THE METHODOLOGY OF THIRD PERSON PHENOMENOLOGY

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Undoubtedly he won't recognise it, but the origin of this monograph lies in a couple of conversations with Lewis Hyland. So, belated thanks to him for the prompt even though he probably does not agree with anything we say!

We would also like to thank Graham Button for helpful comments on a late draft of the monograph.

FOREWORD

This monograph returns to issues of methodology and in particular to aspects which have not really been given extensive consideration in discussions of various forms of Ethnomethodology (EM), namely the choices over investigative metaphysics and epistemology. Such reluctance is quite surprising (even though we are all familiar with aphorisms about plumbers and dripping taps or cobblers and their children's boots) because one of the early sentiments into which EM tuned was a deep sense of dissatisfaction with the standards of methodological rigour evidenced by much of what was then contemporary Sociology. This dissatisfaction concerned matters not only of 'method and measurement' but also the status of claims being made based on the given systems of method and measurement.

To be fair, though, in both his early and late work (admittedly while following his own distinctive lodestars) Garfinkel (1967, 2002, 2019) did raise some of the central issues along with many others which are closely related.[1] Also, in a number of his lectures (e.g. those which explore the notions of a "naturally observable social science" and actors' orientation to the production of "visibles"), Sacks (1984) surfaced a range of important questions, while here and there in his own publications, Schegloff (1991, 2009) allowed himself the luxury of relevant commentary regarding the 'machineries' of Conversation Analysis (CA). However, despite these contributions by founding figures, any balanced assessment would have to conclude many central questions remain open and many problems remain unresolved.

It is not our intention here (even if we thought we were capable of it) to take on the Herculean labour of resolving all the open questions in one swoop. Rather, we follow a more pragmatic and hence circumspect strategy. We will consider only one corner of the EM investigative field, the one we have dubbed "Third Person Phenomenology" (TPP), and its challenges in forming a rigorous methodology. There are two things to say about the limitation we have set ourselves. First and most importantly,

[1] To be equally fair, we have to say the early work, though rich and fertile, is clearly preparatory and the late work fascinating but inconclusive. Those who find many of the core passages of *Ethnomethodology's Program* overly cryptic, do Garfinkel a disservice by managing to overestimate his achievement whilst underestimating the effort he expended trying to wrestle his emerging ideas into as cogent and perspicuous a frame as possible.

we are explicitly acknowledging not all EM conceives itself to be TPP. We have no intention of implying all modes of EM are, unbeknownst to themselves, actually forms of TPP. Second, and this is related to the first, it is important to treat Garfinkel's insights and proposals as facilitative not prescriptive. His own studies demonstrate a number of the alternative (but not strictly alternate) ways in which things might be taken. This is not to say that EM is a church whose credo is a Feyerabendian 'Anything Goes!'. Far from it. There are firm principles and there are firm boundaries. But within their ambit are to be found many differing seams for exploration. TPP is just one.

The central focus of TPP is what we have called "the interior configuration" of action and its task is the description of action as socially organised within contextually reproduced fluid Gestalts. The guiding concern is how to provide analytic accounts of what a course of social action looks like and hence is experienced as from 'within'. For the avoidance of doubt, though, one thing must be made clear. TPP has no truck with formal or informal quasi-therapeutic interventions masquerading as *verstehen*. Neither does it see the purpose of analysis to be the provision of empathy, sympathy, mindfulness or any other kind of emotivist support.[2]

Once again, the heart of this discussion is TPP's central question. How should we describe the lived-experience of courses of action and what status should those descriptions have?[3] We have attempted answers to these questions before. Whilst each endeavour took us several steps forward—or, at least, so we like to think—none were wholly satisfactory. Therefore, a bit like Robert Bruce's spider, we are trying again. This time (and here we recognise we might well be taken to be either hopelessly heretical or massively reckless), we will bolster insights provided by the usual sources with contributions made from elsewhere.

The Arc of the Argument

Unlike the disciplines of Physics, Biology, Mathematics and even Cognitive Science, the philosophy of Sociology[4] remains, to put it as kindly as we can, somewhat underdeveloped. Despite occasional incursions by philosophers armed with arguments derived from other domains, by and large the philosophical basis of Sociology continues to be immured within the conceptual structures of the founding fathers. When it comes down to it, the differing philosophical commitments made by Comte, Durkheim, Weber and Marx define the field. This has brought stability (if not clarity and consensus) but at the cost of locking the discipline onto 19th century conceptions of the sciences and humanities and the nature of (social) reality.

Two closely related things have followed from this. First, by and large philosophical discussion of metaphysics and epistemology have not featured highly in the training of aspiring sociologists nor in the explications they give for the findings they present. As a result, and this is the second consequence,

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2 Interpretive and qualitative sociologies have endured a long history marred by this confusion; a confusion brilliantly diagnosed and described by Egon Bittner (*1973*). Claire D. Nichols' recent offering (*2019)* is simply the latest manifestation of it.

3 We take it as a given this is a matter of choice; a corollary of adopting a strategy which we, following Garfinkel, will call "conceptual play". See Part Two Section 3. Conceptual play defines the point in EM where investigative metaphysics and investigative epistemology come together rather than fall apart (or at least they had better, for all our sakes).

4 The philosophy of Sociology is to be distinguished from the field of social philosophy, though the latter, as an exploration of the conceptual analysis of 'sociality', has an important role in shaping the former.

methodologies have tended loosely to track classical positions in the physical or interpretive sciences which themselves have traditionally relied heavily on the Natural Attitude. The use of the former is marked by a continual translation (and re-jigging) of mathematical and other models from the physical and biological sciences as analytic devices for describing social phenomena. The use of the latter can be picked out in the emphasis placed on narrative constructions based on 'world views' attributed to different social locations. In both cases, the relevant 'investigative methodology' remains schematic at best. Decisions about the framing of investigations are taken by default. In large part, of course, this is because the general principles on which such investigative methodologies might be based are either weakly formed or even missing.

So, what do we mean by an 'investigative methodology'? We use this term in the broad sense we have taken from Felix Kaufman (1958). There are two very important features which we need to stress. First, whilst problem formation and data collection are important, these are matters of investigative technique (or 'methods') and therefore do not wholly define the scope of methodology. Investigative technique is very much the junior partner in methodology, relying for its direction on choices made over investigative epistemology and investigative metaphysics. Since investigative techniques are the one element of EM's methodologies which has been widely discussed, we mention them only in passing. It is the other components, investigative metaphysics and investigative epistemology, with which we are concerned. Here we find the second feature. The principles to be adopted with regard to metaphysics and epistemology are open choices. They are matters for investigators to weigh in the light of their disciplinary investigative interests. None of the available 'bundles' of tenets should be regarded as mandated nor universally applicable. No methodological principles have the status of 'the default'.

Although investigative epistemology and metaphysics are closely related— as we will see, they contain reciprocal commitments — trying to deal with them whilst also tracing out all their criss-crossing interrelationships would make for a very messy presentation. So, even though we will mark the most important correlations, we will take each individually. We begin by positing a baseline for our argument summarised in the following question: Do the methodologies of the natural and biological sciences offer a pathway to adequate description for EM and TPP?

With this question in place, we can look to the alignments. Broadly, the epistemological part of the package making up the methodologies of the natural and special sciences espouses some version of 'realism'. In Part One we will use a standard account of 'realism' as a platform on which to raise issues concerning the truth status of the propositions set out in EM and TPP. With Catherine Elgin and Roman Frigg as guides, we sketch a set of epistemological questions to which EM and TPP should respond. The core concern emerging from these questions turns out to be the lack of a clear position on the issue of 'realism' and hence the lack of a rigorous and systematically derived position on methodology. We offer this as an interim and motivating conclusion. It sets us a task, namely to provide an operational epistemology for TPP's methodology.

In Part Two, we assemble the resources required for the complementary metaphysics. This is drawn not just from the usual sources but from other possibly less familiar ones. For many, the most surprising of our borrowings will be ideas we take from Bergson and Kotarbinsky, two (let's call them 'scholars' to avoid starting a range war over their precise predilections) who are rarely if ever mentioned in EM discussions. The judicious use of elements of their thinking will help smooth a few of the humps and bumps as well as join up a some of the disconnected paths and tracks encountered when trying to lay out a broad and serviceable highway from the questions framed in Part One through Garfinkel's seminal assemblages of *obiter dicta* to the provision of a robust investigative methodology for TPP. Part Three uses the platform so constructed as a springboard for sketching specimens of TPP analysis. Part Four circles back to the beginning by asking about the complement to the challenge set by adequacy of description, namely probativeness.

The purpose of this monograph, then, is not to re-tell the EM story.[5] We are pursuing our own interests and trying to push on from the positions we currently occupy. We accept a lot of what we propose is the scaffolding missing from our earlier studies and no doubt much would be gained if it were to be retro-fitted around those efforts. Though not on the scale of Theseus' ship, this exercise would not be insignificant. However, the much more important possibility this monograph aims for is a delineation of the way future TPP investigations might be shaped and the opportunities which might be opened up thereby.

[5] There is one other important point to make here. Tracing Garfinkel's intellectual biography and mapping the various influences on the evolution of his thought is very important work which requires detailed analyses of the materials in his archive. Fortunately, a start has been made by Anne Rawls (2011) and Mike Lynch (2007, 2019), but only a start, Much is left to be done. This discussion is not directed to that work. Rather we come to the 'program' Garfinkel arrived at— incomplete as it was— with the question 'How do we move on in the direction we would like to go?'. The materials we have assembled here are offered in service of that question and not the equally interesting one of 'How did Garfinkel end up where he did?'.

PART ONE

REALISM, VERITISM AND NARRATIVES

1. INTRODUCTION

To situate the central problem, consider the following stories.

> An experienced researcher, call her Charli, responding to complaints from her students that there are no up to date textbooks on contemporary issues in EM and CA, decides to write her own. Her target audience is 3rd Year undergraduate and 1st Year Postgraduate students. She wants to offer a sophisticated and detailed summary of the current state of the discipline. The chapters will be organised by sections. There will be one section on the history of EM and CA's emergence in the line of development Sacks, Schegloff and others pursued together with a discussion of 'methods'. There will be a second on the key devices for organising sequences in conversation, the Turn Taking Machinery, utterance units and their repair as well as preference structures and conversational activities. The third section will discuss EM devices such as Lebenswelt pairs, instructed actions and 'shop floor' production processes. The final section will summarise the increasingly interdisciplinary nature of EM and CA, hybrid disciplines as well as the emergence of Epistemics and its trajectory of convergence with Linguistics. *At no point does Charli envisage providing a discussion of the epistemic status of EM or CA's 'mechanisms', 'rules', 'generalisations' and 'preferences'.*
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> Peter is discussing a proposed project plan with one of his Graduate students. The student is interested in the social and ethical implications of new technologies and in particular the implications of using AI techniques to provide increasingly 'smart' self-aware devices. The model for the project is Lucy Suchman's very influential study *Plans and Situated Actions*. The student proposes Amazon's Alexa app as the demonstration technology. With a Computer Science friend, he will design and implement a small suite of Alexa 'skills' which require Alexa to 'reason' rather than simply search for pre-packaged data. This, it is proposed, will enable an investigation of Alexa's capability to pass a 'Turing Test' in virtue of her responses to exercises set her by a small number of student volunteers. The student sessions will be video and audio recorded to allow detailed analysis of the 'natural flow' of the inquiries. The discussion covers the usual plan elements (background to AI, background to EM and audio-video analysis, details of the Alexa technology, methods, data analysis as well as ethical clearance). *At no point does Peter ask if the report is going to draw out and offer sociological judgements on the basis for and status of the differences in the pre-suppositions underlying AI and EM views of reasoning and how they should be handled.* In

particular, he makes no mention of the notion of 'conceptual play' as that might be applied in Cognitive Psychology, Cognitive Science and/or EM.

We'll state things bluntly in order to get the issues clear. It is not that experienced EM[6] researchers like Charli and Peter do not know there are debates over the status of theory and description in the natural, special and interpretive sciences. They and we have all read Winch, Putnam, Quine, Papineau, Bhaskar, Foucault, Taylor etc. It is just that generally none of us seems to think the arguments apply 'in the EM space' or, perhaps, we don't see how to apply them there.[7] In particular, the twin issues of 'truth' and 'realism' and EM's take on them are not located and worked through so much as systematically ducked. As a result, the operating procedures of many contemporary EM studies appear to be rooted in an implicit constancy hypothesis coupled with what looks almost like the 'naive realism' of the Natural Attitude. If this is a 'problem' for self-understanding within EM, it is a downright disaster when EM approaches contiguous disciplines (for example CSCW, HCI, Development Studies, Gender Studies and Educational Studies) with offers of research partnership or where its insights and methods are translated into the local dialects of such disciplines in order to deploy them in 'interdisciplinary' studies.

We need to introduce a preliminary caution right up front. In Part One we will be running over (or around) a number of open debates in Philosophy. Our task is not to resolve nor unify them. Sociologists correcting philosophers is always more comical than enlightening. Instead we will be asking what EM's position on the central issues might be. Our initial (and hence eminently dispensable) working assumption is that EM will be unlikely to derive an overarching philosophy of (social) science all of its own but will (somehow) relate to those adopted more generally.[8] Our question in Part One, then, is how the issues of 'truth' and 'realism' might be surfaced and appraised in EM and TPP. Trying to get a clear view on that question is the first part of our task. We then move on to suggest one way those issues might be handled. In Part Two, we discuss a range of issues relevant to EM's investigative stance and TPP's positioning therein. Our challenge is not to impose an analytic unity on EM. It is far too late for that anyway. Rather we will be constructing a framework, a bridge, from the issues we started with to secure a grounding for TPP as we conceive it.

2. WHOLEHEARTED REALISM

In his definitive treatment *Scientific Realism*, Stathis Psillos (1999) identifies the basic scientific realist position (let's call it "wholehearted realism") with three interlinked stances.

1. A metaphysical stance: the world has a mind independent structure.
2. A semantic stance: scientific theories must be taken at face value. They provide 'literal' descriptions of their targets; descriptions which can be true or false.

6 For brevity's sake, we are going to talk only of EM. For our purposes here, we do not think anything substantial turns on the distinction between EM and CA.

7 One signal this is so could be the apparent reluctance of EM researchers to rise up in their wrath and confront the purveyors of 'autoethnography', many of whom seek to ally themselves with EM approaches. (See our 2015)

8 The best argument for this belief is the undeniable fact no-one in EM seems remotely interested in doing much more than sketching the derivational work anyway.

3. An epistemological stance: the mature and predictively successful sciences must be regarded as (at least approximately) true.

Darrell Rowbottom (2019) identifies a fourth stance or thesis in Psillos' account; a methodological commitment.

4. Implicit in the realist thesis is the stance that the ampliative-abuctive methods employed by scientists to arrive at theoretical beliefs are reliable: they tend to generate approximately true beliefs and theories. (Psillos, p. xxi).

Although philosophers have myriads of nuances they wish to introduce into realism (we will get to some of them below), most practising natural and special sciences generally adopt wholehearted realism without demur. The human sciences, though, do not. Some (Economics, Psychology, Geography, Linguistics) have sought ways broadly to align themselves. Some (History, Literary Studies) reject the position out of hand. Yet others manage to face both ways (Anthropology, Archaeology and, of course, Sociology).

Most less than wholehearted realists, of whom there are hordes, qualify their position on 2. Some are happy to weaken the degree of belief required for scientific theories. Others want to select only the defensible parts of a theory to be treated wholeheartedly.

So where does EM stand on wholehearted realism? Given there has been no (let alone extended) discussion of these matters, the only way to work is to see what is implied by what EM says about its work and how it goes about it. We'll take the stances in turn.

THE WORLD HAS A MIND INDEPENDENT STRUCTURE.

There are two parts to this. First, galaxies, giraffes and genes have their existence (Being) independently of our Being. In a counterfactual world where the only difference was that we had not evolved, galaxies, genes and giraffes would still exist. Second, the structures of that world (the objects and their powers and relationships) would also be much as they are now. This, broadly, is the stance adopted by the Natural Attitude so it is not surprising to find it to be the ground on which science has been built. At the core of this view is a position we can call 'entity realism'. In Cartwright's (1983, 1999) initial version, we are justified in taking theorised entities to be real just in case the tenable causal stories in our science require them. This view has been severely criticised. More recently Markus Eronen (2019) has offered an alternative. We are justified in holding entities to be real just in case we have multiple independent ways of measuring, modelling, accessing or manipulating them. This offers a 'likelihood' argument. The more ways of dealing with an entity we have the more likely the entity is to be real.

The trickiness realism poses for any kind of Sociology arises because the objects and relationships it studies are cultural and its focal object, the social actor, is defined as 'interpretive'. The cultures created by social actors are, by definition, the products of interpretive action. The topic for Sociology is the description of patterns of culture (one of which is the discipline of Sociology itself). Of course, the social and cultural worlds we inhabit are not invented *de novo* by each of us (nor by each generation of us). We are, in Heidegger's famous image. "flung into them", encountering and accommodating them as facts of our social existence. While each of us treat the structures of the social world as external and constraining ('Durkheimian' social facts?), the constitution of their 'facticity' cannot be the same as that of galaxies, giraffes and genes. But, nonetheless, if we take their properties to be constraining, we are committed to accepting these entities are real (either in a standard empirical 'science of society' sense or in some version of a W.I. Thomas sense).

So what is EM's position on this aspect of wholehearted realism? It is hard to say. If we look at contemporary CA, for example, we find much talk of 'devices', 'mechanisms' and 'structures', objects which appear to be viewed *as if* they were mind independent in the sense associated with proposition 1. On the other hand, the crucial analytic device remains an 'orientation to recipient design' which is interpretive in character and treated as normative. Other EM objects such as 'Lebenswelt pairs', 'instructed actions', indexicality management devices (glosses, *etc. clauses, factum valet* devices etc) and the famous 'Independent Galilean Pulsar' are clearly interpretive but their *forms* are institutionalised in working cultures. The *modus operandi* for EM seems to be to adopt an unprofessed *analytic realism*, perhaps under the auspices of what in Part Two we call an investigative strategy of 'conceptual play'. But if that is the case, inevitably at some point EM must bump into Phil Agre's question: "What is the fact of the matter?" or its much more dangerous twin, "Is there a fact of the matter?". When that moment arrives, the ghost of Mel Pollner will rise from its grave to haunt us all (Pollner 1991).

SCIENTIFIC THEORIES ARE LITERAL DESCRIPTIONS.

The term 'literal' here can cause some puzzlement, particularly since the majority of theories in the natural and special sciences are actually symbolic not literal in form. They are mathematical formalisms of various kinds. Others are diagrams, physical embodiments, mock-ups, simulations and the like. What is intended in the definition is the mapping between the 'target' (say a water molecule) and the description (say H_2O). The relationship is 1:1. All the essential chemical properties of a molecule of water are contained in the described chemical structure. Obviously other physical properties of water (its boiling and freezing points at atmospheric pressure, its translucence) are set aside because they are not relevant to a molecular description.

So how does EM fare here? This time we can at least find some clarity if not unanimity.[9] In CA there has been a long running debate about the relevance of ancillary sociological parameters such as power, gender and institutional status for the management of the sequential organisation of conversation as well as for the formulation of conversational objects such as co-selected descriptors. More recently, 'status relative to the distribution knowledge' has been added to this list. There is, therefore, a divergence between the proponents of a 'barebones CA' concerned with explicating sequences purely in terms of structural conversational objects and their management and those who seek an 'elaborated CA' which melds sequential organisation with conventional sociological forms of analysis.

Since the rest of EM is more tightly tied to (hobbled by?) the anchor of Phenomenology, the notion of literal description cannot get much traction. All descriptions are 'reflexive' or 'occasioned': the product of being a discipline structured around Brentano intentionality. Given EM's desire for rigorous grounds on which to base choices over the formulation of an investigative metaphysics, literal description does not appear to be an option. In addition, EM operates a policy of 'analytic indifference' to the challenge of 'sociological elaboration'. Such a stance can be an effective policy as long as the ambit of research is circumscribed within domains specified by EM itself. It becomes much more troublesome when EM takes on the role of research partner in domains where the predominant disciplines are characterised (in part) by a commitment to proposition 2. How does a metaphysics of analytic indifference of the kind EM adheres to (grounded as it is in constrained disciplinary choices over the 'problematic

9 Maynard and Clayman (1991) discuss many of the divergences.

possibilities of description') triangulate with a metaphysics of wholehearted realism? The silence on this issue among those EM investigators who undertake inter- and cross- disciplinary work is palpable. The obvious choices seem to be somewhat unhappy ones. Either EM gives up Brentano intentionality and its legacy in Phenomenology (thereby sawing off the branch it is sitting on) or it translates (respecifies) wholehearted realism into its own terms (thereby doing violence to the experiential features of the scientific practice it is partnering with).

THE PROPOSITIONS (PREDICTIVE GENERALISATIONS) OF MATURE SCIENCES ARE (APPROXIMATELY) TRUE.

As we have suggested, this is the keystone of the epistemology of the natural and special sciences. Given 1 and 2 (and an appropriate logical structure to their evaluative methods — see below), the established propositions of the sciences are to be taken as true, or as providing the best picture we can have of the world given the state of our knowledge to date. The usual rationale for this position is that it satisfies the 'no miracles' or 'no cosmic coincidence' requirements which would otherwise have to be violated to justify the successes of science. Theory succession then becomes a matter of 'tuning' and 'recalibration' or occasionally 'reconstruction' and 'renovation'. Despite the giddiness inspired in the sociology of Science by superficial readings of Kuhn's (1962) account of the history of theory change in Physics, we should note that "paradigm shifts" are not wholesale. Necessarily, much is carried over from the old to the new and carries on as it always did. What usually changes, though, is the master narrative in terms of which the translated actions are explicated.

No-one wants to say EM is a mature science and probably no-one wants to claim it has established a suite of predictive generalisations in the sense the natural and special sciences have. Even so, can we say definitively this is not the journey EM has planned for itself? Should we expect the propositions making up the Turn Taking Machinery, the formulations of the rules used by jurors, the descriptions of the rules offered as governing the co-selection of descriptors or the designated properties of Lebenswelt pairs (for just a few examples) to be refined and honed through the accumulation of replicated studies and domain extensions? Does EM think a strategy of epistemic certainty is normative? Looking at the way studies position themselves, it is hard to say it doesn't. But then again looking at how it manages the aggregation of the studies it undertakes, it is hard to say it does.

AMPLIATIVE-ABDUCTIVE METHODS GENERATE APPROXIMATE TRUTHS

What we have here is a logic of inference to the best explanation. The examples are numerous. In the 1846, Le Verrier used of Newtonian mechanics to predict a planet further from the sun than Uranus. The existence of this planet (Neptune) was confirmed by Galle a month later. Einstein predicted the gravitational bending of light which was confirmed in the famous Eddington and Dyson experiment in 1919. Psillos' own example is the discovery of the neutrino. The principle of the conservation of energy seemed to be violated by the standard account of ß-decay until Pauli proposed the energy of the decaying neutron was preserved in an as-yet-undiscovered particle with no mass or charge but spin, i.e. the neutrino. In all three cases, science accepted the existence of the objects or effects prior to their 'discovery' (thereby demonstrating its commitment to entity realism). This was inference to the best explanation. The logical form of this strategy can be captured in one of the various forms of the probability calculus.

Are ampliative-abductive discovery strategies followed in EM? Certainly the selection of programmes of studies seems less focused on exploring clustered conjectures of the kind just illustrated than in taking advantage of contingent opportunities which come along. True, there is an accumulation of findings but it does not appear to be probative in character. We have already discussed this matter extensively (Anderson and Sharrock 2017).

So, what to do?

3. VERITISM: THE ACHILLES HEEL?

These days, no philosopher wants to deny the truth of proposition 1. There are, as we have suggested, some strong voices wanting to qualify the truth of proposition 2 somewhat. While it remains a puzzle as to *how* the truth of proposition 3 is grounded which, given the successes of the natural and special sciences, almost everyone accepts it must be (somehow). Catherine Elgin (2017) is among the naysayers regarding 3. For her, the idea the mature sciences are on a trajectory to home in on 'truth', something she calls "veritism", is an illusion tantamount to self-delusion.[10]

> Although it seems reasonable, this stance has a fatal flaw. It cannot account for the epistemic standing of science: for science unabashedly relies on models, idealizations, and thought experiments that are known not to be true. (Elgin, 2017, p.1))

She would likely caution EM against adopting a strategy of epistemic certainty if that involved accepting veritism. Let's now see why.

Suppose we start with those statements in the natural and special sciences which seem most secure, the classic physical laws. The backdrop to veritism is a conception of disciplinary knowledge as a compendium or aggregate of molecular groups of propositions. The classic cases are Newton's Laws of Motion, the Laws of Thermodynamics, the Gas Laws, Snell's Law and so on. Each is recognised to be an integrated bundle and each bundle is not expected to be independently verifiable on its own. They are woven together and depend on each other. So the truth of the whole stands or falls on the truth of the particulars. The problem is none of the Laws just stated provide a literal description of matters of fact. They are idealisations which hold for constrained circumstances. The gravitational attraction of particle masses varies with mass *and* with charge. The angle of incidence *only* equals the angle of refraction in materials with highly uniform opacity. Particles in motion are not massless.......and so on. Moreover, these limiting conditions are well known and well understood. If science is grounded in the veritism of wholehearted realism, then a lot of it not only fails the test of truth as that stance conceives it, but is also known to fail. There is more.

> ......science routinely transgresses the boundary between truth and falsity. It smooths curves and ignores outliers. It develops and deploys simplified models that diverge, sometimes considerably, from the phenomena they purport to represent. It subjects artificially contrived lab specimens to forces not found in nature. It resorts to thought experiments that defy natural laws. Even the best scientific accounts are not true. Not only are they plagued with

[10] Reading Elgin (and some of the informal discussions of their own work by Einstein, Kelvin and others), what is most striking are the parallels to the cases of 'cognitive dissonance' in *The Three Christs of Ypsilanti* (Rokeach 1964) There is an acknowledgement scientific claims sit on fragile grounds but then a denial of the implications of that acknowledgement.

> anomalies and outstanding problems, but where they are successful, they rely on law like statements, models, and idealizations that are known to diverge from the truth (Elgin 2017 p.14)

Given this, why hasn't science been banished to the outer darkness? Because it works, somehow.[11] The falsehoods it purveys are, in Eglin's phrase, "felicitous".[12]

Felicitous Falsehoods: 'Handy but not so little white lies'

We should remember Elgin is interested in the philosophical strength of scientific epistemology.[13] It espouses veritism but she finds that espousal shot through with contradictions. The actual formulation of scientific theses does not conform to the ostensible standards veritism promotes. This looks like it might offer some air cover for EM. After all, if the epistemology of the 'real' sciences is as ramshackle as Elgin claims, then surely EM does not have to worry overly much about the epistemological status of its own work?

But not so fast. What shores up science's reputation as a rigorous discipline is both its success and its reliance on mathematical 'tools' built with truth conserving logics. For Elgin, these mean 2 must be true. Science does offer literal descriptions. Wholehearted realism is justified because of 1 and 2. It is only veritism she wants to let go. The larger part of *True Enough* is an attempt to provide a different (ethical rather than truth conserving) logical prop to replace 3. Interesting though this line of thinking is, it is not what we are worrying about just now.

We have seen the iconic (in the best sense of that word) summary generalisations of the natural sciences do not satisfy veritism's standard. The reason is they depend in various ways on procedures which result in felicitous falsehoods. Even if EM has not developed an array of summary laws (and it is not clear parts of it do not think it is or will be in the business of doing just that), if we take a first look at how felicitous falsehoods finesse all sorts of 'difficulties' in science, will we will then be able to see parallels for EM? That is the question we now take up.

