

**Ethnomethodology, Brandom's Pragmatism and
Ordinary Language Philosophy: A Reflection on the
Status of Formal-Analytic Work**

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Abstract

This thesis examines the relationship between Garfinkel's initiatives and the motivating insights of ordinary language philosophy, in terms of which it aims to give a coherent and philosophically satisfying account of Garfinkel's attitude to "formal analysis" in the study of social life. It diagnoses confusion in the reception of ethnomethodology as stemming from a misconstrual of a central practice of ethnomethodological research: indifference to problems that arise for the analyst. On the face of it, there is much in common between Wittgenstein's critique of philosophy and the idea of a "methodogenesis" (Garfinkel) of problems of formal analysis. Three interpretations suggest themselves: (1) Ethnomethodology relies on ordinary language philosophy for a motivating argument against Durkheimian sociology. (2) Garfinkel's initiatives situate Wittgenstein's critique of philosophy in the broader context of an ethnomethodological critique of formal analysis. (3) At the level of motivating insights, ethnomethodology and ordinary language philosophy are one and the same project. This thesis argues for (3). It approaches the interpretative issue in terms of an analogy between Durkheimian sociology and analytic philosophy of language. Both rely on a rule of method on the following lines: things of interest to the analyst (social facts, meanings) are to be regarded as separable from historic actions. Ethnomethodology and ordinary language philosophy deny such separability. The interpretative task is to clarify the role of criticism of formal analysis in reflection on members' work. This thesis argues that criticism serves to remove formal-analytic obstructions to a member's understanding of practical actions. Brandom's pragmatism is considered as an example. In accordance with Garfinkel's programmatic statements, the project of *Making It Explicit* is regarded in this thesis both as an obstruction to understanding and as a possible subject matter for ethnomethodological research. The overall aim is to rid this kind of two-sided treatment of formal analysis of the air of a paradox.

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INTRODUCTION

Garfinkel, Wittgenstein and Brandom

This thesis addresses a particular difficulty in the interpretation of Garfinkel's (1967, 2002) initiatives. Ethnomethodologists present their work as a radical alternative to mainstream sociology (Lynch, 2016). They depict sociology as it is usually done as formal (or constructive) analysis, i.e. as analysis of constructed models of people's actual activities. Garfinkel's guiding thought is that the formal (modelled) structures are ongoing, contingent, practical achievements of those same activities (Garfinkel, 1991, p. 11). Order is not to be sought in a model of things we do, but in these (whatever) particular things themselves. The difficulty lies in deciding whether Garfinkel is critical of formal analysis, especially where his thought is to study even analytic practices as phenomena in their own right.

The "procedural policy" (Garfinkel & Sacks, 1970, p. 345) of ethnomethodological indifference has been a source of much confusion in the reception of ethnomethodology. It has seemed to some to force the ethnomethodologist to adopt a position paradoxically external to sociology, and by implication perhaps to any form of human activity (see e.g. Giddens, 1976, p. 41). On the other hand, Lynch (1993) compares the sociologist's "infuriated reaction" to being studied to that of the victims of Garfinkel's breaching experiments:

Ethnomethodology's relationship to sociology is difficult to describe and comprehend. Many ethnomethodologists work in sociology departments, so in that sense they are sociologists, but one of their most infuriating research policies has been to place "professional" sociological methods for generating knowledge of social structures alongside the "lay" know-how that is substantively part of the society that sociologists study. As a matter of research policy (if not personal conduct), ethnomethodologists treat the "family concerns" of professional sociology with studious indifference. By treating lay and professional methods as part of the same domain of study, they distance themselves from the disciplinary form of life in which they and their sociologist colleagues conduct their professional affairs. The infuriated reaction by the colleagues down the hall can be no less understandable and no less well founded than the high rage expressed by family members toward the students who performed an ethnomethodological "breaching experiment" by pretending to be strangers in their family households: "Why must you always create friction in our family harmony?"

“I don’t want any more of *that* out of you, and if you can’t treat your mother decently, you’d better move out!” “We’re not rats, you know!” (pp. 1–2)

Arguably, the sociologist wrongly expects the ethnomethodologist to be concerned about the same things – about problems of sampling, questionnaire design, interview technique, and so on (cf. Cicourel, 1964, 1993). Ethnomethodology either ignores such things or takes a radically altered interest in them, seeing no difference between these and other “organizationally situated methods of practical reasoning” (Garfinkel, 1967, p. viii). It is true that sociologists and ethnomethodologists have often been at cross-purposes here. But there is more to the difficulty than that.

What is ethnomethodology’s relationship to mainstream sociology? One way of answering is to consider philosophical interpretations of Garfinkel’s initiatives. There are two interpretative traditions. A school of predominantly British ethnomethodologists has stressed the importance of Wittgenstein’s later writings and Peter Winch’s classic study (Anderson, Hughes & Sharrock, 1984; Button, 1991b; Coulter, 1991, 2001; Sharrock & Anderson, 1985; Sharrock & Dennis, 2008). Others, including Garfinkel himself, draw inspiration from existential phenomenology, often, if not always, giving pride of place to Husserl’s phenomenology of the life-world (Garfinkel & Liberman, 2007; Garfinkel, 2002, 2007; Liberman, 2007; McHoul, 1998). It is surely correct to take account of the philosophical dimensions of Garfinkel’s initiatives when considering ethnomethodology’s relationship to the sociological mainstream. But I am going to argue that an interpretation of Garfinkel’s initiatives must not rely too heavily on philosophy, since it must also be able to regard philosophy as members’ work.

What is the difference between use and mention of philosophical practices in the ethnomethodological literature? At a programmatic level, the ethnomethodologist must regard even philosophy as a collection of language-games (Wittgenstein) or lived work (Husserl). Then what of the “infuriated reaction” of the philosopher to Garfinkel’s initiatives? The ethnomethodologist embroiled in philosophy cannot be indifferent to the philosopher’s own practical concerns. And the ethnomethodologist who relies on philosophy to explain Garfinkel’s initiatives is embroiled in philosophy. Accordingly, we can approach the difficulty I propose to address in terms of the following question: What could it possi-

bly mean to say that the order of a philosophical problem, that its intelligibility or analysability, is an ongoing, contingent, practical achievement?

My overall aim is to clarify Garfinkel's attitude to any species of formal analysis. I work with an analogy between Durkheimian sociology and analytic philosophy of language. Both may be said to assume a separability thesis. The sociologist assumes as a matter of policy that facts about society are separable from facts about individual manifestations of society. Likewise, the philosopher assumes that facts about content are separable from facts about individual manifestations of content. This the philosopher also does as a matter of policy. In the vernacular of analytic philosophy of language, the policy is to make a distinction between semantics and pragmatics. Ethnomethodology is consistent with the insights of ordinary language philosophy, especially those of Wittgenstein's critique of philosophy. It views problems of sociological method as consequences of a version of Durkheim's (1982) basic rule of method: to consider society, not as something done by members of society, but as a synthesis *sui generis* of things done. Instead of attempting to solve problems of sociological method, the ethnomethodologist offers an account of their "methodogenesis" (Garfinkel, 2002, p. 169). Three interpretations of ethnomethodology's relationship to philosophy suggest themselves. Ethnomethodology either (1) relies on Wittgenstein for an argument against Durkheimian sociology, (2) places Wittgenstein's critique of philosophy in the broader context of a critique of formal analysis or (3) converges with ordinary language philosophy at the level of motivating insights. For reasons sketched in the remainder of my introduction, I opt for (3). This is to opt for a purely descriptivist reading of Garfinkel's initiatives, according to which there is nothing in general to learn about practices and no general objection to formal analysis.

My interpretative strategy is to place Wittgenstein between ethnomethodology and an example of formal analysis in contemporary philosophy of language. I focus on Robert Brandom's major contribution to the literature, *Making It Explicit: Reasoning, Representing, and Discursive Commitment* (1994). Brandom reads Wittgenstein as having shown that that semantic analysis must be "firmly rooted in actual [linguistic] practices" (p. xiii). His task is to show that it can be. His decidedly *un*Wittgensteinian approach is to show that there *is* something common to all language-games, in terms of which semantic analysis is after all intelligible. According to Brandom, what is common to all language-games is that

players are able to give and ask for reasons. Ethnomethodology adds the idea that reasoning is specific to a domain of problem-solving activity (Livingston, 2008). Brandom claims that “social practices of giving and asking for reasons” (p. xiv) are necessary and sufficient for content. Ethnomethodologists might be thought to be giving examples: “practices of legal reasoning, conversational reasoning, divinational reasoning, psychiatric reasoning, and the rest” (Garfinkel & Sacks, pp. 345–6). But it is hard to see the relationship between Brandom’s pragmatism and an ethnomethodological study. If Brandom’s pragmatism is a species of formal analysis, and the Wittgensteinian has grounds for criticising Brandom’s pragmatism, then perhaps we can expect to find such grounds *again* in Garfinkel’s initiatives. In fact, there is a difference here between the Wittgensteinian and the ethnomethodologist. The Wittgensteinian can be unreservedly critical of Brandom’s pragmatism. The ethnomethodologist must be able to adopt an attitude of indifference to problems of philosophical semantics, which include the Wittgensteinian’s grounds for criticising Brandom’s pragmatism. There are also practices of “philosophical” reasoning, to be considered now as phenomena in their own right.

Generally speaking, the aim of an ethnomethodological study is not to criticise and reform, but to describe and render intelligible. It is hard to see what this could mean in the case of Brandom’s pragmatism. The ethnomethodologist will want to say two things: (1) that there is nothing common to all “social practices of giving and asking for reasons,” no set of invariant formal structures; but also (2) that there is nothing to correct or reform in the assumption that there *is* something common to all. My idea for this thesis is to clarify Garfinkel’s attitude to any species of formal analysis by ridding (2) of the air of a paradox. Without making an exception to the rule of indifference, it is possible to make an ethnomethodological objection to Brandom’s pragmatism. Simply put, if Brandom’s pragmatism is indeed a species of formal analysis, his response to Wittgenstein is circular. There is something to learn about ethnomethodology from this example.

It may not be obvious to the Wittgensteinian that there is indeed something to oppose in Brandom’s pragmatism. Even Baz (2012), in several footnotes to his defence of ordinary language philosophy, speaks favourably of Brandom’s work. Accordingly, I would present this thesis as an ethnomethodological contribution to contemporary analytic philosophy of language. It puts Brandom’s work in a new light. However, my aim in considering Bran-

dom's pragmatism is not to assess a theory of content, but to call attention to the difficulty involved in using philosophy to clarify Garfinkel's initiatives. It is one thing to use philosophy to clarify Garfinkel's initiatives, another to study (even) the philosopher's practices from an ethnomethodological point of view. The difficulty is to make this difference perspicuous.

The main body of the thesis falls into four chapters. Chapter One explains the ethnomethodological significance of Brandom's pragmatism. I begin by considering a dilemma for the student of social life. There are two basic rules of method: one is the rule of Durkheimian sociology; the other is the rule of ethnomethodological indifference. There is no mediation, nothing in between. How is the student of social life to choose between them (unless this is a matter of personal preference)? An answer is suggested by Wittgenstein's critique of philosophy: read more generally as a critique of formal analysis, Wittgenstein's critique of philosophy deflates the claims of Durkheimian sociology. Much of the chapter is devoted to the difference between conceptual analysis and ordinary language philosophy. Brandom's pragmatism serves as an example of conceptual analysis, and more generally as an example of formal analysis. It is clear that the defender of ordinary language philosophy must be opposed to Brandom's pragmatism. The interpretative question is how Brandom's pragmatism looks from an ethnomethodological point of view.

Chapter Two presents a uniquely ethnomethodological objection to Brandom's pragmatism. I defend two basic claims. First, Durkheimian sociology is susceptible to Wittgensteinian criticism. Second, Brandom responds to Wittgensteinian criticism by means of a broadly Durkheimian conception of practices. The circularity of Brandom's position becomes apparent in light of Garfinkel's revisionary reading of Durkheimian. According to the Wittgensteinian, it makes no sense to abstract an expression from ways of using it and ask for the meaning of the expression itself. As if to acknowledge this, Brandom proposes to describe a practice that suffices for content. But he abstracts the practice from individual manifestations of it. This move is no less susceptible to Wittgensteinian criticism. Garfinkel's reading of "Durkheim's aphorism" is a way of making this point. In particular, it is a way of making a critical distinction between historic uses of language, as these (whatever) particular language-games, and tokens of a practice-type. For Brandom, it is beside the

point how people do the practice of giving and asking for reasons. On Garfinkel's reading of Durkheim's aphorism, a practice *is* these (whatever) ways of doing it.

What is the place of criticism in ethnomethodological reflection? Chapter Three considers the claim that ethnomethodology is a response to criticism of formal analysis in sociology. It begins by setting out an inconsistent triad:

- (1) Ethnomethodology refrains from judging the adequacy of members' practices.
- (2) Analytic practices are members' practices.
- (3) Ethnomethodology does not refrain from judging the adequacy of analytic practices.

How is the contradiction of (3) to be avoided? One solution is to make a distinction between first-order studies of members' work and second-order studies of formal analysis of members' work *as* members' work. Though criticism of formal analysis motivates first-order ethnomethodological studies, it can be put aside in studies of the analyst's own practices. Another solution is to regard findings of formal analysis as special cases of a member's own analytic gloss. I make a case for the second interpretation of Garfinkel's initiatives. It is the more radical of the two in that it entirely separates ethnomethodology from Durkheimian sociology, its asymmetric "incommensurable" alternate.

Chapter Four works out the consequences of this purely descriptivist interpretation of Garfinkel's initiatives. My main concern is to show, by way of illustration, that a distinction due to Winch must be relative to sociological subject matter: his distinction between internal and external identity criteria. This allows us to say of Winch's concerns as a philosopher that they arise for members as concerns about the "identity" of their own actions. I consider various examples, but draw my conclusions from one in particular: a teacher's "formal analysis" of an exchange with a student. The crucial point is that criticism of the teacher's pedagogical practices must not be confused with global criticism of formal analysis. It is local criticism of this single individual instance. More to the point, it is the teacher's own criticism: a constituent feature of this sociological subject matter.

Throughout this study, I rely on the example of the Chicago Jury Project. There are two reasons for this. Since I am not a sociologist, it is impossible for me to give examples of sociology "as it is usually done" with the authority of an advanced student. I remain very

much a beginner in the field. My discipline is philosophy, which is one of the reasons it is so easy for me to be indifferent to quarrels between sociologists and ethnomethodologists. They do not figure in what I want to say. Indeed, I am ultimately indifferent to ethnomethodological criticism (if such a thing is possible) of Brandom's pragmatism, to say nothing of criticism of contemporary pragmatism in general (Emirbayer & Maynard, 2010). For my concern is not to make a point about Brandom's pragmatism, but to think through a purely descriptivist interpretation of Garfinkel's initiatives. I do make a point about Brandom's pragmatism in Chapters One and Two, but only as means to an end. It is in Chapters Three and Four that I think through the implications of what I begin by saying. In any case, instead of relying on half-baked ideas about the work of a sociological study, I keep coming back to the work reported by Kalven and Zeisel in *The American Jury* (1966). This admission of immaturity is an expression of respect for the "infinite tasks" of formal-analytic sociology. Of course, I have at least as much respect for those of analytic philosophy of language, which I can claim, perhaps, to be competent to discuss in general terms.

My other reason for focusing on this one example is less pragmatic. The following text, from Kalven and Zeisel's Preface, provides some interesting historical background to Garfinkel's accounts of jurors' decision-making:

The jury study has one special burden to bear along with all the customary difficulties of large-scale research. At one point one of its research approaches generated a national scandal. As one of several lines of approach, it was decided to obtain recordings of actual jury deliberations, partly to learn whether post-trial interviews with jurors permit reconstructions of the events of the jury room. The move was undertaken, with the consent of the trial judge and counsel, but without the knowledge of the jurors, in five civil cases in the federal district court in Wichita, Kansas. Although extensive security measures were taken to insure the integrity of the effort, when the fact became public in the summer of 1955, there followed public censure by the Attorney General of the United States, a special hearing before the Sub-Committee on Internal Security of the Senate Judiciary Committee, the enactment of statutes in some thirty-odd jurisdictions prohibiting jury-tapping, and for a brief, painful moment, widespread editorial and news coverage by the national press. (pp. vi-ii)

In the mid 1950s, Garfinkel accepted an invitation from Fred Strodbeck to analyse recordings of actual jury deliberations – presumably the Wichita data – and conduct post-trial interviews (Garfinkel, 1974). It was around this time, reflecting on the "indexical" properties of jurors' ways of making decisions, that he formed the idea of an ethnomethodological

study. Kalven and Zeisel present what even a beginner in sociology can immediately recognise as a social fact (in Durkheim's technical sense): a judge-jury disagreement rate of 24.6 per cent in the direction of jury leniency in a sample of 3576 criminal jury trials. Ethnomethodology invites us to consider its explanation – an account of jurors' "actual" practices – as members' work.

In my conclusion, I defend the claim that the motivating insights of ordinary language philosophy are ethnomethodology's own insights. This is not exactly to say that ethnomethodology *is* ordinary language philosophy. The difference lies in the sort of interest we take in imaginary examples. Ethnomethodology involves a kind of reflection in which a philosopher like Travis, for example, excels. But this reflection is not yet ethnomethodology. If the Wittgensteinian is critical of a "craving for generality" in philosophy and sociology, Wittgenstein's critique of philosophy only shows the need for another way of studying language and action.

I end up with a rather austere (even stoical) conception of ethnomethodology. Ethnomethodology does matter if it has ever been important to understand what a person said or did. Ordinary language philosophy is unreservedly critical of the view that we can understand without paying close attention to the particular case. Ethnomethodology is the patient, empirical, purely descriptive study of things people do: a way of understanding.

NOTE

Unless otherwise indicated, page numbers for quotations of Brandom's work refer throughout this thesis to *Making It Explicit: Reasoning, Representing, and Discursive Commitment* (1994).

CHAPTER ONE

The Ethnomethodological Significance of Brandom's Pragmatism

An interpretation of Garfinkel's initiatives would be incomplete without an account of his attitude to "formal analysis" in sociology. At issue is whether Garfinkel's initiatives are responses to an objection to formal-analytic work. What could the objection be?

Garfinkel places language at the heart of sociology's concerns. Societal members use language to do what sociologists study, to (re)produce the order of the social world. Mastery of language is an essential part of their ongoing accomplishment of order, according to the ethnomethodologist (Garfinkel & Sacks, 1970, pp. 345–6; Francis & Hester, 2004, pp. 1–2). It may be useful, then, to consider the relationship between ethnomethodology and contemporary ordinary language philosophy (Baz, 2012; Travis, 2008). Austin (1961a, 1961b) and Wittgenstein (1953) both warn against formal analysis in philosophy, i.e. against any attempt to assimilate diverse uses of language to a single model of meaning. Does Garfinkel warn against formal-analytic sociology? If so, on what grounds? My idea is to address these questions by considering a clear case of formal analysis in contemporary philosophy of language from an ethnomethodological point of view: Brandom's pragmatist theory of content.

My aim in this chapter is to introduce ethnomethodology via an analogy between Durkheimian sociology and Brandom's pragmatism. Both trade on the idea of a "radical disjunction" (Lukes, 1973, p. 213) between things to be discovered, demonstrated and explained (social facts, meanings) and historic actions. In Section 1, I present Garfinkel's own contrastive definition of ethnomethodology. Garfinkel confronts the student of social life with a choice between two basic research approaches: formal analysis and ethnomethodology. I explain the difference with reference the Chicago Jury Project, which I offer as an example of Durkheimian sociology. In Sections 2 and 3, I suggest an analogy between Durkheimian sociology and analytic philosophy of language and give the example of Brandom's project in *Making It Explicit*. Where the sociologist separates a social fact from man-

ifestations of society, the philosopher of language separates a meaning from uses of language. This analogy is especially clear in Brandom's work. In this case, the "Durkheimian" rule of method is to regard a practice that suffices for content as separable from presumptive instances. In Section 4, I consider the problem of Garfinkel's either/or. To opt for Brandom's pragmatism is to opt against ethnomethodology. Why opt for ethnomethodology? An answer may lie in the motivating insights of ordinary language philosophy, as found in Austin and Wittgenstein. I conclude by presenting three interpretations of the relationship between ethnomethodology and ordinary language philosophy. Does ethnomethodology (1) rely on ordinary language philosophy for motivation, (2) place ordinary language philosophy in the broader context of a critique of formal analysis or (3) converge with ordinary language philosophy at the level of motivating insights?

1. Durkheimian Sociology

In *Ethnomethodology's Program: Working Out Durkheim's Aphorism* (2002), Garfinkel defines ethnomethodology in terms of a distinction between two approaches to the study of social life. One, of course, is ethnomethodology. The other is formal (or constructive) analysis. The hallmark of formal analysis in sociology is "generic representational theorizing" (p. 96). The analyst assumes the existence of properties necessarily shared by members of a class of actions. Analysis of actions is done as analysis of these formal properties. The basic rule of method is to regard formal properties as separable from actions that exhibit them. Garfinkel invites us to consider what "escape[s] from accountability" (p. 103) under the jurisdiction of this rule: ways of doing actions. He does not deny that actions exhibit formal properties, but proposes to study formal properties as practical achievements. Properties necessarily shared by a class of actions are now to be regarded as things somehow done by the actors. This calls for a different rule of method: to adopt an attitude of indifference to problems of formal analysis.

Garfinkel's subtitle is elliptical. His project is to provide a "methodogenesis" (p. 169) of problems of formal analysis. Problems that arise for the analyst are now to be regarded, not as problems to be solved, but as consequences of the analyst's approach to the study of social life. Garfinkel calls them "preposterous problems" (p. 91), presumably to stress that that they arise as a result of the practice of separating what people do (formal properties)

from ways of doing it. Instead of trying to solve them, we are to ask how people do what the analyst says (if and when they do). Ethnomethodology means “working out” the idea of formal properties exhibited by actions. It means asking how people produce the order of such things as “norms, directives, regulations, laws, commands, orders, rules, standards, plans, programs, budgets, maps, manuals” (p. 199) and so on. Problems of formal analysis belong to the list. From an ethnomethodological point of view, they constitute the order of the analyst’s own actions. This is what it means to be indifferent to problems of formal analysis. They are grist for the mill. We are to ask how the analyst, in following a basic rule of analytic method, produces the order of his or her own actions: the order of problems the analyst tries to solve.

Garfinkel’s (2002) reading of “Durkheim’s aphorism” is divisive. It presents the student of social life with an either/or: do formal analysis or ethnomethodology instead! To opt for one is to opt against the other. There is no mediation, nothing in between. But this is not a simple opposition. Formal analysis and ethnomethodology are “asymmetrically alternative forms of analysis” (p. 122). Though they approach the study of social life in radically different ways, forcing us to choose between them, they are connected by the idea that actions exhibit formal properties. Are formal properties separable from actions that exhibit them or “embedded” (p. 181) in the work of their production? That is Garfinkel’s divisive question.

The aphorism derives from a passage of Durkheim’s Preface to the Second Edition of *The Rules of Sociological Method*. Durkheim (1982) urges that “objections [to his project] often arise from the fact that one has refused to admit, or not admitted without reservations, our basic principle, that of the objective reality of social facts” (p. 45). Thus: “The objectivity of social facts is sociology’s basic principle” (cf. Garfinkel, 1967, p. vii; 1991, p. 11). Two expressions require elucidation. What are “social facts”? And in what sense are social facts “objectively real”?

Durkheim’s basic principle is a reiteration of the “first and most basic rule” of sociological method: “to consider social facts as things” (p. 60). It says of a class of facts – of facts about society – that they possess a thing-like property: “the remarkable property of existing outside the consciousness of the individual” (p. 51). For Durkheim, society is “a reality *sui generis* vastly distinct from the individual facts which manifest that reality” (p. 54). Parties

to activities that manifest society may be unaware of social facts. They do not produce social facts, not if producing involves intending to produce. They produce actions that manifest something (society) of which a sociological finding is true or false. The finding is true or false, not of actions that manifest society, but of society itself.

In this thesis, I call “Durkheimian” any sociological study that trades explicitly or implicitly on Durkheim’s idea: the idea of the separability of social facts from actions that manifest society (cf. Parsons, 1949; Pope, Cohen & Hazelrigg, 1975). For reasons I explained in my introduction, I rely heavily on the example of the Chicago Jury Project, a sociological study of the 1950s that aimed to make an empirical contribution to a long-standing controversy of legal theory. Kalven and Zeisel review the controversy in the opening chapter of *The American Jury* (1966), where they write:

The Anglo-American jury is a remarkable institution. We have had it with us for so long that any sense of surprise over its main characteristics has perhaps somewhat dulled. It recruits a group of twelve laymen, chosen at random from the widest population; it convenes them for the purpose of the particular trial; it entrusts them with great official powers of decision; it permits them to carry on deliberations in secret and to report out their final judgment without giving reasons for it; and, after their momentary service to the state has been completed, it orders them to disband and return to private life. The jury thus represents a deep commitment to the use of laymen in the administration of justice, a commitment that finds its analogue in the widespread use of lay judges in the criminal courts of other countries. It opposes the cadre of professional, experienced judges with this transient, ever-changing, ever-inexperienced group of amateurs. The jury is thus by definition an exciting experiment in the conduct of serious human affairs, and it is not surprising that, virtually from its inception, it has been the subject of deep controversy, attracting at once the most extravagant praise and the most harsh criticism. (pp. 3–4)

How might social science usefully contribute to an assessment of the jury system? The study made ingenious use of “judge-jury disagreement” in a sample of 3576 cases of criminal law, asking (1a) how often the judge disagreed with the jury’s decision, (1b) whether the jury tended to be more or less lenient than the judge, (2a) what gave rise to disagreement and (2b) how its causes explained answers to (1a) and (1b):

This study seeks to answer two basic questions: First, what is the magnitude and direction of the disagreement between judge and jury? And, second, what the sources and explanations of such disagreement? (p. 55)

Explanations were sought in judges' accounts of how jurors decided particular cases, reconstructed from the data of the study. The basic finding of the study – confirmation of a “liberation” thesis – was that jurors use(d) doubts to permit themselves to ignore legal principles and decide(d) cases on the basis of certain popular sentiments about the law. Jurors do have ways of deciding cases, legally incorrect principles of sentiment (values) in terms of which the magnitude and direction of judge-jury disagreement became intelligible. For example, disagreement could often be explained by the hypothesis that jurors used a legally incorrect *de minimis* principle, according to which the law does not concern itself with trifles (pp. 258 ff.). For the judge, where it mattered only that the defendant had stolen or indecently exposed himself, the jury was reluctant to convict if what the defendant had stolen was of little value to the victim, or if the victim was a sexually experienced adult. An account of disagreement was an explanation if it ranged over crimes of different types, in ways that need not have occurred to the decision-makers.

Facts revealed by analysis of the data are separable, then, from actions that manifest society. It follows that the ordinary member of society (an individual juror) may not know facts that supervene in this way on his or her own actions, or even so much as suspect the existence of relevant social facts. Perhaps with Parson's use of Freud in mind (see Manning, 2005), Kalven and Zeisel allow people to be “unconscious” of their motives:

[T]he jury, in the guise of resolving doubts about the issues of fact, gives reign to its sense of values. It will not often be doing this consciously; as the equities of the case press, the jury may, as one judge put it, “hunt for doubts.” Its war with the law is thus both modest and subtle. The upshot is that when the jury reaches a different conclusion from the judge on the same evidence, it does so not because it is a sloppy or inaccurate finder of facts, but because it gives recognition to values which fall outside official rules. (p. 495)

Jurors' ways of “giv[ing] recognition to values which fall outside official rules” are not examples of know-how. It was not that jurors managed to do something without having to think about it, like the cyclist who cycles without having to reflect on the action of cycling. There were two actions: a decision as done by the jurors; an explanation as done by the sociologists. Jurors were “unconscious” of explanations of their decisions, which served as examples of what jurors typically do.

Lukes (1974) lists four senses in which social facts might be considered as things. Social facts are: “(1) phenomena with characteristics independent of the observer; (2) phenomena whose characteristics can only be ascertained by empirical investigation (that is, as opposed to *a priori* reasoning or intuition); (3) phenomena whose existence is independent of individuals’ wills; and (4) phenomena which can only be studied through ‘external’ observation – that is, by means of indicators, such as legal codes, statistics, etc.” (p. 9, n. 40). By way of illustration, consider the phenomenon of disagreement between judge and jury. Except by means of instruments of sociological investigation, the study had no access to facts about it. It was inaccessible to parties to decisions, most obviously the judges and jury members. Though it was these people who in some sense produced the phenomenon to be studied, they themselves were unable to study it or influence it in any way. The phenomenon had yet to be discovered, and could be discovered only by means of external observation. The basic instrument of the study was the sample of 3576 cases, produced in such a way as to be representative of “the universe of criminal jury trials in the United States” (Kalven & Zeisel, p. 44). Facts were accessible to the sociologists only insofar as the sample was representative, so that “statements which are true for the sample [could] be projected as true for the full universe of trials” (p. 45). The study established empirically what the armchair intellectual could only guess. To make the point, Kalven and Zeisel playfully invite the reader to speculate before reading the results of a preliminary analysis of the data:

No prior expectations exist either among the legal professionals or in legal tradition as to what a proper amount of disagreement between judge and jury should be. We lack a pre-existing context in which to place the measurements. You may find it amusing to make your own private guess and to see whether it overestimates or underestimates the amount of actual disagreement. (p. 55)

In 62.0 per cent of all 3576 cases, judge and jury agreed that the defendant should be convicted. In 13.4 per cent, they agreed that the defendant should be acquitted. This left 24.6 per cent of all 3576 cases. In 2.2 per cent, the jury convicted where the judge would have acquitted. And in 16.9 per cent, the jury acquitted where the judge would have convicted. This left 5.5 per cent. In 1.1 per cent of all 3576 cases, the jury hung. Since a judge may only acquit or convict, this left 4.4 per cent. Thus, the sample revealed a disagreement rate of 24.6 per cent in the direction of jury leniency. This finding was (1) independent of the observer, (2) inaccessible except by means of empirical investigation, (3) independent of

the wills of parties to decisions and (4) observable only indirectly by means of an analysis of the sample. It was now to be explained in terms of jurors' ways of deciding cases, not sloppy or inaccurate but often legally incorrect.

