setting the Discussion.

This problem only arises if one wants to shoehorn non-literal descriptions into procedures designed for literal ones. In her account of applied mathematics, Penelope Maddy [Maddy 2000] makes the point Physics is actually much more relaxed about shoehorning its descriptions in this way than is often admitted or recognised. It happily adopts an approach which is more underdetermined (shall we say more indexical?) and where its basic terms are being drawn from the foundational Kant/Frege (KF) conceptual structure posited for ordinary life. The rigour is not in the literalness of the mathematics but in the systematicity of the analytic procedures (such as enforced strict adherence to the conventional rules by which the mathematics are deployed on the structures of KF categories). This is, of course, very similar to what underpins the efficacy of everyday descriptions. It is not the possibility of reduction to some common definition but conformity to culturally institutionalised (socially organised) precepts which fixes what counts as 'good' descriptions 'in cases such as this'.

If our biologists could give up the shibboleth of the literalness of representations, they might be less discomforted by the character of their disarticulated sequential renderings of their data and as a consequence less open to being seduced by the metaphysical siren song endlessly repeated by Latourian sociologists of science. Re-setting the issues in this way will not solve their practical problems. What it might do, though, is free up some otherwise wasted time and attention which could then be used to address them.

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8

Epistemology and Feminism

Introduction

Our discussion of Dorothy Smith's *Institutional Ethnography* in Part III notes the temporary role Standpoint Theory played as the theoretical core of Smith's sociological outlook. This, we suggest, was because of the epistemological difficulties generated by any commitment to a 'point -of-view point of view'. Smith struggled with these difficulties and failed to overcome them. Since Standpoint Theory's official role (as distinct from background influence) in Smith's sociology was so short lived, we spend very little time on it. In that context, it does not seem to bulk too large. However, if we switch tack only slightly and consider its place in feminist social epistemology, then the balance of considerations changes significantly. Nancy Hartsock's fusion, amalgam, welding, coupling (or whatever) of the social organisation of gender and sexual divisions of labour to elements of Historical Materialism [Hartsock 1998] and her derivation of "the liberatory standpoint of women" as direct analogy to "the revolutionary standpoint of the working class" was and continues to be a motivating idea for much feminist theorising. This is not to say the concept is pristine. The *idea* of a standpoint has remained intact. What constitutes its radical possibilities has not. In this essay, we will trace the conceptual dynamics driving this change. We will suggest the somewhat fissiparous consequences currently being endured within that domain can be traced to two almost classic theoretical failings: a commitment to the belief sociological analyses can resolve philosophical questions and a failure to ensure the consistency and cohesion of the technical apparatuses being conjoined in order to provide those resolutions. What Hartsock bolted to feminist epistemology were fragments of Marx's sociology. What she produced was a Heath Robinson contraption. Its intended function was precisely what it could never really be employed to do. The consequences were predictable.

Section 1. Reinventing Epistemology

Anyone hoping to find among the tenets of Historical and Dialectical Materialism the wherewithal to rebut Analytic Philosophy in any of its guises has to face up to two challenges. First, it is not Hegel and the dialectics of history one has to confront but Kant and especially Kant's attempted resolution of the antinomies of material objects (the derivation of nature of 'the-thing-in-itself' from 'the thing-as-it-appears') and those of Being (the derivation of experience as both caused and contingent). Moreover, in overcoming this challenge, the philosopher's favourite Marxist (Lukács) is not to be relied on. Even the most sympathetic of his supporters such as Jaffe [Jaffe 2020], accept the Grand Tour of 19**th** century German philosophy set out in *History and Class Consciousness* [Lukacs 1967] failed, not because of the blatant political fawning of the final chapter on "organisation" and the role of the party as the ultimate mediation between theory and practice, nor for the reasons Lukács himself offers in his own prefatory retractions, but because, in the end, it was not philosophical enough. It did not push through to Kant's arguments and deconstruct their logic. Instead, by labelling them "bourgeois thinking", it swerved to avoid them *as philosophy*. The most you can say for Lukács is this: At least he saw Kant posed the central problem for Marxism as a philosophy. He just didn't solve it.

The second challenge might seem much more trivial but has equal effects. The usual proclamations taken from the *Theses on Feuerbach*

> The philosophers have only interpreted the world, in various ways; the point is to change it. [Marx, K., 1969, XI]

and

> The question whether objective truth can be attributed to human thinking is not a question of theory but is a practical question. Man must prove the truth — i.e., the reality and power, the this-sidedness of his thinking in practice. The dispute over the reality or non-reality of thinking that is isolated from practice is a purely *scholastic* question. (ibid., II)

cannot form the foundation stones of a philosophical position because they are neither the premises nor the conclusions of philosophical arguments. If it is anything, the first is a political slogan. The second makes sense only as a pun suggesting theory, like bread, takes shape and grows in substance in the heat of action.

Nancy Hartsock's Lukács-inspired re-purposing of reification and alienation as the building blocks of a feminist standpoint from which to overthrow Epistemology is unsuccessful for just these reasons.[1] The concept of "situated knowing" as an alternative to the Cartesian Knower is not a deduction from Epistemology's premises but the substitution for them of a sociological empirical generalisation.

> I set off from Marx's proposal that a correct vision of class society is only available from one of the two major class positions in capitalist society. (Hartsock 1998, p.108)

This proposal she labels "meta-theoretical" as it carries the sociological stipulation such a correct vision is *collective*.

> A standpoint does not refer to individual activities and perspectives: It can only be produced by a collective subject or group. Moreover, the group must be "marked" rather than an unmarked group, or, in Gramscian terms, a subaltern group. (ibid., p. 82)

In making this stipulation, Hartsock commits herself to three key unacknowledged assumptions—a metaphysics of social space as a Cartesian frame of reference constituted by infinitely discriminable locations, an ontology of individual and collective subjects standing in those locations and an epistemology based on the demarcation of appearance and reality. The first promotes the notion of perception as different lines of sight making up perspectival points of view rather than, say, immersion in an environment of sense perception and thus gives rise to the conceptual problem of generating summary depictions without any equivalent to the devices of mean/mode/median or ideal type field approximations. In addition, neither of the latter two assumptions could be said to be *philosophically* axiomatic since they are precisely expressions of two of the antinomies Kant struggled with and which much subsequent philosophy has felt he did not overcome.[2]

While it might seem patently obvious, we think it is well worth stating the point we are making as clearly as possible. What is meant by "situated knowing" is an abstraction from the everyday truism that people are not cognitive clones. While significant portions of what everyone in some cultural setting knows and believes overlap, there are differences and, in some cases,

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[1] Although we would not endorse his position, one philosopher who does try to argue his way from Enlightenment (and earlier) philosophy to what he calls a "solidaristic" conception not entirely unlike that which Hartsock strives for, is Richard Rorty [Rorty 1991].

[2] On the intractability of the logic of standpoint theorising, see Lipman [Lipman 2023]

important differences. Attributing these differences to variation in background, upbringing, walk of life or general experience is also an everyday truism. What anyone using this or any other common sense social conception as a premise has to show is either that it rests on *different logical principles* to Epistemology's worries about the Cartesian Knower and so displaces that frame of reference by proving a collective conception cannot be expressed as the sum over Cartesian Knowers, or that it enables resolution of the antinomies at the heart of conventional Epistemology's programme.[3]

The suggestion the distribution of knowledge is associated with location within certain 'master' social categories such as class, race and gender, is a sociological generalisation. It is based on certain theoretical presuppositions which license the attribution of findings to the operation of causal forces. Both the main clusters presenting this view (Marxism and Structural Functionalism) have their own explanatory accounts (Standpoint Theory and socialisation theory) of how the social mechanisms they theorise work to produce the effects they describe. Neither is interested in conventional Epistemology's core questions (What is Truth? How do we arrive at it?). Instead, they address different though equally interesting questions: 'Who in general knows what in general'? And 'Why'?[4] That is, they are mainly interested in overlaps within socially distributed knowledge. Even when what is being investigated is an attachment to "marginal" knowledge (Young Earth Creationists, say), they want to know what it is these believers have in common which accounts for why they share this point of view. It is hard to see how an interest in the social origins of shared knowledge could address Epistemology's puzzles about Truth, scepticism and the rest. Unfortunately, Nancy Hartsock doesn't offer us much help.

You cannot derive philosophical conclusions—in this case epistemological conclusions—from sociological empirical generalisations. And yet that is exactly what Hartsock tries to do in her elaborations first, of the notion of a standpoint and second, of a feminist version with which to uncover "patriarchal institutions and ideologies as perverse inversions of more humane social relations." [p. 107]. Notice, we are *not* saying you can't build a feminist account of the place of

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[3] To be fair, Hartsock presents us with quite an unusual situation. It is not often you find a philosopher saddling themselves with sociological findings in order to ride out against some philosophical argument. Much more often, it is sociologists who let their imaginations run riot with and take the findings of their studies of science or other societies to rebut philosophical positions.

[4] Interestingly, neither seems much interested in how those who know things come to know them. In other words, they are not interested in the knowledge acquisition processes producing the distributions they describe and Hartsock assumes.

women in modern (or any) society on the basis of sociological observations. Simply that you can't build a feminist epistemology to displace conventional Epistemology on them.

Section 2. The Place of Domestic Labour in Marx's Political Economy

Class is defined for Marx as relationship to the means of production; that is, how labour is located in the complex network of socio-economic processes through which a society is constituted. To define the feminist standpoint, Hartsock reduces Marx's concept to that of work. The "sexual division of labour" and the nature of women's work are what constitutes their (collective) standpoint. Taking this step though is tricky. As Hartsock admits, Marx did consider these questions and then he dismissed them. Nonetheless she ploughs on, determined to reverse Marx's judgement and even perhaps show that "capitalism is an outgrowth male domination, rather than vice versa" [p. 112].

Ask yourself what is going on here. Does Hartsock really want us to accept that Marx believed the tasks women perform were not "work" in the ordinary sense in which we might use that term? Could anyone seriously think that Marx was saying those who keep house, care for children, cook meals, do the shopping, cleaning and repairing of their own and the other residents' clothes were not doing any work (in the sense of investing application and time in those activities)?

The point to keep firmly in the forefront of minds is this. Marx was working in an intellectual environment where efforts to develop systems of National Accounting were in play. His ambition was to shape those debates. The question he wants to answer are pitched at the level of the 'global economic system'. What should be counted when adding up the total wealth of a nation state and its distribution? Presumably it should be everything which has value. But which things are they? Marx was militantly opposed to the idea value was a subjective, nebulous matter. He was committed to being a thoroughgoing materialist, so value had to be something material. He thought that 'something' was productive labour—thus, his account of value is a labour theory. A thing's value consists in, or equates to, the value of the labour required to bring it into being. In other words, value expresses the physical labour of transforming nature's materials into new forms. The wife wielding the scrubbing brush to shift the chewing gum from the kitchen floor is not engaged in physically creative activity. It cannot be a productive one as Marx conceived it. Tidying up and cleaning the kitchen does not involve transmuting the natural world whereas the shaping of steel to enclose the drums, pumps and motors of washing machines (which relieve housewives of some of the effort they would otherwise have to expend) certainly does. The challenge for Hartsock is not, as she seems to believe, Marx's supposed denial of domestic labour as work, but something far more fundamental. As Marx saw it, systematically resolving the question of which activities involve productive work requires the use of a consistent and coherent conceptual framework to identify those activities which generate value by changing the material nature of the material world and those which don't. His rejection of the productive character of female labour is not a matter of personal misogynistic opinion but the application of his analytic frame in a consistent way. Seeking the economic independence of each individual may be guided by a desire to recognise the value people place on one another, but nonetheless it is an expression of our sentiments and attachments to other people (and so a subjective matter). Such value is not *economic* value, and Marx was, above all else, an economist.

Marx was trying to establish that value resides in the material product, being invested in it through its material production process. For him, the price mechanism does not reliably track the intrinsic value of goods and activities. As a result, changing levels of payment to achieve parity of esteem will not alter the value generated through the labour process. It is not labour but labour power which the productive worker's wage buys. But the price of labour is not determined by the value that the worker produces. The price of labour power is not set by the value of the output of that labour. It is the input value needed to (re)produce the labour power which sets the worker's wage. A wage reflects a value which is intrinsic to labour power. But what creates *that* particular value in regard to the domestic work of women? Very grossly, it is what is involved in giving birth to and rearing children together with whatever activity is needed to ensure that the vital needs of current workers are provided for. In other words, the production and reproduction of the labour force. For Marx, the value of domestic work is priced into the wages of productive workers. Hence for him, the wage earner funds domestic work.

This complexity is central to Marx's thinking because the labour theory licenses the idea the organisation of productive labour under Capitalism is exploitative. It rests on a central postulate: the materially productive worker is the *sole* source of value. Hence other kinds of participants in economic life *don't add any value*. Given the rules of the theoretical system he is constructing, they simply can't. It is the fact that the price and value of labour are divergent which allows for the possibility of 'surplus value' because the purchasing price the employer pays is less than the selling price of the value bearing products which the employer gets in 'exchange' for the exercise of the labour power. It is certainly an exchange, but not a fair one because it is not an exchange of genuine equivalents. The exploitative element arises through the alienation of labour power. Since productive workers create *all the value*, if they were truly free, they would have discretion over the disposal of their value bearing products. Such products would be fully *theirs*.

It is not only control of their labour power which is alienated, though that it is reflects the fact that labour power has become just another commodity among a host of others. In terms of Marx's more philosophical ruminations, alienation of labour power results from its sale to someone else. In so doing, one is being deprived of one's specific human nature, which is to create ourselves by shaping and reshaping the world around us. We see ourselves in the things we have created. They are an extension of our powers and lives. But then again, under current and previous economic systems they are not. We don't have legal ownership of many of the commodities we have created. They not 'ours' in any strong sense, and many of them are used against 'us'. Among the things of value 'we' have produced *must* be the means of production themselves. How could they come into existence without productive labour? There is an accumulation of 'capital' in our environment though its components are alienated too (it is not called Capitalism without reason). Improving the amount and quality of capital reduces the amount of labour power invested in the production of other commodities.

In summary: Marx introduced his theory of value in an effort to find ways to quantify the value of labour by focusing on equivalences in production as the creation of value by means of the transformation of the material world. The treatment of women's work is not an anomaly, distortion or an omission in the labour theory of value. It is included within that portion of value assigned to those active in the value creating activities. It is an allocation to cover the cost of preserving and expanding the labour force and takes the form of funds for feeding, sheltering, and rearing the next generation of productive labour.

Based on what we have just said, it would seem likely a feminist programme launched from a platform in Marxism would not long remain Marxist. (By the by, Engels' hostility to the bourgeois family structure might provide a better 'Marxist' basis for feminism.) It is a familiar practice in Sociology to transplant theoretical ideas from one theory into another. It is equally common when doing so, to disconnect those ideas from their identifying relationships with other parts of the background theory. Starting with Hartsock's objectives, any appropriation of Marx's 'Standpoint Theory' is unlikely to be effective *as an application of Marxism*. After all, the function of the concept 'the working class' in the Marxist scheme is to provide an explanation why the working class can be the 'universal subject'— a subject with a special relationship to reality as well as the 'correct' understanding of the nature of Capitalism (that is, 'correct' according to the theory's terms). Marx's 'epistemology' is not straightforward nor highly elaborated. It is the extension of the thesis that material conditions are logically and historically prior to the systems of thought, together with the hugely transforming idea that characterising a society is therefore best done by understanding how its economic structure functions to give or maintain the power of some sets of ideas over others. Running through a history of a society's culture as such, as Hegel did, is an unworldly way to understand the dynamics of the totality of a society. If you leave out all this explication of why Marx holds the views he does, the idea of Standpoint Theory is in danger of becoming rather arbitrary.

Marxist Standpoint Theory supposes one group in society can be singled out as capable of encountering reality without an overlay of ideological distortion. For example, the notion of 'fairness' is taken to be misapplied when used in regard to the wage contract because labour is sold at its price. This judgement is clarified by showing that price and value are not the same. As we have said, the wage-contract is unfair because (in the logic of Capitalism) workers must necessarily be paid less than the value of their labour. The ideological overlay on the actualities of the labour market originates with money simply because money exchange renders the disparity between the two sides invisible in terms of productive effort and reward. Possibly the clearest case of this kind of obfuscation, though, might well be the (legal) compulsion on serfs to deliver a portion of their actual product to a feudal lord while gaining nothing tangible in return.

The concealment of the price-value divergence resides in the idea the worker is paid for his actual labour rather than labour power. As a result, it looks as if the exchange is a fair one since the value of labour is measured by time and a fair day's pay is given for a fair day's work. Hire yourself out for forty hours, do those forty hours and the bargain is fulfilled. The employer may well offer wages in relation to time worked, but such a contract doesn't usually specify exactly what will be done in the hire-period. What the employer has actually bought is the capacity to decide what the worker will do within the contracted period. This opens up the possibility a worker who is supported by investment in substantial capital can produce much more over the same time as one working with meagre amounts of capital. Workers themselves do not spontaneously recognise the contradiction. They think they are being paid not merely for the value of the process of their work but also for the value of its outputs. But if that were so, then they could as well just share out the product amongst themselves, leaving the employer with nothing. This is why Marx proposes the abolition of Capitalism would be a rational move for productive employees, but not for the capitalists who would lose everything (a tautology).

The German Ideology [Marx and Engels 1968] tells us practical consciousness is aware of material reality because it is directly engaged with it, operating to transform material being in accord with human needs and thereby coming to know the properties and ways of the real (that is the material) world. It is the introduction of the division of labour which lets the snake into the –

socially egalitarian – Garden of Eden since that innovation induces differentiation in social consciousness. Such differentiation reflects a social differentiation, not one based on the specific parts they play in the productive process but a broader one based on the relationship of their consciousness to reality. There are those who do not do productive work, but who do work: officials and functionaries, the managers of the religious imaginary, those engaged in domestic labour and there are those who do no work at all—monarchs, rentiers. The last pair's consciousness is relieved of the restraints of engaged immersion in re-forming materials into items with use value, and thus from the constant demand to resolve pressing practical matters such as the provision tonight's supper, tomorrow's breakfast, housing, clothing and so on. The breaking of the connection with practical activity is possible only because for the rest of your life someone else does the worrying over where your next breakfast comes from. Inequity is built into the division of labour. It is a necessary element of a situation in which some people can delegate the delivery of their practical needs to others. They make themselves dependent on the labour of others. That dependence does not lead them to think they are the subordinate party however, for the other's labour is at the command of the dependent one.

The possibility of 'being relieved' of the burdens of practical consciousness demonstrates for Marx the extent to which the bifurcation of material and mental labour itself is hierarchically structured in terms of 'material' and 'mental' activity, the elevating or demeaning nature of the relevant activities as sequenced relations. All these perceptions additively combine in the assessment of such forms of activity by people participating in them. Thinking is a dignified action compared with the 'mindless' or 'thoughtless' kind. It is an elevated or even spiritual engagement of the mind, removed from the potentially contaminating entanglement with messy and polluting matter. It comes before action and dictates its course. On the collective scale, thought dictates the state of the whole. The fundamental consequence is that those elevated to the status of thinkers mistake their elevated social status for an indicator of ontological priority. For Marx, the fact that their lives are pleasurable, comfortable, even luxurious does not mean that they are less alienated than the industrial wage-slave, for they have lost connection to their own fully human nature, that of world-and-self-transforming working on natural reality itself. The rulers are not in control of things, nor are they any less ideologically separated from reality than the workers under their hegemony.

On Marx's account of the logic of Capitalism, the working classes are not waiting for a scientific theory to enlighten them as to the realities of their situation. Their lives are largely immersed in practical doings, and their consciousness is still mainly anchored the taken for granted detail of achieving them. Most probably have little time in their lives for theoretical thinking let alone the wish to engage in book learning and theoretical inventions. Indeed, they may be suspicious or deeply disparage such activity as lacking more in the way of human dignity than their own lowly but proudly held attachment to their society.

Consciousness is the product of material conditions, and the generation of a totality transforming insurgent force—and here's the point where Marx left Hegel so long before—will be the product of material circumstances, delivered through changes in the means of production. It will not and *cannot* be induced by the elaboration of theoretical schemes. It is this which is the central weakness of Hartsock's position. It aims to engineer a social revolution by constructing an *analogy* to Marx's diagnosis of the logic of the contradictions of Capitalism; an analogy in which a newly minted action-oriented epistemology would take on the role of the transformation of material conditions essential to Marx's account. Ironically, as a 'Marxism' it is an inversion of Marx.

Section 3. Hartsock's Retreat and Harding's Compromise

Fifteen years later, if not exactly turned upside down, Nancy Hartsock's vision had undergone a considerable re-working. It is, perhaps, best captured by what she came to believe is possible for the feminist movement to aim for.

> For Marx, the liberatory role of the proletariat was in part a function of their historical mission. I would like to substitute for that understanding bell hook's phrase of yearning for a better and more just world. (p. 229).

Feminism as liberation, it seems, has morphed into something considerably less sharp edged.

What has brought all this about? Two things. First, there is an acceptance the analogical transposition of gender for class obscures all other dimensions of oppression women (and men) can find themselves suffering. Any monolithic conception must be pluralised. Second, the modernisation of Marxism as the elaboration of a humanising phenomenology of subjectivity proposed by Frederic Jameson in his *History and Class Consciousness as an "Unfinished Project"* [Jameson 1988] must now be found a place. Instead of a relentless focus on (and hence the tunnel vision of) viewing everything from the logic of material relations, we are offered the panorama of kaleidoscopic consciousness. This re-orientation is worked through as a number of injunctions.

(i). Subjective class experience rather than objective class relations is to be the fulcrum on which analysis turns. This will involve finding ways to expose and address the multiplicity of selves and subjectivities which constitute who 'we' are.

(ii). A feminist epistemology is needed to provide for the possibility of secure knowledge. Given the experiential multiplicities just mentioned, this security should take the form of "good enough" certitude.[5] Jameson refers to such a position as "principled relativism".

(iii). To fill out the feminist standpoint, practical knowledge and its epistemology must be treated on a par with abstract or theoretical knowledge. Achieving this requires both direct and theoretical engagement with the ways of life of the oppressed.

(iv). Political action is no longer just a matter of the logic of historical necessity. It arises as a human response to the inhuman relationships within which many are forced to live.

A programme of theoretical and empirical research framed around these injunctions will result in a number of enhancements to the feminist standpoint's conception of the life worlds which women inhabit.

(i). It will be possible to celebrate the variegated nature of women's experience and its interpretation and so allow the exploration of the variety of political consequences which arise therein.

(ii). There will be a greater understanding of groups as the intermediary form of social organisation linking individuals and society. Groups will be seen to be emergent communities rooted in shared experience and not as aggregations of anomic, alienated individuals.

(iii). Finally, an alignment will be forged between the tenets of epistemology and the principles of political action applicable across cultural settings. Inevitably this will involve a "de-centering" the Western model of socio-economic class relations as the explanatory paradigm.

In jettisoning the armaments of Marxist class theory for the therapy of post-modern phenomenology, what has been let go is the robust analytic framework on which feminist epistemology modelled itself. What has been added is fashioned from a collection of sociological concepts within which social relations can be de-constructed and re-materialised at will. We have individuals, groups and societies, but how do they relate?[6] We have experience and

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[5] Surprisingly, the reference offered for "good enough" certitude is Wittgenstein.

[6] One central and deeply knotty issue is what Arthur Child [Child 1941] called "the problem of imputation". How do you move from categorial attributions of knowledge, belief and attitude to attributions about what individuals know

interpretation. How are they aligned? We have varieties of knowledge and 'good enough' certainty, but how do you pass from one to the other without devaluing both? Into this mix is introduced a multiplicity of selves and their situations. On what basis do we now constitute the subjective unity of the social actor and generalise it to the standpoint of an oppressed category, be it class, gender, race, sexuality or the intersection of them all? Hartsock's Standpoint Theory once designated a broad, if ill-defined, programme of purposeful social intervention. It has now turned into just another academic discourse.

In the Afterword to her 1998 collection essays, Hartsock reflects, somewhat ruefully it must be said, on the state of feminist epistemology. The openness to challenge and radical thinking which Dorothy Smith and she had so embraced was now being used as licence for denigration and caricature, so much so that she and other senior members of the discipline could find themselves traduced as "positivists" and any insistence that there were points of view which were objectively correct denounced as forms of 'Fascist', 'totalitarian' or some other form of oppressive thinking. The virtues of rigour in theory and discipline which she and others had taken for granted were now viewed as suffocating constraints on the free rein of personal liberation.

This is the context into which Sandra Harding launched her paper *Rethinking Standpoint Theory* [Harding 1992]. In it, she makes the case for disciplinary rigour in pursuit of what she terms "strong objectivity". The feminist challenge cannot simply be a meta-critique of the epistemological practices of the sciences, including the social sciences. It must engage with those sciences in order to transform them by facilitating the production of knowledge *for* women not simply knowledge about women.[7] This cannot be done by denying theory and rigour but only by transforming what the terms mean in the context of any particular discipline. Following the injunctions of Smith and Hartsock, these transformations must be based in empirically grounded research not personal experience and the expression of feelings about that experience, however justified they may be. In being empirical, though, it cannot be empiricist. For Harding, study after study has shown empiricism and its methodologies to be shot through with bias, prejudice and blindness. What Standpoint Theory has to do is provide an enhanced objectivity which empiricism cannot attain. It will do so by starting from the real problems of the socially marginal and their life

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and believe and what lies behind their actions. See [Child 1944; Child 1956]. This problem remains unresolved in Standpoint Theory and, as is made clear in Part Three, was imported into Institutional Ethnography where it remained even after Standpoint Theory was officially abandoned.

7 Harding has a fine line in ringing phrases and pithy epithets. This one, of course, paraphrases a similarly ringing phrase alleged to have been used by Marx.

struggles with those problems not the theorised versions taken up by the natural and social sciences.

> The starting point of Standpoint Theory—and its claim that is most often misread—is that in societies stratified by race, ethnicity, class, gender, sexuality, or some other such politics shaping the very structure of a society, the activities of those at the top both organize and set limits on what persons who perform such activities can understand about themselves and the world around them [Harding, 1992, p. 442]

The shift, of course, is not just in terms of what the disciplines are about. It is also what they are for. They can no longer be legitimated by appealing to the virtues of curiosity or unmotivated understanding. They must be treated potential instruments of liberation and shaped as such. The justification is brusque.

> ... all knowledge attempts are socially situated, and that some of these objective social locations are better than others as starting points for knowledge projects, challenges some of the most fundamental assumptions of the scientific world view and the Western thought that takes science as its model of how to produce knowledge. It sets out a rigorous "logic of discovery" intended to maximize the objectivity of the results of research, and thereby to produce knowledge that can be for marginalized people (and those who would know what they can know) rather than for the use only of dominant groups in their projects of administering and managing the lives of marginalized people. [Harding 1992, p. 444-5]

Its adoption of this point of departure and its insistence on the impossibility of a view from nowhere does not condemn Standpoint Theory to relativism. Neither is it bound to some feminist version of ethnocentrism. It strives for an epistemology which recognises, honours and enacts a multiplicity of 'ways of knowing' rather than seeking to distil the essence of knowledge by abstracting over them. Developing this transformed sociology will require breaking with both the pre-eminent methodological clusters in the contemporary discipline, empiricism and with Marxism. Both assert the possibility of a singular position from which to survey social life, though of course, how that position is constructed is very different in each.

For Harding, the epistemic virtue against which Standpoint Theory is to be assessed is carried by the contributions its studies actually make to the transformation of the conditions under which their subjects live (they deliver the 'liberatory' goods, so to speak) and a combination of strong objectivity and strong reflexivity. Standpoint Theory brings exactly the same critical perspective to its own programmes as it does to those of other sociologies. It insists on the examination of the cultural presumptions and expectations which shape the framing studies and the preference structures which lead to priority setting in the objectives for research programmes. It is not only in the logic of justification where bias is to be found. It also lurks in the formation of the logic of discovery. Along with broadening of the notion of objectivity is a denial of 'absolute' value freedom for any social activity including research. Unlike empiricism, strong objectivity does not seek an aseptic moral environment clean of values, but one which recognises the differing character of different values. Some values are democratically, ethically and culturally regressive while others are not. The challenge here is to achieve the multiplicity of viewpoints needed to guarantee the achievement of socially transforming objectives whilst not losing adherence to conceptual clarity and precision. In sum, what Harding proposes is a reconstructed investigative methodology in the light of Standpoint Theory as a fully formed feminist sociology not a philosophical tinkering with conceptual reformulations.

Over time Harding's views too moved somewhat. True, the notion of a disciplinary basis remained though what that might mean changed. The core was still to be found in

> ....a logic of research that focuses attention on problems that are deeply disturbing to anyone reflecting on contemporary challenges to Western thought and practice, and yet insoluble within the philosophical, political, and theoretical legacies that they provide. [Harding, 2009, p. 198]

She accepts the identified methodology has a pretty humungous agenda. As a logic of research, the approach should be able to rely on an encompassing set of fully defined concepts and derived theoretical premises in terms of which investigations are to be mounted. There should also be a list of objects or phenomena which are to be the subject of those investigations and a description of practices by which evidence/data/information concerning the phenomena under investigation can be collected as well as rules for the collation of evidence and prescriptions for generalisations over particulars. She makes no bones about it. If Standpoint Theory is a logic of research grounded in this way, then it is part of a disciplinary endeavour. Harding also reinforces two other features of her earlier depiction of Standpoint Theory. It is committed to liberationist politics and opposition to the foes of the oppressed. It knows whose side it is on. In addition, it deals with forms of oppression which involve "intersectionality", loci in the social structure where individuals are subject to multiple dimensions of exploitation and discrimination because of the identities they hold or are perceived to hold. Finally, the requirement of a combination of grounded research logic and political commitment implies an insistence that investigations of actual cases of oppression will excavate and formulate sufficient relevant and effective knowledge to address and overcome the conditions of oppression; conditions which elude other disciplinary approaches.

It is clear, the focus of attention has turned away from radical change in the intersectional settings where Standpoint Theory is to be deployed and the political commitment which drives results through to deliverable outcomes. Instead, the key issues seem to relate to what membership of a discipline should mean for those engaged in the investigations and hence to developing applied theories which confront a variety of not always easily integrated topics. To begin with there is the fact that Standpoint Theory and feminism in general have not evolved into a single free-standing discipline. Instead, they remain themes, problematics, which have been taken up across multiple disciplines. Whether they would or no, Standpoint Theory and feminism are *de facto* multidisciplinary. Because their scope and remit cannot be confined to one disciplinary purview, this development is taken to be an important step forward. Second, they are now transdisciplinary. There has been an acceleration in the use their methods across very many disciplinary settings. This methodological diaspora is noted without further comment or reservation. Does this silence imply an assumption the tailoring and adaptation necessary to facilitate the translations have been universally consistent, coherent and integrity preserving?

Anticipating our later discussion of the debate on intersectionality, Patricia Hill-Collins offers a somewhat more jaundiced comment on the challenges a multi and transdisciplinary intersectionality faces as a form of Standpoint Theory.

> This period of discovery was initially energizing. Yet as intersectionality as a form of critical inquiry and praxis has matured, and continues to be discovered by even more people, its advocates must become more self-reflective about intersectionality's objectives, analyses, and practices. Specifically, intersectionality needs to find ways to adjudicate often competing perspectives on what it is, what it should be doing, and why it should be doing it. Having so many people claim intersectionality and use it in such disparate ways creates definitional dilemmas for intersectionality (Collins 2015). Leaving the theoretical dimensions of intersectionality unexamined only heightens these dilemmas. Without analyzing how its own critical analyses and social actions are interrelated, intersectionality may become trapped in its own crossroads, pulled in multiple directions and drowning in ideas. Without sustained self-reflection, intersectionality will be unable to help anyone grapple with social change, including changes within its own praxis. [Hill-Collins, 2019, p 3-4]

Any strengthening of disciplinarity is likely to be Janus-faced. Standpoint Theory defines disciplines as complicit in the operation of dominant power because of the way social structural interests pervade the foundations of those disciplines. Most important here are University and Government administrations and the way their priorities lock together with those of funding bodies, be they public, private or charitable. But, of course, the way disciplines channel such power is one of the challenges which Standpoint Theory sets out to overcome and so it must continue to press for novel ideas and attitudes as well expositions of divergent experience. What this inevitably does is stretch its range in ways that will threaten internal coherence. And yet there seems to be no recognition of this possibility when what was designed to be an endeavour producing knowledge *for women* is applied to the intersectional situations of male, bi-sexual and transgendered individuals inhabiting the interstices of race, class, ethnicity and migratory status.

What is also missing from this cross disciplinary methodology is any reference to strong objectivity and strong reflexivity. Since the former was rooted in the adoption of a disciplinarily correct stance on the objects of research, this is, perhaps, not surprising. It is hard to insist on certainty whilst trying to maintain an attitude of openness to other ways of seeing things. Regarding strong reflexivity as the contemplation of one's position as a well-placed, successful academic from an alien culture engaging with the issues and problems of the marginalised who have no access to or even conception of the benefits their observing researcher enjoys, perhaps the challenge of

> ..thinking from marginal lives leads one to question the adequacy of the conceptual frameworks that the natural and social sciences have designed to explain (for themselves) themselves and the world around them. This is the sense in which marginal lives ground knowledge for standpoint approaches. (Harding 1992 p. 451)

proved too discomforting?