Here, taken from Elgin, is an initial sketch of a few of the strategies mentioned just now.

Data management

The example Elgin stresses is 'curve smoothing'. Each data point in a display is presumed to mark a true relationship between instances of variables. Investigators interpret over the distribution of the assembled truths to try to discern a pattern. To do this, they extrapolate beyond and interpolate within the given data by creating a single summary description (such as a regression line) to bring out

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11 The 'somehow' covers a multitude of virtues and vices, possibly the most discussed of which is the role Mathematics plays in scientific reasoning.

12 Eglin is explicitly operating with a binary conception of truth because that is the precept at the heart of veritism. She accepts that the molecular propositions of science may actually be neither true nor false. But for now, since it is veritism she is hunting down, she insists on using its conception.

13 In other words, she is concerned only with the logical consistency or strength of arguments which can be made for the truth of Snell's and the other laws. She is not concerned with demonstrating their actual truth (or falsity) only what we can 'logically' or 'rationally' say about them.

the trajectory inherent in them. These summary curves are then used to represent the observed relationships.[14] Other mechanisms for curve smoothing are data excision, where outliers are ignored on the basis they are random eccentricities, and data concentration where particular runs of data are picked out as exemplary of the features under study. Interestingly, an example Elgin uses elsewhere might also be included here. Fluid dynamics has the tools to derive an appropriate second-order partial differential equation to explain laminar air flows over aerofoils. However, the equation is non-linear and so does not admit (currently) of an analytic solution. This being the case, scientists prefer to rely on an extant first-order partial differential equation which can be resolved even though it is known to provide only an approximate (i.e. 'incorrect') solution.

Data management techniques are visible in EM too. The most obvious are the data capture and transcription protocols for audio and video recording. The streams of experience in which those party to an occasion are immersed are rendered as data in 'the tapes'. This is not a theorised selection process but a matter of the practicalities of where recording devices are placed, what they can 'pick up' or 'see', how their intrusiveness is to be managed and so on. These choices, while evident in the data, are excised from the discussion of the fit of the data to the experience. What is not 'on the tapes' is deemed not practically relevant to the interaction as investigated. Then there are the procedures by which the tapes themselves are reduced to instances of the analytic object 'the transcript', the representation in terms of which the completed analysis is presented. Other data management techniques concern the identification and colligation of data bases of cases through the tagging of 'snippets' with analytically relevant identifiers. There are many more, but we have explored them in our (1984) and there is no need to repeat that review here.

Ceteris paribus claims

Snell's Law applies when the opacity of materials is uniform. But this is so only in a small number of cases. Most materials are optically anisotropic. The Law is almost correct, though, for many materials and especially those of interest to science. Equally, the law of gravity does not fully capture the force of attraction between particles when those particles are charged. What redeems both examples is the trading of condition selection for approximate fit. The Law of Gravity is a very good predictor of gravitational attraction in the 'meso-world' of ordinary daily life. In the micro sub-atomic world, charge begins to matter. In the case of both gravity and light refraction, enshrining the generalisation in a ceteris paribus form allows the formulation of a 'base case' in terms of which variation from the standard can be 'normalised'.

It is hard to see CA's classic statement of the Turn Taking Machinery as anything other than a ceteris paribus generalisation. It plainly is a summary of 'normal form' turn taking. Moreover, the analytic explication of the machinery's operation expressly works in terms of the normalisation of apparently aberrant cases. They are presented as repairs within divergent but compliant forms. In EM the general statement that settings are self-explicating[15] has a similar character. They are except when they aren't, as some of the famous breaching experiments showed (Garfinkel 1967). But, as with turn taking, the

[14] Much the same could be said about other statistical representations of the distributions. Averages are particularly perilous objects.

[15] Terminology matters here. The variants 'are treated as self-explicating' and 'are organised to be self-explicating' are very different in character to the statement 'settings are self-explicating' *tout court*.

proposition defines a normal form and the interactions in the experiments were analysed precisely in terms attempts to sustain that normality.

Stylized facts

Perhaps the most obvious stylized fact in the natural sciences is the standardisation of time. Time is characterised as a constant passage of 'instants' thereby allowing the whole apparatus of data measurement to function. Except, it doesn't. At the intergalactic and sub-atomic levels, time morphs and, in the latter case at least, may even go backwards — if the relation between cause and effect is taken as one marker of temporal succession. But for most instances and most purposes, it is good enough to treat time as having the property of standardised progression. Other stylized facts can be found in the way the Central Limit Theorem is used when analysing velocities of particles in large systems where the particle tracks are presumed to be random distributions and hence where the properties of individual particles are statistically independent. In most of these systems, proximity actually does matter (for gravitational and similar reasons, say) but since these effects are impossible to calculate on the scale required, the Central Limit Theorem allows average speed to represent the system as a whole.[16]

One central stylized fact in EM is the ubiquity of recipient design and methods for its production. And, of course, for many cases this holds. People do expressly shape their actions to fit in with those of others and the occasions they find themselves in. But not always and certainly not always explicitly. Talking about the coordination of activities as if they were always expressly constructed to comply with an expectation of recipient design allows attention to be focussed on structural properties and their formation. We can see the activity stream as a production process (a stylised fact in its own right) and look for the praxeological characteristics it can be found to display.

Idealisation and generalisation from limiting cases

Idealisation is widespread in science: particles with zero mass, pendulums with no friction or gravity, gases which are only subject to the local effects of pressure and temperature. These all allow idealised generalisations from limiting cases. In the social sciences, the classic example is the infamous 'economic actor' motivated by utility maximisation alone, having a highly differentiated and fixed preference system and perfect knowledge of the market. Once again, the justification for the use of these devices is their usefulness as codifications of selected possibilities. Everyone knows particles have masses and pendulums experience friction and gravity. Equally everyone knows that no-one makes economic choices solely on the basis of utility maximisation using a fixed and stringent preference system (and as for perfect knowledge of the market......).

Within EM, we find idealisations as well. There is the pivotal EM 'homunculus' defined by Garfinkel as living its life to satisfy the theoretical requirements of the investigative sociologist. There are the turn taking operatives whose attention is focused on managing the co-production of talk. There are the study participants whose understanding of new technologies depends only on what they can glean on 'this' occasion or who are engaged in the repair of 'specifically senseless' circumstances and only have the 'occasion' on which to draw for resources.

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[16] The example Elgin uses several times is the dissipation of mutations in a genetic population. The same Central Limit logic is used.

Using the standards veritism sets for itself as her means of calibration, Elgin finds much scientific practise to fall short of its own stipulations. This contradiction is a epistemological paradox. Since what she calls the "eschatological" option of rejecting science is proscribed by the institutional inhibition most Philosophy has adopted, any denial of the cognitive pre-eminence of science is ruled out. Instead, she opts for an alternative strategy. As already mentioned, she sets out on a search for the grounds of a commitment to the maintenance of ethical virtue to secure science's epistemology. In short: it should come clean on its assumptions, limitations, blind eye turning and the rest. Such a virtuous science might be attractive, but it too has all the hallmarks of an idealised limiting case.

For no other reason than we see no need to try to save science from itself, we will not follow her down this path. Our interest is in EM. From what we have said in regard to its compliance with the three epistemological stances underpinning wholehearted realism, once more it looks like Dibble's Law applies. Some do. Some don't. And it is more complicated than that. The problem is we can't tell how complicated it is because the issues are never raised for serious consideration within the discipline. Investigations are mounted and studies reported with almost no attention paid to the epistemological status of findings, outputs or products they deliver and a lack of derived justification for the formulations by which they are composed. The same is true of science of course but there two very important things hold. Science is a very successful discipline measured by the differences its results have made to our mode of life. Second, those trained in science are very well aware of the conditions under which its claims are to be held. *It is just they don't publicise them*. They are taken to be self-evident in the practise. EM has yet to demonstrate any serious contributions to the enriching of our modes of life, or indeed to declare that outcome as one of its objectives (*pace* Mel Pollner's aspiration for radical discombobulation which we have already mentioned). And, as far as we can tell, the run of the mill researcher is rarely if ever exposed to the conditions under which its summary positions can be deemed to be secured other, perhaps, than a foundational belief that EM encompasses a moral crusade to confront and overthrow 'constructive sociology'. Although engineering a revolution in Sociology might be a much to be desired ambition, the lack of any serious consideration of the grounding of its own prescriptions is unfortunate and, as we mentioned earlier, in some cases desperately so.

But all is not lost. There are other approaches to the epistemology of realism which might serve EM. In the next section, we examine the one which we think looks the most promising..

4. Rhetoric, Analogies and Keyed Descriptions

Elgin elects to give up the 'truth requirement' in wholehearted realism and hangs on to the 'literal description requirement'. Others do precisely the reverse. For them, it is not the logical (truth conditional) status which is crucial but the use to which descriptions are put. The forms are chosen with rhetorical ends in mind. They address some questions but not others. Hence they are to be considered at best approximate or selective. It this characteristic which implies they must, in principle at any rate, be contestable. Assessment of them turns on questions such as: Is some of what has been set aside in the format chosen just as significant as what has been included? It is what this question implies and how it might be answered which defines non-literalist positions.

For Roman Frigg (2005), the above question throws up issues which can be grouped into three bundles which are clearly analogous to those we started with.

a. Ontological questions: just what are the descriptions in science?
b. Representational questions: how does the description represent the target it is a description of?
c. Stylistic questions: how in selecting among forms of representation do we fix types of relationship?

Both the two main battalions marching under the 'non-literalist' banner, those who see descriptions as 'analogies' and those who see them as 'narratives', relax the isomorphic or correspondence conception of 1:1 mapping. They also share the same two concerns; what makes it possible to learn from descriptions? and how do we control for misrepresentation? It is the answers they give which are different.

Mary Morgan's (2012) historical account of the development of modelling in Economics provides a simple and so intuitively attractive initial answer to the first question. Models are organised descriptions constructed according to a given format and refer to a given subject matter. The format might be material miniaturisation, graphical visualisation, mathematical formalism or whatever. The subject matter might be planetary motion, predator-prey dependency or macro-economic flows of value in the form of income from goods and services. Such representations are the 'working objects' of the discipline which uses them, objects which are manipulated according to the format specific rules of miniaturisation, visualisation, arithmetic etc. and the constraints imposed by the properties of the subject matter. Energy flows, for example, can approach the speed of light, flows of goods can't, so the temporal parameters applying to them must be different. In the end, scientific models and descriptions are crafted 'small worlds' and the lack of realism they display is a consequence of the trade-offs which enable a requisite level of workable 'miniaturisation' through simplification, abstraction and idealisation.

The examples Morgan describes cover verbal descriptions, pictures and graphs, working bits of engineered machinery and mathematics. The main thrust of her account centres on the tugging and pulling, shaping and shaving necessary to get a model to do what it is supposed to do. She is much less concerned with is Frigg's second question. How does a model stand for that which it models? Here, following Black and Hesse (Black 1964, Hesse 1966), the usual answer is that models are analogies. In Hesse's account, this decomposes into a statement about *analogical relationships*. Models propose analogical relationships. These run in two dimensions: horizontal symmetry relations and vertical causal relations.

Take, for example, the model of the solar system hanging from the ceiling of the Planet Pavilion at Jodrell Bank. Push the button on a nearby plinth and for a minute or two the model is activated. The globes representing the planets move in their orbits around the sun whilst each revolves on its own axis. Their moons, if they have them, revolve in like manner around the individual planets. The arrangement of globes and their rotations broadly represents the members of the solar system in their order from the sun and their relative velocity. The analogical relationships might be set out like this:

Solar System	**Model**
Objects held in position by gravity and centripetal forces	Objects held in position by rods

Object distances on astronomic scales	Object distances on centimetre scales
Objects made of rock and gas	Objects made of plastic and metal
Dynamism derived from energy release with the creation of the sun	Dynamism derived from electric motor

Table 1-1

The table brings out the symmetries and asymmetries between the model and the planetary system. It is clear the design of the model attempts 'to capture' only a few of the properties of the solar system.

The analogical relationship between the Jodrell Bank model and the solar system seems to work reasonably well as a means of enabling relatively uninformed visitors to understand the various motions involved in the planetary orbits, the sequential ordering of the planets from the sun as well as giving some feeling for the relative distances involved. It is not clear it would allow you to predict precisely when the next total eclipse of the sun in the UK might be nor when we will need to adjust the internet 'clock time' used in modern computers to take account of eccentricities is the earth's orbit around the sun. But since it was not designed solve those problems, this is not surprising. For what it is supposed to do it works pretty well. The symmetry relations are robust.

The same is true for the formal symmetries between the graphical and algebraic proofs of Pythagoras' theorem[17] or, since both can be described by the same mathematical equation, a swinging pendulum and an oscillating electric circuit. Where analogies seem to fray as descriptions is when the relationships depend less on symmetry and more on (claimed) similarity. Gilboa et al (2014) talk of this in terms of a distinction between first and second order comparisons. In a first order comparison, we are looking for symmetry: the ordering of the planets in the model and the ordering in the solar system. With second order comparison we get similarity or metaphoric comparison. In the contemporary world of UK universities, just such a comparison sees the student – university relationship in terms of a customer – retailer one, with universities conceived as rather like large department stores. Leaving aside the crassness of the image, what is going on is a trade-off over what might be called local versions of Type I and Type II errors. The trade-offs are generality v accuracy or idealisation v realism. The 'buyer-seller' market relationship is a general one and is being applied to an idealisation of some aspects of university "business". For the defenders of analogical modelling what are yielded are insightful inaccuracies where the costs of inaccuracy are outweighed by the benefits of insightfulness.

Jay Oldenbaugh (2017) has used one version of what we called "fraying" to raise doubts about how the cost/benefit balance is sometimes being struck. Take any formal mathematical model, say Paul Samuelson's classic model of Keynes' theory of macro-economics.

a. $Y_t = g_t + C_t + I_t$

17 Friedman and Rittberg (2019) would also claim it holds for the use of folded paper to demonstrate many other proofs.

b. $C_t = \alpha(Y_{t-1})$

c. $I_t = \beta(C_t - C_{t-1})$

For any time frame, National Income is the aggregate of Government expenditure, consumption expenditure and private investment. Consumption is a function of National Income in the previous period. Investment is a function of the difference between consumption in this period and the last period. Oldenbaugh's worry goes like this. Government expenditure, consumption expenditure and investment are all the exchange of money or tokens for money for physical goods, provided services, contracts of ownership rights and the like. They have physical realisations of various sorts and properties associated with those realisations. For instance, goods are lodged in certain places, are bought at certain times, have mass and so on. The mathematical object which C_t refers to has none of these things. What therefore is the similarity relationship between the referent for C_t and the goods and services bought and sold? Clearly, this worry does not apply to the Jodrell Bank model. The usual response is to say mathematical models are *interpreted systems* but that only moves the problem back. On just what relationship is the interpretation based and how do we know it does not actually harbour a *misinterpretation*?

It is this conundrum which motivates the proposal scientific models should be treated as "stories" or, in Roman Frigg's (2010) phrase, forms of "make believe". This view does not reduce to a claim science is all fairy tales or an ideology deliberately reinforcing a particular dominant group's interests. The concern, rather, is with how scientific practice is narrated and the forms of narration which are available to it. Literary forms — even science fiction — are not replete with manifestly untruthful or potentially untruthful statements. Rather, genres depend on the adoption of an attitude, the suspension of certain orders of disbelief and the engagement with the world-as-presented on the basis of that suspension. On this view Samuelson's model works as a narrative because it lives in the imagination. It is not close kin to the matter of fact anthropological field report or the journalist's summary of a politician's speech.[18]

What the descriptions as narratives viewpoint draws attention to are the different elements involved in fixing the relationship between a description and its target. First, there has to be some straightforward depiction of the state of affairs to be modelled. Call this the model description. This description has to be translated into a modelled description which Frigg calls a "p-description" (i.e. a set of propositions). The p-description is set beside our elaborated scientific understanding or theory of the target which is being modelled, the t-system. Tying the t-system to the p-description is a t-representation which shows how the constructed data model derived from the p-description maps onto the t-system. When all these elements are in place and work well, we are able to interrogate the description with regard to:

1. Whether two or more alternative descriptions are identical;
2. Which are the defining properties of the target and how they might be understood;
3. How we should draw comparisons between the description and the target;
4. What is true and what is false in the description as a description.

[18] Just in case you wondered, we do know on occasion the latter are less than matter of fact and we have sat through more than our share of anthropologists' romances.

In all of this, the t-representation is crucial since it provides how we will deduce what is true in the model and translate that across to the t-system. What the t-representation offers is a mapping protocol based upon a set of keys for translating the facts about the description into claims about the target. The dependence of translation on keys secures the representation.

> Maps, graphs, architectural plans, diagrams, photographs, (certain kinds of) paintings and drawings, and of course scientific models, are all t-representations .... but they work in very different ways. The differences between them are that these conditions are realised in very different ways: different keys are used and denotation has different sources. (Frigg 2010 p 26)

This idea should not be an unfamiliar one for EM. Roy Turner once remarked it is the formulation of the key or legend of a plan or map which tells us not to interpret the walls of houses as being 10 metres thick or roads as being hundreds of metres wide. In a similar way, Frigg & Nguyen (2019) illustrate the role of keys by referring to the example of (some types of) car wing mirror. The mirror offers an image of a certain space. However, the surface of the mirror is convex and so distorts the spatial relationships. Objects appear to be further away than they are. As a result, many wing mirrors display a warning to this effect. The warning is the key that ties the image seen to the actual distances at which objects are to be found behind the car.

In scientific descriptive modelling two keys pre-dominate: the identity and ideal limit keys. Surprisingly, although scientists often talk as if the relationship between their models and their targets is identity, very few actually work this way. As we have seen, many are idealisations and the majority of these seek to push 'to the limit' one or a small number of properties contained in the description.

Under the descriptions as narratives view, all the emphasis is thrown on the construction and management of the set-up conditions for the use of the description since the closer we can make the conditions under which we use or 'test' the description to those specified for the target system, the narrower we can expect the gap to be between the conditions we derive from the constituents of the description and the way the target system behaves. The key here is a measurement system which allows us to assess the fit of the behaviour; the approximation of the results we get from our use of the description to the equivalent 'results' we might derive from the world.[19] The scale of difference is the goodness of fit or degree of play. The key to the mapping secures the answer to the third of Frigg's questions.

As we move further and further from the operational methodologies and metaphysics of the natural sciences, the harder it is to satisfy the constraints on the use of 'analogies' as genres of narrative. As a result, comparisons slide into metaphor and beyond. Phillips and Newlyn's famous hydraulic model of the economy clearly was a material analogy. However, it is not clear the computer model of the mind is much more than an elaborated metaphor. The same might be said for the model of strategy formation instantiated in contemporary Game Theory and, perhaps more critically, the use of the mathematics of the conservation laws in Physics to model utility theory in macroeconomics. What is at issue as we move away from the analogy of genres of narrative is the loss of control over the shaping of the similarity relationships. The weakest form of similarity is simile itself. We use similes widely in daily life when we refer to politicians behaving like pantomime characters and the encounters in the

[19] This resonates with the familiar claim that the best model of the world is the world.

House of Commons being more akin to a circus than a rational debate. Of course, what we are not doing here is using the comparison for theoretical or investigative ends. Seeing political party competition as similar to competition for retail locations in urban centres or group formation to be the result of something like interpersonal magnetic attraction are comparisons drawn to pick out particular conceptions (regression to the mean distance across the political landscape, polar attraction and repulsion of individuals conceived as culturally charged particles) and are theoretically motivated. The trouble is we are now in danger of indulging in what Sorokin (1956) called "speech defects", the use of terms which have a perfectly good usage in one domain in an entirely distorted way in another.[20]

5. Realism, Narratives and Descriptions.

The stalking horse for this Part has been a 'for sake of the argument' proposition: one reasonable strategy for EM might be to seek to follow the epistemology of the natural sciences. However, on closer inspection we found while wholehearted realism appears to be the ostensible epistemological component of the natural and special sciences, it would be hard to assert EM aspires to conformity with the central stances required. Certainly, it is unclear whether EM either does or should aspire to predictive generalisation or ampliative-abductive method in its reasoning. To infer such strategies are or ought to be in place is unlikely to be met positively anyway given EM's historical opposition to 'positivism'. Luckily, though, this does not leave us at an impasse. Wholehearted realism, one could argue (certainly if we accept Catherine Elgin's strictures), is on shakier ground than might otherwise appear. It seems science is replete with felicitous falsehoods. Instead of following Elgin and seeking an entirely new basis for scientific methodology in some form of virtue ethics, we preferred a more 'sociological' solution; one which looked to the intentionality, or 'rhetorical purpose' (as we called it) of the descriptions being given. This was Frigg's notion of scientific description as 'narratives' organised by selected 'keys'. We illustrated this line of thinking somewhat coarsely by looking first at models in the natural and social sciences as structured 'analogies' where the elements of models are highly organised and specified keyed descriptions. Rather than the relationship between the description and its 'target' being a 1:1 correspondence or approximation, what is provided is comparison based on constrained isomorphism. Since they are highly structured and often formal, models were a good example on which to hang the general analysis of the role of keyed descriptions. However, we have to admit in Sociology, at least, most descriptions are not models per se and many are not analogically organised but use somewhat weaker similarity tropes.

At this point the problem we faced with 'veritism' returns. EM has been almost entirely silent on the structural premises of its descriptive strategies (apart from the aforementioned abiding determination to avoid 'constructivism' if at all possible). Search the literature for a summary of EM's methodology in the sense we are using the term and you are likely to come up short. Short, but not entirely empty. As with so many other important but under discussed aspects of EM, there are passages in Garfinkel's early and late writings which offer hints, clues and signs for the directions we might want to go with some of the things we are fretting over.

[20] One of the terms which Sorokin railed against was "valence" when used as a descriptor of individual attitudes. In his opinion, what is perfectly acceptable use in chemistry is wholly distorted when applied to individuals' group related relationships.

Take *Parsons' Primer* (Garfinkel 2019), for example. Apart from explicating the reasoning structure holding Parsons' General Theory of Action together, Garfinkel spends a fair bit of time examining the demands of what he calls "adequate description". This he represents in a recurring motif.

KØ -> T

Very clearly this motif casts 'seeing sociologically' as a transformation, a rendering, of knowledge gained within the flux of social life. Sociological investigation is itself social action. It requires engagement with a culture or way of life through some form of participation in ongoing courses of action, be they active participation as a member or quasi-member, formal data collection, unobtrusive methods or whatever else. What these techniques yield are bodies of acquired social knowledge which are then transformed by some chosen technical apparatus Ø to yield theorised descriptions. In general terms, this is a view which is consonant with Frigg's.

Within different modes of Sociology, the constituents of Ø are quite distinct and mark what phenomenologists call alternative "turnings". In paradigm cases such as Parsons' theorising, they are directed to demonstrating the *rational* basis of social action, albeit with rationalisations of entirely different forms. The idea of sociologies as different forms of rationalised rendering is also very close to the position found in Felix Kaufman's *Methodology of the Social Sciences* (Kaufman 1958). For Garfinkel, the choices represented by these different forms are selections over investigative metaphysics and epistemology. The first is articulated in the principal of unconstrained doubt. The investigator is free to suspend any common sense belief or scientific conviction concerning how the world appears. This tactic is what Garfinkel later christened the method of "conceptual play" (Garfinkel 1956).[21] The second involves principled adherence to the general tenets of disciplinary procedure. The latter requires conformity to appropriate sets of postulates for the analysis of social action embodying rules of relevance, adequacy, subjective interpretation, rationality, empiricism and action. The bundles of metaphysical choices and associated disciplinary rules are what define the array of distinct sociological attitudes.

Parsons' Primer reveals a great deal about how Parsons chose to render the body of sociological research of his time using an epistemology of analytic realism and the conceptual structures of the emerging field of system science to construct an embryonic but nonetheless elaborated cybernetic model.[22] It shows how Parsons strove to adhere to the disciplinary rules he had found in the writings of Durkheim, Weber, Marx, Marshall etc. Unfortunately, it throws no light at all on the principles and rules EM should adopt nor the modes of description it should deploy. However, some passages in *Ethnomethodology's Program* (Garfinkel 2002) do, but not necessarily as clearly or exhaustively as we might like.

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[21] The importance of the connotations of this term cannot be overestimated for understanding EM's methodology. They range from the theatrical construction of a finite province of meaning through everyday activities constructed under a norm of make-believe to serially organised highly co-ordinated moves in games. Each connotation provides a repertoire of elements forming 'plays'.

[22] It is important not to read *Parsons' Primer* or Parsons himself from the vantage point of today but to see them both as products of their time. Parsons was trying to pull ideas together from systems thinking in the Biological Sciences and Engineering and apply them to sociological phenomena as revealed in sociological research. This is why the notions of functional relationships, feed forward and backward and homeostasis are central. He was not anticipating modern Information Theory nor offering a weak-kneed, ill formed version of it.

The chapters we have in mind are Chapter 3 *Rendering Theorems* and Chapter 5 *Ethnomethodology's Policies and Methods*. Both are written as what might be called EM's now default *chiaroscuro* depiction of its positions. Each topic is introduced in terms of what it is not and, in particular, what feature, properties or style of conventional Sociology it does not accept. The running comparison is with 'Formal Analysis' and the parts EM breaks with. Alas, there is only an outline of how EM actually does the breaking. In terms of Garfinkel's own basic principles set out in *Parsons' Primer*, these chapters are about the elements of Formal Analysis which EM chooses to vary and the vocabulary and other representations which signal that free variation. The core is the pairing of a stipulation attributed to Formal Analysis ("There is no order in the plenum") with its counter point in EM ("Orderlinesses *in* the plenum are interchangeably orderlinesses *as* the plenum" (p.137- 8)). We are then given descriptions of features of EM investigations framed as assertions they are not like parallel features in Formal Analysis.

This is particularly so when Garfinkel takes up the issue of descriptive precision in Chapter 5. He starts where he finds Formal Analysis and EM agree. Both accept descriptive precision requires one of their overriding concerns for adequacy has to be the probativeness of social analysis. By probativeness is meant that an issue can get settled. Straightforward cases of probativeness are found in the natural sciences, in Physics, for example.

> There is unanimous agreement in both disciplines, classic studies of ordinary activities and EM studies, and in the social sciences, that wherever issues of adequacy are of concern, the issue of descriptive precision is primeordial, and unavoidably so. (p. 172)

Given our interests, this looks promising, especially when Garfinkel goes on to say it only looks like EM and Formal Analysis agree but really they don't. Unfortunately we are not then given an account of how the adequate descriptions of both enterprises differ but a repetition of the metaphysical free variation EM works on Formal Analysis' stipulation. EM might well describe *different things* adequately, but what makes such descriptions adequate and probative? That we are not told. Precisely the same thing happens when classical and natural accountability are compared. What we get is explication of the distinctive EM 'move' of misreading philosophers as providing topics for EM analysis together with an underscoring of the importance of framing the 'populations' of studies as self-generating and self-organising structures of action (for example, queues, traffic waves, bench experiments or mathematical proofs). What we don't get is how to distinguish 'adequate' from 'inadequate' descriptions using those techniques.

EM propaedeutics are strong on the basic metaphysical choices underlying its alternative way of seeing sociologically. We can see what choices are made and how they result in the 're-specifications' so often referred to. What they are not strong on is delineating EM's criteria for descriptive adequacy, how those criteria should be followed (as opposed to what phenomena should be adequately described) and what constitutes probativeness in the accumulation of the resulting findings. The EM position on these central methodological issues is barely articulated.