Garfinkel (1967, 1974) asks a radically different question about jurors' decisions. The ethnomethodologist may assume indifferently that a particular explanation is adequate, e.g. that the *de minimis* hypothesis does help explain the disagreement rate of 24.6 per cent in the direction of jury leniency. The question now is how members did what they did, e.g. how jurors did what is properly glossed as "making a decision" (Garfinkel & Sacks, 1970, p. 352). Order is no longer sought in data for a sociological study. It is sought in the doing of something, e.g. in the doing of decision-making. Analysis of aggregate phenomena gives place to analysis of documented actions, where a "documentary method" (Garfinkel, 1967, pp. 76 ff.) was part of the doing of them.

The difference between Durkheimian sociology and ethnomethodology is not a simple opposition. In Durkheimian sociology, the analyst associates "formal structures" with documented actions, which "exhibit upon analysis properties of uniformity, reproducibility, repetitiveness, standardization, typicality, and so on" (Garfinkel & Sacks, 1970, p. 346). The ethnomethodologist does not deny the existence of formal structures, but asks how "particular production cohorts" discovered, created and maintained them in the doing of something, e.g. how particular juries discovered, created and maintained properties of countless documented decisions in the doing of decision-making. Accordingly, the ethnomethodologist takes a radically different view of the actor. As Garfinkel (1967) writes:

Social science theorists – most particularly social psychiatrists, social psychologists, anthropologists, and sociologists – have used the fact of standardization to conceive the character and consequences of actions that comply with standardized expectancies. Generally they have acknowledged but have otherwise neglected the fact that by these same actions persons discover, create, and sustain this standardization. An important and prevalent consequence of this neglect is that of being misled about the nature and conditions of stable actions. This occurs by making out the member of the society to be a judgment dope of a cultural or psychological sort, or both, with the result that the *unpublished* results of any accomplished study of the relationship between actions and standardized expectations will invariably contain enough incongruous material to invite essential revision. (pp. 66–7; original emphasis)

Unlike the analyst of a Durkheimian study, Garfinkel regards the actor as a member of a particular production cohort.¹ Members find themselves engaged in an activity that calls for their attention. They themselves are concerned to do or avoid doing what members “standardly” do, e.g. to decide cases in legally correct ways instead of giving reign to popular sentiments about the law (Garfinkel, 1967, p. 111; 1974, p. 16). By contrast, the analyst regards the actor as a “judgmental dope” who produces an action² that explains a social fact without knowledge of what the action explains. This is fair to the Chicago Jury Project.

Compare Kalven and Zeisel:

A specific hypothesis as the source of judge-jury disagreement, such as in indecent exposure cases, does not assert that the jury is always conscious of this explanation and that all jurors would necessarily give this reason if interviewed as to why they acquitted these defendants. The decision-maker may not be fully aware of his motivation; nevertheless, it makes sense to talk of this or that as the cause of his choice. Moreover, when, as with the jury, it is a group that is making the decision, it will not infrequently be true that not all members of the group will have reached the joint decision for the same reason. (p. 87)

An action that explains a social fact is not intended to be one. The individual juror does not have a social fact in mind, and acts without consciousness of the sociological significance of a way of deciding. Indeed, it is a logical error (a category mistake) to describe the jury as a production cohort. It did not produce a social fact, but only one of the 3576 documented decisions. Conversely, it is absurd to deny that jurors intended the decision. The doing of decision-making is in principle at least implicitly reflexive (Pippin, 1989, p. 21), done with a way of doing of it at least implicitly in mind.

Interest in “the relationship between actions and standardized expectations” (Garfinkel, 1967, p. 67; in context above) radically distinguishes ethnomethodology from Durkheimian sociology. Two basic modes of relationship can be differentiated. Taking any account of what people “standardly” do, the account may be internally related to actions as a practical

¹ Cf. Garfinkel, 2002, p. 183: “Ethnomethodology specifies the local order production staff in its exhibited work properties as a reincarnation of the demographic population cohort, an endogenous population cohort, the congregational population cohort of us, of the local just these of us, in this actual case, doing just what we are accountably doing, and doing just *that* accountable social fact, that thing, in just accountably this case and therein just in any actual case observably and evidently.”

² A token of an action-type.

concern or externally related as an explanation of social facts (Winch, 1990, p. 81). Thus, where an account of what jurors do – the “official juror line” (Garfinkel, 1967, pp. 108–10) – was internally related to documented decisions as a practical concern of actual jury deliberations, an alternative account of the same decisions – the *de minimis* hypothesis – was externally related as a part of the explanation of the disagreement rate of 24.6 per cent of all 3576 cases in direction of jury leniency.

2. Analytic Philosophy of Language

Garfinkel makes a distinction between two approaches to the study of social life: Durkheimian sociology and its asymmetric “incommensurable” alternate (Garfinkel & Wieder, 1992). If, as I have said, to opt for one is to opt against the other, why opt for ethnomethodology?

My idea is to answer by means of an analogy between Durkheimian sociology and analytic philosophy of language. In analytic philosophy as it is usually done, the “Durkheimian” rule of method is to regard meanings as separable from individual uses of language. Analytic philosophy’s own judgmental dope is the ordinary speaker of a language L, whose linguistic performances manifest the language. The analyst abstracts an expression E from utterances and asks for the meaning of E in L. On this analogy, formal properties of documented actions are relations that obtain between meanings, most famously relations between the subject and predicate of analytic statements, which explain the meaning of the subject. Thus, relations that obtain between ‘an only child’ (as subject) and ‘has no siblings’ (as predicate) explain the meaning of ‘an only child’ in English.

It would surprise me to find any philosopher explicitly endorsing Durkheim’s basic rule of method. On the other hand, this characterisation of a way of doing philosophy is no straw man. It explains what it generally meant by “analysis” in the philosophical mainstream, by “conceptual” or “linguistic” analysis. To make this clear, I shall introduce the basic rule of method as a procedure taught to the beginner. Implicit in what philosophers do, it must be made explicit in class. Later in his or her philosophical career, the beginner will not need to be reminded that philosophy is analysis of concepts. Everyone can be expected to know what any competent philosopher knows (Garfinkel, 1967, p. 108).

In *What Does It All Mean? A Very Short Introduction to Philosophy* (1985), Nagel offers the following characterisation of the discipline:

Philosophy is different from science and from mathematics. Unlike science it doesn't rely on experiments or observation, but only on thought. And unlike mathematics it has no formal methods of proof. It is done just by asking questions, arguing, trying out ideas and thinking of possible arguments against them, and wondering how our concepts really work. The main concern of philosophy is to question and understand very common ideas that all of us use every day without thinking about them. A historian may ask what happened at some point in the past, but a philosopher will ask, "What is time?" A mathematician may investigate the relations among numbers, but a philosopher will ask, "What is a number?" A physicist will ask what atoms are made of or what explains gravity, but a philosopher will ask how we can know there is anything outside our own minds. A psychologist may investigate how children learn a language, but a philosopher will ask, "What makes a word mean anything?" Anyone can ask whether it's wrong to sneak into a movie without paying, but a philosopher will ask, "What makes an action right or wrong?" We couldn't get along in life without taking the ideas of time, number, knowledge, language, right and wrong for granted most of the time; but in philosophy we investigate those things themselves. The aim is to push our understanding of the world and ourselves a bit deeper. Obviously it isn't easy. The more basic the ideas you are trying to investigate, the fewer tools you have to work with. There isn't much you can assume or take for granted. So philosophy is a somewhat dizzying activity, and few of its results go unchallenged for long. (p. 5)

It will become important for my argument that there are two ways of reading an account of philosophical inquiry, corresponding to the two modes or extremes of the relationship (internal and external) between actions and norms. Our question (external) might be: "Is what Nagel tells the beginner true of academic philosophy in the United Kingdom today? What about nineteenth century German philosophy? What about twentieth century French philosophy? What about Confucius? Are there alternative conceptions of philosophical inquiry? If so, should we embrace Nagel's conception or some other? Is this the best advice for a beginner?" This is to read Nagel's account as a collection of "docile" instructions (Garfinkel, 2002, p. 199), not currently active in reflection on a philosophical problem.³ However, finding ourselves actually doing philosophy, perhaps in a planned and

³ Cf. Kant, 2006, p. 5: "Even if [the human being] only wants to study himself, he will reach a critical point, particularly as concerns his condition in affect, which normally does not allow *dissimulation*: that is to say,

formal way (in a seminar, at a conference), perhaps spontaneously and informally (in the corridor, at a dinner party), our question (internal) might now be: “Is what Nagel tells the beginner true of *this*, of what we find ourselves doing here and now?” It is unusual for co-conversationalists, even for people doing philosophy, to reflect intensely or for very long on their own actions. Ordinarily, we lose ourselves in the matter at hand (Heidegger, 1962, p. 83, and *passim*). Still, an account of what people standardly do may be “indexical” in this special sense, that it provides linguistic material for “self-explicating colloquy” (Garfinkel & Sacks, 1970, p. 350; Heritage & Watson, 1979). Nagel’s text for the beginner could conceivably be of interest to an ethnomethodologist as an example (cf. Livingston, 1995, 2008; McHoul, 1982; Watson, 2009). For the time being, I want to consider this passage more straightforwardly as an account of philosophical inquiry designed for the beginner.

Unlike the ordinary speaker of L, the philosopher abstracts an expression E from uses of E and asks for the meaning of E in L, as though E had a meaning that could exist apart from uses of E (cf. Durkheim, 1982, pp. 54–5). Consider the concept of time. The beginner learns to regard it as separable from uses of the word ‘time’ in English. Instead of considering its various roles in reasoning about historical events, the beginner is to ask what a word actually means, what it designates as used by speakers of English, what time (its meaning) actually *is*. We can expect the beginner to have difficulty following this rule of method. After all, we are usually satisfied by an account of what a person used E to say, as distinct from an account of the meaning of any standard use of E. The beginner must learn to ask an oddly impractical question. What does E mean? What, for example, does ‘time’ mean in English?

A paradigm case of philosophical inquiry may be of use to the beginner. Danto’s (1964) analysis of the concept of art is as good as any. Philosophers sometimes speak of philosophical theories. Danto speaks of a theory of art. This sounds like science, where there are facts to discover, demonstrate and explain. In analytic philosophy, a theory is an account of how a word is to be used. It explains the (factual) correctness or incorrectness of applications. It is an explanation of the meaning of a word traditionally of interest to philosophers, and is intended to clear up problems surrounding its use. Danto reflects on the

when the incentives are active, he does not observe himself, and when he does observe himself, the incentives are at rest.”

history of the concept of art. Until quite recently, and stretching back to Plato and Aristotle, it implied that the object called art was in some sense mimetic, and the imitation theory of art (IT) was true. Now it applies to objects as they are, without any implication of mimesis, requiring a realistic theory of art (RT). There must be a theory of the concept of art, an explanation of the meaning of the word 'art' in English, that allows for the historical transition from IT to RT and acknowledges the possibility of other (future) theories. For we see in the art of the twentieth century that anything goes. A person may still have applied the concept incorrectly, even though anything goes, calling art what is not art, or not calling art what is. How can the philosopher cope with this?

Danto makes a helpful distinction between the speaker's practical mastery of the concept up for analysis and the result of the analysis itself. Generally speaking, the result of philosophical analysis is a "real definition" of a troublesome word. The analyst's aim is not to correct ways of using it, but to infer a way of using it – the standard way – from relations that obtain between meanings. Is mimesis necessary for art, for example? Is justification necessary for knowledge? Is knowing what it is like sufficient for consciousness? The speaker is a judgmental dope of a semantic sort, a person who may never have thought of such questions, and could not be expected to answer them. Analysis is called for: a theory of the concept, an explanation of the meaning of a philosophically troublesome word. The explanandum is what the speaker knows, yet cannot say, about the concept up for analysis. How does the speaker apply it? How *is* it correctly applied? The analyst's aim is to make this, what the speaker knows, propositionally explicit:

It is, of course, indispensable in Socratic discussion that all participants be masters of the concept up for analysis, since the aim is to match a real defining expression to a term in active use, and the test for adequacy presumably consists in showing that the former analysis applies to all and only those things of which the latter is true. The popular disclaimer notwithstanding, then, Socrates' auditors purportedly knew what art was as well as what they liked; and a theory of art, regarded here as a real definition of 'Art', is accordingly not to be of great use in helping men recognize instances of its application. Their antecedent ability to do this is precisely what the adequacy of the theory is to be tested against, the problem being only to make explicit what they already know. (pp. 571–2)

What is the beginner to see in this paradigm case of analytic philosophy? The analyst (Danto) considers examples of applications of a concept (art): what a speaker might say about Warhol's Brillo boxes or Rauschenberg's bed (where anything goes). But he considers

examples only to ask for an account of a concept. He abstracts an expression from cases for the application of the concept and asks for the meaning of the expression itself. This fits in with Nagel's advice for the beginner. The art historian asks why Duchamp forsook art as it had previously been done and had a urinal put on display. Danto asks, "What is art?"

My own aim in giving this example is not to submit or assess a definition of philosophy, but to suggest an analogy between Durkheimian sociology and analytic philosophy of language. The basic rule of conceptual analysis (a species of formal analysis) is to regard the meanings of philosophically troublesome words as separable from examples of ordinary uses. Analysis concerns what the speaker knows yet cannot say: relations that obtain between meanings. Ordinary uses are governed by these formal relations and explained by them. The speaker, though subject to them in speaking, cannot be expected to know what they are. Analysis is called for. In these ways, conceptual analysis resembles Durkheimian sociology. It discovers, demonstrates and explains facts that supervene on actions: socio-semantic facts.

Analytic philosophy of language, as I propose to understand that heading, is analysis of concepts of linguistic⁴ meaning. A paradigm case is Russell's (1905) theory of descriptions. The speaker will be happy to say that a word or phrase denotes an object. But what is denotation? What does it mean to say of a word or phrase that it denotes, describes or signifies? The speaker (a judgmental dope of a semantic sort) cannot be expected to know. For this is to ask about the formal properties of expressions (socio-semantic facts), which the analyst regards as separable from uses of expressions. Frege's name is synonymous with the principle that a word has a meaning only in the context of a sentence. The analyst studies rules for combining words and constructs theories designed to give the meanings of an arbitrary sentence in the object language. The aim of these theories, as Dummett suggests (1993), is to represent the speaker's practical knowledge of the language as a whole:

What is it that a speaker knows when he knows a language, and what, in particular, does he thereby know about any given sentence of the language? Of course, what he has when he knows the language is practical knowledge, knowledge how to speak the language: but this is no objection to its representation as propositional knowledge; mastery of a procedure, of a conventional practice, can always be so represented, and, whenever the practice is complex, such a representation often pro-

⁴ This excludes, for example, concepts of the meaning (or meaningfulness) of a piece of music.

vides the only convenient analysis of it. Thus what we seek is a theoretical representation of it. (p. 36)

A theory of meaning is an account of what the speaker knows without being able to explain, e.g. that the meaning of 'Snow is white' in English is that snow is white.

There are two overlapping areas of controversy in analytic philosophy of language that deserve special mention. The first concerns the very idea of a theory of the semantics of a natural language. To quote Fodor and Lepore (1992):

Philosophers have traditionally disagreed not only about what the right theory of meaning is for a natural language (for English, as it might be), but even about what kind of theory a theory of meaning for a natural language ought to be. Correspondingly, a traditional project in the philosophy of language is to provide a general, abstract characterization of meaning theories for natural languages; to make clear, in particular, what form they should take and what conditions of adequacy they should be required to satisfy. (pp. 59–60)

Semantic analysis of an English word falls short of a theory of the semantics of English. Think of Hume's analysis of the idea of a causal relation. Hume makes a semantically relevant distinction between customary conjunction (*is*) and necessary connection (*ought*). But a semantically relevant distinction is not yet a theory of the semantics of a natural language. A theory of the semantics of English is meant to give the meaning of an arbitrary English sentence. For example, it must give the meaning of the sentence 'Fire produces heat' in English. The task here is not to say what a causal relation is supposed to be, but what a theory of meaning for the English language is supposed to be.

The other area of controversy concerns the relationship between meaning and use. In another text for the beginner, Kearns (2000) presents the standard view:

The study of linguistic meaning is generally divided in practice into two main fields, semantics and pragmatics. Semantics deals with the literal meaning of words and the meaning of the way they are combined, which taken together form the core of meaning, or the starting point from which the whole meaning of a particular utterance is constructed. Pragmatics deals with all the ways in which the literal meaning must be refined, enriched or extended to arrive at an understanding of what a speaker meant in uttering a particular expression. (p. 1)

The basic rule of analytic method is to regard the meaning of an arbitrary sentence of the object language as separable from uses of the sentence. The separable meaning is the meaning of the sentence itself, as distinct from any of its pragmatically refined, enriched or

extended meanings. Kearns invites the beginner to consider the sentence ‘I forgot the paper’:

Semantics provides the literal meaning of the elements *I*, *forget*, past tense, *the* and *paper*, and the meaning drawn from the order of the words, giving very approximately ‘The person who is speaking at some time before the time of speaking forgot a particular item which is a paper’. Pragmatic considerations flesh this out to a more complete communication. (p. 1)

Kearns goes on to give two examples, or sources of examples, of pragmatic rather than semantic phenomena. On one occasion, a speaker says ‘I forgot the paper’ to her flatmate as she returns from a trip to the shop. On another occasion, the speaker is a police detective constructing a theory from crime scene clues. Kearns tells the beginner that a sentence has a meaning that remains “fairly constant from one occasion of use to the other” (p. 2). But for the advanced student of analytic philosophy of language, the exact relationship between semantics and pragmatics is still unclear (Bach, 2004; Carston, 2002; Davidson, 2005a; Dummett, 1986; Recanati, 2004; Travis, 2008).

As we are just about to see, a philosopher who says that meaning is use does not necessarily deny the basic rule of analytic philosophy of language: “to consider [meanings] as things” (Durkheim, 1982, p. 60). Consider the claim that the meaning of a word depends on a way of using. The rule is to regard a way of using a word as separable from instances. The speaker is a judgmental dope of a pragmatic sort, a person with no knowledge of what confers the content of a concept of an act of speech. Semantic analysis takes a practice as its object. The meaning of a word caught up in a practice *is* the practice in which it is caught up. Meaning *is* use. But the same rule applies. The analyst abstracts a practice from uses of language and asks about the formal properties of the practice itself.

3. Brandom’s Pragmatism

I have suggested an analogy between Durkheimian sociology and analytic philosophy of language. The analogy may seem forced where the analyst is concerned simply to explain a meaning, making no mention of actual practices except to question their semantic relevance (e.g. Davidson, 2005b, pp. 111–2). But in the case of Brandom’s explicitly pragmatist theory of content, the rule of formal analysis (Durkheim) is clear. Uses of language are expected to exhibit the formal properties of a content-conferring practice. Of ethnometh-

odological interest here is once again “the relationship between actions [uses of language] and standardized expectations” (Garfinkel, 1967, p. 67; in context above). Brandom asks what speakers of a language must be capable of doing to qualify as speakers at all. His aim is to show what is necessary and sufficient for content. He abstracts a particular practice from uses of language – the practice of giving and asking for reasons – and asks two questions: Do sign-producing creatures incapable of producing it qualify as speakers? Does it suffice to confer content on expression caught up in it in suitable ways? My idea is to clarify Garfinkel’s attitude to formal analysis by considering Brandom’s pragmatism from an ethnomethodological point of view.

Brandom is a difficult philosopher. He combines an interest in the history of philosophy, particularly in the Kantian tradition (Brandom, 2008a, pp. 201–2), with commitments both to analytic philosophy of language and to pragmatism broadly defined (Brandom, 2008b, p. 13). He acknowledges the primacy of actual practices, but is not led by pragmatist commitments to deny the legitimacy of analytic philosophy of language. Instead, he sets himself the task of grounding semantic theory in accounts of speakers’ actual activities. This he does against the historical backdrop of a reading of Hegel (Redding, 2007), whose response to Kant he reads as prefiguring a pragmatic reframing of problems of meaning (Brandom, 2000, pp. 33–4; 2002, pp. 31–2). He criticises the classical pragmatists for neglecting the dimension of human activity which distinguishes us from the rest of the animal kingdom: our free acknowledgement of norms of rationality (Bacon, 2012, p. 171). Then, advancing from Kant and Hegel, he looks for ways of understanding meaning-constitutive norms of rationality as socially instituted – not, in other words, as transcendental structures of cognition, but as ongoing accomplishments of social life (Schatzki, 2001, p. 49; Rouse, 2001, p. 190; Wanderer, 2008, p. 203). In *Making It Explicit*, he develops Rorty’s (1979) critique of the representationalist assumptions of modern philosophy. But he breaks with Rorty in attempting to elucidate the concept of representation in pragmatic terms (Brandom, 2011, p. 197). In work produced after the publication of *Making It Explicit*, he calls his position “analytic pragmatism” (Brandom, 2008b, p. 13). This is perhaps the best overall term, since it covers the many technically specific senses in which he describes himself as pragmatist.

An account of the all-encompassing system of *Making It Explicit* is beyond the scope of the present study. In what follows, I introduce some themes of Part I of the book, where Brandom introduces his “core theory” (p. xxii): a model of a practice in which conceptual norms (explanations of meanings) are implicit. He begins his Preface with this candid self-interrogation:

This book is an investigation into the nature of *language*: of the social practices that distinguish us as rational, indeed logical, concept-mongering creatures – knowers and agents. This is of course a topic that has been much explored by philosophers, both the mighty dead and the ablest contemporary thinkers. Surrounded as we are by the riches they have bequeathed, it is hard to avoid asking why one should bother reading – let alone writing – yet another such work. (p. xi)

Why bother? Brandom reminds the reader of the influence of Wittgenstein’s critique of philosophy:

One of the overarching methodological commitments that orients this project is to explain the *meanings* of linguistic expressions in terms of their *use* – an endorsement of one dimension of Wittgenstein’s pragmatism. For although he drove home the importance of such an approach, other features of this thought – in particular his theoretical quietism – have discouraged his admirers from attempting to work out the details of a theory of meaning or, for that matter, of use. One result has been a substantial disjunction between *semantic* theorizing (about the sorts of contents expressed by various locutions), on the one hand, and *pragmatic* theorizing (about the linguistic practices in which those locutions are employed), on the other. (pp. xii–iii; original emphasis)

I am going to use Garfinkel to show that the motive of Brandom’s response to Wittgenstein begs the question. Garfinkel helps us see that Wittgenstein’s critique of philosophy is a critique of Brandom’s basic rule of method, that of the separability of what is necessary and sufficient for content (a content-conferring practice) from historic uses of language.

Brandom reads Wittgenstein as establishing criteria of adequacy for a theory of norms that govern linguistic performances. (Compare the passage reproduced above from Fodor and Lepore [1992, pp. 59–60].) He admits that Wittgenstein would not have endorsed the project of *Making It Explicit*, but sees no reason for theoretical quietism:

Wittgenstein, the principled theoretical quietist, does not attempt to provide a theory of practices, nor would he endorse the project of doing so. The last thing he thinks we need is more philosophical theories. Nonetheless, one of the projects pursued in [*Making It Explicit*] is to come up with an account of norms implicit in practices that will satisfy the criteria of adequacy Wittgenstein’s arguments have established. (pp. 29–30)

On Brandom's reading of *Philosophical Investigations* circa §201, Wittgenstein shows that the intelligibility of semantic analysis depends on a theory of norms implicit in what speakers do. His master argument is an argument from the regress of interpretations of rules:

The conclusion of the regress argument is that there is a need for a *pragmatist* conception of norms – a notion of primitive correctnesses of performance *implicit in practice* that precede and are presupposed by their *explicit* formulation in *rules* and *principles*. [...] There is a kind of correctness that does not depend on explicit justification, a kind of correctness of practice [...] The regress argument does not by itself provide such a conception of proprieties of practice; it just shows that without one we cannot understand how rules can codify the correctnesses that they do. (pp. 21–2; original emphasis)

Wittgenstein sets the task of explaining the bindingness of a rule of interpretation. By way of illustration, consider the Russellian analysis of sentences like (1):

- (1) The present King of France is bald
- (2) There is one and only one entity that is F and that entity is G
- (2') $\exists x(Fx \ \& \ \forall y(Fy \rightarrow y=x) \ \& \ Gx)$

What permits the analyst to apply Russell's procedures for interpreting definite descriptions? According to Brandom, (2') is to be read as a codification of norms implicit in what speakers do. The Russellian analysis elaborates rules that make these norms explicit, and shows what speakers must be able to do for (1) to have the formal properties of (2'). Brandom's name for this explanatory obligation is "methodological" pragmatism (p. 592; 2008a, p. 4; 2011, p. 58).⁵

Brandom has a memorable slogan for his project: "Semantics must answer to pragmatics" (p. 83). But he has three significantly different versions of the distinction between semantics and pragmatics.

⁵ Or perhaps "semantic" pragmatism. See Brandom, 2011, p. 61: "[Considered] just as *noises* – apart from the way we use them, the role they play in practices – our utterances do not *mean* anything. The noise 'horse' could mean anything (or nothing) at all, depending on how it came to be used. This truism at least motivates a methodological requirement on the semantic theorist: that whenever she associates with expressions some semantically relevant *whatsis* [sic] as its content or meaning, she undertakes an obligation to explain what it is about the use of that expression that establishes in practice an association between it and the semantically relevant *whatsis*."

1. In some places, Brandom makes a fairly conventional distinction between semantic and pragmatic theory. (See pp. 158–9.) Semantics concerns itself with meanings of different kinds: definite descriptions (Russell, 1905), subjunctive conditionals (Goodman, 1983), indexicals (Kaplan, 1989; Perry, 1979), and so on. Pragmatics concerns itself with practices in which content-bearing items are caught up. Content-bearing items include intentional states (intending, believing, desiring), normative attitudes (taking a performance to be correct or incorrect), speech acts (especially the act of asserting) and, of course, the words, phrases and sentences of a natural language. There are two main approaches to pragmatics. One focuses on conventions in terms of which an act of speech is appropriate or inappropriate (Austin, 1962; Searle, 1969; Sadock, 2004), the other on conversational heuristics (Grice, 1989; Horn, 2004). Intentional states are content-bearing items, and Brandom wants to explain content. To avoid circularity, he must not use intentional states to identify or analyse the practice that is meant to be necessary and sufficient for content. So he opts for a sophisticated version of speech-act theory (p. 7; pp. 146–7).

2. Brandom (pp. 143–5) makes a further distinction between formal and philosophical semantics. The point of formal semantics is to represent a speaker’s mastery of the language as whole. The point of philosophical semantics is to explain semantic concepts, including the concept of content (meaning) itself.

3. On the standard view of the relationship between semantic and pragmatic theory, pragmatics must answer to semantics. Pragmatics is the study of the refinement, enrichment and extension of literal meanings. It must answer to a theory (semantics) of what comes to be refined, enriched and extended in use. Brandom’s slogan is not to be read as a repudiation of the standard view. On the contrary, it lays down a rule for explaining the idea of a literal meaning. Accordingly, Brandom (pp. 158–9) makes still another distinction between *inferential* practices of two kinds: *semantic* inferential practices and *pragmatic* inferential practices. Literal meanings are explained by rules that codify norms of *semantic* inferential practices. Uses of language that refine, enrich and extend literal meanings are explained by *pragmatic* inferential practices.

What Brandom typically means by “pragmatics” is the study of norms implicit in what speakers do. Semantics must answer to the study of norms that explain meanings, since they govern performances in the context of which expressions mean what they do. It must

answer to an account of the normative structure of any linguistic performance. Brandom's own rule of method – his “Durkheimian” rule – is to regard this structure as separable from presumptive instances. He abstracts it from individual uses of language and asks how it explains the meanings of expressions of various kinds (“the rich variety of kinds of content that philosophers of language have revealed and revelled in” [p. xiii]).

Brandom's abiding concern in *Making It Explicit* is to explain the concept of representation. A word is said to represent items of extra-linguistic reality. ('Cat' represents cats.) A sentence is said to represent a fact. ('A cat is a mammal' represents that a cat is a mammal.) What is representation? Brandom approaches this problem of philosophical semantics in terms of the inferential dimension of linguistic meaning, to be regarded now as a source of norms implicit in what speakers do. His central claim is that the practice of giving and asking for reasons is necessary and sufficient for content. It is necessary in the sense that members of a “group of interacting organisms” (p. 644), to qualify as speakers, must be able to produce it. It is sufficient in the sense that norms implicit in any local co-production, made explicit in the form of rules, provide for the analysis of meanings of various kinds. Since meanings are irreducibly representational (pp. 495–6), Brandom needs to be able to explain representation in terms of what practitioners do. His solution is to describe a “social route” from reasoning to representing. Consider the problem of ambiguity in ascriptions of propositional attitudes (one of Frege's puzzles). Jimmy believes that Superman wears a red cape. What about Clark Kent? Lois knows what Jimmy could scarcely suspect, that Clark Kent is none other than Superman. Her perspective on what Jimmy says differs from his own. There is interplay, then, between the inferential dimension of semantic content (what follows from what) and the social dimension of the practice of giving and asking for reasons (who is committed or entitled to say what). In the climactic chapter of his study, Brandom will use this interplay to explain representation. Roughly, to say of a word or phrase that it represents is to regard it as index of shifts of perspectives on the correctness of an inference between parties to the practice of giving and asking for reasons, and to say of a sentence that it represents is to say that a speaker may or may not be entitled to use it to assert something (what it says). The speaker is a judgmental dope of social-perspectival sort, who knows nothing of the pragmatics of any of this.