Section 4. Crenshaw's Fragmentation

Whatever else they do or do not agree with one another about, it should be pretty clear by now standpoint theorists share a common metaphor: members of society occupy distinct locations in social space. It is when things get substantive, they get a little tangled. If we talk about this space being multi-dimensional and parameterise the dimensions using notions of class, race, gender, religion etc. etc. and so talk about the individual in terms of a bundle of *variables*, this is certain to be seen as objectivist, scientistic and unfaithful to the phenomenology of the member's experience. The way this issue was first handled was to select what, in other discussions of research method, might be called 'control variables', namely class and gender, or rather class refracted through "the lens" (another metaphor) of gender. Eventually, what emerged was the conception of social space as a 1+1/N dimensional world with N standing for the count of all the other variables being refracted through gender. For Kimberlé Crenshaw [Crenshaw 1989; Crenshaw 1991], this was not just unsatisfactory, it was misleading and wrong. The majority of women she encountered in her studies of violence were as much black and working class as they were women. They sat in a confluence/intersection where the oppressive forces of race, class and gender were equally on display not one where the first two forces worked through the dominant factor of gender.

Since that critique, intersectionalism has permeated the field of what, just because its proponents do, we will now call 'resistance knowledge production'. The result has been extraordinary.

> Over the intervening decades, intersectionality has proved to be a productive concept that has been deployed in disciplines such as history, sociology, literature, philosophy, and anthropology as well as in feminist studies, ethnic studies, queer studies, and legal studies. Intersectionality's insistence on examining the dynamics of difference and sameness has played a major role in facilitating consideration of gender, race, and other axes of power in a wide range of political discussions and academic disciplines, including new developments in fields such as geography and organizational studies. [Cho, 2013 p 787]

Although this proliferation has been met with some reservations (we will talk about Hill Collins' influential contribution in a moment), Crenshaw thinks the trade-offs involved are well worth making. In fact, for her many of these reservations reflect lack of sufficient familiarity with what the burgeoning work comes to.

> .....the widening scope of intersectional scholarship and praxis has not only clarified intersectionality's capacities; it has also amplified its generative focus as an analytical tool to capture and engage contextual dynamics of power. In consequence, we think answers to questions about what intersectional analysis is have been amply demonstrated by what people are deploying it to do. (ibid., p. 788)

There is more.

> Thus, interpretations of intersectionality within other discursive fields may not escape the dynamics that rendered Black female plaintiffs illegible to courts in the cases initially analyzed. It is far from mere coincidence that current debates about intersectionality's capacity to represent anyone other than Black women bear striking resemblance to courts' discomfort with centering Black women in class-action lawsuits. (ibid., pp 791-2)

What Crenshaw and her colleagues see the range of studies which work with an intersectional "sensibility" (their term) displaying is the fluid character of the melange of categories being invoked in analysis as those categories are deployed either on the circumstances of actual marginalised persons or on the forms of conventional and institutionalised corpora of knowledge brought to bear when such persons become entangled in the machineries of social welfare, the legal system and government. All underscore the need to surface, frame, appropriately weight and relentlessly oppose the scale of political inequality described. Eventually this opposition must successfully overcome the injustices such marginal persons experience.

For Crenshaw, intersectionality represents a starburst of radical Action Theory. For Patricia Hill Collins [Hill-Collins 2019], it is more like incandescent flying debris. Collins is conflicted though. She too wants to confront all the forms of oppression, subjugation and disadvantage which studies of intersectionality have rooted out. Unfortunately, she can find no intellectual coherence nor a systematically set out the rationale motivating what is being done. To begin with, what does the term 'intersectionality' actually designate? There is no point, she thinks, in saying (as Crenshaw does) that it means whatever those who use the term want it to mean because they take it in so many different ways. For some, it is an insightful metaphor alerting them to issues. For others it is a handy heuristic. The corpus of studies built up provides exemplars, rules of thumb and templates of investigative protocols which can be borrowed, adapted and re-purposed. Yet again others take it to be a paradigm more or less in the Kuhnian sense. It constitutes an (inter/multi-) disciplinary frame of reference which has both overthrown the old 'objectivist' and 'externalist' explanatory order and is addressing problems and questions which that order had no place or account for. True, intersectionality has broken down the silos of disciplinary explanations. But, instead, it is trying to hold them together not as multiple competing perspectives but, borrowing a mathematical image, as manifolds of surfaces. The trouble with this unconstrained usage is that claiming some initiative to be intersectional legitimises almost any kind of mode of investigation and intervention and can be justified merely by asserting the study's articulation of "liberatory" resistant knowledge.

Hill-Collins' reaction is epitomised in a concern over the much over-used term "critical". Every instance of intersectionality promotes itself as critical. In many, many cases Hill-Collins thinks this just means they dislike, disapprove of or want to change the institutionalised routine practices of public agencies because such processes tend to prioritise the wants and needs institutional processes can deal with over the wants and needs their 'clients' call out. Although she doesn't say so, Hill-Collins implies the emphasis Crenshaw and the other leading intersectional researchers have placed on starting from the situation and experience of the individual has generated not just tolerance for atheoretical sentimentality but a contempt for those who would do things differently. Where, Hill-Collins asks, are intersectionality's Max Horkheimer, Stuart Hall or Franz Fanon? From each, she extracts a nugget, a theme, which could be used to tie intersectionality together more tightly. From Horkheimer, it is the Frankfurt School's notion of "massification" as late-Capitalism's mechanism for reproducing alienated consciousness. From Hall, it is the way the different cultures of disadvantaged and discriminated groups exhibit adaptive "articulations" of alternative modes of subjugation. Fanon brings a post-structural awareness of the importance of legacy of colonialism and the rise of neo-imperialism to be found in the psychologies of subjugation among once colonised communities and their immigrant migrations. Appropriately theorised, Hill-Collins suggests, some conjugation of these ideas could provide the scaffolding for a robust theory to underpin intersectionalism. On its basis, coruscating analyses of the generative structural conditions and subliminal constraints shaping the consciousness of oppression could be developed rather than overwrought condemnation which she thinks far too often passes for critique in intersectionality.

We could go on, following her through her consideration of the muddles of "resistant knowledge", the idiosyncratic position of "Black feminism" within intersectionality, the conceptual elusiveness of relationism and the characterisation of what constitutes social justice (among many other things). But we won't. Instead, we will close with her summary of what intersectionality needs to do *now* if it is to survive as a credible theoretically motivated form of radical social action.

First, intersectionality cannot carry on dodging the problems posed by its freewheeling epistemology. It has to develop an appropriate conception of *truth*, one which can stand as a counterweight to the absolutist conceptions which underpin the various forms of empiricist naturalism and realism. Inevitably this will mean returning to the topics of social epistemology and the variability of what is taken to be 'justified, true belief' in societies as well as why some matters are foregrounded and others ignored or even repressed. This will require developing an approach to data which gives it due weight while recognising its stochastic character. You might say what Hill-Collins wants is a conception of "reasonable truth" based in "circumstantial evidence". But it has to be one which allows a closure of attempts to contest that reasonability.

Second, there is power. Within intersectionality (and most political and social philosophy), power is a residual category. It is the fallback explanation when other reasons, factors and causes don't close the explanatory loop. The complexity of contemporary social relations has only increased its unsystematic usage. It is conceptualised as force, influence, repressive tolerance, alienated consciousness and in many other ways without working through and co-ordinating within a single conceptual framework the denotations and connotations carried by each.

Finally, there is the place of ethics and social justice.

> What is the place of ethics within intersectionality as critical social theory? What role should ethics or normative principles play within intersectionality's methodology or way of arriving at truths? Can ethical issues such as social justice be so neatly cordoned off and ignored because they seemingly lie outside intersectionality's theoretical concerns? (2019 p. 289).

Section 5. Summary

What is grounded knowledge in the analysis of the social and how should we acquire it? What is the relationship between power, position and interests and how can we evidence it? What is the role of social values in resolving the first two questions? These are Patricia Hill-Collins' challenges to intersectionality as the latest manifestation of Hartsock's Standpoint Theory. It is only by addressing and resolving these three clusters of issues that Hill-Collins believes Standpoint Theory will be able to find a path back to the focus and mission it once defined for itself. Unless it does so, she feels it will inevitably continue to become even more diffuse, differentiated, and uncoordinated. Eventually, rather than a body of theoretically devised analytic concepts, its central terms will have degenerated into political slogans. That following the path she seeks involves collectively addressing and overcoming precisely the same paradoxes, dilemmas, and other wicked problems the social and human sciences have unsuccessfully grappled with over the last 3000 years, does not bode well.

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Part III

Institutional Ethnography and Sociology

Introduction

Symbolic Interactionism has the notion of a "moral career" to describe the trajectory which a person's sense of identity takes they move through the stages of assimilation into a way of life. Not surprisingly, perhaps, researchers have been more interested in the adjustments in moral alignment made by those who move from relatively conventional worlds into those which are more outré or even deviant. But, of course, the concept is symmetric. Those going in the other direction similarly pass through a moral career.

We want to borrow this idea and use it to summarise the trajectory of transitions which a sociological methodology, Institutional Ethnography, has undergone over the period of its existence. Apart from the great fondness we have for Dorothy Smith's early work and its ethnomethodological character, Institutional Ethnography is of interest because of its role as a possible harbinger. The dilemmas faced by Institutional Ethnography as it negotiated its relationship to the rest of professional Sociology could well stand as an example to other ethnomethodological themes (such as informed ethnography, applied conversation analysis and workplace studies) of the challenges and opportunities they will face as they negotiate their relationships with Sociology on the one hand and HCI, or Welfare Studies or Management Science etc. on the other. Would they want to make the same choices Institutional Ethnography has? At the moment, this does not seem to be a question which researchers in these thematic areas are asking themselves.

Institutional Ethnography is Dorothy Smith's self-professed radical sociology. The first essay in this Part presents a relatively conventional—but we hope even handed—appraisal from the point of view of sociological method in general. For many reasons, we conclude Smith's initial ambition was ultimately unfulfilled. What Institutional Ethnography matured into was a mode of investigation which, at the point at which it was finally defined, looked as if it was becoming a very familiar form of normal sociologising.

The second study tacks a different tack. It looks at examples of Institutional Ethnography as pieces of mature practical sociological reasoning. Here the central theme is what has now become our key orienting device, the management of the possibility of a praxeological gap which emerges when sociological reports reason from data to findings. We have claimed this is an endemic feature of professional Sociology and sure enough our demonstration reveals it emerging in Institutional Ethnography. Its presence, then, is yet another sign or marker of its assimilation and normalisation as a conventional sociology.

9

The Sociology of Experience

INTRODUCTION

Dorothy Smith talks a lot about the politicised nature of experience and the need to understand the structural basis on which that politicisation takes place. What she does not do is describe her approach as a political sociology. For her, if it is a species of Sociology (as opposed a sociology defined by whom it is for), it is a Sociology of Knowledge. However, like Nancy Hartsock, Sonia Harding and other feminist thinkers of the time, the cast which she gives her work invites the epithet 'political'. It *has* a politics and is *about* the political dimensions of social life. When looked at in this way, it seems reasonable to say what Smith provides is a very particular sociological account of the politics of experience.

Our aim is to take a close look at this sociology, the principles it draws on and its likely effectiveness. We will, therefore, be homing in on just one aspect of Smith's contribution, possibly the least important for feminists in general and, perhaps, for Smith herself. We shall not be considering Smith's work as a contribution to the cause of feminism and its objectives, though we will cross reference debates in the mainstream of feminism which were taking place contemporaneously. We shall claim the overall trajectory of Smith's work is a very familiar one. Initial progress on a proposed radical sociological innovation was followed by a broadening of scope and a growth in adherents. Together with increasingly difficult to ignore omissions and contradictions in the original formulation, these developments gradually weakened the radical agenda. Eventually, its *modus operandi* as a form of sociological work became almost indistinguishable from other conventional forms. The objective of a theory driven social revolution morphed into the local piecemeal engineering of social justice with the sociological approach adopted bearing little or no resemblance to either the original critical theory it drew on or the methodology it sought to deploy.

Section 1. Social Epistemology and Situated Knowledge[1]

At the beginning of her summary of Feminist Epistemology's somewhat fractious relationship with science, Elizabeth Anderson [2020] laid out the list of complaints feminists make about mainstream Philosophy's attitude to women and the disadvantages which flow from it. The most important of these are: (i) exclusion from participation; (ii) denial of epistemic authority; (iii) denigration of 'feminine' cognitive styles; (iv) imposition of male oriented conceptions of phenomena resulting in the assessment of female conceptions as inferior; (v) foci of interests rendering female activities invisible; (vi) production of knowledge which reinforces the subordination or subjugation of women. These weighty charges are linked to a further allegation. Whilst philosophical epistemology's ostensible positioning is a concern with knowledge claims and their status as abstract and universal phenomena (Nagel's [1986] famous "view from nowhere"), in fact the viewpoint is masculine. Rather than being free from social, political, economic, historical or personal prejudice and so merely an empty shell of Cartesian subjectivity, the philosophical attitude is imbued with the patriarchal gaze of the post-Enlightenment male. Its conception of reasoning as ratiocination is held to be content free and the principles on which that reasoning works (formal logic) said to represent a distillation of the essence of rational inference. But these principles and that conception are in fact derivatives of the content over which they run. Such content is almost entirely conceived in terms of the patriarchal gaze and hence the male point of view, or standpoint as it became known, was what provided the subjectivity necessary for knowing. The Cartesian Ego is masculine.

In place of the Cartesian Ego, Feminist Epistemology wants what it calls "the situated knower". Grappling with the philosophical issues of truth, reason and justification takes place in a world where philosophers are just as engaged in the mêlée of social life as the rest of us. In that world, knowledge is socially distributed and socially differentiated. Priests, pedlars, pornographers, perfumers and philosophers all know (some) things those occupying other walks of life don't. In

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1 This section runs over many topics also covered in *Epistemology and Feminism*. However, it comes at them with a different point of view and a different purpose.

addition, the 'interests' they have in pursuing their walks of life mean they frame the world (or, at least, parts of it) differently. Because their knowledge of the world is what shapes their interpretation of it, these walks of life encounter the world differently from each other and from the rest of us. Indeed, for some feminists what they encounter are different worlds.

At face value, this all looks a bit trite. But the implication to be drawn from its triteness is what matters for feminism. The puzzles taken up by philosophical epistemology can be traced back to the postulated distinction between appearance and reality. That distinction is a major presupposition of the post-Enlightenment male gaze. Feminist epistemology argues the structure and differentiation of knowledge and interpretation are the articulation of social forces at work in different social locations. The gender division of labour so central to modern society generates one such force, patriarchal power. *As a category*, women occupy a distinct social location to men and so acquire a distinct corpus of structured knowledge. Being a member of the category 'woman' means occupying a standpoint on the natural and social worlds; a view not available to those who do not share the standpoint.[2]

The first thing to note, then, is that feminist epistemology does not want to throw over everything to do with its philosophical counterpart.[3] It is not *that* radical. It accepts the appearance/reality distinction and it accepts its considerations apply to persons only in as much as they are categorised as 'women'. That Rosie might also be a riveter and hence have an occupation usually associated with male occupants has absolutely no bearing at all. It is Rosie qua 'woman' whom feminism insists has a distinct social location consequent upon her being female. These considerations will be important later in our discussion. What feminist epistemology wants to shift is the conception of what 'knowing' should be for Philosophy. Such claims as "there are some perspectives on society from which, however well-intentioned one may be, the real relations of humans with each other and with the natural world are not visible" [Hartsock, 1998 p. 107] make this clear. Rather than the principles of ratiocination and the machinery of formal logic, background beliefs—and hence what is taken to be justified truth—are determined by location in society. Moreover, some locations (including that of being a woman) endow some standpoints with their

2 We will not essay a view on how much any of this argument actually touches philosophical epistemology's project, except to say that it does look for all the world to be an instance of the classic philosophical ploy of premise denial. See [Chalmers 2015].

3 We should note that social epistemology met with some resistance among feminists right from the start. See [Patai 1994] and [Haak 1993].

validity. The question, of course, is how that validity is arrived at. If not through 'pure thought', how is the necessary transcendence achieved?

RELATIONS OF POWER

The emergence of and debates over social epistemology provide one context for the initial line of work which became Institutional Ethnography. Another was Dorothy Smith's personal life. She has told the story of her Damascene moment many times. (See for example [Smith 1987 kindle loc. 139].) Finding herself newly divorced, caring for two small children, with few friends and far away from family whilst at the same time holding down a temporary position teaching Sociology at Berkeley, she came to see her experience (and that of multitudes of women like her) was barely recognised by the concepts and practices of the discipline she followed nor the institutional and organisational arrangements it operated under. To use her phrase, the discipline had eclipsed women and their concerns. The extent of this obscuring became even more clear when having moved to Vancouver, with others Smith taught a course designed to surface how far women's experience differs from the way Sociology describes their lives. The tenets of social epistemology provided an account of why this was so. Sociology offered a way of knowing about the world. Following social epistemology's line of argument, this way of knowledge expressed a standpoint, an outlook, that of the male Cartesian Ego. It abstracted from and interpreted over social experience as if from an Archimedean point floating above the social world in which that experience was located. In actual fact, its avowed objectivity was the subjectivity of the male gaze.

Smith took on the challenge of threading together a sociology of Sociology's own ways of describing the life world of experience and of showing how (in particular through the discursive practices of its texts) it produced the eclipsing she had noted. Second, she set out to re-make Sociology on a new basis, the "*point d'appui*", as she referred to it, of women's experience. This "turning" would be based on the perspective of women as embodied beings-in-the-world and not a theorised social formation conceived under the reflective interpretation of male apodictic assumptions.[4] Smith's new approach would construct a sociological world for women entirely different to and parallel with the conventional sociological world constructed by and for men.

During this period, Nancy Hartsock (see her [1998 ch.6]) was turning to Marx for a social philosophy of class as an analogy for a social philosophy of gender on which a critical

[4] Later, Smith was to regret adopting this 'point of view' metaphor (See [Smith 1992]) and tried to distance her approach from it and Standpoint Theory. It was, alas, too late. The genie was out of the bottle.

philosophical epistemology could be mounted. It was she who coined the name 'Standpoint Theory' for it. What she finds in Marx can be summarised as follows:

1. Class position both structures and limits the understanding of social relations.
2. Since the character of their material lives (class position) are fundamentally different, the standpoints of the classes will be inversions of each other and "in systems of domination, the vision of the rulers will be both partial and perverse" [Hartsock, 1998 p. 107].
3. Since the vision of social relations held by the ruling class structures the experience of all, mere rejection of it because of its inadequacy is insufficient.
4. Articulation of the vision associated with the oppressed group must be struggled for and realised by means of scientific understanding and appropriate education.
5. The vision of the oppressed exposes the oppressive character of social relations and points to an historical "liberatory" role for that class.[5]

The idea of paired inverted visions located in standpoints set within relations of production is the isomorphic principle on which Hartsock's analogy rested. She replaces the social division of labour in a capitalist economy with the sexual division of labour in patriarchy. In place of capitalist and proletarian, the opposing categories are men and women. In place of the historical revolutionary mission and destiny of the proletariat in the overthrow of Capitalism, we have the revolutionary mission and destiny of women in the overthrow of patriarchal oppression.

Having set out her analogy, Hartsock offers a sweeping view of female experience such as the ways "natural cycle" of birth and death are projected and sublimated in women's own consciousness.

> (The) levels of determination and laws of motion or tendency of phallocentric society must be worked out on the basis of female experience. ......The difficulty of the problem faced by feminist theory can be illustrated by the fact that it required a struggle even to define household labor, if not done for wages, as work, to argue that what are held to be acts of love instead must be recognised as work, whether or not wages are paid. Both the valuation of women's experience and the use of this experience as a ground for critique are required. A feminist standpoint may be present on the basis of the common threads of

[5] "Liberatory" is a particularly horrid neologism of Hartsock's. She also seems to assume its definition is obvious. Unfortunately, we find it philosophically opaque.

> female experience, but it is neither self-evident nor obvious. [[Hartsock 1998, p. 124]

Once this work is complete

> ...generalising the activity of women to the social system as a whole would raise, for the first time in human history, the possibility of a fully formed human community, a community structured by connection rather than by separation and opposition. One can conclude then that women's life activity does form the basis of a specifically feminist materialism, a materialism from which to provide a point from which both to critique and to work against phallocratic ideology and institutions. [ibid, p. 128].

Dorothy Smith had a much less ambitious programme. Initially, she simply wanted a sociology which could offer a radical critique of Sociology as gendered practice. Her approach rested on a number of connected propositions. As a form of knowledge, Sociology is an epiphenomenon of society's structures of domination and power. By creating an ideology of false consciousness, its function is to legitimise the interests of those who exercise such dominion. This legitimation is achieved through the moulding of a conceptual structure and sets of analytic, investigative and operational practices.

We will examine all three assertions and the path Smith takes through them. Like Hartsock, the first (her premise) is the direct adoption of precepts she finds in Marx and Engels' *The German Ideology* [Marx & Engels, 1968]. The second licenses her adaptive inference, namely the analogising of the gender division of labour and the power structures associated with it to the division of ownership and control of the means of production which was central for Marx and Engels. The third (and here the empirical rather than philosophical cashing out of the framework is to be found) is a generalisation whose validity is to be demonstrated by her analyses of Sociology's textual working practices.

Section 2. Sociology as Ideology

Here is as clear a statement as we can possibly get of Smith's fundamental axiom. It comes from a very early formulation and so is not loaded with qualifications and other hedges against criticism.

> Social relations for the sociologists refer to the abstracted forms of normative structures held to link positions or roles, the relation between husband and wife, between positions in an authority structure, the interpersonal relations of group members, and the like. How actions of individuals are organized conceptually as relations between individuals does not yield an account of social relation "naturally." This is because

> the individual is the focus. Concepts such as relation, rule or norm, etc., provide for the social process when the basis of the analysis does not. [Smith 1979, p. 317]

The postulate on which this axiom is constructed is taken from a famous passage in *The German Ideology*.

> The way in which men produce their means of subsistence depends first of all on the nature of the actual means of subsistence they find in existence and have to reproduce. This mode of production must not be considered simply as being the production of the physical existence of the individuals. Rather it is a definite form of activity of these individuals, a definite form of expressing their life, a definite mode of life on their part. As individuals express their life, so they are. What they are, therefore, coincides with their production, both with what they produce and with how they produce. The nature of individuals thus depends on the material conditions determining their production. [Marx & Engels 1968 p. 3]

The German Ideology was one of a number of investigations Marx and Engels undertook during 1844-46. These studies applied the Historical Materialist framework Marx had derived from his studies of Saint Simon and the French socialists as well as his tussle with the idealism of Hegel. The break though came with his analysis of the writings of the English Political Economists, Smith, Say and Ricardo and via them Nassau Senior and Bentham. His argument is simple but devastating. The categories used in the accounts economists give of the workings of Capitalism are direct reproductions of (and hence themselves part of) the social reality they are attempting to describe. The economic laws promulgated are couched as 'universal abstractions' but are formed as historically located articulations drawn from the commerce (literally) of daily life. Concepts such as land, labour and capital (the 'factors' of production), the 'distribution' and 'circulation' of 'capital', the efficiency and effectiveness of the division of labour and, most of all, wages as a measure of the value of labour power used by the bourgeois economists are closely congruent with the ways these terms are used in the daily life of Capitalism.

The key for Marx is the commodification of labour. This entails a rupture in the relationship between the worker/producer and the product of (his) labour. Work is no longer an expressive relationship but an instrumental one in which the worker is positioned in the system of exchanges. As a commodity, labour which is the expression of worker's existence, has been alienated from his true Being. The idea consciousness itself might be alienated is first found in the *Theses on Feuerbach* [Marx, 1969]. Feuerbach had argued that religion was an illusion, a mysticism, promulgated by those in power to keep 'the masses' subdued. Marx agreed it was that, but it functioned in a more subtle way. Religious tenets didn't just distract the oppressed masses from the travails of their daily existence. They provided modes of understanding by which the subjugated came to accept those travails as part of the natural or God-given social order. In making this move, Marx developed an embryonic political sociology of experience as an explication of how collective representations which contain normative accounts of social order such as religion, law, political constitutions and genres of literature, function to refract and reflect the fundamental exploitative features of the social order they describe. *The German Ideology* confronts the ideas of neo-Hegelians such as Feuerbach, Bauer and Stirner and extends the notion even further. As a result, Marx holds all historically located accounts of experience (what Marx and Engels refer to as "philosophies") can be mapped onto the power structure of society. In turn, this power structure is to be seen as an emanation of the relationships of ownership and control over the material means of production. In other words, all framing of experience is ideology; its nature constituted by a distorted metaphysics and epistemology of the social. Of course, such modes of distortion need not have arisen within the groups whose interests they serve.

Ideology is, then, something of a Russian doll notion. Marx never provided a fully worked out account of how it was to be used both as a critical concept *and* as a revolutionary one. He knew he needed to do so because the third, outer, form encompassed all modes of thinking including science and, like most 19**th** century intellectuals, Marx was convinced that Newtonian Mechanics had shown science properly pursued was the means to certain knowledge about the natural world. What was needed was a demonstration of how Historical Materialism's diagnosis of the contradictions of Capitalism leading to the universalising of the precarious character of working-class labour could be integrated with the theorised conception of alienation necessary to engender the revolutionary mission of the working class. Unfortunately, he never provided that demonstration. It remained an unfulfilled promise.

The preliminary status of the concept of ideology is, then, the first thing we should note. The second is that the use Marx and Engels make of it does not require ideas, theories and descriptions necessarily to be calculated misrepresentations. Now, they are not naïve. They know this happens. They know the press, politicians, administrators and other power holders spin events, are economical with the truth, and purvey fake news when they think it necessary. But that is not what is meant by ideology. Most importantly, this kind of 'messaging' is not to be confused with forms of description and argument generated by social philosophers (a term which included the Political Economists). Expressing all social relations as the exchange of commodities (objects, labour and all forms of creativity are commodities to be bought and sold) is a form of 'fetishism'. In that they obscure the true nature of the relationships (exploitative and oppressive) and hence render them mysterious, social philosophies using those concepts reinforce the relations they describe. This, Marx says, makes them ideological. The point here is that since such philosophers are convinced by the ideas they have developed and hold to them (albeit sometimes only temporarily), the producers of ideology are themselves alienated. As individuals, they are not to be blamed for their ideas, since they too are subject to the structural power relations associated with the social organisation of the means of production. They are not traders in hypocrisy but generators of false consciousness.

In applying Marx's analysis of Political Economy to Sociology, Smith claims not to take any more from his analysis than the equation of forms of ideas and relations of power.

> '"Ruling class" has acquired a deposit of meaning since Marx and Engels used it in The German Ideology to identify that class which disposes of the means of production. I am using it here with deliberate imprecision to draw attention to the class which in various ways and from various kinds of position is responsible for the management, government and administration of this form of society [Smith, 1974, p. 258].

In the world Smith was experiencing, that class was men.[6] The theories and descriptions she reads, the studies and investigations of social life she undertakes and the administrative order she participates in are all components of an ideology emanating from a set of disciplinary power relations over the ownership and control of the resources and methods for producing Sociology as a body of ideas; arrangements and practices which displace and eclipse the realities of women's lives. Moreover, such ownership and control are overwhelmingly in the hands of men. Rather than false class consciousness, what Sociology produces is false gender consciousness. The metaphysics and epistemology from which Sociology is constituted, ineluctably construct the social world from the point of view of men and represent women's activities and the social relationships therein in terms of those of men.

> ...there is a singular coincidence between the standpoint of men implicit in the relevancies, interests, and perspectives objectified in Sociology, and a standpoint in the relations of ruling with which Sociology's objectified forms of social consciousness coordinates. Established Sociology has objectified a consciousness of society and social relations that "knows" them from the standpoint of their ruling and from the standpoint of men who do that ruling. To learn how to know society

[6] Again, note the parallel with Hartsock.

> from Sociology—as indeed many of us do whether we are sociologists or not, for sociological concepts and thinking constantly leak into the general currency of thinking about society—is to look at it from those standpoints. It is to take on the view of ruling and to view society and social relations in terms of the perspectives, interests, and relevances of men active in relations of ruling. It is to know ourselves thus. [Smith 1987 kindle loc. 77]

Whereas Marx attributes ideology's causation to a class structure erected on the extensive division and commodification of labour as a mode of production in capitalist society, Smith attributes it to the asymmetries created by the gender division of labour as a mode of life in modern societies. The parallels are clear. Economics and Philosophy are class ideologies in 19th century Capitalism. They and Sociology (and science and everything else....) are gender ideologies in the modern world. Note the phrasing. In this initial move, Smith is trying to open up an awareness of the possibility of alternative ways of representing things. Just as Marx did, she wants to elucidate how the dominant ways of thinking about social life and its institutions shape how we think about ourselves and our lives. And, again just like Marx, she wants to offer a *different* way of looking at those things, one which she hopes will have radical implications not just for Sociology but for the way social life is structured.

Sociology's Ideological Practices

Since it is Sociology as ideology Dorothy Smith is confronting, it is Sociology as a way of looking at social relations she has to transform. Sociology traffics in descriptions and explanations of the social world, so it with these that she starts. The approach she adopts is to adapt the investigative method Ethnomethodology had developed in pursuing its interest in the social organisation of appearances as an interactional achievement. Just as Marx insisted on taking the ideas of Economics and Philosophy back to the real worldly settings in which they have their original home, Smith proposes to start with the real worldly settings in which Sociology gets produced.

Her first exercise is a sketch analysis of fieldwork investigation and the practices by which sociological descriptions are manufactured within them. Her starting point is the "radical indeterminacy" [Smith 1981 p.314] she says Ethnomethodology's investigations have revealed in the formulation of sociological descriptions. The work she is referring to is best exemplified by Aaron Cicourel's *Method and Measurement in Sociology* [Cicourel 1964] which examined examples of practice across the complete range of sociological method. Each of the cases he reviewed displayed precisely the same fundamental problem. To provide a systematic sociological description, be it couched as a summary of measured social facts and forces or the depiction of patterns of culture, requires the construction of equivalence classes to which individual instances of social behaviour can be allocated. The terms or categories used to name these classes are drawn from the common culture which investigators and their subjects share. The meanings of the terms, their scope and the rules of their application (even if formally specified in protocol statements) are irredeemably open and it is a matter of 'common sense' sociological judgement as to how they should be applied.

> The observer begins with unstated common-sense procedures for defining the problem, then relies on operationalised measures for formalized common-sense categories for obtaining his indicators....for treating the subjects "obvious"......responses as literal reflections of their perception and interpretation of their environments [Cicourel 1964 p.21]

This whole tangle is made complicated beyond practical management because the use of such indigenous concepts as categories in theoretical analysis proceeds without asking first how those concepts are used in their cultural settings and without providing clear rules for discriminating when the analyst is using them as theorised homonyms for analytical special purposes and when not. The result is a variety of practices which inevitably display the combination of hope-for-the-best short term practical solutions enforced by measurement by fiat.

Smith spells out the implications of this analysis for sociological method in the following way.

1. Sociological observation is to be treated as an interpretive act. The observer is not outside the setting being observed but is part of that setting and any observations made are conditioned by the fluid configuration the relationships in play in the setting.
2. In understanding what is happening around them as they conduct their investigations, sociologists must draw upon their own background understandings to make sense of "appearances". What is happening and how to characterise individual participant's engagements cannot simply be "read off" a list of *dramatis personae*. Who is whom for what are features is determined in the unfolding interaction. How sociologists resolve the 'puzzles' they are presented with in making their observations is an unexplicated resource enabling the sociological account to be constructed.
3. 'Describing' is a very particular activity and is done in different ways in different circumstances. Moreover, not all 'descriptions' describe in the same way and some 'descriptions' do not seem to be describing at all. What is to count as a description is what those engaged in the interaction take to be a description and the forms by which

such 'interactional objects' are constructed vary across situations and the relevancies, interests, objectives of those who are participating. All these features are relevant to the production and recognition of an object as 'a description'.

4. These considerations imply the idea of a complete, all-purpose, context free 'literal' description of the setting and the activities going on in it is an illusion. Modes of expression are contextually tied to the occasions and settings in which they occur. Moreover, not only can there not be a literal description, neither can there be a singular one. The concatenation of 'descriptions from different points of view' is additive only in the sense the description is an expanded composite. Just as adding numbers together does not bring one closer to the largest number, so one does not thereby get closer to a complete, exhaustive description by treating individual descriptions as 'partials'.

On this view, sociological description is a mode of *commonsense analytical reasoning* involving occasioned selection and interpretation.

With all this in place, Smith identifies what she calls three descriptive "tricks" investigators use to construct their descriptions. In her early paper [Smith 1974] she describes the process as follows.