When we parted company with Elgin it looked like felicitous falsehoods might be one way EM could save its epistemological bacon. The natural and special sciences are rife with them, so we should not worry too much if EM employs them as well. Unfortunately, to be sustainable as analytic narratives, felicitous falsehoods must come with the required t-representation, the key to the mapping of the description onto the target. If they do, then the approximate truthfulness (the relative distortion in the mirror or the scaling for the layout of the map) can be understood and managed. If they don't, Frigg's questions c and d cannot be answered.

This is where the challenges lie in trying to draw an appropriate epistemology for TPP from EM. Its descriptions don't come with the equivalent of standardised t-representations. We don't have established, widely known and used sets of keys for mapping the descriptions given onto the domains which are studied. There are few if any protocols for establishing scope and format. As a result, how can we be sure our TPP descriptions of social activities are not actually the 'true enough' descriptions of Elgin's felicitous falsehoods and Frigg's narratives but instead somewhat loose metaphors and even looser fitting similes? Do we know what the defined modes of assessment for determining goodness of fit of EM descriptions are?

It is clear one objective of Garfinkel's *Program* was to instil more methodological discipline within EM. However, impassioned though his strictures are, many seem to be reiterations and re-workings of just part of what we need. As a consequence, it seems the only way to make progress in TPP is to 'go it alone' and adopt the EM mode of free variation in order to define as robust an investigative metaphysics as we can from which we can then derive the t-representations and the correlated modes of assessment we require for the provision of robust and systematic TPP description. That is the task for Parts Two and Three.

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PART TWO

INSIDE THE WORKFLOW OF THE SELF EVIDENT

At the end of Part One, we said our task now is to develop a robust and systematic investigative metaphysics to act as the t-representations for TPP and to derive a basis for the assessment of their goodness of fit.[23] To do this we will use the following strategy.

1. First we will sketch TPP's fundamental problematic and scope its associated disciplinary metaphysics. We locate this in an outlook which centres on the 'sense assembly' of action. Hopefully, the first part of the traverse will be over relatively familiar ground.
2. We will then develop an investigative metaphysics for TPP by:
    a. transposing the phenomenological conception of 'sense assembly' from Gestalt Social Psychology into a sociological description of objects and relationships defined for a socially organised production process.
    b. drawing out an EM interpretation of the work of Tadeusz Kotarbinsky to explicate how the Garfinkellian conception of 'praxeology' can be applied to sense assembly.
    c. Having brought these two bundles of ideas together, we will turn to Henri Bergson for resources to re-construe the workflow of sense assembly as a fluid rather than punctuated dynamic. This will allow TPP to capture otherwise excluded or distorted elements of the interior configuration the experience of action.[24]
3. In Part Three, we will look at TPP's own practice to suggest how the position we have sketched out might be implemented in investigations.

1. THE LIVED WORK OF 'SENSE ASSEMBLING'

> Whatever we may think about what it is to be an ordinary person in the world, an initial shift is not to think of an 'ordinary person' as some person, but as somebody having as their job, as their constant preoccupation, doing

[23] A quick reminder. An investigative metaphysics is a series of choices about objects and their relationships which an investigator stipulates as the basis for the descriptions being provided.

[24] Turning to experience in this way marks where epistemology and metaphysics come together.

> 'being ordinary.' It's not that somebody ***is*** ordinary, it's perhaps that that's what their business is. And it takes work, as any other business does. (Harvey Sacks.1995, p. 216 emphasis in original)

Possibly the most prominent 'discovery' in the body of findings EM has built up over the last 50 years is just how humdrum even the most esoteric ways of life are. In case after case, we find, yes, momentous things do sometimes happen at the laboratory bench, in the court room, on patrol, in the seminar room, control room or treatment room, but these extraordinary events are embedded in the flow of ordinary daily doings. In other less exotic settings, out of the ordinary things happen too. But here again they are part of the flow of the routine. In many ways, this has to be reassuring since demonstrating the quotidian character of everyone's life could be said to be an existence proof of Sacks' version of EM's central claim.[25] In the lived daily world, whether it be astrophysics, surgery or keeping in touch with friends, social order is construed as being produced and reproduced through the routine deployment of members' methods for constructing the normality of social life.

In another sense, though, this re-assurance requires some qualification. Husserl's uncovering of the deep dependence of scientific and other 'specialist' finite provinces of meaning on that of the life world was the key assumption Schutz placed at the heart of his social philosophy and which, therefore, became absorbed into what Alec McHoul (1998) calls EM's "field assumptions". For Husserl, what Schutz called finite provinces of meaning, are "world beliefs", structures of assumptions through which reality is constituted. The "attitude" associated with any "world belief" is the body of commitments implied by those beliefs. Moreover, every such attitude is distinct from but idiosyncratically interpenetrated by the Natural Attitude of the life world. For those engaged in them, the subsequent meld across the specialist attitudes and the Natural Attitude is what gives daily life within each finite province its recognisable character as routine 'astrophysics', 'brain surgery', 'executive management' or 'family organisation'.

To get preliminary sense of the analytic issues emerging from this stance, consider the following taken from *The Formal Structures of Practical Action.*

> However extensive or explicit what a speaker says may be, it does not, by its extensiveness or its explicitness pose a task of deciding the correspondence between what he says and what he means that is resolved by citing his talk verbatim. Instead, his talk itself, in that it becomes a part of the selfsame occasion of interaction, becomes another contingency of that interaction. (Garfinkel & Sacks 1970, p344-5).

Attached to the final sentence is the following intriguing footnote.

> The developmental sense of ***becomes*** is intended; not its sense of a development in the past that is now finished. To emphasise "process" the sentence might be read as follows: "Instead, his talk itself, in that it is becoming a part of the selfsame occasion of interaction is in becoming

[25] For some, though, this constitutes the discipline's central weakness as the studies seem to amount to no more than a heterogeneous accumulation of existence proofs,

> another contingency of that interaction". Similar remarks might be made about "another". (ibid fn. 15, emphasis in original)

Although the footnote refers only to conversation, it points to a more general phenomenon, namely the experiential unfurling of the ramified contingencies of action and the challenges thereby posed to the continued maintenance of mutual intelligibility as the basis of social order. Talk is but one context in which this unfurling occurs. The collaborative ways in which such ramification is managed are how the commitments of a particular "attitude" are realised and enacted. They are the work of 'doing' normal science, therapy, surgery or daily life. In the paper just quoted, Garfinkel and Sacks call this collaborative work "sense assembly", a phrase which is a widely used but rarely explicated term of art in EM. It has its origins in Husserl's analysis of the constitution of *sinn* (sense) as distinct from *bedeutung* (meaning) in his discussion of perception and experience and how that distinction is developed in his theory of the intentionality of consciousness.[26] For EM, such collaborative constitution is how the potential problematic nature of mutual intelligibility is overcome.

In their understandable anxiety to get on with undertaking studies and laying out findings, EM researchers often scoot past this lineage and thus leave obscure, take for granted (or, even worse, don't recognise) key elements in the assumptions they have adopted. As we said, we will draw out these elements and explicate "sense assembly" by conjoining it to a hitherto rather underused concept, Aron Gurswitsch's (1966, 1979) notion of 'field of consciousness' as a synthesis of Husserl's phenomenology of perception together with some of the insights motivating Kotarbinsky's (1965) praxeology. Subsequently, we will complement this innovation with the addition of some of Henri Bergson's phenomenologically-motivated notions. One not very imaginative label for the resulting hybrid might be "the workflow of attitudinal sense assembling". However, given TPP's focus on the apodictic, we prefer to call it "the workflow of the self-evident" and propose TPP's investigative task is the description of that workflow's interior configuration. This, we assert, is the elusive "animal" (to use Garfinkel's image) which we ourselves have been trying to extract from the "foliage" of those forms of daily life we have studied.

2. The Grounds of Experience

The Search for Certitude[27]

In one very important way, Husserl was an Enlightenment philosopher. Although he was frequently deeply critical of thinkers such as Descartes, Kant, Hume, Spinoza and Leibnitz, the vector he took through the philosophical problems he addressed was aligned with theirs. Husserl was convinced modern science and philosophy had lost their sense of intellectual community as *scientia*; the community bound by the search for certain knowledge of the world and our place in it. Within that community, the role of the natural and human sciences is to understand the structure of the physical and human worlds as objective realities; the role of philosophy is to excavate the grounds on which

[26] For those who like these sorts of morsels, Husserl frequently corresponded with Frege whose own wrestling with the distinction between *sinn* and *bedeutung* had enormous impact in a very different field of Philosophy.

[27] This is Lazek Kolokowski's title for his introduction to Husserl's thinking (1975).

that understanding can be made *certain*. Its role is as the integrating discipline which binds the others together.

Husserl's main life's work was to reconstitute the grounds of *scientia* by developing his philosophical approach, Phenomenology, as a *rigorous science*, not in the sense of empirical investigation but as a partner in the community of *scientia*. For him, Philosophy — and hence his own work — is a deeply serious personal matter.[28] Philosophy is a 'calling' in the Weberian sense, not a task nor a skill nor a facility with a repertoire of clever argumentational tricks. Each philosopher has to undertake their own journey of philosophical discovery; their own understanding of the relationship between mind and the world. Just as someone cannot become a mathematician simply by rote learning the steps in a suite of mathematical proofs but has to grasp for themselves how and why the proving proves what it proves, so the philosopher has to grasp for themselves how and why a conceptual distinction is secure and necessary, how and why an argument can be sustained and how and why certain conclusions follow from it. *They* must be convinced and not simply re-assured or accepting because others are so convinced.

We said, just like the classical philosophers, Husserl saw philosophy as concerned with the relationship between reflective thinking (mind) and objective reality (world). Ostensibly this should be the domain of the science of Psychology. However, with the 'splintering' (as he put it) of *scientia*, Psychology had lost its roots in rigorous philosophy and had become entranced by the methods of the natural sciences. What was needed was a complete rebuilding of philosophical Psychology. This was what Phenomenology was to be. To undertake the rebuilding, Husserl adapts a fundamental principle of Enlightenment Philosophy. Certitude regarding the relationship of mind and world can only be obtained by systematic reflection; i.e. the Cartesian principle of 'turning inwards' to reflect on reflective thinking or cognition about the world. This does not mean he fails to recognise other more practical modes of engagement with and understanding of the world, but he does not treat them as *philosophically* fundamental. The aim of this turning inwards is to reflect upon the concepts through which we constitute the world when perceiving, reflecting on, judging between and discovering things in science as well as in our daily lives. Rather than taking a chain saw to our practical reasoning, Husserl adopts a process of carefully paring away taken for granted presuppositions and differences which he finds not to be indubitable until he arrives at the grounding of absolutely certain, apodictic concepts.

The method Husserl uses is not the Cartesian one of axiomatic deduction; the seizing on a single premise and the deduction of the world (including the existence of God) from it. It is not a matter of logical construction, but of reflective revealing and reduction. Starting with the conscious individual reflecting from within the 'world' on how that 'world' appears to them, step by step everything pertaining to that world is set aside or 'bracketed' until all that is left is the "essence", the indubitable basis of reflection or cognition. This process Husserl called "the *époche*". What is given up are all our presuppositions about the reality of the world, the existence of other individuals, the security of logic as a method of truth discovery (which is why the method cannot be a deductive one) and ultimately everything about ourselves. These things are given up so that their constitution and hence recovery

[28] At the very end of his life, he became convinced the need for this work was a deeply serious cultural matter too. See (Husserl 1970)

can be revealed.[29] To borrow Weber's phrase, the journey to philosophical certitude necessarily must be a solitary path of inner loneliness.

For Husserl, ultimately the philosopher following this path confronts the need to pass from describing the essence of particular and occasioned cognitions (*cogitationes*) from within the point of view of a particular Ego immersed in its own stream of conscious to the description of the essence of cognition in general. In this transition, the analysis adopts the point of view of a transcendental Ego; an 'ideal' Ego in the Platonic or mathematical sense. Here what is described is not the experience of an individual thinker, perceiver, judger but thinking, perceiving, judging *as such*. What is under scrutiny is the locationless, personless, homeless mind contemplating the world free of all but apodictic presuppositions.[30] Husserl developed and refined this method as he deployed it on concrete problems such as the basis of logical thought, inner time consciousness, the life world, the origin of the mathematised sciences and the one we will draw on, perception.

Paradoxically, it has been necessary to summarise Husserl's philosophic method in the detail we have simply because our interest is not philosophical. Since we are not seeking to follow Husserl on his journey from formal to transcendental Phenomenology but are only concerned with his detailed, deep and subtle, reflections on the constitution of reality in the stream of consciousness, we need to be fully aware of the status of those reflections. If we want to use some his conclusions and a few of his distinctions to frame empirical investigations of the practical constitution of social worlds, we cannot simply incorporate them as 'findings' all of a piece with extant findings provided by empirical investigation. Rather, to put them to sociological work we will have to recast them. Our recasting is an explication of the notion 'sense assembly' based on Husserl's descriptions.[31]

The Constitution of Experience

Husserl's analysis of consciousness or experience is difficult and complex. Moreover, it evolved throughout his working life. We cannot here — and neither do we have the need— to trace all the lines of exploration, re-workings and re-formulations. Instead, we will provide only a partial account of his thinking and draw the concepts we wish to deploy from that account.

Husserl starts from a hallowed distinction, that between the objective world as it is itself and our perceptions of that world; or, more concretely, the character of an object independent of our viewing of it and the object as we see it. In our ordinary life and in the sciences, our perceptions of an object and the object in itself are assumed to correspond. Each time we view an object from a different angle,

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[29] To repeat. Husserl starts from the acceptance of an objective, external world which is there *for* the sciences. He is asking how is our knowledge of that world possible not questioning that reality nor whether knowledge of it is possible.

[30] Part of the essence of experience and thus of the transcendental Ego itself is that it is intersubjective, a feature which underpins the core assumption at the heart of the unthematized intuition of the Natural Attitude, namely the others we relate to are subjective beings such as ourselves. Schutz fixed this as the corner stone of his phenomenological reconstruction of Weber's *Verstehende Soziologie*. The strategy he followed was to use Husserl's philosophical reflections to reconstitute ('re-specify' in EM-speak) the life world realisation of central elements of Weber's sociology such as 'rational actor', 'rational action' and 'subjective interpretation' in terms of the concepts and conclusions Husserl had arrived at on his way to Transcendental Phenomenology. See Schutz (1967a, 1967b)

[31] This point raises the profound question of why, at least at this point in its development, EM should prefer description to explanation and what that means for its relationship to other sociologies. Answering this question would require yet another discussion at least as long as this one.

we presume we see the same object. Each time, we come back to that object, we presume it is the same object. When others view the object we are viewing, we presume they see the same object. These assumptions make up the constancy hypothesis.[32] The correspondence we presuppose is the basis for objective knowledge and hence of *scientia*. The philosophical challenge is to provide for the possibility of that correspondence rather than assume it. For Husserl, any rigorous description of this relationship must be based on an egological account of the thinking, perception or experience which are constitutive of Being.[33] It must, then, be based in the analysis of a reflective conscious subject.

From his own reflections using the époche, Husserl offers three foundational observations:

1. Experience is Brentano intentional. Our experience is always experience *of* things, be they objects, thoughts, events, images or whatever.
2. This experience is 'owned', 'placed', 'contextual' or, to use his term, "occasional". It is always a subject's experience.

In combination, these two lead to the third.

3. Experience is a *three place* relation: x represents y to z, where 'represents' is defined by Husserl's concept of "apperception".

Take one of his favourite examples. We are walking in the garden. Around us are trees, bushes and flowers. Each individual tree, bush or flower is a real object existing in the world. The collection of objects within our view are the *noeses* of our experience. However, as we approach an individual tree, it presents itself to us as a unique profile against a background of other objects. This is the phenomenal character of the tree as we see it now, its *noema*. Turning away down another path, we have another view of the tree with another unique profile. Turning yet again, we have a third unique profile of the tree. Our experience of the tree is of a succession of *noemata*. Each is integrated into the flow of our experience of the tree as *this* tree through the constancy hypothesis. Only if we turn a fourth time and discover what we have been seeing is 'actually' a cleverly constructed facade should we be surprised and find the hypothesis violated. Husserl suggests the constancy hypothesis is secure as we walk around the tree because we are primed to see a symmetry of profiles. Such priming is a consequence of our beliefs, previous experience and accumulated knowledge. These 'primings' or expectations, Husserl calls "anticipations" or "possibilities". They are our projections onto and from what we are experiencing and form the "horizon" of that experience. They constitute our phenomenal experience but also reach beyond it. These horizons are, of course, open or indeterminate and hence the possibilities so constructed are "problematic". There are many aspects of the tree we can come to know as our experience unfolds. Coming closer for the first time, for instance, we may learn its leaf shape or the texture of the bark or the scent of its flowers.

For Husserl, the horizonal "reaching beyond" of experience is "motivational". That is, it is constituted by what we see now set in the context of our prior life experiences. Walking around the garden, the trees have never turned out to be facades, so we 'anticipate' this one this time will not be a facade

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32 Note. Husserl does not use the term 'constancy hypothesis', Aron Gurswitsch does. He takes it from Gestalt Psychology and uses it as the term for the root phenomenological problem of consciousness or experience; how does the melange of successive perceptions become organised into a structured field of consciousness? (See Gurwitsch 1966)

33 In what follows, we will group these modes together as 'experience'. In doing so, we do Husserl a disservice since much of the subtlety of his philosophy involves marking distinctions among them.

either. When we look down from above on a table, we 'know' it has legs and is not floating in the air. We know this because we have not (or only rarely, and then under special circumstances) found tables to be free floating in space. Apperception is Husserl's term for the way the *noem*a of phenomenal profiles reach beyond themselves to the horizon of anticipations or problematic possibilities we associate with them. Apperception is the constitution of our field of consciousness, our Gestalt, as the bundle of phenomenal experience and anticipations and that constitution is motivational.

> The intentional content of an experience that obtains in virtue of its motivated horizon of possibility obviously exceeds its phenomenal content, i.e. there is no corresponding phenomenal property for each of the motivated possibilities that constitute the horizon of the experience's explicit content. But the felt associative pull of motivation is a phenomenal property of experience.....(Walsh 2017, p.21)

Husserl, of course, comes to these findings through a process of abstraction and reflection. Thus he describes apperception as a series of successive, horizon-bounded profiles. As we examine first the table, then the table top and its legs, then the carpentry of the table and legs what we apperceive is successively ramifying horizon-bounded profiles. However, he is clear this is an *analytic* conception. In daily life, our experience seems to "hang together" (to use Walsh's phrase) as an integrated and blended flux and flow of *noeses* rather than a structured series of *noemata*. Moreover, the distinctive way it "hangs together" each time every time is itself "a phenomenal feature of experience" (Walsh 2017 p.23).

The key to this blending of the flux and flow of our experience is an implicit feature of our motivated horizons; a feature which contrasts with the explicit possibilities or anticipations tied to the phenomenal experience itself. We see smoke on a neighbouring hillside and assume it means a fire somewhere. The possibility of fire is explicit in the apperception of the smoke. Apperception is immersed in a temporal flow and points backwards and forwards in that flow. This temporality provides the possibilities of the apperception of smoke. Gurswitsch talks of explicit possibilities as the 'thematic field' associated with the 'theme' of consciousness — the focus of our attention. Associated implicit possibilities are the 'fringes' or 'margins' of the horizonal field which, at this point, are not relevant to our thematising but are nonetheless part of the perceptually correlated object and so, as our experience unfolds, may be drawn into the centre of the thematic field.

In *The Field of Consciousness*, Gurswitsch (2010) structures Husserl's analysis of apperception in terms of three foci: theme, thematic field and fringes or margins. The theme is a 'singling out' from the thematic field of some 'object of our consciousness' to which we attend. As we have seen, this is motivationally driven. Using a more (quasi?) formalised conception of a 'field' and relying on Husserl's notion of the 'attractive force' they have in drawing our awareness to them, Gurswitch proposes the distribution of possible themes in the field (that is, the other ways objects which might possibly be thematised) is a distribution of potentialities. In any thematisation, these potentials have a configuration. Each item can be described in terms of its 'positional index' in the thematic field or on the margin. Gurswitch thinks of this as a kind of gravitational field. As we attend to the theme, its relation to the thematic field shifts and the positional indices are reconfigured. We look at a picture and remember when we saw it last and the associations it has for us. When we turn to the objects placed beside it, the relationships among all the elements shift. The co-ordination of potentialities in the field (i.e. the relationships between thematic field and its margins) is determined by both Gestalt coherence (the overall integration of theme, thematic field and fringe) and its motivational relevance. Depending on how we look at Carravaggio's *The Crucifixion of St Peter*, the coherence of the figure/ground structuring of the scene may be fused with a consideration of the chemical composition of the tinctures by which the shadow effects work to construct the figure/ground we see.[34]

This organisation or structuring of the field of consciousness is its sense assembly.[35] The pointing forwards and backwards in the flow of experience constitutes the continuity of our experience as *our* experience. This is the third place in the three place relation described earlier. The kind of indeterminate multiplicity in the horizons of possibility we experience in the flux and flow of daily life is what Garfinkel and Sacks are pointing to when, in the quotation given earlier, they speak of talk as a contingent process of ramification. Just like talk, all our courses of action and those of the others with whom we relate are contingent ramifications 'occasioned' by the contexts in which we are engaged and so are understood phenomenologically in terms of their implicit and explicit possibilities.

We can now begin to frame at least part of our task. We have suggested Husserl's notion of explicit and implicit possibilities as summarised in Gurswitsch's field of consciousness provides a way of encapsulating attitudinal sense assembly as a metaphysics for the ramification of action from within the flow of that action. We now need to see how that metaphysics can be re-cast to frame sociological investigations.

3. Praxeologised Sense Assembly

The route from Garfinkel's encounter with Husserl in the social philosophy of Schutz and Gurwitsch to the formulation of EM as the shop floor work of social order via the investigation of motivated compliance, problematic possibilities framed by background expectancies and displays of accountability has been well documented. We want to pick out for special emphasis just one aspect of that journey, namely the thread associated with the notion of 'praxeology'. Although more visible and perhaps important in the latter stages, it has a continuous presence almost from the beginning. Appreciating how that presence morphed provides one way of understanding more fully how to continue the development of TPP.

At the Berkeley Conference for the Unity of Science in July 1954, Henry Hiz presented a paper on Kotarbinsky's (1954) approach to praxeology. It was published as Hiz (1954) in *Philosophy and Phenomenological Research*. This is likely to have been the first time anyone in the West (outside the confines of those interested in post-war Eastern European Philosophy) had encountered Kotarbinky's ideas. The same would not be true of the concept of 'praxeology' itself, of course. That term had some currency as a central construct in von Mises' (1998) development of a distinctive 'Austrian Economics' to challenge the predominant 'positivistic' paradigm in the then contemporary Economics. We don't know how closely Garfinkel was tracking the debates in Economics, but given his familiarity with the work of Felix Kaufmann and his closeness to the emigré community of Austrian philosophers, we can assume he had at least in some general understanding of the notion prior to Hiz's paper. Certainly, as

[34] For a fascinating initial account of the embodied character of what he calls the "fields of life", see Todes (2001)

[35] Our use of 'field' here is as an informal analogy. The possibility of a more rigorous, perhaps even formal, definition such as the one Gurwitsch was pointing to remains an open question.

Murray Rothbard notes (1997), von Mises regularly makes a point of distinguishing his own work from Kotarbinsky's.

We can start to see how the Husserlian notion of sense assembly might be brought together with praxeology if we look at Garfinkel's 1956 paper *Some Sociological Concepts and Methods for Psychiatrists* (Garfinkel 1956). For a start, he actually cites Hiz's paper. The reference comes in the discussion of how sociologists 'work' on a problem, and is part of the section on one way the discipline uses the strategy of "conceptual play". This usage involves an investigative ploy which he dubs "the praxeological rule". His characterisation of praxeology is directly drawn from Hiz.

> .....it consists of the search for similarities of successful methods in many different domains of activity. Praxeology seeks to formulate statements of method, and to extend their generality, seeking as wide a domain of applicability as possible. (Garfinkel 1956 p. 191).

Following Hiz's lead, he lists the theory of games and the investigation of scientific method as putative exemplars of social science praxeologies. Garfinkel's argument is that the use of the praxeological rule gives sociological investigations a distinctive cast. For Kotarbinsky, the technical values of some activity are its displayed degree of efficiency, effectiveness, simplicity and so on; values which are achieved through the ways the activity is organised and functions. Garfinkel suggests the same 'logic' is standardly applied in Sociology, where an analytical determination that some pattern of social arrangements exhibits properties such as class subordination, the accumulation of cultural capital, anomic social relations or a gendered division of labour amounts to the allocation of disciplinary praxeological technical values which can then be treated as the outcome or accomplishment of the arrangements and practices members of society adopt.[36] As a result

> Accounts by sociologists of the conditions under which a phenomenon occurs may be mapped point for point into the terms of the strategies that persons follow whereby, knowingly or not, they achieve the pay-off represented in the value of the variable under study. The praxeological rule states that any and all properties whatsoever of a social system that a sociologist might elect to study and account for are to be treated as technical values which the personnel of the system achieve by their actual modes of play. (ibid)

Of course, one could take this as a lightweight usage, an allusion to a passing academic fashion perhaps, or even as more a pun than a serious suggestion. Indeed, when stripped of its lingo, it might seem to be no more than a provocative platitude.[37]

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36 It is worth marking this early instance of a relationship which became important later on. As an accounting, the 'work' of doing Sociology is paired with the 'work' members of society do to achieve the technical values sociological analysis determines their activities have.

37 The provocation is to be found in the way the upshots of the analyses provided are formulated. For instance, Merton's analysis of anomie and social structure is rendered as the following maxim:

> If you wish to multiply the frequency in a society of frustration, psychosocial isolation, delinquency, alienation, compulsive conformity, and dissent then socialize the society's members so that there is uniform respect for ultimately

But this would be unfair and misleading. Elsewhere, in what has become a famous summary (at least in the EM community), Garfinkel seems to draw once again on praxeology as an element in the conceptual play constituting EM's metaphysics when he proposes that in Sociology….

> …(t)he seen but unnoticed backgrounds of everyday activities are made visible and described from a perspective in which persons live out the lives they do, have the children they do, think the thoughts, enter the relationships they do, all in order to permit the sociologist to solve his theoretical problems. (Garfinkel 1967, p 37).

It emerges more definitively in the *Purdue Symposium*, where, having been asked to provide the origins of the term 'Ethnomethodology', Garfinkel muses on its "vicissitudes".

> ….(Ethnomethodology) is the organizational study of a members' knowledge of his ordinary affairs, or his own organizational enterprises, where that knowledge is treated by us as part of the same setting that it makes orderable. Now. Let us say you want the term ethnomethodology to mean something. Dave Sudnow and I were thinking that one way to start this meeting would be to say, 'We've stopped using the term ethnomethodology. We are now going to call it "neopraxeology".' That would at least make it clear to whoever wants the term ethnomethodology, for whatever you want it for, go ahead and take it. (1974 p. 18)

Once again, of course, the terminology might be simply rhetorical, an attempt to head off a line of questioning presaging arguments of which Garfinkel had grown tired. And there is surely something to that. But we are still left with what the prefix 'neo' might be alluding to. Does it offer anything? To get a sense of what might be lurking behind the rhetoric, we need to go back to Kotarbinsky.