Brandom separates meaning from use by separating norms that explain meanings from individual uses of language. Norms implicit in the practice up for analysis explain meanings by way of algorithmic elaboration of rules that makes them explicit. They are (or are expected to be) sufficient for content. If they are also necessary for content, they are implicit in “actual practices of producing and consuming speech acts” (p. xiii). Brandom’s way of showing that they are is to ask a decidedly *un*Wittgensteinian question. It requires some setting up.

In the section just before the celebrated examples of games, Wittgenstein (1953) replies to an ill-conceived objection to his use of the idea of a language-game. On Wittgenstein’s view, language is essentially part of an activity to be considered as a whole, an activity “consisting of language and the actions into which it is woven” (§7). A language-game is a transparent example. It helps us see what words mean by showing a possible use of words. It also helps us see that confusion may result from separating language from language-games and considering it on its own. His interlocutor objects: “But what is common to all these activities, and what makes them into language or parts of language?” Wittgenstein replies:

Instead of producing something common to all that we call language, I am saying that these phenomena [language-games] have no one thing in common which makes us use the same word for all, – but that they are *related* to one another in different ways. And it is because of this relationship, or these relationships, that we call them all “language”. (§65; original emphasis)

He explains with the example of games (§66). What is common to all games? What makes us use the same word for all? Wittgenstein rejects this question, asking instead why there must be something common. If one resembles an other, the other a third, and the third a fourth, why expect to find something present in them all, a single defining property? Why not accept that there may simply be a “network of similarities overlapping and criss-crossing: sometimes overall similarities, sometimes similarities of detail”?

Brandom casts “admirers” (p. xii; in context above) of Wittgenstein’s question as semantic pessimists (2008a, p. 7). Why not assume instead that there *is* something common to all uses of language, even something that explains the meanings of a speaker’s words? This will strike the Brandomian as a bold and interesting question, more likely to produce

clarity than Wittgenstein's. At worst, it will at least reveal the limit of "algebraic" rather than "hermeneutic" analysis (Brandom, 2008a, pp. 211–6).

Brandom proposes to explain meanings in terms of the invariant normative structures of any "specifically linguistic" social practice. His theory has three distinct stages:

The explanatory strategy pursued here is to begin with an account of social practices, identify the particular structure they must exhibit in order to qualify as specifically *linguistic* practices, and then consider what different sorts of semantic contents those practices can confer on states, performances, and expressions caught up in them in suitable ways. The result is a new kind of conceptual-role semantics. It is at once firmly rooted in actual practices of producing and consuming speech acts, and sufficiently finely articulated to make clear how those practices are capable of conferring the rich variety of kinds of content that philosophers of language have revealed and revelled in. (p. xiii; original emphasis)

Brandom makes a distinction between pre-linguistic social practices (those perhaps of "chimpanzees, dolphins, gaseous extraterrestrials, or digital computers" [p. 4]) and specifically linguistic ones. Things only speakers do – actual practices of producing and consuming speech acts – must exhibit a particular structure. Brandom will show that they exhibit the structure of the practice of giving and asking for reasons. The result of his analysis is a pragmatist theory of content. It explains meanings in terms of norms implicit in presumptive instances, which it makes explicit and elaborates algorithmically into the rules of a new kind of conceptual-role semantics.

The first step is to give an account of social practices. Brandom resorts to a familiar metaphor, which he combines with an idea due to Lewis (1979). "Social practices are games," he says (p. 166). Our ways of doing things on particular occasions are moves in games. They exhibit socially instituted deontic statuses of two kinds: commitments and entitlements. A person sneaks into a theatre without a ticket. This was a move in a game. There was a commitment to show the usher a ticket. There was the entitlement only ticket-holders had on entering. There was also vigilance on the part of the usher, who attributed the entitlement to some people (to those who were able to produce a ticket) but not to others. The usher, then, was keeping track of deontic statuses. Specifically linguistic social practices are also games. Our ways of using expressions on particular occasions are moves in games. There are commitments and entitlements. Speakers undertake commitments and demonstrate entitlements in or by saying (asserting) something. They keep deontic score.

“It’s Tuesday,” says one to another, and other takes the speaker to be committed to the content of what was just spoken (the sentence). If today is Tuesday, tomorrow is Wednesday. So the speaker has undertaken another commitment, a commitment to the content of what will be spoken tomorrow: “It’s Wednesday.” According to the calendar, today is Tuesday. So the speaker is entitled to the content of what was just spoken, and what will be spoken tomorrow. And so on.

The second step is to identify the structure of any specifically linguistic practice. Brandom (p. 172) suggests that it must be part of what speakers do that they are able to make assertions. Otherwise, they would not be able to ask questions, give commands or anything else. He now asks what speakers must be able to do to make assertions, and defends the claim that they must be able to play the game of giving and asking for reasons. In more than one place (1995a, 1997), he invites the reader to imagine a parrot trained to make the sound of the word ‘red’ when and only when a red-coloured object enters its visual field. Does it say of the object that it is red? In other words, are we entitled to attribute a commitment to the content of an assertion? Not unless it is able to demonstrate its entitlement to the saying of it, by playing the game of the giving and asking for reasons.

The third step is to explain meanings of various kinds in terms of the normative structure of any specifically linguistic practice: the invariant structure of the practice (game) of giving and asking for reasons. It occupies Brandom in Part II of *Making It Explicit*, where he shows “what [his] model can do, what it is good for” (p. xxiii). His analysis is guided by one problem in particular, which I have already mentioned: the problem of representation. What is representation? This question is not for the beginner, but for the advanced student of analytic philosophy of language. Still, the rule of method should by now be clear. Brandom regards meanings as separable from uses, and proposes to explain them in terms of the formal properties of a content-conferring practice. The question, then, is how his project looks from an ethnomethodological point of view.

4. Ordinary Language Philosophy

Why opt against formal analysis for ethnomethodology? The answer may lie in the motivating insights of ordinary language philosophy. Consider, then, the following claims:

- (A) Questions of meaning do not arise, and cannot be settled, at the level of expressions, but only at the level of historic uses of language.
- (B) As for expressions, so for rules that give the meanings of expressions.
- (C) There may be nothing common to all linguistic phenomena, nothing that makes us use the word 'language' for all.
- (D) There is a difference between tokens of an action-type (speech acts) and historic uses of language.

I shall introduce these claims, via the exposition of the previous section, as positions taken by Austin and Wittgenstein and regarded as unduly pessimistic by Brandom. Later, the question can then be put whether Garfinkel's initiatives yield an objection to Brandom's project, considered as an example of formal analysis.

(A) Questions of meaning do not arise, and cannot be settled, at the level of expressions, but only at the level of historic uses of language.

It is easy to see why Brandom needs to explain the concept of representation. If, as Lewis (1972) says, "[s]emantics with no treatment of truth conditions is not semantics," (p. 169; cf. Travis, 2008. p. 87), and the task is to explain what it means to say that sentences have semantic content, then it is part of that task to explain what it means to say that sentences have truth conditions. Brandom begins with interplay between the inferential dimension of semantic content (what follows from what) and the social dimension of the practice of giving and asking for reasons (who is committed or entitled to say what), which gives him things of which a predicate is true or false. He understands truth in terms of discursive entitlements. From Lois' perspective, but not from Jimmy's, Clark Kent wears a red cape if Superman does. It is true that he does (believable, sayable, assertible) for the same reasons.

Instead of asking whether Brandom succeeds in explaining reference and truth, we could opt instead to consider the task he sets himself methodogenetically (Garfinkel, 2002, p. 169). It is a consequence of a "Durkheimian" rule of method. The task itself takes two separations of meaning and use for granted. First there is the separation of the meaning of a sentence from individual uses of language. Then there is the separation of norms implicit in any specifically linguistic practice. The task is to explain a feature of meanings, their

truth-evaluability, in terms of an invariant normative structure. Should we accept these separations of meaning and use? If not, there is little point in asking whether Brandom succeeds in explaining truth and reference. The task itself may be ill-conceived.

Austin (1961a) makes a methodogenetically relevant point about what we ordinarily regard as being true or false. Does it make sense to ask (as we do in philosophy) for conditions for the truth of a sentence? Austin thinks otherwise. He argues from linguistic evidence that questions of truth do not arise, and cannot be settled, at the level of sentences, but only at the level of a person's use of words. A person's use of words is a statement, an act of saying something by means of a sentence. Crucially, "[a] statement is made and its making is an historic event, the utterance by a certain speaker or writer of certain words (a sentence) to an audience with reference to an historic situation, event or what not" (pp. 87–8). Brandom's second separation of meaning and use is a separation of norms that explain meanings from "historic" uses of language, i.e. from a person's use of words on a particular occasion for speaking. If Austin is right, it makes no more sense to consider a practice on its own, apart from uses of language, than it does to consider a meaning on its own. Arguing against the primary separation of semantic theory, Austin points out that conditions for the truth of a sentence (for the truth of what a person says by means of a sentence) are dependent on the circumstances of an occasion for speaking: "The *same* sentence is used in making *different* statements (I say 'It is mine', you say 'It is mine'): it may also be used on two occasions or by two persons in making the *same* statement, but for this the utterance must be made with reference to the same situation or event" (p. 88; original emphasis). Against the secondary separation of methodological pragmatism, we must ask whether the same practice is used in doing different things. Compare jurors and philosophers. If jurors deciding cases are producing the practice of giving and asking for reasons, are philosophers explaining meanings doing the same thing? Does it make sense to separate a practice from the doing of it here or there? Not if Austin is right. So before trying to explain representation, it is surely worth considering the task Brandom sets himself from an ethnomethodological point of view.

(B) As for expressions, so for rules that give the meanings of expressions: questions of meaning do not arise, and cannot be settled, at the level of rules of interpretations, but only at the level of historic uses of language.

Austin's recommendation, properly adjusted to the secondary separation of methodological pragmatism, is to think of a practice as the (historic) doing of a practice. Take the practice of asserting p , where ' p ' stands for an arbitrary proposition. If it makes no sense to abstract a sentence from individual uses of language and ask for conditions for the truth of the sentence itself, we should always bear mind that a person will have done what is properly glossed as "asserting p " in some historically particular way. To ask what a person who "asserted p " actually did, what *was* the practice of asserting p , is to ask for an account of a person's use of a practice.

Brandom does not deny that assertions are historic events. But he takes the decidedly *un*Wittgensteinian view that there is something common to all assertions, something that permits us to describe them all as such. More systematically, he takes the view that a game qualifies as a language-game only if it involves assertions: only, then, if it exhibits the formal properties of any assertion at all. The formal properties of an arbitrary assertion are norms implicit in a content-conferring practice. Like social facts, they "possess the remarkable property of existing outside the consciousness of the individual" (Durkheim, 1982, p. 51). This is not to foist a rule of method on Brandom's project. The analogy illuminates important features of his work. Consider, for example, his reply to Laukötter et al. (2008), who propose to read *Making It Explicit* as "a theory of social norms" (p. 79). In his own terms, Brandom (2008b) stresses that norms implicit in assertions (historic events), which explain the meanings of expressions, are "attitude-transcendent" semantic constraints on pragmatic interpretation:

All the norms are supposed to be norms of assessment. The sense in which they have a grip [on assertions] is that one is liable to be assessed as having correctly or incorrectly done things. What practitioners actually do or are motivated by is really neither here nor there. (p. 175)

There is nothing about the world that makes you subject to these norms. Non-discursive creatures are not subject to them, and the world does not oblige us to be discursive creatures (though our biology may all but do that). But to be one is to be subject to these norms. These norms are unconditional given that one is playing the game of giving and asking for reasons at all. So if one can have deontic attitudes at all, then one is subject to these constraints. In that sense they are attitude-transcendent. (p. 176)

Remember here that Brandom's project is to ground semantic theory in a model of a practice both necessary and sufficient for content. The practice is necessary in the sense that an

assertion is unrecognisable as such apart from the practice of giving and asking for reasons. A parrot's squawking of the sound of an English sentence is unrecognisable as an act of asserting that an object is red because the parrot is unable to give reasons for believing what 'This is red' says. It is in this sense that Brandom's norms are invariantly there, implicitly if not explicitly, in what speakers do. They are constraints on interpreting a sign-producing creature as having said something. Though there is nothing to prevent these norms from becoming known to the speaker (a judgment dope of a norm-governed sort), there is no need for the speaker to know them. It is the analyst who makes them explicit in the form of rules, in preparation for the work of explaining meanings by way of algorithmic elaboration. The speaker simply speaks, but in speaking does what necessarily exhibits a particular normative structure. These are Durkheimian moves.

Brandom (pp. 20–21) uses Wittgenstein to get his project up and running. He offers a reading of the argument of *Philosophical Investigations* §201, where Wittgenstein makes a distinction between interpreting a rule and obeying or following a rule in actual cases. For Brandom, the problem here is to show how rules can be codifications of things speakers actually do (historic events). He uses the metaphor of games to construe the rules of a theory of meaning as codifications of norms implicit in a social practice. Social practices are games, and specifically linguistic social practices (language-games) are performances governed by constraints on interpreting noise-making entities as having said something. Brandom makes these constraints explicit in the form of rules, but without requiring that the speaker know them. Wittgenstein's point, on Brandom's reading of §201, is that the speaker must be able to speak without having to apply the rules of a theory of the semantics of a natural language. The speaker uses sentences to speak. The semantic theorist uses rules of interpretation to explain their meanings. Brandom finally grounds semantic theory in an account of norms implicit in what the speaker does in using sentences to speak.

But what is Wittgenstein's argument? The briefest relevant reconstruction must begin with the assumption that the analyst, a perfectly competent speaker of L, *has* a theory of the semantics of L. This assumption may seem wrong on the face of it for two reasons. For one thing, no one *has* such a theory, neither the analyst *qua* speaker of L nor the analyst *qua* analyst. ("It goes without saying that nobody has a worked out semantics for any natural language or for any substantial fragment there of" [Fodor & Lepore, 2010, p. 182]). There is

also the worry that the project of constructing such a theory dates back no earlier than the 1960s (Recanati, 2004, p. 442). Wittgenstein's remarks on rules and rule-following may seem to be only incidentally relevant to controversies in contemporary philosophy of language. Baker (1981) argues that they are "relevant to the whole framework of controversy" (p. 65), since they call into question a model of meaning simply taken for granted today, not clarified and defended in light of Wittgenstein's critique of philosophy. But Wittgenstein himself does not argue (nor could he have argued) explicitly against the project of formal semantics.

Here, however, is an argument derivable from *Philosophical Investigations* circa §201: (1) The analyst, a speaker of L, *has* a theory of the semantics of L. (2) Doubts about the meaning of a sentence in L may still arise for the analyst *qua* speaker of L. (3) When such doubts do arise for the analyst, they encompass the theory. (4a) So a theory of the semantics of L does not settle doubts about the meaning of a sentence in L. (4b) Only ad hoc considerations settle such doubts.

In reply, it might be argued that doubts about meanings belong as phenomena of interest to psychology, that they are simply irrelevant to study of the semantics of L (cf. Davidson, 2005b, pp. 111–2). Indeed, it might be argued that doubts presuppose that there is something for the analyst to have doubts about: a literal meaning explained by a theory of the semantics of L (which the theorist *has*). This is to deny what Austin says about truth conditions, perhaps on the grounds that a sentence obviously does have a meaning of its own. Unless it did, what use would it be to the speaker? The sentence 'I forgot the paper' does not say that the speaker 'remembered' the paper, but that the speaker 'forgot' the paper! The verb 'to forget' would not have helped the flatmate or police detective of Kearns' example if it meant nothing or something else. Nothing more is intended by the claim that conventions (norms, rules, practices) are involved in language. There are constraints, semantic constraints, on what the speaker of L can use 'I forget the paper' to do. (But see Davidson, 2005a.) The point of a theory of the semantics of L is to make semantic constraints (literal meanings) propositionally explicit, to individuate meanings using rules that permit associations of meanings with sentences in L.

Furthermore, it might be presented as evidence against Wittgenstein that he needs to invent truly fantastic scenarios to introduce doubts about meanings, all but admitting that

meanings are obvious and do ordinarily impose constraints on uses of expressions. Think of the sign-post pointing the way, which could still be interpreted (but how?) as pointing in the opposite direction. Or think of the instruction telling us to continue adding 2, which could still be interpreted (but how?) as telling us to add 4 to 1000. We can invent a scenario in which a perfectly competent speaker of English, an analyst of meanings, begins to have doubts about the meaning of 'forgot' in English. But it is obvious that 'forgot' in English means a forgetting in the past and not a remembering or anything else.

Of most concern to Brandom is the claim that doubts about meanings encompass the rules of a semantic theory. (3) is the problem of codification: how can rules codify correctnesses of linguistic performance, e.g. the correctness of 'forgot' and incorrectness of 'remembered'? Doubts about meanings are doubts about codifications of semantic constraints on uses of language, to be answered by a theory of what it is correct or incorrect (a practice) before the theorist appears on the scene. Brandom reads Wittgenstein as rejecting a particular theory of normativity, the "regulist" rather than "pragmatist" model of the correctness of a performance, and as leaving construction of a better theory for others to do (pp. xi–iii, pp. 18–20, pp. 20–30). Regulism is the view that the performance must be done according to a rule to be correct. This doctrine is obviously regressive if the person producing or assessing a performance "according to a rule" is producing a further performance, that of an interpretation of the rule. The point of Wittgenstein's fantastic scenarios, on Brandom's reading, is to force the analyst to consider the problem of a regress of rules as interpretations of rules. There is nothing for it but to abandon regulism in favour of a pragmatist theory of the normative as such. Where the invocation of a rule is obviously regressive, it is a regress-halting practice that makes a performance correct or incorrect. Norms that govern performances and assessments of performances must be implicit in the practice itself. They are there to be discovered in what people actually do, even where the people producing a performance may have no explicit knowledge of them, but only practical knowledge of the language (pp. 64–6). They are there for the analyst to make explicit in the form of rules.

Brandom's way of reading Wittgenstein represents an impasse (cf. Baz, 2012, Chapter 1). Wittgenstein is supposed to have been a "principled theoretical quietist" (p. 29). There is something to explain, but Wittgenstein chooses not to attempt to explain it. There is

such a thing as content, e.g. the literal meaning of ‘I forgot the paper’ in English. There is also conceivably such a thing as an adequate analysis of content. Recall the first of two controversies of contemporary philosophy of language, invoked by Fodor and Lepore (1992) to frame criticism of Davidson’s holism:

Philosophers have traditionally disagreed not only about what the right theory of meaning is for a natural language (for English, as it might be), but even about what kind of theory a theory of meaning for a natural language ought to be. [...] A recurrent theme in Davidson’s writings is the claim that a Tarski-like truth theory is the appropriate form for a theory of meaning, and most of Davidson’s philosophy of language is an attempt to elucidate the conditions of adequacy for meaning theories that take this form. (pp. 59–60)

Brandom uses Wittgenstein to establish a “pragmatist” condition (or criterion) of adequacy for meaning theories:

A fundamental methodological criterion of adequacy of the [pragmatist’s] account [of meaning] is that the theorist not attach semantic contents to expressions by *stipulation*; it must always be showing how such contents can be *conferred* on expressions by the scorekeeping activities the theorist [properly] attributes to the linguistic practitioners themselves. (p. xviii; original emphasis)

Brandom’s Wittgenstein disagrees with Austin. Brandom reads Wittgenstein as conceding that there is something for the theorist to explain: the literal meaning of an arbitrary sentence in English, separable from historic uses of language as the meaning of a type of expression. For example, there is the literal meaning of a sentence containing a definite description (‘The present King of France is bald’) or that of a Frege example (‘Jimmy believes that Superman wears a red cape’). As McDowell (2009) puts it, Brandom’s Wittgenstein “shows a need for some constructive philosophy, but pleads ‘quietism’ as a pretext for leaving the job to others” (p. 104). A person who rejects astrology as a false science is not a quietist about astrological facts. Austin, then, is not a quietist about literal meanings. He does not argue (in effect) that semantic analysis must be rooted in practices that suffice for content, but calls the very idea of semantic analysis into question. Brandom’s Wittgenstein is a pessimist first and descriptive particularist and theoretical quietist second. Though he acknowledges the possibility of semantic analysis, he can see no way to explain it. Linguist practices seem to him to be too much of a motley (Brandom, 2008a, pp. 5–7). His pessimism prevents him from attempting what Brandom claims to have achieved in *Making It Explicit*: a theory of content that satisfies the condition of methodological pragmatism.

The impasse is an inability or unwillingness on the part of the analyst to take occasionalism seriously. According to Austin, the meaning of a speaker's words "depend[s] on their have been spoken, or reacted to, in a certain way, or in certain conditions, or in the way, or conditions, they were" (Travis, 2008, p. 87). This is not to deny that there are meanings, not even that there are, in a qualified sense, literal meanings. Occasionalism is the view that meanings are accomplishments of historic uses of language. Meanings, then, can be arrived at only *inductively*, by describing what has gone well or badly for language-users in the past; not *deductively*, by applying a theory of meaning to the sentence up for analysis. Brandom takes the possibility of semantic analysis for granted, perhaps on grounds of philosophical common sense (cf. Baz, 2012, Chapter 1). For him, a sentence obviously does have a meaning of its own, and Wittgenstein should be read as establishing a condition of adequate analysis, not as casting doubt on the whole project of formal semantics.

It is difficult to see a way beyond this impasse, which should remind the reader of Garfinkel's either/or. What could be of interest here to the ethnomethodologist? Not the truth (plausibility, believability, sayability) of what Brandom says. The ethnomethodologist must adopt an attitude of indifference even to this ongoing dispute. On the other hand, it is clear that Garfinkel is closer to Wittgenstein than he is to Brandom in his own view of the relationship between meaning and use. His initiatives might still yield an objection to Brandom's pragmatism. Or does it work the other way round? Where the Wittgensteinian takes issue with Brandom's pragmatism, does Wittgenstein's critique of philosophy yield an objection to Durkheimian sociology? There is another possible interpretation of the relationship between ethnomethodology and ordinary language philosophy. At the level of motivating insights, do they not perhaps converge in a single project (methodogenesis)?

(C) There may be nothing common to all linguistic phenomena, nothing that makes us use the word 'language' for all language-games.

The core theory of *Making It Explicit* is a model of norms (formal structures) that must be there in any linguistic performance, implicitly if not explicitly. They must be there in what speakers do for sign-producing creatures to qualify as speakers at all. They govern performances that suffice for content as the doing of a content-conferring practice. Brandom sees an opportunity here to satisfy his pragmatist constraint on a theory of content

(methodological pragmatism). It must be possible by way of algorithmic elaboration to explain meanings in terms of uses.

The model itself is suggested by, and constructed from, some expressions of conversational English: to give something (a reason), to ask for something (a reason), to be committed to something (a claim), to be entitled to something (a claim), to play a game (the game of giving and asking for reasons), and so on. Brandom shows no interest in the etymology of this linguistic material, but supposes transparency of use. How we learnt to employ it, or do employ it, is again beside the point. We have this material at our disposal and can help ourselves. We know (roughly?) what it means to say that a speaker demonstrated entitlement to a claim, vindicating a discursive or doxastic commitment by giving a reason. Brandom's question is what it means to say that semantic content has a representational dimension, e.g. that a sentence is true or false of, intentionally or representationally directed toward, the world. His explanatory strategy is to explain representation in terms a model of socially articulated reasoning, treading a social path from reasoning to representing.

Austin and Wittgenstein share a concern. Where we wonder what an expression means, as used by a person on a particular occasion for speaking, we naturally apply a paradigm of interpretation to the person's words. What the expression meant is to be grasped in terms of what the person did with it, which we compare with what other people did on particular occasions with the same or some similar expression. This is analysis by means of a model, but need not be formal analysis. It need not be analysis that "proceeds to a foregone conclusion" (Garfinkel, 2002, p. 263; cf. Gadamer, 1975, pp. 268–73). Brandom always has a certain model in mind, a certain fore-conception of what speakers do. If a word-uttering creature is to qualify as a speaker, it must first qualify as concept-mongering creature, and fails to qualify unless it is able to play a part in the practice of giving and asking for reasons. Accordingly, whatever a noise-making creature is doing, if it qualifies as a speaker, then its performances are governed by (subject to assessment in light of) norms implicit in that practice. The concern here is not just that Brandom's results are empirically indeterminate (Emirbayer & Maynard, 2011), but that his theory of practices may be a symptom of confusion and a source of further confusion.

According to Austin (1961b), we should always remember that the linguistic material of a model of action of any kind (including a model of speech acts) is highly abstract and liable to baffle us just where its meaning seems clear:

The beginning of sense, not to say wisdom, is to realize that 'doing an action', as used in philosophy, is a highly abstract expression – it is a stand-in used in the place of any (or almost any?) verb with a person subject, in the same sort of way that 'thing' is a stand-in for any (or when we remember, almost any) noun substantive, and 'quality' a stand-in for the adjective. (p. 126)

Consider the idea that an action (the doing of something) differs from the event of a mere movement of body parts. It is easy to think of examples that launch semantic analysis and keep it afloat. A would be responsible for keeping B awake if what kept B awake was A's listening to music, but not if what kept B awake was A's snoring. To listen to music is an action, something a person does intentionally, and then stands responsible for having done. It is something done for a reason, not the effect of a cause. Behold! There are indeed relations that obtain between the concept of an action and other concepts: an action is a means to an end; the actor does it for a reason; it is purposeful, intelligible in light of a purpose, perhaps inexplicable in causal terms. Austin wonders what such results might actually mean:

We treat the expression 'doing an action' no longer as a stand-in for a verb with a personal subject, as which it has no doubt some uses, and might have more if the range of verbs were not left unspecified, but as a self-explanatory, ground-level description, one which brings adequately into the open the essential features of everything that comes, by simple inspection, under it. We scarcely notice even the most obvious and patent exceptions or difficulties (is to think something, or to say something, or try to do something, to do an action?), any more than we fret, in the *ivresse des grandes profondeurs* [drunkenness of the great depths], as to whether flames are things or events. So we come easily to think of our behaviour over any time, and of a life as a whole, as consisting in doing now action A, next action B, then action C, and so on, just as elsewhere we come to think of the world as consisting of this, that and the other substance or material thing, each with its properties. All 'actions' are, as actions (meaning what?), equal, composing a quarrel with striking a match, winning a war with sneezing; worse still, we assimilate them one and all to the supposedly most obvious and easy cases, such as posting letters or moving fingers, just as we assimilate all 'things' to horses or beds. (pp. 126–7)

Think of the action of washing hands as done by the neurotic of classical psychoanalysis, or as done by the client receiving help for OCD from a cognitive behavioural therapist. What

does it mean to say that a person does the symptomatic action? This question is of practical interest to the person supposedly so afflicted, as it must also be to the expert employed to help, and is (or ought to be) of great theoretical interest to the philosophy of action. Does it help to regard it as just another instance of an action, essentially no different from the action of washing hands as done by a surgeon about to operate, or as done by a child about to eat? Austin has his doubts:

Going back into the history of a word, very often into Latin, we come back pretty commonly to pictures or *models* of how things happen or are done. These models may be fairly sophisticated and recent, as is perhaps the case with 'motive' or 'impulse', but one of the commonest and more primitive types of model is one which is apt to baffle us through its very naturalness and simplicity. We take *some very simple action*, like shoving a stone, usually done by and viewed by oneself, and use *this*, with features distinguishable in it, as our model in terms which to talk about other actions and events: and we continue to do so, scarcely realizing it, even when these other actions are pretty remote and perhaps much more interesting to us in their own right than the acts originally used in constructing the model ever were, and even when the model is really distorting the facts rather than helping us to observe them. (p. 150; original emphasis)

These are not the merely intellectual anxieties, nowhere grounded in actual practice, of an invented scenario. Austin is not the cultural outsider who studies an arrow pointing left and wonders whether to go right. A fundamental question of method does arise. If our task is to explain what it means to say that a person did an action, should we proceed by analysing the simplest and most obvious cases, or by considering various interestingly different cases? Austin's "plea for excuses" is to consider some cases of naturally occurring analysis of actions, where parties to an inquiry (a legal inquiry) were practically concerned to clear up what was meant in saying that a person did something. An excuse is a way of bringing into the open the essential features of this or that particular action. How do we excuse a night of snoring, our part in a quarrel, a miscalculation, a clumsy apology, a life decision? It is by means of analysis of various interestingly different cases that we come to see what the expression 'doing an action' might cover, or what the results of an analysis of the concept of an action could conceivably mean.

In the opening sections of *Philosophical Investigations*, Wittgenstein calls attention to an especially compelling paradigm of interpretative work, suggested by a passage from Augustine. As Wittgenstein reads it, Augustine "gives us a particular picture of the essence

of human language” (§1). His elders taught him to speak by pointing to objects and saying their names. Words, then, are the names of objects and sentences are combinations of words. What is Wittgenstein’s worry? Not that a picture so simple and obvious could still turn out to be false, but that it holds us captive. “A *picture* held us captive,” he says; “we could not get outside it, for it lay in our language and language seemed to repeat it to us inexorably” (§115; original emphasis). A paradigm of interpretation is neither true nor false. It is a way of viewing uses of language, and may illuminate here and obscure there. Indeed, Wittgenstein’s first example of a language-game is one “for which the description given by Augustine is right” (§2). Four expressions are woven into the actions of a builder and his assistant: ‘block’, ‘pillar’, ‘slab’ and ‘beam’. These are the names of four kinds of stone: blocks, pillars, slabs and beams. The builder says ‘block’ and his assistant brings a block. Later, the words of this “primitive language” will be combined with others, in the course of activities linguistically more complicated than this one. Augustine’s model of meaning is only occasionally right. But it holds us captive. We go on trying to interpret uses of language as instances or elaborations of the practice of calling out names.