1. Sociologists elicit information in the form of the answers to questions. Answers are extracted from the occasion in which they were given and treated as 'data' while the information given is set aside and no longer subject to consideration.
2. The 'data' so collected are coded in some theoretically relevant way by reference to a set of framing constructs. Thus, they are re-interpreted in terms of a selected range of sociological categories and then, as members of these induced categories, summarised, aggregated or patterned.
3. In turn, the patterns thus revealed are re-interpreted as the operation of societal forces, factors, drivers. This representational function provides the explanatory account of their character.

These three steps show how descriptions given in the flow of a fieldwork interview-as-a-species-of-interaction are re-constructed as abstract, theorised 'levels'. The local context of the answer as a point in a sequence of questions and answers is completely "leached out" (Smith's phrase). The description found in the answer is re-described in terms of the sociological apparatus being applied in the investigation.

One of the examples Smith offers to illustrate this stepwise abductive practice comes from her own research. With a colleague she was studying a newspaper office. During the study, it became clear "assignment" was a key concept in the organisation of activities. The term was used in many different ways on many different occasions. During discussions of what an "assignment" was, they found themselves talking of assignments as "authorizations to use and deploy the resources of the organization in the collection of news" [Smith, 1981, p. 320]. It was only on reflection they saw they too had extracted the term from its array of occasioned uses and cast it as a general descriptive procedure which could then be translated in terms of an available organisational schema. They had moved from the level of daily journalist life to that of sociological categorisation. With "assignment" as an organisational category, it became possible to talk about the object they had defined in terms of the hierarchical, organisational and power relations it stood in and expressed.

> ...we saw that we had landed ourselves in precisely that situation from which we had tried to move in our method of doing Sociology that our practice was ideological rather than scientific. We were in business making up our own ideological forms as methods for detaching the concept from the actual social relations it expresses (Smith, 1981 p.320)

There is a fourth re-description. The 'writing up' of the findings for presentation or publication provides another framing. The normative structures covering, for example, under what conditions sociological analysis can be published, where should it be published, for whom should it be published, what formats such publications should take, the acceptable descriptive framings to be used, the role allocated to the 'observer/author in the publication, the positioning of what is published vis a vis other prior and intended subsequent publications and so forth constitute what, borrowing from Wittgenstein, she originally called elements of "language games". However, that term was deemed too passive and insufficient for the role she wished to allocate to this fourth re-description. She came to talk of genres of these practices as "discourses" in the sense Foucault gives the term in *The Archaeology of Knowledge* [Foucault 1989].

> By virtue of publication or appropriately sited public reading, a text becomes part of the literature that is Sociology. This literature is exemplary in the sense that sociologists look to what has already been done and is already identifiable as a legitimate piece of sociological work to exhibit what is recognizable as Sociology. The discourse is maintained by practices that determine who can participate in it as fully competent members. [Smith, 1987, kindle loc 1189].

Key to sociological discourse is the presupposition descriptions of social activities should be couched as trajectories of selected, designed, planned, organised, rational action, be they intentional acts undertaken by interpretive social actors or the systemic consequences of functional or world historical structural forces. What Sociology does is rationalise social life.

> If we began from women's experience of the world, we would not find these assumptions built into its Sociology, since they do not conform to the organization of our experience. Characteristically for women (as also for others in the society similarly excluded), the organization of daily experience, the work routines, and the structuring of our lives through time have been and to a very large extent still are determined and ordered by processes external to, and beyond, our everyday world. [ibid, kindle loc. 1293]

As a result

> Sociology provides a mode in which people can relate to themselves and to others in a mode that locates them as subjects outside themselves, in which the coordinates are shifted to a general abstracted frame and the relation of actions, events, and the like to the local and particular is suspended or discarded. [ibid, kindle loc. 1489]

Section 3. A First Set of Considerations

Thus far, we have been largely content to lay out the arguments put forward on behalf of social epistemology and Smith's application of it as a critique of Sociology. We had a lot of ground to cover and it was simpler not to disrupt the presentation with questions, qualifications and counter suggestions. Before we move on to look at how Smith built her new "Sociology for Women" and its evolution into Institutional Ethnography, it would be as well to lay down some pointers both as a way of indicating how we will shape our assessment of the base on which her sociology has been built as well as providing markers for what it might be important to attend to when considering its empirical investigative practices.

What we have we got so far? We have a sociological theory of the structural relations of power, ideology and consciousness premised in a social philosophy (in this case a theory of history), i.e., Historical Materialism. Into that theory has been injected the gender division of labour to replace the determining character of the division of ownership and control of the means of production in Capitalist society. Attached to the resulting gendered sociological theory, we have an investigative method derived from Ethnomethodology's studies of commonsense reasoning as the social organisation of appearances or experience. From this amalgam, claims are derived regarding the gendered character of sociological accounts of social life. The implication is Sociology articulates a gender ideology and, moreover, this articulation can be made visible for analysis in its texts.[7]

The first thing to notice is the origin of the initial proposal for a new sociology. This was not the *discovery* mainstream Sociology was male dominated nor that it operated on 'ideological' principles. Assertions about all that came subsequently. It was its failure to provide descriptions which matched Smith's experience as a young, divorced, immigrant, professional mother. However, it was not suggested Smith's circumstances represented a wholly new social phenomenon which Sociology had not hitherto addressed and which its current categories could not fit. Neither was it claimed that Sociology did not have categories with which to describe situations such as those in which Smith found herself. Clearly it did. It was simply Smith didn't feel those categories and descriptions *represented* her experience. This is the vital point. A criterion for the acceptability of sociological or other accounts of social life has been stipulated: how far do its descriptions accord with the experience of particular categories of social actors?

There are two things here. First, at the time Smith was beginning to develop her ideas, there were (and still are) very serious debates being carried on in the Human and Social Sciences concerning the place of 'experience-near' and 'experience-distant' concepts, 'emic' and 'etic' categories of description as well as debates about the possibility of 'methodological individualist' and 'collectivist' explanations and the causal, functional or supervenient relationships each form of explanation traded in. None of these surfaces in her account of her thinking and yet the issues these debates raised and the ways in which they were addressed are directly germane to Smith's project. This is not to say she should have adopted any of them. She could still have gone her own way with her own mix of theory and method. But they were serious debates about serious issues and her view on their relevance ought at least to have been offered. In addition, although she spends a significant amount of time laying out and interpreting the background to Marx and Engels' thinking to justify her adaptation of their theory of ideology, she spends almost no time undertaking the same work for Garfinkel's Ethnomethodology. The work of ethnomethodologists is cited but the reasoning which brought them to the positions they adopt is not. Had she done so, her nonchalant annexation of Ethnomethodology's 'method' might well have proven more problematic.

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7 'Imputation' might be a better term. Here we see glimpses of the problems identified by Arthur Child [Child 1941] to which we referred in our discussion of Standpoint Theory in Part II.

Second, there is the move to place her own personal feelings and, by extension, the personal feelings of a whole category of persons at the centre of a new disciplinary endeavour. What Smith wants is a Sociology *for* women not, it must be said, a Sociology *about* women (though, to all intents and purposes, that is what, for a while, it became). So now we have a lemma on the original rationale. It is not Smith's (or any other young divorced, immigrant, professional mother's) personal feelings Sociology must address, but those of women as a category. It is to be for women and provide descriptions which resonate with their feelings. The trouble with this lemma is there is no in-built stopping rule. If you can sustain a case that Sociology does not provide accounts which resonate with any particular category (white working class males, middle class knowledge workers, members of the Asian diaspora........) does that mean a new sociology should be crafted *just for them*? And if not, what are the grounds for female exceptionalism? The suggestion that the experience of being a woman is a wholly distinctive is a metaphysical claim or, at the very least, a metaphysical assumption requiring not inconsiderable justification. To fail to provide that justification is to run a very significant risk. The lack of a stopping rule could cause any social epistemology to fragment.[8]

Third, there are the possible consequences of the metaphysics trailing behind the perspectivalism on which the justifications for an alternative view stand. The need for a new sociology is to provide an alternative (and corrective) standpoint on social life. This sounds like an open-hearted position to take. Unfortunately, the assumption these views are alternatives implies they are views on *the same phenomena*. But how do we know the phenomena *are* the same unless we assume a constancy of object under different conditions of examination? Unless that conclusion is to be a wholly speculative proposition, we have to assume there is some one way we can descry phenomena 'as they are in themselves' (to coin a phrase). But this requires the transcending procedure Marx failed to provide. Without it, it is likely what starts as the adoption of a political stance of liberal tolerance could turn into an exclusivist, authoritarian dogma; namely, that there is a correct way to see the world and a feminist epistemology and sociology for women either have or will have discovered it. Over a decade after she had set out Standpoint Theory, Nancy Hartsock [Hartsock 1998], Ch. 7] thought this was indeed happening.[9]

In our view, the risks we have just identified arose because of the way feminist epistemology and Smith's new sociology were constructed. In both cases, the desire to get on and

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8 Which is, of course, precisely what has happened to Standpoint Theory (see *Epistemology and Feminism*).

9 We trace this process in *Epistemology and Feminism*.

make devastating critiques overpowered the need for careful intellectual craftsmanship. In the rest of this section, we will pick out the most important of the issues which arose thereby for Smith's project.

THE CO-OPTION OF MARX

There are two key elements here: the status of the 1844-6 manuscripts within the canon of Marxist theory and their provenance. Neither is uncontested. This is more than a matter of choosing a 'scientific' Marxism over an 'Hegelian' one or taking sides in debates over 'structuralist' versus 'phenomenological' Marxism. It is well known that in later life Engels regarded these writings as 'juvenilia' and was dismissive of them. As we have said, in the *Grundrisse* Marx himself promised to lay out the relationships between consciousness, class position and mode of production, but never did.

There are two reasons these debates matter. The first is that they concern the metaphysics and epistemology which are attached to the Marxist schema. What is the set of 'real things' being analysed (collectives, individuals, forces, material objects?), how are they to be related? The second is how we come to know about them (direct experience, a scientific method, transcendental philosophy?). Clearly, taking a position on these questions matters for the claims one would want to make on their basis. Smith can't just lift what she wants out of the Marxist frame of reference without making clear where she stands on these fundamental matters. However, at this point in the development of her ideas at least, that is exactly what she did do. Much later, she returned to this collection of issues and positions her early thinking regarding Marx as follows:

> I could not bring to its reading a more than elementary knowledge of the background of the period in and for which his texts were written. Nor was I familiar with the philosophical tradition out of which his work emerged. I read naïvely and closely. [Smith, 2004, p. 446]

Her intention in this later piece is to remedy the weakness of her earlier reading. She does so by working through the background to and text of *The German Ideology*. What she finds is a very different account of the nature of class and ideology to that which is usually presented. For her, Marx locates ideology in the social relations and working conditions of those whose role in society is the manufacture and processing of ideas.

> Reading The German Ideology in this way does not support the equation of ideology with the ideas of the ruling class as has become the standard interpretation. Ideology is not to be defined as the ruling ideas of a class. Rather, it is a specific intellectual or theoretical form

> emerging under historical conditions which create a distinctive working experience for members of the intelligentsia. [ibid., p. 451]

What Marx is doing, she proposes, is demarcating one form of 'knowledge work' (ideology) from another (science). She summarises this demarcation as

1. While Marxism has theories of ideology, Marx does not. His theory of the social determination of experience and thought is simply that people's experience arises in definite settings of their work and that ideological forms of thought are developed by people working in contexts in which language is experienced as an autonomous realm with power to influence or change the social.
2. For Marx, the concept of ideology criticizes a method of reasoning about society and history that treats concepts as if they were causal agents or determinants. Science, by contrast, does not take such concepts for granted as given entities but explores the actual social relations expressed in the concepts and categories on which ideology builds. [ibid p. 454]

What is missing from this positioning is any acknowledgement Marx began with a flat rejection of Hegel's ambition to transform reality by single-handedly changing the realm of thought. Marx knew an attempt at changing social reality has to involve the concerted efforts of large numbers of people and has to be conducted on the basis of a common consciousness. Productive workers were the potential universal subject, shaped into such by their practical consciousness of the stark inequities of capitalist life as those inequities were brought more clearly into view by the continuing extension and increasing intensity of the capitalist mode of production. Ideology was consciousness that could develop when people were liberated from participation in life-reproduction and thus disconnected from their practical consciousness, enabled to imagine things upside down, wrongly supposing that thought encounters reality by distancing itself from practical consciousness (the living of a life) having direct contact with material reality. Marx issued a demand that theories should prove themselves through practice, that they should be used to achieve changes in the material world. Smith wants to use Sociology as a means to change the respective positions of rulers and ruled and of men and of women, and thus the relations between them but has not demonstrated how an academic discipline can directly connect with the material reality of life-production. Without such demonstration, there remains the possibility that even such a sociology as she hoped for will remain more ideology than science.

Having developed her interpretation of Marx, Smith suggests it is possible to repurpose the "method" described in the quotation above and apply it to the social sciences. If you do so, it becomes clear the Frankfurt School, Foucault, her own work and even that of Althusser can all be seen as ploughing similar furrows. All are attempting to analyse social conditions which are present now but were not available to nor even visible to Marx.[10]

> The objectified relations that these various theories conceptualized were effectively not there for Marx. They are now, however, pervasive and powerful, increasingly dominated by capital and still significantly androcentric as well, of course, as being ground in class and imperialist relations. [ibid., p. 459]

The subordinate clause "and still significantly androcentric as well" with its modifier is the only time the central problematic in Smith's sociology is referenced in this analysis. And yet it is what the textual analysis has been leading to and what it is for. So, even though she provides a quite distinctive reading of Marx's early work and scatters a number of seemingly settled pigeons on the way, the necessary grounding for a substitution of gender for class is still missing. All we are left with is the hint that were Marx to be working to-day, "of course" he would be a feminist sociologist.

Such economy of inferencing covers a multitude of Smith's rapid conclusions. She is aware of this, of course.

> '"Ruling class" has acquired a deposit of meaning since Marx and Engels used it in The German Ideology to identify that class which disposes of the means of production. I am using it here with deliberate imprecision to draw attention to the class which in various ways and from various kinds of position is responsible for the management, government and administration of this form of society. [SMITH, 1973, p. 258]

But this just won't do. Class has a very precise definition for Marx, a definition which underpins the conception of the proletariat as a revolutionary group capable of confronting and dissolving the illusions of ideological experience and grasping its own historical mission.

There are two things to bring out here. First, there is the centrality of surplus value, its production and expropriation. Marx himself identified the sexual division of labour as a "primitive"

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[10] This tendency has become even more pronounced in recent accounts of Smith's thinking and its relationship to Marx. See [Lund 2023] for a leading example of attempting to find a Marx feminism would like to have rather than the one we have been left with.

form overtaken and transformed by Capitalism's relations of production. In addition, and this was drawn out in particular by Nicos Poulantzas [1975], not all fractions of the ostensible working class carry the revolutionary destiny of the proletariat, only those whose labour is "productive" in the specific sense defined by Marx. That is, the truly revolutionary are those whose labour leads *directly* to the creation of surplus value which can be expropriated. As categories, the petty bourgeoisie, the servant classes, shop workers, street cleaners and what today we would call those in service industries do not contribute productive labour. By the same token, when looked at as a category, neither do women.

Second, for Marx when women are excluded from the productive labour force, their work conditions limit their capacity to form a common consciousness with others who are in the same position in relation to the productive forces (but most importantly not in the same position *in* the productive forces). The working conditions of such women involve being centred on the home in caring relationships with babies and small children and hence capable of forming far fewer direct relations with other women in the same position as themselves compared with people involved in the collective labour process and spending whole days with others in the same occupational position. The housewife's working day is not defined as a fixed period. Engaging in child rearing might well be voluntary and rewarding but does not perhaps compensate for being confined at home or most of the time with little adult contact. Involvement in the responsible formation of new individuals can be seen as a social duty, losing contact with the wider world or merely the world of work might not be. The relationship between husband and wife might be analogisable with power relations with the political or business spheres but the category of 'husband' is neither an employer nor a ruler but a 'spouse'. There are very diffuse relations amongst members of household, with the ways those members treat each other being only irregularly and tenuously surveyed. The consequence of these features of women's experience is that according to Marx's explanatory scheme, only those women engaged in the production of surplus value as defined within the collective capitalist production process could be said to represent revolutionary possibility. According to that scheme, women as a social category were not capable of transcending ideology in the precisely the same way men as a social category also were not. That the male productive worker is capable of a revolutionary standpoint is a result of his position in the system of economic relationships, not because he is male.

Smith's repositioning of Marx turns almost entirely on how she reads *The German Ideology*. The problem is, as Terrell Carver put it, "*The German Ideology* Never Took Place" [Carver 2010]. Smith, along with countless others, operated on the assumption that *The German*

Ideology is obviously a book or at least an extensive set of connected passages which Marx and Engels developed during 1844-6. However, as Marxist scholars are very well aware...

> The German Ideology is well known to have been editorially constructed from uncorrected manuscripts, which are famously eccentric and difficult to decipher [Carver 2010 p. 108]

Rather than a free standing, self-evidently integrated argument, the manuscripts seem to be a reconstruction from fragmentary drafts, notes, scribbles and other flotsam and jetsam held in the Moscow archive. In addition, they were first published in 1926 as part of the debate over conflicting interpretations of the canon which were prominent at the time (See Carver and Blank [2014] and [Carver, 2019]). By examining the relevant historical documents and the manuscripts themselves, it becomes obvious......

> The manuscript materials that were organized by Ryazanov and subsequent editors in a chapter-like way ....These sequences, known since 1924 as '1. Feuerbach', were not in fact part of the materials actually prepared by Marx, Engels and Weydemeyer for publication in April and June 1846, and so were never sent by them to any publishers at all. [Carver 2010, p. 116]

Considerations like these do not lead to the conclusion Marx and Engels's ideas as 'captured' in *The German Ideology* and the other manuscripts should simply be ignored. Rather, it imposes a burden on those who would use them. They have a duty to make clear the difficulty of attributing a completed or even well-formed schematic argument to those writings. They are, indeed, exploratory, radical and contain premonitions of what was to come. But they are also fragmentary, contradictory and elusive. Perhaps they are best treated as evidence of a compositional process in action. Two things fall out from this. If, as many including Smith have done, one wishes to synthesise this thinking, thereby giving it a coherence it does not itself display, it is important to make clear what links, straps and hooks are being added to hold the whole thing together. Second, in doing this reparative work, the volume known today as *The German Ideology* has to be treated for what it is—a particular *post hoc* re-construction produced at a particular time for very particular purposes. As much as it speaks to Marx and Engels' mode of collaboration, it also speaks to the ideological and political turmoil of Russian politics in the immediate aftermath of Lenin's death.

FORM AND CONTENT

Smith's views evolve over time as she returns again and again to the topic of method and, in particular, how she wants to frame her *point d'appui* in order to make use of ethnomethodological methods of textual analysis. The final pivot occurs in the first chapter of *The Conceptual Practices of Power* [Smith 1990] with her consolidation of the notion of 'bifurcated consciousness' as the distinctive property of her sociological attitude. Her adoption of Marx's theory of alienation had allowed her to see the situation she had encountered in her early career was generalisable beyond Sociology. Everywhere the gender division of labour and women's roles in it acted to create two forms of subjectivity and so reproduce worlds of oppressive social relations in which women were relegated to the management of the local detail of daily life whilst men trafficked in the abstract, the general and the global. Feeding that insight back into her own experience, she could now see herself as a woman and as a trained sociologist living in two worlds and hence possessing both forms of subjectivity. This placed her and those like her in a distinctive position.

> Women who move between these two worlds have access to an experience that displays for us the structure of the bifurcated consciousness. For those of us who are sociologists, it undermines our commitment to a Sociology aimed at an externalised body of knowledge based on an organization of experience that excludes ours. [Smith 1990], p. 21]

It also gives her sociology its purpose.

> The aim of an alternative Sociology would be to explore and unfold the relations beyond our direct experience that shape and determine it. An alternative Sociology would be a means to anyone of understanding how the world comes about for us and how it is organised so that it happens to us as it does in our experience. [ibid., p. 27]

The purpose now is not just critique of Sociology as a mode of knowing the world but all governing modes of consciousness. The mission is to take the new way of understanding the world out into domains where women experience governing relations and thereby foster the possibility of others adopting the ambiguous, ambivalent standpoint on their local domain which women sociologists had adopted on Sociology.

The complaint Smith makes about Sociology is that it interprets over women's experience using categories of some constructed sociological theory. That it is an androcentric sociological theory just makes things worse. What she fails to notice (or is unwilling to point out) is that although her 'world view' may have a different tone, topic and vocabulary, the form used corresponds point for point with the general structure she sees in the forms of sociology she is seeking to displace. Marx's theory of alienation is an abstracted interpretation over everyday experience. True it is an alternative account of how things come about but it is based in a particular sociological construction of reality. As a mode of sociologising, it follows precisely the same process she says Sociology in general does. If ideology is a reconstructed rationalisation of social phenomena, using that process to define the frame of reference for her method of translating the experience of the two forms of consciousness she has identified cannot help but replicate Sociology's central tendency. If the form is the same, how different will the content be?

Section 4. Ideology in Action

We now want to turn to the bridge between Smith's initial formulation of her new Sociology for women and its eventual realisation as Institutional Ethnography. This transition is more or less carried out in the essays contained in *The Conceptual Practices of Power* [1990]. These essays achieve three things. They extend the compass of ideology as a mechanism of power and control. They enlarge the range of ideologies to be included to allow theories and principles deployed in social policy formation and administration to be addressed. Finally, the whole frame of reference is applied to the institution of Psychiatry. At the same time, there is a relaxation in the stricture to adopt the standpoint of women. Women as a category still figure highly, often as characters in the descriptions given, but the *marginality* of particular women in the social order is what matters now. As marginalised members of a social order, women provide leading examples of how taking a point of view radically different to the dominant one can open up the possibility of engineering change in the prevailing orthodoxies constituting and shaping people's experience.

As ever, the place to start is with Smith's view of Sociology, though this time she has her eyes on a different kind of practise to the observational field research method we discussed earlier. Now it is the use of official statistics and other data.

> Sociological discourse, like other social scientific discourses that provide a systematically developed consciousness of society, characteristically relies on the data generated by the state in the course of its practices of governing, [Smith 1990 p. 86]

This finding is not hers. It is a gloss on the findings of authors such as Aaron Cicourel [1968], Don Zimmerman [1969], Zimmerman and Pollner [1971] and Atkinson [1978], all of whom rely on earlier work by Garfinkel [1967]. What these studies show is how the practices of data collection and organisation carried out by state and other public agencies are taken over by, but rendered invisible in, the formulation of sociological accounts based on them. Since for Smith, *ex hypothesi* these agencies must be ideological, there can be no doubt this makes Sociology an accomplice to ideology too. This finding adds further weight to her earlier allegation about the nature of the male gaze in sociological theory and investigation. The resources which much of 'data-driven' Sociology relies on are collection mechanisms used by state and public agencies for the accumulation of 'data' on such things as youth crime, forms of addiction, suicide, mental illness, unemployment, housing need and much, much more. These mechanisms are document production and processing systems and the social organisation of these texts as the administrative documents they are constitutes the data available in them. What is important here is that Smith sees a general ideological mechanism at work, the textual construction of facticity. She offers a schematic for how this works.

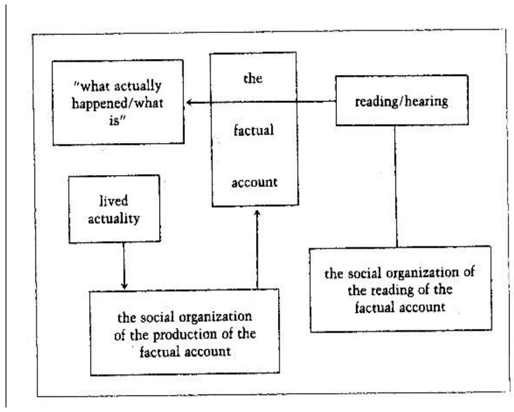

Figure 1 The social organisation of textual reality (Smith 1990 p.72)

Here we see two converging social processes: the construction of the 'facts' and the reading/hearing of the 'facts'. The ethnomethodologists she cites were pointing to the recipient-designed character of the administrative protocols for the production of documentary accounts. They are written for others who share the same sets of background knowledge and interests and hence understand how such documents are structured. In other words, they know how to read them. What the documents produce are descriptions of "the facts for all practical administrative purposes" to paraphrase Garfinkel. The sociological investigator does not read for administrative interests and purposes but sociological ones, but those interests and purposes are being served by administrative mechanisms. Hence the disjunction. It is a disjunction which, for Smith, goes even deeper. It concerns the difference between ideology and science which we discussed in regard to Marx's analysis in *The German Ideology*. The counts, measures, and other numbers and summaries which are the stock in trade of the sociologist speak not to 'the facts of things as they are' but to administrative procedures for constituting those facts for administrative ends.[11]

These procedures operate to produce an administrative double bind which she represents in a reworking of her earlier schematic to fit the administrative context.

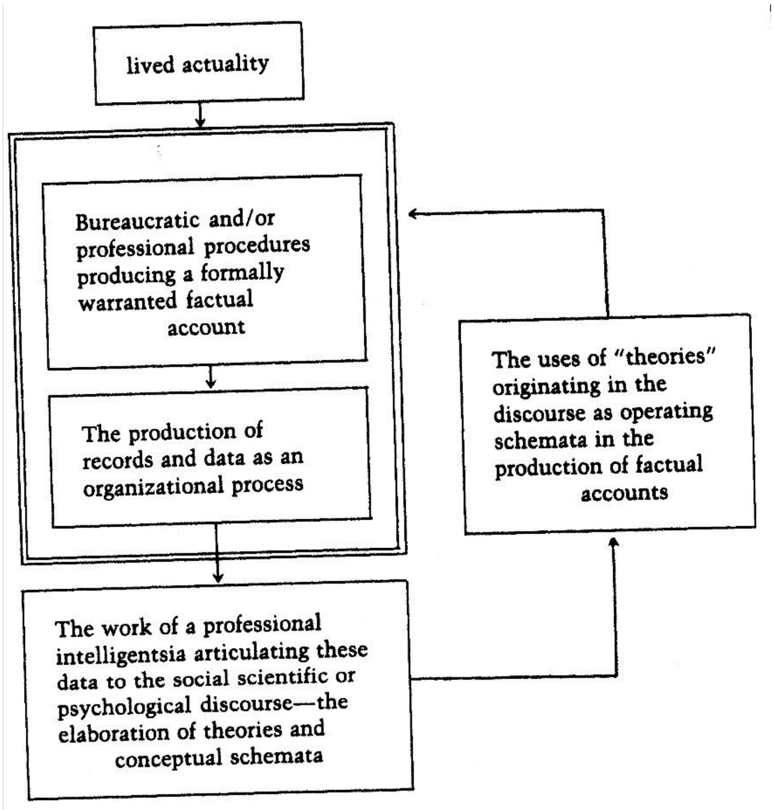

Figure 2 The Circuit of Ideology p. 148

Just as Marx argued that the British Political Economists relied on and deployed concepts and forms of reasoning used in the very practices of Capitalism in their 'external' and 'scientific' account of the economics of the Capitalist mode of production, so Smith is arguing sociologists and social policy analysts are doing the same thing in their analyses of social phenomena, issues and problems. The double bind comes when those analyses are taken into administrative and policy arenas themselves either as 'input' to strategy and policy formation or used as part of the career preparation and developmental practice of administrators. Courses labelled 'Applied Sociology'

11 For Smith the issue is *not* Sociology's corner-cutting re-purposing of the products of administrative practices *per se* but the way such practices 'leach out' the experience of those whose 'data' is being 'captured' by the practices. What giving birth means as a lived experience for a mother and her immediate family, say, is processed into an impersonal count stripped of any direct connection to actualities it represents.

or 'Social Problems' for example, end up playing sociologically endorsed administrative constructions of reality back to students.

It is at this point in the transition that several crucial moves are made. The first is a change in vocabulary; one which has important implications. Smith now talks of 'reading/hearing' as a mode of experience rather than merely the acquisition of knowledge. It is sense-making as purposeful action; i.e., the interpretation and ordering of factuality. Second, her reliance on Ethnomethodology forces a decision over the science v. ideology disjuncture. She abandons it.

> We do not suppose there is one objective account of "what actually happened" against which other accounts will be measured. [Smith 1990, p. 157]

She does not, however, abandon dualism. Instead of appearance and hidden reality, we have "primary" and "ideological" narratives. The primary narrative is based on the lived experience of those whose actions are the subject of administrative and sociological accounts. The ideological narratives are derived administrative and sociological interpretations. The major resources for accessing primary narratives are the 'oral histories' provided by subjects and participants. In these narratives lived experience is re-constituted and re-presented by those whose experience it is. This includes administrators' reflections on their experience of performing the task of building secondary narratives. The key difference between a primary and a secondary narrative is the place of experience. In the former, experience shapes the narrative. In a secondary narrative, the interpretive conceptual frame shapes the experience.[12]

The most extended analysis by Smith of the primary and secondary narratives within a single text is of Quentin Bell's account of Virginia Woolf's suicide. As usual, Smith is brusquely up-front about her approach.

> I selected this text because I could already see in it the dim forms of a phenomenon that my analytical work will explicate. These became visible to me because I read Bell's biography as a feminist who has written critically of psychiatry. [ibid, p. 178]

Her situation as a feminist and critic allows her to give a reading to Bell's text which does not rely solely on the materials Bell presented. To use a phrase which has become far too prevalent in Sociology, it enabled her to get outside the text and reflect back on it, thereby "problematising"

[12] The same distinction but taken in a different way was central to Sonia Harding's [2009] reworking of Standpoint Theory. As such, it replaced the emphasis on "strong objectivity" prominent in her earlier discussion [Harding 1992].

Bell's biographical presentation. This is the third of the crucial moves to which we referred and follows through on the loosening of the ties of the new sociology to the woman's standpoint. It is not Smith as a woman which allows her to prise apart the narratives but Smith as a particular 'situated reader'. Anyone, male or female, who was a feminist and hence convinced of the subjugation of women and had a critical view of Psychiatry could have done the same. As we will see, in Institutional Ethnography the externalised role occupied by Smith as situated reader is mirrored in the devices which investigators deploy to step outside the local context of the organisation they are studying.

Working through the detail of Bell's text, Smith shows how the primary narrative of Woolf's experience of the early years of World War II available in the letters, diaries and other writings she left together with the documented recollections of friends and family is re-shaped into a secondary one in which events appear inevitably to lead to her suicide. The sequence of her emotions as she moves from terror at the thought of a Nazi invasion and its personal implications (the Woolfs were prominent socialists. Leonard was Jewish.), through being distraught at the destruction of their homes during the Blitz, to the euphoria of realising an invasion was unlikely, and finally to the agony of depression after she was diagnosed as mentally ill, are all extracted from the immediacy of the biographical flow in which they occurred and threaded together as yet more exemplification of what is presented as a constant theme; Virginia's unstable emotions and manic depression. Using the primary narrative as hooks, Bell is found to have constructed a schematic psychiatric diagnosis in which terms like 'manic depression' and 'emotional instability' translate Virginia's behaviour into 'objective' medical descriptions. Moreover, these descriptions are naturally associated with other descriptors such as 'potentially suicidal'. This schematic diagnosis together with the possible outcomes it alludes to are resources for projecting the culmination of events in line with Bell's account. Having convinced Virginia to be medically examined, Leonard told her she had been diagnosed as mentally ill. The next day Virginia committed suicide. Rather than the shock it was experienced as by the family, Bell's account leads us to find it all too expectable. The reader and Bell together produce the facts of Virginia Woolf's death.

Section 5. Conclusion

For Smith, the ideological nature of such collaborative readings is a critical element in all forms of institutionalisation, not simply literature and not only Psychiatry. It forms one of the major ways power and control work to create a culture of acceptance, conformity, and subjugation in modern society. The purpose of her sociology is to sensitise investigators to this outcome so that, just as she does with Bell's narrative, they themselves can confront this culture and reveal its transformation of primary narratives into ideological ones together with the courses of action which are set in motion thereby. Revealing this process and enabling those subjected to it to address and overthrow it, is the task for Institutional Ethnography.

The analyses of the ideological function of Psychiatry discussed in the previous section were developments of a continuing line of interest in Smith's work. It focused on the role of texts in academic, administrative and other forms of reasoning. Such interest became one of the central motifs of Institutional Ethnography as a sociology of experience construed from the viewpoint, position, *point d'appui* of those living that experience. This possibility was realised by Smith and her students towards the latter part of her career. Realising the possibility entailed moving from critiques, rationales and foundations to application and practice. At the end of this long discussion we now know what she does and does not want this new sociology to be. The question is: Is that the way it has turned out?

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10

A Sociology for People

Introduction

In the previous essay, we set out Dorothy Smith's ambition to provide a radical overhaul of sociological method. This was based in the conjunction of two principles: the adoption of the feminist version of Standpoint Theory and Ethnomethodology's critique of Sociology's investigative protocols. We then traced how Smith adjusted and adapted her ambitions and how she grappled with the theoretical and practical dilemmas posed by both parts of her conjunction. Over time, her responses amounted to a significant re-positioning in which each of the original principles was severely demoted, if not set aside altogether.