The "Economising" of Action

Kotarbinksy's goal is a science of efficient and hence, for him, effective action or what he calls "good work". Such action may be individual or collective and, if the latter, collaborative or antagonistic. The hope is the resulting investigations will uncover principles on which general recommendations for the organisation of activities might be formed. Whilst digging a ditch may be very different to formulating a mathematical proof and playing jazz in an ensemble may be very different to playing chess in a competition, Kotarbinsky is after the highest level general descriptions of what is common among these varied courses of action. As Krystyna Skurjat (2018) suggests, once he had these principles Kotarbinsky hoped we would have mechanisms through which to procure the positive conditions under which work can be organised. These conditions are arrangements such as the fostering of innovation, the disclosure of available knowledge, the verification of results, the repeatability and

> valued goals while you routinely and differentially deprive the members of the legitimate means of attaining them. (Garfinkel 1956 p. 192)

whilst social actors are presented as 'doing being' anomic, alienated, class oppressed, members of social categories and the rest. The platitude resides in a listing of standard sociological themes.

regularity of work and many more. Here is an excerpt from the list of principles Skurjat finds in Kotarbinsky's analysis.

> All complex actors, including all organizations, are essentially unstable entities. In order to prevent their destruction, one should:
> - take protective measures;
> - replace used elements with working ones;
> - care about making all components of the system more efficient;
> - adopt the appropriate hierarchy of importance elements of the complex;
> - eliminate unnecessary burdens;
> - coordinate activities of the components, that is, make them not impede the performance of tasks, but rather support one another;
> - coordinate tasks and goals;
> ....
> - together with the transformation of the formal structure of a company, re-define obligations and responsibilities of employees;
> - adjust employee responsibilities – in terms of categorization and specificity – to requirements of the dynamically changing market;
> - implement the principles of coordination as an activity of work teams that increases the efficiency of teams as a result of, among others, planning/placement, providing a substantial area of freedom of action and introducing a simple and clear motivation system. (Skurjat 2018 p. 126)

Nothing in this list is surprising and, of course, that is the point. Each of us knows how to make our activities effective. What Kotarbinsky wants to set out is the logic behind the whole range of effective work members of society undertake.

In doing this, he steps through issues such as causality, freedom, agenthood, unit (or single) acts, compound acts, collective acts, the selection and minimisation of 'interventions', the preparedness for action and the instrumentation of action. The abiding characteristic of these discussions is an insistence on the need to pay close attention to managing the specific details of the particular acts in train. His listings, then, have an advisory or pedagogic character. If one knew nothing of the mechanisms of effective action, Kotabinsky offers what might be thought of as the most general 'workshop manual' for 'doing good work'. Here is a snippet which conveys the tenor of his approach.

> It would be difficult to decide which is worse from the point of view of a job well done – wasting.......or being satisfied with easy solutions, ... It is only the narrow door of maximum and highly strenuous effort which constitutes an exit from a situation with only one way out. This, in my opinion, is the same door which opens on to the summits of creativeness and mastery. (Kotarbinsky 1965 p.113)

Having laid out his conceptual regime, Kotabinsky moves on to the 'politics' of action, the principles necessary for successful efficient collaboration. This is achieved when

> specified elements are in contact with other specified elements, transition from one element to another leads through other specified elements, the

> various parts are connected by rivets, belts, cables, contacts, etc. Every element has its place in the spatial order, every process contributing to the working of the machine as a whole has its place in the temporal order, and the various elements depend on others in the various specified ways. It is obvious that when building or maintaining a composite whole, we need not make it consist of the greatest possible number of most varied elements, connected as closely as possible by relationships as complicated and as stratified as possible. On the other hand, it is obvious that in all those respects we should go just so far as is necessary — how far varies from case to case. (ibid p. 136)

What collaboration generates is concentration of effort and its organising principle is the division of labour.

The complement to collaboration is 'struggle' and Kotarbinsky devotes a chapter to its organisation. Once again the concern is with the detailed structure required for its success.

> Thus, the issues of the technique of struggle intertwine in many ways with the issues of the technique of positive co-operation: problems of good work as applied to a team refer to both. This becomes quite clear when we realize that in a great many cases the parties to a struggle have not only certain incompatible objectives, but also certain objectives in common. (ibid p. 173).

The final chapter of *Praxeology* deals with techniques for the extension and improvement of coordinated action. He ends the chapter and the book on this note

> ....as improvements progress, the proportions of the importance of the various elements of that progress are constantly changing. In particular, it is the relative importance of knowledge which is increasing, and let it be borne in mind that knowledge is an essential element of dispositive possibility, of preparation and of its rationalization. And knowledge includes the self-knowledge of the acting man as such, and consequently the advances made in his realizing the nature of action as such, its elements and possible forms, its advantages and disadvantages from the point of view of efficiency conceived in the most general way, the conditions for approaching the maximum efficiency, and finally those factors which contribute to, or divert from, such maximum efficiency in individual and in collective life. Comprehension of the contributory factors enhances their working; comprehension of the diversionary factors diminishes their influence. (ibid p. 208)

Neo-Praxeology

Whatever might be thought of the earliest manifestations of EM, they surely look nothing like Kotarbinsky's praxeology (and certainly nothing like von Mises' either). Which, of course, prompts the question: In what sense can Garfinkel be proposing EM to be a 'neo-praxeology'? As is nearly always the case, the answer lies in the obvious. Garfinkel's standard sociological move is a proceduralising one. With the stratagem of "misreading" theorists, philosophers......anybody, Garfinkel takes a pronouncement, a proposition, a text or a schema and asks (a) how it could be transformed into a prescription for sociological investigations? and (b) having so transformed it, what are the implications, findings or outcomes likely to be?.

Applying the same move to Kotarbinksy yields not the infamous breaching experiments (Garfinkel 1967) but studies of the 'work' required to 'do' routine social action. Where Kotarbinksy sought to abstract upwards towards the most generalised precepts and principles, Garfinkel specifies downwards to the detail of actual cases. As he says in the *Psychiatrists Paper* discussed earlier, the idea of praxeology as a general approach seems obvious in the context of the familiar factory production setting. But if it is a general account of action, it should apply to each and every type of action social actors engage in. So, for any course of action, we should not look for the principles it has in common with all other forms of action but for the arrangement of all the local specifics which make it the distinctive social production process those engaged in it take to be. The arrangement of these local specifics ensures what is carried out on the shop floor of social life is "good work" *and recognisably so*. The accountability of the shop floor work of social life is the praxeological character of its performance. It is the praxeological character of the accountability of action which is 'sense assembled' in the organisation of the detail of the shop floor work of social Being.

The 'praxeologisation' of the 'sense assembly' of social order as "instructed action" referenced many times in *Ethnomethodology's Program* is simply one more step. Given the tightly structured serial organisation of acts making up a course of action (what might be thought of as their nose-to-tail character), each element or component in a course of action (2 or 3 part structures in a telephone call opening, stating price, proffering money, accepting money, etc in purchasing a product, writing and reading a set of financial accounts, reading and following a map and so on and so on) can be treated as a both the consequence of prior acts and the progenitor of subsequent ones. Each is to be treated as the collaborative achievement of both the performance of a proper next and the provision of of what should be a proper next to itself.

This much is familiar, so why throw in the notion of 'instruction'? We suggest it has to do with the coherence requirement. Given the in-the-course-of-the-action production of shared understanding, there appear to be just two alternatives for creating the internal coherence experience has. Either actors have to be tasked with trading descriptions (somehow) to provide 'accounts' of what they are doing as an integrated part of the performances of their 'turns' with such descriptions being recognised and understood (a line of thinking which just pushes the whole problem of understanding and collaboration back to shared expectations and normative compliance) or they must 'exchange' instruction and competent performance in a rolling serially organised way. The advantage the latter has is its termination of the regress on the action pairing. The conception of an 'instruction' and 'performance' exchange is the central move in the 'instructed action' conceptual play laid out in *Ethnomethodology's Program*.

The most straightforward way to make sense of all this is through the lens of Paul Grice's (1981) notion of "implicature". If Bob asks Wes whether they can meet on Wednesday and he says it's Evie's birthday, Bob infers from his response that he is planning things with his granddaughter and can't meet. This is what Bob take him to mean although his words are not directly a refusal. What Bob takes him to mean is the implicature he draw from Wes' response. Now, with instructed action, co-participants responding to some course of action of the other can be viewed as determining its meaning (i.e. its implicature) as an instruction to respond in a particular way in their own next course of action. In turn, the other determines the instructional implicature of the response and responds to that, and so on...... A central key in EM contemporary description, then, is consociate courses of action as coordinated structures of implicatures as instructed action.

Adopting this construction, we can begin to get traction on the vexed notion of 'praxeological validity' as an analytic categorisation of the relationship between any action and its next under the rubric of action-as-instructed-action. 'Recognisably competent production' of the next 'validates' the competent functional performance of the prior instructions. Praxeological validity, then, is a binding relationship between an act and its next. Remember, though, all acts are *both* consequence and cause in the flow of experienced action.

Praxeological validity is an EM technical value bestowed on the social binding of acts one to another; an investigator's designation of the tightness of the relationship between the first and second parts of the pairing 'instruction-enactment'. In other words, the determination of praxeological validity is an EM claim about the procedures and conditions in place for successfully enacting an "instructed action" (for whatever value of 'successful' is contextually germane at the time). What is recognised as an intelligible social form (a social fact, if you will) is the degree of binding of one to the other not, as is sometimes suggested (e.g. Rawls 2002), the material or social product (the constructed 'flat-packed' chair, the successful telephone opening, the comprehension of the accounts). Its use is the proceduralisation of Kotarbinsky's praxeology to build an investigative metaphysics.

The Interior Character of Action

The mode of EM offered in *Ethnomethodology's Program* offers a quite distinctive form of conceptual play which does however build directly on Garfinkel's previous work. Unfortunately, if handled clumsily, this way of thinking can result (and often does) in an analytic attitude which presents courses of action as strings of punctuated unit acts. Presenting the details of organisational ethnographies and other studies of work as the shop floor production of bio-science, physics, welfare provision, medicine, IT design, law, management etc. etc. etc. is a demonstrably fertile sociological move, one which forces attention on how the self-evident is constructed and arranged by participating actors. However, it can militate against preservation of the character of the 'work' as experienced. It was just this possibility to which Michael Moerman was objecting when he talked of…

> …(t)he clacking of "turns" over their "possible completion points"…..the neat scattering of "repair initiators" in their three-turn space……: dry bones of the talk with which roles, passions institutions and private strategies are embodied and lived. (Moerman, 1988, p ix)

It could also be what lay behind both the mathematicians' complaints about Livingston's work which Garfinkel comments on (Garfinkel 2002 p. 278) and, in a very different way, Garfinkel's own recurring insistence that action is autochthonous not automated. Without careful thought, there is a strong possibility the proceduralisation of Kotarbinsky's praxeology could drag with it an investigative metaphysics which does disservice to the interior configuration of the courses of action being considered. Often our social lives seem more the immersion (a telling term) in an ongoing and unfolding flow of fluxes and turbulences which comprise conjoining and separating streams than a piecemeal, piecewise, iterative succession of unitary 'acts'. It so happens we can provide for such experience by embedding a fluid or elastic dynamic within the investigative metaphysics of social order as 'instructed action'. However, to do that we need to return to the discipline's phenomenological roots, but in a rather unlooked for way.

4. THE PHENOMENOLOGY OF HENRI BERGSON

Bergson's Metaphilosophy

Bergson's thinking is recursive. He returned to the same themes again and again, each time formulating them differently. In this discussion, we cannot follow him through the back and forth and round and round. We will have to break the cycle somewhere. We will do so by starting with what might be thought of as his 'metaphilosophy'; his ideas about the character of (modern Western) Philosophy as a mode of thinking. There is a danger to this. It will look as if we think Bergson arrived at a settled view of the nature of philosophical practice first and then, based on that view, proposed an alternative. That is not what we think. However, to provide a reasonably brief and reasonably structured account of his views, we start with what seems to be the keystone and build the arch back from there.

Since Kant, the central problem of philosophy has been how to describe the relationships between thought, language and reality. As the articulation of our thoughts, how do our concepts hook onto the world (to use the familiar phrase)? Bergson's earliest writings took issue with Kant's formulation of what had become in the conventional view. Although Bergson was Kantian enough to accept part of the answer must be that our concepts shape our percepts and hence what we take the world (reality) to be, he was also mathematician enough (and we will come back to the importance of mathematics for his thought) to insist that language is symbolic. As representations, our concepts convey our thoughts more or less adequately *but always insufficiently*. There is an inevitable gap between that which is represented and its representation. For Bergson, we close the gap by articulating our thoughts in metaphor or imagery.[38] As we will see, since Bergson takes the essence of existence to be continuousness, endurance or, as he put it, "*le durée*" which, paradoxically, is itself a process of change, it follows that philosophy is not and cannot be static. Ineluctably, our philosophical concepts, the representations we give of reality, will be superseded by others.

> For Bergson, philosophy is not about discovering the right expression to represent reality, be that reality a process one or not: the absolute is not comprehended "simply by giving it a name". On the contrary, because logical essences themselves mutate, philosophy is about *creating* the right expression. (Mullarkey, 1999 p 185 emphasis in original)

The central metaphor he sees in the foundations of modern philosophy is solidification. Thoughts are conceived as if their form was like that of solid, discriminable objects. When we think about thoughts and the 'laws of thought' (that is, the rules for right or logical thinking) we think in much the same ways we think about unitary objects located in space. Take Russell's formulation of these laws:

1. The law of identity: 'Whatever is, is'
2. The law of contradiction: 'Nothing can both be and not be'

[38] There are two connections here. First, Bergson has his finger on what, from the sociological point of view, elsewhere we have called the praxeological gap between meaning and expression. Second, there is more than a little of Wittgenstein's " A picture us captive. And we could not get outside of it, for it lay in our language and language seemed to repeat it to us inexorably", though it is a different picture to the one Wittgenstein had in mind.

3. The law of excluded middle: 'Everything must either be or not be'

For Bergson, these laws have the form they do because we naturally think of 'being an existent' (the 'what is') on the lines of differentiated solid objects. What is, is and continues to be so. What is, therefore, cannot be something else. A thing cannot be two (singular) things since two things cannot be in the same place at the same time.

The carryover of the solidification and spatialisation of ordinary thought into philosophy is Bergson's the central theme. He sees its origins in the Eleatic philosophy of Parmenides, Zeno and others and its apotheosis in Mill's attempt to formulate the 'Principles of Method' for inductive (or analytic) thinking. In Mill's account, *propositions* are conceptual objects and conceived on the analogy of solids. Employing the McHoul term we used earlier, Bergson held spatialisation to be the central field assumption for modern philosophy. As a consequence, it is a mode of thinking which regiments the pluridimensionality and manifold textures of our experience.

Localisation and Spatialisation

Bergson accepts spatialisation and its correlate localisation are perfectly natural ways of thinking. In our ordinary lives, they provide a means by which we can grasp and express the realities of our experience. Think about numbering and counting things. When we count the apples in the bowl, the bowl-of-apples is the unit we attend to and the reality of the individual apples is given by their membership of the total in the bowl. We are indifferent to the apples themselves. Enumerating the apples, numbering them, applies a conception of numbers as pegs on a line or points in an infinitely extensible one dimensional space separated, in turn, by equally infinitely divisible spaces. Each number occupies a unique position and no two numbers can be in the same place. Even though the spaces between the pegs are infinitely divisible, the basic conception is of each point and sub-division of space as unique points.[39] In this way, our concept of number is a localized and spatialized one.

What common sense does unproblematically, Bergson thinks philosophical metaphysics does far more problematically. The result is to render the multidimensionality of our lived experience as an encounter with a series of configurations of limited low dimensional spaces, the concatenation of which can only give an inadequate representation of the whole. When we listen to Cesar Frank's Cello Sonata or sing a nursery rhyme with the grandchildren, we are, to use Heidegger's term, at one with the melody, immersed in the flow of the words and the tune. That is the reality of our lived experience. Of course, we can and do recognize an analytic reality of successive notes and their phrasing which make up the weft and warp of the music. But spatialised succession is not how we live through the music. The one is 'Being' as an essence, a single continuous whole; the other is 'Being' as localised, successive, objects.

To replace the metaphor of spatialised, singular objects, Bergson advocates a different conception of space, one he feels more adequately expresses experience. Experience is a multiplicity to be analogised (he never denies the need for analogies, it is just which analogies Philosophy uses he objects to) to the mathematical object of a manifold. A manifold is an n-dimensional surface which locally (i.e. at any

[39] As Milik Capek remarks in his paper *The Fiction of Instants* (1991) the philosophical difficulties in the notion of an infinitely divisible mathematical space have long been known. The most famous of them is the antinomy forming the basis of Zeno's notorious paradox of the arrow which is thereby generated. As a result, mathematicians treat mathematical points and spaces as fictions, albeit conveniently "felicitous" ones.

location) appears as a lower dimensional space. The usual illustration is our experience of the earth's surface. As we all know, the earth's surface is roughly a sphere. But at any local point, the surface appears to us to be an infinitely extending plane. We use the tools of planar geometry to lay tiles, plan journeys, draw maps and so on and yet we know if we set off in one direction and continue on that trajectory, we will (eventually) return to where we started. Of course, spheres are not the only manifolds. There are many other more bizarre topologies to which the earth, galaxies or the universe might have conformed. In the end, the whole of Bergson's philosophy reduces to a rejection of the limitations of Euclidean spatialisation and localisation as the basis for the metaphysics of our experience.

The Flow of Experience

As a contribution to debates in and between philosophies, Bergson's arguments might have some spectatorial interest for us, but they could seem a little removed from EM's (and Sociology's) more prosaic concerns with the tasks of proceduralisation and investigation. And yet these tasks require an investigative metaphysics; the definition of what objects we are to study and how they relate to one another. In previous sections, we have worked through EM's inheritance from Phenomenology and how this was shaped by use of Kotarbinsky's praxeology. Different ways of taking the intersection of these two circumscribe EM's analytic space and the objects which populate them (the shop floor production of ordinary talk, science, professional life and multiple other lived finite provinces of meaning). Using a modern idiom but with Bergsonian filters on our analytic lenses, one might say the default conceptualisation of relationships in these spaces is likely to be spatialisation and localisation simply because that is the modality we ordinarily adopt. It is what thinking under the Natural Attitude is at home with and which is reproduced in science and philosophy. The question for us, though, is simple. Does that conceptualisation capture all aspects of the interior configuration of lived experience? If the standard EM approaches are likely to lead to disjuncts between our analytic accounts and forms of lived experience of the kind Moerman objected to, how might those approaches be adapted to ameliorate this unhappy outcome? Two of the key features of our experience are temporal succession and intentional multiplicity.[40] They are also the two key features of the alternative investigative metaphysics we take from Bergson.

Le Durée

For Kant, space and time were the absolutely basic forms of our experience. Space is an infinitely extending, infinitely divisible manifold of appearances which we experience 'instant slice' by 'instant slice' as time extends forward into infinity. Bergson disputed this characterisation.[41] For him, the essence of being was *le durée* experienced as a manifold of permanent transience. What he has in mind by this term is a very similar to that of entropy in Mechanics. [42] The state of an object at any point can

[40] Just in case, once again "Intentionality" here means Brentano-intentionality.

[41] Kleinherenbrink (2014) makes the point that Bergson's departure from Kant's conception of time originates in his earliest work Time and Free Will (2001), where he disputes the necessity for Kant's positing of a distinction between phenomena and noumena in order to provide a 'world' in which human actors can escape causal determination and thus have free will.

[42] The nub of Bergson's (1965) famous debate with Einstein was simply (a) his attempt to show that Einstein's conception of space-time was derived from a broader multidimensional conception; and (b) Einstein's presumption that Bergson was seeking to replace 'measurable absolute time' as a presupposition for Physics. That Bergson totally mangled the twin clock thought experiment on which relativity theory was based didn't help. See Canales (2005).

be defined by its entropification, its asymmetric transition from a more orderly to less orderly organisation of energy. Bergson drops any sense of degeneration but holds on to the idea of continuous transformation. All forms of Being, that is all forms of reality, have their associated forms of continuous transformation or *durée*. It is just the form of continuance associated with clocks (and hence 'time') has come to predominate our Western metaphysics. It is this form, he claims, which defines moments or instants as spatialized mathematical points and thus decomposes continuous transformation into series of locally temporal units by means of which 'time' can be measured.

> Real duration is *experienced*: we learn that time unfolds and, moreover, we are unable to measure it without converting it into space and without assuming all we know of it to be unfolded. (Bergson 2002 p 216 emphasis in original)

Although *le durée* is a highly general concept, Bergson is most remembered for his application of it to what Husserl calls "inner time consciousness", the primeordial temporality of experience. His analysis is part of an extended argument he runs against the reduction of the mental to the biological. The lines of the argument should now be familiar. Having offered a description of hearing the chiming of a clock, Bergson says:

> We should……distinguish two forms of multiplicity, two very different ways of regarding duration, two aspects of conscious life. Below homogeneous duration, which is the extensive symbol of true duration, a close psychological analysis distinguishes a duration whose heterogeneous moments permeate one another; below the numerical multiplicity of conscious states, a qualitative multiplicity of conscious states, a qualitative multiplicity; below the self with well-defined states, a self in which *succeeding each other* means *melting into one another* and forming an organic whole. (ibid, p 72 emphasis in original).

The fluid dynamic at the heart of this conception of succession and sequence in experience is what we wish to take from Bergson and add to our investigative metaphysics for TPP.

Attention à La Vie

For Bergson, our primordial experience of *le durée* is one of immersion in the flux of a multiplicity of sensations, perceptions, stimuli and memories. When driving, cooking, reading or walking the dog we maintain an appropriate harmonic tension across this flux. Bergson calls the achieving of this tension '*attention à la vie*. The effort (or as EM might put it the "work") of *attention à la vie* is visible when a juggler spins a set of plates (this is a very familiar colloquial metaphor, of course) on upright rods. To keep all plates spinning at the necessary rate, each is attended to as needed. Pretty much the same idea can be found in discussions of the work production teams undertake in the management of resources, production machinery and product distribution to ensure smooth flows of product inputs and outputs. The configuration of harmonic tension appropriate for any occasion which makes each experience the unique organic whole Bergson refers to in the above quotation, is its praxeology.

Gurswitch describes this same harmonic tension across the flux of experience as the configuration of potentialities in the field of consciousness. What we can add to Gurswitch's description is Bergson's idea of the 'work' needed to maintain this tension. Using Newtonian mechanics as a very loose analogy, Gurswitch talks of a force field of potentialities which is reconfigured as our experience unfolds. What Bergson's notion of *attention a la vie* provides is the 'energy' needed to undertake the work of reconfiguring the forces and hence produce a new harmonic tension.

Looked at in this way, *attention a la vie* within the field of consciousness captures precisely what we have called "attitudinal sense assembling". On any occasion, the modality such sense assembly takes is shaped by the array of commitments or "attitude" associated with the particular meld of the relevant finite province of meaning and the life world of the Natural Attitude currently in place. As the meaning structures of ramifying courses of action unfold, that meld is continuously reproduced by reconfiguring explicit and implicit possibilities within the field of consciousness. We can use this conception to give a distinct form to TPP by requiring it to provide *compound descriptions* of emergent workflows attitudinally sense assembling a finite province of meaning *from within;* workflows whose praxeological organisation is visible to those who share sufficient of the competencies of the relevant 'production cohort'.

The Method of Intuition

Although Bergson never described himself as such, his whole philosophical practice was imbued with phenomenology.[43] Like Husserl, he saw the challenge for metaphysics, and thus for all Philosophy, as stepping beyond both reflective theories of the sciences and common sense to grasp the constitution of our experience, our acquaintance with things in themselves. This acquaintance is the foundational basis of Being and is what Bergson labelled "intuition". To access intuition analytically, we have to reverse the flow of our natural inclinations as well as go against the grain of conventional intellectualism. Rather than adopting either an external, 'objective' viewpoint or an internal, 'subjective' one, both of which are detached from experience, we should place ourselves within a flow of experience in order to grasp what is unique and therefore only expressible through concepts which capture its flux and fluidity as a continuous unity. Bergson concedes practising the method of intuition will be neither easy nor straightforward. In a fragmentary introduction to metaphysics, he warns….

> VI. But the truth is that our mind is able to follow the reverse procedure. It can be installed in the mobile reality, adopt its ceaselessly changing direction, in short, grasp it intuitively. But to do that, it must do itself violence, reverse the direction of the operation by which it ordinarily thinks, continually upsetting its categories, or rather, recasting them. In so doing, it will arrive at fluid concepts, capable of following reality in all its windings and of adopting the very movement of the inner life of things. (Bergson 2002 p 275)

The Method of Intuition offers a pointer but not a procedure for the investigative method TPP might adopt.

In EM analysis generally, reliance is placed on the investigator's vernacular competences as a fully or partially socialised member of the local setting under investigation. This sharing allows the investigator to tune into the flow of the interaction and thus constitute the configuration of horizons of explicit and implicit possibilities in place. The examples are well known. Shared commonality of conversational

43 On the other hand, Husserl *is* supposed to have described himself as Bergsonian (Winkler 2006)

competences has enabled the development of studies which capture attitudinal sense assembly through deployment of 'machineries' to manage the horizonal aspects of the sequential organisation action-in-talk as the co-production of structural features of conversational objects such as turns, stories, descriptions and so on as well as the methods of glossing so brilliantly brought out in the *Formal Structures* paper. In other more ethnographic or ethnographically informed studies, investigators are rarely fully 'at home' in the local culture being investigated. As a result, the descriptions which emerge are very often perspicuous but nonetheless 'outsider' accounts of the management of the horizons of the arrays of explicit possibilities visible when working at bench science, undertaking managerial decision making, conducting medical examinations, keeping the peace on the streets, organising responses to emergency calls and the like.

One line of studies has emerged, however, where the investigator has possessed the full panoply of competences possessed by members of the local culture. These studies provide access to an order of 'knowledge *de re*', or knowledge by acquaintance, of the relevant experience rather than 'knowledge *de dictu*' or knowledge by instruction. Examples are David Sudnow (1978) on the playing of jazz, Eric Livingston (1986) on advanced mathematical proving and Kenneth Liberman (2007) on Tibetan philosophy. These studies have been able to bring out orders of implicit and hence unremarked possibilities 'taken as given' and hence not available to non-members and only occasionally available to the partially socialised; those things which for competent members pass without comment but which investigators and others may not discern. These studies have been able to address the order of phenomena Garfinkel and Sacks (1970) had in mind when, in their discussion of talk. they stress speakers always mean more than they can say (Gurswich's 'possibilities'). This meaning is the 'what goes without saying', the 'implicit' rather than 'explicit' possibilities. Studies which excavate these potentialities by drawing upon 'the view from within the flow of experience' in presenting an analytic account of the relevant finite province of meaning are undertaking TPP.[44]

5. CRITERIA OF ADEQUACY : AN INITIAL SCOPING

The purpose of Part One was to frame an approach to description in which TPP, even if it adopted realism, was not forced into the straightjacket of veritism. The line we followed instead was to take analytic descriptions to be keyed narratives; a strategy we proposed should not be that strange for EM styles of investigation. Adopting this strategy throws descriptive weight on the clear identification of what Frigg terms 't-descriptions', the keys around which representations are formed. In this Part, we have developed a metaphysics for TPP which we believe provides the basis for such t-descriptions. What we want to do now is to set out some initial conditions or criteria of adequacy for analytic descriptions which have been keyed to those t-descriptions. The provision of such criteria is an important step on the journey towards ensuring TPP can display the probativeness Garfinkel felt was a common virtue of all 'scientific' or disciplinary analyses.