Wittgenstein’s leading thought, as Fogelin (1996) puts it, is that philosophers “are led into confusion because they are antecedently disposed to view various uses of language in ways inappropriate to them” (p. 34). His approach to philosophy in *Philosophical Investigations* is to regard philosophical problems as symptoms of a kind of illness (§254). Problems arise for philosophers because they are held captive by a picture of the essence of human language. The right way to do philosophy is not to make the picture more elaborate or replace it with another, but to return from the abstractions of semantic analysis to particular cases. “There is not *a* philosophical method, though there are indeed methods, like different therapies” (§133) – for the multiplicity of difficulties that occupy philosophers. If the meaning of name is its bearer, what does it mean to say that Mr N. N. is dead? Must meanings survive the bearers of a name if this sentence is to be spoken meaningfully? How should we understand the contribution of ‘the present King of France’ to sentences that contain it? Is there something round and square for ‘the round square’ or is this phrase meaningless? If it is meaningless, how does it differ from ‘the slythy toves’? Russell (1905) constructs a theory to cope with these problems. As evidence for its correctness, he cites the absurdity of the view that an object must exist for every denoting phrase:

The evidence for the [suggested] theory is derived from difficulties which seem unavoidable if we regard denoting phrases as standing for genuine constituents of the propositions in whose verbal expressions they occur. Of possible theories which admit such constituents the simplest is that of Meinong. This theory regards any grammatically correct denoting phrase as standing for an *object*. Thus “the present King of France,” “the round square,” etc., are supposed to be genuine objects. It is admitted that such objects do not *subsist*, but nevertheless they are supposed to be objects. This is in itself a difficult view; but the chief difficulty is that such objects, admittedly, are apt to infringe the law of contradiction. It is contended, for example, that the existent present King of France exists, and also does not exist; that the round square is round, and also not round; etc. But this is intolerable; and if any theory can be found to avoid this result, it is surely to be preferred. (pp. 482–3; original emphasis)

Instead of asking how to solve such problems, Wittgenstein asks how they arise for the philosopher at all. A possible source of trouble is that we expect to find the same thing in diverse uses of language. Unable to see the practice of calling out names, we demand a theory that explains the meaning of denoting phrases. Would it not be better to ask how an expression has been used, without expecting to find an instance or elaboration of a core practice (naming, asserting, inferring, etc.)?

Brandom takes the point to be that our ability to name, where there is no sign of another practice, presupposes an ability to deploy assertions as “fodder for inferences” (p. 168). His first thoughts are that “*discursive* (in the Kantian sense of concept-mongering) practice can only be *linguistic* practice,” and that “what distinguishes a practice as specifically linguistic is that within it some performances are accorded the significance of *assertions*” (p. 172; original emphasis). To call out a name, if this is already to speak, is to do what requires an ability to apply a concept (the name) to an object. And to apply a concept to an object, saying of a particular stone that it is some particular kind of stone (a block), is to do what requires an ability to give and ask for reasons. But this is just another picture of the essence of human language. It is part of Brandom’s account of the “ultimate source of the meanings and noises [we] make, and of the other things [we] do” (p. 159). There is the same worry.

Brandom gives an example that is indeed “apt to baffle us through its very naturalness and simplicity” (Austin, 1961b, p. 150; in context above). What does it mean to say that a person is entitled to a claim? What is a discursive entitlement? What does the person have that counts as one? Brandom considers the action of entering the auditorium of a theatre:

Ordinarily the relation of an authorizing event to the performances it licenses requires at least that in the context of the event, performances become socially appropriate that otherwise would not be. For example, purchasing a ticket entitles one to take a seat in the theatre, which it would be inappropriate to do without the ticket. This observation presents a dilemma. If asserting a sentence is not a performance requiring prior authorization, then it seems one cannot understand the function of assertion as inferentially licensing other assertions. If, however, asserting is a performance requiring authorization, how does one become entitled to the original licensing assertion? Talk of *inheritance* of entitlement makes sense only in an explanatory context that includes a story about the significance of *possession* of entitlement. It is this question that is addressed by an account of the dimension of *responsibility* characteristic of asserting. In asserting a claim, one not only authorizes further assertions (for oneself and others) but undertakes a responsibility, for one commits oneself to being able to vindicate the original claim by showing that one is entitled to make it. Others cannot inherit an entitlement that the asserter does not possess. Overtly acknowledging or undertaking a doxastic commitment by issuing an assertional performance can warrant further commitments, whether by the asserter or by the audience, only if that warranting commitment itself is one the asserter is entitled to. Only assertions one is entitled to make can serve to entitle anyone to their inferential consequences. (pp. 170–1; original emphasis)

Austin's doubts are relevant here. Is it helpful to apply the model of possession of ticket (entitlement to enter) to an act of speech? Is meaningful speech a game of sorts in which the meaning of an expression is a counter moved from one square on the board ('Superman wears a red cape') to another ('So Clark Kent does')? This is one way of seeing things. But it is not the only way, and may be too simplistic and obvious to be of much use. It is hard to imagine any act of speech quite so simple and obvious.

(D) There is difference between tokens of an action-type (speech acts) and historic uses of language.

Brandom casts the Wittgensteinian as a pessimist (pp. xii–iii; 2008a, p. 7). Austin and Wittgenstein make the same point, which the Wittgensteinian accepts as a decisive argument against the project of formal semantics. This is Austin (1961b):

There is too another danger in words that invoke models, half-forgotten or not. It must be remembered that there is no necessity whatsoever that the various models used in creating our vocabulary, primitive or recent, should all fit together neatly as parts into one single, total model or scheme of, for instance, the doing of actions. It is possible, and indeed highly likely, that our assortment of models will include some, or many, that are overlapping, conflicting, or more generally simply *disparate*. (p. 151; original emphasis)

And this is Brandom's (2008a) Wittgenstein:

[Wittgenstein] is profoundly skeptical about the utility or applicability of the model of postulation, explanation, and theoretical systematization in the case of discursive practice – about the possibility of systematically *deriving* aspects of correct use from assigned meanings. [...] More specifically, Wittgenstein uses the image of “family resemblances” to urge that *kinds* into which linguistic practices and the vocabularies caught up in them are functionally sorted – what belong together in boxes labelled ‘game’, ‘name’, ‘description’, ‘assertion’, ‘observation’, and so on – do not admit of specification in terms of underlying principles specifiable in other vocabularies, whether by genus and differentia(e) or any other kind of explicit rule or definition. It is easy to understand this line of thought as entailing a straightforward denial of the possibility of semantic analysis in the classical sense. (pp. 5–6)

Brandom exploits an ambiguity in the idea of a practice to cast himself as more hopeful or experimental than the Wittgensteinian. He acknowledges the diversity of uses of language, but sees no reason to deny the possibility of systematic analysis. His method in *Making It Explicit* is to work backwards from any act of speech to an ability speakers necessarily share. In more recent work, he offers a theory of “pragmatically mediated semantic relations” (pp. 7–14), which exploits a distinction between practices sufficient for the meanings of a vocabulary (modal, deontic, logical, indexical, etc.) and vocabularies sufficient for description and analysis of a practice. But Brandom's optimism involves a misconstrual of Wittgenstein's worry, at least on this occasionalist reading of ordinary language philosophy (Baz, 2012; Travis, 2008). Are there too many distinct practices to count, classify, analyse and explain? That is Brandom's question, not Wittgenstein's. Does a picture hold us captive? That is what Wittgenstein asks instead.

The insight more obviously due to Austin is that the linguistic material of Brandom's model is indeed highly abstract. Brandom counts types of practices: the practice of naming, the practice of describing, the practice assertion or observation, and so on. Performances are tokens of a practice-type. What is true of one token of a content-conferring practice is true of any other. The performance exhibits certain formal properties, to be regarded as separable from historic uses of language. The practice of returning a verdict (jurors) or determining cause of death (coroners) is a species of the genus of making a claim. But an account of a practice falls short of an account of what *was* that practice on particular occasions. The technical expression ‘to make a claim’ glosses over various ways in which a

person could be said to have made a claim. There is a multiplicity of uses of language to notice here, but it is not a multiplicity of practice-types. We see the same practice exhibited in a multiplicity of different ways. It is not pessimism to deny the possibility of formal analysis of historic uses of language. Nor is it the possibility of such analysis that Brandom affirms. He affirms the possibility of a theory of content firmly rooted in a practice-type, to be regarded as separable (with literal meanings) from the doing of the practice on particular occasions.

These, then, are the four motivating insights of ordinary language philosophy: (A) Questions of meaning do not arise, and cannot be settled, at the level of expressions, but only at the level of historic uses of language. (B) As for expressions, so for rules that give the meanings of expressions. (C) There may be nothing common to all linguistic phenomena. (D) There is a difference between tokens of an action-type and historic actions.

Now recall the basic explanandum of the Chicago Jury Project: the judge-jury disagreement rate of 24.6 per cent of all 3576 cases in the direction of jury leniency. There is a sense in which the study asks what historic decisions meant. It asks what to make of this or that decision, what significance to attach to it, what it meant (or implied) in the context of an explanation of judge-jury disagreement. There may be arguments in ordinary language philosophy for ethnomethodology. Compare: (A') Questions of meaning do not arise, and cannot be settled, at the level of an analysis of a representative sample, but only at the level of methods jurors used to decide a case. (B') As for the analysis of the representative sample, so for rules of sociological method. (C') There may be nothing common to methods jurors used to decide a case, but only a complicated network of similarities. (D') There is a difference between explanations of judge-jury disagreement (ways of deciding cases) and methods jurors used to decide a case.

The analogy of formal analysis reveals a relationship between ordinary language philosophy and ethnomethodology. How should we understand it? There are three possible interpretations: (1) The ethnomethodologist leans on ordinary language philosophy for a motivating argument against Durkheimian sociology. (2) Garfinkel's initiatives place Wittgenstein's critique of philosophy in the broader context of an ethnomethodological critique of formal analysis. (3) At the level of motivating insights, ethnomethodology and ordinary language philosophy are one and the same project.

5. Summary

I have stressed the fact that Garfinkel's mature characterisation of ethnomethodology gives rise to a question. It presents us with a choice between two radically different approaches to the study of social life: Durkheimian sociology and its asymmetrically incommensurate alternate. To opt for one is to opt against the other. Why, then, should we opt for ethnomethodology?

I have proposed to answer in light of an analogy between Durkheimian sociology and analytic philosophy of language. Both are examples of formal analysis. They abide in the different ways by the same rule: to consider explananda (social facts, meanings) as things somehow separable from historic actions. Accordingly, an argument against analytic philosophy of language may perhaps do double duty as an argument for ethnomethodology.

Brandom's pragmatism provides an especially clear example of the "Durkheimian" rule of analytic method. Not only does Brandom regard meanings as separable from uses, as other philosophers do; he also explains them in terms of norms implicit in a content-conferring practice, which he regards as separable once again from historic uses of language. His approach to the study of meanings is decidedly *un*Wittgensteinian, in ways that help us see the relationship between ethnomethodology and ordinary language philosophy. Where Wittgenstein warns that a picture of language may be holding us captive, Garfinkel recommends a "methodogenesis" of problems of formal analysis. The task of an ethnomethodological study is emphatically *not* to explain a phenomenon of social life, but to describe procedures people use to generate an explanandum. By way of illustration, I have considered the problem of a judge-jury disagreement rate of approximately 25 per cent in the direction of jury leniency, which will serve as an example throughout this study. More difficult are the two basic examples of Brandom's pragmatism: on the level of semantic analysis, the example of literal meanings; on the level of pragmatist elucidation of semantic concepts, the example of a rule of method for using (or for refraining from using) a philosophically troublesome word. For the ethnomethodologist, the task is emphatically *not* to assess Brandom's theory of practices, but to describe procedures philosophers use to generate the problem of what an expression means.

I have indicated three interpretations of ethnomethodology's relationship to ordinary language philosophy. I have also indicated what I take to be the crucial interpretative issue.

The Wittgensteinian can afford to be unreservedly critical of Brandom's pragmatism, reading his work as illustrative of such remarks of *Philosophical Investigations* as the following:

The results of philosophy [as it ought to be done!] are the uncovering of one or another piece of plain nonsense and of bumps that the understanding has got by running its head up against the limits of language. These bumps make us see the value of the discovery. (§119)

By contrast, the ethnomethodologist must be indifferent to disputes between philosophers, in order to study them as "organizationally situated methods of practical reasoning" (Garfinkel, 1967, p. viii). Indeed, the ethnomethodologist must be indifferent even to philosophical objections to Garfinkel's initiatives. I am going to argue that the practice of such indifference precludes certain interpretations of the relationship between ethnomethodology and ordinary language philosophy, since it makes it difficult to understand that practice at all. Ethnomethodology does not lean on Wittgenstein for a motivating argument, or situate Wittgenstein in the broader context of an alternative to formal analysis. There must be a way of agreeing with Garfinkel without having to disagree with Brandom. Likewise, there must be an argument for ordinary language philosophy that precedes objections to semantic analysis. The value of the analogy of formal analysis is that it brings the argument to light.

CHAPTER TWO

Durkheim's Aporism

It is fair to say that Garfinkel's attitude to Durkheim encapsulates his attitude to the sociological mainstream. In the very first paragraph of *Studies in Ethnomethodology*, he suggests an alternative reading of Durkheim's (1982) "first and most basic rule" of sociological method:

In doing sociology, lay and professional, every reference to the "real world," even where the reference is to physical or biological events, is a reference to the organized activities of everyday life. Thereby, in contrast to certain versions of Durkheim that teach that the objective reality of social facts is sociology's fundamental principle, the lesson is taken instead, and used as a study policy, that the objective reality of social facts *as* an ongoing accomplishment of the concerted activities of everyday life, with the ordinary, artful ways of that accomplishment being by members known, used, and taken for granted, is, for members doing sociology, a fundamental phenomenon. Because, and in the ways that it is practical sociology's fundamental phenomenon, it is the prevailing topic for ethnomethodological study. (p. vii)

Garfinkel's interest in the idea of the "objectivity of social facts" is more explicit in *Ethnomethodology's Program: Working Out Durkheim's Aporism*, where he presents Durkheimian sociology as a species of formal analysis. My overall aim in this chapter is to reconstruct his revisionary reading of Durkheim in light of the motivating insights of ordinary language philosophy. I defend three specific claims. First, Garfinkel's rejection of "certain versions of Durkheim" is a rejection of the idea that social facts are separable from historic actions. Second, it is consistent with Wittgenstein's critique of philosophy. Third, it reveals the circularity of Brandom's response to Wittgenstein.

I begin in Section 1 with a brief review of the dilemma of Garfinkel's initiatives. The student of social life must opt to do one of just two things: ethnomethodology or Durkheimian sociology. Is this a matter of personal preference? Or is there an argument for undertaking ethnomethodological research? In Section 2, I turn to Baz's (2012) defence of ordinary language philosophy for help in answering. Baz calls attention to question-begging misunderstandings of ordinary language philosophy. There have been similar

misunderstandings of Garfinkel's initiatives. Without considering Garfinkel's reading of Durkheim (which is consistent with Wittgenstein's critique of philosophy), his critics have demanded explanations of social facts. In Section 3, I present exegetical evidence of an intellectual debt on Brandom's part to Durkheim. This is by way of preparation for Section 6, where I spell out the circularity of his response to Wittgenstein. My aim in Sections 4, central to the argument of this thesis as a whole, is to point out the absurdity of the idea of doing a social fact. It is absurd to say of language-users that they are doing a content-conferring practice. They are simply using language. But a way of using language may strike the analyst as an instance of what is necessary and sufficient for content: *the* practice of giving and asking for reasons. In Section 5, I present an example of a problem of philosophical semantics, that of specifying conditions on interpreting interactions between noise-making entities as uses of language. It is "preposterous" if read literally as a description of things we do. My aim here is to show that the analyst may be using an account of a practice P, not to describe a way of using language, but to explain meanings in terms of features of P. In Section 6, I present the idea of a Lebenswelt pair as Garfinkel's version of the idea of language-game, i.e. as the idea of an activity consisting of language and actions into which it is woven. It is in terms of this comparison that I finally state an uniquely "ethnomethodological" objection to Brandom's pragmatism. Under ethnomethodological scrutiny, his response to Wittgenstein turns out to be circular.

1. Garfinkel's Durkheim

On Garfinkel's (2002) revisionary reading of Durkheim, the aim of Durkheim's basic rule of method is to reduce the details of historic actions, considered as phenomena in their own right, to variables susceptible to analysis. Consider just one of the historic decisions of the sample of 3576 jury trials. Presumably, it was already accountable in terms of the features, circumstances and events of the trial, together with what any experienced judge could be expected to have learnt about a jury's ways of thinking. It would serve as an example of the "endogenous" (p. 72) or "natural" rather than "classical" (pp. 173–5) accountability of practical actions. The point of Durkheimian sociology is to make practical actions accountable, not as phenomena in their own right, but as tokens of an action-type. Practical actions are to individual incarnations of a society what propositions are to the variable

'*p*' in a sequent of propositional calculus. The analyst explains them in terms of relations that obtain between tokens of an action-type. These are peculiarly and irreducibly sociological relations. They exist outside the minds of ordinary members of society, who have no direct access to them. Or so the analyst supposes. They are "classically" rather than "naturally" accountable, in the sense that the analyst must resort to paradigms of empirical research to study them at all. Accordingly, the first and most basic rule of sociological method is simply to accept that practical actions are individually unanalysable, that they admit of analysis only as individual incarnations of society. The analyst begins by "respecifying" practical actions as representations of social facts.

Garfinkel interprets Durkheimian sociology as recoiling from the idea that practical accounts are endogenously accountable. How does an experienced judge explain the fact that a jury returned an incorrect decision? The analyst takes no interest in this question, asking instead what explains the extent and direction of judge-jury disagreement in a representative sample of 3576 historic decisions. The great heap of endogenously accountable practical actions that enter into a social fact is a "plenum" in which there is no order to be found. Instead of considering members' practical concerns, the analyst consults a "bibliography" of the sociological literature. There will be no discussion of these (whatever) practical concerns in the corpus of formal-analytic studies, except insofar as they present familiar problems of formal analysis. For example, instead of considering problems as jurors' practical concerns, the analyst asks how a statistical finding – judge-jury disagreement in 24.6 per cent of all 3576 cases in the direction of jury leniency – is now to be explained. In Garfinkel's words:

According to the worldwide social science movement and the corpus status of its bibliographies there is no order in the concreteness of things. The research enterprises of the social science movement are defeated by the apparently hopelessly circumstantially overwhelming details of everyday activities – the plenum, the plenty, the plenilunium. To get a remedy, the social sciences have worked out policies and methods of formal analysis. These respecify the concrete details of ordinary activities as details of the analyzing devices and of the methods that warrant the use of these devices. They respecify the circumstantiality of ordinary activities so that order can be exhibited analytically. It is essentially an empirical demonstration. The details found in the model reveal the essential recurring invariant features which are FA's phenomena. (p. 95)

The rule here is to look for order (or accountability), not in particular practical actions, but in a very large number of presumptive instances of a social fact. On this almost Platonic view of things, the order of a jury's decision is not the order of their practical actions, but the order of an aggregate phenomenon. The practical actions are to be studied as instances of a rule of conduct. It is the rule of conduct – the deed done as distinct from a way of doing of it – which explains the social fact.

It is neutral between ethnomethodology and Durkheimian sociology that “No Order in the Plenum” is a methodological requirement: not a denial of order in people's actual activities, but a rule of sociological method. Garfinkel points out the absurdity of the claim that there is “no order” in things people do. But on his own conception of Durkheimian sociology, the analyst does not make that absurd claim. The analyst is only meant to be making a distinction between the order (analysability, intelligibility) of social facts and the order of individual manifestations of social facts considered merely *as* people's actual activities. According to Garfinkel, the analyst ignores the fundamental fact that people's actual activities are orderly (analysable, intelligible) so as to discover, demonstrate and explain social facts. It is worth asking whether Garfinkel's “analyst” may be, at least in some cases, a straw man. For example, is it true to say of Kalven and Zeisel, the “analysts” of *The American Jury*, that they ignore the order of jurors' decision-making, when they pay such close attention to judges' explanations of particular cases of judge-jury disagreement? These “analysts” might not accept Garfinkel's conception of Durkheimian sociology as a fair depiction of their own work. Be that as it may, the interpretative question is whether Garfinkel concedes the legitimacy of Durkheim's distinction, i.e. whether he thinks that there are things (social facts) that call for separate analytic treatment.

Garfinkel makes two claims in the following passage. The first, a negation of formal-analytic negation, is that there *is* order in the plenum. It is the order of a way of doing a practice. Indeed, it is the order of what is being rendered orderly in the doing of it:

[T]here *is* order in the most ordinary activities of everyday life in their full concreteness, and that means in the ongoingly *procedurally* enacted coherence of substantive, ordered phenomenal details without loss of generality. And these evidently. (p. 96; original emphasis)

The second claim is that order produced by parties to a practice – the order of this (whatever) way of doing it – can be studied “without loss of generality” as such. This is a difficult

view. Garfinkel presents it in terms reminiscent of Husserl's clarification of a fundamental philosophical error (Husserl, 1982, §43; Mulligan, 1995; but cf. Rogers, 1983). The basic rule of ethnomethodology is to regard practical actions as society appearing "in person" to members, i.e. as the lived work of order production:

EM is not in the business of interpreting signs. It is not an interpretative enterprise. Enacted local practices are not texts which symbolize "meanings" or events. They are in detail identical with themselves, and not representative of something else. The witnessably recurrent details of ordinary practices are constitutive of their own reality. They are studied in their unmediated details and not as signed enterprises. (p. 97)

Could there be a simple parting of ways between the analyst and the ethnomethodologist? Without denying that there is order in the plenum, the analyst makes a distinction between (1) the order of a social fact and (2) the order of an individual incarnation of a social fact. "No order in the plenum" may simply mean that (1) is not to be found in (2). In the meantime, the ethnomethodologist examines (2) apart from (1), paying no attention to problems of formal analysis. On this interpretation of Garfinkel's initiatives, the student of social life is indeed confronted with a dilemma. Which basic rule of method is preferable, the one that says there is no order in the plenum (Durkheimian sociology) or the one that says there is (ethnomethodology)?

This dilemma is too basic, simply too programmatic, to be decided on an ad hoc basis. It is not like having to decide whether to resort to interviews, polls or mail questionnaires. Ethnomethodology is not an occasionally appropriate method of Durkheimian sociology, to be adopted as need be. It is a radical alternative to the study of social life. This is certainly how members of Garfinkel's "company of bastards" (Lynch, 2011) present their predicament. To give just one example:

For Garfinkel, there is no beating about the bush. Ethnomethodology is not sociology as sociologists know it. Neither can it become part of what sociologists consider sociology to be. For Garfinkel, it stands in contrast to what sociology is, it is incommensurate to sociology. He calls it an *alternate* sociology. This is a bitter pill for some to swallow, because it rules out many of the ways that 'new moments' in sociology have been traditionally accommodated. Ethnomethodology cannot be used by some, as they have used other arguments, as a corrective to the predominant 'positivistic' methodology in the human sciences, because ethnomethodology treats alternatives as it treats 'positivistic' methodologies, as something to be enquired into. Ethnomethodology cannot be

lumped together with other arguments it is taken as having a family resemblance to, it construes those arguments in different ways from their progenitors. Ethnomethodology cannot be used to solve problems ‘constructively’ specified, because ethnomethodology would constantly address the relationship between the construction and the problem, and never get to grips with what is traditionally considered to be the problem. (Button, 1991, p. 7)

In taking this extreme view, the ethnomethodologist makes it virtually impossible to avoid circularity in defending Garfinkel’s initiatives. This is no objection in itself. After all, not all circles are vicious. (Hermeneutic circles are virtuous.) Plus, the analyst’s position may be no better when it comes to defending Durkheimian sociology against the charge of an intellectually pathological “craving for generality” (Wittgenstein, 1958, p. 17). Still, unless the defender of a rule of method can only ever preach to the converted, the ethnomethodologist may need to look beyond sociology for a motive for doing ethnomethodology. This is perhaps why so many British ethnomethodologists have turned to Wittgenstein studies for help: to defend ethnomethodology, or attack Durkheimian sociology, in neutrally Wittgensteinian terms.

2. Misunderstandings

The dilemma for the student of social life is a choice between two basic policies of research, the policy of Durkheimian sociology or the alternative policy of ethnomethodology. Does it help to consider the relationship between ordinary language philosophy and the (anglo-phone) philosophical mainstream?

Durkheim’s idea is that social facts stand separate from facts about historic actions. Consider another kind of separateness, another “radical disjunction” (Lukes, 1973, p. 213) between one level of order and another. According to what Baz (2012, pp. 9–22) presents as the “prevailing conception of meaning” in mainstream analytic philosophy, the meaning of an expression E is separable from the meaning of a speaker’s use of E on a particular occasion for speaking. In Garfinkel’s terms, there is a plenum of endogenously accountable historic uses of language, all somehow involving E. This is one level of order. The other is the level of the analysability of the literal meaning of E, which is meant to remain “fairly constant from one occasion of use to another” (Kearns, 2000, p. 2). What varies is its pragmatic modification. For example, though the sentence ‘What a lovely day!’ means what

it does in English, a speaker may nevertheless have used it ironically to say the opposite. On this conception of meaning, the rule of method is to regard uses of E as unintelligible apart from the literal meaning of E. As a matter of policy, the analyst holds that explanations of ways of using E are answerable to the meaning of the expression itself. Pragmatics is answerable to semantics.

Baz begins his defence of ordinary language philosophy by considering a “recurrent charge” against practitioners. They confuse meaning and use. They give examples of uses of E, using the meaning of E to explain them, as though this were to explain the meaning of E. Their accounts are uninformative at best, circular at worst. In fact, this is only how things look from the perspective of the principle of separateness. For practitioners of ordinary language philosophy, meanings and uses are inseparable. A meaning is what E has in the context of a practice of using E. It is inseparable from the practice. To deny that two things are separate is not to confuse them unless they are separate. Separateness, then, is not an unproblematic assumption. Critics of ordinary language philosophy must somehow show that the meaning of E does stand separate from the practice of using E, or from various practices in the context of which E has a meaning – though not necessarily always the same meaning (Ebersole, 2001, Chapter 1; Travis, 2008).

But this reply goes deeper than the level of semantic analysis, as Baz is careful to note. It reaches right down to the level of fundamental conceptions of philosophical inquiry, to the level of what, as philosophers, we take ourselves to be doing. The analyst who denies separateness, as Brandom does, may still be preparing to explain the literal meaning of E, to be construed now as the result of countless uses of E, which enter into the synthesis that *is* the literal meaning of an expression. “Meaning is use” might now just mean that semantic analysis must be grounded in an account of actual practices. The deeper question, according to Baz, is what we take a philosophical problem to be, not least of all a problem of meaning:

I have characterized the conflict between OLP [ordinary language philosophy] and its detractors as being rooted in two ways of thinking about the meaning of words and how it relates to what may be said by means of them. This initial characterization must be taken with a grain of salt, however, and not merely because it is extremely schematic. For, as I will emphasize later on, the conflict, at its core, is better seen as concerning not the nature of linguistic meaning, but the nature of traditional philosophical difficulties and the response for which they call. (p. 21)

There are two ways of misunderstanding ordinary language philosophy. One is to assume that practitioners seek to explain meanings in terms of uses, despite the fact that analysis of uses is answerable to analysis of meanings! The other is to assume that philosophical difficulties call for analysis of literal (invariant) meanings at all. This latter claim is just what practitioners deny. They take the view that semantic analysis, though occasionally illuminating, may be a symptom of confusion and a source of further confusion. Philosophical difficulties call for analysis of historic uses of language.

There have been similar misunderstandings of ethnomethodology. The theory-building critic, assuming separability of A from B, accuses the “theoretical quietist” and “descriptive particularist” (Brandom, 1994, pp. xii–iii; 2008a, p. 7) of having contributed nothing to a theory of A. This is obviously to miss the point. Neither Austin nor Wittgenstein propose to build theory of a meaning. They deny that meanings are separable from uses, and recommend a radical alternative to semantic analysis. Likewise, the theory-building critic of ethnomethodology accuses Garfinkel of having contributed nothing to any sociological theory. Consider Coleman’s (1968) scathing review of *Studies in Ethnomethodology*. Coleman presents Garfinkel’s breaching experiments as attempts “to uncover the unstated, implicit, common-sense perceptions held and acted upon by participants in a situation” (p. 126). Garfinkel’s book is repetitive and un insightful:

[I]t would be fortunate if the reader could leave the book (or, rather, non-book: it is actually a disconnected collection of papers, some previously published, others not) after having read only Chapter 2. For the same point can be made only so many times; beyond that, one must look for its fruits, either in theory or research. And Garfinkel simply fails to generate any insights at all from the approach. (pp. 126–7)

His claims are disguised banalities, largely a waste of the sociologist’s time:

I think it is useful to make the general point that the social theorist’s definition of rationality on the part of an actor must be based upon the actor’s state of knowledge and the actor’s perception of the means-end relation. But this having been said, [*Studies in Ethnomethodology*] provides us with little more that is of use. Once again, Garfinkel elaborates very greatly points which are so commonplace that they would appear banal if stated in straightforward English. As it is, there is an extraordinarily high ratio of reading time to information transfer, so that the banality is not directly apparent on a casual reading. (p. 130)

But Coleman misunderstands. He assumes separability of A from B and looks in vain for a contribution to a theory of A. He looks for sociology as it is usually done in what is meant to be a radical alternative to the sociological mainstream. Where critics of ordinary language philosophy, missing the point of Austin's and Wittgenstein's initiatives, demand a contribution to a theory of meaning, Garfinkel's critics demand a contribution to a theory of society in terms of the production of social order.

So there are parallel dilemmas. Durkheimian sociology is to ethnomethodology what semantic analysis is to ordinary language philosophy. To opt for one is to opt against the other. Why opt for ethnomethodology? The argument is simple enough. We need to make a decision as students of social life: examine actual practices (ethnomethodology) or analyse artificially produced data (formal analysis). Achievements of formal analysis, unquestionable as such, are not accounts of actual practices. They are inferences from artificially produced data to practices that explain social facts. They divert attention from what people do to problems that arise for the analyst in constructing a theory of a social fact.