In this discussion, we begin from this point and review some of the work currently being undertaken within Institutional Ethnography. This review will use as its lodestone the set of questions which emerged from the trajectory of Smith's endeavours. They are summarised in the first section. There then follows a consideration of them in relation to three different kinds of empirical investigation. We have chosen studies which are characteristic of how Institutional Ethnography is now understood and represent its current state as well as the scale of its current ambitions. Both, we suggest, bear little resemblance to Smith's original intent and have all the hallmarks of conventional forms of sociologising.

Section 1. Some Questions

A SIGNIFICANT RE-POSITIONING

Throughout her career, Smith insisted that texts are intentional social objects. Repeatedly she emphasised formulations, particularly strategically located formulations, are rarely if ever innocent. So, in turning to how she presented her position in *Institutional Ethnography* [Smith 2005], we took the drafting of the subtitle ("A Sociology for People") to be of some moment. The first thing we noted was how the gender division of labour was being given a much less prominent place. The transition from the first to the second essay in the volume completed this erasure without further explication or justification.

Having been alerted to this shift, we can look both to this volume and its companion, *Institutional Ethnography as Practice* [Smith 2006], for signals as to what is driving the change. First, and possibly most important, is the fact the original stance taken (a sociology *for* women) in common with the positioning of Standpoint Theory itself were subject to significant critique within the feminist movement. We know from Smith's own comments in her 're-appraisal' commentary [Smith 1992] that she took these criticisms seriously but was not entirely convinced by them. Comments made in the introductory sections of some of the contributions to the 2006 companion book indicate others were more easily persuaded. These internal critiques concerned attribution (whether real or apparent) of a monolithic 'point of view' to women as a social type. Insisting on the use of the general category 'women' was alleged to reproduce the same fallacy Philosophy, Sociology and other supposed organs of intellectual power had perpetrated, namely the reduction of the experience of women to a unified bundle which would inevitably be largely centred on domestic roles and their professional extensions. We know Smith rejected that allegation but the local politics of the feminist movement may well have made holding to the initial ambition a difficult position to take. Sonia Harding had faced similar opposition to her formulation of the principle of "strong objectivity". Over time she too softened her position. She still wanted Standpoint Theory to be part of a philosophical discipline and hence oriented to important epistemic virtues but now she was proposing...

> ....a logic of research that focuses attention on problems that are deeply disturbing to anyone reflecting on contemporary challenges to Western thought and practice, and yet insoluble within the philosophical, political, and theoretical legacies that they provide. [Harding, 2009, p. 198]

The core focus has become much more diffuse.

In addition, and this may have been more inadvertent than anything else, Smith's vision for a challenging version of Action Theory was proving attractive to men as well as women.[1] She was acquiring male students and they were undertaking Institutional Ethnographies of their own. The most well-known are Tim Diamond's [1992] study of nursing in a care home for the elderly and George Smith's [1995] investigation of people afflicted with HIV. Given Dorothy Smith's critique of the debilitating nature of the male gaze in Sociology and elsewhere, it would have been more than a little implausible to claim Diamond, George Smith and others were representing the standpoint of women. As in all walks of life, success required pragmatic adjustment.

The trouble is this re-positioning involved a great deal more than cosmetic refreshing. To begin with, the central organising principle in the adaptation of Historical Materialism in Smith's new sociology has been displaced. 'People' is not a gendered term. Neither is it a class one. The gender division of labour is no longer the force determining the experience from which analysis must start. However, nothing is put in its place. Because we still have the central motif, namely the elucidation of social relations of power, but without the motivating sociology of experience of an identified category premised in the social structural consequences of an appropriate division of labour, we are left with a lacuna at the centre of the methodology. Of course, it could be argued what is happening here is really a rejection of sociological interpretations of the social actor as a homunculus defined by a set of abstract postulates. But unless some explicit definition is given of what 'people' is to stand for, all those undertaking investigations (and the readers of their findings) have to call on to sense assemble texts are common sense typifications of what is usually intended by the use of that term. But, as Smith has repeated many times, such typifications feature in sociological descriptions as shared unexplicated elements of the account given. Centring the effort on such a general category, risks weakening the positioning and could well lead to the grounds of experience remaining unexamined. Such an outcome would replicate the omission she had found in mainstream Sociology.

This issue generates others. With this re-positioning, can clear lines of demarcation be maintained between Institutional Ethnography as an approach and older sociologies of experience forged in the traditions of Social Anthropology, Chicago Ethnography and Symbolic Interactionism? If gender is dropped as the organising principle and 'people' put in its place, what is left other than the task of offering a sociological description of what it is like to be a ....hunter

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[1] Action Theory had undergone its own moral career and was by this time an all-purpose term for interventionist sociologies of almost any stripe.

gatherer, witch, participant in a cock fight, alcoholic, hobo, jackroller, factory hand, HIV sufferer or nurse in a care home? Any such study faces the challenge of providing a sociology which fills the contents of the relevant ideal type actor's Brentano-intentionality. It has to show how they see the social world they are in and what it means for them. One point of difference might be that in other sociologies, a standpoint is not simply of identifying a point of view which enables a description of the configuration of a social *Gestalt*. It also endorses that point of view's legitimacy by hiding the facts of constraining institutional power. But if that is the case, aren't we endorsing an assumption we should give the same answer to his famous question Howard Becker did [Becker 1967]? A constant theme in ethnographic reportage has been the striving to re-balance how we should see those who are marginal to any segment of society. If the form and contents of the ethnographies are similar and the motivating attitude is too, what is so new about Institutional Ethnography? This is another question we will need to see if we can answer.

A Notable Absence

Associated with the above re-positioning is the disappearance of ideology as a leading analytic concept. In earlier accounts, ideology was produced by the intelligentsia as a normative account of the structure of ruling relations. Sociology was a purveyor of ideology about women; an ideology which shaped and defined the sociological point of view. With the dropping of gender (class got dropped earlier, remember), there is no work for ideology to do and so no place for it. The net result is the 'point-of-view point of view' loses its analytic edge. As we commented, Smith recognised this in her 1992 responses to her critics and commentators.

The net result is that a variety of alternative, equally valid accounts of some setting can always be developed (since there is no singular truth) and we cannot claim any one to be better, fuller or preferred. Definitive conclusions are put beyond reach because the transcendental mechanism has gone. What is being espoused is just one among an array of points of view. The danger here is of being left with a situation in which Sociology does what Sociology does and, if some our questions have no convincing answers, Smith's sociology will also do what Sociology does. Both translate daily experience into theoretical categories. The only difference is Smith likes her version better because it is faithful to its point of origin in people's experience. But is it? That is another question we will have to answer.[2]

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[2] There was and still is a major debate in feminist epistemology over the need for a transcendent position or some other criterion for allocating preference to forms of knowledge. One version was championed by Patricia Hill

RECOGNISABLE CONTINUITY

What the re-positioning hasn't dropped is the assumption of epistemological dualism. Its latest manifestation is in the analytic pairing of primary and ideological narratives. Its grip is brought out by the repeated use of a diagram in a number of Smith's own accounts of Institutional Ethnography and its positive referencing in the studies of her colleagues.

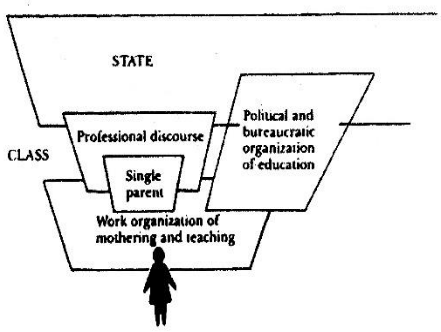

FIGURE 1.1.
A Woman's Standpoint: Single Parenthood and Educational Institutions

Here we see the "hero" (Smith's term and, given the backstory to all this, we assume an idealisation of Smith's scenario of herself in her early life) confronted by a series of configured social spaces. We are to take the point of view of the experience of this person (i.e., social actor) negotiating these spaces. But, of course, that is not all.

> The reach of inquiry goes from where actual people are in their own lives, activities and experience to open up relations and organisations that are, in a sense, actually *present* in them but are not observable. Institutional ethnography aims to discover and make visible so that from where our small hero stands, she can see things are coming about for her as they do. (Smith 2005, p.4)

Standing where she does and with the experience, understandings and context she has, the "hero" struggles to make out what is going on *and what she could, should and must do*. The social spaces are configured worlds of power relations whose logic is hidden from her. All she has are the misleading appearances which her institutionalised culture provides. It is the task of Institutional Ethnography to allow her to transcend her state and see things as they really are. In her

Collins in her critical comments on Institutional Ethnography [Hill-Collins 1992]. Kathleen Lennon [Lennon 2004] and Helen Longino [Longino 2017] offer others.

introduction to *Institutional Ethnography*, Smith talks explicitly in terms of the need to enable the hero to make a Kuhnian paradigm shift. The social world is constituted by the "hero" in one way. Having been informed by the insights of Institutional Ethnography, the constitution of social space will be configured in another, putatively more recognisable, salient, or preferable way. Given the issues we have just been discussing, there is a real possibility the sociology of experience we are to be offered will amount to little more to than an epistemology of relationism and hence be heir to all the troubles such an approach brings. How is the new epistemology supposed to work? That's another important question we will have to answer.

THE EQUIVALENCE CLASS PROBLEM

Our initial discussion noted Smith predicated her initial critique of Sociology on Ethnomethodology's identification of a fundamental methodological problem in that discipline, namely the inadvertent entanglement of formal, theoretical classes and their common sense, culturally embedded counterparts. The failure to address this entanglement made it impossible to separate out when, where and for what purposes, terms were being used in virtue of their informal, flexible cultural meanings rather than formally defined and delimited theoretical meanings. As a result, sociological investigators had to resort to practical work arounds ("tricks" she called them) to achieve adequate sociological description. These work arounds rendered the reliance analysis placed on common sense understandings invisible and led to categorisation by fiat.

By shifting the locus of research to the life world of daily experience and the relations of control displayed therein, it was hoped that the equivalence class problem might be avoided or at least nullified. The experience of the subjects of investigations was not to be translated into decontextualised sociological abstractions trading on unexplicated, contextually defined meanings. Institutional Ethnography is the method by which this approach is made systematic and replicable. The question is: Has it worked? Has Institutional Ethnography managed to wriggle free of classification by fiat? To judge by the analyses of sociology's textual practices and that of Quintin Bell's description of Virginia Woolf's suicide, it is hard to say it has. The descriptions offered trade in standard sociological tropes such as the distinction between appearance and reality and its disciplinary organisation into structured levels. In Smith's case, it is the levels of Primary and Secondary Narratives. Quintin Bell thinks he is describing the inevitable trajectory of Woolf's last months. Dorothy Smith thinks he is providing a Secondary Narrative premised in psychiatric diagnosis.

Section 2. Studies and The Documentary Method of Attribution

Not surprisingly, a number of the themes which preoccupied Smith during her career are central to Institutional Ethnography. There is, of course, the ubiquitous motif of the social relations of power together with experience, institutions, work and texts. Not every study majors on them all, but they are all mentioned at some point. We will look at three examples of Institutional Ethnography to see how these themes are both explicated and used in actual sociological investigations. We have chosen them because they have been identified as illustrating the best work done in the genre. As such, they should provide a representative base for determining the extent to which Smith's initiative is likely to be successful.

NARRATIVES AND THE "COLONISATION OF MINDS"

Total Quality Management (TQM) was a fashionable management theory in the last third of the 20th century.[3] Marie Campbell [Campbell 2006] studied a community care hospital for the elderly which was adopting versions of some of the practises associated with TQM. In her description, she picks out two aspects of the TQM approach for particular attention: the determination to drive decision-making as close as possible to the point at which such decisions are enacted and the introduction of 'customer and supplier' metaphor for the organisation of service provision. What is perhaps most important for TQM, namely measurement of customer satisfaction, doesn't get addressed. Campbell presents the consequence of the introduction of the TQM approach as the "creeping colonisation of minds" whereby management interests and relevances for the provision of patient care are superimposed on or even displace the nursing interest and relevances of the ward staff who provide that care. Interestingly, Campbell does not talk of these outlooks as 'primary' and 'ideological' narratives though she does want to show how this substitution is visible in "the actualities of people's lives" which make up the real-worldly experience of nursing. Footnote 17 makes explicit the lens though which we are to look at these actualities.

> The Canadian public care system has not until recently been directly subject to competitive capitalism. This paper shows how, within the nonprofit (publicly funded and administered) Canadian hospital system, market relations are being established and are becoming the legitimate basis of caregiving decisions. (Campbell 2006, fn. 17, p. 107)

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[3] We should probably declare some sort of interest (or knowledge at any rate) here. We were closely involved with one of the Companies (Xerox) touted as demonstrating the value of this approach. Our experience means we can certainly recognise many of the patterns brought out in this study, though whether we would construe them in the same way is not so certain.

The case concerns the unexpected consequences of introducing a new practice for managing nighttime incontinence. This initiative had been developed jointly with ward staff and was justified both in terms of improvement in comfort and care and of reduction of cost. Unfortunately, the envisaged reduction in bed linen laundry costs had not been realised. Indeed, those costs had risen. The experiential 'actualities' we are presented with are two sets of field notes made by one of the researchers observing meetings of the hospital staff. The first summarises a meeting between senior staff and the nursing assistants who deal with patients. In this meeting, it was made clear the increased cost of the new practice was not sustainable. Having received this announcement, the nursing staff went on to discuss what changes might be made to achieve the needed cost re-balancing. The second set of notes summarises a later meeting held among 'Team Leaders' which also discussed the problem of rising costs and budget limitations. These Team Leaders (whom we assume are experienced nurses) drew up a new practice which the nursing assistants were to be instructed to carry out.

Let's just remind ourselves of what taking people's experience as the standpoint from which to begin analysis is supposed to mean. The approach

> ...is disciplined by the relations that organize or co-ordinate what actually happens among those involved—what they experience. The procedure is to make problematic (or a topic for inquiry) those everyday experiences to which the observer makes us privy. (ibid., 2006 p.98)

Such experience is the "sense making" which those involved in some course of action go though. The trouble is Campbell does not offer us any direct access to the sense making of any of the staff. Neither are we given anything that could pass for one of Smith's 'oral histories' of either meeting. Instead, we get the flow of the action as interpreted by the field worker. There is no detail on the actual exchanges in each meeting, the questions asked and answers given. Neither are we offered any detail on the options considered and the process gone through by either the nursing assistants or the Team Leaders before they settled on the choices they did. The only sense making to be seen in the notes is that of the field researcher. Similar considerations apply to the reports which Campbell gives of her own interviews with a member of senior nursing staff concerning the basis for the original decision. Here we get Campbell's reconstruction of the reconstructed logic which the member of staff provided. In this second order construal, we are told that to justify the new practice the person concerned pointed to the principles of the Quality Initiative which were set out in a widely distributed document (in particular, the meeting of customer needs) and the cost reduction plan she herself had produced. For Campbell, this is significant evidence managerial considerations were driving the decisions. In both these sets of renderings, we, as readers, are kept at least two steps away from the "actualities" of the occasion on which they occurred.

The introduction of the Quality Initiative documentation and the 'business plan' for the new practice allow Campbell to move her focus to the managerial mode of discourse as exemplified by the 'customer/supplier' vocabulary being used. Once again, though, how these terms are being used in each of the documents is not shown to us. Instead, Campbell works up an elaborate account of how this pairing *must have been interpreted differently* by managers and nurses; the former being assumed to have had an 'organisational' conception which depicted relations among members of the hospital as a market while the latter are assumed to have a more common sense one of patients as the receivers as care rather than being customers of the hospital. No doubt both conceptions were in play in the organisation and no doubt there were times when they were counterposed or used to justify alternative viewpoints. But, yet again, we are not offered evidence of occasions when anyone actually expressed these views, how they were exhibited and how such occasions fed into the flow of the ongoing experience of those there. What the approach taken does allow, though, is an unsurprising summary position.

> I see the Harmonie Brief story as an extension of the social relations of ruling into the individual effort of caregivers in the hospital workplace. (ibid, p. 102)

Having arrived at this conclusion, Campbell launches into a general complaint concerning how the provision of care is being changed by the replacement of ward-level local ways of knowing and thinking by management-level ways of knowing and thinking and how the two frames of reference are inevitably at odds if not directly contradictory. (These are Smith's 'narratives' in disguise). Such a depiction of constrained control, however, sits rather oddly alongside Campbell's comments on p.100 to the effect the nursing staff were far from 'captured' by the controlling mode of thinking and had a variety of ways of pushing back against it.[4] These "rituals of resistance" (as the Birmingham Cultural Studies Group called them) are not called out and explicitly examined. We find this odd since everything Sociology has learned about the cultures of organisations would lead us to expect such rituals to be an important locally operated control mechanism and hence vital to preserving the independence of ward-level (in this case) sense-making.

As a piece of ethnography, what does Campbell's study consist in? We have the presentation of a very familiar management issue, one which is found in every organisation,

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[4] Something we can certainly confirm was the case in Xerox.

budget slippage. This issue is reviewed through the lens of TQM simply because, at that point, TQM was used to frame the way the organisation felt it should respond: TQM as an 'ideology' or 'mechanism of control'. This sociological conceptualisation is scrambled together with TQM's vocabulary as a practical, day to day, way of talking encountered in this organisation. The local occasioned usage of TQM's terms is read as exhibits of the sociological classification.[5] However, it might equally well be suggested that TQM provides an eminently re-configurable kaleidoscope. Each re-configuration scatters management components and their consequences in different ways. Organisational initiatives have consequences. They are supposed to, though not all the consequences they have are desired or planned. Re-configuring the relationships and perceptions of actual events and courses of action across the organisation through the lens of a management initiative is an interesting way to bring out the diversity of interests, relevances and orientations which any organisation exhibits. In fact, it is a tried and trusted mode of ethnographic reportage and is precisely what Campbell has done.

GEARING INTO THE INSTITUTION

We said Campbell promises to present the experience of her nurses but doesn't. Liza McCoy's [2006] study of HIV patients doesn't even try. For her, what is important is to show how institutional considerations shape the detail of everyday activities. To do this, she talks of everyday activities as "work". This expanded notion of work was long championed by Smith as moving the concept beyond the paid/unpaid dichotomy. The concept of 'work' as an analytic device like this owes a very great deal to the pioneering work of Harold Garfinkel and Harvey Sacks. This usage is rooted in Garfinkel's discussions of the views of Schutz and Kaufman regarding concept and theory formation in the social sciences and in Sacks' attempt to build 'naturalistic' sociological descriptions. To exemplify it, we will just point to the way Garfinkel [Garfinkel 1967] used it in his study of 'Agnes', a transgendered person. In describing Agnes' mode of being-in-the-everyday-world, Garfinkel refers to her as "doing being a woman". Similarly, in his lecture *On Doing "Being Ordinary"* [Sacks 1985], Sacks recommends the adoption of an observational stance which treats persons engaged in ordinary everyday activities as displaying the work of "doing being ordinary". This analytic attitude of treating ordinary activities as exhibiting the work of getting those activities

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[5] We suspect Campbell would say you cannot unscramble the two and this is the source of the controlling power. But if you can't unscramble the two, what is the basis for claiming there are two distinct narratives in play here in the first place?

done in a recognisable normal and unproblematic way is what Smith was drawing on and McCoy takes over.

McCoy's data are taken from answers given by HIV patients to questions about their use of the public medical system. These are oral histories in which patients recount how they manage their engagements with medical professionals. However, the excepts are brief and sometimes very brief. As McCoy points out, what they attest to is the range of experiences which these patients had. We are not given an extended section of any transcribed interviews, so we cannot follow the re-construction of experience as it unfolds. Instead, we have multiple layers of interpretation between what is being talked about and what we are offered.

In her earlier study [McCoy 2005], the 'stories' McCoy presents are gathered into types labelled in terms of the assessment the patient made of the attitudes expressed by the medical professionals who dealt with them. As one might expect, they are found to fall mostly into a 'heroes and villains' pairing; such judgements being based on the tolerance, respect and trust which the medical practitioner appeared to display in the story told. The later account works in the same way. This time, though, the focus is on how the patients tried to manage—sometimes successfully, sometimes not—the medical encounters they were engaged in. She calls this the work of "gearing into" the social form of the medical appointment. This work took the form of patients presenting themselves and their symptoms in particular ways in order to fit into what they knew from prior experience was the format of 'medical consultations'. They had learned only some things were medically relevant and only some attitudes were likely to be acceptable. Some patients had also learned to use relevant medical terminology to describe their symptoms, physical states and so on and had found this made the interaction more like a dialogue and less like an interrogation. In undertaking this work, McCoy says they have adopted the institutional discourse relevant to their condition. To the degree such adoption took place, the patient had become compliant with or had subjugated themselves to the demands of the medical institution. This, she suggests, means they had objectified themselves as 'cases' defined by medical relevances. The abstract and general institutional narrative had taken over from the local, particular experiential one.

Even though her topic is the patient's experience of and within the medical system, the content of the two ethnographies concentrates on the interpretation of that experience. This is deliberate. McCoy is keen to avoid any chance of "analytic drift" (2006 p. 114) away from the focus on the relevant systems of institutional control and onto the character of the experience of individual patients. This is puzzling since the experience of HIV patients as 'people' what was we were led to believe this was all about. However, the result of following such a strategy is predictable. The history of sociological investigations of hospitals as organisations and their medical practice as a social institution is replete with analyses just like McCoy's. It is true that her concern is with HIV patients, not a group very widely studied before her time. But the point and tenor of her findings and how they are structured as a mode of sociology is in direct line of descent to such masterpieces *Psychiatric Ideologies and Institutions* [Strauss et al. 1981] and *Boys in White* [Becker et al., 1977]) with its analysis of the acquisition of what it called 'medical culture'. The vocabulary and relevant medical conditions may be new but the sociology served up is precisely the same.

DOCUMENTARY POLITICS

Susan Turner's [2001; 2006]) studies looked promising for two reasons. First, starting with the magnificent *K is Mentally Ill*, Smith's (1978) description of the social organisation of diagnostic facticity, we have long admired Dorothy Smith's technical analysis of texts as institutional objects, even if we haven't always agreed with the interpretations she feels impelled to give on their basis. Turner's analyses, we hoped, would be of the same order. Second, the cases are about public sector administration and the history of a public consultation over a proposed planning application, processes we too have spent time with.

The background to the cases is straightforward enough. The local Municipal Government issued a planning proposal and consultation document concerning Turner's own neighbourhood. Obviously of personal interest to her as a resident, she decided to follow its progress and examine how the various participants in the process (most importantly, the residents) engaged with and understood its documentary processes. Right from the start, then, Turner seems to be a paradigm case of that "bifurcated consciousness" which Smith defined as the central requirement when undertaking Institutional Ethnography. Her approach is classic Dorothy Smith.

> I am approaching planning as involving speech genres and textual processes. I treat the organization of the dialogic in planning relations as such a secondary sphere of activities that actively organizes what people say in the institutional mode. I treat texts as in the action as active participants in the organization of planning relations and its public discourse. Representation is similarly understood as dialogic in character. Participating in these relations means entering into their routine practices, in time and in space, and becoming competent in them. (2001, p. 307)

The two studies take different cuts through the unfolding process. The first, which might be thought of as an autoethnographic 'subjectifying' view, tracks Turner's gradual understanding of the process by tracing her reading of the consultation document and her role as a representative of the residents. This is accompanied by an 'objectifying' view which looks at how the residents came to represent their concerns about the proposal in planning relevant terms. This parallels McCoy's account of how her HIV-patients subjugated their personal engagement with their illness to an institutionalised processual one. The second study provides a synoptic view of the documentary mode of development planning, consultation and decision making. Here the aim is to provide a map of the formal processes and their outcomes. Both studies are rich in detail and replete in acute observations. We will take a single theme from each. Both relate to Turner's own bifurcated experience of the processes: 'learning to read the consultation document' from the first study and 'mapping the text flow' from the second.

Reading the Notice

The act of receiving the Planning Notice is how residents first encounter and thus enter the process. Even those whose determination of the contents as 'junk mail' have nonetheless entered it since they can be deemed to have been served with its notice. The encounter, then, is uninvited. Those who chose to open the letter and read the notice are confronted with a sense assembly task. From the diagrams, descriptions and vocabulary, they have to construct 'a place' which is familiar to them and about changes to which they are being consulted. Although she doesn't talk of it this way, what Turner is interested in is how the performativity of the Notice has been designed to construct a particular response. Residents are to 'see' the area under discussion in planning relevant terms and respond appropriately.

The arrival of the Notice is clearly a communicative act. Turner begins by suggesting Bakhtin's dialogic metaphor might be a fruitful place to start. In responding to the Notice, residents, including herself, can be seen to be in a dialogue of social action with and within the planning process. Not surprisingly, she finds this to be a bit of a one-sided dialogue. We suggest an alternative framing might be in terms of the misfiring of the reader's reasonable expectations. A quite normal expectation for those in receipt of letters and the like is that they are the intended recipients of missives which are relevant to them and have been designed for them. This is the conjunction of recipient design and the reciprocity of perspectives. You might call the stance the reader takes in the normal course of things, an 'egological' one. For the planning process, the Notice is 'regiological' (an ugly neologism, we know, but it captures the thought). It is not directed at any one in particular, but to everyone for whom some area or place may be a relevant interest. It is for the reader to determine if they are such a person, a position which is the antithesis of assuming the communication is for and to themselves.

Instead of a dialogue, we have a broadcast. Various features of the Notice provide the clues to its character. The most important of these is the salutation. The addressee is not an identifiable person but a member of a category defined regiologically. Second, in the penultimate paragraph those who may wish to self-identify as relevant recipients should identify themselves as having been "invited" to the public meeting and hence those who do not so self-identify should not. The ambiguity of the quantification of "you" in both the penultimate and final paragraphs plays into this. Third, there is the formatted character of the Notice. Self-identification as "interested" and "invited" does not define the relevance categories those so designated might have. The format of the document provides for alternative reading routes through the information presented. Some, for example residents such as Turner, may go through it from beginning to end. Others, for example developers with competing proposals in the area, may start from the specification of use. The formatted character allows for this non-prescriptive plural sense assembling.

The disjunction of the personal and planning stances towards the document underpins the two main clusters of points Turner makes about 'her' reading of the Notice. The first is about the vocabulary and associated semantics carrying the descriptions. Turner as resident finds a disjuncture between her way of knowing the locale and the planning processes way of knowing it. For her, it is a favourite walk or a refuge. For the process, it is a site or a zone and the values defined for the parties are 'rights' and 'ownership' not enjoyment and identification. The second cluster is about process momentum and the scripted engagement provided for the reader. The places at which the reader can intercept the on-going proposal are pre-defined and the form of their interception (questions and comments) is also pre-defined. Dates for consultation and consideration are fixed. Whilst this may be the beginning of the reader's involvement in the planning process, the process itself is well underway and is following a prescribed schedule, tied no doubt into other related schedules. Whilst you can see the disjuncture as a confrontation between a local experiential and global administrative ways of knowing where[6]

> People's utterances are scripted and formed within the relations in which the text is embedded; 'subjectivities' and capacities to act are organised (2001, p. 313).

such a view reifies the differences of outlook and relevance into competing *welantanschauungen* and the engineering of relations of control.

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[6] Note "utterances" in this quotation refers to the Bakhtin notion of social action as dialogue.

Deconstructing Document Flows

The second study reinforces this reification. Starting from the belief it would be useful to show what "doing planning" as a routine manner consisted in, Turner finds

> (t)he standardised working relationships and forms of language and text-based sequences of action through which democratic planning and governing processes operate......(as) replicable forms of social action that actual situated textual activities produce. When they are put together they *are* the acts of the institution. (Turner 2006, p. 140 emphasis in original)

Her questions are clear enough. What are texts designed to do when, where, how and for whom in the planning process?

> Residents wanted answers to their questions—"what happens next?", "where?" and "who does it?"—and to see just what "it" was they would be doing and did. I also wanted to see just how and what texts or parts of texts could be activated, how and by whom, to produce the characteristic power in these relations, and move "the process" along so inevitably. (ibid., p. 142)

The Municipality had defined a 6-step process for planning applications, their approval and initial implementation. Turner takes this shell and using it as a representational device, places the relevant documents (or as many as she can collect or find out about) in sequential order within the steps. She then marks where residents are expected to be involved in the document-driven flow of activities. These are the points where residents as members of the public 'experience' the planning process. As can be imagined, this "map" is both humungous and highly detailed. Whilst numerous relationships are picked out, Turner accepts what she has tracked is not exhaustive. This is a document-centric world and documents touch off other documents, rely on further documents and are consequential for even more documents, many of which fall outside the scope of her scheme.

To help us make sense of this first "sense assembly", Turner offers higher order representations. One picks out the institutional texts being processed and those texts to which these processual texts are related. This is the process as 'text production and management'. Such sorting brings out the diverse array of administrative functions which 'have an interest in' any planning application. What is revealed is not 'the' structure of the organisation (conceived as a planning process) but one way of structuring it. No doubt the residents would find such diagramming a revelation, but we are not told if they were ever shown it and what they made of it if they were. What the 'map' does bring out is the multifaceted character of the planning function as a democratic, legally constrained, politically imbued, administratively managed process with real worldly consequences for a range of stakeholders, all of which have to be 'co-ordinated' if the process is going to work effectively as a stable, routine, accepted and trusted system.

A second higher order representation picks out the process through which (some) conflicts articulated in the consultation process were resolved. This is the document process as an instrument of "governing". The resolution device used is the attachment of 'conditions' to the planning agreement made. Once again, the consequentiality of the texts is drawn out. They mandate certain actions or determine sequences of actions which municipal employees must follow or the business practices developers and others might undertake. Eventually we get the highest representation of all, a summary flow chart the administrative evaluation and political approval process and an indication of the kinds of performativity the document bundles used at each stage can have.

At each 'level' of representation we get further and further away from the experience of the residents as participants in this process. But do we get further and further away from the actuality of the experience of other 'people' engaged with undertaking whatever aspects of the process we might be focused on? For the Minister signing off the decision, the package he or she is presented with is something that they will have to engage with and make sense of, even if only superficially. Their engagement is not so different to that of the residents with the original consultation document which was sent out to them. Equally, the agreements arrived at with developers, contractors and whoever else is involved are no doubt the product of their (long drawn out and difficult) negotiating work and now have to be engaged with as practical matters and hence sense assembled by their own planning and engineering groups.

It would be fair to say that Turner does provide us with direct access to the actuality of the document structures she is concerned with. The problem is the actuality she presents is a pre-theorised one. In the case of the Notice, we are offered a preliminary 'phenomenology' of its initial reading but that reading is couched in terms of an opposition of narratives. With the document flow mapping, the description is wholly in terms of the organisation of a co-ordinated and controlling process. If we reflect on both features, it becomes clear that what we are being given access to is Turner's bifurcated consciousness as an investigator and resident. It is that bifurcated stance which generates the readings she gives not the 'ignorance' of the process which one could reasonably expect a resident to have. In both cases, we have a reconstructed 'narrative history' of how the documentary order was experienced once it had been placed within the administrative relations of governing and control.

To have pulled this feature out of her investigation would have been a radically reflexive and hence very interesting exercise for Turner to have gone through, one which is all too rare in interactional and ethnographic studies.[7] However, that opportunity is missed and instead we are given admirably dense detail of a formalised administrative process where each step is legally constrained and politically charged. In such circumstances, any document management process will be carefully constructed in response to the relevances these features impose. However, it is hard to see what sociological news there is here either in the mode of representation or in the analysis. They are virtuous enough. But innumerable studies of organisations, public and private, have attested to the labyrinthine ways of administrations and the various states of 'negotiated order' which exist across them. From the rich materials she had at her disposal, in the end Turner offered up what is now a routine analysis.

Section 3. The Management of Accommodation

The story of Institutional Ethnography is a familiar one and one perhaps Ethnomethodology should reflect on and learn from. Youthful exuberant radicalism evolves into middle-aged determined pragmatism and then eventually retreats into assimilated quietude. This happens so often and in so many different guises and locations, one might be forgiven for thinking it a natural process. And perhaps it is. But, natural or not, when seen in Sociology the process does have some common features. So, by way of concluding this whole discussion of Institutional Ethnography and looking forward to Part IV and its concerns with Ethnomethodology and its future, we'd like to pick out a few of the most prominent of these features and the trickiest to manage.

The features we point to do not manifest themselves as distinct lines of argument or analysis. The infuse almost every quandary an emerging research endeavour has to resolve in order to acquire and then shore up its position in a disciplinary field. In fact, it is the interactions across these resolutions and the interdependences among them which constitute the realities of the management problem. They are encountered as unlooked for and generally unwelcome but seemingly necessary trade-offs, where the making of one decision inevitably results in the need for others as the consequences ripple their way through the project's theory and practise. Among the cluster, though, the first two stand out. Somehow, all the other major and minor adaptations and adjustments made over an initiative's trajectory usually lead back to them and the way they were handled.