The framing of our criteria will be premised on the assumption TPP's central concern is with the explication of common sense reasoning as the unfolding of actual courses of action. Refracted through the particularities of the metaphysics we have constructed, this presumption shapes the distinctiveness of TPP's mode of EM. TPP's descriptions, therefore, are keyed representations of those actual courses

[44] As a method, TPP has many of the characteristics Garfinkel stipulated for "unique adequacy" (Garfinkel 2002). As we will see in Part Four, it offers one way of implementingg that somewhat opaque notion.

of reasoning formed under a particular EM mode of conceptual play. We take it as given TPP's criteria of descriptive adequacy must respect and preserve the naturally occurring, contingently unfolding character of such reasoning as this is formulated within its own exercise of **KØ -> T**.[45] We leave it for others to determine how far, once it has been suitably adapted, the form or content of this framing could be of service to other modes of EM analysis. At this point, we will present the framework only in the most general terms. In Part Three, we provide illustrations of how the methodology we are proposing might be implemented. At the end of Part Three, we will return to these criteria and provide more detailed specification using the illustrations we have given.

Descriptive Concatenation

Coherence and integrity are among the primary virtues of analytic descriptions. This much seems obvious. So, as personal or course of action social types, actors should not, for example, be attributed motivations, relevances, interests or knowledge they can't have or be presumed to have adopted sets of thematic foci which are mutually exclusive. Coherence and integrity require:

a) Topics taken as the fulcra of descriptions must be congruent with the set of keys defined for the account being given and must be entailed by the principles of Gestalt coherence in play.

b) Summary descriptions must be couched in terms of the organisation of category structures demonstrably relevant to the context in hand.

c) Assemblages of descriptive elements composing topics and summary descriptions must be isomorphic with oriented-to particulars of the reasoning in hand.

Structures of Relevant Detail

To be coherent, orders of relevant manifold detail must observe the thematic configuration of the reasoning being undertaken.

a) Matters of validity, contradiction, consistency, reasonableness and entailment are locally determined in-course-of-action properties of action.

b) Matters of modality (what actors "must", "should", "can only") be concerned with are provided in the specifiable detail of the setting or discoverable as matters of orientated to relevances derived from the contingent configuration of that detail.

Determinations of Meaning

The unfolding dynamic of actors' apperceptions is publicly visible in the serial organisation of their component acts. Actors do not wear their reasoning on their sleeve— i.e. provide symbolic external representations of 'internal' processes. Consociate reasoning is an intersubjective production.

a) What actors "see", "know", "infer", "feel", "think" is demonstrable only through the thematic coherence of oriented-to properties of the unfolding courses of reasoning in hand.

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[45] There is an implication in this. It follows TPP's descriptions should not be judged against criteria developed for other modes of EM let alone the criteria adopted by the formal or constructive descriptions of the rest of professional Sociology.

b) That thematic coherence is available only from the configuration of contingent detail being managed in the setting.

Patterns and Configurations

A course of action is a fluid kaleidoscopic reconfiguring of the setting's Gestalt under processes of evanescence and emergence.

a) Regions and margins are specifiable only in terms of continuously morphing moments in the co-produced workflow of the course of reasoning.

b) Parameters of description must be derived from the displayed oriented-to properties of continuity and ramified contingency available in the context.

6. ARE WE NEARLY THERE YET?

Almost. For now, let's just pick the bones out of the discussions in Parts One and Two to try to shape up a schematic of TPP's methodology.

1. We have adopted the non-literalist realism of Roman Frigg and others in which investigative descriptions are keyed narratives and have adapted the related forms of assessment as relative goodness of fit to the requirements of the mode of conceptual play motivating the form of that narrative.
2. We have taken the central idea of EM to be the 'occasional' accountability of the sense assembly of action and have described the 'praxeologisation' of that accountability as the workflow of sense assembly.
3. We have defined TPP's topic as the interior configuration of sense assembly and its 'occasional' accountability. That is, how action seen in this way is experienced from within.
4. We have characterised such experience as a manifold and proposed the integrity of that manifold can be preserved through the formation of compound not concatenated descriptions of the ramifying detail attended to.
5. We propose replacing the standard punctuated or percussive characterisation of the sequencing and succession of our experience of social action with a fluid and elastic conception.
6. We have drawn on Bergson's concept of the Method of Intuition to propose an analytic stance which places the investigator in the flow of action in order to capture the manifold fluidity of course of action as the sense assembly of social order.
7. Finally, we have offered a general set of guidelines for descriptive adequacy.

In Part Three we will demonstrate how this methodology might be operationalised.

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PART THREE

DEPTH CONTINGENCY AND THE WORKFLOW OF SOCIAL ACTION

In this Part, we apply the schema we have developed to two 'specimen studies'. The purpose is illustrate how the schema might be realised in the undertaking of actual investigations. Whilst the examples are 'specimens' and hence not fully worked out analyses, each illustrates the approach we are proposing. [46] Both analyses start from a common departure point, the presumption the activities under consideration are consociate in nature and the practices involved are socially structured. Both involve the management of institutionalised processes applied to cultural objects. The institutionalised processes are 'technical practices' and the objects are tools as well as related artefacts and materials. As cultural objects, the tools and artefacts etc. are Durkheimian in nature in so far as the general characteristics of what they are and how they are to be used is a given for those who know how to use them. In a previous discussion (Anderson and Sharrock 1993), we suggested such apodictic features of socially organised objects might be thought of as 'affordances' in some attenuated sense. They are the seen but unnoticed, taken-for-granted features of the context of their use. Their effectiveness in the workflow of action is one of the grounds of social order.

1. THE EMBODIED PRAXEOLOGY OF MILLING:

Ever since Heidegger's (1962) description of the 'being' of hammering, we have grown used to phenomenological descriptions stressing the corporeal as much as the cognitive. In our first illustration, we follow suit. The example provides an idealised and simplified description of a particular workflow in an actual production workshop.

> Jodie is milling tongues for edging to bookshelves. She is working at a bench-top router. Methodically, she picks up a length of profiled ash edging from the bench on her right hand side, passes it across the spinning router bit and places the milled piece on the bench to her left. She is wearing a mask, ear defenders and protective glasses. Her hair is tied back in a ponytail. Attached to the router table is a hose running to a box the size of a carry-on

[46] For those who wonder about these things. If asked, we would probably call the specimens instances of 'storographies' since they are part invention and part reflection on experience (we marginally prefer this term to 'narralyses'). We would have loved to have called them 'autoethnographies' if the thought of being associated with that brand didn't bring us out in hives. Pressed to justify their use, we would retort: if philosophers and physicists can lean on stories for far more than the pumping of intuitions (trolleyological assassination, hyperplane worlds with multiple times rates) we see no reason why sociologists shouldn't too.

> case. Every now and then, she knocks the router off and runs the piece she has just milled through a groove in a piece of board kept beside the finished rails. She is obviously concentrating, but it looks to be a procedure she has carried out many times before.

We will focus on just three aspects of Jodi's work: preparedness for action; the gestalt of milling; and the management of manifolds.

Preparedness for Action

Jodi's work takes place in an environment and on tasks which are pre-structured both for her and by her. This aspect of the management of the workflow of action is often under-emphasised. Take Douglas Harper's vignettes (Harper 1987) of the way Willie (his subject) goes about his work. Wonderful though Harper's descriptions are of the skill and precision of Willie's "mechanicking", what is missing is any discussion is the way the work task and the work site are arranged to enable the work to be performed in the ways it is. In a phrase we owe to our late colleague John Hughes and which calls to mind Kotabinsky's (1965) insights, we call these arrangements 'preparedness for action'. In Jodi's case, the pre-structuring falls into two broad domains. The first has to do with the designed character of the objects Jodi is managing, namely the router and its set up, the milling bit and the work site. In managing these, Jodi has to see, understand, engage with and modify features which are provided for her by the context. In the second, Jodi takes the contingent possibilities of these features and structures them for her intended use. The open possibilities of the field of consciousness are reduced by the choice of structured arrangements for the workflow.

Laying out the site and knowing the lore of the machine[47]

First there are the given features of the site and the machine. Key among the former is the collocation of machine and other resources, in this case storage space. Then there is the 'busyness' of the space. Routing is a potentially dangerous activity and so throughways, clutter and other such potential hazards will have to be controlled or managed. The lore of the machine is what any woodworker knows about routers in general and what Jodi knows about 'this machine' in particular. The spindle on a router generally turns anti-clockwise at speeds upwards of 16000 to 20000 revs per minute. Because the workpiece is moved by hand across the cutter, the cutting area cannot be completely enclosed. The speed and the openness of the cutter mean the router is an effective but loud and possibly dangerous tool. On a bench or table top router, the user has to control the rate at which the workpiece moves across the table. This means their hands are in close contact with the machine. The 'lore' of any bench router, then, always has these two principles:

1. Milling means flying debris and dust so ensure you are prepared and protected. Hence the importance of the mask, glasses, and exhaust system. Also ensure your hearing is protected during use.
2. The router cannot have an enclosing protective fence so the user must work in close proximity to the cutter. This means you must ensure clothes and hair cannot be entangled in the spindle as it turns.

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[47] An obvious paraphrase of Melinda Baccus' (1986) term.

The above items which might be thought of as general H&S considerations, are supplemented by a third.

3. The most efficient (and safe) cut is against the direction of the cutter. This means a right to left pass of the revolving bit. It follows the right to left flow of the pieces being worked in the most efficient process to use.

Understanding the setup of 'this machine'

The above are general principles. To them are added what is known about this particular machine and its current state as well as the interrelationships between these features and properties of the task which have been already determined by prior decisions about the product. The main considerations here are the idiosyncrasies and limitations of the machine, the particular cutter bit being used and the material being cut. The material as well as the length and depth of cut will have been fixed by the design. Jodi's task is to produce this design on this material with this machine in this setting.

Again, some considerations will be well known. For instance, unlike pine ash does not 'break out' easily so the finish is usually clean and smooth. All the edge pieces will have been 'thicknessed' to a standard tolerance. Inevitable variation around that tolerance means the tongue cutter needs precise alignment and even so may leave a thin 'wafer' when the edge is passed across. Other matters are more particular. How stable is the machine at high speed (does it shake slightly or rock?)? Does the fence along which the piece slides tend to move if not really tightly secured? What is the depth of the cut and sharpness of the cutter and how many passes will be required for each piece? These are features of using the machine Jodi will be oriented to because of her experience on it and because of the way she has set it up for this use when she selected the height of the cutter, the speed of the motor and placed feathered bench-dogs on the table to prevent kickback. Her test cuts on this setup will have prepared her for what she will need to pay attention to during the production run.

Jodi knows how to cut tongues on rails. She knows how to do it on this machine. She has done it before and has prepared both herself and the machine to do it again. But, while this is 'cutting tongues on this machine again', each time is different in its details. And it is in managing the detail of the differences as well as utilising the general principles of the similarities that Jodi ensures a successful operation *this time*.

The Gestalt of the Milling

The field of consciousness in the flow of the cutting of the tongue is the continuous kaleidoscopic reconfiguring of *attention a la vie*. Primary in this reconfiguration are zones of attentiveness and their margins. Whilst making the cut, the stacks of uncut and cut rails on the benches each side are on the margins. They shift to the centre as one cut finishes and preparation for another begins. But, even if the spinning cutter retreats from the centre of attention at these junctures, it still remains prominent in the "awareness context" of the run. Only when switched off during the test-checks on the cut, does it too retreat to the margin.

During a cut, Jodi tunes into the machine. Changes in the level and type of noise made tell her a lot about how much work the motor is doing and how good a cut is likely to be. Listening to the machine, she can tell if she is pushing the piece too hard against the cutter or too fast across it. She can also hear any 'chatter' or 'flutter' the interaction of the cutter and the workpiece might create, both of which indicate less than optimal cutting. Tuning is also physical. She can feel the vibrations of the motor and the cutter. She can feel if the workpiece is nestled against the fence or tending to lift as the piece approaches the bit or as the cut ends move off the table thereby causing misalignment of the tongue. She can sense if the cut is getting marginally deeper because of the pressure of the workpiece against it has caused the fence to move slightly backwards. She can see the cut and the nature of the flakes, chips and dust created. Tuning into the machine is not attending to these features one at a time in the same way one might balance radiators in a central heating system. It is immersion in a sensory flux and flow, where all are attended to at once and marginal adjustments made to each to manage the dynamic tension across them all as the cut proceeds.

A key feature of the maintenance of this harmonic tension across the zones of attentiveness is relative calibration of 'what is happening' against 'normal working' and 'natural troubles'. Both are present as aspects of the phenomenal field. Continuous assessment of the 'fit' of any cut to the parameters of these two classes of normality is a permanent feature of the craft being enacted. Any routine procedure is likely to produce random differences across instances. The question is whether these differences are small enough to ignore (chips on the tongue itself will be hidden in the groove), marginal enough to be remedied later in production (slightly deeper tongues can be trimmed) or such that the utility of the tongue is in doubt (significant deflection of the line of the tongue). In the latter two cases, although they are assessed "in the flow of the milling", the purpose of the test of the tongue against a standard template is to confirm or reject the assessments. Regular testing is also protection against unnoticed drift in the cut. However, if Jodi feels a particular cut looks, feels or sounds 'different enough' it will be tested and its acceptability assessed.

Managing the Manifolds of Milling

The focus of the previous sub-section was the milling a single edge. We noted the zonal organisation of the attended-to-workspace with its reconfiguring foci, horizons and margins. Absorption in the task is not, as is often depicted, a matter of 'tunnel vision', as if there was a scoping down of the attentional gaze to a smaller space within a continuous geometry. With absorption the geometry changes, not in a Cartesian mapping sense from 3 to 2 dimensions but in a kinaesthetic sense of intentionally located objects. There is no sense of moving from cuboid to planar spaces, for example, but to aural, tactile and visible spaces defined by attentionally-given intentionality. The sound of the motor is not above, behind, beside, the chatter of the cutter. The vibration of the machine is not spatially related to the tremor of the wood being cut.

The same reconfiguration of experiential geometry occurs within the temporal manifold, a reconfiguration which is directly consonant with that of the spatial. The attended to parameters are optimal speed of cut as a conjunction of cutter speed, milling produced, strain on the motor and character of the chips produced rather than an elapsed clock-time-defined rate. In the midst of the milling, the same order of reconfiguration takes place with the temporal parameters of the job, i.e. the milling of the batch. Progress is not 'measured' in hours and minutes but in terms of the job's harmonic tension in the traction of the flow of "fat moments" (Garfinkel 2019) in the milling of each piece and the transitions through progression markers such as 'not started', 'part done, 'half done' to 'almost done and 'completed'.

The central theme of the above description is the extensiveness and structure of the detail which Jodi is oriented to and managing. What is really a very straight forward, not to say basic, woodworking task is replete with deep contingencies. Drawing out the self-evident structure of this detail and the methods provided for managing it is what TTP is about.

So, what, in broad brush terms, are the analytic features we want to point to in Jodi's milling? In sum, they can be summarised as an orientation to the depth contingency of her work .

1. The achievement of the job, both each individual tongue milled and the whole batch, is an orchestrated management of the flow of contingencies arising from the work in hand and its prepared structure. While each edge and each batch is brought off 'on the fly' as a unique action, nonetheless this enactment has an organisation, its workflow, which has been designed and prepared for.
2. When immersed in the milling, Jodi engages with a dynamically layered and continuously reconfiguring tectonic structure of actualities to be attended to and managed. This management maintains the harmonic tension across the fluid succession of critical detail.

In managing the dynamic tension of the field of consciousness in the Gestalt of the milling, Jodi treats her working with the behaviour the machine, its cutting tool and the materials under her hands as an iterative sequence of instructed action. To maintain the flow of work in an orderly manner, she treats the behaviour of the machine and the materials she is working with as meaningful in the sense that such behaviour is implicative for the successful performance of her task. Her construal of these implicatures determines appropriate responses.

Consociation, contingency management, construal of implicatures and determination of appropriate responses are all displayed in our second illustration, albeit in a very different way.

2. THE PRAXEOLOGY OF ANALYTIC REASONING

One prevalent belief (call it another "myth", if you prefer) about the practise of science asserts it is hypothesis driven. Whilst this might be so in many domains, for some, especially those in the applied special sciences, interrogating the world (i.e. data) to test a well formed proposition is not necessarily how things go.[48] Instead the challenge is to try to work out from the data the world provides just what is happening and then figure out an appropriate description of what is going on which might or might not be formulated (in retrospect) as a well formed proposition to be tested. The task is to get to the description *and then see* what can be done from there.

The case we now examine follows an instance of this kind of reasoning. In some ways it involves a return to the issues worked through in the study (Sharrock and Anderson 2011) which set us off on TPP. On the other hand, it has much in common with the analyses we presented in Anderson and Sharrock (2018).

> Fran has been undertaking long term monitoring of a wild flower meadow. The meadow was cleared and re-seeded 20 years ago and Fran has accumulated 17 years of data on the developing diversity of the flora. Each year, the meadow has been surveyed by counting the abundance of species in 30 quadrats selected randomly across the site. Over the monitoring period upwards of 60 species have been identified and their abundance measured.

[48] Clearly, they might not go that way in many other scientific domains as well. It just seems they do from the reconstructed logics of the presented analyses.

> The base data set is large-ish (17 x 30 x 60 = 30600 data points), so simply eyeballing the distributions and handcrafting some summaries would be both inadequate and extremely demanding. Talking with professional colleagues, it has been suggested she use computational tools written for biological and botanical analysis contained in a 'package' called `vegan` available on the open-source R platform. Fran has some experience with R but has not used the `vegan` package before.
>
> The objective of the analysis is quite straightforward. Fran's botanical background tells her the meadow will have undergone a three stage development process. The first stage, colonisation, sees an invasive flush of early stage plants, often annuals. Over time, these 'colonisers' are replaced by 'settlers' which are mostly perennials. The settlement period is succeeded by maturation in which the diversity measured by the abundance and number of species stabilises. What Fran wants to know is:
>
> 1. Where are the inflexion points between the stages the meadow has passed through?
> 2. What is the pattern of stabilisation and what is affecting it?
>
> These guiding questions shape how she goes about the analysis.

This, then, is Fran's task, but what is ours? We think it is to identify and trace the patterns of practical consociate reasoning Fran follows as she steps through a two-fold 'double-fitting' exercise. We will do so by treating that reasoning as a workflow of instructed action. Engaged with the 'resources' to hand, she adjusts the data and tailors the analytic tools so her material can be analysed successfully and allow her to fit her interpretation of the revealed patterns to the constraints of standard botanical science. The tools have been designed to do some things and not others and to do them in some ways and not others. Since the tools are new to her, she has to uncover and work with their design features. The science is a set of institutionalised (i.e. normative) practices to which she must broadly comply. She can't just bend the expected protocols to suit her data neither can she make things up to suit the protocols. In addition, the data are being analysed for the first time and so, not surprisingly, working them up into an acceptable account turns out to be far from straightforward. The hinge on which our analysis will turn is the interior configuration of her reasoning as courses of instructed action as she manages the consociate character of both the tools and her chosen analytic method and closes the praxeological gap between the recalcitrance of the data and the requirements of the science. Both the tools and her method are intentional (i.e. cultural) objects. To do what she wants to do, Fran has to work out how they can be wrangled into docility. In our description, bringing off this wrangling is the workflow of instructed action.

BOTANICAL BACKGROUND

To understand the rationale behind the way Fran sets about doing the analysis, we need to lay out a little of the basic botanical science.

1. A central assumption for studies of plant diversity is that species abundance is controlled by environmental factors such as rainfall, temperature, soil acidity, soil depth and so on. Each species is presumed to have a preference distribution for any of these factors. For example, some plants like acid soils; others do not. Whatever the preference, it is also assumed, absent any other factors, individual plants will arrange themselves in a unimodal (i.e. normal) distribution around the species' preference point. This is the species standard profile and its statistical 'normality' is key to the choices of analytic methods used.

2. Sites are presumed to vary with regard to the factors. Variation can be both within and between sites. Some sites are sheltered, some are shady, some have variable soil depth etc. These variations effect the profiles the species display. The actual diversity displayed by a site is an interplay of the local effects on the species profiles.
3. It is assumed some factors are more important than others in controlling variation. Temperature, rainfall and soil acidity are the leading ones but levels of pollution, light and soil fertility can also be important. The analytic logic is to work back from the diversity presented to the identification of the controlling factors. Since Fran has been monitoring a single site, some of the relevant factors are (relatively) fixed. Others, particularly the ones presumed to be most important, e.g. temperature and rainfall, vary annually. Fran's aim is to get to a point where she can ask which factors are controlling the diversity of her meadow. To do that, she wants to run an ordination analysis on the abundance data for each year, first in an unconstrained way to see what pattern emerges and then by constraining the analysis with climate data to see how well individual climate factors 'explain' the pattern.

In general terms, the steps in Fran's workflow look to be clear.

1. Pour the abundance data into `R` to enable it to be analysed.
2. Churn the data through the `vegan` `R` tools to generate patterns
3. Interpret over the patterns to provide the botanical account.

It turns out each of these steps is a welter of ramifying contingencies she has to manage.

PREPAREDNESS FOR ACTION

Here are three examples of the set-up tasks Fran has to complete just get to the point where she could begin the analysis. Each differently shapes the ramifying labyrinths of detail she struggles with.

Readying the environment

`R` is an open-source development platform which is regularly updated. It is designed for relatively experienced users and does not provide automatic updates to installed systems. Providers of packages of tools take it on themselves (or not) to bring their tools into line with the latest version and users have to ensure the compatibility between the packages they want to use and the version of `R` they have installed. In addition, most packages are not 'stand-alone'. They depend on others to work. When you install a package, these other dependencies are (helpfully) installed too. A user's repository of packages can very quickly become large, often containing many packages about which the user has no idea what they do or what other packages they might relate to.

The problem this poses is as follows. When `R` is updated, the updated system only comes with the (revised) required system packages. Other, so called "user installed", packages are left behind. The user has to transfer them themselves. Luckily, Fran had been told there is a package, `installr`, which will undertake the task. Unfortunately, when Fran ran `installr`, it wrapped up her old packages for transfer but failed to install them and (not so helpfully) also did not tell her where it had located the wrapped up packages.

Fran has used 'Stack Exchange' in the past. It has a pretty lively `R` forum, so she searched there for possible accounts of what might have happened and how to fix it. One post suggested `installr` might have dumped the data in an obscure file called `'Rtmp***'` somewhere in her APPdata directory on

C. After a tedious search, she found it. But how to install them all? Again, Stack Exchange proved useful. After numerous queries and searches she found a script she felt confident enough to modify in the hope it would work. She ran it and it did. Finding the file and the script involved extensive rummaging using automatic searches and eyeballing posts and lists accompanied by lead-following on Stack Exchange and in her APPdata folder, checking titles, names, topics, possibly similar terms and possible answers against what she thought she needed.

To rectify the `installr` fail over, Fran engaged in a strategy of complex thematic searching, with the focal terms for the search being reconfigured as the search proceeded. The search space of '`installr` problems', for example, produced lists of issues some of which looked like her problem but many which didn't. Sifting through the former, chasing down leads, comparing terms with her problem involved managing a manifold of considerations relative to the question: "Does this describe my problem?". Calibrating the answers shifted the open search terms around. The same was true for finding the wrapped up packages. Using the Windows automatic search found a collection of files in response to the term '`Rtmp***`'. But which was the one she wanted? She solved that problem by checking the directory file stamps and trying to remember which was the last set of packages she had installed, so she could look for them. In both cases, her sense of how 'close' or how 'far' she was from the 'answer' stretched and shrank as it seemed she was homing quickly or slowly on a possible result or thrashing about seemingly getting nowhere.

Conditioning the data

Fran had two tasks here. The raw data had to be cleaned and then shaped up to get it into `R` in a usable form. The data set for each year was kept on separate Excel spreadsheets. Skimming through them, Fran noticed two obvious glitches. First, one year's data was for species frequency (i.e. numbers of species per quadrat) not abundance (plants per species per quadrat). It was the second year of surveying, so there was no way to retrodict abundances by, say, averaging over 5 years each side. That year's data had to be dropped. Second, the sample frames changed over the survey period as more and more species were added during the colonisation to maturation period. Fran decided on a two-step approach. She would freeze the research frame using the species set for the last year (2016). This would mean a large species list with lots of zeros in the early years for species which were later colonisers and vice versa for the later years. This data would be used simply as a phasing diagnostic. The whole data set would then be filtered using a threshold abundance value and partitioned at the 'maturing' inflexion point. Only the data from the inflexion point onwards would be subjected to detailed analysis. In the event, only 4 years' data was partitioned as 'colonising'.

Porting the data to `R` involved several handcrafted steps in Excel. The `.xsl` files mirrored the paper field sheets with species as rows and quadrats as columns. These had to be transposed and the year column removed. Each sheet was then ported to `R` and the blank cells filled with zeros using a base R routine called `naTounknown`. This process was a matter of trial and error since the routine failed on columns which were empty. Eventually, Fran discovered adding a zero to the first cell of these columns worked. This meant going back to the `.xls` files and amending them by hand.

Cleaning the data, then, involved re-balancing the praxeological values of the protocol being followed. First, the marginal depreciation in the 'representativeness' of the data was traded for 'efficacy' of the analysis. Without this trade-off the first and second year's data would have had to be excised altogether and the run begun in year 3. Then there was the re-focusing of the project on the maturing and after stages and the filtering of the data to manage the likely impacts of large numbers of empty species columns on the statistical results since the proportion of species in the data frame present at any point is one of the factors used to assess diversity. Lots of nil return species would deflate or skew the results. Here the values traded different forms of representativeness; 'global completeness' and 'underlying reality' being balanced to allow the central thematic of serviceable data to be retained. These decisions were 'practical analytic' ones necessary to enable the Botany to be done. Of course, all these issues were just the kind of "normal, natural troubles" any investigator is likely to encounter in any project. Work arounds were invented on the fly with an eye to keeping as close to the standard protocol as possible whilst not derailing the original intent.

Facility with the tools

Although Fran had some experience with `R`, `vegan` was completely new to her. To find her way around in it, she first looked through two basic documents, the '`R` Package Summary' on the CRAN repository and the 'backgrounder' written by the package's author Jari Oksanen.[49] The first is an alphabetical listing of function descriptions together with some examples of their use. The second is a step by step guide to the use of `vegan` for multi variate analysis. Fran decided to work through some of the examples in Oksanen's guide in order to get a feel for the package and how it might work. To do this she used the R markdown facility to save her workings. Here are three of the exercises she ran through.[50]

Exercise 1 Running functions and Plotting Results

```
library(MASS)

data(varespec)

vare.dis <- vegdist(varespec)

vare.mds0 <-isoMDS(vare.dis)

stressplot(vare.mds0, vare.dis)
```

Exhibit III - 1

49 vegan: Community Ecology Package: Authors: Jari Oksanen, F. Guillaume Blanchet, Michael Friendly, Roeland Kindt, Pierre Legendre, Dan McGlinn, Peter R. Minchin, R. B. O'Hara, Gavin L. Simpson, Peter Solymos, M. Henry H. Stevens, Eduard Szoecs, Helene Wagner. https://cran.r-project.org, https://github.com/vegandevs/vegan

50 Note. All the code snippets cited in this section are missing their line numbering. This is a quirk of copy and paste in R Studio running on Windows.