Garfinkel adopts an attitude of indifference to problems of formal analysis. The argument for such indifference is an argument from the indeterminateness of formal-analytic findings. I have given the example of the *de minimis* finding. It leaves us wondering what jurors actually did in deciding a case, as distinct from what contributes to a theory of judge-jury disagreement: a hypothetically general practice. We learn what jurors must typically do if the extent and pattern of judge-jury disagreement is to make sense, not what a particular production cohort ever did do. If we want to understand historic decisions, we need to adopt an attitude of indifference to what mattered most to parties to the Chicago Jury Practices: an explanation of a judge-jury disagreement rate of 24.6 per cent of all 3576 cases in the direction of jury leniency.

Garfinkel suggests that historic actions "escape from accountability" when we abstract an element of meaning from historic actions and try to explain it apart from ad hoc considerations:

The literatures of the social science movement provide for [Durkheim's] things with technologies of formal analysis. They provide the worldly existence of things and real existence of things. These are made accountable adequately and evidently by administering the technology of formal analysis. However, Durkheim's things escape from accountability with the technology of formal analysis.

They escape with the very same methods that are used to describe them. Their escape occurs just in any actual case that methods of formal analysis are used to describe Durkheim's things. (p. 103)

That formal analysis fails to account for Durkheim's things will need to be shown. This can be shown only inductively if Austin and Wittgenstein are right. It can be shown only by considering the relationship between historic actions and explanations of literal meanings (Travis, 2008). Note, however, that we also need to make a decision as students of philosophy. Should we examine actual practices (ordinary language philosophy, ethnomethodology) or construct and analyse models of meaning (semantic analysis)? Achievements of semantic analysis, also unquestionable as such, are not accounts of what language-users do, but inferences from imagined cases to practices that explain literal meanings. Baz (2012, pp. 105 ff.) considers discussion of scenarios carefully designed by the philosopher to reveal constraints on uses of the verb 'to know' (Gettier cases). We learn conditions for the truth of an application of 'knows such-and-such', but without learning what anyone ever did with this philosophically troublesome word, except philosophers analysing its meaning.

If Austin (1961a; Travis, 2008) is right about truth, semantic findings can only be accounts of analytic practices: of what the analyst must do to explain a literal meaning. I try to make this clear in Section 5, in terms of the idea of a Lebenswelt pair. First, I am going to argue that Brandom relies on a version of Durkheim's basic rule of method to explain content.

3. Socio-Semantic Facts

What is the relationship between ethnomethodology and ordinary language philosophy? I have suggested three basic interpretations. Ethnomethodology either (1) depends on ordinary language philosophy for an argument against Durkheimian sociology, (2) places Wittgenstein's critique of philosophy in the broader context of a critique of formal analysis, or (3) converges with ordinary language philosophy at the level of motivating insights. I am arguing in this thesis for (3). There is a single argument against formal analysis, an argument from the indeterminateness of formal-analytic findings. For reasons I present in Chapters 3 and 4, it should not be understood as a motivating argument, but rather as a response to obstructive misunderstanding. It is a reparative argument.

There is also, however, a uniquely ethnomethodological argument against Brandom's pragmatism. It turns on the claim that Brandom relies on a broadly Durkheimian distinction between a content-conferring practice and historic uses of language. Garfinkel's argument against "certain versions of Durkheim" only repeats Wittgenstein's critique of philosophy. But because it does no more than that, it helps us see the circularity of Brandom's pragmatism.

The first thing to note is that Brandom often permits himself to say that people "actually *do*" what his model says. Here, for example, is a bold declaration from a friendly yet critical piece on McDowell:

[A] point I want to make about McDowell's [1995] Sellarsian starting point is that the "space of reasons" that he discusses ought to be understood as an abstraction from concrete practices of giving and asking for reasons. The space of reasons is a normative space. It is articulated by properties that govern practices of citing one standing [or position] as committing or entitling one to another – that is, as a reason for another. What people actually *do* is adopt, assess and attribute such standings – and if they did not, there would be no such standings. For in the absence of such normative attitudes of taking or treating people as committed or entitled, there are no commitments or entitlements. They are not part of the furniture of the prehuman world. (Brandom, 1995a, p. 898; original emphasis)

Like McDowell, Brandom is concerned to understand the relationship between "the logical space of reasons" (Sellars, 1997, p. 76) and the logical space of causal explanation. His guiding idea is that norms of rationality must be acknowledged to exist at all. There were no norms of rationality before there were human beings to regard themselves and one another as subject to them. Norms of rationality are irreducibly social in the sense of this "phenomenalist" insight (Gibbard, 2010). Brandom criticises McDowell for having failed to mention the social dimension of the logical space of reasons, but not without agreeing with everything he says:

[McDowell] presents a deep and interesting argument. I think everything he says is true and important. (p. 895)

My only complaints against McDowell [are] accusations of sins of omission – a matter of what he has *not* said. But I have not reproached him for saying nothing about the effects of the discovery of silver in the New World or the spread of the Hussite heresy in Central Europe, even though he has indeed been silent regarding this important topic. I have reproached him for saying nothing about

the social articulation of the space of reasons in the context of a discussion of a deformed conception of the space of reasons that makes it impossible for us to understand how knowledge and reliability are related to such standings. (p. 908)

Brandom is clearly not a sociologist. His concern in this exchange, as he says here, is to understand how knowledge and observational authority are related to socially articulated reasoning. He takes the view that perceptual knowledge is only intelligible in the context of “concrete” practices, only in the context of what people “actually *do*” (to reproduce his own emphasis on the *doing* of it). This on its own would be enough to bring his pragmatism within range of ethnomethodological criticism.

There is more. Recall that Brandom (1994) acknowledges a constraint on semantic explanation. “Semantics must answer to pragmatics” (p. 83). This reads from left to right as a reversal of the basic rule of analytic practice. But as we saw in the last chapter, Brandom makes a distinction between formal and philosophical semantics, which elevates the distinction between meanings and uses to the level of meta-semantic analysis. In Brandom’s idiom of “actual” practices, there are practices that suffice for literal meanings (semantic inferential practices) and practices that presuppose them (pragmatic inferential practices). What Brandom actually means is that a theory of the semantics of a natural language must answer to an account of a practice that suffices for literal meanings (pp. 158–9).

The pragmatist constraint gives rise to a technical difficulty. Brandom resolves it in an unmistakably Durkheimian way. He wants to say that people “actually *do*” what suffices for content, that they produce a practice that confers meanings on expressions caught up in it in suitable ways. This is not a practice in the usual sense. Compare the practice of sweetening tea with sugar. It is obviously part of producing it (part of sweetening tea with sugar) to intend to be sweetening tea with sugar. To produce it accidentally, by accidentally dropping sugar in to a cup of tea, is not to produce it at all. Some people marry. But people cannot do what we call marrying by accident, without intending to marry. Some people cycle to work. And so on. The strangeness of the idiom that allows us to speak of the production of a practice (not to mention the locally enacted production of an endogenously accountable practice) must not be allowed to disguise the fact that it is part of doing things (practices) to intend to do them. This is not to say that the person who does them must have planned in advance to do them. To plan is already to do. So if intending meant planning, we would

never be able to do anything. Ryle (1949) avoids this difficulty with his famous distinction between knowing that and knowing how. Brandom's difficulty is different. His practitioners must be able to produce a practice without intending to produce it at all. The reason is simple: the practice explains what it means to say of a content-bearing item that it bears content. A sentence in English ('Jack sweetened a cup of tea for Jill with sugar') is a content-bearing item. It has no content considered on its own, just as a sequence of sounds or marks, but acquires content in the context of a practice. (So Brandom says [p. 146].) An intention (Jack's intention to sweeten a cup of tea for Jill with sugar) is also a content-bearing item. It is surely incorrect to say that it has no content on its own, since there is nothing like a sequence of sounds or marks to be devoid of meaning in isolation. An intention is the intention it is, the very intention we ascribe to a person doing something. But Brandom wants to explain what it means to say that a content-bearing item has content at all, in terms of what people "actually *do*" as parties to the practice of the game of giving and asking for reasons. He must not take the intelligibility of ascriptions of intentions for granted, but must explain even what it means to say that a person intends to do something. His account of the practice (explanans) must be free of ascriptions of intentions (explanandum). "What is needed," he says, "is to tell a story about practices that are sufficient to confer propositionally contentful intentional states on those who engage in them, without presupposing such states on the part of the practitioners" (p. 7).

How does Brandom expect to discuss features of a practice without ascribing an intention to people who produce it? The solution is to make a distinction between two levels of order. There is (now to be) a "radical disjunction" between social-practical explanations of content and accounts of historic uses of language. This is not the distinction between knowing that and knowing how, even if Brandom does occasionally encourage us to regard it as such. Here, for example, he makes a false analogy between mastering a practice and learning to ride a bike:

It is useful to approach the sort of understanding that is involved in mastering a practice, for instance the practice of applying or assessing applications of a rule, by means of Ryle's distinction between knowing *how* and knowing *that*. Knowing how to do something is a matter of practical ability. To know how is just to be reliably able. Thus one knows how to ride a bicycle, apply a concept, draw an inference, and so on just in case one can discriminate in practice, in the performances one assesses, between correct and incorrect ways of doing these things. (p. 23)

The analogy holds for the practice of sweetening tea with sugar. It even holds for the practice of applying a rule of correct reasoning. We do the most difficult things without deliberation. (Small talk.) We do them effortlessly, fluently, absent-mindedly. That is not what Brandom means. He uses Ryle to introduce a Durkheimian rule of method. Speakers have practical knowledge of performance-governing norms, which they have yet to make explicit in the form of rules. It is Brandom who makes them explicit, as if to intend what speakers do on their behalf. But it is difficult to see what it could mean to say that a person, much less a whole linguistic community, gracefully or clumsily confers meanings on expressions. This is not a case for the application of Ryle's distinction. Nor is Brandom making a distinction between an "action that accomplishes a previously set end," an action intended to be sufficient for content, "and the type of action stressed by pragmatism and phenomenology, which finds its ends within situations" (Joas, 1993, p. 129). He is making a distinction between socially instituted semantic constraints on uses of expressions and historic uses of language.

Things that suffice for content, if they are practices at all, are not like other practices. They have always already been done by speakers of the language. Indeed, they bear a much closer resemblance to transcendental rules of synthesis. This is fair to Brandom. By "practices" (even "concrete" practices) he does not really mean what people do. He means what people must be capable of doing to qualify as speakers. "Practices" are capacities speakers necessarily share. They are "fundamental abilities that make possible participation in those activities by which we (thereby) define ourselves" (p. 4). We define ourselves as "the ones on whom reasons are binding, who are subject to the peculiar force of the better reason" (p. 5). Brandom's idea is that our ways of being subject to rational norms suffice for the content of content-bearing items. This is Durkheim.

Brandom does not cite the sociological literature. Rather, he derives a version of Durkheim's basic rule of method from "a non-standard and rather free-wheeling interpretation of *Being and Time*," as its author describes it (Haugeland, 1982, p. 15). Brandom's real inspiration is Durkheim's (2014) account of mechanical solidarity (or solidarity by similarities), which Haugeland contrasts with a similarly unacknowledged recycling of Durkheim's account of organic solidarity. The purpose of Haugeland's reading of Heidegger is to clarify the idea of rational autonomy. It is a reading, not of Durkheim on the division of

labour in society, but of “Heidegger on Being a Person,” as Haugeland announces in his title. He writes:

Imagine a community of versatile and interactive creatures, not otherwise specified except that they are *conformists*. “Conformism” here means not just imitateness (monkey see, monkey do), but also censoriousness – that is, a positive tendency to see that one’s neighbour do likewise, and to suppress variation. This is to be thought of as a complicated behavioural disposition, which the creatures have by nature (“wired in”). It presupposes in them a capacity to react differentially (e.g. perception), and also some power to alter one another’s dispositions more or less permanently (compare reinforcement, punishment, etc.). But it does not presuppose thoughts, reasoning, language, or any other “higher” faculty. The net effect of this conformism is a systematic peer pressure within the community, which can be viewed as a kind of mutual attraction among the various members’ behavioural dispositions. Under its influence, these dispositions draw “closer” to each other, in the sense that they become similar; that is, the community members tend to act alike (in like circumstances). The result is analogous to that of the gregariousness among range animals: given only their tendency to aggregate, they will tend also to form and maintain distinct herds. (pp. 15–6)

“New animals slowly replace the old,” says Haugeland. This is Durkheim’s “immortal, ordinary society” (Garfinkel, 2002, p. 121; in context below). But most obviously Durkheimian is Haugeland’s account as a synthesis of behavioural dispositions, a synthesis *sui generis* that emerges from social interactions and reacts back on them:

When behavioural dispositions aggregate under the force of conformism, it isn’t herds that coalesce, but *norms*. Other factors (including chance) will determine the number of norms, how narrow (strict) they are, and where they are in the “space” of feasible behaviour; conformism determines only that there will be norms – distinct, enduring clusters of dispositions in behavioural feasibility space, separated in that space between clear gaps where there are no dispositions (save the odd stray). Like herds, norms are a kind of “emergent” property, with an entity and life of their own, over and above that of their constituents. New animals slowly replace the old, and thus a single herd can outlast many generations; likewise, though each individual’s dispositions eventually pass away, they beget their successors in conformist youth, and thereby the norms are handed down to the generations. (p. 16)

There is also Haugeland’s social-practical elucidation of the idea of existential authenticity:

A case of *Dasein* is genuinely self-critical when, in response to discovered tensions among its roles, it does something about them. Thus, I might quit the priesthood and embrace my lover, or decide to subordinate everything to my art. The important point is that I don’t just let some dispositions override others (which may be weaker at the moment); rather, I resolutely alter or eliminate others.

As a unit of self-accountability, I find and root out inconsistency in my overall self-understanding; instead of vacillating unwittingly between one “me” and another, I become one of them (or perhaps a third) constantly and explicitly, and thereby achieve “truer” self-understanding. [...] To be self-owned (“authentic”) is not to rise above the anyone, not to wash away the taint of common sense and vulgar custom, but rather to embrace (some part of) what these have to offer in a particular selective way. The result is a critically realized, maximally self-constant ability to lead an individual, cohesive, limited life: *mine!* This is what’s at stake in trying to understand oneself. (pp. 23–4)

Readers of *The Division of Labour in Society* might turn to Durkheim for help in understanding Haugeland’s Heidegger, just as Dreyfus (1991) occasionally turns to Bourdieu. The difference between authentic and inauthentic existence is the difference between embracing organic solidarity, with its norm of abstract individuality (enshrined in contractual law), and settling for mechanical solidarity, with its norm of unthinking conformity. Without acknowledging Durkheim, Haugeland uses his ideas to explain Heidegger (cf. Bourdieu, 1993, p. 255; Dreyfus & Rabinow, 1993). Brandom uses Durkheim in response to Wittgenstein to explain the possibility of semantic analysis.

Brandom applauds Haugeland for devising a slogan he could also have lifted from *The Elementary Forms of Religious Life*: “All constitution is institution” (Haugeland, *ibid.*, p. 18). Brandom’s version is even more Durkheimian than the original: “All transcendental constitution is social institution” (2000, p. 34; 2002, p. 48). This time, there is an unacknowledged recycling of Durkheim’s critique of Kant’s apriorism. Sellars says somewhere that he means to usher analytic philosophy from its Humean to its Kantian phase. Rorty (1976) endorses *Making It Explicit* as “an attempt usher analytic philosophy from its Kantian to its Hegelian phase” (pp. 8–9). In this movement from Kant to Hegel, readers of Durkheim’s classic could be forgiven for seeing only textbook sociology. Brandom (2000) interprets Hegel, rather than Durkheim, as grounding Kant’s transcendental rules of synthesis in an account of their social institution:

One of Kant’s great insights is that judgments and actions are to be distinguished from the responses of merely natural creatures by their distinctive *normative* statuses, as things we are in a distinctive sense *responsible* for. He understood *concepts* as the norms that determine just what we have made ourselves responsible for, what we have committed ourselves to and what would entitle us to it, by particular acts of judging and acting. Kant, however, punted hard questions about the nature and origins of this normativity, of the bindingness of concepts, out of the familiar phenomenal realm of experience into the noumenal realm. Hegel brought these issues back to earth by un-

derstanding *normative* statuses as *social* statuses – by developing a view according to which (as my colleague John Haugeland put point in another context) *all transcendental constitution is social institution*. The background against which the conceptual activity of making things explicit is intelligible is taken to be implicitly normative essentially *social* practice. (pp. 33–4; original emphasis)

This is closer to Durkheim’s sociology of religion than it is to Hegel’s dialectic. Durkheim (1995) argues against Kant that “the categories of human thought are never fixed in a definite form [but are] ceaselessly made, unmade and remade [and] vary according to time and place” (p. 14). Brandom would not agree with this, since the norms of his ideal language-game must be formally invariant, like Kant’s categories. But it is Durkheim who develops the idea of a synthesis of historic actions, appropriated by Brandom as the idea of a conceptual norm. Durkheim grounds Kant’s pure forms of intuition and thought in the ongoing production of society:

Collective representations are the product of an immense cooperation that extends not only through space but also through time; to make them, a multitude of different minds have associated, intermixed, and combined their ideas and feelings; long generations have accumulated their experience and knowledge. A very special intellectuality that is infinitely richer and more complex than that of the individual is distilled in them. That being the case, we understand how reason has gained the power to go beyond the range of empirical cognition. (*ibid.*, p. 15)

Brandom’s norms are the product of an immense cooperation on the part of speakers, who collectively confer meanings on expressions, speech acts and intentional states. Without acknowledging a debt to Durkheim, he grounds semantic analysis in the ongoing production of meaning (Wanderer, 2008, p. 203).

Coming back to *The Rules of Sociological Method*, Durkheim surely has Kant in mind when he speaks of a “synthesis *sui genesis*” of individual incarnations of society, and virtually formulates the slogan of Brandom’s pragmatism. In *Making It Explicit*, Brandom takes the view that “linguistic norms [should be] understood as *instituted* by social-practical activity” (p. xiii; original emphasis). It is Durkheim, not Hegel, who invents the idea of social institution:

As this synthesis occurs outside each one of us (since a plurality of consciousnesses are involved) it has necessarily the effect of crystallising, of instating outside ourselves, certain modes of action and certain ways of judging which are independent of the particular individual will considered separately. [And] there is one word which, provided one extends a little its normal meaning, expresses

moderately will this very special kind of existence: it is that of *institution*. In fact, without doing violence to the meaning of the word, we may term an *institution* all the beliefs and modes of behaviour instituted by the collectivity; sociology can then be defined as the science of institutions, their genesis and their functioning. (1982, p. 45; original emphasis)

Brandom's Durkheimian idea is that norms implicit in performances that suffice for content are instituted by the collectivity. This is once again to make a distinction between two levels of order. What people "actually *do*" is a practice that confers meanings on expressions. It is not a practice in any ordinary sense. It is the normative structure of an any specifically linguistic practice.

Brandom relies on a Durkheimian conception of social norms to avoid the circularity of ascriptions of intentions to practitioners. In the following passage, he goes so far as to suggest that content is a "side effect" of use:

That the content of intentional states cannot be understood as conferred on them by proprieties governing their significance – when it is appropriate to acquire them and what the appropriate consequences of acquiring them are – follows only if the only candidate for content-conferring use is deliberate, instrumental employment in order to secure the *explicitly* envisaged purpose of being understood as having a certain content. It would not follow that semantic content could not be conferred on intentional states by proprieties *implicit* in the way those states are treated in practice. According to such a conception, the conferral of content might be a side effect of the way they are treated, not requiring that anyone explicitly intend to confer it by their behaviour. (p. 147; original emphasis)

Content is a side effect of historic uses of language, which need not involve an intention to do what people "actually *do*" as parties to the practice of giving and asking for reasons. People need not intend to produce a practice that suffices for content, and must not be required to intend to do anything at all. Otherwise, any explanation of content in social-practical terms would be circular. Nor can the solution be to credit practitioners with a kind of know how. It would still be circular to describe them as doing in an effortless way what discursive deontic scorekeepers do. They could not be doing it without effort unless they were doing it in the ordinary sense, meaning (without having to plan) to do it. The solution is to apply a version of Durkheim's basic rule of method: the normative structure of a content-conferring practice supervenes on what we do.

So if there *is* an argument against Durkheimian sociology, from ethnomethodology or from other quarters, it applies to Brandom's pragmatism. It applies to the idea that norms implicit in content-conferring performances – in performances which are, or which instantiate, what people “actually *do*” – are separable from the performances themselves.

4. The Absurdity of the Idea of Doing a Social Fact

One thing is clear. It would be senseless to say we “actually *do*” social facts. We do an action. For example, you greet me in the corridor, catching my eye and nodding your head as you pass, and I return the greeting. An action may (or may not) lend itself to analysis as an instance of a social fact. But the person who does it still only does an action: not a social fact.

Compare what people studying grammar do in discussing grammatical facts. It is a fact about the grammar of the English language (as we speak it today) that the relative pronoun of a restrictive relative clause is omissible only if it is the object of the verb and not the subject. It omissible in the sentence ‘This is the book (which) I mentioned’ but not in the sentence ‘This is the book which got me interested in philosophy’. In Brandom's terms, this rule is a codification of a performance-governing norm of grammar. We are free to do as we like, regardless of what sounds (or is) grammatically correct or incorrect: to omit or not to omit the relative pronoun of a restrictive relative clause. But to be free in this way is still to be subject to a norm of assessment. The facts (norms) of grammar are the facts.

It would be senseless to say that speakers of English do grammatical facts. A person says a sentence (‘This is the book I mentioned’) and the sentence lends itself to grammatical analysis, serving as an instance of certain grammatical facts. The point of the idea of social institution is to make this distinction.

As for grammatical facts, so for social facts. One finding of the Chicago Jury Project, nicely illustrative of the “logic of explanation” in Durkheimian sociology, is that “the jurors, in disagreeing with the judge, express a *de minimis* sentiment” (p. 86). This is a remark on the sample of 3576 historic decisions. The order of jurors' actions is the order of an action-type, comparable to the order of an expression-type in an analysis of grammar. An account of the action-type “does not assert that the jury is always conscious of this explanation [or] that all jurors would necessarily give this reason if interviewed as to why they acquitted” (p.

87). It asserts that a practice attributed to jurors helps explain the extent and direction of judge-jury disagreement. Again, the point of the idea of social institution is to make this distinction.

As for social facts, so for socio-semantic facts. Jack and Jill are philosophy students. Jack opts for substance dualism and Jill for subjective idealism. Jack has an argument against Jill's position: against this standing of Jill's in the space of reasons. What are they doing? The Brandomian sees an instance of a content-conferring practice. Jack and Jill are producing a practice that confers meanings on expressions caught up in it in suitable ways: *the* practice of giving and asking for reasons. This is not the same as saying that historic uses of language confer content, except in the sense that tokens of the action-type can only be historic actions (the doing of what people "actually *do*"). It would be senseless to say that Jack and Jill do an instance of a content-conferring practice. Jack and Jill do an action. The analyst sees it as an element of a practice that suffices for content. The practice itself (what people "actually *do*") explains the possibility that expressions have meanings at all. They must be woven into historic actions in the right ways, i.e. according to the practice of giving and asking for reasons. This is an example of social institution.

What follows from the absurdity of the idea of doing a social fact? To some sociologists, it might seem obvious to say that there is nothing more to society than linguistically mediated interactions between members of society (see e.g. Blumer, 1969; Hammersley, 1989; Pope, Cohen & Hazelrigg, 1975); in which case, there can be no such thing as a social fact. On this view, what other sociologists describe incorrectly as a social fact is really just an artefact of their own analytic practices. It is an analytic construct designed to solve a problem of formal analysis. This is how Coulon (1995) reads Garfinkel. He introduces Garfinkel's initiatives via three "important sources" (p. 3) of inspiration. After briefly outlining Talcott Parsons' (1949) theory of action and Schutz's (1967) social phenomenology, he presents symbolic interactionism as a position simply incompatible with Durkheim's:

Symbolic interactionism takes a perspective opposite to that of the Durkheimian conception of the actor. Durkheim recognized the actor's capacity of describing the surrounding social facts, but he considered that those descriptions are too vague and too ambiguous to allow the researcher to use them scientifically. He claimed that their subjective descriptions do not belong to the field of sociology. Conversely, symbolic interactionism maintains that the conceptions that actors have of the social world constitute, in the last analysis, the essential object of sociological research. (p. 6)

On Coulon's reading of Garfinkel, ethnomethodology also concerns itself with linguistically mediated social interaction (cf. Francis & Hester, 2004), against the view that society is a phenomenon *sui generis*. Garfinkel is supposed to be saying that the social reality, not to be confused with the objective reality of social facts, exists only as the process of an ongoing production of order. There is such a thing as social reality only insofar as we carry on creating it, which we could hardly be doing by accident. It is the reality, for example, of rules (or norms) of correct decisions. Coulon's Garfinkel takes the fundamentally anti-Durkheimian view that we produce the reality of such rules:

Social reality is constantly created by the actors; it is not a preexisting entity. This is why, for instance, ethnomethodology pays so much attention to the way members make their decisions. Instead of making the assumption that actors follow rules, the interest of ethnomethodology is to uncover the methods with which the actors "actualize" those rules. These methods are what make the rules observable and capable of being described. The practical activities of members, engaged in their concrete activities, reveal the rules and the processes that can be studied. In other words, the careful observation and analysis of the processes used in the members' actions will uncover the processes by which actors constantly interpret social reality and invent life in a permanent tinkering. Therefore, it is crucial to observe how, in a commonsense manner, actors produce and treat information in their exchanges and how they use language as a resource; in short, how they build up a "reasonable" world to be able to live in it. (pp. 16-7)

To build something up (a stock of provisions), or to produce something (a work of art), is to do an action. Ethnomethodology is the study of methods people use to build up or produce society. But on this reading of Garfinkel's initiatives, there is nothing in addition to the action itself: no order other than that of the doing of an action, no reality distinct from that of the creation of reality. There is really no such thing as a social fact (in the sense of Durkheim's distinction).

One problem with this reading of Garfinkel, simply *as* a reading of Garfinkel, is that Garfinkel says the opposite. Consider the anecdote with which he begins Part I of *Ethnomethodology's Programme*:

Ethnomethodology gets reintroduced to me in a recurrent episode of the annual meeting of the American Sociological Association. I'm waiting for the elevator. The doors open. I walk in. THE QUESTION is asked. "Garfinkel, what IS ethnomethodology? The elevator doors close. We're on our way to the ninth floor. I'm only able to say, "Ethnomethodology is working out some very preposterous problems." The elevator doors open. On the way to my room it occurs to me that I

should have said Ethnomethodology is respecifying Durkheim's lived immortal, ordinary society, evidently, doing so by working out a schedule of preposterous problems. The problems have their sources in the worldwide social science movement. They are motivated by that movement's ubiquitous commitments to the policies and methods of formal analysis and general representational theorizing and by its unquestionable achievements. (p. 91)

Perhaps with Husserl in mind,⁶ Garfinkel concedes that the achievements of Durkheimian sociology are "unquestionable" achievements. Is this concession ironic? Does Garfinkel intend to be read as saying that what sociologists regard as unquestionable achievements are in fact questionable? It is difficult to sustain such a reading. Again:

Ethnomethodology is NOT a corrective enterprise. It is NOT a rival science in the worldwide social science movement. EM does not offer a rival social science to the established methods of carrying on analytic studies of what, following Durkheim, EM addresses as immortal, ordinary society. (p. 121)

If ethnomethodology is emphatically *not* a corrective enterprise, how can we interpret it as questioning the unquestionable achievements of Durkheimian sociology? Husserl's phenomenology of the life-world may be of help in answering this question (Garfinkel, 2007; Garfinkel & Liberman, 2007; Liberman, 2007).

5. A Preposterous Problem

I take Garfinkel (2002) to mean that the standard reading of Durkheim's aphorism generates problems. They are "preposterous problems" (p. 91; in context above) if they are meant to be read literally as possible for members, but make sense when read as difficulties, motives and even resources of formal analysis. Bandom provides a good example. I shall call it the candidacy problem, since it has to do with conditions on regarding candidate

⁶ See e.g. Husserl, 1970, pp. 4-5: "The scientific rigour of all these disciplines, the convincingness of their theoretical accomplishments, and their enduringly compelling successes are unquestionable. Only of psychology must we perhaps be less sure, in spite of its claim to be the abstract, ultimately explanatory, basic science of the concrete human disciplines. But generally we let psychology stand, attributing its obvious retardation of method and accomplishment to a naturally slower development. At any rate, the contrast between the 'scientific' character of this group of sciences and the 'unscientific' character of philosophy is unmistakable. Thus we concede in advance some justification to the first inner protest against the title of these lectures from scientists who are sure of their method."

speakers as belonging to “the one great Community comprising all particular [linguistic] communities” (p. 4).

Brandom begins with findings of semantic analysis. He asks what speakers must be able to do for “some bit of vocabulary [...] to express a certain kind of semantic content: to be being taken or treated in practice by the linguistic community *as* a conditional, a singular term, a bit of normative vocabulary, a propositional-attitude-ascribing locution, and so on” (pp. xvii–iii). Consider the content of a definite description. One question for a hypothetical practice is whether it permits the analyst to associate an interpretation of ‘the present King of France’ with that expression. Another is whether people “actually *do*” the practice. Do they do what permits the analyst to associate the practice with their actual uses of language? Brandom imagines an intersection of two groups of sign-producing things. We may regard only some as speakers: human beings, but probably not “chimpanzees, dolphins, gaseous extraterrestrials, or digital computers” (p. 4). All are for the time being to be considered only as “[c]andidates for recognition among us” (p. 4). It would be self-contradictory to deny that *we* are speakers, since to deny something is already to speak. (The pragmatist’s *Cogito*.) But what is the difference between *us* and everything else?