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7 The only one we can think of is Weider and Pratt [1990]

Methodological Graft Failure

The initial conceptual core of Institutional Ethnography was a fusion of Standpoint Theory (itself a gluing of the political aspirations of feminism to the Historical Materialism of Marxism) with the investigative orientation of Ethnomethodology. Our review of the early to middle stages of Institutional Ethnography's history pointed to the instability of this conjoining. Such instability had its source in *the way* the groups of ideas were brought together and *the reasons* they were felt to be both necessary and complementary. Both, it turned out, were less than robust. The fusion was achieved rapidly on the basis of hoped-for cross-fertility but without step-by-step testing for compatibility and what, in other domains, is called 'interoperability'. Over time, the core motivations of each pulled in different directions and choices had to be made over which of the once key concepts, methods and objectives would have to be demoted and eventually laid aside.

Re-tuning of epistemic values

Although every academic or broadly scientific investigative endeavour will aspire to evidential adequacy, conceptual coherence, aesthetic attractiveness and methodological sturdiness, routine practicalities ensure not all of these virtues can be displayed to the same extent at the same time. There will be adjustments in the light of what, at any point, it appears must be done as opposed what can be treated in a more relaxed manner. With Institutional Ethnography, this tuning followed the increasing importance of *applicability* as the primary epistemic value. It was applicability to revolutionary ends of feminism that motivated Standpoint Theory and it was applicability to the investigative ends of Action Theory and its use of ethnographic field work which required the shift to a diffuse sociology for people. With applicability as the leading value, issues of coherence, evidential and causal adequacy, simplicity, reproducibility and the like, all shifted around in the background.

Plying the relevance of different relevances

In Institutional Ethnography's case, the tuning of epistemic values is most visible in the working through of how to formulate of the relevance of relevance. Whilst the concern was always couched in terms of the experience of the oppressed, disposed and marginal, the determination of to whom those designations should be applied gradually broadened. The major shift, of course, was from a sociology for women to a sociology for people. But once that shift had taken place and, resonating with the similar adjustments taking place in feminism and Standpoint Theory, Institutional Ethnography's criteria of relevance became increasing loose and its definitional boundaries porous. At the moment, what determines relevance, namely the identifiable discourses of power in institutional contexts, has all the market advantages of capaciousness and all the brand disadvantages of indistinguishability.

Suffering The Pushes and Pulls of Success

No-one should begrudge a novel research domain its success; and Institutional Ethnography certainly has been and continuous to be a success in the ways which are marked in academic circles. It is well established in University Departments. Its members publish frequently and it attracts lots of enthusiastic students. But with this success come challenges and dilemmas. Perhaps the most universal are how to maintain quality control, how to solve the 'loaves and fishes' trick and how to optimise selective amnesia. Whilst each of this may be experienced at the personal, project and programme area level, the commonality of methods for dealing with them mean they are disciplinary in character.

Success brings interest, attention and sometimes a degree faddishness. Not everyone who wants to 'do' Institutional Ethnography has had or will have the personal or intellectual skills to rise to its research challenges. But at the same time no-one wants to turn away the willing and the well intentioned. The result is that standards of undergraduate, graduate and sometimes even professionally qualified work can slip as research topics are selected on the basis of personal inclination or situation, as investigative short-cuts are taken and as less than meritorious work replicated, cited and celebrated. The point we are making here is not that similar challenges were not present right at the start. They certainly were! But the issue now is one of scale and the need for methods to control quality at that scale.

The management of quality is closely tied to the need to feed the five thousand. Increasing numbers of students wanting to join the research ranks creates the need for increasing research funds and that means generating more project opportunities and shaping them so they are fundable. When needs must, picking and choosing, especially picking and choosing in order to achieve strategic disciplinary and research programme objectives while ensuring fit to available skill sets, is no longer an option.

Selective amnesia arises in the context of the discipline's auto-historiography. How it tells its own story. The wish to adopt a rhetoric of continuity is quite understandable. After all, there is not much profit to be gained by emphasising how one's own work, that of colleagues and even the discipline more broadly, has veered and tacked in the stormy seas of changing academic fashion and professional practicalities. An example of just such amnesia can be found in Freeden Blume Oeur's recent summary history of Dorothy Smith's work [Blume Oeur 2023]. In laying out the driving forces behind Smith's determination to form a new sociology, Ethnomethodology is introduced as a purveyor of "blob ontology of categorization" implied by a shift in focus from gender to difference. Its role in helping shape Smith's challenge to conventional Sociology seems quite forgotten. This matters because it signals that what is being erased from the collective memory are the *questions* which Dorothy Smith posed to Sociology and her rejection of the answers she found there. They were heavily influenced by her understanding of Ethnomethodology and the stand-off over them eventually led her to argue for a change in the questions a sociology should address and hence the answers it should provide. It is precisely the loss of this questioning among the research community and hence an acquiescence to the questions and answers enshrined in the accommodations made with the broader discipline which are the hallmarks of normalisation. Selective amnesia leads to backward and forward projections of coherence, continuity and unity and with them both an underemphasising of the degree of critique embraced at the foundation and an overemphasising of the potential for radical impact of what is currently being done.

Perhaps the most telling feature of any normalising mode of sociology is its lack of concern for its own new model and whether it actually has solved solve the problems which generated it. In what is to-day an almost forgotten and certainly much unappreciated essay, A. R. Louch [Louch 1966] suggested Sociology, as a putative science, had a rather peculiar way with its abstractions. In the physical and natural sciences, theories and other abstractions are designed to facilitate the understanding of phenomena which are not at that point well understood by science—if they are understood at all. As descriptions of the physics of the natural world, matter, mass and motion were barely scientifically understood before the development of Classical Mechanics. To-day, the mind-boggling intricacies of Quantum Mechanics are evidence of the struggle to organise a systematic account of the basic components and forces from which matter is constituted. With Sociology, almost the reverse seems to be the case. Sociological descriptions of activities, topics and patterns of behaviour like child rearing, voting, T.V. watching and the organisation of working life, most of which are thoroughly well grasped by everyone whose lives are taken up with them, can only be understood by translating the abstract sociological concepts and terminology in which they are couched back into the terms of familiar discourse.

Institutional Ethnography doesn't propose to break with this strategy. The purpose of abstraction is to be able to offer general descriptions. As Gilbert Ryle [Ryle 1954] once put it, it doesn't matter to Physics whether a falling body is a wheel bearing, a house mouse or a man wearing a Panama hat and striped braces. All will fall at a velocity of $v_f = gt$. Velocity, gravity and time are all Physics is interested in. Since Sociology also is in the business of giving general descriptions, it too will have to use selective abstractions. It is not the abstractions themselves but the relationship between what we understand and the description we give of it which is important. And it is this which Institutional Ethnography has not appreciated. As a result, it too trades in obscure abstractions, albeit ones which are different to those given currency in conventional Sociology. It takes abstractions such as standpoint, ideology, gender division of labour and empowerment and bends them to its ends thereby reproducing the very problem of which Dorothy Smith complained.

At one point early in the development of Institutional Ethnography, it looked as if a thoroughgoing re-setting move could be on the cards, though exactly what that might have been and how it might have evolved out of the amalgam of Standpoint Theory and Ethnomethodology was not clear. Alas that moment passed, and Institutional Ethnography set off down the well-worn path to normalisation. The adjustments, re-orientations, re-shaping and re-definitions found to be necessary as more ambitious analyses were attempted and the scope of the field grew, gradually blunted the approach's distinctiveness and pulled it closer and closer to the mainstream. In the end, the original motivating and radical components disappeared and an innovation which had begun as a high-minded attempt to reconstruct Sociology turned into yet another exercise in well intentioned sermonising.

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Part IV

Ethnomethodology and Sociology

Introduction

The essays presented in this Part bring forward various themes from earlier Parts, especially Part I and Part III, and apply them to recent developments in Ethnomethodology. In addition, many pick up a consideration which is given more prominence in Garfinkel's later work than in his earlier efforts, namely the probativeness of investigative disciplines. Garfinkel attributes probativeness to the natural sciences and in doing so, intentionally or not, invites its consideration in regard to the social sciences including Ethnomethodology. The backdrop to all the discussions is Garfinkel's *Ethnomethodology's Program* [Garfinkel 2002], his final summary statement of what constitutes an ethnomethodological sociology.

With the search for probativeness as a background consideration, in the first essay on Garfinkel's inclined plane experiment we delve deeper into what is involved in mounting a successful ethnomethodological investigation. We emphasise the set-up aspects of selecting a perspicuous setting and defining the actor's point of view appropriately and discuss the approach Garfinkel deployed to mount his own version of Galileo's famous experiment. Central to the inclined plane experiment (though we only lightly touch on it in our discussion) are the question of disciplinary relations (or rather *inter*disciplinary relations) and the making of findings in interdisciplinary contexts. This provides the focus for the third and fourth essays. Here the issues we drew out from the discussion of Institutional Ethnography also come into play. In the third, we pick up the notion of 'hybridity' and the possibility of hybrid EM-disciplines of the kind Garfinkel tried to illustrate with his inclined plane study. Our conclusion is not that these are impossible, only that their requirements are extremely demanding, both conceptually and practically. In the fourth essay, the issue of interdisciplinarity is set in the context of recent trends towards greater accommodation with other social sciences. Here what we have our eyes on are what might be called cross-border relationships, and so whatever lessons there might be for us from Institutional Ethnography's experience of how a well-meant pursuit of alignment led ultimately to loss of distinctiveness and assimilation become highly relevant.

We conclude with a Coda, a brief review of the issues and the choices related to them which seem on face to Ethnomethodology at this point. Each of the options has its attractions. But each too comes with drawbacks. Whilst we are clear about the range of choices, they are not ours to make. It is for those in early- and mid-career to sort and weigh them. It is they who must pick up the burden of forging Ethnomethodology's future and they must do so in a context which is very different from that in which it was founded. We can only point out which way we think they should go and wish them well on their journey. We have done all we can. It is up to them now.

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11

On the Importance of "Being There"

Introduction

Eric Livingston has a wonderful reflection on a feeling every empirical investigator has had.

> I can spend a lot of time doing things that I don't think are getting me anywhere. I am not seeing or finding anything new. Whether or not such appraisal is correct can't be determined at the time. The larger point is that without "being there" we have little opportunity to learn anything. (Livingston, 2008, p. 156)

What Eric means by "being there" is, of course, much more than simply being present on the scene. We have to be present *to* the action going on as well. And for that, we should be clear about what we are looking at and what we are looking for. In other words, we must have an analytic frame within which to sense assemble the social activities going on *as* the sociological phenomena we want to examine. Using Garfinkel's inclined plane experiment as its example, this discussion looks at some key aspects of constructing such a frame and implementing it within fieldwork investigations. We choose the inclined plane experiment because it wears its challenges and successes on its sleeve, something which makes it wonderful as an example.

Chapter Nine (Ch9) of *Ethnomethodology's Program* [Garfinkel 2002] provides the only extended account of a project which pre-occupied Garfinkel for over a decade in the 1980s and early 1990s and which, in the end, he concluded was not the success he had hoped for.[1] Even so, from the recently published cache of materials from Garfinkel's

[1] In a recent paper on ethnography in Ethnomethodology, Anne Rawls and Mike Lynch [2022] suggest this might not be necessarily so. Following Rawls' own positioning of the report in her Introduction to it, they propose its purpose was to highlight the contingencies faced by pre-early modern scientists when mounting

nachlass, we now know that the project was to be one of the evidential centrepieces of a long-planned summary of Ethnomethodology's investigations of the natural sciences as "discovering" disciplines. This fact alone means the project is still of considerable interest. Unfortunately, the account given of it in Ch9 presents us with much the same array of problems the whole volume does. In his own introduction, Garfinkel talks of it as "proleptic", meaning it was very much an unfinished stand-in for the account he had intended to give. The Ch9 version was extracted from a compendium of notes, memoranda, letters and taped discussions and stitched together by Anne Rawls. It is neither a unified whole nor the completed account of what the inclined plane investigation was supposed to be about and what it achieved.

We are as much in awe of Garfinkel radical thinking and foundational contributions to Sociology as anyone. We understand and admire his relentless insistence on pushing his ideas to the limit. The inclined plane experiment is no different. Garfinkel takes his departure point from three sets of findings: Husserl's recovery of the foundations of modern science (what he called "Galilean Science") in the Natural Attitude of common sense; Phenomenology's display of the apodictic character of fields of consciousness grounded in sensory immersion in embodied action; and Ethnomethodology's discoveries concerning the detailed order of practical social activity. To these he adds his own aspiration to have ethnomethodological investigations yield outcomes which are of direct disciplinary/professional relevance to those engaged in the domains being studied. In the inclined plane experiment, his ambition was to demonstrate to contemporary novice and professional physical scientists how, as a practical social activity, Galilean Science is premised on commonsense interpretive methods. Had he achieved this objective, he certainly would have concretised the interdisciplinary value ethnomethodological sociology could bring a joint research endeavour.

What makes his proposal quite distinctive is that the exercise was not to be a philosophical/sociological armchair envisionment. Nor was it to be controlled historical re-enactment of what it must have been like for pre-modern scientists to come to see the material world through the lens of Galilean Science. Either would have enabled him to stipulate all

of experiments and making them work. In this, the fact that Garfinkel "lost" the phenomenon demonstrates his success. As will become clear, while it is certainly true Garfinkel claimed to have lost his phenomenon, from what he says about the exercise it was not that which he thought was central to the project's success or failure.

the conditions he required to show how that transition could have happened and, by careful scene setting and script crafting, ensure those conditions were fulfilled. Instead, with the help of colleagues he set out to learn how to do Galilean physics by setting up and running his own version of Galileo's famous experiment. The thesis is that the collective experience of acquiring this learning would be equivalent to the epiphany—revelation— Galileo himself must have undergone when forming his new approach to scientific practice.[2] Having engineered that outcome, Garfinkel intended to reflect upon his experience, extract the general implications for understanding the foundations of Galilean Science and then convey those implications to a cohort of novice and professional physicists.

All this required a number of things to be accomplished. Garfinkel had to define what should count as the pre-Galilean and Galilean scientific outlooks. Adopting a 'misreading' of Galileo's own account, he had to design, build and run a version of the inclined plane experiment which could reveal 'the gap in the Galileo's text' and hence force attention on how a transition between the defined outlooks might be achieved. To acquire the Galilean outlook requires the production of scientific results of the general form Galileo reports. These need not necessarily be new results but certainly must be results (even if basic) which practising scientists would accept as properly scientific. Once these were in place, he would need to trace how they had been 'produced' during the deployment of the practical, embodied courses of social action which constituted the work of the experiment. Finally, from the whole set-up, he had to distil scientifically relevant (i.e., not philosophically or sociologically relevant) implications concerning the dependence of Galilean scientific practise on commonsense embodied reasoning and package them up for a scientific audience.

These are stiff objectives. The reading we offer of Ch9 looks at the character and challenges of just two tasks which would need to be completed successfully for the constraints to be satisfied; the selection of a perspicuous setting for the research and the delineation and accessing of the pre-Galilean scientific actor's point of view from which to sense assemble the experimental context. We have selected these two because they are key not just when mounting empirical studies as radical as Garfinkel's but for any

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2 The key point which everyone agrees was part of his planning, was the stripping out or bracketing of the professional and institutional overlay which has accreted on science's practice since the 17th Century.

ethnomethodological investigation. Our review traces some of the methodological decisions Garfinkel made in the implementation of the project.

Section 1. The Problem and its Set-up

Problem Statement

"Galilean" is the epithet Husserl attached to the "turn" which Natural Philosophy made during the late Renaissance and early modern period. From Ancient Greek and Medieval philosophy, Renaissance philosophy had inherited the problem of securing the relationship between the world-as-experienced and the world-as-it-is-in-itself. This was encapsulated in the perennial question: How can we know how the world is independent of our experience of it? In the 16**th** and 17**th** century, increasingly the answer to this question was felt to be in the application of a logical 'method' and the primary example of such method was Euclid's Geometry. Systematic reasoning about phenomena using methods akin to those of Geometry, it was thought, would reveal the structure of the natural world. Turning to the world in this way involved three preparatory steps: abstraction of phenomena from experience; idealisation of the features of abstracted phenomena; and formalisation of the relationships among idealised phenomena. Galileo's primary notation for this formalisation was Geometric and Algebraic. To his contemporaries as well as to later scholars, Galileo represented the epitome of this nascent approach, hence Husserl's attribution.

Husserl's interest in Galilean science is its philosophical significance as a breaking apart of the common-sense identification of experience and reality and the resulting inversion in their epistemic status. For Husserl, the Galilean method became what we would now call the "imaginary" [Taylor 2004] we still rely on to acquire truth about the natural world. In his *Crisis*, Husserl [1970] analysed at length the philosophical grounding of Galilean science in our common-sense 'natural attitude' to the 'Lifeworld' of daily life and what he took to be the unfortunate consequences it has had for our understanding of the world and our place in it. It was Aron Gurwitsch [1966 ; 2010] who identified the social scientific significance of Husserl's problematisation of Galilean science. This was the requirement for the scientific actor to hold the two distinct Gestalts of Galilean Science and the Natural Attitude simultaneously. In the theoretical praxis of Galilean science, these two alternative configurations have to be bound together. However, as distinct configurations of the world-for-experience, logically they could not be fused.

It is this binding which defines Garfinkel's problem. He articulates it by 'misreading' Gurwitsch as proposing when scientists undertake commonsense practical science, they face the task of continually integrating, synthesising or entangling the configuration of experience under the natural attitude with the configuration of experience under the Galilean attitude. Identifying and describing the 'shopfloor work' of learning how to perform this task is the problem Garfinkel set himself.

To show how this binding is carried out, Garfinkel sought a setting in which the uncovering of the possibility of a mathematisation of common-sense experience of the natural world could be brought clearly into view and with it the possibility of a binding of the two Gestalts demonstrated. This demonstration would be the trajectory of the epiphany to which we referred earlier. The problem in doing this is that modern science takes such mathematisation for granted. It has become institutionalised at the heart of its investigative processes. It is built into the very fabric of its operative experimental method. Rawls and Lynch are right about this. To reveal 'the work' of accomplishing the Galilean binding in contemporary science, Garfinkel would have had to find some way to frame and set aside (or "bracket") that assumption about mathematisation whilst performing that science itself. And to do that, he would have to learn the contemporary science being done. Alternatively, he could have found a setting in which the science was mathematically naïve and hence closer to pre-Galilean forms. If he could find such a setting, he ought to have been able to acquire such early-modern science relatively easily and subject his experience of doing to ethnomethodological investigation. As we sat, he chose the first option and took Galileo and his inclined plane experiment as suitable candidate model for a demonstration of the transition from pre- to Galilean science.

Registration

Garfinkel makes his problem tractable by rendering it under EM's master trope: treat social life as the sense assembly of instructed action. The device which this rendering relies on is carried by the familiar distinction between logic-in-use and reconstructed logic. Garfinkel casts this distinction as juxtaposing of two possible "accounts" of the work of science. The first is the work as a flow of sense assembled experience undertaken under the rubrics of Galilean science. The second is the work as a flow of sense assembled experience undertaken under the rubrics of the Natural Attitude. These are the two Gestalts. Garfinkel asks what is missing from the first ("the gap in the texts") which is required under the second for the work to be done? What, to use our preferred expression, has been "effaced" from the Galileo's text which has to be included in a pre-Galilean description for the latter to be a description of how to undertake a successful experiment in terms accepted by post-Galilean science? The gap in the texts and what it stands for *is* Garfinkel's phenomenon. It consists of the concrete details of the practical doings which are filtered out from the abstracted, idealised and formalised account of the science undertaken which was provided by Galileo. In setting up his investigation, Garfinkel proposes to counterpose the description of the experiment which Galileo provides with his own suitably shaped description of the embodied flow of practical decision making and contingency management through which he and his colleagues will have managed to carry out the inclined experiment for themselves.

There are three things going on here which we need to make sure we keep separate.

1. The distinction Garfinkel proposes between the text of the inclined plane experiment as the social object which Galileo and his peers oriented to and that text as the particular sociological object Garfinkel needs it to be for his investigation. We have some comments on this.
2. The distinction he needs to make between the social experience of running the inclined plane experiment under the Natural Attitude and the nature of the experiment as construed under Galilean science from which he can extract results he can communicate to his designated scientific audience. We will have very little to say about this.
3. The alignment and elision of features of experience under both Galilean science and the Natural Attitude as enabling a possible trajectory for the epiphany required via the achievement of a binding of these Gestalts. If he can demonstrate the possibility of the transition, Garfinkel will have shown how a view which registers the world in mathematical terms could be acquired. This is what will take up most of our attention.

NOTATION

The gap in the texts provides Garfinkel with an analytic space but what precisely does he want to focus his analysis on? Here he turns to two characteristics of Galilean science which he believes have general consent. First is its aim to be probative. While this feature might exhibit entail Mao-like progress, overall progression can be and is marked by the cumulative sum of propositions whose evidential status is taken to be closed. Second, it can lose its phenomenon.[3] This latter is particularly important for Garfinkel because it offers a clear contrast with Sociology. Sociologists *never* lose their phenomenon. Or, perhaps more accurately, Sociologists can't lose their phenomenon. What he means by "losing the phenomenon" is described at length in the materials released from his *nachlass.* He takes it to be an endemic feature of practical science that a set-up, experiment or simulation can capture a desired phenomenon in one run and, on the next, completely fail under what appear to be identical circumstances.[4] Ensuring the continued presence of the phenomenon across initial findings and their replication is what scientific method is designed to do. It is how the probative character of Galilean science is secured. What is done to secure and preserve the phenomenon in this way is the element of the gap in the texts which Garfinkel homes in on.

Since the activity effaced by the gap in the text is essential to the doing of the science, there must be a way of making it describable in scientific terms. And yet, though it is known by scientists under the scientific Gestalt, the gap can only be made visible by sociological investigation of its social character. What is known is that which is routinely necessary for the science's practice, but which is also taken for granted in that practice and in its accounts of that practice's performance. This taking for granted is what makes it invisible.[5] Remember, the reason Garfinkel undertook this study was not just to make the binding of the Gestalts *sociologically analysable*. He also wanted to make these details available to physicists and hence *scientifically analysable* as relevant findings for them. If the details are necessary for preserving the phenomenon and hence securing probativeness, they *must* be scientifically relevant. But, of course, sociological description of practising science is not scientific description of practising science. To make the findings available to

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[3] Garfinkel talks a lot about this and regales us with stories which he heard from his wife, his scientific friends and others. However, it is not clear just how common the loss of phenomenon is nor whether it should really be taken as one of the characterising features of scientific practise. After all, most cultures have their popularly recounted "myths" which function in all sorts of beneficial ways to reinforce normative order. The same could well be true for science and the talk of 'loss of phenomenon'.

[4] One version of this contingency which gives the term surface plausibility is known to every R&D manager. It is the regular way a 'canned demo' of some technology only seems to work reliably when the scientist who put it together is within 20 feet of it. They don't have to do anything. They don't have to be leading the presentation. But if they are 'away', you can bet the demo won't work.

[5] Garfinkel's *Studies* [Garfinkel 1967] is replete with examples of how and why this is so.

scientists in ways that they can see as relevant means finding a form of description which straddles both worlds. It is, to use the phrase he adopts, a description made "inside-with" the science.[6] It will be a hybrid; a science-EM hybrid.

To bring out the distinctiveness of what he is aiming for, in the version of the summary introduction to his planned series of studies of EM and the sciences contained in the recently released cache of materials, Garfinkel runs an extended contrast between what he terms 'analytic ethnography' and his proposed science-EM hybrid. The first is exemplified by Lynch's work on Micro-biology; the second by Livingston's work on Mathematics. What marks them apart is the seriousness* with which their findings can be treated by the scientists they study. This difference arises because Lynch is not competent in the Micro-biology and Livingston is competent in Mathematics. Lynch gives an "outside-in" description and Livingston gives an "inside-with" one. What Garfinkel asserts Lynch cannot provide is the interior configuration of the phenomenal field of the practical science of Micro-biology as a construal of the social-actor-as-scientist's point of view. How that point of view is constituted and what it imports for the early-modern researcher is what Garfinkel himself hopes to discover for himself. This is the field of consciousness achieved through embodied immersion in the phenomenal field of the shopfloor pre-Gailean scientific organisation and management of the concrete contingencies of scientific method as evidenced in the preservation of the phenomenon.

As we have already noted, the implication of this analytic/hybrid distinction for Garfinkel is that if he is to provide a depiction of scientific practice on the basis of his approach, he will have to learn the science relevant to the work he wishes to examine. This gives him the last component of his research design. He must learn the science he needs by carrying out his scientific experiment under the conditions described above whilst *at the same time* undertaking an ethnomethodological investigation of that experiment. Rendered through his transformations under the ethnomethodological attitude, he will find what has fallen through the gap in the texts. It is that which will form the basis of his findings for science.

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6 This phrase is explained as follows:

> "Inside-with" is a phrase that Lois Meyer coined in her Ph.D. dissertation. The use of "inside-with" by EM authors should be used to criticise Merleau-Ponty's "intertwining" and "chiasm" as well as recent variants on these metaphors. [Garfinkel, 2002, p.271, fn.12.]

Section 2. Problem Specification

As already noted, the setting Garfinkel chooses to investigate is the inclined plane demonstration of the Law of Falling Bodies as described by Galileo. Here are the reasons he gives for this choice.

> We figured, we'll go to the ancient accounts, including Galileo's account of his science, and pick it up at a time when he wasn't answerable to the professional association of physicists. He had something like a crowd at court. When he would go to court what they wanted to know from him was, "What's new?" And if he could tell them what's new, then what they wanted to know in what's new was "What could we do with these balls?" What are you telling us? To roll them down this plank? Okay, that sounds like it's just right.
>
> So, we figured, if that's what physics could have been at that time it's good enough for us. We went for that.
>
> The idea was to specify what discovering work in the work site looked like when there was a serious science you were doing, but no professional association or accepted literature to which the work was accountable. [Garfinkel 2002, p. 267]

Now, we don't for a moment think Garfinkel approached the specification of his project in as off-hand a way as he makes out. But the above does point to two important features we should explore. The first is the context in which "Galileo's account of his science" was originally given, which was not, of course, anything like the scenario Garfinkel offers us in his text as well as the endogenous character that account has in the context it is given in. The second is the accountability as science for Galileo (and his peers) of the work reported; what were the expectations Galileo oriented to and hence had to satisfy in order to show his work could be taken seriously? Just as important, of course, is what he wanted his peers to see his contribution as being. What sense assembly of his work was he seeking? While Galileo may not have been a member of a modern professional body, he wasn't Robinson Crusoe the physicist either.

THE DISCORSI ACCOUNT

Ch9 quotes the whole of Galileo's description of his demonstration. However, no context is provided. The only piece of information about the text's character (what it was for and how it might have come about) is the highly unlikely suggestion made in the quotation above about the occasion on which the description might been given. Galileo's account is to be 'misread' as instructed action but without any attention being given as to what kind of a document it was and how it 'worked' in the settings in which it was read. What is the relationship of the description to the experiment it describes? Without considering its context-of-use, we have no way to tell if the account-as-social-text could reasonably be expected to bear the weight of being deployed sociologically under the rubric of instructed action.[7] As we work through the case Ch9 presents, this problem will return again and again.

What do we mean by 'context' here? Galileo describes his experiment in an aside which occurs in the middle of a mathematical treatise. It is presented as an interruption in the flow of a conversation. But, of course, the conversation is Galileo's rhetorical artifice—a standard narrative construction used in the Natural Philosophy of the time. These narratives consisted of extended monologues of declarations and responses containing formal arguments and their proofs. In the *Discorsi*, there are three interlocutors: Salviati (Galileo the scientist), Salgredo, and Simplicio (interested and well-informed citizens). The text quoted from the *Discorsi* is not extracted from nor a rendition of Galileo's own laboratory journal (and not taken from any of the relevant *folios* which contain his journal entries either). Neither does it appear in any similar memorandum. It exists only in this one place, a published formal presentation of *mathematical* results. In the treatise, Galileo does describe concrete, substantive (and hence empirically investigable) objects in mathematical terms. He also provides descriptions using terms referencing physical objects, their motions and relationships in term of idealised mathematical objects. It is not immediately clear the description of the demonstration is not yet another idealisation.

Throughout the treatise, Galileo introduces his theorems as the preliminary results of his investigations. Whatever his inductive/abductive method is, from the way the dialogues generally go, we can presume the point of describing the experiments is to confirm the formal proofs given. They are demonstrations of the empirical adequacy of those proofs. In other words, they are not the 'data' from which the findings (proofs) are derived. Galileo

[7] Some say that was the point. By treating it in the way he does, Garfinkel ensures the "gap in the text" will have to be confronted. The gap is an axiom about the text. But that justification entirely misses the key issue of the whole design. This is not any old exercise in demonstrating the Law of Free Fall. There are lots of ways to demonstrate the Law which do not require reproducing a version of how Galileo says he demonstrated it. The question is how can Garfinkel assure himself (and us, the readers) that the ways in which he fills out the missing detail does support and sustain his exercise as a reasonable version of what Galileo would have had to have done? That, presumably, is what he wanted to do rather than just replicate either a demonstration of the gap or a demonstration of the Law. If it wasn't, why bother with Galileo in the first place? There are plenty of other places you can find mathematically naïve science going on.

proceeds by stating axioms, deriving theorems and proving them. When he describes any of the experiments he has undertaken, it is to show results which match (or seem close enough) to those 'predicted' by the proofs.

The theorem in this case states the distance travelled by a body falling under constant acceleration is directly proportional to elapsed time ($d \propto t$). Having proved this theorem, Galileo develops a corollary. Under constant acceleration, $d = t^2$. He then proves this using a combination of geometric and algebraic methods. His proof implies if he can consistently collect measures for d and t and they show d is equal to t^2, he can claim to have demonstrated the acceleration of a falling body is constant. At the end of the proof (p.178), Simplicio asks for evidence that 'nature' (the motion under gravity is called 'natural') really does conform to the 'law' Galileo has proved.

In response, Galileo tells a story. A board 33 feet x 9 inches x 3 inches (modern conversions of cubits and finger widths) was sourced. A channel 1 inch wide was grooved on its edge and lined with parchment. At one end, the board was lifted by 3 feet. Repeated runs of rolling a bronze ball down the slope were undertaken and the elapsed time measured in pulse beats. Runs which varied by more than 1/10 of a pulse beat were rejected. Runs with the board fixed at different inclinations were included. The full, ¾, ⅔ and ½ lengths were used. Time of descent was measured by synchronising the run of the ball with the collection of water in a glass. Ratios of the weights of the water were used to measure the ratios of elapsed times. This set up allowed the proofs to be confirmed. The phenomenon in Galileo's account is an isomorphism of the Law deduced from the mathematics and the empirical results obtained by rolling balls down a slope. What he has demonstrated is the proportionality of distance to time for a body in free fall (given the absence of any relevant dynamic forces other than gravity, but he doesn't say that. His story has already told you how the most important of such forces, friction, was effaced).

Garfinkel calls Galileo's story a 'careful*' description of the demonstration's 'details*'. Careful* description of the details* of social action is one of the desiderata for ethnomethodological accounts. Elsewhere in *Program*, he defines what these terms mean. Details* means a description of an action exhibits the "immediacies and certainty of its identifying orderliness" (p. 273) and careful* means "so written as to lend itself to reading alternately as instructed actions" (p. 264 note 1). To invoke a phrase Garfinkel himself used elsewhere, these are curious terms to apply to Galileo's description if only because it lacks almost all of the detail of how the setup was built and used.

The most obvious thing missing is a set of results. In other descriptions, Galileo does give his measurements and their construction. Not here. Then there are the detailed dimensions of the board and groove which are given and but not the kind of wood from which it is made.[8] Since the dimensions are crucial parameters for the demonstration, what are Galileo's reasons for choosing them? In fact, did he choose them? What difference would it make, for example, if the board was not 22 cubits by ½ cubit by 3 finger widths? Or do these numerals, like Ali Baba's 40 thieves and Goldilocks' 3 wishes, really function as culturally given 'magic' numbers? What about the 2 cubits lift? Is that special? Why not 1 or 3 cubits? Are these features ways of managing unknown "demonically wild contingencies" which Galileo was forced to use or just any old set of arrangements which he threw into the story? If, as Rawls and Lynch suggest, Garfinkel wants to know how any of this might matter, would he not call these issues out? And if he doesn't comment when this detail is missing then what is all the fuss over the criticality of the configuration of details* about? Certainly, compared to Garfinkel's own list of things he says he was planning to include in his own description, Galileo's presentation of his own cupboard of contingencies seems pretty bare.