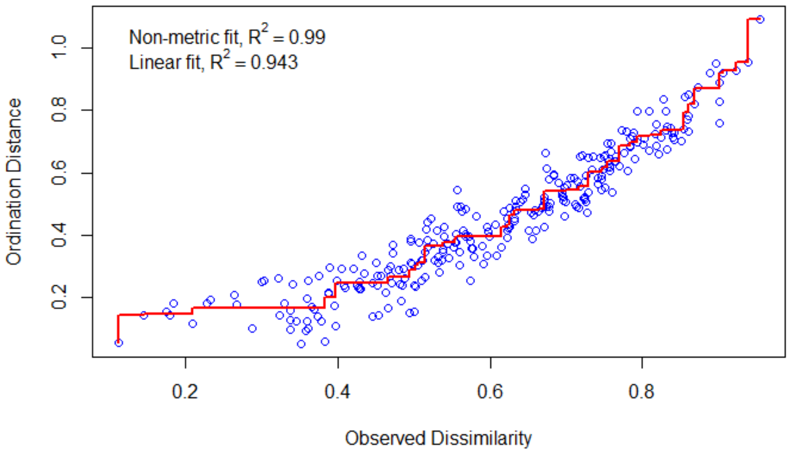

Figure III-1

The list of species `varespec` is turned into a matrix of dissimilarity distances `vare.dis`. This is then ordinated using multidimensional scaling to create the object `vare.mds0`. The two results are plotted against one another

Exercise 2 Reshaping Data

```
dis <- vegdist(decostand(varespec, "hell"), "euclidean")
```

Exhibit III - 2

`Varespec` is standardised to resolve some data anomalies using the 'Hellinger' method. This is then used as the basis of a dissimilarity matrix. Both operations are contained in one function call in `vegan`

Exercise 3 Principal Components Analysis

```
vare.pca <- rda(varespec)

vare.pca

plot(vare.pca)
```

Exhibit III - 3

```
Call: rda(X = varespec)

                Inertia Rank
Total              1826
Unconstrained      1826   23
Inertia is variance

Eigenvalues for unconstrained axes:
  PC1    PC2    PC3    PC4    PC5    PC6    PC7    PC8
983.0 464.3 132.3  73.9  48.4  37.0  25.7  19.7
(Showing 8 of 23 unconstrained eigenvalues)
```

Exhibit III-4

A principal components analysis is run on `varespecc` and the created object, `vare.pca` called. The resulting distribution is then plotted. The plot is below.

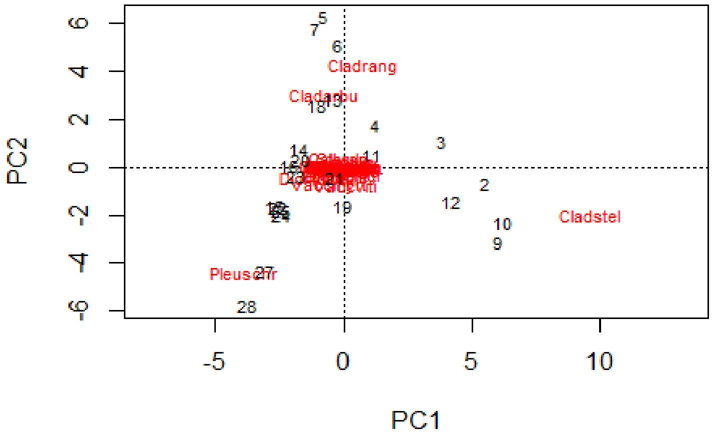

Figure III-2

These are just three of the dozen or so exercises Fran went through. They display instructed action in two ways. First, and most obviously, she took Oksanen's exercises as paradigm cases of `vegan` code and worked through them to familiarise herself with its naming conventions, distinctive structure, peculiarities of its displays and read-outs etc. The precise meaning of the read-outs and displays were given in the text and Fran's task when she ran the code was to check it all behaved (for her) as it was supposed to.[51] Second, this 'following along' strategy involved look ahead copy typing and tracking of the system's responses using the `R` markdown functionality. Of course, the copy typing inevitably included mistakes (missing commas, misspelt names, missing brackets) which raised errors and if uncorrected caused the code to fail. Fran resolved the errors by looking to the tracking feedback om

[51] Experienced (and inexperienced) programmers all know this is not a foregone conclusion. Beneath the hood of the code, differences in the implementation of R or the platform it is running on can all have un predictable effects.

the console and cross checking the code in Oksanen. The plots and values had a form she was familiar with. At least for simple things, `vegan` looked as it was straightforward to use.

To follow the instructions in the exemplars, Fran had to bring her prior knowledge of R and multi-dimensional scaling to bear on each exercise in order to see what, at each point, she should do and why it failed when it did. The majority of the errors were typos. This is entirely normal in the use of a new package and, indeed, a very familiar feature of computer use in general. Helpfully, the R markdown 'live' trace catches the most obvious errata, so by looking at the text being typed, i.e. the running code being produced, the trace feedback and the outputs shown, Fran knitted together a weave of instructed actions as her attention flitted from element to element clicking and pointing, checking and monitoring as she typed and re-typed.

In the Gestalt of the Modelling

We do not have space to do more than take one example of the complexity Fran had to manage when she eventually started to do the detailed analysis. Instead of a scenario text, we will start with a screenshot of 'the world' she was immersed in and discuss the structures of detail to which she was attending. We will not dwell on the statistical components making up her analysis, though we will, here and there, make reference to statistical objects. Rather, our concern is with how the statistics was made to work by managing orders of contingent detail. Fran's preparatory diagnostic analysis had identified 2004 as the inflexion point where initial colonisation began to decline and stabilisation took over. As we have said, she partitioned on that year, further filtered the data and ran a series on ordinations on the 2004 – 2016 data.

The screen shot below shows the work space for the analysis.

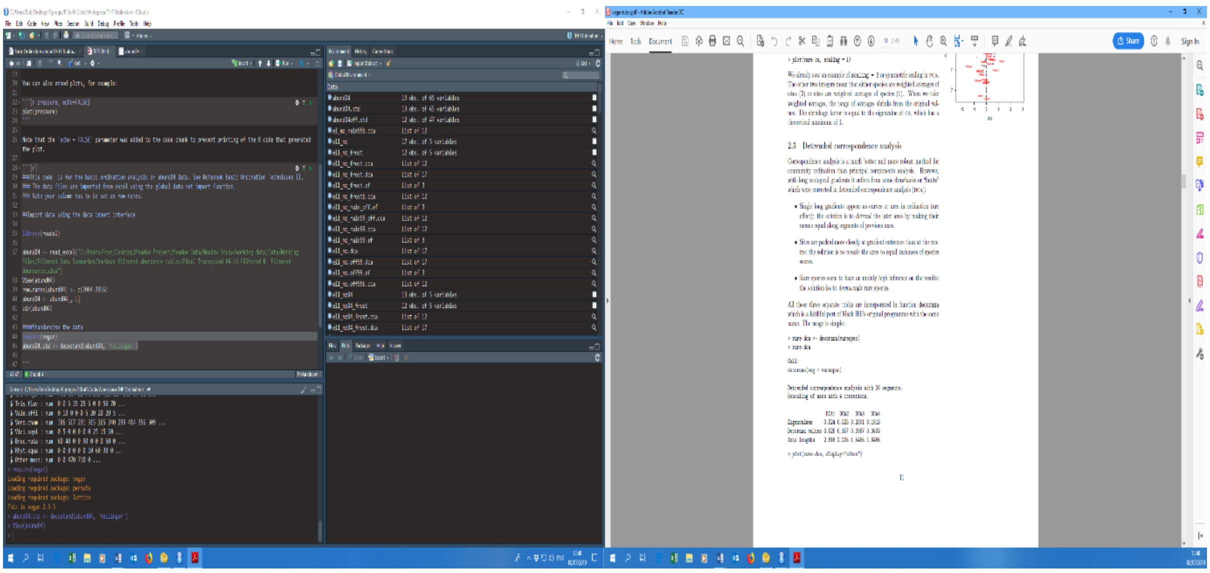

Figure III - 3

This space has two regions displayed across a pair of integrated screens. On the right hand side is a layered stack of the documents she is working with; these are her own notebook in which she is recording her analysis, Oksanen's notes and the plots she produces as she goes along. Each is foregrounded as needed. On the left is the *R Studio* IDE in which she carries out the computational analysis. The IDE has 4 panes. Working left, right: top, bottom, there is first an *R Markdown* space in which Fran builds and runs code and jots down notes concerning her tests. This code and associated notes are exported to the notebook when a set of analyses is complete. Individual `.rmd` files are saved to a directory for later inspection should it prove necessary. To the right there is a resource space which lists all the R objects Fran has assembled or created in her analyses. These objects are to hand for her analysis and so do not have to be repeatedly called up from file. Bottom left is the `R` console, the central IDE computing space which traces the processes activated by Fran in the *R Markdown* space. The bottom right offers listings of system resources and packages, the working directory and generated plots. These regions define the computational and documentary world in which Fran is immersed.

In the flow of her work, the central thematic focus is the *RMarkdown* pane where she operates and the console pane where she monitors the running of code. The objects in these two are managed in a dynamic tension along with objects near to hand in other IDE panes and in the document space. Her attention moves between them all as she calls up objects, checks the syntax of functions and expressions, looks at plots run on the fly, or makes decisions on the acceptability or otherwise of outputs. After an initial scan, accepted plots are exported to the document space for closer inspection.

Workflow-in-Vitro

The aim of ordination is to compress data into a form which can be displayed on 2 dimensions. These are axes are the data's 'principal components'. The method for obtaining these components is rotation using eigenvectors. There are two standard procedures to calculate the eigenvalues required, Euclidean dissimilarity distances and Chi-square dissimilarity distances. Both measure the 'distance' individual species are from one another in a diversity space. Having decided to partition the 2004-16 data, Fran's task is to run a set of ordination tests and 'constrain' her analysis to see if any climate factors can be found which 'fit' the principal components. Consulting texts, Oksanen, colleagues and various forums, Fran decided to run an diagnostic unconstrained analysis first to scope the distribution and then run alternative 'constrained' tests to see which climate factors best 'explain" the ordinations.[52] The steps are below.

```
###Notes for Analysis of Subset of Meadow Data 04 - 16 ####

##Diagnostics###

require(vegan)
require (CAinterprTools)
wkdata.std <- decostand(wkdata, 'hellinger')
wkdata.ca <- cca(wkdata.std)
wkdata.ca
aver.rule(wkdata.std)
plot(wkdata.ca, display = 'sites')
plot(wkdata.ca, display = 'species')
cols.cntr(wkdata, 1,T)
cols.cntr(wkdata,2, T)
cols.cntr.scatter(wkdata, 1, 2

#### Fitting environmental data to DCA.###

wkdata.dca <- decorana(wkdata.std)
wkdata.ef <- envfit(wkdata.dca, spei04, permu = 999)
wkdata.ef

#####Constrained ordination, using spei04 (see notes)###

wkdata.cca <- cca(wkdata.std ~ apr + aug, data = spei04)
plot(wkdata.cca)
anova(wkdata.cca)
anova(wkdata.cca, by = "term", step = 200)

#######Using SPEI variables to forward build a model##

wkdata.mod0 <- cca(wkdata.std ~ 1, spei04)
wkdata.mod1 <- cca(wkdata.std ~ ., spei04)
wkdata.mod <- step(wkdata.mod0, scope = formula(wkdata.mod1), test =
"perm")
wkdata.mod
```
```

*Exhibit III-5*

52 Assembling the climate data involved not just data assembly but the creation of a transpiration/evaporation index for the key 'growing months' of each year. We will not be describing that process.
```

On the face of it, following the workflow steps seems to be relatively straightforward. Simply call up functions in vegan, plug in the relevant data and run the tests. In the code above, we have provided hashed comments to mark up the order of the steps. We have extracted this example from just one of Fran's runs. In it, she tests the ordination against the April and August evapo-transpiration indices.

Workflow-in-Vivo

The core of the workflow is arriving at a 'meaning' for the analyses run. We construe Fran as treating vegan's responses as 'indirectly intentional' in the sense she takes the tests she runs to be telling her what her data 'means' and her task is to deduce the implicatures of that assessment. Under this description, her objective is to close the praxeological gaps between the calculation production process, the 'meaning' she finds in the numbers presented and the next steps in her analysis. She does so by taking the calculations to be stronger or weaker 'instructions' on how to make her analytic decisions. In this course of reasoning, she manages three orders of detail: vegan's computational processes in R; the calculative processes of the various stages in the ordination analyses; and the production processes of inputting data and generating interpretable results. Given the complexities of the data, the opacity of vegan 's calculative processes and the strict requirements for formulation and formatting of production inputs and outputs, managing a harmonic tension across the orders of contingency thrown up by each process is far from trivial.

The workspace for the tests is displayed below. The layout is essentially the same as that already described but the objects are those relevant to ordination of the data. We will step through the Fran's analysis and illustrate how the Gestalt of workspace is reconfigured as a analytic space during the running of the tests.

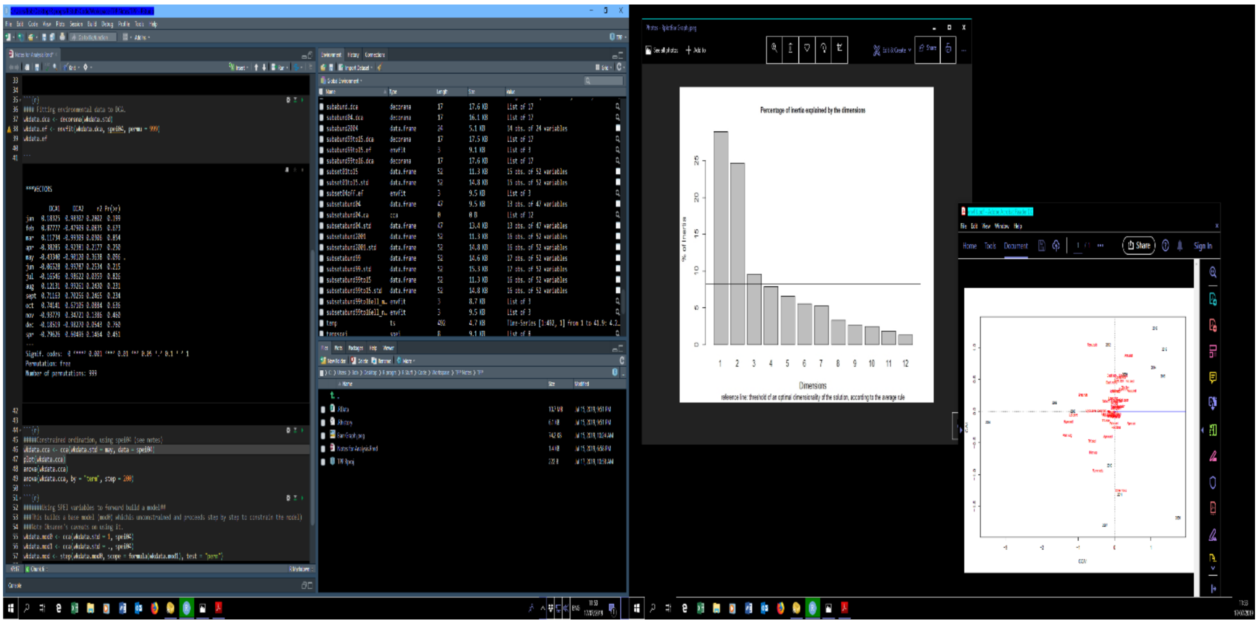

Figure III-4

Stepping Through

Fran knows ordination plots (known as 'bi-plots') are very often hard to make sense of. To illustrate this, look at the graph below. It is similar to the one Fran has pasted into her document space.

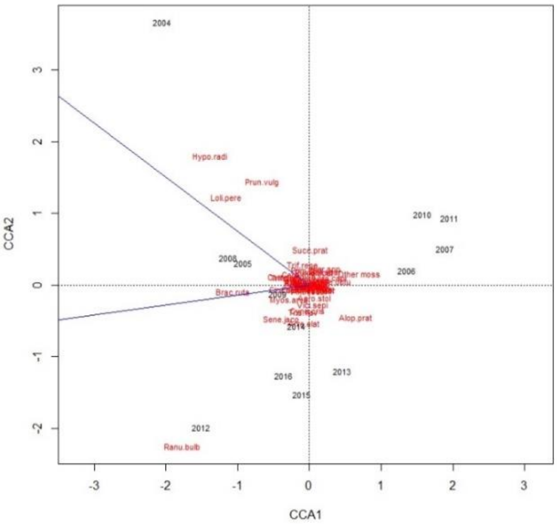

Figure III - 5

The species and yearly distributions are scattered around the origin of the two identified dimensions, some positively associated and some negatively. The vectors marked represent the relative correlation of the test factors with the dimensions and their explanatory strength. To guide her interpretation of these plots, Fran first runs a different analysis in which the principal components are laid out as a bar graph. This is a sensitising exercise. The example on her screen is below.

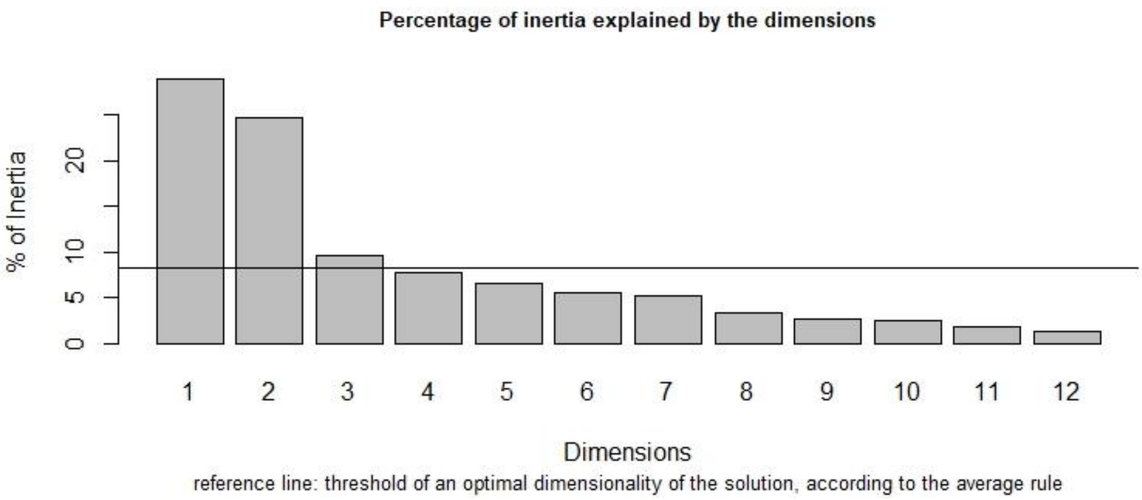

Figure III - 6

This clearly shows the first two dimensions (i.e. the ones used in the bi-plots) 'capture' most of the variance in the distribution and are statistically significant. Whatever they stand for, they appear to be organising the data. This inference is cross checked against two other representations (not shown) which set out which dimension is most associated with which species. Both indicate significant relationships for most species. So, given her supposition that climate is the/a major driver, testing for climate appears to be worthwhile. Fran treats this diagnostic as a progress checkpoint for the base data. When running the test, the plots appear as thumbnails in the console window below the code chunk. So Fran's feedback on the code chunk can be interrogated immediately below the code.[53]

The next test Fran runs is a detrended correspondence analysis (DCA) on the data which she then constrains with the evapo-transpiration data as one kind of climate data. This is literally a (brute) force function which tests the run of indices for each month against the year by year distributions. Fran is hoping 'something will show up'. The output is below.

```
***VECTORS
         DCA1     DCA2     r2 Pr(>r)

jan   0.18325  0.98307 0.2802  0.199
feb   0.87777 -0.47909 0.0835  0.673
mar   0.11734 -0.99309 0.0306  0.854
apr  -0.38285  0.92381 0.2177  0.250
may  -0.43340 -0.90120 0.3638  0.096 .
jun  -0.06528  0.99787 0.2534  0.215
jul  -0.16546  0.98622 0.0359  0.826
aug   0.12131  0.99261 0.2430  0.231
sept  0.71163  0.70256 0.2465  0.234
oct   0.74141  0.67105 0.0884  0.636
nov  -0.93779  0.34721 0.1386  0.460
dec  -0.18519 -0.98270 0.0543  0.760
spr  -0.79626  0.60496 0.1464  0.451
---

Signif. codes:  0  '***'  0.001  '**'  0.01  '*'  0.05  '.'  0.1  ' '
1

Permutation: free

Number of permutations: 999
```

Exhibit III - 6

Scanning this table, Fran is looking for possible 'significance' across the different factors. Only one is marked. The May evapo-transpiration index just fails the minimum significance threshold. But it gives

[53] To repeat what we said above. Everyone knows even the best of us are not perfect typists. Fran's monitoring of her coding attends to feedback in the Rmarkdown pane. Here she finds typos, mispellings, wrongly identified objects and all sorts of other errors. We will take these to be normal natural keyboarding troubles.

her something she can pull on. She can constrain the data with the May spei indices. The result is shown in the plot below.

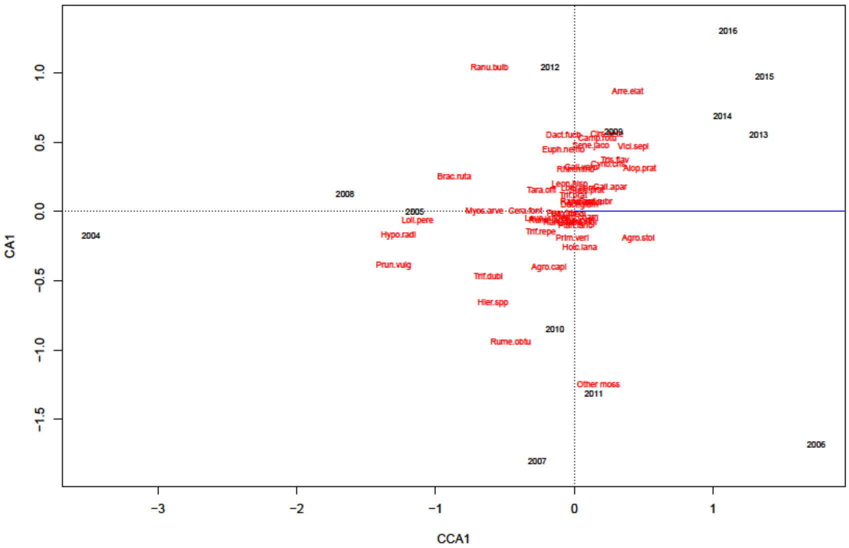

Figure III - 7

The May rainfall pattern fits the first constrained dimension well and, as indicated by its length (it 'disappears' off the page)' appears to be strong. She has one explanatory factor which 'works', at least to some extent. The key question, though, is how strong its association is when compared to other possible factors such as frost days, spring temperature ranges, and so on. But that will only be determined by the complete suite of runs. Her last test is to forward build a model using the evapo-transpiration data. Her hope is that this too will show May is a strong explanatory factor, but given the reservations about it expressed by Oksanen, the model's author, she is not going to place too much weight on it. It is just as well, because the test is inconclusive (results not shown).

In her workflow, Fran steers her analysis by scanning and responding to the results `vegan` presents. This is not a matter of deep cogitation but skimming and focusing on particular objects as she moves from one step to another. This focus shifts between what she can tell at a glance from the `Rmarkdown` errors, to what she can pick out by skimming plots and tables and to what she can figure out from the console traces. She moves across, among and between these analytic regions guided first by the bar graph and later by the bi-plot. The workspace is, in effect, a reconfiguring search domain as the terms of the search and the objects looked for change shift in the process. In this search, Fran is hunting down 'useable meaning', an interpretation of the distributions which can be framed in an appropriately robust botanical account. The analysis will not be 'the' account but a possible component of it. To change the image: her main objective is to find a story line, a hook on which to hang her results and `vegan` is being used to test plausible contenders.

Coordinating the Manifold

The above description offers a glimpse into the Gestalt of the analytic data space in which Fran is immersed. Even though we have looked at just one moment in what was a long and complex analytical exercise, hopefully we have done enough to bring out the depth contingency of the detail she is engaged with. The work of analysis is both an encountering with and a management of orders of disciplinary and common sense contextures. And yet, looking through the steps in Fran's work flow, one would be hard pressed to identify particular points where she was 'doing programming' or 'doing Statistics' or 'doing Botany'. If anything, her analysis looks to be doing all three at once. Coordination across the disciplinary contextures is an on the fly relativistic interweaving of particular disciplinary relevances utilising the bedrock of common sense reasoning as the mode of triangulation between the disciplinary considerations. Such 'manifold triangulation' is how the Gestalt is synthesised as an operational workspace.

First of all, let's look at a relatively straightforward example: the meaning of the `envfit` bi-plot.

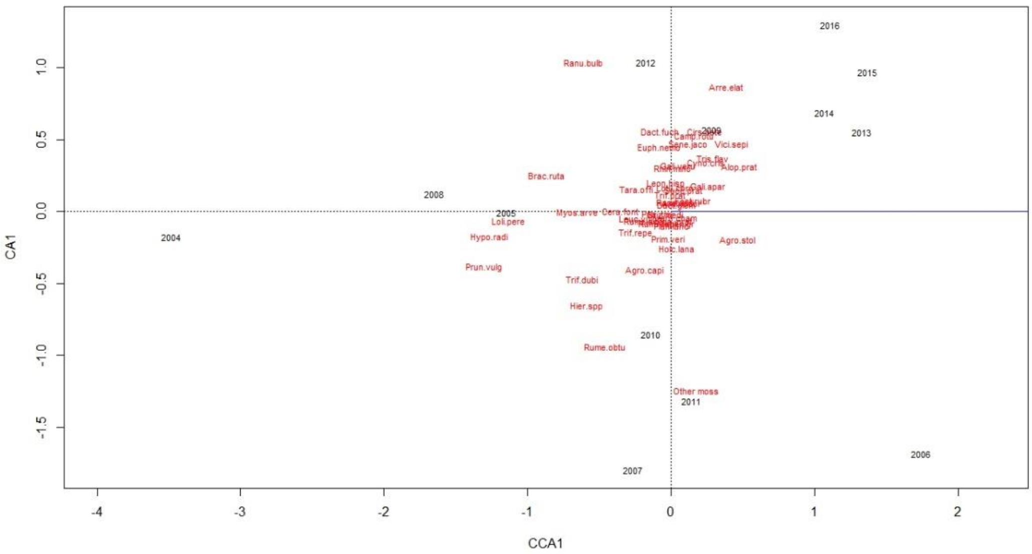

Figure III- 8

Wherever the code places it, the plot will show a vector representing May rainfall indexed through evapotranspiration. That vector turns out to align closely with the first constrained dimension. But what does that mean? The points scattered on the plot are two distributions: those of the dissimilarity distances of the plants and those of the dissimilarities of the years over the timeframe. In a direct common sense way Fran takes relative distance on the plot to mark relative distance in the analytic space. But what does *that* mean? From the clusterings of the plants, Fran can see species which she knows have differential growing periods (early and late flowering, say) or are more or less hydrophobic, are located 'closer' to one another in the plotted space. Equally, she can see years where she knows rainfall was above or below average are also 'close' to or 'distant' from the plant clusters. Fran imposes the statistical framing of the plot on these distributions defined by naturalistically[54] determined distance metrics,. The distributions to the right of the CA dimension are positive relative to the CCA (constrained) dimension and vice versa; the distributions to above the CCA dimension are positive to the CA (unconstrained) dimension and vice versa (remember, Fran only constrained on one factor so she only has one CCA dimension). Added to this are her use of the common sense properties of the vector itself. To the eye, this appears to be 1:1 positively correlated with the CCA dimension itself and long (it runs off the plotting space). This she takes to mean increases in rainfall in

[54] That is, determined under the 'Natural Attitude'.

May have a significant effect spreading the distribution in the analytic space. Fran doesn't work her way to this conclusion through an explicit course of propositional reasoning. Its obvious meaning is seen 'at a glance' by common sense triangulation of the disciplinary manifold of the plot.