Brandom’s crowning argument explains the possibility of “treat[ing] each of the members of some alien [linguistic] community as one of us” (p. 645). The analyst begins “at home” with rational norms. These, remember, are norms philosophers are meant to be acknowledging in or by doing philosophy. The practice of giving and asking for reasons is what we ourselves “actually *do*” as philosophers. If what we ourselves “actually *do*” suffices for content, and alien speakers must also be able to do it, then Brandom makes what alien speakers must be able to do explicit. Not only that, but alien speakers must themselves be able to say of themselves what Brandom says of members of any linguistic community, equally of us and them. Presumably, what Brandom means in the following passage is that people doing things (jurors deciding a case, philosophers explaining a meaning) are “hypothetical practitioners” for the analyst, who consider their performances as instances of an “idealized Sprachspiel”:

What is required is just that the scorekeeping practices that confer conceptual contents on the fundamental sorts of explicating vocabulary used in stating the theory and specifying the content-conferring discursive scorekeeping practices in the first place be themselves specified within the

terms of the theory. The hypothetical practitioners who play the idealized Sprachspiel of giving and asking for reasons herein described can then be understood as themselves capable of *saying* what they have been supposed to be *doing*: they can make explicit the implicit practical proprieties in virtue of which they can make anything explicit at all. (p. 641; original emphasis)

Speakers must be able to do what explains the meanings of their expressions, and must be able to see their own practices in Brandom's model.

The candidacy problem is indeed a "preposterous" problem if it is meant to be a practical concern for people doing other things. It is a possible problem of semantic analysis, a practical concern only for people doing analysis of literal meanings. To make this clear, we need only read Wanderer's (2008) summary literally as a possible problem for people having a conversation:

[Brandom] show[s] how to develop, via algorithmic elaboration, those abilities required to deploy a vocabulary sufficient to make [content-conferring] gameplaying practices explicit and part of the game itself. The game [Brandom's "idealized Sprachspiel"] and the vocabulary deployed [the "explicitating vocabulary used in stating the theory"] bear a marked similarity to our own rational practices and logical vocabulary. Brandom's bold conjecture is not just that the game is interestingly similar to ours, but that it is ours, so that we should treat the gameplayers as rational and sapient beings and converse with them. (p. 93)

What does Wanderer's concluding remark mean? We should treat creatures doing something (a content-conferring practice) as rational and sapient beings and have conversations with them. An absurdly literal reading: "Pay close attention to the quacking of these candidate speakers. Are ducks doing what *we* do? If so, we should include them in our conversations." In fact, this passage of critical commentary is a shorthand account of an analytic practice. It describes a procedure of semantic analysis, a way of avoiding the circularity of instrumentalist pragmatic theory. To repeat a remark from *Making It Explicit*: "What is needed is to tell a story about practices that are sufficient to confer propositionally contentful intentional states on those who engage in them, without presupposing such states on the part of practitioners" (p. 7). *We* are the hypothetical practitioners. Brandom's explanatory strategy is to say what suffices for content without saying that we intend to do it. This is Durkheim. What suffices for content is a synthesis *sui generis* of historic uses of language, a system of socially instituted conceptual norms.

In this case, the aim of ethnomethodology is not to correct Brandom's pragmatism, but to examine the methodogenesis of the candidacy problem. How does it arise for Brandom and his readers? This is to treat a possible problem of semantic analysis as members' work, no different from a possible problem for people doing other things. Schegloff and Sacks (1974) consider what they call the "closing" problem, a possible problem for people having a conversation. Ryave and Schenkein (1974) consider what they call the "navigational" problem, a possible problem for people walking through campus. The candidacy problem is a possible problem for people constructing a theory of content. It is grist for the mill: just another example of a possible problem for parties to an actual practice. Here, however, the practice is to ask what people doing other things must be able to do.

6. Language-Games and Lebenswelt Pairs

There is an analogy between Durkheimian sociology and analytic philosophy of language. Both trade on a presumption of separability. The sociologist presumes that social facts are separable from individual incarnations of society. The philosopher presumes that meanings are separable from individual uses of language. I am arguing here that there is more than an analogy between ethnomethodology and ordinary language philosophy. At the level of motivating insights, they are one and the same project. This means that the argument of Garfinkel's revisionary reading of Durkheim's aphorism – his argument against Durkheimian sociology – is essentially the same as the argument (or an argument) of Wittgenstein's critique of philosophy. To clarify this point, I am going to suggest that the standard reading of Durkheim's aphorism is guided by "the prevailing conception of meaning [in the philosophical mainstream] and the conception of language of which it is a part" (Baz, 2012, p. 19). I am also going to suggest that "Lebenswelt pairs" (Garfinkel, 2002, pp. 187 ff.), or "FA/EM pairs" (pp. 127 ff.), are ethnographic counterparts of language-games.

If meanings are separable from uses, and must be explained apart from ad hoc considerations, then the basic rule of ordinary language philosophy is wrong. The basic rule is "to take the ordinary and normal use(s) of a philosophically troublesome word as primary, and as the best guide to what, if anything, it refers to, or picks out – in general, or on a particular occasion" (Baz, 2012, p. 20). It is wrong if there are semantic constraints on the roles played by an expression in meaningful speech. The charge against ordinary language phi-

philosophy is that it confuses meanings and uses; or, if pragmatic terms are preferred, that it confuses practices that suffice for meanings and possible language-games. But Wittgenstein questions the presumption of separability. Language-games show that expressions get their meanings from ways of using them. They also show that meanings vary, that there is such a thing as “semantic variety” (Travis, 2008, p. 89) in uses of expressions. For Wittgenstein, semantic variety is a fundamental phenomenon. There are indefinitely many language-games, indefinitely many activities in which an expression E may be woven into actions that confer a meaning on E (not necessarily always the same meaning). On this view, semantic constraints on uses of E are features of language-games, subject to variation with the practical concerns of language-users.

Garfinkel also questions the presumption of separability. In the following notoriously obscure passage, he denies that social facts are separable from members’ work:

For [the sociological mainstream], Durkheim’s aphorism is intact: ‘The objective reality of social facts is sociology’s fundamental principle’. For ethnomethodology the objective reality of social facts, in that, and just how, it is every society’s locally, endogenously produced, naturally organised, reflexively accountable, ongoing, practical achievement, being everywhere, always, only, exactly and entirely, members’ work, with no time out, and with no possibility of evasion, hiding out, passing, postponement, or buy-outs, is *thereby* sociology’s fundamental phenomenon. (Garfinkel, 1991, p. 11)

The objective reality of social facts is a practical achievement. From the perspective of the sociological mainstream, this is once again to confuse A and B: not meanings and uses, but social facts and individual incarnations of society. But Garfinkel calls attention to a special case of semantic variety. Like other expressions, sociological findings get their meanings from ways of using them.

The crucial question is how to understand the availability of an expression. According to philosophical common sense, a word has uses because it has a meaning. For example, we can use ‘slab’ to refer to a building-stone of a certain type because ‘slab’ has a meaning, that of a building-stone of a certain type. A philosophically troublesome word also has uses in conversational English because it has a meaning. The point of semantic analysis is say what its meaning is, what constraints its meaning puts on ways of using it. On this view, there is no variety at the level of literal meanings, but only at the level of their pragmatic modification. According to ordinary language philosophy, expressions are available to language-

users as linguistic equipment. They allow us to do various things. Their meanings on particular occasions for speaking are to be regarded as contingent practical achievements. In Wittgenstein's example, 'slab' is equipment for calling out for a slab, 'pillar' for call out for a pillar, and so on. This is not to appeal to literal meanings, but to notice what is being done by means of these expressions. Nor is it to deny that it makes sense to use expressions only this way or that way under the circumstances, but only to deny that ways of using an expression depend on literal meanings for their intelligibility.

"No order in the plenum" means overwhelming variety at the level of historic actions. There is order only at the level of a very general social phenomenon, to be explained as a phenomenon separable from these (whatever) ways of doing what serves as instance. This is one way of reading Durkheim (1982), who speaks in the following passage of a "duality of kind" between social facts and their individual incarnations:

[I]t is not the fact that they are general which can serve to characterise sociological phenomena. Thoughts to be found in the consciousness of each individual and movements which are repeated by all individuals are not for this reason social facts. If some have been content with using this characterisation in order to define them it is because they have been confused, wrongly, with what might be termed their individual incarnations. What constitutes social facts are the beliefs, tendencies and practices of the group taken collectively. But the forms that these collective states may assume when they are 'refracted' through individuals are things of a different kind. What irrefutably demonstrates this duality of kind is that these two categories of facts frequently are manifested dissociated from one another. Indeed some ways of acting or thinking acquire, by dint of repetition, a sort of consistency which, so to speak, separates them out, isolating them from particular events which reflect them. Thus they assume a shape, a tangible form peculiar to them and constitute a reality *sui generis* vastly distinct from the individual facts which manifest that reality. (p. 54)

But Garfinkel suggests another way of interpreting Durkheim's aphorism. He calls attention to an ambiguity in the idea of something repeatedly observed. A practice is always the doing of a practice. And the doing of a practice is itself an unrepeatable event: just *this* doing of it. There may be variety in ways of doing it. This is not what Durkheim is standardly taken to mean. He is interpreted as saying that facts about society supervene on activities, just as meanings supervene on uses. They remain the same despite variety in what we "actually *do*" as members of society. The analyst is meant to be observing what recurs in historic actions without effort or intention on the part a judgmental dope. Historic actions

are intended. But on another level of analysis, there are individual manifestations or incarnations of the reality *sui generis* of social facts.

There are formulae of social life in which the sociologist takes an interest: rules, aphorism, articles of faith, standards of taste, and so on. On the prevailing conception of meaning in the philosophical mainstream, they have uses in everyday life and in sociology *because* they mean what they do. And they are true or false, just as findings of social science are true or false. Compare Durkheim:

Collective custom does not exist only in a state of immanence in the successive actions which it determines, but, by a privilege without example in the biological kingdom, expresses itself once and for all in a formula repeated by word of mouth, transmitted by education and even enshrined in the written word. Such are the origins and nature of legal and moral rules, aphorisms and popular sayings, articles of faith in which religious or political sects epitomise their beliefs, and standards of taste drawn up by literary schools, etc. None of these modes of acting or thinking are to be found wholly in the application made of them by individuals [in applications made of the formulae of social life], since they can even exist without being applied at the time. (pp. 54–5)

What is Durkheim saying? If meanings are separable from uses, he can only be saying that the meanings of formulae of social life are separable from actions into which they are woven. Meanings are separable from historic uses of language. They “possess the remarkable quality of existing outside the consciousness of the individual” (p. 51).

Garfinkel rejects this reading of Durkheim. He takes the view that sociological findings mean what they do in virtue of their roles in analytic practices. They are no different in this respect from commonly available accounts of activities. Drawing on Husserl’s phenomenology of the life-world, he invites us to (mis)read them as collections of instructions. They “provide for a phenomenon in two constituent segments of a pair,” for the fundamental phenomenon of semantic variety in the doing of a practice:

The EM catalog examines as astronomically, massively prevalent work, various ways that an account that is readably descriptive, say diagrammatically, or as freeway signing, or wall announcements, or in the prose of declarative sentences – can be read alternatively so that the reading provides for a phenomenon in two constituent segments of a pair: (a) the-first-segment-of-a-pair that consists of a collection of instructions; and (b) the work, just in any actual case of following [the instructions,] which somehow turns the first segment into a description of *the pair*. (pp. 105–6; original emphasis)

An account of a practice is the first segment of a Lebenswelt pair (or FA/EM pair), the second segment of which is the doing of the practice. Garfinkel adds a footnote worthy of Wittgenstein studies:

I emphasize *of the pair*. This is in contrast to a use that would read this passage like this: following instructions somehow turns them – i.e., the disengaged and disengage-able [sic] instructions – into a description of following them. (p. 106, n. 29; original emphasis)

Lebenswelt pairs are ethnographic counterparts of language-games. A language-game is an activity “consisting of language and the actions into which it is woven” (Wittgenstein, 1953, §7). It is a way of using an expression. A Lebenswelt pair is a kind of redoable language-game. It consists of instructions to be followed once again (repetition) and the work of following them on some particular occasion (variety). The aim of an ethnomethodological study is not to state conditions for the truth of an account of a practice (literal meanings, social facts), but to show how the account had this or that meaning on a particular occasion for speaking, a meaning inseparable from a way (just *this* way) of using it.

Consider the example (Garfinkel’s own example [pp. 199–207]) of what is “diagrammatically” readable as an account of the assembly of flat pack furniture. A person reading the usual sort of diagram does read it as a collection of instructions, and does follow the instructions in assembling the product. What does the diagram mean? On Garfinkel’s view, it makes no sense to separate it from the lived work of following instructions and ask for the meaning of the diagram itself. This is not to say of the diagram what Brandom says of an expression E in language L, that it is “semantically mute, inert, dead” (p. 146) apart from its role in a content-conferring practice. The candidacy problem follows from the idea of a sign devoid of meaning on its own. Brandom regards meaning as what E acquires in the context of a practice. Garfinkel regards it as what the analyst removes from E by abstracting an expression from historic uses of language. Only to those incapable of using it would the diagram seem to be a mere collection of marks on a piece of paper (semantically mute, inert, dead). That is not what it is.⁷ To ask what it means is to ask what people use it to do (assemble the product) and how, on one occasion or another, they did in fact use it.

⁷ Cf. McDowell, 2009, p. 105: “We fall into the bind that concerns [Wittgenstein] if we abstract sign-posts (for example) from their place in the lives of those who use them. For someone who is party to the relevant practice, a sign-post is something that points the way. And that is what a sign-post as such is. It is true that for

Garfinkel applies the same principle to Durkheim's formulae of social life. Consider the claim that "the jury, in disagreeing with the judge, express a *de minimis* sentiment" (Kalven and Zeisel, p. 86). This, too, is an account of a practice: a sociological account of how jurors typically decide cases. It attaches an analytically constructed meaning to the account, comparable to the meaning Russell (1905) attaches to a definite description. The analyst constructs it from accounts of historic decisions provided by the judges who completed questionnaires on cases. The account explains a social fact: a judge-jury disagreement of 24.6 per cent of all 3576 cases in the direction of jury leniency. It serves as an example of what Coleman (1968) demands from ethnomethodology: illumination of artificially produced data. The meaning of the explanans ("expresses a *de minimis* sentiment") is meant to be obvious. It is the meaning of a phrase in English, containing two vernacular expressions ('to express', 'sentiment') and a piece of legal terminology. The dictionary gives its meaning. Garfinkel irritates Coleman just as his students irritated the victims of his breaching experiments. He asks the analyst to clarify a sociological finding. What is the analyst doing with an account of a practice? What is the language-game? What is the work of explaining the extent and pattern of judge-jury disagreement in a representative sample of criminal jury trials?

As for findings of Durkheimian sociology, so for findings of analytic philosophy of language. We can (mis)read the following passage from *Making It Explicit* as the first segment of Lebenswelt pair:

The model presented here has what might be called a *default and challenge structure* of entitlement. Often when a commitment is attributed to an interlocutor, entitlement to it is attributed as well, by default. The prima facie status of the commitment as one the interlocutor is entitled to is not permanent or unshakeable; entitlement to an assertional commitment can be challenged. When it is *appropriately* challenged (when the challenger is *entitled* to challenge it), the effect is to void the inferential and communicational authority of the corresponding assertions (their capacity to trans-

someone who is not party to the practice, an object that is a sign-post might be merely a board of a certain configuration affixed to a post. But it is disastrous to conclude that what points the way, to someone to whom a sign-post does point the way, is such a thing – a board on a post – under an interpretation. On the contrary, what points the way is a sign-post, something that is what it is by virtue of its involvement in the relevant practice. And it points the way, to those who are party to the practice, without their needing to put an interpretation on it."

mit entitlement) unless the asserter can vindicate the commitment by demonstrating entitlement to it. (pp. 177–8; original emphasis)

The second segment of the pair is the work of following these instructions of philosophical semantics. It turns this passage into a description of the pair. This is not to use the idea of a language-game to do semantic analysis, but to regard even semantic analysis as a language-game. Instead of asking for conditions for the truth of an account of a practice, we ask what the analyst is doing with Brandom's model. For example, we examine the work of using an "idealized Sprachspiel" (p. 641; in context above) to explain how semantic analysis is possible at all.

Why bother? This is the beginner's question, where a textbook sets out these two ways of studying social life: ethnomethodology and Durkheimian sociology. The appeal of findings is understandable. The physicist can say that $E = MC^2$. The sociologist can say that the jury expresses a *de minimis* sentiment. What can Garfinkel say? Or the Wittgensteinian, for that matter? Garfinkel can only give examples of ethnomethodological research. This objection to ethnomethodology (e.g. Coleman, 1968) may stem from a conception of language rejected equally by ethnomethodology and ordinary language philosophy. It may simply beg the question. For what is now in question is the language of professional sociology.

Austin (1961a) and Wittgenstein (1953) both regard semantic variety as a fundamental phenomenon: variety of meaning in and as variety of use. The possibility of semantic variety confronts us with a choice between two approaches to philosophical inquiry: ordinary language philosophy or semantic analysis. To opt for ordinary language philosophy is to opt against the prevailing conception of meaning in the philosophical mainstream, according to which a word has an invariant (core or literal) meaning. Must we really be prepared, as speakers of a language and as philosophers, for variety of meaning? Must we reject the idea of meaning invariance? Austin and Wittgenstein both argue that the burden of proof falls on the analyst of invariant meanings. It is the analyst who takes the more extreme view, assuming "too readily that if we can only discover the true meanings of each of a cluster of key terms [...] that we use in some particular field (as, for example, 'right', 'good' and the rest in morals), then it must without question transpire that each will fit into place in some single, interlocking, consistent, conceptual scheme" (Austin, 1961b, p. 151, n. 1). The ana-

lyst feels unsatisfied with our ordinary ways of explaining meanings. Methods imported from the study of formal languages are preferred. By contrast, “Wittgensteinian conceptual or grammatical investigation [...] is not *essentially* different from what competent speakers regularly do when they wish to become clearer about what they or others say or think” (Baz, 2012, p. xii; original emphasis). To opt for semantic analysis is to opt against our ordinary ways of making meanings clear. Austin and Wittgenstein call for a return from the abstractions of semantic analysis to the things themselves: our various ways of using words. The analyst mentions what we do only to invoke the principle of separability. “What is described,” says Brandom, “is not our actual practice but an artificial idealization of it” (p. 158), an “ideal Sprachspiel” (p. 179), a “model of social practices [?] of giving and asking for reasons” (p. xiv). A description of an ideal language-game is not yet an explanation of meaning, not if questions of meaning can sensibly arise or be settled only at the level of historic uses of language. It is at most a fore-conception of a word’s meaning (Gadamer, 1975, p. 269), an account of a way of using language that may be illuminating here but not there. Augustine’s “picture of the essence of human language” (§1), though adequate to *one* way of using a particular word (‘slab’) or particular kind of word (a common noun), will be inadequate to others. There is semantic variety.

At the level of motivating insights, Garfinkel adds nothing to Wittgenstein’s critique of philosophy. But he does provide a uniquely “ethnomethodological” objection to Brandom’s pragmatism. Brandom works with the idea that performances exhibit a normative structure, the structure of a particular practice. He asks what structure a practice must exhibit to qualify as a specifically linguistic practice (language-game). There is an ambiguity here. When we talk about *what* people did on one occasion or another, we mean either (a) what was done again in the doing of it or (b) an unrepeatable doing of what was done again. (See Sandis, 2017.) If what was done again was a content-conferring practice, there may be variety of meaning in the doing of it. Garfinkel has a way of studying the relationship between (a) and (b): we should regard people’s activities as “identical with” (Garfinkel, 1967, p. 1; 2002, p. 72) their own ways of making them observable and reportable (accountable) as such. So, for example, a way of walking down a busy street (what was done again) is a way of making it observable and reportable as such. To do, then, is also to make visible what is now being done again, to “make it explicit” in Austin’s (1962) sense:

[M]aking explicit' is not the same as describing or stating [...] what I am doing. If 'making explicit' conveys this, then *pro tanto* it is a bad term. The situation in the case of actions which are non-linguistic but similar to performative utterances in that they are the performance of a conventional action (here ritual or ceremonial) is rather like this: suppose I bow deeply before you; it might not be clear whether I am doing obeisance to you or, say, stooping to observe the flora or to ease my indigestion. Generally speaking, then, to make clear both *that* it is a conventional ceremonial act, and *which* act it is, the act (for example of doing obeisance) will as a rule include some special further feature, for example raising my hat, tapping my head on the ground, sweeping my hand to my heart, or even very likely uttering some noise or word, for example 'Salaam'. Now uttering 'Salaam' is no more describing my performance, stating that I am performing an act of obeisance, than is taking off my hat: and by the same token [...] saying 'I salute you' is no more describing my performance than is saying 'Salaam'. To do or say these things is to make plain how the action is to be taken or understood, what action it is. And so it is with putting in the expression 'I promise that'.

(p. 70)

To study practices as things actually done, Garfinkel makes a distinction between *what* was done again and *how* it was done, i.e. between the first and second segment of a Lebenswelt pair. "How" is an unrepeatable doing of something. It allows for semantic variety. There are indefinitely many ways of doing something again. There are, then, indefinably many ways of saying something (not necessarily the same thing) by means of a word.

This distinction between the "what" and "how" of a practice applies not only to practices of interest to the sociological or philosophical analyst, but also to analytic practices (ethnomethodological indifference). Garfinkel (1974) seems to have intended the prefix 'ethno' to mark a difference between sociologists' rules of method and jurors' ad hoc uses of rules of correct decisions. There is also a difference between sociologists' rules of method and their own ad hoc uses of them. Consider representative sampling as a practice. Sociologists' rules of method are accounts of what was (being) done again in this or that, e.g. in the Chicago Jury Project. Or consider Brandom's commitment to methodological pragmatism. This ism of analytic philosophy of language is itself a practice. It is a way of doing semantic analysis, a procedure that permits the analyst to associate meanings with expressions and explain them. A way of doing semantic analysis is grist for the mill of Garfinkel's initiatives. It cannot dodge past the motivating insights of Wittgenstein's critique of philosophy. Ironically, it provides examples of what it denies: variety of meaning in and as variety of use.

Garfinkel's most bewildering initiative (Lynch, 1993, pp. 1–2) is that of sustained indifference to the analyst's distinction between ordinary and analytic practices. The worry here is that Garfinkel does what Baz (2012) accuses the philosophical mainstream of doing, and perhaps what Garfinkel himself (1991, pp. 16–7) accuses the sociological mainstream of doing. Does he beg the question? Does he make a case for ethnomethodology by examining analytic practices from an ethnomethodological point of view, without answering the analyst's objections to his initiatives?

For the time being, I want to restrict attention to a uniquely "ethnomethodological" objection to Brandom's pragmatism. It turns on the claim that the insights of ordinary language philosophy are ethnomethodology's own insights. Garfinkel helps us see that Brandom's commitment to methodological pragmatism is really just another denial of semantic variety. Brandom makes a Durkheimian distinction between a core practice and performances that exhibit its formal properties. He denies semantic variety in pragmatic terms, insisting that there *must* be "something common to all that we call language," something "which makes us use the same word for all" (Wittgenstein, 1953, §65). Arguments of ordinary language philosophy apply equally to the project of formal analysis (Baz, 2012; Travis, 2008) and Brandom's pragmatist grounding of it (McDowell, 2009). Garfinkel's revisionary reading of Durkheim makes this clear. Indeed, we could speak here of regress argument for Garfinkel's initiatives: an argument from the regress performances as algorithmic elaborations of a core practice. What *is* the practice of giving and asking for reasons? Brandom denies semantic variety at higher level of abstraction, ignoring variety in uses of a model of meaning. Without considering his own analytic practices, he gives an account of what is meant to be invariant in language-games. He does not consider the language-games of analytic philosophy of language. Before his deduction of an ideal language-game, the question had been this: can we expect to be able to reduce uses of language to single model of meaning? Brandom sees no reason to be pessimistic (Wittgenstein). He sets himself the task of constructing a model of meaning, an account of what is common to all language-games. Though it *is* a model of meaning, "it should nonetheless be recognisable as a version of what we do" (p. 158). An ideal language-game is like a definition of the word 'game' in English. If it illuminates one way of using language, it may obscure another. Brandom's optimism begs the question. Nowhere does he address this problem, except to

say that speaking beings must be able to do what suffices for content: *the* practice of giving and asking for reasons. But we must still brace ourselves for semantic variety, since there is still a difference between the “what” and “how” of the practice. Brandom does not dodge past this motivating insight of ethnomethodological research, which precedes his response to Wittgenstein and puts it in the correct light.

Ethnomethodology begins just here, where Brandom ends. It does not question Brandom’s achievements, but (mis)reads a model of meaning as the first segment of a *Lebenswelt* pair, i.e. as just one of indefinitely many possible language-games. For the ethnomethodologist, the question is emphatically *not* whether speaking beings must be able to play a part in such-and-such practice, but what the practice *was* on a particular occasion.

7. Summary

Garfinkel’s reading of Durkheim’s aphorism sets ethnomethodology apart from what I am calling Durkheimian sociology. Brandom is no sociologist, as his lack of interest in Durkheim shows. His aims and methods are those of analytic philosophy of language. But Brandom’s pragmatism and Durkheimian sociology have something in common. For purposes of philosophical semantics, Brandom devises his own version of Durkheim’s (1982) most basic rule of method: “to consider social facts as things” (p. 60). He considers certain properties of words, as they might be used on particular occasions for speaking, as attitude-transcendent norms. Speakers acknowledge these norms implicitly, producing performances that exhibit the normative structure of a particular practice. The practice itself is socially instituted by the collectivity. It is not done by practitioners, except in the sense that the analyst is justified in regarding various language-games as instances. What suffices for content – the practice of giving and asking for reasons – is a synthesis *sui generis* of performances subject to normative assessment. Garfinkel does not deny findings of formal analysis, but (mis)reads them as accounts of historic actions. He has a reason for treating them in this way, an argument from the indeterminateness of formal-analytic findings. Brandom ignores what Garfinkel’s reading of Durkheim’s aphorism helps bring to light: the implications of Wittgenstein’s critique of philosophy for formal-analytic studies of practices. There is in this sense a uniquely “ethnomethodological” objection to Brandom’s pragmatism, which turns on the claim that the motivating insights of ordinary language

philosophy are ethnomethodology's own insights. Under ethnomethodological scrutiny, his response to Wittgenstein's critique of philosophy turns out to be circular.

CHAPTER THREE

Ethnomethodological Indifference

I have three aims in this chapter. First, I want to show that the contradiction of (3) is a genuine problem for any interpreter of Garfinkel's writings:

- (1) Ethnomethodology refrains from criticising members' practices.
- (2) Analytic practices are also and *eo ipso* members' practices.
- (3) Ethnomethodology does not refrain from criticising analytic practices

Second, I also want to show in defence of Garfinkel's writings that the contradiction of (3) can be avoided. I am going to present two alternative ways. One is to make a qualifying distinction between first-order studies of members' work and second-order studies of formal analysis *as* members' work. This distinction allows the ethnomethodologist to be critical of sociology at the level of other activities, while refraining from criticism at the level of sociology as an activity in its own right. The other way is to regard even a member's account of "what people do" as gloss for indefinitely many ways of doing things (semantic variety). Finally, I want to make a case for the second way of avoiding the contradiction of (3). The main consideration is that Garfinkel applies (1) to sociology.

In Section 1, I say a little more about the inconstant triad, with the aim of persuading the reader that it does present a difficulty. Sections 2 and 3 jointly address an ambiguity in the idea of ethnomethodological indifference. In Section 2, I introduce the idea of ways of reasoning specific to a domain of problem-solving activity, and use it to define indifference comprehensively as indifference to members' practical concerns. In Section 3, I show how indifference may be restricted to problems of formal analysis, regarded as special cases of members' practical concerns. My basic concern in these sections is to call attention to a potentially confusing bifurcation in accounts of practices. An account of a practice P may have been used by parties to P or by parties to the (derivative) practice of explaining a social fact. For example, an account of a juror's way of deciding a case may have been used by jurors in deciding a case or by sociologists in explaining the magnitude and direction of judge-jury disagreement in a representative sample of trials. In Section 4, I return to the

inconsistent triad and suggest a solution (rejected by the end of the chapter): we can avoid contradiction by making a distinction between first-order accounts of members' work and second-order accounts of formal analysis *as* members' work. Criticism of formal analysis is permissible at the level of *first*-order ethnomethodological studies, impermissible (only) at the level of *second*-order ethnomethodological studies. In Section 5, I present what I regard as the correct interpretation of Garfinkel's initiatives. On this view, the aim of an ethnomethodological study is to examine semantic variety in uses of an account of practices. Indifference takes as its object anyone's aims in using the account. It is indifference equally to the sociologist's aims in explaining a social fact and to the juror's in deciding a case of criminal law. I defend this interpretation on the grounds of simplicity and consistency with Garfinkel's programmatic statements. I also admit a serious defect, namely that it precludes any general motive for doing ethnomethodology.

1. The Interpretative Issue

I recall hearing an ethnomethodologist deliver a paper at an EM/CA seminar on a "trick" of philosophical analysis. Some passages from Garfinkel's writings (as obscure as ever) were read as commentary on examples from Descartes and Russell. In Q&A, I put an inconsistent triad to the speaker. Does Garfinkel criticise members' methods? No. Are philosophers members? Yes. Then does Garfinkel criticise philosophers' methods? The speaker's response surprised me. He suggested that Garfinkel was implicitly critical of formal analysis. If that is so, what are we to make of such passages as the following?