If we step back for a moment, though, and think about the text and the experiment's place in it, one thing becomes obvious. While a lot of the detail about how the experiment was designed and run is missing, one central thing from the point of view of his new science is very visible, the place of indirect measurement. This is important. Pre-Galilean scientists were familiar with cross-tabulations of tallies and direct measurement of natural properties (counts of pulse beats and length of board runs, say, in this case). These we can call "naturalistic noticings". What was crucial in the shift to the Galilean point of view was the realisation that the relationships among these noticings could be expressed arithmetically.[9] That is, some observed and measured data could be expressed as arithmetic functions (sums, products, ratios or powers) of others. Measurement, then, is not only direct observation. Galileo's Law involves direct measures of distance and time being used to generate an

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[8] Anyone who works with wood knows different woods have different surface properties. Some (e.g., Ash and Elm) can be planed very smooth. Others (e.g., Cedar) remain fluffy no matter what you do. So, surface properties matter which might be the reason parchment was required. Now, you might say his interlocuters know all this. May be so. But then what is being 'omitted' from Galileo's presentation is just the same 'taken for granted background knowledge' Garfinkel suggests required to make texts in Sociology and Physics careful descriptions. The description Galileo gives is both careful* and not careful*

[9] We should remember that although Galileo is credited with this 'revolution', he did not achieve it all on his own. Innovations in double entry book keeping, for example, were going on in Italy at much the same time. These led to the creation of all sorts of new 'derived accountants' objects' such as measures of cash-flow.

indirect measure of the rate of change in velocity (i.e., acceleration). This is done by the application of a mathematical function (the squaring of time) to those direct measures. The length of the board and the time of descent have a *functional* relationship to the ball's rate of change in velocity. Under that function, the two numbers deliver a value for acceleration. If the length of the board is varied, does the rate of change in that value vary? If it doesn't, then the ball travels under constant acceleration. That falling bodies travel at constant acceleration is Galileo's Law. The whole experiment is envisaged as a device to produce indirectly measured values for acceleration by generating the direct measures as input for the function. It is a demonstration machine not a discovery machine.

What has to be untangled in the specification of Garfinkel's problem, then, are three not two steps. The first is commonsense experience of informal measuring (this ball rolls faster than that ball). The second is the experience of scaled measuring (this ball takes 5 pulse beats. That ball takes 6 pulse beats). The third is the combination of direct measures to provide indirect measures of the kind which Salviati reports. The first and second tasks are Pre-Galilean science. The third is Galilean. The point of the Galilean experiment is to provide a setup which, under controlled manipulation, demonstrates the functional relationships of the proof actually do hold in nature. As Galileo put it, his experiments are designed to show the laws of nature are written in the language of mathematics. In his version of the experiment, Garfinkel has to discover for himself how to turn commonsense experience of perceptual differences into directly scaled measures and then into indirect measures by demonstrating their arithmetical relationship to some properties which can't be directly measured. Learning first that this last step can be successfully done and then how to do it systematically is the epiphany.[10]

Given all this, it is pretty clear Salviati can't be used as the resource for constituting the social-actor-as-pre-Galilean-scientist. The outlook he presents in the text is not pre-Galilean and the conduct of the experiment as described in the text is not pre-Galilean either. As we

[10] In the paper we quoted at the beginning of this discussion, Livingston brings out this very point but with regard to another of Galileo's experiments, that of the pendulum. When undertaking his own studies of the pendulum, Livingston had to learn how to manipulate physical features of the experiment in a controlled fashion in order to produce direct measures, that is observable, scalable measures of actions of one part of the set-up which in some mathematised combination could stand for actions of another part. In so doing, he moved across the pre-Galilean/Galilean divide.

have just explained, the key to Galilean science is the combination of methods applied to direct *and* indirect measurement. It is Salviati's proofs which uncover the functional relationship. The experiment demonstrates it. This does not mean the inclined plane set up could not be re-designed and used to discover that measured relationships might be possible. It is simply that Salviati does not describe setting it up and using it that way. For Salviati, it is a given that relationships among objects in the world can be described in terms of mathematical functions. The pre-Galilean scientific outlook consists in looking at the world in terms of scaled measures of directly observable variables. In contrast, the Galilean scientific outlook consists in looking at the world for functional (usually arithmetical) relationships between scaled measures which can stand for indirect measures of other variables. Galileo didn't discover this possibility by running experiments. He discovered it by reflecting relationships he observed in the world around him, undertaking formal proofs of theorems he derived for those relationships and then proving them. His experiments were to demonstrate the "reality" of those relationships in nature. The demonstrations he describes in the *Discorsi* are exercises in Galilean science not a discovery process for the possibility of Galilean science.

This might sound like nit-picking, but it isn't. It is about the construal of the social actor as an object for investigation. What do we attribute to them as their point of view? The Salviati-in-the-text can't be used as the resource for Garfinkel's pre-Galilean social-actor-as-scientist because he arrives at the experiment and its properties (the measures) via the proofs. He is already Galilean. What Garfinkel needs for his version of the pre-Galilean attitude is a social actor who can discover indirect measuring through the experiment. And that is not Salviati. Or rather, it might be Salviati if he did not already have the mathematical view on the world he clearly has. To constitute his scientist-as-sociological-object, Garfinkel would have to set aside the mathematical knowledge and expertise Salviati has acquired and displays regarding functional relationships. It is because it is designed to enable those relationships to be exhibited which makes the experiment Galean science. What Garfinkel and his colleagues have to do to achieve the objective they have set themselves is to turn a set of noticings of their own into direct measures and then derive an arithmetical function for those measures which transforms them into indirect measures of some relevant phenomenon. Simply repeating what Galileo/Saviati claims he did won't do it, because that whole experiment (the setup and the results) is predicated on the certainty such relationships can be demonstrated.

SAVING THE THEORY

There may well be a reason for the lack of detail in Galileo's description. As we have already noted, the story Galileo gives is nowhere to be found in his notebooks. There are other setups with measurements which approximate to the Law, but they are not used. The consensus among the numerous attempts to reproduce his experiment is that Galileo could not possibly demonstrate the Law because of the practical circumstances of his own experimental conditions. Naylor (1974), for example, points to the following:

a. Although the groove is lined with a smoothing surface (parchment), replications of the experiment show this probably would have added rather than reduced significant friction. Galileo was well aware of the effects of friction as well as an acute observer of his experiments, so he is unlikely not to have noticed the difference the parchment would have made.
b. Inevitably, the bronze balls would have caused increasing wear and tear on the parchment surface, especially if the experiment was run 100 times (the number Galileo claims). This wear would create lateral (rocking) momentum and small ridges, both of which would slow the ball's descent by more than the 1/10 pulse beat rule. The need to discount so much data would surely have forced Galileo to re-design the set-up.
c. It is impossible to achieve the required accuracy of timing (pulse counts to a tolerance of 1/10 of a second approximately) without modern stop watches.
d. Consistent synchronisation of ball release/arrival and water collection opening/shut off to the tolerances required has not been reproducible. This implies (random) measurement error in the results for distance and time were inevitable.

Because of a – d, the mostly likely scenario is that Galileo opted to accept his 'ideal' mathematical proofs in the face of an inability to get better than overly loose confirmations of the proof's predictions. In other words, the story he tells 'saves' the theory (the proofs of the relationship $d \propto t$ and $d = t^2$) in which he was interested. Of course, even though this has been widely commented on in research on method in Physics, it hasn't stopped descriptions of the inclined plane experiment featuring prominently in introductory texts in Classical Mechanics. But there are very good pedagogical reasons for that. The Law is a crucial stepping stone in the development of Classical Mechanics and its eventual formulation by

Newton. If you want to understand what Newton achieves, it is essential to know what Galileo proved. This is notwithstanding the difficulty of demonstrating it.

So, a not unreasonable way of viewing Galileo's experiment is as a convenient fiction inserted into the *Discorsi* as a way of saving his theory. This is important but not for the obvious reason. It does not imply there is no point is trying to make Galileo's experiment or some version of it work. Generations of seasoned and novice physicists have tried and continue to try to do just that. It is rather that the *Discorsi* does not provide a resource for descrying and accessing an occasion when the move from a pre-Galilean, naïve mathematical scientific Gestalt, to a Galilean one took place. Everything in it, and this is especially true of the inclined plane experiment, pre-supposes the mathematisation of nature is not just possible but can be done and so is required. It is the mathematisation which makes the rolling of the balls, the counting of the pulses and the collecting of the water Galilean scientific practices.[11] The whole text is not just about finding the possibility of the mathematisation of nature but the certainty of the results if that is done. In other words, the experiment described in the *Discorsi* is not a perspicuous setting for revealing the transition to Galilean science and the idealisation of Salviati is not a good resource for characterising a pre-Galilean-scientist-as-social-actor who undergoes the transition from pre-Galilean to Galilean scientific points of view.

Clearly, this has implications for Garfinkel's planned exposition of the gap in the texts and for his findings as representations of how to bind the two Gestalts. He cannot just take the text "as is". Instead, if he wants to use the text as he proposes, he has to interrogate that text for its methods of mathematisation and for the practical skills Galileo could call upon as well as the resources he must have had access to. Having circumscribed their relevance, scope and application, Garfinkel could then try to find ways of stripping these aspects out from his own exercise. Doing that would amount to an *epoché* of the Galilean scientific attitude. His choice of text has taken him back to where he started; the need to discount the mathematisation in the science. What the *Discorsi* is about is demonstrating the efficacy of what were for the times sophisticated algorithms providing measurements of natural phenomena and not just the application of numerical procedures to generate numbers

[11] In that sense, the identification of the practicalities of the 'measuring' provides the answer to the application of Shills' question to the inclined plane experiment. See [Garfinkel et al. 1981]. It is what makes it science and not, for example, play, a pastime or some other small group activity going on using the set up.

representing physical events. In sum, his reliance on the *Discorsi* is likely to complicate the counterposing strategy Garfinkel was pursuing.

Section 3. Analytic Protocols

THE EXPERIMENT

The premises for the inclined plane exercise were:

1. The lived work of doing the proving of the Law and demonstrating its truth status in the experiment was structured to prevent the contingent loss of the phenomenon which the Law describes.

2. As with all scientific reports, Galileo's description of what was done and how it was done effaces almost all of this work.

Because he had never been a scientist nor conducted an experiment as a practising scientist would, Garfinkel feels he is unable to 'read between the lines' and fill in this omission. He doesn't know what it means for a science/scientist to find and lose a phenomenon. As he says, he can ask the scientists but since he doesn't know the science, he cannot understand what their answers mean in the ways the scientists do. No matter what they told him "We could not make their affairs teachable *to them* by what they were teaching us".

In undertaking the experiment, Garfinkel wanted to reveal the "measured and measurable" lived work of doing the proof and its demonstration in *all* the detail required to ensure the preservation of the Law of bodies in free fall.[12] This lived work is his phenomenon. The preliminary list of what will need to be recorded on p. 268 is, of course, just a list and necessarily contains only some of the detail. It might seem 'obvious' much of what is involved in mounting and carrying out his experiment cannot be materially relevant for the success of the exercise as he has defined success. The only trouble is Garfinkel is fond of quoting the story about James Olds' reaction to his lab assistant's cleaning and tidying the Lab shelves. Who knows in advance of any scientific experiment what will turn out to be critically material to it? In fact, that very question is the lodestone for what he calls h-sociology*. It is the *raison d'être* for studies like the one we are discussing. We are given lots and lots of details whose properties are to be examined but not the basis for selecting them nor the enumeration of

[12] Except, of course, only the physical demonstration is covered in Garfinkel's description.

which of the features/properties of those details will be picked out. Both Garfinkel and we know he could not list all the relevant detail in advance nor summarise in the eventual report all that did eventually turn out to be important. The question is what were his selection criteria and how did he operate his stopping rule? Answers to these questions indicate how he set the boundary conditions on his version of the experiment.[13]

PRE-GALILEAN SCIENCE AND ITS PHENOMENAL FIELDS

The heading for the section we now have in view is "What Did We Do?" and it opens with the following:

> An Ethnomethodological study of Galileo's inclined experiment describes the demonstration's lived work in phenomenal details. These details exhibit the demonstration's engineered design as a coherent domain of empirical phenomena of social order in physics: The law of free falling bodies. [Garfinkel, 2002, p. 273]

This is pretty cogent. What Garfinkel has his eyes on are all the phenomenal details of the experiment's lived work *modulo* the unavoidable lived differences which arise from the different circumstances in which the original and Garfinkel's version were carried out. Naturally, neither we nor he can know how much difference these unavoidable differences might turn out to make for the comparability of the two exercises. Will Garfinkel's version work? Will the results confirm the proportionality of time to distance enough times to count as a demonstration? Then again, the Law and its corollary ($d \propto t$ and d = t^2) are mathematical objects. The giving of proofs using the arithmetic of Geometry can certainly be treated as a social practice and it is a perfectly proper sociological axiom to insist their practical demonstration also constitutes the construction of a social order. Although one can imagine descriptions of the lived detail of proving the laws featuring in a description of a version of Galileo's demonstration, the performativity of that version does not affect their fundamental epistemological status *for Galileo* as empirical certainties.

What Garfinkel does go on to provide are informal summaries of baskets of difficulties, challenges, troubles, requirements, necessities and redirections he and his colleagues encountered when setting the demonstration up and then running it. As an

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13 Notice we do not say "how well Garfinkel's set up maps onto that of Galileo". It remains to be seen if Garfinkel's experiment has any kind of mapping relationship to that of a pre-Galilean scientist we might be able to envisage and the work such a scientist would have to have carried out.

ethnographic account, though, much of the key detail is missing. Take the grooving. This was first to be done using a DIY router, but that idea was quickly given up. Why was that and did those reasons matter? Did opting for wall moulding glued onto the board change the phenomenal character of the experiment? Did the very different physical properties of the solidified, sanded epoxy resin forming the channelling make an important/minor difference? Since the experiment had not already been run, they could not know the answer to such questions, so on what basis was this decision made? What alternatives together with their advantages/disadvantages did they consider? How did they check and calibrate their suppositions?

In assembling descriptions of relevant phenomenal field properties, Ethnomethodology borrows a simple sorting device from other forms of fieldwork ethnography. It asks: "Why that there?" The answers provide grounds on which to array the identified properties. Here are a few examples where Garfinkel might have asked this question but, from the description given, appears not to have done.

1. Setting up the audio and video. How were video angles selected? How were cameras positioned to allow consistent marking of transitions down the board? Did the requirements for these choices affect the design of the core set up? If so, how was that design adjusted? What precisely was the purpose of the audio? Was it for ancillary commentary while the experiment was in flight? Or were there expectations concerning intrinsic critical 'experimental auditory details' which meant they should be collected? If so, what were they, how were they identified and how were they to be used?
2. We are told lots of different metaphors, images, formulations and modes of description were tried out by the team as useful ways of describing for each other what was happening during the building and running of the experiment. Which of these survived and why? Even more important, how did they feature within the courses of action in which they were used? What was their 'performativity'?
3. At one point, the basic distinction between a set of experimental protocols and its actual implementation is pointed to. Given how key measurement and its transformations is for the binding of the Gestalts, it is odd there is no description of how that distinction emerged in the experiment, nor how it was understood, worked through and then assimilated and effaced.

4. Reference is made to lots of learning and lots of practice requiring lots of repetition. Actors, sports people and practitioners of all kinds know the importance of training 'muscle memory' to allow focused attention on the physical particulars of performing a course of embodied action to be dispensed with. But just how was the training of muscle memory done in the experiment?[14] As part of this description, reference to made to the Bergsonian distinction between experienced inner time consciousness with its intrinsic fluidity and outer 'marked', metronomic clock time. Again, we are given no detail of how this distinction was deployed and hence no 'inside-with' description of its contextual character.

The most important of these under-described bundles of detail is the "loss of the phenomenon". We learn "the board absorbed moisture during a night of heavy rain, So, *we had lost our phenomenon*." [p. 276]. But what exactly constituted 'losing the phenomenon' here? Which phenomenon had been lost and how did they know? From the discussion, we can assume it was the ability to demonstrate $d = t^2$, but this was Galileo's phenomenon. The phenomenon for Garfinkel's version is the lived-work details of carrying out the experiment as part of an epiphany. So how did the warping affect the ability to demonstrate that? [15] And what was it exactly about the board caused it or might have caused it.[16] On this key feature of the lived work and its scientific implications, the detail is missing.

In the end, while the account contains an extensive list of things, many of what on the surface appear to be critical elements are not described in the detail seemingly required under the rubrics for hybrid EM-science* Garfinkel sets out in *Ethnomethodological Policies and Methods* (Ch5 of *Program*).

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[14] There is reference in the Lynch release to Todes' (2001) reflections on this issue. They are not picked up in this report where they would seem to be very germane.

[15] What was it about the condition of the (wet) board which prevented them being able *now* to see d would not continue to be equal to t^2?

[16] Remember the central feature of the Law is its universality. On the theory, the Law remains true no matter what circumstances it is tested under. It is just you can't demonstrate it. What trials did they make? What remedies did they apply? What determined the stopping point on remediation? That it might seem obvious a warped board is no use *cannot* count as an h-sociological* conclusion. It is not an "inside-with" description.

Section 4. Analytical Results

THE SHOP FLOOR WORK OF PRE-GALILEAN SCIENCE

As with the previous section, this section of CH9 begins by summarizing what it will reveal. This the carefully* described domain of the Law of falling bodies. The term domain refers to workplace-specific-this-worldly-work, in this case of uncovering the Law of falling bodies. This introduction seems clearly to couch the phenomenon as the co-relation of the two orders of description, Ethnomethodology's description of the lived work and Physics' description of the demonstration. It is followed by a list of topics. Eventually, we get this:

> We came upon the *phenomenal field of Galileo's experiment with the inclined plane in detail given everything that detail could possibly be*. That's a big mouthful. We found we were getting beautiful, beautiful things with an inclined twenty-two foot board, with billiard balls, and putting those balls on a track of finely sanded wall moulding, and hearing-watching them roll, to the point of their rolling in the watched-hearable ways they did. So we were beginning to specify the phenomenal field of the experiment—and now you can sense the trouble. [p. 278]

The one thing not mentioned is the forging of a collection of measured results concerning the proportionality of distance to time or some other feature of the experiment, which is what, from the acquisition of the Galilean point of view, the experiment was about. Before they "lost" their phenomenon, did they confirm Galileo's Law or some other similar relationship? If they did, that would certainly close any potential gap between some envisaged piece of pre-Galilean scientific work and the that which Garfinkel undertook. It would also lend some credence to the claim the "big, beautiful things" they were getting bear some relationship to the shifts in phenomenal field a pre-Galilean scientist might experience. It might even lend some validity to the claim that going through the experience of undertaking the experiment had resulted in the required epiphany and that the ethnomethodological account of the experiment was a description of that transition.

The trouble Garfinkel says they ran into can be put in a single sentence. The description they could give of what they had done was of no interest to the professional and novice physicists to whom they tried to give it. The team had run up against the Gestalt divide which had defined their problem and they had no way get through or over it. Although Garfinkel does not say so, it seems safe to surmise that they did not do what Livingston had learned to do. They had not developed a method for generating properties of the experiment's phenomenology (their embodied experience of immersion in it as a field of consciousness) which could then be mathematised as measures of the required physical properties of the runs of billiard balls. If they had, they might well be talking hybrid EM/Physics. Instead, what they could describe was the ethnomethodologically construed embodied practices of building and running an experiment. But the physicists had no interest in that. There was no learning for physicists in it. All the interesting work, all the innovation that has been secured was on the ethnomethodological side of the EM/Physics constructed experiment.

Garfinkel did "come upon" a phenomenal field and he certainly "found" some phenomena. But they didn't constitute the experience of coming to see the world from the Galilean scientific point of view. He worked hard to create/produce the configuration of an experimental experience and keep producing it. The problem was the things he and the team had found were either invisible for Physics, ineffable in Physics or of no interest to Physics. What they had learned the physicists didn't want to learn or could see no point in learning. No transition had been made and no gap closing achieved. Since Garfinkel says teaching the physicists what the team had learned about in doing the physics they did and its scientific significance was the express purpose of the experiment, the experiment hadn't achieved its main objective. In addition, from the report we don't know if it achieved its secondary purpose, the revealing of the shopfloor lived work of learning to demonstrate the Law of falling bodies or some other putative law. We do know, the team was convinced they lost their phenomenon. But the report doesn't tell us what *ethnomethodologically* this amounted to. Failures in science and elsewhere are not uncommon. They are certainly not necessarily disasters. If responded to appropriately, can be of great value.

Outcomes of the Experiment

The objective of the inclined plane experiment was to develop a hybrid EM/Physics account of that experiment's lived work. Validation that the objective had been achieved would be found in the seriousness* with which the audience of physicists to whom the results were to be disseminated took those results. In taking them seriously, they would confirm the results were of significance for the conduct of the Physics being done and not simply a sociologically interesting rendering of what doing the experiment entailed. Garfinkel is frank about the outcome. They did not achieve that validation.

To what was the failure attributed? Here are the options offered.

- It was part and parcel of the character of this kind of investigation and of doing this kind of work. It forms one of the normal natural troubles of hybrid ethnomethodological investigations.
- It was Garfinkel and his colleagues' poor communication and/or pedagogic skills.
- They started in the wrong place with the wrong topic. The one they chose was always likely to be recalcitrant. But if this was so, which would be better ones to start with?

As we have suggested on several occasions, we think the third option is the most likely. The problem set-up and its registration made it unlikely the experiment could be successful. There were three elements to this. The text chosen, i.e., Galileo's account of the Law and its demonstration, resists being treated as instructed action of the kind Garfinkel wanted. The description chosen did not readily fit the role of representing an idealised trajectory from a pre-Galilean to a Galilean scientific outlook. Neither does it display the counterposing of viewpoints necessary for that transition. Developing the proofs and then running the demonstrations would have required sophisticated (for the time) mathematical and physics knowledge as well as significant understanding and skills regarding the performance of the physics which was being developed. In short: seeking to discover the proofs and arranging their demonstration would only make sense to and be possible for someone who was already a reasonably proficient practicing Galilean scientist. Third, Garfinkel and his colleagues found no way to render their ethnomethodological descriptions of the shop floor work of the experiment into scientifically relevant and scientifically shaped measured findings. In other words, they had found no way to render their findings as in ways which would align with how Physics might describe the experiment.

One way to summarise the problem which Garfinkel started with is to suggest the translation of common-sense experience into mathematical formulations ruptures the structures of the initial experience. At the end of their experiment, he and his colleagues were unable to find an "inside-with" form of description which repaired that rupture sufficiently for the scientists who were their audience and partners to be able to use it to translate their Galilean viewpoint back into the phenomenology Garfinkel had described to them. Garfinkel and his colleagues had not been able to traverse the hoped for epiphanic transition. As a consequence, but going in the other direction, neither could the physicists.

Section 5. Conclusion

The inclined plane experiment was not the only investigation of Galilean science which Garfinkel undertook. He made some initial studies of introductory Chemistry with David Sudow and there is the famous Pulsar paper. In their own ways, neither was entirely successful. Nor is it the only ethnomethodological study of Galileo's work. Dusan Bjelic and Eric Livingston too have looked at Galileo's experiments [Livingston 2008; Bjelic 2023]. What is interestingly different about their studies is that the deployment of the physics being done is front and centre in the exercises they carried out. Both show they understand the nature of the physics being undertaken in the experiments they are concerned with. Garfinkel underscored the importance of acquiring relevant domain knowledge when setting up his problem. In the end, the centrality of the practicalities of that knowledge is missing from his description and it is that which lies behind the failure of the experiment. Perhaps the lesson we should draw is that the shop floor work of experimental science relies as much on what constitutes the 'haecceities' of the relevant science as it does on those of the common sense Natural Attitude.

All of which takes us back to the importance of "Being There" and the problems of selecting a perspicuous setting and defining the actor's point of view. Neither can be decided in advance of close observation of the domain to be investigated and the activities which go on there. Whether the research site is a factory, an office, a school or even one's own study, it is a mistake to presume you know in advance what it must be like as an environment of objects for sociological investigation. The first step in any investigation should be reconnaissance and the acquisition of enough local knowledge for an initial projection of its sociological construal as co-ordinated courses of action. From that construal, decisions can be made about its suitability as a perspicuous setting for the phenomenon one is interested in and the constitution of the social actors who carry out the courses of action under the sociological description to be given. As Garfinkel's study shows us, without "being there" and present to the action taking place as the endogenously produced courses of action they are, there is very little chance of providing any kind of ethnomethodological description, let alone the inside-with descriptions required by hybrid/EM.

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12

The Possibility of Hybridity

INTRODUCTION

Garfinkel talks in his *nachlass* of there being two Ethnomethodologies. In fact, his comments are about two Ethnomethodologies of the natural sciences, but the point is a general one. One is the kind which most of us have done and continue to do. Garfinkel calls this "analytic ethnography". The other is a form which few if any of us can do and which he calls "hybrid* Ethnomethodology" (hybrid*EM). For Garfinkel, the distinction turns on whether the findings of an investigation are couched as "inside-with" descriptions which ensures their results can be "taken seriously" by practitioners in the domain under investigation. By "taken seriously", he means incorporated either in formulations of how it sees the world of its endeavors or in how it acts on that world within the praxis of the relevant domain. What "inside-with" designates is more complicated and has to do with overcoming the observer/observed, insider/outsider dualities and, with them, the counterposing of category related accounts. Providing inside-with descriptions requires the observer/investigator to be both competent practitioner in the setting and hence able engage with it in the same ways practitioners do and, at the same time, able to reflect sociologically on the taken for granted ethno-methods which practitioners deploy. Inside-with descriptions are the joint product of both points of view.

In this commentary, we ask about the reality of the analytic ethnography/hybrid* EM distinction. By "reality", we mean two things. First, does the distinction catch an empirically real distinction? Has it been realised and displayed in actual cases? We will suggest there are scant grounds to suggest it has and some significant ones to suppose it hasn't. Despite what Garfinkel says in the draft just mentioned and elsewhere, the evidence for the existence of these two types of investigation is just not there.[1] This does not mean the term does not mark a possible distinction, only that the distinction has not been demonstrated through actual cases. The second sense of "reality" refers to conceptual coherence, a property which is required for it to be a distinction which can possibly be deployed. We will propose it could be a possible distinction, but pursuing it is likely to take EM a long way from where it is now and demand much more of it than it seems willing to give.

We begin from what is admittedly a debateable premise. The subtitle to *Ethnomethodology's Program* [Garfinkel 2002] is not a piece of intradisciplinary marketing, but a serious claim about Ethnomethodology's (EM) central achievement. EM has provided demonstrations of Durkheim's aphorism "the objective reality of social facts is sociology's most fundamental phenomenon".[2] If EM has shown how to access and describe the objective reality of Durkheimian social facts, then it is because Garfinkel got lucky. In 1947, when he began work on what would eventually lead to his thesis and the beginnings of EM, there was no available way to get from where he was then to Durkheim as he wanted to interpret him during the period after 1970, when the work presented in *Ethnomethodology's Program* was largely carried out. He had to get to that position via Parsons, something he didn't know when he joined Harvard to pursue his PhD. In fact, he could only have got there via Parsons—so proving the Irishman's dictum both true and false. In demonstrating the lacuna at the heart of Parson's solution to the problem of order, Garfinkel found EM's 'method'. Having found that method, he undertook the programme of investigations which became Ethnomethodology.

Garfinkel's luck ran out when, using the wholly new conception of EM as a 'hybridising' discipline, he tried to extend its reach to cover the natural sciences. He couldn't bring off the same 'coup' regarding theory and description in the natural sciences, and Physics in particular, he had with Sociology. As a result, his own study of Galileo's demonstration of the Law of Free Fall was unsuccessful. As we suggested in our discussion of that experiment, this was because Garfinkel had failed to appreciate both the extent to which Physics after Galileo is itself a hybrid and its reliance on modalities of mathematisation. At its core is the development of mathematical theorems and

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1 We are aware of the implication of this conclusion for how we should read Garfinkel's later writings. We will not dwell on it here.

2 This is Anne Rawls' paraphrase of one of Durkheim's propositions. We won't ask which, though it probably does matter.

related proofs. What Physics does is demonstrate those theorems in experimental set-ups. In developing its theorems and proofs, Physics applies mathematics to physical phenomena. The mathematics drives the demonstrations. If EM wants to intervene in the praxis of Physics in the ways that Garfinkel describes, it has got to show how its findings impact the mathematics—the theorems and proofs. It has got to reach through the data to the mathematically formulated phenomena. This he failed to do.

Section 1. Foundations

To appreciate what is at issue in the distinction Garfinkel makes, we need to remind ourselves of how it all began: his Doctoral Thesis. These days, if this is mentioned at all, it is usually identified as a critique of Parsons' Structural Functionalism. Like most soundbites, this summary is both partly right and mostly wrong; somewhat helpful and extraordinarily misleading. The thesis certainly does have the identification of an omission (or oversight) in Parsons' standard account at its heart. Moreover, it does makes it clear the gap in Parsons' argument leads to some irresolvable problems with its viability as the general sociological theory of the nature of social order.[3] But what is often missed is that these problems only become apparent and can only be grappled with if one commits oneself to a single-minded pursuit of Parsons' goal, to the frame of reference he sets out and to its premises. Garfinkel does this and then *pushes them to the limit*. What the thesis says is that there is nothing out of the way in Parsons' premise as a *premise* for a theory of social order. The phenomenology of our experience of social life is orderly.[4] There is nothing wrong with the 'what' of Parsons' account. The difficulties lie in using the theory to describe the *how*. These only become visible if the account is treated as a detailed prescription for the workings of social order in any and all circumstances rather than an albeit detailed theoretical gloss of its general character. This is hardly critique in the normal sociological sense of "problematisation". It is, rather, an exercise in rigorous adherence to the terms of a conceptual structure; an exercise which revealed a previously unremarked and monumental lacuna in the argument.

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[3] The thing to bear in mind at this point is that in North America during the early 1950s, there really was only one general theory which anyone could take seriously. In his *Theory of Action,* Parsons had hammered critical bits and pieces of 19th and early 20th century social thinking together as a possible sociological solution to the problem of order. In *The Social System*, he went on to integrate that solution into the tenets of system theory. No other theory at the time (or after) matched the scope, systematicity and precision which Parsons aimed for and achieved.

[4] As Harvey Sacks once said: "There is order at all points".

Furthermore, EM did not leap, Athena-like, from Garfinkel's brow in the midst of his thesis. The work he completed left him with the conviction radical adjustments were needed in the premises of standard professional Sociology. What those adjustments should be and what their implications might entail had yet to be worked out. As he scoped and investigated the issues, he became convinced no amendments to Structural Functionalism nor even its reconstitution would suffice. A wholly new and radical methodology was required.[5] Creating that methodology was his lifelong project.

PARSONS' LACUNA

Garfinkel arrived at Harvard already convinced Sociology's central problem was 'the problem of meaning'. By this he meant not a technical challenge in the framing of investigative protocols but how to describe the intersubjective coordination of definitions of the situation fundamental to sociality. His thesis title, *The Perception of the Other,* homes in on this problem by addressing Parsons' phenomenology. The idea Parsons had a phenomenology might seem a bit surprising but, in fact, he had two. Following the premises of his theory, Parsons treats the experience of the social actor from the point of view of the total social system and its homeostasis. The social actor is Parson's *homunculi* fashioned within his frame of reference. How actors experience the social is the product of patterns of management processes responding to systemically experienced tensions. These responses ensure adjustments in normative structures while related value orientations act as feedback mechanisms sustaining continued motivated compliance and hence the orderly experience of social life. Components of the cultural system shift and re-balance, thereby allowing order to be preserved. Putting it briefly, the phenomenology of the social actor's experience of social orderliness is a functional consequence of the phenomenology of the system's experience of its own inconsistency. These inconsistencies generate threats to its coherence and hence orderliness. As with any dynamic process, there is always a degree of 'play' in the mechanisms maintaining the process. Such 'play' appears as differences in commitments, interests, relevances and the like and are expressed as differential orders of motivated compliance.

Garfinkel's crucial insight concerned how time features in these two phenomenologies. At the level of the social actor, time is marked as the flow of routine courses of action. The "tensions" to which variation in motivation gives rise, are encountered, managed, and resolved on the

[5] Note 'methodology here does not mean investigative technique but the whole apparatus required to construct and then investigate specified small worlds. See Garfinkel's discussion of Felix Kaufmann [Garfinkel 2006; Garfinkel 1967] and [Anderson and Sharrock 2019].

temporal scale of the flow of intersubjective action. At the level of the system, adjustments take place in what Garfinkel calls a "fat moment" of cultural evolution and social change.[6] The gap in Parsons' account is the lack of specified, theorised structures for managing the integration of the temporal orders.

To bring out the import of this gap, Garfinkel constructed a slightly different localised small world. It provides a frame of reference for 'objects' such as actors, roles, identities and so forth defined in terms of Parsons' theoretical schema. This small world has just two dimensions; time as an experienced succession of courses of action and normative orientation as degree of motivated compliance. From the beginning, Parsons had insisted the characteristic of social action was that it was meaningful. This required a solution to the interpretive double contingency of definitions of the situation held by actors within the small world frame of reference of social action. To bridge the gap in Parsons' theory, Garfinkel introduces themes drawn from Husserl's phenomenology as interpreted by Alfred Schutz and Aron Gurwitsch. An understanding or interpretation of action is possible because of the intersubjective sharing of finite provinces of meaning. Shared interpretation is provided by agreement on the 'relevance' of definitions, values and norms associated with a finite province of meaning. Parsons' conception of motivated compliance is treated as an expression of this agreement. Garfinkel operationalises this conception by defining compliance as ranging to the limits of complete (programmed) compliance (the default position in Parsons' theory with the actors as "cultural dopes") and complete absence of compliance (where at least one actor is 'fully anomic'). His question is: 'What happens to the theory's empirical fit if you push the frame of reference to the limits by using a combination of full compliance and full anomie?'