The second example is slightly more complicated. Look at the read-out from the `envfit` analysis given in the table above. The correlations of the spei data have been run through an ANOVA and subject to significance testing. The usual 'rule' for significance testing is to 'reject' the significance of any result whose probability is greater than $p = 0.05$. That is, there is a greater than 5% probability the result is simply random. The May spei data fails this test but Fran goes ahead. Why? Fran triangulates this result against what she knows about the botanical practices adopted in the study and the 'reliability' of the spei data. While the data collection (ie the fieldwork) has been based on random quadrats, this is not a standard 'controlled experiment'. Because the same site is being surveyed repeatedly, there is likely to be some autocorrelation in the data. Also, the timeframe spreads over the period in which transformational processes were dominant (tailing off of early colonisaton, increasing stability of 'climax' species). These two phases involve very different species with different properties. In addition, the relative abundance (think denseness of the plant cover) increased over the period, thereby changing the local evapotranspiration gradient. Alongside these considerations, there was the 'iffyness' of the rainfall data. This was not measured on site but taken from the nearest national weather station and had a number of gaps which had to be interpolated. What she is working with is the best data she could get and the best field trial design she could use. Because she had to run in less than "experimentally optimal conditions", Fran's approach to applying the statistical 'rules' was more relaxed than it might otherwise have been. Two things weighed here. If she wasn't prepared to accept results like this one (close to but not quite 'over the line') she might not have any usable results at all. Second, she was still in the midst of working through the long list of possible factors and had no idea what the eventual results for all of them would be. Carrying on 'just in case' was, then, a sensible strategy. Whether she would wind up using these results, she would have to wait and see. Common sense practicalities combined with botanical necessity modified the statistical reasoning.

3. Criteria for Descriptive Adequacy

At the end of Part Two, we listed a set of initial criteria of adequacy for TPP descriptions. these were derived by applying the epistemological principles worked out in Part One to the metaphysical framework constructed in Part Two. At that point, our presentation was necessarily content free. Any criteria of adequacy must be independent of the content of the descriptions being offered. The principles and framework do not provide guidance on what elements of a setting should feature in a description simply how those elements should be arranged. We gathered our preliminary list under four broad heads:

- Descriptive concatenation
- Structures of relevant detail
- Determinations of meaning
- Patterns and configurations

In this section we will reflect on the criteria we proposed using the illustrations of tongue and groove milling and ordination analysis in Botany. No doubt much of what we say will appear pitifully obvious to many colleagues. However, over the years we have found recitals of what seems pitifully obvious have proven not just necessary but enlightening.

DESCRIPTIVE CONCATENATION

'Jodi' and 'Fran' are labels for course of action types; constructs whose properties are determined by the analyst. Jodi was 'someone routinely milling tongues on rail edges'. Fran was 'someone using a new package in R to do ordination analysis'. We provided designations of the relevances in play for each course of action and the stocks of knowledge each could call on. Jodi knew the machine she was using and its set up. She had performed the task many times before. Fran knew ordination analysis and R but not the package she was using. The style we used seemed to involve describing two people doing the tasks in hand, but it would be an violation of the criteria of descriptive adequacy for the analysis to attribute to Jodi of Fran membership of social categories not specified in the descriptive set up, even though such attributions are central to normal reasoning under the Natural Attitude. The personal pronouns we used, for example, were selected for *rhetorical not analytic* coherence. Our account is, therefore, necessarily disinterested in matters of gender. There is a further aspect to this. Descriptive coherence is set in terms of the keys specified for the account. For our account, there were preparation for action; immersion in the relevant Gestalt and managing the manifolds. The scenarios or detailed descriptions of the setting (Fran's screen layout, for example) are not analytic descriptive elements. They are handrails for the reader and in some measure, no doubt, evidence of the credibility of the account being given. To put it bluntly: analysis does not consist in reportage of the setting and its detailed components (i.e. the who, the what and the where).

Topic selection turns on the descriptive keys and the objective specified for the courses of action in play. In the Gestalt of their milling or their ordination analysis, actual woodworkers and actual scientists talk to colleagues as they pass by, break off for coffee, take lunch and do all the other things ordinary people do. But 'Jodi' and 'Fran' are neither people nor ordinary. All they do is the milling and the ordination. And their thematic focus is the task in hand within the Gestalt as we have specified it.

These strictures have implications for co-selection of descriptive elements and descriptive summarisation. The unfolding contingent detail of milling an edge does not encompass the music Jodi plays on the radio she has on. The trade-offs Fran makes over adherence to the protocols and the usability of data are bounded by the need to get to a point where she can find 'a hook' or a 'theme' for the analysis not by how she will defend or even refer to the need for trade-offs in any 'written up' presentation. The music and the writing up are beyond the horizons of the Gestalt. Similar considerations apply to summary generalisations such as references to or listings of 'normal, natural troubles'. Relevance to the particulars in hand is determinate. Thus we noted the problems posed by tilt when a rail passes over the edge of the router table or keyboarding errors when inputting code. These are troubles being managed in the workflow of the task. Troubles to do with system crashes, data corruption, deposit build up on the bit edge, bush wear in the spindle motor are all normal natural troubles and are undoubtedly managed. But they are not relevant to the organisation of the contingent detail in the workflow of the courses of action we describe.

STRUCTURES OF RELEVANT DETAIL

We said just now that relevance was determinate. This relevance is to the keyed configuration of the Gestalt as a field of consciousness. This is particularly so when considering the relationships among constituent features of the workflow. The grounds for determining what Kotarbinski called "good work" are given in the courses of action as decisions concerning what to do 'now' and what to do 'next' within the structuring of the flow. Fran, for example, decides whether to begin an ordination using a simple graphical display. While, in the abstract, making such decisions runs the risk of Type I and Type II errors (omitting positive cases, wasting time on cases that turn out negative), where she is in the process of sorting the data makes it a perfectly reasonable triage strategy since it offers a 'guessimate' of the weight of the first two principal components. Similar considerations apply to Jodi's 'living with' fixable errors in the milling of the edges. What is 'good enough' to work with depends on what she knows can be done in some other process. Both Jodi and Fran know what 'good work' is. They also know it isn't perfection. The efficacy of their decision making depends on their decision strategies for ensuring the 'good enough' has a reasonable approximation to 'perfection', where what is reasonable *is for them to determine*.

Determinations of Meaning

In our characterisation of the workflows, the centrality of instructed action makes sense assembly critical for TPP description. This is why we have emphasised the importance of the investigator's possession of a broad range of the competences necessary for the structures of reasoning being analysed. These competences allow the delimitation and delineation of thematic fields social actors are immersed in and which shape their apperceptions. We referred to these competences as the shared and hence consociate means by which Jodi tuned into the manifold of the milling and Fran found her way about in the manifold of the information space she was manipulating. It is these competences which allow the analyst to discern the internal structure of the component actions being oriented to and hence the ramifying sense assembly which is underway. Thus descriptive adequacy does not consist in the observation Jodi is concentrating on the work in hand or Fran is absorbed in her screens typing, clicking and erasing. It is what that concentration is concentration on and its internal configuration as the 'work' of tuning into machine and its interaction with the rail or reasoning through the results `vegan` has thrown up. It is what that absorption consists in as a matter of the structuring of knowledge at hand and within restorable reach, what is foregrounded and backgrounded and what is acted upon in what order. In both cases, what the *attention a la vie* is directed to in the *durée* of the course of action is a matter of moment by moment emergent configuration.

Patterns and Configurations

When assembled in a fashion which coheres with the keyed configuration of the Gestalt, the elements set out above depict the fluidity of the kaleidoscopic reconfiguring of the evolving and ramifying field of consciousness. As we discussed each of the headings, the illustrations we gave made clear the organic reciprocally designed relationship between the descriptive elements and the patterning of the overall configuration. These are not separate components or levels but a synecdochical totality. It is that totality which is TPP's phenomenon.

4. Summary

Our aim in this Part has been to illustrate our approach to the description of the interior configuration of the workflow of social action. We have taken the Garfinkelian 'shop floor work' motif as our theme and have rendered it in terms of the general methodological schema we have developed for TPP. As accounts of action workflows, the two specimen analyses we provided are reminiscent of our early studies of mathematical problem solving and paediatric practice (Sharrock and Anderson 2011, 1986). However, they offer a more fully articulated analytic scaffolding to support and shape the way the analyses are constituted. The mode of social action we have described is consociation as reasoning through engagement with cultural objects. The first object is a woodworking machine designed and set up to produce a tongue and groove edges. The second is an institutionalised scientific practice, ordination analysis in Botany. Our treatment of these two cases of social action construed them as flows of 'instructed action' through which the detailed management of the "shop floor work" was accomplished. The construal we have adopted provides for the description of social action as a continuous and fluid melding of culturally provided pairings of inferred instruction and determined appropriate response. The scaffolding to which we referred just now is manifest in our presentation as the presentational format:

- Preparedness for Action
- Immersion in the Gestalt of Action
- Managing the Manifold

Each heading is a shorthand for distinct budgets of analytic entities and processes which, although realised in different ways in different circumstances, make up the interior configuration of the task(s) in hand as those tasks are experienced by the social actor(s) concerned. The format constructed thereby forms the key our rendering of these cases as instances of *third person* phenomenology. Finally, we have offered suggestions for how the criteria of adequacy we have advocated are satisfied in our descriptions of the materials presented.

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PART FOUR

REFLECTIONS ON THE CHARACTERISATION PROBLEM

1. INTRODUCTION

Towards the end of his splendid collection of studies *Ethnographies of Reasoning*, Eric Livingston spends a few moments considering what he calls "the characterisation problem".

> We want to find the identifying features of ……domain-specific work for the people engaged in that work. And we want to find these identifying features as the discovered, omnipresent, intrinsic, utterly ordinary, social character of skill and reasoning. This theme/orientation/research directive is referred to as "the characterization problem", the attempt to characterise an activity, in its identifying lived detail, as the recognizable work of its production. (Livingston 2008, 246)

Of course, he actually doesn't mean everything he seems to be saying here. As we have insisted several times, EM's metaphysics is not about "people" as we usually use that term but a sociological object, the 'social actor', constructed as a social type under the stipulations of various sociological renderings. This quibble aside, Livingston's two sentences provide an excellent summary of EM's analytic objective, namely to provide, as Garfinkel put it, uniquely adequate descriptions of particular courses of action and reasoning. He goes on

> Whether anyone has solved the characterization problem for a particular domain of practice, whether it's possible to solve this problem, and whether the problem provides criteria for assessing the adequacy of a study are matters I leave to theorists. For myself, such discussions are a distraction. The characterization problem *as a practical problem*, is absolutely fundamental to our research: it helps us see where we are in our studies, where we want to go, and where the inadequacies of our present work lie. The attempt to solve this problem gives coherence, continuity, and direction to studies that, at time, look like a inchoate collection of unrelated, fragmented and incomplete projects. (Ibid p. 246)

While he is not interested in the probative aspects of the characterisation problem, indeed he finds them "distracting", Livingston is vitally concerned with how to do characterisation as a sociological activity. So are we. That task has been the burden of the first three Parts of this monograph. However, as we pointed out in Part One, alongside descriptive adequacy Garfinkel also saw probativeness as a key feature of sociological descriptions. He insisted both were equally fundamental, so in this Part we will take up (some might say belatedly) initial aspects of probativeness. At the same time, we will also pick up a component of methodology which we deliberately side lined when, at the start of this monograph, we introduced the approach we would take. We will look at analytic techniques through the lens of probativeness. To do so, we will go back to the discipline which was the source of the term "characterisation", Mathematics, to seek some guidance on the kinds of way analytic techniques might be deployed to provide probative descriptions as well the challenges of doing so.

Discussions of the contribution Mathematics might make to Sociology usually focus on the whether the adoption of some piece of mathematical machinery will improve the rigour of a sociological explanation or do violence to its subject matter. Voices are raised, allegations tossed hither and thither and the usual brawl carried on. All this commotion is pointless. Before we can determine what (if any) value Mathematics might bring to sociological analysis, we have to get much clearer about what counts as a good or even adequate description of Sociology's phenomena and what relationships we should put those descriptions in. Before we have settled those questions, the debates over analytical devices can only seem premature. In a BBC interview in 1981, Richard Feynman commented that the thing about the social sciences was that they hadn't done the work to be scientific. Sorting out what expectations to have of proper sociological descriptions is, we would claim, a major part of the work to be done.

This does not mean that Sociology can gain nothing at all right now from looking to Mathematics. On the contrary, the example Mathematics sets us for how to go about carefully and systematically assessing a description of a phenomenon can be very revealing not for the application of mathematical devices or techniques but of professional analytic practice and, dare we say, the standards to which such practice adheres. In what follows, we will compare the ways professional Sociology[55] arrives at descriptions of its phenomena and the approach adopted by Mathematics. We will argue that whereas Sociology tends to be satisfied with 'good enough' intuitions, Mathematics is at pains to arrive at formulation which are (as Livingston's own studies illustrate) tightly bound to its phenomena and which bring out what is unique or distinctive about them. In sum, what we can learn from Mathematics are standards *of* description not devices *for* description. Armed with these standards, or something like them, we might (at last) be able to do the work which Feynman says (rightly in our opinion) has not yet been completed.

2. PRELIMINARIES

As with all disciplines, analysis and generalisation in Sociology begins with the establishment of cases. Properties of social phenomena are described so that they can be gathered into equivalence classes. To quote Dorothy Smith, the purpose of such description is….

> …. to represent what actually happened, what was there, or some describable state of affairs, such as an actuality capable of observation and report to be

[55] We count EM and what it calls "constructive" analysis (or "formal" analysis, or analysis of the "hidden social orders") as professional sociologies.

> rendered accessible to other sociologists in a manner which enables them to treat that report as a datum. (Smith. 1981 p 314)

But, as she goes on to point out, giving such descriptions is far from straightforward.

> The aim is not just any old description but a description which is both relevant sociologically (whatever that may be) and one which can be used and relied upon by others......It is, therefore, a description which must describe the observed rather than the observer, and which does not distort the original in ways which are products of the observer's particular interest or perspective. (Ibid p 314)

As Smith hints, there are various troubles which can arise in the giving of sociological descriptions. Here are just a few.

1. A resolution to what we have described as the challenge of 'problematic possibilities' has to be found. Under the analytic gaze, seeing is always seeing as and so the investigator has to decide the framing rubric to be adopted. Often enough, this resolution is taken for granted, the criteria being bundled with the theoretical or methodological position the investigator adheres to.
2. Added to this is the fact that the instance or case is often presented to us through some form of representation (talk, writing, objects) which is embedded in a local culture. Thus to form our description, we rely on our informants or our own (or both) competency in that culture. How *those* competences relate to *sociological* competences and how *those* descriptions relate to *sociological* descriptions often remains unexamined.
3. And then there are the standards of acceptability of description. Although we might know enough or learn enough to be able to agree that some description given to us as 'data' by our informant is culturally adequate, how do we determine that our transformations of it are sociologically adequate while remaining faithful to the phenomenon? What 'tests' should it have to pass?

Although the first two problems are enticing, in this discussion we will focus only on the third: the problem of standards of description. And here, of course, we run straight into what is probably, if you will pardon the pun, the defining characteristic of the profession. It is not that there is an absence of standards. It is just there is no agreement over them, and what is held to be a paragon of rigour for some is taken by others to be a symptom of methodological, theoretical or epistemological delusion. Looked at from the outside, say from the natural sciences or the arts, Sociology seems to be irredeemably free form. Just about any account of any phenomena will find some advocate somewhere.

In this discussion, we take this "mess of method" (to quote John Law (2004)) as both our topic and our resource. We will confine ourselves to the first few steps of the process of sociological analysis and the ways data is 'shaped up' for analysis. In line with the description we gave in Part One, we suggest this shaping involves at least three steps.

modal transformation => rendering => characterisation

By modal transformation, we mean the re-organisation of the ('raw') 'materials' into 'analysable data'. Observations are turned into structured field notes, interview responses into mappings of counts and frequencies. Much of the discussion over method in Sociology (though elsewhere it is usually referred to as 'technique') focuses on this step. Students are schooled in a range of techniques. Investigators define and promote new variants whilst their opponents offer critiques. Our interest centres on the second and third steps ,"rendering" and "characterisation". We will look at some examples of 'devices' used to organise data so it can be rigorously described and the descriptions so generated. We focus on the plausibility structures deployed to rationalise and justify the adequacy of the descriptions given. Such plausibility structures tell us how the descriptions are designed to satisfy Smith's criteria of sociological relevance and reliability. Actually, it is slightly more than that. Behind our concern is what has been an abiding (now designatable 'TPP') analytic interest in the *effort* it takes to construct an acceptable description, particularly in Sociology, and the organisation of its production workflow. Just what is involved in this work? We are not (or at least not here) interested in picking and choosing among the kinds of sociological work involved. To do that would take us back to 1 and 2 above and involve a discussion which would be impossible to cram into the space we have available here.[56]

Two of the examples we discuss are taken from Sociology; one 'formalist' and the other 'interpretivist' (to use those loose and capacious grab-bags). The third is taken from Mathematics and more specifically the Mathematics of Art. Nothing turns on the particularities of the cases we have chosen. They are simply examples we have to hand. Our claim is these studies exemplify some important differences in the work required by the different disciplinary practices both in terms of the 'grounding' of what counts as a description as well as its assessment. This comparison leads us to conclude we can learn a lot from what Mathematics does to 'ground'[57] its descriptions and the ways that mathematicians assess the adequacy of their own work. What it takes to give an acceptable mathematical description, how that description is assessed and what the role of the investigator should be in that assessment are all lines of thought Sociology might do well to reflect on.

3. Exemplar Sociological Descriptions

We have chosen two contrasting sociological studies as the initial cases for our argument. They are run of the mill and that is the point. The studies themselves are typical of their type and have been published in well-respected sociological journals. Since the investigative practices, approaches and descriptive forms will be familiar to our readers, we will spend only a short time setting out the detail of each. Our hope is that having sketched the issues as we see them illustrated in Sociology, we can spend more time explicating how the same issues are addressed in Mathematics. We start with an example which, at least in our view, manifestly displays the initial steps of analysis we set out above. It just happens to be formal in nature but that is not the feature we pick out. We then turn to a case where the steps seem to have been elided or foreshortened and hence the process is more difficult to exhume. This one is interpretivist which is probably not an accident. In our experience, interpretivism has a tendency to slide quickly (though not always effortlessly) from observation to characterisation by conflating or even confusing the two. In a separate section, we look at the example we have taken from Mathematics.

56 We have had at these issues many times over our careers, most recently in Anderson and Sharrock (2013).

57 Obviously we do not mean to invoke 'Grounded Theory' here.

CHAINS OF AFFECTION

Since at least the publication of *Coming of Age in Samoa* and *Growing Up in New Guinea*, investigation of the sexual lives of adolescents has been a central pillar of sociological research, though one which has recently been reinvigorated by the increasing spread of sexually transmitted diseases among that particular segment of the population. Investigators have been especially interested in the patterns of diffusion of the diseases and their social correlates. Peter Bearman and his colleagues (Bearman, Moody and Stovel 2004, hereafter BMS) undertook a detailed analysis of the sexual partnerships among 800 adolescents attending a particular High School. The participants were asked if they had been in a romantic relationship recently and, if so, were asked to name no more than 3 sexual partners they had been romantically engaged with over the previous 18 months and no more than 3 non-romantic partners they might have been involved with. This resulted in a data set of 573 participants.

BMS took the lists provided by the respondents and mapped the identities and frequencies into a graphed network. The pattern the graph took was a spanning tree; that is, a network with a clearly identifiable spinal ring and short branches. The researchers compared it to the network architecture of a rural telephone line. This modal transformation provided the phenomenon. The puzzle was how to render it sociologically. Assuming the structure was very unlikely to have been produced by individuals randomly selecting partners, they asked what social process might have produced it?

> Put most starkly, adolescents do not account for their partner choice by saying, "By selecting this partner, I maximize the probability of inducing a spanning tree." First, they cannot see the global structure, and second, they do not care about it. What 'social rules' might the students be following which could produce such a distinctive pattern? (Bearman et al 2004 p67)

In Western cultures, homophily is generally held to be the basis of partner choice. People form relationships with people like themselves. Using this piece of conventional wisdom, BMS used partner similarity as their first rendering device. Examining the demographic and other properties of students, they looked for homophily. Unsurprisingly, it was indeed evident. Individuals tended to seek partners who were like themselves in almost all characteristics. However, the rule did not apply to the two usual leading terms in these forms of sociological description; gender and age. Both genders formed partnerships across gender categories and girls formed partnerships with older boys.

Conventional wisdom about homophily, then, appears to be generally correct but what is the social process producing the spanning tree distribution? The next rendering device BMS used was simulation. To do this, they re-formed their data by stripping out the actual partnerships formed and modelling the resulting homophilic data on the basis of random selection or prior sexual experience. Neither simulation produced a spanning tree. Whatever process was at work in addition to homophily, it wasn't random selection or a preference for partners with previous experience. A third simulation was run in which there was choice was fixed by 2x2 partner sharing (John partners Jane and Mary partners Michael; after which John and Mary and Jane and Michael get together) which gives a 4 link cycle. This did generate a network somewhat similar to the observed one. Finally, BMS simulated a model based ≤ 3 links (John partners Jane and Mary partners Michael; after which Jane and Michael get together but John does not get together with Mary). This simulation produced the requisite spanning tree. The rendering device of homophily together with a ≤ 3 link rule simulated the spanning tree graph of the transformed data.

The question BMS now faced was what sociological formulation should be used to express this result. The sociological rationalisation they offer for their pair of simulation rules (homophily and ≤ 3 link cycles) turns on the suggestion that peer group status is important to the students and its loss avoided. They propose two social forces are at work; attraction to people like oneself and one's immediate peer group and the avoidance of what they term "seconds" (i.e. recent partners of an individual who is closely linked through the network). This rationalisation then provides a social rule which governs the unarticulated (and possibly unarticulatable) preferences of the students. *If* they were following a norm such this *then* they would produce this pattern. The rationalisation makes the pattern sociologically intelligible. However, and this is key, since the norm was not recognised or invoked by the adolescents in question, the acceptability of the rationalisation as a social motivator for *their* behaviour is at best underdetermined.

The point we want to make about this study is not that the graphing of partnership choices is but one mode of formalisation which might have been used. The dead metaphor of friendship and sexual 'networks' in ordinary talk does not necessarily mean we have to use graph and network theory to analyse such relationships and BMS provide no reasoning for their choice. Neither do we want to say anything about the proclivities of adolescent girls and boys to dampen or amplify claims about their 'conquests' nor to ask what constituted 'sexual' activity for the adolescents concerned and whether this was consistent across the group. In other words, we are setting aside issues to do with the reliability of the data. Our point is far simpler. Having worked hard to find a mathematical form to summarise the data they had obtained so that it could be described sociologically, the description they give is premised in nothing more than conventional wisdom, intuition and untested presumptions about adolescent culture. The description consists of a preference, its formulation as a social norm and its attribution to their participants. As a result, the sociological description hangs in analytical mid-air.[58]

Of course, BMS are not unusual. Descriptive levitation is common in formal sociology. Here is Stanley Lieberson — never one to mince words — offering a view of the contribution that Qualitative Comparative Analysis (QCA) might make to sociological analysis. QCA is the application of forms of fuzzy set theory to small comparative data sets, and as far as Lieberson is concerned, is so lacking in empirical grounding as to be next to useless. By way of a test (or is it demonstration?) of his thesis, Lieberson formulates three hypotheses. This is the third.

> Suppose we obtain data for which there are 40 cases. We randomly assign the 40 into two sets of 20 cases (sets A and B). We apply QCA to set A. An account is obtained. We then apply QCA to set B. Again an account is obtained. Hypothesis III predicts that the two accounts are substantially different from each other. And, if that proves generally true, it tells us that QCA is being used far beyond what it is capable of doing. QCA over-interprets its data and therefore goes well beyond what is justified (again, unless an extraordinary form of determinism is assumed). (Stanley Lieberson. 2004, pp 13 -14)

58 Leaving it hanging so is, of course, one way of finessing (as we called the practice in Part One) the challenges of detailed systematic description. Mark Wilson (2017, 2019) called much the same strategy "physics avoidance" when he discovered its prevalence in that discipline.

What Leiberson is complaining about, and what BMS unfortunately demonstrate, is the rickety nature of a discipline in which, as Arthur Stinchcombe pointed out years ago, the formation of adequate descriptions is taken so lightly.

> Choose any relationship between two or more variables which you are interested in; invent at least three theories not known to be false, which might explain these relations; choosing appropriate indicators, derive at least three different empirical consequences from each theory, such that the factual consequences distinguish among the theories……My experience with gifted analysts of empirical data suggests the student ought to be able to produce the three theories and nine derivations within an hour or so…. (Stinchcombe 1968, p 13)

Internet Use and Discursive Strategies

Our next example is different in two ways. It uses a different form of data, transcribed interview talk, and offers a different kind of analysis, one which interprets the meaning or significance for the participants of the phenomena in the data. Sarah Nettleton, Roger Burrows and Lisa O'Malley's (hereafter NBM) (2005) looked at how people talked about their internet use, in particular for gathering health information. They interviewed 69 parents and 19 children whose families had taken part in a much larger study of related topics. These interviews constitute their data. The data were transcribed utilising a standard set of protocols broadly based on those developed by Gail Jefferson. The resulting transcription is a modal transformation of the interview talk. The transcribed talk was then reviewed for possible sociological phenomena. This structuring of this review (i.e. the finding of phenomena in the transcribed data) is NBM's rendering device. Here is their description of how this works.

> We interrogate our interview data to try and begin to decipher and make explicit some of the ways in which talk about internet use is socially organised and 'trust' accomplished….. (A) discourse analytic approach….and in particular….emphasis on 'the rhetorical or argumentative organisation of talk' ….is one that chimes well with our approach here. (p.978)

The answers which participants gave to questions about their internet use are treated as ("interrogated" for) argumentational strategies. That is, descriptions of what they do and why are designed to promote or defend possible interpretations. The classification of these strategies is NBM's sociological description.

From their data, NBM identified a single broad discursive strategy of 'legitimation' for the way the internet was used. Through this strategy, the participants demonstrated recognition of the variegated nature of the information available and the inconsistency of its reliability. This strategy NBM dubbed "Being cautious in contrast to…" Within this strategy they identified a number of "rhetorics of reliability" which took the form of implied contrasts. In NBM's words….

> Such rhetorical devices are readily available to the speakers who are able to articulate them to locate themselves in relation to prevailing social norms and values. Speakers use these rhetorics to rebut blame or enhance their social standing in relation to internet use. (NBM 2005, p 979)

Identifying instances of the rhetorics is the sociological description of the transcribed data. The following table lists the categories of rhetoric found and provides samples of the transcripts NMB use to demonstrate them. There is, then, a process of mutual explication at work here. The categories are used to tell us what the data are; and the data are used to tell us what the categories mean.