Ethnomethodological studies are not directed to formulating or arguing correctives. They are useless when they are done as ironies. Although they are directed to the preparation of manuals on sociological methods, these are in *no way* supplements to "standard" procedure, but are distinct from them. They do not formulate a remedy for practical actions, as if it was being found about practical actions that they were better or worse than they are usually cracked up to be. Nor are they in search of humanistic arguments, nor do they engage in or encourage permissive discussions of theory. (Garfinkel, 1967, p. viii)

Ethnomethodology is NOT a corrective enterprise. It is NOT a rival science in the worldwide social science movement. EM does not offer a rival social science to the established methods of carrying on analytic studies of what, following Durkheim, EM addresses as immortal, ordinary society. (Garfinkel, 2002, p. 121)

It seems to me that my interlocutor's way of reading Garfinkel must be rejected on account of these passages alone. But the interpretative problem goes somewhat deeper than this.

It is worth making the point that ethnomethodology has the right to defend itself against false accusations. According to Lynch and Sharrock (2003), what sociologists have received as criticism is simply indifference to their concerns. Garfinkel and his followers take an interest in other things. Sociologists have tended to criticise ethnomethodology for its inability to solve their problems, as though ethnomethodology could be only another way of doing sociology – not something radically different from the sociological mainstream, as Garfinkel and his followers say. This is not to criticise the sociologist's practices, except to say that they leave no room for Garfinkel's initiatives. In Lynch and Sharrock's words:

Garfinkel's and related studies of sociological methods are sometimes misconstrued as criticisms, even though Garfinkel explicitly denies that criticism is meant or implied; ethnomethodology carries no message for professional social inquiry about how that might be done better, since the problems with which ethnomethodology is preoccupied are ones that professional inquiry "can have none of". If sociology were to attempt to take up ethnomethodology's program, it would effectively dissolve itself, distracting itself from the problems that have hitherto constituted its identity. Ethnomethodology offers no satisfaction to someone who wants to answer those questions and can only, as it plainly does, prove to be a disappointment for those who retain such a desire. Much criticism of ethnomethodology actually proves to be no more than a reproach for its failure to satisfy the critic's idea of what sociology might be. Hence the predominant form of critical writing on ethnomethodology is that of complaining that it does not do, and is constitutionally incapable of doing, what the critics regard as definitive sociological tasks. (pp. xvii–iii)

"Garfinkel," they say, "explicitly denies that criticism is meant or implied." Lynch and Sharrock do the same. They just say what ethnomethodology *is*, not what sociology *ought* to be, denying the implication of any criticism of mainstream sociology.

But there is more than this to sociology's misunderstanding of ethnomethodology. To complicate matters, Garfinkel (1967) suggests that sociology itself may be selected as a subject matter for ethnomethodological studies. The difficulty here is that sociology (now) includes its view of ethnomethodology. Even this, its view of ethnomethodology, may be selected as an ethnomethodological subject matter. In effect, Garfinkel suggests that the ethnomethodologist is permitted by the idea of an ethnomethodological study to ignore

criticism of that idea. “[E]xcept,” he says, “that quarrels between those doing professional [sociological] inquiries and ethnomethodology may be of interest as phenomena for ethnomethodological studies, these quarrels need not be taken seriously” (p. viii). Dennis (2003) alludes to Garfinkel’s “argument [!] that ‘lay’ and ‘professional’ sociology should be treated as the same in ethnomethodological studies,” and calls it “one of Garfinkel’s most scandalous ideas” (p. 160). If there is an argument to be worked out here, Garfinkel can perhaps be fairly accused of begging the question in his programmatic statements. He assumes what sociologists deny – that his initiatives make sense – by means of a sort of synecdochic inclusion of criticism of his work.

In fact, it is hard to be sure whether it is Garfinkel and his followers or his bewildered critics who beg the question. How is the practice of ethnomethodological indifference to be understood? Is it implicitly critical of the sociologist’s analytic practices? Did my interlocutor at the EM/CA seminar speak truly in response to my third question? Was he right to read Garfinkel as exposing a “trick” of formal analysis? If so, is there a way of avoiding the absurdity of the inconsistent triad? There is a genuine interpretative problem. In what follows, I am going to address it in terms of the idea of domain-specific ways of reasoning.

2. Indifference to Members’ Practical Concerns

Consider the idea that there are ways of reasoning specific to a domain of problem-solving activity. Garfinkel and Sacks (1970) suggest some examples: “practices of legal reasoning, conversational reasoning, divinational reasoning, psychiatric reasoning” (p. 346) – meaning not, of course, that “practices of reasoning” are invariant across these domains (the law, conversation, divination, psychiatry), but that each domain has its own practices of reasoning. If members are people competent in the handling of legal (or whatever) problems, the idea is that they are competent in ways of reasoning specific to the domain of legal (or whatever) problems.

Ethnomethodology can be understood as the study of domain-specific ways of reasoning. The aim of an ethnomethodological study of legal (or whatever) problems is not, of course, to solve them, but to identify and describe a member’s ways of reasoning about them. Garfinkel (1967) recommends a “study policy” in some ways reminiscent of Ryle. The researcher is to suppose, contrary to textbook readings of Durkheim’s aphorism, that

ways of doing things are “identical with” (p. 1) ways of reasoning about the doing of them. This will be a difficult view for those who picture reasoning as explicit (verbalised) thinking through. In any case, the broadly Rylean study policy is to regard as one and the same (1) the doing of an action and (2) reasoning about the doing of it.

The clearest illustrations are conversational. It is easy to see that people talking are doing certain analysable things without having to stop and think (Sacks, 1963). Their ways of doing them are perfect examples of reasoning about something in and as the doing of it. Here, for example, is an example of a competent handling of the “closing problem” (Schegloff & Sacks, 1974):

- A Well that’s why I *said* ‘I’m not gonna say anything, I’m not making *any* comments // about anybody’
B Hmh
A Ehyeah
B Yeah
A *Alrighty*. Well *I’ll* give you a call before we come down. O.K.?
B O.K.
A *Alrighty*
B O.K.
A We’ll see you then
B O.K.
A *Bye* bye
B Bye

People talking are not reasoning as conversation analysts do. But they are evidently attuned to things of interest to conversation analysts. Without having to stop and think, they do these actions in case of something (misunderstandings) and for the sake of something (completion of the conversation). Their ways of using language (“*Alrighty*,” etc.) are, so to speak, consequents and antecedents of conversational reasoning. People are reasoning here about a sequence of concerted practical actions, about the bringing of *this* conversation to a close. This is reasoning in and as the doing of what is being reasoned about.

Reasoning is to be distinguished in this broadly Rylean way from propositionally explicit thinking through. Still, it will be practically necessary from time to time for a member to give an account of an action. Think of people queuing. One says to the other, “I was here first.” The other replies, “You left the queue.” Is it true that the person who was there first left the queue? This is to be settled in light of the circumstances. An account of the action in question – of an alleged leaving of the queue – will now be part of that sequence of actions that *is* the forming of a queue. In such cases, domain-specific ways of reasoning in-

clude ways of giving accounts. They include “accounting practices” (Garfinkel, 1967, p. 1), which are also to be distinguished from propositionally explicit thinking. An accounting practice is not a way of preparing to do an action, but part of the doing of it. Garfinkel and Sacks (1970) characterise this phenomenon in terms of the distinction between indexical and non-indexical expressions. They give the example of Durkheim’s aphorism:

[T]he natural language formula, ‘The objective reality of social facts is sociology’s fundamental principle,’ is heard by professionals according to occasion as a definition of [sociologists’] activities, as their slogan, their task, aim, achievement, brag, sales pitch, justification, discovery, social phenomenon, or research constraint. Like any other indexical expression, the transient circumstances of its use assure it a definiteness of sense as definition or task or whatever, to someone who knows how to hear it. (pp. 338–9)

Durkheim’s aphorism is analytic gloss for indefinitely many ways of doing sociology. But it is not like a philosophical theory of action, which may belong to a domain of activity distinct from that of a case for its application (as when a dentist extracting a tooth or a logician proving a sequent is said indifferently to be “doing an action”). An account of the practice of queuing may be spoken by people queuing in their interests *as* people queuing. Likewise, an account of Durkheim’s aphorism may be spoken by people doing sociology. It is an “indexical” expression: an account of just *this* sequence of practical actions, occurring within it and helping it along.

An account may be ambiguous as between (1) a description of a practice P and (2) an identification of the doing of something *as* the doing of P. Consider rules of play. One person asks another, “Would you like to go first?” The other replies, “White begins.” Black speaks a rule of play that describes what White does in making this next (whatever) first move. In the broadest terms, an account of “formal structures of practical actions” (Garfinkel & Sacks, 1970, p. 346) may be reasoning about such things in and as the doing of them.

Garfinkel’s own term for such reasoning about X in and as the doing of X is “practical sociological reasoning” (Garfinkel, 1967, pp. viii; Garfinkel & Sacks, 1970, p. 346). It is “practical” in the sense that people doing things are concerned to do them. It is “sociological” in the sense that they rely in their reasoning on pre-theoretical knowledge of the social world. People talking rely on knowledge of how members of their society talk. Consider the

difference between dinner party conversation and cross-examination in a court of law. If it is part of talking to be accountable for a way of talking, it is part of being accountable for a way of talking to be reasoning in the Rylean way about the social setting of a question, warning, refusal, joke, or whatever else.

It is obviously part of practical sociological reasoning that members care about the adequacy of ways of doing something. Is *this* way of standing adequate for purposes of forming a queue? Is *this* way of asking questions adequate for purposes of cross-examination? In the first instance, ethnomethodological indifference is indifference to such questions. Negatively, this means that the point of a study of queuing or cross-examination as members' work is not to queue or cross-examine. Positively, it means that people doing ethnomethodology are themselves concerned to do something else. They are concerned to show that (even) judgments of practical adequacy are ways of doing things. The point is to present them as phenomena in their own right.

Suppose we were to ask of a sociological study, "Is *this* way of collecting and analysing data adequate for purposes of explanation?" From an ethnomethodological point of view, a practice of professional sociological reasoning (*this* way of collecting and analysing data) is just another example of reasoning about something in and as the doing of it. It is reasoning in and as the doing of a method of research. In this case, ethnomethodological indifference is still simply indifference to questions of practical adequacy. Compare Garfinkel and Sacks (1970):

Ethnomethodological indifference cannot be viewed as a position which would claim that no matter how extensive a volume like Berelson's [*The Behavioural Sciences Today*] might become, problems could yet be found. Nor, in that regard, would it be the case that insofar as the predictive efficacy of professional sociology had an asymptotic form, one could count on a margin of error as a stable property within which research could proceed. Counting on the fact that given the statistical orientations of professional sociology one would always have unexplained variance is not our way of locating yet unexplained phenomena. Our work does not stand then in any modifying, elaborating, contributing, detailing, subdividing, explicating, foundation-building relationship to professional sociological reasoning, nor is our "indifference" to those orders of tasks. Rather, our "indifference" is to the whole of practical sociological reasoning, and *that* reasoning involves for us, in whatever form of development, with whatever error or adequacy, in whatever forms, inseparably and unavoidably, the mastery of natural language. Professional sociological reasoning is in no way singled out as a phenomenon for our research attention. Persons doing ethnomethodological stud-

ies can “care” no more or less about professional sociological reasoning than they can “care” about the practices of legal reasoning, conversational reasoning, divinational reasoning, psychiatric reasoning, and the rest. (pp. 345–6)

The point is not to judge the adequacy of a method of sociological research, but to present it (a method of sociological research) as a phenomenon in its own right. Accordingly, the ethnomethodologist is to adopt an attitude of indifference to questions of adequacy, and view even criticism of professional sociological reasoning as reasoning about something in and as the doing of it.

3. Indifference to Problems of Formal Analysis

Formal analysis adds an extra dimension to ethnomethodological indifference. Consider Brandom’s pragmatist theory of content. Brandom holds (1) that there is a practice P that is necessary and sufficient for content and (2) that P is the practice of giving and asking for reasons. Brandom’s description of P is an account of the “simple commitment structures” of any linguistic performance. Domain-specific ways of reasoning are to be seen as instances. For example, jurors deciding a case of criminal law are to be seen as doing essentially the same thing as sociologists explaining the extent and direction of judge-jury disagreement: as doing *the* practice of giving and asking for reasons. It is clear that P is not a practice in Garfinkel’s sense. It is not something members are concerned to do, but something noise-making creatures must be capable of doing to qualify as speakers. So it is not to be confused with any practice of legal (or whatever) reasoning. On the other hand, a practice of legal (or whatever) reasoning may be regarded with formal-analytic indifference as an instance of P. This is Brandom’s unacknowledged reworking of Durkheim’s rule of method. His norms are meant to supervene on what speakers do, most obviously where speakers are reasoning about something in propositionally explicit ways.

How is the ethnomethodologist to respond to Brandom’s pragmatist theory of content? There may seem to be just two basic options.⁸ One is to deny the existence of P. The other is to ignore problems of philosophical semantics and examine members’ practical

⁸ See Section 5 for a third option.

concerns. I am going to consider the first option in the next section. The second generates an ambiguity that must now be addressed.

It is especially clear in the case of Brandom's pragmatism that an account of a practice may not be "indexical" in the ethnomethodological sense. Brandom offers P as a means of addressing problems of philosophical semantics. It is meant to help us see what it means to say that an expression has a meaning at all. Jurors deciding a case are to be seen as doing P. But it is no part of what jurors do that semantic and intentional concepts remain to be elucidated in terms of a practice. Problems that arise for Brandom in constructing a pragmatist theory of content, or for Brandom's readers in assessing his theory, do not arise for jurors in deciding a case. Nor do problems that arise for Kalven and Zeisel (1966) in explaining disagreement between judge and jury. It is not paradoxical to say of their study that it ignores jurors' actual practices. For although Kalven and Zeisel defend the claim that the jurors tend to something (that they decide a case on the grounds of sentiments about the law), they infer jurors' actual practices from a preliminary finding as explanations. They begin with the discovery of a disagreement rate of 24.6 per cent of 3576 cases in the direction of jury leniency. Its role in their reasoning is similar to that of a semantic finding in Brandom's pragmatist theory of content. The philosopher conjectures that P explains the fact that an expression means what it does. The sociologist conjectures that P explains judge-jury disagreement. Hearing of P, the ethnomethodologist can decide to do either of two things: (1) ignore the philosopher or sociologist and examine jurors' practices or (2) examine practices of philosophical or sociological reasoning.

By way of illustration, I am going to review some questions of practical adequacy from Kalven and Zeisel's study. They serve as examples of possible objects of indifference. They also serve to illustrate the ambiguity of the idea of ethnomethodological indifference, which is indifference either (1) to problems of formal analysis or (2) to members' practical concerns. In this case, (2) is analytic gloss for problems that arise of jurors in deciding a case of criminal law.

Five chapters of *The American Jury* are devoted to problems of sociological method. Chapter Three begins with a litany of obstacles to the application of "an authoritatively established theory [of sampling]" (p. 34). In particular, since judges could not be forced to participate, the sample of 3576 cases did not satisfy the requirement of random selection. It

was “simply the residue of self-selecting judges, derived from a list imperfect to begin with” (p. 35). This was “[t]he most critical challenge to the validity of the sample, namely that it comprise[d] only ‘cooperative’ judges” (pp. 42–3). There was another question of practical adequacy concerning the sample:

We were unable to control the exact period in which the cooperating judges did in fact cooperate. Some helped for a short while only and some for many months; some judges submitted but a few reports, others a large number. (p. 38)

Th[is] problem is not an easy one to solve, since one cannot make a direct comparison of the non-cooperators, who by definition have deprived us of data about themselves. One can, however, as is so often the case in social science, “take the second ship.” In this case, the next best thing involved comparing judges who cooperated for only a very short time and sent in only a few reports [...] with judges who cooperated for a long time and sent in many reports. If, so the argument runs, there is no difference between highly cooperative and minimally cooperative judges, it is then a fair inference that the judges who did not cooperate at all would not be too different from those who cooperate to only a minimal degree. (p. 43)

In the case of a study of jurors’ decision-making as members’ work, there is no need to decide whether the sample used was adequate for purposes of sociological explanation. On the other hand, the practice of representative sampling is just another example of domain-specific reasoning. The ethnomethodologist can opt either to ignore or examine it.

Chapter 4 describes the collection of data relevant to an explanation of judge-jury disagreement. The cooperative judges were asked to return a questionnaire at the end of individual trials. There were two versions. Questionnaire I “was a relatively open, unstructured effort” (p. 47). It consisted of 20 questions, the majority of which required little more from the respondent than an ability to record the facts. It asked what crimes the defendant had been charged with, whether the jury had been instructed on other included offences, whether the defendant had taken the stand, and so on. It also asked for an account of the decision “[i]n the event of a significant difference between the judgement you [the judge] would have rendered and the jury’s verdict” (p. 528). It elicited specific reasons: “Was it because of: (a) The composition of the jury? (b) Crucial events during the trial? (c) Personalities in the case (defendant, witnesses, attorneys)? (d) Peculiarities of the case? (e) Other reasons?” It also left room for the judge to comment further. Questionnaire II was “more tightly controlled and more detailed” (p. 47). It also included “a series of new descriptive

questions that involved a summary, *gestalt* evaluation by the judge” (p. 49). For example, after asking the judge whether “there [were] circumstances in *this* case which made the crime *particularly atrocious*” (eliciting “Not out of the ordinary,” “Somewhat so” and “Very much so”), it then asked what the circumstances were (p. 532; original emphasis). It also simply asked (p. 534; original emphasis): “If you disagreed with the jury, what in your opinion was the *main reason for the jury’s verdict?*” Questionnaire II made up 33 percent of all 3576 cases. It was in this data that explanations were sought.

It was a point more in a favour of the use of a questionnaire that respondents had a judge’s expertise. If they had lacked mastery of the language of legal reasoning, a trained interviewer would have been needed to ensure the relevance and reliability of data. Failing that, questions would have needed to be “rigidly framed once and for all in advance” (p. 46) to rule out randomness of inexpert interpretation (guesswork on the respondent’s part). But the respondents “were anything but inexpert; they were judges being asked about a matter in which they had the greatest interest and the greatest professional competence” (p. 47). Judges could be expected to understand a jury’s decision, and to know in particular why a jury might have made an incorrect decision.

Chapter 7 begins with some general comments on explanations of conduct, followed by an example of a jury’s conduct in deciding a case. The method here was to look for patterns of disagreements in crimes of different kinds. For example, there was a “sharp contrast between the 41 per cent jury leniency in indecent exposure cases and the 6 per cent leniency in narcotics trials” (p. 86). This made sense on the hypothesis of “a bootlegging of the tort concepts of contributory negligence and assumption of risk into the criminal law” (p. 243), which explained various instances of jury leniency in terms of “jury sentiments about the law” (p. 106). With crimes as diverse as homicide, auto theft, petty theft, fraud, liquor violations and rape, jurors tended to acquit where the judge would have convicted (or to convict on a lesser charge) if they felt the victim was partly to blame for the harm received. As one judge reported (p. 243): “Because the jury did not follow the charge of the court, they saw some evidence of contributory negligence on part of person assaulted. Contributory negligence is no defense in the laws of this state.”

Chapter 37 deals with the problem of the “non-homogeneity” (p. 464) of judge and jury. If no two judges or juries are exactly alike, what is to be made of a general explanation

of judge-jury disagreement? It is fair to say that Kalven and Zeisel beg the question. Instead of answering, they “engage primarily in a methodological exercise” (p. 467): a statistical analysis of the behaviour of judges and juries in cities of different sizes in different regions of the United States. If analysis established a certain degree of homogeneity, it was a foregone conclusion that it would.

Chapter 38 addresses two questions of practical adequacy. The first concerns the decision to derive explanations from cases of disagreement. Kalven and Zeisel declare that “a theory as to what causes judge-jury disagreement has been implicit throughout the book,” and present it as “nothing more complicated than that agreement is caused by the absence of whatever causes disagreement” (p. 475). Since jurors are given instructions as to how to decide, it did not call for explanation that they decided correctly when they did. Indeed, an expression of a correct decision is just an account of a principle of criminal law. Only in cases of disagreement was an explanation sought, by extrapolation from particular explanations provided by the judges.

The second question of Chapter 38 concerns the fact that “the methodology of this study cuts us off from what would appear to be the humanly most interesting and colourful aspect of the whole jury enterprise, the deliberation process” (p. 474). This is a more serious objection to the design of the study. Kalven and Zeisel make three points. In deliberations of mock juries, it is difficult to discover any particular order:

The initial impact of [a] deliberation, judging from our experience, is fairly standard. There is at first, in William James’ phrase about the baby, the sense of buzzing, booming [sic], confusion. After a while, we become accustomed to the quick, fluid movement of jury discussion and realize that the talk moves in small bursts of coherence, shifting from topic to topic with remarkable flexibility. It touches an issue, leaves it, and returns again. Even casual inspection makes it evident that this is interesting and arresting human behaviour. It is not formal debate; nor, although it is mercurial and difficult to pick up on, is it just excited talk. (p. 486)

There is also the fact that that “real causes” of a decision may not be known to the individual juror. This means that they will not be talked about:

To begin with, we suspect that the jurors’ talk may often be not very revealing. If the theory of this book is correct, the real cause of a juror’s decision will be in many instances a fact he himself is only dimly aware of and perhaps unable to articulate. (p. 486)

An added difficulty is that the individual juror may be secretive or reticent, leaving the analyst to guess actual reasons for an incorrect decision:

But even when the juror is aware of his motives, he may not wish to disclose them. He may choose to argue his point more in terms of proper legal considerations than in terms of the extra-legal ones he privately finds persuasive. Finally, we know that some jurors either cannot or prefer not to talk much and rather limit their participation to voting. (pp. 486–7)

Kalven and Zeisel are “thus disposed to assert that the analysis of jury deliberation would add little to a theory of why the jury disagrees with the judge” (p. 487). This would be an absurd conclusion if not for the thesis that facts about society are separable from facts about their individual manifestations. In fairness to this example of Durkheimian sociology, it is correct to ignore the “blooming, buzzing confusion” (Garfinkel, 1991, p. 13) of members’ talk. The explanandum is not a feature of any actual case of jury deliberations. It is an aggregate phenomenon. Accordingly, the thing that explains it is not to be found in what jurors did on some particular occasion.

So much for these problems of formal analysis. The ethnomethodologist can decide either to ignore or examine them. Garfinkel (1967) ignores them. He turns his attention to what he calls the “official juror line” (p. 108): a list of some ten rules for making a legally correct decision. Consider the rule that the juror is to put the law before considerations of fairness. This is something parties to a decision might well have been concerned to do. We can imagine questions of practical adequacy posed by jurors for the sake of this rule of correct decisions. Garfinkel takes a radically altered interest in them. His aim is not to judge the adequacy of certain ways of deciding, but to present them as phenomena in their own right. One way of understanding such indifference is to think of a rule of correct decisions as analytic gloss for members’ work. The point of indifference is to make diverse uses of a rule perspicuous as such, regardless of the correctness or incorrectness of decisions. Garfinkel notes the availability of his list of rules to jurors:

Jurors learned the official line from various places: from the juror’s handbook; from the instructions they received from the court; from the procedures of the *voire dire* when jurors were invited by the court to disqualify themselves if they could find for themselves reasons why they could not act in this fashion. They learned it from court personnel; they learned it from what jurors told each other, from TV, and from the movies. Several jurors got a quick tutoring by their high school children who had taken courses in civics. Finally, there is the fact that in the course of their ordinary

affairs, jurors had built up a stock of information about procedures that were in their view merely theoretic, impractical, playful, make-believe, “high-class,” “low-class” and so on. (p. 110)

Compare the availability of Durkheim’s aphorism to sociologists. A rule of the official line is once again an “indexical” expression. It facilitates reasoning that is identical with what is being reasoned about. To present it as such, we need to put all thought of problems of formal analysis aside. For these other questions of adequacy have nothing to do with members’ work.

To sum up. In the first instance, ethnomethodological indifference is indifference to questions of practical adequacy in some particular domain of activity. Formal analysis complicates this practice of ethnomethodological research. Consider any relationship between two activities A and B such that A involves analysis of B. Problems that arise for parties of A in analysing B do not arise of parties of B. They are “formal structures” of another activity. In such cases, the ethnomethodologist may either (1) ignore problems that arise for parties to A, so as to examine problems that arise for parties to B, or (2) examine problems that arise for parties to A instead. Either way, it will be necessary to adopt an attitude of indifference to questions of practical adequacy. In the first instance, questions of practical adequacy will be (1) problems that arise for parties of B. But they may also be (2) problems that arise for parties to A. For example, they may be either (1) problems that arise for jurors in deciding a case of criminal law or (2) problems that arise for sociologists in explaining a disagreement rate of 24.6 per cent of all 3576 cases in the direction of jury leniency.

4. Critical Indifference

Garfinkel’s followers present their work as a radical alternative to mainstream sociology (Lynch, 2016). They characterise sociology as it is usually done as formal analysis, i.e. as analysis of constructed models of people’s actual activities. I have given the example of Kalven and Zeisel’s *The American Jury*. Instead of considering examples of juror’s actual deliberations, Kalven and Zeisel construct and analyse a particular model of decisions. Jurors were thought to use doubts, uncertainties, ambiguities, etc. to ignore legal principles and decide on the basis of certain popular sentiments about the law. The analytic task, as it emerged from reflection on the data of the study, was to test this hypothesis by means of

the sample, looking for patterns of disagreement in crimes of different kinds. The hypothesis was true of these 3576 cases, and of the universe of criminal jury trials, if it brought order into sociological reflection on this data.

Ethnomethodologists eschew formal analysis. Invoking either existential phenomenology or Wittgenstein's later writings, they propose to examine methods people use to make their own actions analysable. So far from denying the distinction of Durkheim's aphorism, they insist that sociologists are primarily concerned to discover, demonstrate and explain social facts. The ethnomethodological alternative is not to examine practical actions as individual manifestations of social facts, but to examine them simply as such. Individual (unrepeatable) decisions are not to be analysed as cases for the application of a theory of decision-making, but as a phenomena in their own right. Let Kalven and Zeisel explain the fact that judge and jury disagreed in 24.6 per cent of all 3576 cases in the direction of jury leniency. In the meantime, Garfinkel's followers turn their attention to the language-games and lived work of individual decisions.

Talk of what is "radical" in Garfinkel's initiatives confronts the student of social life with a stark either/or. At the root of interest in these (whatever) particular social phenomena, formal analysis and ethnomethodology are mutually exclusive. But they do not stand in simple opposition to one another. The thought here is that formal analysis implies the possibility of ethnomethodology, and that ethnomethodology in turn takes findings of formal-analytic studies as departure points. Negating formal-analytic negations, it respecifies as language-games and lived work what has already been respecified (language-games and lived work) as an individual manifestation of a social fact. Formal analysis and ethnomethodology are in this sense "incommensurable, asymmetrically alternate technologies of social analysis" (Garfinkel & Wieder, 1992).

The either/or is such that there *can* be no intelligible criticism of formal analysis. We must always bear in mind that the ethnomethodologist is indifferent to problems of formal analysis. This is not the same as a physicist's indifference, *qua* physicist, to problems that arise for the musicologist, which have nothing to do with physics. The ethnomethodologist uses problems of formal analysis to specify a subject matter, by means of the "procedural policy" (Garfinkel & Sacks, 1970, p. 345) of active indifference. Problems of formal analysis, the analyst's own problems (as distinct from members' practical concerns), are to be noted

and set aside. There is something else to study. Accordingly, Garfinkel (2002) speaks ambiguously of “a procedure for not *needing* to consult the corpus of classic methods and findings with which to carry out the tasks of EM research” (p. 170; original emphasis). Problems of formal analysis are things with which to carry out ethnomethodological study tasks. But there is no need for the ethnomethodologist to consult the corpus of classic methods and findings. For example, problems that arose for Kalven and Zeisel serve as points of departure for a study of jurors’ actual deliberations. But Garfinkel had no need to consult an “authoritative theory of sampling,” etc. Indifference is indeed a procedure here: a way of uncovering a vast diversity of endogenously accountable practical actions.

On the other hand, we may begin to wonder what ethnomethodology can be expected to reveal, except that there *is* a vast diversity of endogenously accountable actions. It is one thing to say that ethnomethodology must not be judged by sociological standards, as if it meant itself to be contributing to theories of social facts. It is another thing to make a positive case for doing ethnomethodology. Unless it is enough to preach to the converted, Garfinkel’s followers should be able to say what we learn from ethnomethodological studies. After all, Garfinkel was not alone in calling attention to the “fundamental phenomenon” of studies of social life, the fact that people do somehow produce order. Compare Bateson (2000), Goffman (1974) and Luhmann (1995). These are thinkers with something in general to say about order production – about schizogenesis, management of frames or self-observing systems. From an ethnomethodological point of view, theories of order production must also be regarded as examples of formal analysis, i.e. as analysis of people’s actual activities *as* schizogenesis, management of frames or self-observing system. Ethnomethodology eschews the substitution of a model of people’s actual activities for the things themselves. But why does it do that? What is the radical alternative to formal analysis good for?

It is tempting to reply with criticism of formal analysis. If there *is* something to criticise, there is a reason to opt instead for ethnomethodology. The difficulty here is to avoid the contradiction of (3):

- (1) Ethnomethodology refrains from criticising members’ practices.
- (2) Analytic practices are also and *eo ipso* members’ practices.
- (3) Ethnomethodology does not refrain from criticising analytic practices

One solution is to make a distinction between first-order studies of members' work and second-order studies of formal analysis *as* members work. Note the parenthesis of this programmatic statement:

The simple point about 'social actions' is that the relevant criteria of identity belong to the social settings in which those actions occur, and are not contrived by or taken from the theories of social science (except in a secondary and derivative case). (Hutchinson, Read & Sharrock, 2008, p. 96)

At the level of first-order ethnomethodological studies, indifference is a response to global criticism of formal analysis. At the level of second-order ethnomethodological studies, there is simple indifference to questions raised by such criticism. Where (1) applies to the language-games and lived work of jurors' decision-making, (3) applies to the sample, questionnaires and general methodological principles of Kalven and Zeisel's study. Where (2) applies to these examples of analytic practices, there can be nothing to criticise. Formal analysis is now to be regarded as just another example of members' work.