To investigate this question, Garfinkel formalised his frame of reference and then derived and proved a theorem. The theorem proposed that in the absence of shared motivated compliance to the normativity of a finite province of meaning, fully compliant actors will be unable to find the other's actions meaningful. The double contingency and hence Sociology's problem of meaning, cannot be resolved. Instead, the compliant actor will see the actions of the other as "specifically

[6] 'Moment' here is a technical term. It refers to the smallest possible unit of time in the frame of reference's operations. In the same way, the "social actor" is the primary unit of action in the system in much the same way as the particle is the primary unity of mass in Classical Physics. The frame of reference constructs a small world of defined primitive objects.

senseless". Using a later terminology, under such circumstances the compliant actor will fail to sense assemble the joint activity.

The issue for Garfinkel was not whether he could derive and prove his theorem from the principles on which he had built his small world but whether he could show how the conditions set out by the theorem can be reproduced in actual courses of social action by using a setup which approximates as closely possible to his envisaged scenario. If his theorem holds up under circumstances even loosely akin to naturally occurring behaviour, Garfinkel will have demonstrated the import of the lacuna in Parsons' scheme. The theory is incomplete.

The demonstration Garfinkel ran was the first of what later became known as his "breaching experiments". The setup involved playing an interview of a boorish applicant to medical school to an experimental subject. Having formed an opinion of the student, the subject was given further information which subverted the evidence provided in the interview on which the opinion was based. The aim was to promote "maximum incongruity" between the two accounts of what was happening. The rationale was to push the subject to re-organise their account of the interview beyond the point where further adaptation and re-organisation is reasonably possible. At that point, it was expected the subject would claim not to be able to make sense of what was going on in the interview. However, that didn't happen. In every case, the subjects used all sorts of modifying, ameliorating and explanatory stratagems to try to remedy the apparent "senselessness" of what they were being told. The experiment did not demonstrate the theorem.

For Garfinkel, this did not imply Parsons' theory was complete. Neither did it suggest we should jettison the problem of meaning and its relation to social order as the central analytic topic for Sociology. Rather, the whole methodology underpinning the conceptualisation and investigation of the social should be re-built. Relying on Whitehead's metaphysics, Parsons had maintained a distinction between concrete activity and theorised social action. The latter was the former construed under the auspices of sociological theory. To find order in the concrete, we had to theorise it. Its orderliness was revealed in our theoretical descriptions. Garfinkel took his 'demonstration' to show there was an endogenous orderliness in the concrete which was not captured by Parsons' theory. Note, and this is important, he did not conclude we should not 'theorise' sociologically about it. It was how a particular theory and its categories were shaped and used in investigations which was at issue not the use of theories and categories as such. Throughout the rest of his career, wrangling theoretical categories and types was central to his thinking about how to investigate the social, including the social character of the natural sciences.

FORMULATING THE METHODOLOGY

After Harvard, Garfinkel continued this interest in how the coordination of understandings is achieved. Using Schutz's conceptualisation of the rationalities of common sense and science, he explored the presuppositions of intersubjectivity under the heading of "trust" in relation to both the reality of how things appear as well as the repeatability of action. The concern with "rationalities" in the organisation of the resolution of meaning and the achievement of reciprocal understanding was explored in studies of decision making by jurors as well as by those charged to code organisational records.

A key shift occurred in the way he framed his studies when he began to use concepts from Phenomenological Psychology in the same way he had used Schutz and Parsons. Drawing first upon the analyses of Aron Gurwitsch and later of Maurice Merleau-Ponty, he began to explore the processes of configuring or registering the details of context as a means of coordinated definition of the situation. Using optical illusions such as the famous 'Duck/Rabbit' drawings, Phenomenological Psychology had emphasised the different configurations of what Gurwitsch called "fields of consciousness". Pointing to how the zonation of attention to the detail of the field was shaped by interests and relevances, Gurwitsch analysed how fields of consciousness were reconfigured during courses of action.

Merleau-Ponty brought a different emphasis. For him, it was 'sensation' which was the central concept for the analysis of experience and how particularly sensations were experienced by embodied beings immersed in a world of sensation. *The Phenomenology of Perception* [Merleau-Ponty 1962] plays a major role in Garfinkel's thinking at this point, as can be appreciated from the repeated references to Merleau-Ponty's organising conception, 'the phenomenal field', as well as the broadening of analytic scope to sensorily engaged embodied perception. Using an image which Garfinkel borrowed, Merleau-Ponty states his problem as the description of how we extract the animal from the foliage by means of sensorily engaged embodied perception when constituting a phenomenal field. [7]

An important text for Merleau-Ponty and one which must have been important for Garfinkel too (although he nowhere refers to it) is Husserl's *The Origin of Geometry* [Husserl 1970]. 'Origin' here is not simply intended historically. It refers to the 'foundation' of Geometry in spatial experience. Husserl calls his 'depth analysis' of the sensorial foundations of Geometry in

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[7] This image refers to the 'task' set in a widely known children's puzzle of the time.

our management of objects in space an "archaeology" of Geometry's "sense origin". This term was picked up and re-purposed by Merleau-Ponty's close friend, Foucault. For Husserl, Geometry and all the empirical sciences which rely on it, are premised on our immersion in spatiality. Merleau-Ponty suggests Husserl came to 'limits of Phenomenology' as he had conceived it with this analysis because central to his approach was an essentially intellectualised conception. Husserl turned to Geometry as an intrapersonal problem with the perceiving subject, the Ego, "riding in the car" of the body (as he put it in the fragment subtitled *The Originary Ark* [Husserl 1981]). This notion of the subjective being of perception separated or distinct from the objective being in the world creates a zone or boundary between them along which Geometry originated. For Merleau-Ponty. it was necessary to step over that boundary and embrace a position where, in that sense world, subject and object were an 'intertwined chiasm'. Garfinkel balked at this.

In the investigations included in *Studies,* Garfinkel adopted an approach of 'misreading' the categories and frameworks proposed by the philosophies just summarised and applied those 'misreadings' to various social settings. In doing so, he was able to point how the detailed management of the flow of action involved the use of procedures which, in a joint paper with Harvey Sacks [Garfinkel and Sacks 1970], he would identify as "the formal structures of practical action". These consist of context free formats for the sequencing of action available to be used in context dependent circumstances. What he called the 'recognisability' and 'accountability' of these formal structures provided for the constitution of our experience of social order. The class of formal structures are 'social objects' used to sense assemble such 'social facts' as the operation of turn taking in conversation, queues and traffic, the deployment of systems of extensible categories and related actions (normal crimes, routine troubles, etcetera clauses). This sense assembly was the achievement of the identification of actions and actors as well as the provision of locally relevant descriptions via generally applicable formats (storytelling and glosses).

At the time of the publication of *Studies*, the position adopted by EM might be summarised as follows.

1. Professional Sociology studies the constitution and dynamics of social order. It does so by applying theoretically deduced propositions ('theorems') concerning resolutions of 'the problem of meaning' to 'concrete' courses of action.
2. Investigations of the application of Sociology's central tenets pushed them to their limits and so revealed:

   a. The dominant theory is incomplete as an account of the production of social order through the resolution of the problem of meaning. There is 'a gap in the texts'.
   b. Examination of 'concrete' courses of action revealed the existence of a domain of endogenous analytic practise (ethno-methods) in daily life which had hitherto been overlooked. These provide for the accountability of actions within the course of their flow and hence solve the problem of meaning. It is these methods which provide the solution to the problem of social order.
3. Professional Sociology relies on members of society and their cultural objects (either as informants or as exhibits) to provide its entreé to concrete social action. As a consequence, it relies on the use by social actors of ethno-methods for the successful deployment of its investigative protocols. Professional sociological descriptions are built on and from contextually generated and locally ordered sociological methods. This result is important for professional Sociology's ambitions to be rigorous and probative.
4. The implication of the findings described in 3 is that the practise of professional Sociology must be treated from within the same analytic frame of reference as the rest of social life. Its practices and settings are as available for EM analyses as any other.
5. EM motivates its own studies by 'misreading' professional Sociology's descriptions of social order and treats them as providing protocols for the investigation the achievement of social order in some designated setting (including within Sociology itself) and then pointing to 'a gap in the texts' which can only be closed by the endogenous ethno-methods deployed in the setting.

In the above positioning, EM is defined as an 'alternate sociology'. This is key. There is an interdependency or possible interdependency between EM and professional Sociology. EM draws its topics from professional Sociology. Its disclosure of the grounding of Sociology's methods of investigation and analysis was made possible because EM investigators were professional sociologists *and* culturally competent members. They could "see" how the orderliness of social life was made accountable both in the theory and in the concrete. Analyses juxtaposed these points of view. This interdependency was extended to other disciplines when EM turned to settings where similar 'sociological' accounts of phenomena are used as part of analytically dominant modes of theorising (Management Science and executive decision making, Medicine and diagnostic routines, Jurisprudence and legal trials etc.). In what became Garfinkel's master descriptive trope, these studies were revealing the "shopfloor work" of the setting; the actual production of the phenomenon of social order for which the theory provided an abstract design.

Section 2. Hybrid Ethnomethodology

For reasons which are not altogether clear, from the late 1970s onwards Garfinkel began to take more and more interest in the natural sciences.[8] This interest engendered a number of projects. A study of introductory lectures to Chemistry was mounted in collaboration with David Sudnow. This study is included in *Ethnomethodology's Program*. There was a study of the discovery of the optical pulsar with Mike Lynch and Eric Livingston [Garfinkel et al. 1981] which was separately published. Alongside these, Garfinkel undertook some informal prospecting by 'hanging around' science labs as well as talking to scientists and historians of science. In addition, for a while he took a lively interest in Kuhn's analysis of theory and theory change in the sciences.[9]

THE PROPOSAL

The 1988 summary released by Lynch [Garfinkel 2022] identifies three broad theses arising from the exercises just mentioned.

1. The natural sciences are 'discovering' disciplines. This is because they share two complementary characteristics:

    a. They can 'lose their phenomenon'. In other words, changes or differences in the constitutions of the phenomenal field can cause investigators to fail to be able to extract the animal from the foliage. As scientists sometimes have it: nature is not always cooperative.

    b. They are "probative". The possibility of losing the phenomenon implies the possibility of not being able to recover the phenomenon and extract it from its foliage. However, it is a (social) fact scientists routinely do agree

[8] We could hazard any number of guesses. See [Greiffenhagen and Sharrock 2019] for some suggestions. Hopefully, as more of the unpublished materials are examined, the considerations motivating Garfinkel will become apparent. Garfinkel rationalises it simply as the pursuit of the bases of Galilean Science, which it is. But this could have been undertaken in numerous ways.

[9] We offer some comments on his interest here in Essay 12..

on the identicality-for-all-practical-purposes (or not) of results and hence the demonstration of the theorems under test. In that sense, questions can be resolved and things settled (at least for now).

2. Because of the relationship between 1a. and 1b. above, contingencies matter. This makes identification of the range and import of contextually relevant contingencies as they are available and displayed in the praxis of the science EM's paramount investigative task in a study of a natural science. It was this aspect where Garfinkel felt his exercises had been inadequate. Hanging around the labs and asking "coathanger" questions of scientists made no essential difference to his and his colleagues' ability to learn, see and follow the science-work in hand in the ways the scientists did.

> The aim of our research was via discussions with bench scientists to ask for and get from them an explicit explanation of the coat hangers. Remember, we didn't know and we wouldn't know to see for ourselves what we were asking the scientists do describe for us, in detail. Given the foregoing particulars, and in their light, we pose the question: In any actual case of discovering work in a natural science, just what can be settled by "saying so?" [Garfinkel 2022, p. 39]

In Garfinkel's view, two modes of EM study of the natural sciences we mentioned at the beginning of this discussion are possible. The first is exemplified by Lynch's work on Micro-biology; the second by Livingston's work on Mathematics. What marks them apart is the 'seriousness*' with which their findings can be taken by the scientists they study. This difference arises because Lynch is not competent in the Micro-biology he describes, whereas Livingston is competent in Mathematics. Crudely, Lynch gives an outside-in description and Livingston gives what Garfinkel calls an "inside-with" one.[10] What escapes Lynch is the interior configuration of the phenomenal field of the practical science of Micro-biology as that is oriented to and worked with by the scientists he studies. This phenomenal field is the shopfloor organisation of scientific contingencies.

Taking an investigation of the practise of Physics as an example, this summary and the detailed discussion on which it is based seems to imply the following positioning analogous to that for early EM provided just now.

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[10] We return to this term in depth in later.

1. Professional Physics studies the constitution and dynamics of material order. It does so by utilising theoretically deduced propositions ('theorems') about the interaction of forces and particles as descriptions of 'concrete' examples of the actions of objects.
2. If EM investigations of the application of Physics' central tenets mount investigations which test them to their limits:
    a. The dominant theories will be shown to be incomplete as an account of the production of material order. There will be 'a gap in the texts'.
    b. Examination of 'concrete' cases of studies of objects and their actions will reveal the existence of a domain of analytic practise (endogenous physics-methods) which has hitherto been overlooked. These endogenous physics-methods provide how Physics provides for the continuous coordination of matter in motion. These methods thus provide a solution to the problem of material order.
3. Since professional Physics uses the exhibited properties of material objects as its entreé to the study of concrete material, it relies on the yet to be identified endogenous physics-methods for the successful deployment of its investigative protocols. Professional Physics accounts are built on and from endogenous physics-methods. This result should be of signal importance to professional Physics' achievement of rigour and probativeness.
4. The implication of producing the findings described is a requirement to place the practise of professional Physics within the same analytic frame of reference as the rest of the material order.
5. EM could motivate studies endogenous physics-methods by 'misreading' professional Physics' descriptions of material order as providing protocols for the investigation of the production of material order in some designated context. It could then point to an inevitable 'gap in the texts' which can only be closed by revealing the endogenous-physics-methods being deployed there.

Any adequate EM study formed under this positioning should be characterised by a requirement and two conditions. The investigators must be competent in the Physics which they study. This is the requirement. The conditions are:

1. The findings will be rendered as "inside-with" EM/Physics descriptions.

2. These descriptions will be construed as instructed action so they can be incorporated in the praxis of the relevant Physics.

EM's strategy of pushing professional Sociology to its limits was achieved by asking *how* the problem of meaning was resolved in the theory, since resolution of the problem of meaning as a solution to the double contingency was the condition of social order in Parsons' theory. This solution allowed for the coordination of social action. EM studies pointed out such coordination was achieved *within* the flow of action. Pushing professional Physics to its limits can be achieved by using the same strategy. The challenge is to find Physics' version of the problem of meaning.[11]

Section 3. The Scarcity of Existence Proofs

The core objectives of hybrid*-EM are practitioner competence, inside-with descriptions and transferred findings as instructed action. In our discussion of the inclined plane experiment, we pointed out Garfinkel was unsuccessful on all three fronts. In our view no other ethnomethodological investigation of the sciences or any other domain has been successful either. To understand why, we have to understand what would be required to deliver the set of objectives as an integrated and coherent course of practical investigative action. Because these objectives are an integrated triplet, such analysis will involve a great deal of back and forth between them. But, as the whole rationale of hybrid*-EM seems to turn on the transfer of findings, it might be as well to start with that.[12]

Instructed Action

'Instructed Action' is a term of art in EM and is used in three closely related ways.[13]

1. As a general analytic category for moves in a joint course of action, be it co-present interaction or action at a distance. An actor designs an action (asking a question,

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[11] It is important to recognise Garfinkel does not mean simply describing what Physicists do on a day-to-day basis in their labs to ensure their experiments work and that such working conforms to standard disciplinary expectations. These practical embodied and other skills are relevant *in* the practise of Physics but not *as* the Physics itself. Identifying their use in Classical Mechanics is talking about Physics not talking Physics and hence takes the form of analytic ethnography. See [Liberman 2007, p.4] for this distinction.

[12] We will talk indiscriminately of findings and results. We recognise they are not necessarily the same. However, since EM does not place any weight on their difference, neither will we.

[13] See [Lynch and Lindwall 2024] for an array of studies of various ways the term might be employed analytically.

writing a memorandum, presenting a purchase at a till) such that the action produced either initiates an action sequence or responds appropriately to the Other's prior action and so frames what the Other's next action should be. Conceiving of the flow of activity as sequences of appropriately structured instructed action provides a mechanism for the coordination of definitions of the situation and configuration of phenomenal fields.

2. As a ploy for designing investigations. An investigator chooses to 'misread' a sociological, philosophical or other theoretical account of some phenomenon as instructed action for its investigation. This 'misreading' generates the necessary "gap in the texts" on which EM analyses rely.
3. As a design feature for the findings of hybrid* EM. To be transferred into the praxis of a discipline, findings have to be (co-)constructed to be followable by recipients as instructed action for how to deploy the recommendations contained in the findings.

Notice the different role reflexivity plays in these uses. In 1. and 2., the means of sequencing actions is reflexive on the in-situ shared local history and context. Thus, it is a purely *analytic* conception and hence grounded in the principles of the disciplinary outlook of EM. In the third usage, it is a deployment issue and shaped by the requirements of achieving practical success. Its grounding, therefore, reaches beyond EM to the recipient professional practise. This is where the audience question (which, as we will see, is raised by Meyer's [Meyer 1991] concept of inside-with description) appears. If recommendations based on the contents of inside-with descriptions are to be transferred as instructed action, a key question in framing of any hybrid*-EM project cannot just be how much competence in the setting the investigator can or should acquire but also how much competence in the sociology those in the setting are expected to have acquired. As a corollary, if the double fitting of competences is required, how is the calibration to be carried out to prevent misfires, mismatches and misunderstandings? In addition, if EM is not so much a 'discovering discipline' as a 'transferring discipline', what project demands must be satisfied for it to carry out its function successfully? Although EM has touted this possibility, it has spent no time or effort thinking through what the requirement imposes on the project team.

Garfinkel addresses what hybrid* EM might entail in the following way.

> Ethnomethodology is not critical of formal analytic investigations. But neither is it the case that EM.......has no concern with a remedial expertise and has nothing to promise or deliver. Ethnomethodology is

> applied Ethnomethodology. However, its remedial transactions are distinctive to EM expertise.
>
> That expertise is offered for phenomena whose local, endogenous production is troubled in ordered phenomenal detail of structures. EM does not offer a remedial expertise that is transcendental to these phenomena. [Garfinkel 2002, p. 114]

This statement is important for more than its relaxing of the stipulation EM should be "indifferent" to FA claims, theories, accounts and so on. It seems to be intimating EM can or might offer correctives to the particular forms of FA reasoning relevant to the specific circumstances in which they have been studied. A little later in the same discussion, during a laudatory account of Robillard and Pack's work on paediatrics, we find the following.

> Their program was notable for working out and demonstrating the condition of EM adequacy that the analyst's Ethnomethodological findings be taken seriously in the FA discipline that was studied. By being taken seriously I mean that the work site practitioners *will demand of EM findings just as they demand of FA findings* that they satisfy the work-site-specific, discipline-specific corpus status of FA investigations and that EM findings be incorporated in FA work at hand or reasons be given for not doing so. [ibid p. 127. Italics in original]

It is here Garfinkel lays out what we termed the "condition" for hybrid studies. The adequacy of an applied and remedial EM stands on its ability to ensure the deployment of its results in its host/partner domain where, remember, that domain may be any scientific, professional or other routine working practice and not just professional Sociology. All such disciplines have previously been categorised as Formal Analysis (FA). Accepting this is a condensed sketch account, nonetheless two crucial aspects seem to have been lightly skipped over. The first is the conceptual coherence of holding to ethnomethodological indifference whilst pursuing hybrid studies as just defined, since the former is simply the operationalisation of the radical asymmetry claimed for the incommensurability between EM and FA. The second is a more operational matter; the defining of criteria for designating particular sets of activities as "troubled" and who makes that judgement. There is a further aspect. The operational challenge of satisfying the requirements of seriousness and the engagement with FA disciplines in order to transfer results.[14]

[14] More recent work in domains such as HCI (see, for example, Crabtree [Crabtree 2004] and Hartswood et al [Hartswood et al. 2002]) for early statements (but note Pollner's [Pollner 2012a] disquiet) indicate such partnerships require a great deal more than "throwing a few recommendations over the fence".

Let's take the question of logical coherence first. If EM involves a conceptual disjuncture with any and all FA disciplines of the order often stipulated by Garfinkel, Weider and others, how can it be logically possible to incorporate the results of EM investigations of FA practices into the accounts which the FA being studied gives of its own procedures *whilst at the same time* insisting on maintaining conceptual integrity and coherence of both EM and the FA discipline? To propose this requirement without clear demonstration of a logical fit with claims about EM's relationship to FA, risks not just the appearance but the reality of conceptual incoherence and inconsistency. Search as we might in *Program* and elsewhere have not found an argument showing why this risk does not arise.

Does this absence matter? Well, it depends on just how seriously we are to take "seriousness". Presuming EM's promulgations about itself speak to how we should view the essential character of authentic EM work, i.e., those features which the discipline should aspire to and try to exhibit, then surely there is an obligation to provide analytic re-assurance concerning the necessary logical continuity between the condition being laid down for adequacy of investigations and the way the discipline is defined? This cannot be done simply by citing lists of studies as exemplars of authentic EM. Any itemising cannot demonstrate logical consistency and coherence between the two propositional stances. Neither would it identify which adjustments might have to be made in the protocols of EM and FA's own descriptive methods for consistency and coherence to be attained and maintained. At the very least, if studies do show the required consistency and coherence, there must be some detailing of how the triangulation between the format, content and potential deployment of EM results was achieved in the conceptual and practical contexts within which the target FA is operating.

What makes the notion of "troubles" problematic is both how they are identified and what kind of "trouble" they are. The question of identification turns upon the skills of description and assessment we require of EM researchers engaged in hybrid studies as well as how we expect they will normally have acquired them. If the domain is an academic or professional discipline (Astrophysics or Neurosurgery, for example), do we expect at least entry level Graduate competence in both the explicit and tacit knowledge of the target discipline melded with Graduate level understanding of EM and its methods in order for the investigator to "see" the identifying configuration of details of disciplinary reasoning? And if so, how do we imagine that melding might be achieved? Putting it at its simplest (and its grandest!) just to make the point, take the contrast between EM's favourite category of "normal natural troubles" such as those which arise any accountable social production process (the sorts of things Livingston points to as the necessary correlated troubles involved in proving or untold numbers of studies of science labs have revealed about experimental practice). Now compare these to mathematicians' compilations of key problems in Mathematics such as Hilbert's famous programme or the lists of fundamental problems like as the measurement problem in post-Quantum Mechanics. Whilst any competent EM investigation should be able to churn out observations on the 'bench science' and its normal, natural troubles, it is hard to imagine how EM might make a direct contribution to solving the kinds of "troubles" scientists think they are confronted by. The one area where there might be interplay is pedagogy. Even here, though, to make a contribution would require reformulating EM's descriptions of the informal logic of practical scientific and mathematical reasoning in ways which would allow translation into the formal rules for constituting well-posed problems or the disciplinary norms for generating robust findings. Although he has returned to this issue a number of times, Livingston, for one very leading example [Livingston 1999, 2015], acknowledges he has not found a way to fashion the necessary scaffolding to allow the desired bridging to take place. This does not mean it can't be done, simply that it hasn't be done. The fact as able as exemplary an interlocutor as Livingston has not managed to make much progress should give us considerable pause for thought.[15]

Whichever way the issue of conceptual coherence is addressed, a second challenge remains. This is we will call the requirement to "organise for delivery". What is involved here is way downstream from abstract determinations of what incommensurability might be and its application to EM and FA or strategies for misreading theories and philosophies. Ensuring particular project outcomes are implemented involves matters rarely if ever discussed in Sociology. By "delivery" we mean the transferring a suitably packaged set of research results to be incorporated into their own work by those who take them. Engineering disciplines have a whole raft of procedures, advisories and rules of thumb for managing successful research and development projects to ensure delivery

[15] We have not been able to access the Robillard and Pack materials, so we cannot comment on those. However, some researchers cited by Garfinkel such as Stacey Lee Burns [Burns 1996, 1997, 2023] do not comment on the problem of defining "troubles" nor on the uptake of their results in regard to them. Things are not quite so straightforward with others such as Suchman [Suchman, et al 1999, Suchman 2000] where it certainly is true some of her findings were acted upon. However, the extent to which those findings derived from the utilisation of EM methods rather than fieldwork ethnography or Scandinavian Socio-Technical Systems techniques of user centred system design is not clear. To help calibrate these cases, it would be useful to have Lynch's reflections on his own experience of researching in a number of 'applied' fields.

to time, to budget and to specification.[16] By laying down the stipulation he does, Garfinkel commits us to the successful transfer of deliverables to time, budget and specification.

What sorts of things are we talking about when we speak of procedures, advisories and rules of thumb? They are the praxeological disciplines of project management. Project management is not research proposal forward thinking; the designing of research activities in the hope of obtaining sufficient results to justify a project in ways which approximate to how the initial proposal justified itself. It is results implementation backwards thinking, very much cast in the pluperfect tense. What will we have to have accomplished in order to be able to transfer the results we are certain we will have secured by then? Projects are scientific, organisational and social structures. All three elements require management to ensure successful hand off of deliverables. Hand off can take many forms: adopted implementation plans for policy recommendations; enacted business plans for re-organised activities; deployed prototype technologies, and many, many more. One aspect of all this is the infamous Error 33 problem (See [Kay 2004]). At Xerox PARC, Error 33 was the term used by the designers of the Alto to describe a strategy of saving time, money and other resources in the development of an innovative system by incorporating onto a project's critical path as-yet-unproven or perhaps even as-yet-undesigned (so called "slideware") technologies from other research groups. Obviously, if you can't control a technology's development, you can't be certain of its availability nor its capabilities. In effect, what Error 33 does is predicate a project's success on the expectation/hope of someone else's research progress. By making the transfer of EM findings into the *practise* of FA a condition of adequacy for research findings, Garfinkel seems to be awfully close to requiring EM to risk Error 33 in the delivery phase of its projects. If EM wishes to satisfy Garfinkel's condition, it seems the arts and sciences of serious project management are going to have to be part of its stock in trade. That they are not may well be an in-principle reason why there are no existential proofs of hybridity.[17]

Although Garfinkel does insist on the necessity for his objective, at the same time he offers an escape clause in the final words of the passage we cited: "....or reasons be given for not doing so". As ever, though, we are given no indication of what are and what are not acceptable exculpatory reasons disregarding the objective. Adopting the least line of resistance through a strategy of studied disattention to all these issues might make life easier but it comes with its own

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[16] None of which involve simply publishing papers, the method favoured by Sociology and most of the social sciences for transferring results as deliverables.

[17] Thanks to Graham Button for pointing this out.

risks. If problems are not raised and arguments are not resolved, rigour may well be traded off against practicality. The result could only be intellectual drift and disciplinary amorphousness. Eventually, EM will have become whatever anyone wants it to be.

INSIDE-WITH DESCRIPTION

Near the beginning of the description of the inclined plane experiment, Garfinkel tells us:

> To display the demonstration's [Galileo's] properties of social order, to make them examinable, to make them instructably observable bench work, and to make the bench work adequately articulate, requires the work-site disciplinary expertise of both physics and sociology.
>
> The tasks of adequate and evident display by collaborators are endogenously interior to the display. Therein their tasks being embodiedly oriented within the display and as of the display, "inside-with" the display, are to describe Galileo's achievements of physics, and for physics and western science, in coherent details of sociological phenomena of social order. These phenomena are Durkheim's neglected phenomena of order. [Garfinkel 2002, pp., 290-1 italics in original]

Attached to the term "inside-with" is the following footnote.

> "Inside-with" is a phrase that Lois Meyer coined in her Ph.D. dissertation. The use of "inside-with" by EM authors should be used to criticise Merleau-Ponty's "intertwining" and "chiasm" as well as recent variants on these metaphors. [ibid, p.271, fn.12].

What is odd about the footnote is the injunction Garfinkel propounds in regard to the phrase "inside-with". Whatever its meaning, it should be used to criticise Merleau-Ponty's "ontology of Flesh" as developed in *The Visible and the Invisible* [Merleau-Ponty 1968]. This appears to be a flat contradiction to EM's standard line regarding the rationality, reasonability, logic, coherence, consistency and all other epistemic virtues of theoretical and philosophical discussions. As we have already said, this standard line is the stance of ethnomethodological indifference. EM's interests, objects and findings do not bear upon these matters and so cannot throw any light, illuminative or critical, on them. Why then, does he want EM to use Meyer's term as critique of Merleau-Ponty in precisely this way?

We can get some insight if we look at the context in which the concept of "inside-with" describing was developed.[18] In the Introduction to her thesis, Lois Meyer [Meyer 1991] set herself three tasks:

1. To provide an analysis of the teaching and learning of a school "as it occurred *in situ,* the local effort of a specific group of people at a specific school site" (p.9).
2. The descriptions provided would have "to look and feel familiar to those who do that work locally, inside the school". This recognisability was a criterion of adequacy.
3. To provide an account of the practices of "The Language Circle" as a method of teaching and learning language in which those practices could be taken as objects for instruction by "outsiders" who did not participate in that particular local scene.

One of Meyer's audiences is defined as those who are interested in but unfamiliar with the context she is reporting on. This means she cannot trade on insider assumptions to satisfy her third objective but must render her account in more generally recognisable terms. To be successful, the first two objectives must satisfy precisely the inverse condition; contextually shaped and located descriptions. To resolve the tension, Meyer decided on the adoption of an investigative stance where the praxis of investigation would be "inside-with". Her explanation of what she took it to entail is worth quoting at length.

> In stark contrast to a subjectivized, psychologized internal probe, the term "inside" in this study refers to Heidegger's existential and phenomenal sense of "inside" or Being-in-the-world, where "the world is always the one that I share with Others" [Heidegger 1962:155], ..."those among whom one is too" [154]. Dreyfus [1990] points out that Heidegger's Being-in-the-world implies involvement and caring; it has a meaning much more like "being in love" than "being in Houston." The Language Circle is composed of a company of teachers and children who are care-fully involved with each other in its teaching-learning work; each participant is "inside" the Circle's work, together with the rest of the company. In this way, participants' knowledge is "inside" knowledge precisely because it is witnessable by, available to, and dependent upon, others who are also at work inside the Circle. Bringing newcomers inside the Language Circle means teaching them "our work," not monitoring their internal thoughts. The social, phenomenal meaning of "inside," that is, the "inside" which Language Circle participants experience and know as their work world shared and

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[18] We are grateful to Lois Meyer for providing sections of her thesis and details of how she approached the study it contains.

> lived together with others and in each others' company, is crucial to this study. To underscore this meaning, the use of the term "inside" in the title of this study and in the text itself should be understood to mean " inside with" that is, "inside with other participants in the Language Circle." [Meyer 1991, p. 11]

Inside-with descriptions, then, speak from the experience of the community who share that experience and in terms which convey that experience as the lived world of daily, ordinary life as a community member of The Language Circle. But it is not just a description *of* the community and *for* the community. It is also *about* the community for those who are not members. (In addition to those just mentioned, she also had her PhD Committee to satisfy.) As a result, because of the plethora of audiences for whom the account is to be provided (that is, the one account given 'here and now' in the context of the thesis being read), the text has to be recipient-designed so it can be sense assembled by *all* these audiences as a coherent, plausible, rationally acceptable account in terms of whatever relevances they have. As she puts it, the text has to be report, novel and parable depending on who is reading it. What allows for this is the possibility 'the same detail' might be given different configurations depending on the frame of reference adopted for any reading. To put it another way: the aim is for the lines on the paper to assemble themselves into the duck or the rabbit in and through the reading. But achieving that is the exercise of artistry in itself.

Note the stance brings with it an investigative stance, one in which it is possible for the investigator to be a member of the community whilst adopting the analytic attitude of investigation and yet not shaping description in terms of a disciplinary gaze. Foucault's concept of the (professional) gaze has had quite an airing in discussions of ethnographic reporting in the social sciences. Using it, commentators have argued the investigator fillets social activities of all but sociologically relevant meaning. What activities mean for the participants, what they amount to and demand of them are downplayed under the theoretically constructed objective purview. The inside-with description wants to bring meaning back in without sacrificing technical clarity, precision and interests. This means reporting investigations by talking sociologically and locally. However, what it takes to do that is as yet unsolved. It is interesting very much the same tensions are evident in Dorothy Smith's later work.[19] It is no part of our interest here to determine if Lois Meyer resolved these tensions nor how effective her descriptions as descriptions for her various audiences were. All we can say is that so far EM has failed to do so.

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[19] See Part III.