Real versus virtual organisation	You must be very wary of anything nowadays, where you get it from. But if you go somewhere like the National Asthma Society and places like that, and there's asthma clinics as well. I think they are the best place to go to, rather than, you know, some oddball place that you've never heard of (P40, middle area).
UK versus 'other'	I can see they're both doctors, one trained in Guys, one trained in Edinburgh, I know where they are, I'm more wary of foreign sites where I don't know these people's qualifications, I don't know where they're coming from. It makes sense to me I would trust that, and you know where it's coming from. So yeah, a lot of this American stuff I think, I can't trust it cos I don't know it (P42, Mother of son with asthma, middle area).
Non-commercial versus commercial	Commercial, I don't do commercial at all really, more the medical. I don't go in for gizmos. I don't go in for anything like that. It's got to have a logical sound medical reason behind it (P10, Couple, two children with eczema and asthma, wealthy area).
Professional/Official versus non professional	If it mentioned a medical board or things like that, then I would be more inclined to read it and believe it, than if it is just somebody who has posted the site up. It would have to have some recommendation somewhere (P23, Couple son with asthma, poorest area).
Codified versus experiential knowledge	I don't do chatrooms because again I wouldn't value anyone's opinion. Aren't I funny? No, I wouldn't do chatrooms at all. I wouldn't want advice from a chat room, cos it'd be like me giving, it's only an opinion isn't it (P19, Mother, 2 children with eczema and asthma, wealthy area).
Replication – going with the majority view	I think finding out what was coming up most, cos the contradictory information I would think would only appear once or twice in a whole web of stuff. Whereas if there was the same pattern emerging and you could say well hang on a minute this is OK, this is generally what tends to happen (P1, Mother, 2 children with eczema, wealthiest area)

Table IV - 1

Unlike the BMS study above, NBM do not explain the steps by which the rendering of the transcripts into 'arguments' is achieved nor the process for gathering the 'arguments' into the "rhetorics". This does not mean the work was not done, simply that it is invisible in the account offered. In addition, whereas the BMS study deployed a combination of conventional wisdom, intuition and attribution to provide a rationalisation of the pattern they had found, NBM use their own rationalisation to drive the patterning of the data. As a result, the data and its sociological description are so entangled, it is almost impossible to assess the adequacy of the description *as a description of the data*. With BMS, we said that the investigators worked hard to find a rule that 'explained' the mathematical pattern and then relied on common sense, intuition and attribution to render it sociologically. NBM no doubt worked equally hard to find cases of the sociological phenomena their presuppositions ensure *must* be in the data. But what they got out is what they put into the data in the first place.

The point we are making right now about this study is not that we do not know how representative the excerpts are of the general run of accounts their informants gave of their internet use. Neither are we concerned here with the basis for attributing *this kind* of intentionality to them. Once again our interest is much simpler. It is why were *these* categories settled on rather than others and what is the basis for allocating the statements to them? What tests have been applied to check that this type of categorisation is appropriate and to what extent are the contrasts indeed descriptive of the categorisations used by the participants? NBM provide no help. Instead, we are told in advance how to read the data and then the data are allowed to speak for themselves. Such mutual explication is the plausibility structure for their distributions. Rather than presenting an abductive path from data to types, what we are offered are cases as the instantiation of an abstractly constructed classification scheme.

Of course, NBM's study is a conventional enough approach which follows the standard professional practice of most qualitative analysis, a practice that has been queried even by the most iconic of interpretive researchers. Here, for instance, is Clifford Geertz musing on the problem of describing the beliefs of a non-western, non-Judeo-Christian culture.

> The issue here, how we are to name and classify cultural formations in other societies that are at once broadly similar to ones in our own and oddly *sui generis*, strange and different, is quite general in anthropology: a recurrent crux. Whether it is 'the family,' or "the market," or "the state," or "law," or "art," or "politics," or "status," deciding just what goes into the category, as we say, cross-culturally, and why it does so, is an essentially contested matter, a circular discussion that stirs polemic, never ends, and only marginally, and then rather diagonally, advances. (Clifford Geertz. 2010 p 223)

The reason for this, or so Geertz suggests, is that to clarify the concepts articulated in the categories, we end up inevitably talking about 'meaning', 'purpose', 'point', 'significance' and the like and

> (p)ut that way, it might seem that all we have succeeded in doing is replacing one obscurity with another; the indefinite with the indistinct. (p 224)

Geertz, of course, was famously eclectic in the sources of his ideas, as happy to shop in Analytic Philosophy and Phenomenology for spurs to his interpretative ends as he was Game Theory and Economics. Others, wearing the term 'ethnography' more as a badge of honour than a description of their sociological intent, have been less insouciant. For them, the directedness of the anthropological gaze, and with it ethnographic description, is profoundly troubling. The advent of the portable (digital) recorder and camera whilst perhaps promising a degree of 'objectivity' and 'neutrality' has, paradoxically, made matters worse as has the migration of the 'setting' which is being studied into the online world. Here is Anne Beaulieu on the problems of field work which faces both challenges.

> I am left wondering whether capture is not a new theme arising as a dominant trope within the variety of exploration of the internet. This trope may be developing in reaction to the notion that transcription is no longer an epistemic gain in a mediated context. The new possibilities for archiving and retrieving, may also be giving rise to these different ethnographic values. Certainly, a focus

> on accessing (the totality of) traces would seem to align ethnographers with those who are able to grant such access: system operators, developers of prototypical tools, and other gatekeepers echoing other strategic alliances made by ethnographers with colonial authorities. (Anne Beaulieu. 2004 p 158)

4. THE MATHEMATICS OF STRIPES

By way of comparison with our sociological examples, we now want to look at Neil Dodgson's (2012) attempt to provide a precise mathematical description (a characterisation) of Bridget Riley's stripe paintings. We choose this example for a number of reasons.

1. The topic is not an abstract mathematical object such as a torus or hypersphere nor a class of functions like Markov chains. We do not need refined mathematical intuitions to appreciate what is being described.
2. The characterisation is applied to what is obviously the product of a skilled creative process but one which so far has resisted sociological 'deconstruction'. This means we do not have to step around (or over) sociological accounts of the phenomenon which are alleged to compete with mathematical ones.
3. The mathematics deployed is not overly demanding. Readers who are relatively mathematically unsophisticated should be able to follow what is going on.
4. The work of the mathematical reasoning underpinning the acceptability of any description is on display. Dodgson is clear what demands he is trying to satisfy and why, in the end, he fails.

Definition and characterisation are two different things in Mathematics. We might define a circle as the locus of all points equidistant from a fixed point but characterise it by the property that its circumference is given by $2\pi r$. The characterisation provides a simpler and mathematically more useful description. It should also serve to identify what makes a circle distinctively a circle and not a disk, an ellipse, a sphere or any other manifold. The aim of characterisation is to find a mathematical formalisation which uniquely specifies a class of objects — circles in this case. In Neil Dodgson's example, the class is a set of stripe paintings produced by Bridget Riley. In her paintings, Riley uses a small number of colours (turquoise, red, blue, yellow plus black and white) laid down in stripes. What Dodgson is looking for is a simple yet precise mathematical description of some property of the configuration of the coloured stripes which would define what makes Riley's paintings distinctive. Putting it another way, what Dodgson is aiming to do is answer the Ed Shils question for Bridget Riley paintings.[59]

The approach Dodgson uses is to compare the configurations of Riley paintings with the configurations of a number (301) of randomly generated stripe 'paintings' using the same colour set at Riley. Can he find a way of describing Riley's paintings which allows him to group all her paintings together without including any of the randomly generated ones? If he can then he will have a possible *characterisation* of Riley's work. The modal transformations Dodgson uses are various counts and measures of the stripes.

59 We know what makes it a stripe painting; but what makes it a Bridget Riley stripe painting?

These are rendered for description under a number of mathematical formalisms.[60] The hope is that one of them will not just discriminate Riley's paintings but uniquely identify them.

ENTROPY

Dodgson starts with the distribution of stripes. Is the frequency with which Riley choses particular colours for her stripes distinctively different to the frequencies in the random paintings? The frequencies for individual colours in the paintings is a probability distribution. These distributions can be used to measure the randomness (entropy) of the paintings in the Riley set as compared to the random set.

Zeroth Entropy: Here the comparison is for single stripes of colour. From observation and Riley's own documentation Dodgson knows that her early work displayed the colours she used [t,r,b,y,b,w] in the following proportions [333311]. In other words, turquoise, red, blue and yellow are used three times as often as black and white. We can use these proportions to define a vector of probabilities for each painting, **p**, and can calculate a measure of its entropy (H). Dodgson calculates entropy value for the standard form to be H = 2.45.

If the paintings follow Riley's 'standard form' their zeroth order entropy value (H_0) will be close to H = 2.45. Most of Riley's paintings are indeed close to this statistic but not all. Hence the measure is not discriminating enough.

First Order Entropy: Dodgson now asks about pairs of stripes. Are there pairings which are 'forbidden' in Riley's paintings? This would mean that the distribution of probabilities for pairings is skewed. Clearly stripes of the same colour are forbidden (that would give a 'double width' stripe) but so, it turns out, are pairings of black and white. Calculation of the H_1 values for pairs of stripes in the paintings reveals interesting differences with two pairs of colours other than black and white also not being placed together. Mapping the zeroth entropy and first order entropy values against one another for Riley's paintings and the 301 randomly generated paintings shows distributions with the majority of Riley's paintings clearly separated from the random ones. However, several are not. Again, the device is not discriminating enough.

Second Order Entropy: This is about triples of stripes. Is the deployment of triplet combinations distinctive? Once again, mapping the H_2 values against the H_0 values provides no better discrimination than first order entropy.

Entropy measures for whole paintings, it seems, are insufficient to characterise Riley's paintings.

SEPARATION DISTANCE

Given the paintings are deliberate constructions, perhaps distribution of the colours measured by the distance between stripes of the same colour might provide a discriminator? Dodgson compares the distributions and suggests that the most obvious measure of discrimination is the minimum distances between black stripes and between white stripes. These fall in the ranges 3 – 8 and 5 – 8 respectively.

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[60] As with our discussion of the BMS study, we will disregard the mathematical details of Dodgson's calculations. Our interest is not with the mathematical propriety of the methods used (we leave that to the journal reviewers) but with the practice of deploying them.

Unfortunately, once again the Riley minima are not significantly different from the randomly generated patterns of stripes.

AUTO-CORRELATION

Here what Dodgson looks at are combinations of the same colour and the extent to which they are associated. However, when comparing graphs of the auto-correlation of Riley pictures using 6 colours and a randomly generated pictures with no same colour adjacency, the variability in the distributions smears the graph with indecipherable noise. If there is a discriminator here, it will need to be extracted by strong filtering. That could well result in 'over fitting' the model. Over fitting is a technical term for using the requirements of the model to massage the data until both are aligned.

LOCAL ENTROPY

As a final test, Dodgson looks to the internal structure of the paintings. Using 'windows' of various sizes (ie numbers of stripes), he calculates the zeroth entropy as the windows move across the paintings. The best discriminator was a window size of 24 stripes. Here the measures of variability (overall range) for the H_0 values resulted in a distinct separation of most of the Riley paintings from the random patterns. Riley's paintings clustered towards the low end of variability. However, applying the same rule also placed 5 of the random paintings in the same region as the Riley paintings. Even this refined measure with its 3% 'error rate' is unsatisfactory.

Dodgson's conclusion from all this work is informative.

> On the one hand, we can say that there are indications that Riley's compositions can be characterised mathematically in ways that identify the underlying features. However, it is not clear cut. This leaves a quandary for how to extend this work to more complicated patterns: if we cannot adequately characterise stripes, can we reasonably try to characterise patterns of more complexity? (p 20)

Such sentiments brings home to us a difference in attitude between Mathematics and Sociology in regard to description and classification. Having rigorously tried a number of well-defined rendering devices, Dodgson admits (albeit temporary) defeat. The devices he deploys generate descriptions some of which serve to provide almost exhaustive discrimination of Riley's paintings. However none does so completely. As a result, despite systematic and strenuous effort, he acknowledges that giving a precise and simple description of Riley's paintings has eluded him and that he has failed the task he set himself. We cannot think of a single example in Sociology where an investigator goes to the same lengths to establish the basis of description and classification only to admit failure in the end. Indeed, we cannot, offhand, think of an instance of an investigator admitting failure at all. And that, we suggest, is something all parts of the discipline might do well to reflect on.

5. DISCUSSION

What is impressive about Dodgson's approach is that it does not accept 'the best it can do' is 'good enough'. It holds itself to far more stringent standards for determining the adequacy of its descriptions. We can elicit an initial series of 'tests' for the adequacy of description in professional Sociology from these standards. It is well worth spending a little time reflecting on them and their implications for methodological practice in order to get a better understanding of the challenges a desire for probativeness might sets us.

GOODNESS OF FIT

Dodgson tests his characterisations against a number of statistical measures of Bridget Riley's paintings. These measures allow him to compare how well the different patterns characterise the collection of paintings he looks at. These measures are themselves derived from established mathematical proofs and have been tried and tested in numerous studies. His testing protocol is not idiosyncratic but a conventional one and provides a crisp decision process. Either the calculated result passes the standard test or it doesn't. Even an error rate of just 3% is not acceptable.

Bearman and his colleagues certainly tested and rejected a number of 'tests' but they were not of their data on adolescent relationships. Rather, they tested choice patterns based on simulations derived from their data. These choice patterns were not simply of the homophilic properties which actually were the object of study but also of the characteristics of prior partners. The ≤3 link did produce a spanning tree, but it was produced by 'rewiring' 5% of the simulated data as random selections. What this did was (fairly predictably) reproduce the overall shape of the empirical pattern but with one notable difference, the proportion of small cycles. There were far more short cycles in the re-wired data than the original. This rather than homophily then became their phenomenon. Bearman et al's own description is revealing here.

> Compared with our earlier simulations, the rewired graphs are quite similar to the observed network. This is what we would expect, since we change only 5% of the ties at random, while holding the distribution constant. Consequently, all of the network centralization measures are fit well, as are the reach measures. The difference between the observed number of components in Jefferson and those arising from the simulations is trivial. *The only statistic that is fit poorly is the number of cycles.* The rewired networks have almost twice as many cycles as are observed in Jefferson. Since we observe a spanning tree in the Jefferson network, it is not surprising that rewiring produces redundant ties, which appear here as cycles. Thus rewiring isolates the single structural feature we have to account for—in this case, the absence of cycles. Thus the only puzzle is, Why are they absent? (Bearman et al. 2004 p.73)

In other words, it is the lack of goodness of fit of their simulation which is the analogy to the decision rule Dodgson ran.

With Nettleton et al, we have nothing like the 'tests' offered by Dodgson and Bearman et al. The data themselves are described as "qualitative interviews" with users of the internet. Yet again, it is worth looking at what the authors themselves say about their method.

> Our approach to the analysis of discourse is pragmatic, but is probably closer to the analytic and methodological orientation of discursive psychology than it is to conversation analysis (Wooffitt 2004). This discourse analytic approach outlined by Potter and Wetherell (1994), and in particular their emphasis on 'the rhetorical or argumentative organisation of talk' (1994: 48)

> is one that chimes well with our approach here. As Silverman (1993: 108) points out 'by analysing how people talk to one another, one is directly gaining access to a cultural universe and its content of moral assumptions'. (Nettleton et al . 2005 p. 978)

When we turn to the "rhetorical strategies" which Nettleton et al offer, we find the categories of argumentational accountability are mapped onto the data by exemplification. Excerpts from the interviews are quoted as cases of the relevant strategy. Six strategies are listed but no attempt is made to assess how well the categories fit the cases. What we are pointing to here is the difference between the nature of Dodgson and Bearman et al's descriptions as a type of rendering and Nettleton et al's. Strong or weak, Dodgson and Bearman et al did at least try to estimate goodness of fit for the reader. Nettleton et al make no attempt to do this. We have to accept the performativity of the utterances in the interview conversations just is as they describe it. Now, to be clear, we are not saying Nettleton et al might be misleading us, fudging their data or even inventing it. All we are saying is we have no idea how representative the cases exemplified are (a) of the actual talk's conversational character and (b) of the run of cases of the categories. To put it in the terminology of CA which they would recognise, how tight (and hence distinctive) are the categories in which the cited cases are co-classed and why is that class of strategy used "there"?

Traceability

A second test Dodgson's analysis clearly satisfies is traceability of analysis/description. The search for a characterisation consists in a serially organised set of steps with the transition from one set to another fixed by assessment against one statistical test and the specification of the next. The internal ordering of the steps is given by the orders of the entropy measures and the obvious possible feature of autocorrelation. Each provides a 'possible' characterisation.

A similar stepwise structure is evidenced by Bearman et al in their assessments of the adequacy of the simulation results generated. As we pointed out in our discussion of the analysis, traceability falters (and partially breaks down) in the sociological accounting offered for the pattern of homophily and ≤3 link structure used in the final simulation. To produce a credible sociological account (that is, an account which is motivated by conventional sociological forces and applicable to adolescents as a social category) Bearman et al parachute in an orientation to peer group status articulated as a preference for avoiding "seconds". The attribution of this preference was not part of the set up conditions of the simulations nor is it an abduction from the empirical data. For Bearman et al, it obviously 'makes sense of the data' but at the cost of introducing an explanatory (causal?) entity which does not feature in the aggregative analysis. What we are left with is a description resting upon an "it stands to reason, they would think like this" premise which is used to glue homophily and the described structure to the discovered pattern.

In our discussion of the Nettleton et al study, we emphasised just how "entangled" the presentation of the data and the description offered of it were. This entanglement operates as the paper's own rhetorical/argumentational strategy. We can understand the data excerpts by looking at the categories and understand the categories by looking at the data but with a common sense rather than analytic attitude. A different issue of traceability arises when the paper discusses what it calls 'broader discursive strategies'. Here is an example.

> Whilst these *rhetorics of reliability* capture some of the specificities of the ways in which people account for what they would and would not trust, they

> also have to be inserted within far broader discursive formations designed to accomplish the person generating the account as someone using the internet for health in a manner that is legitimate, appropriate and sensible. This, we suggest, is done in two main ways. First, the parents construct themselves as 'sensible' and 'cautious' users by distancing themselves from 'other people' who they presume use the internet inappropriately. Second, the participants articulate that their use of e-health information takes place within a broader context of healthcare use in general, and with interactions with healthcare professionals in particular. (Nettleton et al 2005, p.984)

It is important to remember the initial rendering of the data categorises the speakers as "users of the internet for health information". The broader rhetorics require these users to be further categorised and their contributions to be motivated by their membership of particular social types. They are found to be speaking as representatives of that type. But no discourse analytic (i.e. conversation analytic or similar) evidence is offered for this designation. Instead, the designation appears to be derived from the family status identifers with which the excerpts are tagged ("Lone mother with daughter", "Couple with two children" etc.). In other words, traceability turns on the coding scheme used to sort the data in the modal transformation not on the internal logic of the structure of the data being described.

Tractability

The final feature of Dodgson's analysis we will pick out is one which is vital to the issue of probativeness. It concerns aspirations for analytic tailoring and the need to cut one's descriptive suit according to one's data-provided cloth. There is simply no point in trying to provide an adequate description for a problem which is wholly intractable to your data. Dodgson counts stripes in Bridget Riley's paintings and want to see if there is a pattern in them which is distinctive. He is not trying to adduce reasons for Riley's choice of stripes or make aesthetic judgements about the 'artistic' quality of one kind of pattern over another. His problem ought to be tractable to his data. Unfortunately, he was not successful — which might be for all sorts of reasons (e.g. he's looking in the wrong place, he hasn't been imaginative enough in formulating his entropy measures or even there is no mathematical measure for the data set's characterisation).

With Bearman et al. we find partial tractability. Suitably "re-wired", the data do bear upon the patterning. Both the survey and the simulation produced spanning trees. Where the wheels on the description come off (if you will pardon the image) is in the explication of the reasons for that pattern. The data Bearman et al have is not sufficient to support the explication given of it. They want to tell us why these adolescents (at this school at this point in time etc.) chose the sexual partners they did. But the data makes no reference to the reasons why individual choices of romantic partner were made. They do have tractability on the purely mathematical aspects of the problem but the aspiration for a sociological account eventually outran the data with the result any analytic plausibility/credibility of the description as a sociological description of this data set collapsed.

With Nettleton et al, the wheels were never on in the first place and so any traction was unlikely (bear with us!). The data consists of transcribed interviews in which users of the internet provide *post hoc* rationalisations for the way they use web sites to look for health-related information. What Nettleton et al want to provide is a framework of accounts which those users are assumed to have for their usage. What the interviews produce are 'occasioned' responses to being questioned about why and how they use the internet to find this information. In this context the quotation from Silverman given in the excerpt from the paper above is very revealing. Paying attention to how people talk to one another may well reveal cultural universes and their moral dimensions. But to know which universes and which moral dimensions, it is important to get a firm analytic grip on who is talking to whom and what it is they think they are talking about. Which sociological categories of social actors are in play and what are their interests, relevances and motives? In addition, the data are not of naturally occurring conversations, they are interviews. The social actors are not members of the same peer group talking to one another, they are interviewee and subject. And the purpose of the conversation is one-way collection (even recording) of data. If there are discoverable rhetorical strategies in play under the analytic stance pursued by CA and adopted by Nettleton et. al.'s version of Discourse Analysis, these must be revealed through detailed analysis of the particularities of conversational features not by the attribution of abstracted reasoning processes such as those concerning decontextualised web-based search procedures.

6. CONCLUSION

We want to end by reflecting on some of the implications for TPP (and perhaps EM more generally) of a concern for probativeness. The most obvious, and perhaps the most contentious, would seem to be the need to increase or at least enhance disciplinary coherence. By this we mean not an even more prominent standardisation of investigative premises but the development of stronger alignment in actual disciplinary practice. Central to this will be an emphasis on discipline-driven problem specification. At present, following the culture of professional Sociology more generally, the selection of investigative problems in EM is driven by personal preference. We are not saying researchers should be 'forced' to take up particular problems but that the space of selection should be shaped by general agreement on the current state and development needs of the discipline rather than opportunism or the predilections of researchers themselves. Shifting professional culture in this way will not be enough on its own but it is a requirement for the adoption of a number of particular practices aimed at facilitating increased probativeness.

Chief among these practices will be the directing of significant research effort into *brigading the current corpus of results* into integrated aggregative programmes (of which TPP might be one). This strategy is precisely what occurred in CA with the emergence of parallel and interlinked programmes on turn taking, categorisation and description, topic organisation and so on. These programmes should:

1. Identify those theoretically specified research questions which have been settled as distinct from those which are still open or unaddressed. It is the latter two on which effort should be concentrated rather than the pursuit of further instantiations of the former;
2. Aggregate research results and specify criteria of additivity so that determinations of relative promise and progress can be made. Assessment of planned project outcomes would place weight on the potential contribution a project might make to increasing the coherence or expanding the boundaries of a programme.

Second, and this is closely associated with the first, will be the need for increased rigour in the *operationalisation of "rendering theorems"*, i.e. the variety of protocols by which

modal transformation => rendering => characterisation

is accomplished. In particular, more emphasis will need to be placed on ensuring initial planning is driven by the requirement for definitive outcomes, on the premises adopted as part of the relevant rendering and on criteria by which success against the initial objectives is to be assessed. This will require increased attention to be paid to:

1. Detailed specification of investigative problems;
2. Determination of the relative appropriateness of chosen data forms to that problem.

Introducing the two sets of practices we have identified in the context of a shift to disciplinary driven problem setting is likely to have a third implication. This will be a *reconfiguration of the research space*. The current categories used to bundle groups of projects (e.g. studies of work, studies of scientific and common sense reasoning, studies of professional practice, Technomethodology, informed ethnography and perhaps even TPP itself) are unlikely to capture the essential features of the programmes of work which emerge from them especially if the driving force of this reconfiguration is response to disciplinary demands and the unique adequacy of programme organising principles rather than recognisable alignment with conventional sociologies and the interests of other disciplines. The themes grounding the reconfiguration will necessarily be framed in what is general across specific renderings as that might be demonstrated through consideration of the detail of cases rather than the particularities of categories of activities such as bench science, police work or management reasoning.

It is here that unique adequacy plays a second role. As Livingston insists, the first role is with regard to sociological descriptions of cases, instances or data. The second has to do with the character of the principles around which programmes of investigation are shaped. Not only should these principles be general enough to catch the commonalities across a corpus of work, they should do so in ways that articulate the distinctive modes of conceptual play motivating that programme. Unique adequacy is at the heart of the methodological basis of probativeness, at both the level of method and the level of the organisation of disciplinary principles. Without the three initiatives we had identified above, we can see little hope of increasing the tractability and traceability of our research nor the goodness of fit of the results obtained to the problems specified. Without them, the reconfiguration of architectonics of the disciplines will be impossible. In the end, it will be only when all of these elements are in place that EM and TPP can claim to be satisfying the essential conditions of probativeness.

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AFTERWORD

Confronted with this monograph, some will ask "Why bother? Why spend time on programmatics when the important thing is to carry out the studies?" Others will be happy for someone else to take up these concerns. Like Eric Livingston, they would rather concentrate on their own investigations. We recognise these feelings and, as we have said many times, it would be foolish to deny such reluctance is almost institutionalised in EM. However, not to think about the reasoning structures used within the discipline at this juncture would be to run a serious risk, in fact a number of serious risks. First, the internal context of the discipline is changing rapidly. Several centrifugal forces are in play which threaten its current structure. CA seems to be on an inevitable trajectory towards co-option or colonisation by Linguistics. Many who ply their EM trade undertaking studies of particular technical disciplines (the informed ethnographies of science, engineering, artistic performance, the professions) are finding homes in those or closely related disciplines. The first and second generation leaders have passed away, are retiring or stepping back for other reasons and are being replaced by many who did not serve their apprenticeship in the foundries of Southern California, Boston and Manchester. It is not that the centre can no longer hold but that increasingly there is no disciplinary centre. Then there are the changes in the external sociological context. Sociology in general remains (relatively) open to innovative ideas but is no longer growing at the rate it was even 20 years ago. As a consequence, there are fewer opportunities to pursue a career within a 'standard' sociological environment (hence the migration mentioned above). Second, the 'fashion' for multiple/multi/cross/inter disciplinary investigations has created opportunities but also possible (and actual) drawbacks especially, given the austerity of EM's mode of sociological reasoning, for EM practitioners. One symptom of the effects of all these internal and external changes is the feeling EM is drifting. Studies accumulate but there is little sense of a positive drive and direction.

Addressing the coring out of the discipline will require much work on many fronts. One of those, we suggest, must be the articulation of the essential structure of EM as a body of modes of sociological reasoning. By this we do not mean yet another version of the story of how Garfinkel assembled the basic components from Parsons, Schutz and Gurswitsch, nor how (in Garfinkel's own description), having come upon the written and signed cheque, Sacks went out and cashed it. What is wanted is a worked through architectonics to help guide and shape the responses to changes of the order we have identified; a framework of how EM is structured now and what further components will be required as the discipline (hopefully) moves forward. Our proposal for a methodology for TPP is a small contribution to this work.

At the beginning of this monograph, we drew a distinction between probative and imaginative disciplines and framed it in terms of the relationship between research problems and the results which address them. We did so merely to gain a foothold from which to hack out enough space in which to place our argument. It is now time to relinquish that distinction, at least in terms of the simplistic manner in which we initially framed it. As we hope we have shown, in EM and TPP methodologies are probative and what is imaginative is the conceptual play through which new and transformative ones are constituted. Conceptual play ranges over choices of epistemology, metaphysics and method and is articulated in the rendering processes we have described. Once enshrined, these choices define the alternative architectonics which currently characterise EM and TPP.

The purpose of an architectonics is to describe the regulated nature of a discipline. These regulations set out the patterns of normative practice which conform to the stipulations laid down within methodological frameworks identified and adopted. Of course, we acknowledge individual investigators may choose not to be bound by those stipulations and hence depart from accepted practices. If such refusal extends to core methodological questions like the grounds for the assessment of claims about the adequacy of descriptions provided, such a strategy poses a problem in forming judgements about the *disciplinary* value of the work undertaken. To be sure, where alternative grounds to those usually accepted are adopted, peers will have to calibrate those alternatives. If no grounds are offered, the question arises as to how to value the work at all, either on its own terms or on those of the rest of EM or Sociology more generally. Collectively following a strategy disattention to these matters runs the not inconsiderable risk of work being assessed as valueless simply because there is no standard by which to value it. If that is the position EM comes to find itself in, it will have ceased to be an alternate discipline to Constructive Sociology.