Perhaps EM's failure derives from a 'misreading' of Merleau-Ponty's descriptions of embodied perception? Could that be the reason Garfinkel was unable to deliver the detailed analysis of the transition from pre- to post-Galilean phenomenal fields he was seeking? The discussion in *The Visible and The Invisible* suggests the progress which Merleau-Ponty had made using Husserl's analyses to extend beyond the *Psychology of Perception* was insufficient to ground the apodictic character of embodied perception. He had, as his title says, reached the limits of Phenomenology. A new ontology was required to supersede that on which earlier studies were based. Is the stance of 'inside-with' description consistent with either Merleau-Ponty's new ontology or, as in the case of the Inclined Plane Experiment, the conjunction of the configurational orders of practical detail in EM and Physics? In other words, does the difficulty arise because EM is committed to 'inside-with' descriptions or because 'inside-with' descriptions in the contexts studied so far are impossible? Could it also be, and this is perhaps the most likely, that inside-with descriptions would not fit *any* Merleau-Ponty ontology EM might wish to employ?

We are now at the heart of the inside-with problem which hybrid* EM sets itself. Garfinkel's proposal/claim that EM has substantiated Durkheim's aphorism to the effect that sociology's fundamental phenomena are social facts has been taken in two ways. The first takes statements like "The unemployment rate is 8%" and co-classes them with other statements like "light is a stream of photons and a wave of radiant energy". The co-classing is brought off by proposing both are "produced" by methods of investigation involving the imposition of disciplinary and or professional categories on phenomena. Call this the "professional gaze" stance. What is involved here is a substitution of 'sociological' for 'social' in 'social fact'. Substitution is often complemented by an "as-told-to" stance of sociological analyses. Under that feature, sociologists can only learn about different ways of life from those who are engaged in them. They are social facts 'as told to' the investigator (in whatever way the telling takes place). In both stances, once the sociologist encounters a 'social fact' in a setting, it is re-construed as a "sociological fact". This is precisely what happens in the majority EM "studies" as well. The detail of experiments, diagnoses, taught classes, traffic flow, police work and conversations etc. are re-construed as the shopfloor work of the setting.

An alternative way such statements might be taken in an EM investigation would be to reveal the constitution of the "natural attitude" which characterises the setting as that setting. The objectivity which is substantiated is the 'objectivity' which is presumed, taken for granted, treated as the case, found to be the case or whichever other way you want to describe "bracketed under the natural attitude". What this kind of EM then has to struggle with settling on a construal of that attitude in order to suspend and analyse it. But to suspend that attitude, you have to hold it. Hence the competency requirement.

Under the first interpretation, neither Sociology nor EM can claim 'inside-with' descriptions. The best one can achieve is analytic ethnography. Under the second, you have to find a way to do the 'inside-with' describing by conjoining the sociological and the local account. We can now see why Garfinkel has to resist Merleau-Ponty's fusion of accountability using the duality of interleaving and chiasm. That would dissolve the disciplinary identity of the account for the audiences likely to receive it. He has to maintain a conjoined approach. But neither he nor anyone else has managed it. The question is whether this means strict adherence to hybridity's tenets makes EM impossible?

COMPETENCY

Competency in professional practice is not like perfect pitch. It is not a question of either having it or not having it. There are degrees of competency and degrees of understanding. Discussion of competency in hybrid*-EM never makes clear exactly what level of competency is being looking for nor what degree of understanding is required. However, given the centrality of the requirement to configure the phenomenal field as a practitioner does, we have to assume it is some combination of being able to 'see' as a practitioner does and to 'do' what a practitioner can do. The question is: has anyone in EM acquired that level of competence? And if not, why not?

Work by Robillard and Pack is heavily referenced by Garfinkel but unfortunately details of their projects and findings are unavailable. So we will set them to one side. David Sudnow's often referenced *Ways of the Hand* [Sudnow 1978] is a remarkable phenomenological analysis of his experience of struggling to learn how to play jazz piano. It is about his acquisition of the competence to hear, see and do things which others did "thoughtlessly". It is not a manual on the methods of its performance. The commercial "method" which he developed is hardly a piece of instructed action in jazz piano but simply a set of useful recommendations (for instance, start by learning just a few tunes) and some tutorial exercises for how to become more accomplished. According to at least one person who used it [Valeo 1987], it worked remarkably well. Stacey Burns [1997;1996] did practice Law but her studies of instances of law practice in action are precisely the kind of analytic ethnography Garfinkel wished to move beyond.

This leaves Eric Livingston's studies of Mathematics and Physics.[20] It is interesting that while Livingston's studies were Garfinkel's touchstone for hybridity, Livingston himself never talks about them that way. He does express the hope that one day they might be. (See, for example, the Appendix to his [1986] and the Epilogue to his [2008]). Discussions in *The Disciplinarity of Mathematical Practice* [Livingston 2015] and elsewhere, repeatedly observe that he is a novice prover[21] and struggles with the gap between what he can do and what mathematicians seem to be able to do effortlessly. It is of significance, we suggest, that Livingston prefers to refer to his studies, both of modes of mathematics and other modes of reasoning, as "ethnographies".

Regarding the competency requirement, then, only Sudnow and Burns seem unequivocally to have attained the level required, but nowhere does either offer an inside-with account of its performance as instructed action.[22] The published cases of hybrid*-EM cited by Garfinkel and others either fail to satisfy the requirement or fail to attain the objectives. In other words, there are no existence proofs of hybrid*-EM. But, as we have said, this does not mean hybridity is impossible only that, perhaps for the reasons summarised earlier, it has not so far been done.

Section 4. Summary

Resolution of the problem of meaning as a solution to the double contingency provided the intersubjective coordination required for social order in Parsons' theory. This solution allowed for the coordination of courses of action. Garfinkel's strategy of pushing Parsons' sociology to its limits was achieved by asking *how* the problem of meaning was resolved in the theory. Further EM studies pointed out such coordination was achieved *within* the flow of action. The problem is resolved both within the theory and in the concrete. The result of this 'discovery' is that the investigator\observer should be placed within the analytic frame of reference because of the dependency of the theory on the resolution of the solution in the concrete. That is EM's fundamental premise.

[20] Dusan Bjelic [2023] has also studied Galileo's work and was the stimulus for Livingston's own studies of pendula. It is not clear, though, that Bjelic is aiming for anything other than analytic ethnography.

[21] Note in many of his studies he acknowledges the contribution his brother, Charles Livingston who is a professional mathematician, has made to his work.

[22] Larry Weider was, apparently, a highly proficient conjuror. However, we have no analytic accounts by him of the lived work of conjuring.

This premise was actionable in Garfinkel's early studies and those of other members of the EM research community because what they were examining were the 'gaps in the texts' between social scientific (predominantly sociological) accounts of social phenomena and the descriptions available to investigators in the accounts and actions of social actors themselves. As social scientists and members or possible members of the cultural communities being studied, investigators could meld the sociological and the social into the required "inside-with" descriptions. Only a few (for example, Dorothy Smith) tried to transfer their results into the practice of social science as recommendations for its improvement. As we saw in our discussion of Institutional Ethnography, that did not work out as she intended. Retrospectively, then, it would not be unfair to talk of the first phase of EM work as creating a quasi-hybrid*-Sociology/EM

When EM turned to the natural sciences, the dual competency feature and with it the capacity to deliver 'inside-with' descriptions was at risk and had to be managed by ensuring the acquisition of the required domain competency as part of or in advance of the investigation. Neither the inclined plane experiment nor any of the available studies achieved this or laid claim to achieving this. On the other hand, many did learn how to find their way around their settings sufficiently well to be able to provide plausible analytic-ethnographic descriptions. Such analytic-ethnography was not, however, the hybrid analysis stipulated to be the goal of hybrid*-EM.

Nothing we have said in this discussion proves hybrid*-EM is impossible. All we have claimed is that the challenges are formidable and require significant conceptual, methodological, and practical work to shape and hone EM's investigative *modus operandi* so it can achieve the triplet of objectives Garfinkel set for it. Hard questions about ethnomethodological indifference and incommensurability must be addressed. Tricky questions about the place of the investigator as actor/observer in the investigative frame of reference need be resolved. Perhaps most challenging of all is the likely requirement to create a raft of mechanisms and associated techniques for framing EM results and findings as instructed action so that they can be transferred into the domain of study. All of these will take time and effort. The very things the EM community has shown little inclination to spend on them.

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13

Beware the Primrose Path

.....like a puffed and reckless libertine,
Himself the primrose path of dalliance treads.
(Hamlet)

Introduction

In tracing the history of Institutional Ethnography, we saw how Dorothy Smith's original proposal of a sociology for women gradually lost both its radical edge and its distinctive *point d'appui* as it was assimilated into the broad mass of conventional Sociology. An element in this process was the form of disciplinary liaisons it engaged in. Sociology added yet another investigative domain (the sociology of the marginal and dispossessed) to its repertoire whilst Institutional Ethnography gained conventional investigative techniques, academic respectability, students and career structures. This brief discussion looks at why just the same consequences might result from similar processes of accommodation between Ethnomethodology more broadly and Sociology and/or the wider social sciences. In so doing, it looks behind or beyond the lessons drawn from the history of Institutional Ethnography and the story of hybridity.

Section 1. Background

No discipline worth its salt can remain static, endlessly re-running the arguments which formed its original rationale. Equally, to survive and prosper no discipline can waste energy and resources hopelessly chasing every vision (or fantasy) proposed for its future. Time, funds, manpower and mental energy (especially mental energy!) are always in short supply. Choices over where the discipline should go next and how to get there must be set against the strategic objectives which it has set itself. In addition, the strategic benefits likely to accrue from innovation must at least match, if not outweigh, the practical costs incurred in the effort of acquiring them. It is the character of such balances and the need for them which make many current suggestions for closer collaboration between Ethnomethodology (EM) and some of the institutional bodies of conventional Sociology (as well as the swathe of related disciplines and professional practices to which EM applies the epithet Formal Analysis (FA)) not quite the straightforward matter some have presented it as. Historically, EM has always insisted on an arm's length relationship with FA, one which it enshrined in the principle of "ethnomethodological indifference". Moreover, it has continuously and vigorously asserted the only viable relationship it can have with FA disciplines is to treat them as sites within which to reveal the social organisation of practical reasoning and the practical activities which underpin it. One would have thought this attitude would militate against the expectation closer, more arm-in-arm, cooperation might be feasible and profitable both for EM as well as its putative partners.

This does not mean we do not recognise and share some part of the aspiration. EM was begot by Sociology (reluctantly, maybe) and there is considerable shared heritage. In addition, to-day many of those who identify with the EM community earn their corn contributing to the general teaching of Sociology either in dedicated departments or as in-house specialists in other places. There ought at least to be cordial working relationships between practitioners of both modes. However, we are not convinced either the mutual understandings or the conditions required for larger scale collaborations to be successful are in place. In fact, we are quite sure what might be thought of as the general Heads of Terms for such inter-disciplinary endeavours have never been contemplated, let alone worked out. The issue is not how individual researchers or research teams might work together and even prosper under ad hoc or long-term collegial working arrangements. It is how the disciplines *qua* disciplines can be first aligned and second coupled without either feeling violated or exploited.

The central questions from the EM side (and no doubt possible collaborative partners will not be wholly ignorant of EM's official stances) revolve around how the precept of ethnomethodological indifference to FA disciplines, their interests, theories and problems is to be managed, rooted as it is in a fundamental judgement EM has made of its own incommensurability with FA *tout court*. Can conditions for mounting sets of practicable collaborations be arrived at which satisfy the requirements of both partners without turning either side into lowly service providers, colonial subjects, fashionable playthings or the whole transaction descending into one-way traffic?[1] None of these possibilities are outcomes to be risked by a serious discipline. Moreover, for those finding themselves caught up in them, they are hardly marks of success either.

Graham Button and colleagues [Button et al. 2022] as well as others have issued stern warnings about what they see as a creeping recidivist constructivism in many proto and embryonic collaborations, particularly but not only between Conversation Analysis and Linguistics and Social Psychology. The same drift is clearly visible in other, earlier prominent partnerships, for example between EM studies of work and HCI or the natural sciences. In all these cases, the programmes of collaboration appear to violate a central tenet of EM, namely the determination to prioritise and pay relentless attention to the local organisation of solutions to the problem of meaning in courses of action and the consequences which follow from that focus. The considerations to which we draw attention in what follows are related to this apparent violation. The warnings from others which we have just mentioned are motivated by worries about the intellectual ballast which keeps the ship of EM stable.[2] We are more concerned about the composition of the crew, the working relationships among them and the likelihood neither the relations nor the crew will last the voyage.

Section 2. The Principle of Indifference

The precept EM and FA are incommensurable is the single most important background issue whose implications will surely radiate through any set of arrangements put in place. Needs must we start with it, what it means and how it applies. Starting in this way gives the primary knot of issues to be untangled before we can move on to frame subsidiary questions. Even though the precept is widely cited as the centrepiece of EM position statements, its precise basis and its methodological consequences are often under appreciated. This is because when the notion is used as a way of defining EM, invariably it is not given the order of careful explication it demands. One pivotal

[1] In that respect, the last of these might be the most efficacious outcome for EM, assuming all the value in the arrangements accrued to it. However, such arrangements wouldn't last long.

[2] This is not an idle image. Just like the ship of Theseus, EM has undergone and is undergoing regular maintenance, remodelling, refurbishment and extension. Today, it is both the same and not the same as Garfinkel's original enterprise.

example of what we mean can be found in Larry Weider's [Weider 1993] discussion of interdisciplinary Conversation Analysis (CA). Although Weider accepts the notion of incommensurability has had a history of opaque use and conceptual abuse, he offers no worked through definition of what he takes it to mean or imply. Instead, he begins by insisting the determination of the paradigmatic status of CA and Experimental Social Psychology (the two disciplines with which he is concerned) is not a relevant matter for his argument. He then goes right on to assert:

> ....the exclusive claims of interest here are that these enterprises are incommensurable in the same way that work guided by incommensurable paradigms in the physical sciences is. [Weider 1993, p. 214],

The only clarification he offers is:

> CA procedures and those of experimental social psychology present analysts working within these enterprises with different sets of entities. [ibid]

How this *ontological* construal of incommensurability maps on to EM and CA's relationship to FA disciplines such Social Psychology is left unexplicated.[3]

We know (and indeed are sympathetic with) what Weider was trying to do here. For some time, Garfinkel had clearly become exasperated with mainstream Sociology's unwillingness to see the significance of EM's findings for its disciplinary practise and its inability to accept EM's desire just to be different. What seemed to irritate him most was not ludicrous attempts to deny any sociological legitimacy to EM but the refusal to accept the scale of difference entailed in EM's mode of doing sociological investigations. This exasperation is clearly on view in many informal notes, texts of talks and meetings and elsewhere. (See for example, Hill and Crittendon [1968] and Garfinkel [1990]). This seems to have spurred him to want to make the point as forcefully as he could. During the late 1970s and early 1980s, Garfinkel began to talk of EM and FA as "incommensurable" [Garfinkel 1988; Garfinkel and Weider 1992; Garfinkel 2002]. The problem is that in linking EM's incommensurability (and hence its principle of indifference) to the debate over scientific paradigms stimulated by Kuhnian analysis [Kuhn 1962; Kuhn, 1972], even in the lightweight way they did, Weider and Garfinkel jointly and severally missed the opportunity to mark the difference between what the term meant for them and what it meant for Kuhn. Missing this

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[3] As we shall see, the fact Kuhn toyed with this and many other construals of the concept did not help matters. But saying that does not mitigate Weider's omission.

opportunity left the space open for commentators, interpreters and some would-be defenders of EM to 'join the dots' in ways which had little or nothing to do with what originally motivated EM's position. Not only were the waters muddied, unnecessary flotsam and jetsam were thrown in as well.

To see why, we first need to go back to basics and recapitulate the grounds of the original position. The place to start is with the rarely noticed and hardly ever remarked upon fact that many of Garfinkel's studies of 'investigative work' and 'data collection' explore whether, and if so how, an investigator could actually follow the standard requirements of Sociology's official methods as part of their actual practise of sociological investigation. The point he is seeking to elucidate is simply that those official methods commonly serve to obscure a persistent disparity between 'data' and 'phenomena'. The data do not capture 'the things themselves' but in large part consist of abstracted renderings of the 'materials' assembled by 'data collection'. Garfinkel is not condemning investigations for failing to meet the standard requirements of method but asking about the practicality of those methods since manuals of them do not acknowledge nor attend to the requirement they be applied, not as outlined in the abstractions, but as part and parcel of managing the necessary detail of practical sociological actions. This means sociological investigations cannot exclude practical (that is, everyday) reasoning. Indeed, they depend entirely upon it. The 'data' they deploy is created through the pervasive use of everyday understandings but such understandings are not specifically recoverable from the worked over materials presented to Sociology's research audiences.

Sometimes Garfinkel presents this as a matter of Sociology proceeding primarily by "shoving words around" or of dealing in "signed objects" (words, diagrams, records etc.) in contrast to investigating instances of situated action. At its deepest, the issue emerges with respect to Sociology's use of natural language. Here natural language is used as a means, a "resource" as it was described at the time, for talking about instances of situated actions rather than as a constituent of those actions and, therefore, as a topic of sociological investigation and analysis in its own right. It is this characterisation of the distinctive modes of investigation which gives whatever substance there is to the idea of an incommensurability of FA and EM. Treating FA and EM as topics for sociological investigation and analysis entails understanding that *both* FA and EM are empirically located within the domain of practical action and thus examinable in the same way as all other activities found there.

This entailment imposes three constraints on EM. Accepting them prevents EM from accommodating FA's approaches, topics, issues and problems, and vice versa. The constraints can be summarised quite succinctly.

1. EM rejects the extensively accepted (and often unwittingly incorporated) presupposition social order can only be found through the contrivance of a general scheme designed to satisfy the overarching requirements of institutionalised Sociology's aims and ambitions. EM declines this presupposition because it has set itself the task of identifying an order which is intrinsic to its phenomena, an order that is 'built in' to the performance or production of the intelligible actions which make up the evident order of practical doings.
2. Consequently, EM denies forms of Sociology *must* comprise two principal elements: theory and method. In contrast, it proposes this distinction embodies the adoption of a transcendental standpoint. EM sees no *necessity* for this distinction because, whatever its merits from the point of view of FA's aims and achievements, it functions to distract from the very things EM would call attention to. The distinction (unavoidably) must be treated as 'a given' by FA and as such cannot feature as topic of investigation in its own work.
3. The first two constraints lead to the third and EM's most radical proposal.
    a. Investigators should give the same time and intensity to the examination of common, obvious features of social life as they give to things which Sociology has hitherto regarded as interesting, provocative and momentous.
    b. Investigators cannot be analytically separated or effaced from the social world under investigation. Investigations are as much in play as practical actions as anything else and are located in the self-same social world of daily life as those actions which are under investigation. The ordinary, mundane stuff of daily life is or should be first class phenomena for sociological investigation and the sociological attitude to such phenomena should be one of reflexivity. The presuppositions of Sociology and other FA disciplines mean they can neither accept nor adopt either suggestion.

The implications of these constraints ripple through the way EM specifies its sociological world *for* investigation and how it constitutes its own social world *of* investigation. Out go the ontologies of the social actor imbricated in macro-, meso- and micro-structures and in comes the social actor as the organiser of courses of practical reasoning. Out goes the metaphysics of functional relations and structural emergence and in comes the metaphysics of intersubjectivity and social order as collaborative production. And with these two, out go the operationalisation of transcendental method based in epistemologies of analytic and structural realism and in comes interpretive understanding and the calibration of the adequacy of method to the particularities of the finite province of meaning under view. With the observer now placed within the investigative frame of reference, these transitions make non-negotiable the adoption of a methodological relativism articulated as the indexicality of descriptions (or 'formulations', as EM often puts it) and the reflexivity of describing. This adoption is operationalised as an orientation to the problematic possibilities of description as an in-situ, omnirelevant praxeological problem for investigators and their subjects alike.

One result of this radical re-specification is that the flotilla of issues associated with Sociology's axiom of a three-way relationship between theory, method and the social world fade away. These are the "problems" of Realism, Objectivity, and Truth and the practical ways their contingencies have been grappled with under the stipulations of Sociology's topics and methods. To all of these, EM has quite rightly and understandably adopted its attitude of principled indifference. It can contribute nothing to the resolution of these "problems" and they have no bearing upon its practise. It has nothing to say about them and has no interest in them.

Section 3. The Distraction of Kuhn

It is here where the missed opportunity we referred to earlier enters the discussion. What drove Kuhn's original concern with theory succession in the physical sciences and what continued to drive it long after he had abandoned talk of "paradigms" and their organisation, was precisely the same three-way interrelationship between theory, method and the world which Sociology struggles with. Kuhn encountered them, though, under the rubrics of the physical sciences. The active elements might have different terms as well as different characteristics (measurement, for example, matters a great deal more in science), but the order of problems was the same. How is the validity and objectivity of description to be guaranteed as part of the practical detail of undertaking scientific investigations in the face of the helter-skelter of theoretical 'churn', 'competition' and 'development'? Nonchalantly invoking Kuhnian incommensurability as a descriptor of EM's principle of indifference hooks EM to these 'troubles' not because EM openly debated its encounter with them but simply because it was assumed they *had to be* pressing questions for EM because Kuhn had asserted they were what was confronting the paragon natural science disciplines.

If Kuhn's concept of incommensurability is to be used as a designation for the EM/FA relationship, then that can only be as a technical concept in the philosophical arguments about the natural and social sciences. Its adoption depends upon the outcome of an examination of the appositeness of a direct comparison between EM's stance on FA as summarised above and Kuhn's account of theory succession consequent on his conjoining of the metaphysical consequences of distinct "thought communities" as described by Fleck [Fleck 1979] with notions of the logical irreconcilability certain orders of scientific postulates. Kuhn argued adopting revolutionary theories necessitated a collective Gestalt switch. Those adopting the revolutionary theory constituted the natural world in an entirely different way. Either side of the revolution, scientists lived "in different worlds". This metaphysical addition provided the backbone for Kuhn's much-discussed term "paradigm". The image Kuhn offers is of the history of science as punctuated evolution, the driving forces of which are socially (as opposed to logically) organised tensions over the interpretation of experimental results generated by the incommensurability of revolutionary theories. Unlike Feyerabend, Kuhn's attribution of incommensurability to theory change is not a formal analogy of the original mathematical definition of the term. It is more a rhetorical trope enhancing his argument for the complete divergence between certain domain theories and their metaphysical consequences. In an early working through of his account [Kuhn 1964], Kuhn explicitly associated the philosophical psychology underpinning paradigm change with the Sapir-Whorf hypothesis in Cognitive Anthropology.

As we all know, in the face of stern criticism Kuhn began to retreat from the forward positions he initially adopted. This was not a return to a well-defined use of "incommensurability" but part of a series of shifts to other, often more obscure, technical definitions, none of which those in EM wishing to lean on the association seem to have tracked. The major line Kuhn adopted reinforced the semantic character of theory succession by identifying incommensurability as non-translatability of terms rather than Gestalt shifts. His [1987] interim position, examines three very different types and orders of theory change: from Aristotelian to Classical Mechanics; from Volta's electrostatics to modern theories of electricity; and Plank's introduction of the notion of the "quantum". From these cases, he identifies the following significant properties of major theory changes.

1. The shift is not piecemeal but happens as a rapid condensing out of agreement on the new view. This may take some time to be realised, but when it is, it happens quickly.
2. The central change is in the character of "metaphor-like juxtapositions" (p. 21) which provide entirely different pictures of that portion of the observable natural and physical worlds the theory addresses.
3. Such changes are evident in the re-configuring of the constituents of operational taxonomies, both in terms of the re-arrangement of members and the development of new taxonomic structures.

Towards the end of his life, Kuhn [2002] talked of "lexicons" and the way changes of terms often merely re-ordered and re-clustered properties by using of different definitions. The wholesale disjuncture of the earlier conception had turned into a gradual 'speciation' of distinguishable concepts. Once introduced, the meanings of key terms morph and are then institutionalised as the bundle of referenced properties stabilises. There are two important features of such lexicons. First, they are projectable. Their application can be extended to new contexts, settings and bodies of observations. Second, as they are acquired and deployed, sets of expectations solidify around them. These provide a normativity to their use which secures their acceptance. The result of these amendments, however, leaves Kuhn in an uncertain position.

> Some of the kinds that populate the worlds of the two communities are then irreconcilably different and the difference is no longer between descriptions but between the populations so described. Is it, in these circumstances, inappropriate to say that the members of these two communities live in different worlds? [Kuhn, 1993, p. 319]

For Ian Hacking [1993], the terms Kuhn picked out are 'scientific kinds' gathered in disciplinary taxonomies. Either side of a revolutionary theory change, there are different orderings across some of the categories. Such re-arrangements are not of the order the lowly slime mould has been subject to, pushed back and forth between plant and animal kingdoms, but consist in new (such as quark and pulsar) and reconstructed (such as quantum and black hole) conceptions structured under the innovative theoretical frameworks. What the novel taxonomy provides is a new way of describing the 'old' observational data as well as new locales in which to look for new data. As a result, new ways of using old instruments are found as well as entirely new instruments designed for the new theoretical landscape. Hacking's rendering of Kuhn's notion of lexicon is restricted to natural science and natural scientific terms. In his comments on Hacking, Kuhn [Kuhn 1993] is clearly unhappy with this constraint and wants to extend the scope to natural kinds not simply scientific ones. His recently published posthumous work [Kuhn 2022] presents his final arguments for this position.

Section 4. Conclusion

Why does all this matter now? Certainly not because EM has responded to the Lazarene re-emergence of incommensurability as a topic in discussions of inter-disciplinary relations by restating the methodological logic of its own position and underscoring what its implications were. It has simply hewed to the "messaging" it has adopted from the outset: the principle of indifference. This ignores the fact the expectation these issues and their Kuhnian interpretations are relevant remains integral to any investigative discipline. Such expectations are embedded in sociological, social science and other FA disciplinary considerations of the relationships among their theoretical constellations. These days such expectation is articulated as the need to reconcile features of different 'modes of discourse' and 'standpoints' by translating their 'lexicons', integrating their 'taxonomies' and concatenating their 'results'. Only by reconciliation (or so it is felt) can the possibilities of "multi-method" and "interdisciplinary" investigation be realised. Here, just as one straightforward example of the expectation and its aspiration, is a comment about the coming together of Socio-Linguistics and CA (as well as a motley of other disciplines).

> The most important premise of interactional linguistic research is that linguistic categories and structures are designed for service in the organization of social interaction and must be described and explained accordingly. For this, descriptions of linguistic structure are combined with CA-informed analyses of sequential organization. Where relevant for an account of particular actions or action sequences, interactional linguistic analyses should also be combined with multimodal analysis, for example, gaze, facial expression, gesture, body posture, etc. [Couper-Kuhlen and Selting 2018, p. 15]

It seems the extent to which any discipline can support reconciliation of this kind has become a signal criterion of its worth.

We'll sum up by putting this outlook in the terms in which we think these issues will be encountered by those seeking accommodation and collaboration with FA disciplines. What they amount to is a budget of problems to be faced when setting out the practicalities of how alignment, contribution, integration and synthesis will be achieved. Such challenges are no more than the latter-day version of just those matters EM set its face against right at the outset. EM itself can provide no resources for the collaboration-seeking adventurers to call on. As a result, it seems to us the most likely consequence will be the subordination of EM's conceptual apparatus to that of whatever FA discipline is being flirted with. The outcome of that can only be unhappiness. Hence the moral of our title.

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14

Coda

Whilst we are interested in the future of Sociology and its ongoing relationships with Philosophy, the natural sciences, and other disciplines, it is Ethnomethodology we care about and its future we are concerned for. That future looks uncertain. We have commented in other places on the institutional and academic context in which the discipline finds itself and the constraints and opportunities afforded therein. For the early-stage ethnomethodologist, a career within a Sociology Department has become increasingly less likely. Even for those who do manage to find such positions, combining the demands of teaching with research may be prohibitive. In this Coda to our Swansong, we set these important matters aside for a moment and turn back to the discipline's intellectual environment and the disciplinary choices Ethnomethodology faces.

The crux concerns cross and inter-disciplinary relations and the probability Ethnomethodology's distinctive and guiding tenets will bleed out if closer and closer relations are sought with disciplines which do not share those pre-suppositions. Two potential alliances seem to be on the table: with natural sciences and with social sciences. The option of the natural sciences can be dealt with quite quickly. Achieving the kind of hybrid relationship to which Garfinkel aspired seems a long way off. Satisfying its requirements would make extraordinary demands on ethnomethodological researchers. We see no evidence these demands will be satisfied in the near or medium term. The social sciences are different. Adherence to its own tenets is what guarantees Ethnomethodology's character as an *alternate* sociology. However, Sociology has its own family resemblances as well as sibling rivalries with Psychology, Economics, Social History, Management Science, Linguistics and so on. These lines of commonality and disjuncture mean if

Ethnomethodology continues to seek closer alignment with Sociology, we see little chance it will be able to avoid the mishaps of methodological graft failure, the re-tuning of epistemic values, a constant plying of disciplinary relevances and the pushes and pulls of initial success which we suggested had plagued Institutional Ethnography.

Here's the rub. When we look across the field broadly defined by Ethnomethodology and Conversation Analysis, we see little evidence of any interest in, let alone continuing investigative struggle with the two defining themes we believe characterise the broad discipline as an alternate sociology: commitment to faithfulness to the actor's point of view and determination to place the observer/analyst within the analytic frame of reference. Look where we will, in the plethora of studies of leisure activities, work settings, bench science, medical practise, family life, classrooms, control centres, welfare agencies and so on and so on, we find minimal or no exploration of these questions. It is as if the community thinks the central challenges have been addressed and their practical investigative implications resolved. As we have tried to show in the essays in Parts Three and Four of this collection, this is far from the case. True, progress has been made. But only in fits and starts and only to a limited extent. Implementable disciplinary-wide programmes couched as project level investigative protocols (in other words, self-consciously reflexive characterisations of the 'methods' in the 'methodology') are wholly missing. In their stead, we have weakly-informed and overly optimistic reliance on a few analytic tropes and related data collecting instruments both of which have been culled from a small set of exemplar investigations. Ask yourself this. Where would most contemporary ethnomethodological investigations be without the near ubiquitous use of the Jeffersonian transcript (or some version of it) as the representational device around which to organise collections of noticings about social settings? What is the basis for assuming a transcript provides the best (or perhaps the only) account on which to rest an analysis?[1] Moreover, where in this usage is the researcher-as-transcriber? Even more important, where is the investigator *in the analysis*? What has happened to the locally embedded, endogenously produced representations of the action in flow available to those in the setting? Where are the analyses of them? And if they were to be used, how would the adequacy of those analyses be grounded?

Could it be that the alacrity with which members of the ethnomethodological community and those in communities taking leadership from Ethnomethodology, have turned to accommodation, integration or even assimilation within social science disciplines, is telling us the

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[1] The use of the transcript as a device for reasoning about talk in Conversation Analysis is a separate question. But it, too, has remained largely unexamined.

decisive severing implied by adherence to Ethnomethodology's incommensurability and its commitment to working out in all its detail the requirements for an alternate sociology, never really took hold? If so, this might account for why so much of Ethnomethodology's practise as a practical investigative sociology continues to rely on standard, empiricist-inspired sociological mechanisms for reasoning from data to phenomena and onwards to findings. Note the quantifier. We are not saying all Ethnomethodology is like this; but a great deal of it is. Filter out the emblematic vocabulary and obligatory referential gestures, and the analyses seem very little different to those offered in mainstream social sciences.

No doubt this suggestion will cause heartburn and increased blood pressure among many colleagues and friends. But, given reflection, it really needn't. If Ethnomethodology has demonstrated anything, it is that all special modalities of social life (academic disciplines, professional occupations, and other finite provinces of meaning) can be construed as being rooted in local concatenations of domain and commonsense reasoning. They rely on these melded methods to sense assemble the worlds in which their provinces of meaning are immersed. Ethnomethodology emerged from 'common sense sociology'—the routine, taken for granted, quotidian practices of conventional sociological analysis—and so it should not be surprising that without constant self-vigilance, there would be a tendency to a recidivism and the use of these methods carried through into its own practise. Reliance on the common sense reasoning out of which a discipline emerged is, after all, precisely what Ethnomethodology has revealed at the heart of bench science, medical practice, organisational life, leisure activities, software engineering and endlessly on. It has also been our own finding about different forms of sociologising, including examples of work originating in Ethnomethodology itself.

It is against this background that we make our suggestion concerning the choices the discipline faces. If grounding Ethnomethodology as a substantive alternate to conventional Sociology is still the overriding objective, then we need to look to look anew at the modes of reasoning we operate with. It is precisely to such modes our suggestions for reading sociologically and investigating sociologically have been addressed. As an alternate sociology, Ethnomethodology must deploy entirely new and different ways of turning to the social world; ways which place the investigator at the heart of the frame of reference and which do not rely on methods premised in the adoption of decontextualised and abstracted systems of categorisation. Ethnomethodological descriptions of the particular and its typologies of general forms derived from them, must preserve the structure of the social actor's experience as part and parcel of the sociologically constituted objects being subjected to analysis. Colleagues and friends are urging the community to return to the beginnings and re-forge the discipline. We have a lot of sympathy with that plea. But the repetition of history as farce can only be avoided if, in doing so, we ensure things are done somewhat differently next time.

Of course, grounding Ethnomethodology as a substantive alternate to conventional Sociology may no longer be the overriding objective. But, if that is the case, couldn't we save a lot of time, angst and ill-temper just by saying so?