What may be said to be wrong with formal analysis in general? One possible criticism, nicely illustrated by Brandom's pragmatism, is that it floats free of people's actual activities. In its original form, Durkheim's aphorism was a reply to criticism on these lines. In his Preface to the Second Edition of *The Rules of Sociological Method*, Durkheim writes that "objections often arise from the fact that one has refused to admit, or not admitted without reservations, our basic principle, that of the objective reality of social facts" (p. 45). This is circular if the legitimacy of his "basic principle" is at issue. The thought might be that a fact about society can only be a fact about individual interactions between members of society (Bourdieu, 1990; Pope, Cohen & Hazelrigg, 1975; O'Neill, 1975; Taylor, 1982). In that case, formal analysis is fundamentally ill-conceived. Accordingly, one possible reason for doing a study of members' practices is to reveal a gap between people's actual activities and an analyst's account of them.

First-order ethnomethodological studies provide three distinct arguments for the global ("floats free") criticism of formal analysis. First, the analyst must be able to see what is relevant in historic actions to the explanation of a social fact. This calls for a member's understanding. Formal analysis passes over that achievement. Consider the following example from Kalven and Zeisel:

It remains to say a further word about *our* role in making assessments. In about 10 per cent of the cases some reasons for disagreements were assessed by us and not by the judge. This variant on reason assessment recommended itself at several points. In many instances little more was involved than a matter of form. The judge would leave blank the space provided for the explanation, but would mention elsewhere on the questionnaire, simply by way of describing the case, factors which clearly implied an explanation. Thus he might describe in sensitive detail a characteristic which made the defendant sympathetic to the jury, without explicitly offering this as an explanation of jury leniency. In other cases, the judge, although again offering distinctive and helpful facts in briefing the case, might announce that he was mystified by the jury's disagreement and had no explanation for it. Occasionally a judge would give one reason and we would add another based on the implications of his description. In the end there are two things to emphasize about our intrusion into the assessment process. First, the clues always came from something the judge himself had said and considered worth reporting. Second, in view of our training and intense experience with the questionnaires in the study, we acquired some degree of expertise for the very special problem at hand. (p. 97; original emphasis)

The analyst relies on "some degree of expertise" to find the explanation of a social fact in details of historic actions (a judge's). This is different from relying on a method of professional sociological reasoning. It is not discussed in chapters devoted to methodology. If it is mentioned at all, it is mentioned only in passing – as it is here.

Second, to see what is relevant in historic actions to the explanation of a social fact, the analyst must be able to consider historic actions as phenomena in their own right. Kalven and Zeisel give the example of a sympathetic description, which they interpreted as having explanatory relevance. Presumably, this was not to foist a reading on a judge's way of filling in the questionnaire. Something must first have appeared to them, a detail which seemed to be properly described as an explanation. (The jury took a liking to the defendant.) This argument could be worked out in terms of the openness of the hermeneutic circle. Formal analysis involves a kind of foreclosure of interpretation, such that the analyst insists on finding a certain practice no matter what. Genuine explanation could be said to depend on what differs hermeneutically from formal analysis, even in cases of what was meant to *be* formal analysis. Alternatively, the argument could be presented in terms of the documentary method of interpretation (Garfinkel, 1967). We learn from first-order studies that members make a distinction between "witnessed actual appearances" (p. 76) and their own adumbrations of meaning (common-sense knowledge of social structures). Formal analysis,

in denying this sort of reflexivity, denies its own possibility. It must give up its conception of itself.

Third, formal analysis does not really explain historic actions. Though it purports to do so, it only explains social facts. For example, though it purports to explain a jury's ways of deciding a case on a particular occasion, it only explains the magnitude and direction of judge-jury disagreement. It explains a social fact by regarding numerous historic actions as tokens of an action-type. The jury in general, not any particular jury, is said to be liable in deciding a case to be swayed by sentiments about the law. The conditions for the truth of this claim are separated off in formal analysis from conditions for the truth of any claim about historic actions. It is true if it explains the fact that judge and jury disagreed in 24.6 per cent of all 3576 cases in the direction of jury leniency.

A study of analytic practices might be thought to go even further than these three claims, by showing that the analyst is ironically unable to explain "formal analysis" by formal-analytic means. Again, Brandom's pragmatism helps us to see what is wrong with formal analysis at this fundamental level. The same points apply. First, Brandom must be able to see in these (his own) historic actions what is relevant for an explanation of content. Second, he must also be able to consider them as phenomena in their own right. Third, he cannot claim without absurdity to be doing the practice of giving and asking for reasons.⁹ If we now bring into play his own condition of adequacy for an explanation of content, we have an almost syllogistic objection to make to the whole project of *Making It Explicit*. He claims to have shown that semantic theory can be "firmly rooted in actual [linguistic] practices" (p. xiii). But what are the practices of Brandom's theory of content? So far from being people's actual activities, they are elements of a model constructed for purposes of philosophical semantics. This is an example of the global objection to formal analysis. Generally speaking, formal analysis floats free of people's actual activities. Ironically, it even floats free of the analyst's own activities!

However, ethnomethodology must not make an absolute exception of formal analysis. It must be able to say without contradiction that formal analysis itself is always just another Lebenswelt pair. It is the language-games of formal-analytic studies. It is lived work. This is

⁹ See Chapter 2, Section 4, for the argument.

the point at which to make the qualifying distinction. At the level of first-order ethnomethodological studies, formal analysis is a fundamentally ill-conceived approach to the study of social life. At the level of second-order ethnomethodological studies, it is just another example of the local co-production and endogenous accountability of phenomena of order.

By way of illustration, consider the subject matter of Greiffenhagen, Mair and Sharrock's "From Methodology to Methodography: A Study of Qualitative and Quantitative Reasoning in Practice" (2011). The pretext of the study is a curious lack of interest on the part of students of scientific knowledge in the practices of social science:

[T]here are very few studies that have investigated the social sciences with the same scrutiny as the investigations of the experimental sciences. This could be down to a fear of (self-)scrutiny: Goffman [...] has observed a touching tendency to keep a part of the world safe from sociology" (in particular, sociology itself), while Coser [...] has remarked that "sociologists still find it easier to study status superiors than to study themselves". [...] Whatever the reason for the paucity of studies, undertaking research of this kind presented us with a unique opportunity: the chance to study a rather elusive figure, the social scientist, in their ordinary working environment over an extended period of time. (pp. 94-5)

A distinction may be made between methodological and methodographic reflection. The analyst reflects on problems of sociological method for practical purposes, as problems to be solved. By contrast, Garfinkel's followers reflect on these same problems as constitutive features of the analyst's own practices. In this case, they reflect on differences between qualitative and quantitative research as matters of practical concern for people doing professional sociological studies (cf. Coulter, 2001). Their study looks at "the working practices of two groups of social scientists, one with a predominantly qualitative approach, the other involved in statistical modelling" (p. 93). The working practices of the second group are obvious targets for ethnomethodological criticism. But "methodography" is a radical alternative even to qualitative research. Greiffenhagen, Mair and Sharrock are careful to point this out:

We must not be misread. There is no intention to match the practices of either group against anyone's ideals of method so as to attribute methodological failings (or successes) to them. Acknowledging that these were specific moments in ongoing projects, what we have attempted to bring out is that the described practices are constitutive aspects of producing sound research for practical so-

ciological purposes. In other words, social scientists' capacity to hold to methodological prescriptions relies upon them being able to display heterogeneous, but largely unspecified, practical competencies. As a result, locally adequate ways of reasoning problems through occupy an important place in ordinary social scientific work. (p. 104)

Consider this example. In the first group, members were “producing sound research for practical sociological purposes” by interviewing people who had previously contributed to a book of personal histories. In conversations recorded and transcribed for alternately methodographic purposes, these researchers – the experimental subjects of a methodographic study – look for evidence of “associations in place” (p. 96) in interview material. At one point, they “unpack a detail” of one of their interviews to show that the interviewees are “really connected” to their neighbours on a housing estate (p. 98). That they are “really connected” was not a phenomenon co-produced by the interviewees and their neighbours, there where “associations in place” were meant to have taken place. It is another artefact of formal analysis. This (the researchers' qualitative research) is once again the discovery, demonstration and explanation of a fact about the society. At the level of first-order ethnomethodological study, indifference to problems of qualitative research is critical. It identifies an account of associations in place as analysis of a constructed model of people's actual activities. However, at the level of this second-order ethnomethodological study (methodography), there is simple indifference to practices of professional sociological reasoning, done by members for practical purposes. Here as elsewhere, members reflect on their own practices in case of something (criticism) and for the sake of something (sound research). There can be nothing to criticise. There is only something to describe.

In fact, Garfinkel's followers do criticise formal analysis. They find fault in representative studies and call for reforms in accordance with Garfinkel's radical initiatives. This is not to say that it makes sense to do so. We are reminded of Hegel's unhappy consciousness. “There *is* order in the plenum.” This is Garfinkel's determinate negation of formal-analytic negations. It is radical as a comment on what remains indeterminate in any account of people's actual activities – not just in an analyst's account of judge-jury disagreement, self-observing systems or associations in place. It is radical as a comment on what is already indeterminate in a member's own account of some very general social phenomenon, e.g. on what is indeterminate in a pedestrian's account of a way of walking down a busy street

(Ryave & Shenkein, 1974). Awkwardly, the ethnomethodologist may want to say two things of formal analysis: that it is fundamentally ill-conceived, but still just another possible subject matter for ethnomethodological studies. Critical indifference is a phase in oscillations between these two positions.

I do not mean to suggest that Greiffenhagen, Mair and Sharrock are using a qualifying distinction to criticise even in refraining from criticism. Indeed, it is part of the problem that critical indifference is undetectably implicit. What Garfinkel and Sacks (1970) actually say is that ethnomethodology is indifference to questions of practical adequacy raised by members themselves. Formal analysis “is in no way singled out as a phenomenon for our research attention” (p. 346). Could they not perhaps mean that it *is* singled out as a fundamentally ill-conceived approach to the study of social life? The point of reading them in this way is to recruit interest in ethnomethodology. But we cannot have it both ways. If ethnomethodology is indeed a radical alternative to mainstream sociology, there can be no criticism to make. What is radical in Garfinkel’s initiatives is the thought of a purely descriptive enterprise: the study of order production simply as such.

5. Simple Indifference

In the remainder of this chapter, I want to present another way of interpreting ethnomethodological interest in formal analysis. The case for it is virtually automatic. As well as being simpler than the qualifying distinction, it is consistent with Garfinkel’s own programmatic statements. It has only one defect. On this account, there is no reason to do ethnomethodology apart from interest in these (whatever) particular ways of doing things.

It may seem fair to say that a summary, for example, of Popper’s philosophy of science is either true or false of what scientists do. But recall the idea of ways of reasoning specific to a domain of problem-solving activity. What does it imply in this particular case? Compare these two interpretations of the idea:

- (1) Practices of “scientific” reasoning are a motley. There are the physicist’s practices, the biologist’s practices, the sociologist’s practices, the psychiatrist’s practises, and so on.

- (2) Truth conditions for an account of practice may vary with ways in which, and circumstances under which, the account is spoken.

These claims are subtly different. (1) says that there is something unique to the physicist's (or whoever's) practices, some set of uniquely identifying characteristics. (2) makes no mention of a domain of problem-solving activity. It formulates a problem that applies indifferently to the philosopher's account of the physicist's practices and the physicist's own account. This problem comes earlier than the difficulty of critical indifference and helps to explain it. Call it the haecceity¹⁰ problem.

Which, if either, of these two claims, (1) or (2), do Garfinkel, Lynch and Livingston make in "The Work of a Discovering Science Construed with Materials from the Optically Discovered Pulsar" (1981)? They write:

On the evening of the discovery of the optical pulsar at Steward Observatory, January 16, 1969, by John Cocke, Michael Disney, Don Taylor and Robert McCallister, a tape recording in which they reported their series of observations was left running and before it ran out recorded the evening's 'conversations' from Observations 18 through 23. This unique document, on the file at the Centre for History and Philosophy of Physics at the American Institute of Physics, was made available for our examination. The tape was transcribed by us using the conventions of conversation analysis. Our question was: 'What does the optically discovered pulsar consist of as Cocke and Disney's night's work?' The tape and transcript permit us to treat some relevancies that are not otherwise available in science studies: (1) That the discovery as their night's work had the property of 'first time through'. (2) The local historicity of the night's collection of observations. (3) The quiddity [better: haecceity] of their night's work. (pp. 131-2)

A discussion of these points, just 12 pages long, is followed by 16 pages of appended material: Cocke, Disney and Taylor's report of the discovery in a peer reviewed journal, some facsimiles of notebook log entries, transcripts of the recording of two observations, and some examples of "projections" of observations yet to be made.

Hak (1995) takes a critical view of Garfinkel, Lynch and Livingston's study. He reads them as claiming that "[t]he literature on work in institutional settings does not provide

¹⁰ A dictionary definition (Speake & Mitchell, 1979): "A term originally employed by Duns Scotus to denote the formal property of an object or person in virtue of which it is uniquely individuated [...] as just *this* object or person."

descriptions of what work practice in these settings consist of and how practitioners manage tasks they are confronted with” (p. 110). He cites Zimmerman and Pollner (1970):

Any feature of a setting – its perceived regularity, purposiveness, typicality – is [to be] conceived as the accomplishment of the work done in and on the occasion of that feature’s recognition. The practices through which a feature is displayed and detected, however, are assumed to display invariant properties across settings whose substantive features they make observable. It is to the discovery of these practices and their invariant properties that inquiry is to be addressed. (p. 95; Hak, p. 114)

Working with this passage, Hak makes a distinction between the “contingencies and historicity” of scientific work and the specificity of its “competent reporting,” to be understood in terms of the idea of “invariant properties” of “these [the discovering astronomer’s] practices”:

[O]n one hand, Garfinkel, Lynch and Livingston demonstrate clearly ethnomethodology’s argument that work can only be studied adequately by acknowledging its contingencies and historicity. But, on the other hand, they disregard a very important feature of the work, namely, that the discovery of an optical pulsar (as viewed by the practitioners) does not only consist of the work of observing its features (pulse frequencies, etc.) but of its competent reporting as well. The result is at best a partial description of how the work in the observatory is done, in which its observable orientation to the wider context of the discipline of astronomy, a specific interweaving of doing observations and reporting about them, is downplayed. (pp. 118–9)

In short, Hak reads Garfinkel, Lynch and Livingston as committed to (1). Practices of “scientific” reasoning are a motley. This can be learnt from ethnomethodological studies of the work practices of a science. As we might have expected, there are indefinitely many ways in which a science is done. Still, the aim of inquiry is to be able to see in the contingencies and historicity of this or that example what a competent practitioner sees: the invariant properties of this (whatever) particular science as such. Hak is disappointed. He looks in vain for an account specifically of the astronomer’s work practices.

To make my own position clear, I am suggesting here that Hak’s criticism involves a common misunderstanding of the haecceity problem. Take any passable definition D of the word “discovery” as it is now being used. Does it make sense to separate D from historic uses of language and ask for conditions for the truth of D? Not according to an occasionalist reading of Wittgenstein. If D is true considered on its own, or false considered on its

own, it is already true or false of this (Cocke, Disney and Taylor's) and any other discovery. The haecceity problem follows from the rejection of this view. The problem is not to specify practices of such-and-such a domain of reasoning, but to examine various ways in which D was intelligibly spoken. The object of interest, then, is not a system or collection of invariant properties of the work of a discovering science (formal analysis), but semantic variety in uses of D. This is the point of studying of practices of "scientific" reasoning: not to define science, but to understand what a person did in speaking of a science.

The haecceity problem comes before the distinction between an analyst's account of a member's practices and a member's own account. Accordingly, it can be read as a sort of rule of method for any ethnomethodological study. (So much for the qualifying distinction.) There are two parts to the problem. Linguistic material has been made available for purposes of an analysis of practical actions. It has also been used. Ethnomethodology is the study of semantic variety in how it has been used.

Simple indifference is surely the correct way to understand the parable of "Sacks' gloss" and the discovery of, or chancing upon, a perspicuous setting. Garfinkel (2002) recalls Sacks announcing a distinction. "Possessables" are things we see and want and may go ahead and take. They have no owner. "Possessitives" are things we see and want but may *not* go ahead and take. They may only appear to have no owner. This was linguistic material for purposes of an analysis of practical actions. It was not made by members, but by an analyst (Sacks) of practical actions. Sacks is reported to have said:

"Now, Harold, what do I mean by that distinction? That is what I want to *find out*. I don't want you to tell me. I don't want to settle it like that. I *could* go to the ULCA law library; I know how to use the library. I could find discussions that would bear on what I might as well mean, but that's not the way I want to learn what I mean." He trusted himself to write definitions, "but I don't want to write definitions; and I don't want to consult authorities. Instead, I want to *find* a work group, somewhere, perhaps in Los Angeles, who, *as their day's work*, and because they know it as their day's work, will be able to teach me what I could be talking about as *they* know it as the day's work." (p. 182; original emphasis)

Some time later, Sacks came to Garfinkel with an account of a distinction made by members and used by members themselves as gloss for practical actions:

In the Los Angeles Police Department are police who, in riding around their territories, as part of their work, spot cars that have been abandoned. Other cars look equally bad, but it could be found

that they were not abandoned. They are “possessitives.” You call the tow truck for one of these cars; the other you ticket. As their day’s work the police must make this distinction; make it fast; make it subject to supervisory review for the truth, correctness, and other adequacies of recognition; make it in each particular case; do so within the bureaucratically organized Los Angeles Police Department; and having among its consequences that various parties, who as members of populations become involved, become forensically interested parties to issues of truth and correctness. (*ibid.*)

Garfinkel’s point could hardly be that Sacks’ gloss (possessables/possessitives) was devoid of meaning. The moral of the story is that uses of a distinction potentially descriptive of things people do may admit of semantic variety. What does it mean to say (perhaps by way of a warning) that a vehicle may only appear to have been abandoned? This question calls for competence in the handling of a problem. It is a question for domain-specific ways of reasoning. It would be regressive, however, to explain this point in terms of some supposed invariant property of a work practice. Wittgenstein’s remarks on rules and rule-following apply. No account of “what people do” (of what “we” do or of what “they” do) is immune from misinterpretation. There is none such that uses of it are semantically invariant.

Note, then, that what varies (meaning) does not necessarily vary, where it does, from one domain of problem-solving activity to another. It varies from one occasion for speaking to another, even within some particular domain. This is part of what it means to say that members are competent in the handling of a problem (possessable or possessitive?). Note, too, that this is not merely a constructed problem of formal analysis. It arises for people doing these (whatever) particular things. For example, it arose for jurors who were concerned to put the law before considerations of fairness, and for sociologists concerned to have a representative sample. According to Campbell (1981), Garfinkel and his followers “make rather too much of the very ancient philosophical problem of how we are able to make general statements about particular situations” (p. 222). But this is to confuse an approach to the study social life with a philosophical insight. It is not a case of learning from experience that interpretations of an account of a practice must be occasion-sensitive. That is something we learn from Wittgenstein (and not only from Wittgenstein). Going forward, members are therefore to be seen as encountering and handling the haecceity problem in an endless variety of ways.

The haecceity problem does not imply a criticism of formal analysis. It could be said, perhaps, that people doing formal analysis deny semantic variety. But even its denial, con-

sidered now as the following of a rule, serves as an example of this fundamental phenomenon. Accordingly, we can read the following passage from *Making It Explicit* – part of Brandom’s denial of Wittgenstein’s insight – in either of two ways:

Wittgenstein reminds us that even [the] practice of pointing requires a great deal of social stage-setting – the untrained may be unable to transfer their attention beyond the tip of the pointing finger, or may perversely trace the line of indication in the wrong direction, from fingertip to base, and to take it that something behind the one pointing has been singled out. Again, he reminds us of the emptiness of ‘bare’ demonstration. The use of ‘this’ or ‘that’ must at least implicitly be connected with some sortal, for the same physical gesture can have the significance of pointing to a book or to its cover, its title, its shape, and so on. (p. 461)

Brandom is in the process here of explaining the possibility of deixis in terms of the practice of giving and asking for reason. He has no choice but to reject the “indexical paradigm” of conventional explanations, since it relies on concepts of representation. His alternative approach is to assimilate deictic uses of pronouns, demonstratives and other such expressions to anaphoric uses, by means of the idea of substitution-inferences. He considers the following example (p. 492): “*That pig* is grunting, so *it* must be happy. I’m glad, because *it* is our champion boar, Wilbur.” Substituting *that pig* for *it*, I infer that *that pig* must be hungry. Substituting *that pig* again for *it*, I infer that *that pig* is our champion boar, Wilbur. At the end of this (constructed) anaphoric chain, I infer that it is Wilbur that is grunting. None of this requires the idea of a pointing to something (Wilbur) by means of words.

One way of reading Brandom on indexicality is to raise questions of practical adequacy. For example, Emirbayer and Maynard (2010) express dissatisfaction with contemporary pragmatism and cite Brandom on indexicality as an example. According to them, ethnomethodology goes further than contemporary pragmatism in developing three guiding ideas of classical pragmatism: “its call for a return to experience or recovery of concrete practices; its idea that obstacles in experience give rise to efforts at creative problem solving; and its understanding of language in use, including conversation interaction, as an order of empirical practices in and through which problem-solving efforts are undertaken and social order ongoingly and collaboratively accomplished” (p. 221). These three ideas remain “empirically underdeveloped” or “insufficiently explored” in the work of contemporary pragmatists. Brandom commits a familiar error:

All pragmatism proceeds from the notion that the Western tradition, which includes not only philosophy but also the philosophic assumptions underpinning modern science (both natural and social), erroneously directs us away from lived experience, from concrete practices, towards theoretical abstractions. (p. 255)

He erroneously directs us way from concrete practices towards the theoretical abstractions of methodological pragmatism. He *ought* to be doing empirical studies underpinned by the three guiding ideas of classical pragmatism:

Brandom's discussions of anaphora, deixis, and other aspects of indexical utterances employ constructed examples, suppositions, and literary extracts to *illustrate* his arguments, returning us to the exact point at which the early pragmatists left social science in the first place: without an empirical program having theoretical underpinnings with pragmatic philosophy. (p. 255; original emphasis)

He *ought* to be doing ethnomethodology, with a view to finishing the "unfinished business" (p. 252) of classical pragmatism.

Another way of reading Brandom on indexicality is to consider a rule of philosophical practice – "No representational concepts!" – as analytic gloss for indefinitely many ways of doing philosophy. This will seem derivate on first-order studies to anyone who reads "indexicality" as a proxy for a description of people doing other things. Apparently, Emirbayer and Maynard have other activities in mind, not those in which they find themselves involved as Brandom's critics: medical consultations, police interrogations, cross-examinations, etc. Anything, in fact, but these practices of analytic philosophy of language. It is easy to appreciate their dissatisfaction with Brandom's work. Already doing empirical studies, they hear that Brandom has a model of norms implicit in speakers' actual activities. They find, in fact, that there is nothing in *Making It Explicit* about medical consultations or any other domain of activity. On the other hand, we would not go looking in the literature on diagnostic reasoning for an account of the philosopher's practices. It is a curious fact that we tend to look elsewhere for examples of language-games and lived work, as though our own practices were hardly worthy of analysis. More simply, we can read Brandom on indexicality as a philosopher discussing and demonstrating a rule of philosophical method.

The interpretative question is whether a study of semantic analysis as members' work must be ironic and implicitly critical. To my mind, there is no real difficulty here. The difficulty occurs only where we do indulge in criticism (the inconsistent triad). Semantic

analysis is members' work wherever a problem of semantic analysis catches hold of us. If we see the problem, we see an example (this or that example) of members' work. This is the same as seeing the "closing problem" (Schegloff & Sacks, 1974). We see it in the turns of a transcription of naturally occurring conversation, even perhaps in our own contributions to a conversation still incomplete. Indifference is the technique of refraining here from problem-solving activity. By means of this technique, we take a radically altered interest in *what* is being done, which we now begin to see as various *ways* of doing it.

We can avoid the contradiction of critical indifference by regarding even a member's account of "what people do" as analytic gloss for indefinitely many ways of doing it. We would not object to semantic variety in (for example) a juror's use of the official jury line, or in a philosopher's use of a rule of philosophical method. Why, then, would we object to semantic variety in Durkheimian sociology, where the sociologist has failed to make a rule of method propositionally explicit? The mistake is to read an account of a practice always as a description, as though nothing could be done with a declarative sentence but to describe, and then to say: "But that is *not* what members actually do!" Under ethnomethodological scrutiny, it is at least dubious to regard Popper's philosophy of science as a description of scientific practice at all. As formal analysis, it is first of all an account of problems that arise for people doing philosophy of science. This is not to criticise it. We could say the same of the biologist's account of biological phenomena, of the musicologist's account of musicological phenomena, of the psychiatrist's account of psychiatric phenomena, and so on. As Garfinkel (1969) writes:

In doing sociology, lay and professional, every reference to the "real world," even where the reference is to physical or biological events, is a reference to the organized activities of everyday life. Thereby, in contrast to certain versions of Durkheim that teach that the objective reality of social facts is sociology's fundamental principle, the lesson is taken instead, and used as a study policy, that the objective reality of social facts *as* an ongoing accomplishment of the concerted activities of everyday life [...] is practical sociology's fundamental phenomenon [and] the prevailing topic for ethnomethodological study. (p. vii)

Prototypically, people talking are also and *eo ipso* making their own practical actions analysable. This "also and *eo ipso*" is radically indifferent to the institutional politics of ethnomethodology. It makes no exceptions for people doing Durkheimian sociology or analytic philosophy of language. On the contrary, it expresses stoicism in the face of a familiar

hardship and embarrassment of commitment to Garfinkel's radical initiatives: the difficulty of conveying the point. It does not permit the ethnomethodologist to indulge in criticism, tit for tat, of colleagues down the hall. It must read as expressing simple indifference, i.e. indifference to members' practical concerns without critical reserve.

6. Summary

I have presented "critical indifference" as a hopping from foot to foot. In an effort to defend, clarify, advertise or applaud ethnomethodological studies, or in some other way to secure a position for ethnomethodological research and win respect for this (the ethnomethodologist's own) special expertise, or to teach ethnomethodology, or whatever else, the ethnomethodologist indulges in criticism of formal analysis. We must think of this as a kind of intermission of studies: for whatever reason, the ethnomethodologist stops doing ethnomethodology to do something else. Thus, Emirbayer and Maynard (2010) take a break from ethnomethodology to construct an *ad hominem* argument against contemporary pragmatism, and by way of illustration against Brandom on indexicality. This argument *against* Brandom is also an argument *for* ethnomethodology. But since "simple indifference" is constitutive of ethnomethodological research, we must think of such criticism as an intermission of studies. For it can be no part of ethnomethodological research to judge the adequacy of anyone's practices.

How, then, should we understand the "procedural policy" of ethnomethodological indifference? My answer involves the idea of ways of reasoning specific to a domain of problem-solving activity. I have rejected the view (Hak, 1995) that the ethnomethodologist owes an account of *what* is specific to a domain. The departure point of any study is an account of something people do. The task is not to produce or assess such an account (formal analysis), but to consider semantic variety in uses of it. It is a matter of indifference whether it is spoken by sociologists explaining judge-jury disagreement or by jurors deciding a case. After all, there is nothing to prevent a sociologist's account from falling into the hands of the subjects of a sociological study. (Who could be indifferent to the "liberation thesis" as a juror in the process deciding a case?) The task is to show how an account of something people do, whatever the provenance of the account, has been used by parties to

a problem-solving activity. For example, it is to show how Garfinkel's followers occasionally use criticism of formal analysis. To do what with it? To say what?

This interpretation of Garfinkel's initiatives has two obvious merits. It is simpler than any involving a qualifying distinction. It also consistent with Garfinkel's programmatic statements. It has only one defect, but a defect serious enough to count against what I have said. On this view, there can be no motive for doing ethnomethodology apart from interest in these (whatever) practical actions, considered *not* as manifestations of some very general social phenomenon (schizogenesis, frame ambiguity, self-observing systems, etc.), but as the actions they are. The point is just to understand something said or done on some particular occasion. I argue in my conclusion that understanding is surely motive enough for ethnomethodological research. It may not seem so to some. It is easy to see why "people doing philosophy" have taken an interest in Brandom's work. But what could be of interest in Brandom's pragmatism to the ethnomethodologist? Conversely, what could be of interest in ethnomethodology to Brandom's readers? In response to lack of interest on the philosopher's part, the ethnomethodologist may be tempted to indulge, but must refrain from indulging, in global criticism of formal analysis, taking Brandom's pragmatism as an example.

CHAPTER FOUR

Philosophy as Members' Work

“Actions are analysable.” Read as a practical ideal of professional sociological inquiry, this sentence means that it should always be possible to analyse actions as manifestations of a social phenomenon. Sociologists strive to make actions analysable in various ways, but in ways that rarely belong to the social setting of an action. Garfinkel’s (1967, pp. vii–iii) basic initiative is to study methods members use to make their own actions analysable. In most cases, the specific subject matter of an ethnomethodological study can be introduced in terms of differences between sociologists’ methods (representative sampling, surveys, interviews, etc.) and members’ own methods, in particular the fact that members’ methods are “identical with” (p. 1) their activities. However, it follows from Garfinkel’s programmatic statements that sociologists are also members. This is not simply to say that sociologists rely on a member’s (their own) pre-theoretical knowledge of the social world. The more radical claim is that sociology itself is members’ work. In the broadest terms, people doing formal analysis are also and *eo ipso* members. Accordingly, it would be incorrect to read Garfinkel as making an absolute distinction between sociologists’ methods and members own methods. There *is* a distinction to make, but it is relative to subject matter, and may not always be expressible in such terms: a distinction between (a) practically-oriented assimilation of activities to some particular model and (b) reflection on the possibility of such assimilation. I am going to argue that (b) must be purely descriptive, and that (b) is the most generally correct characterisation of ethnomethodology.

It is a good test of any interpretation of Garfinkel’s basic initiative to remove the support of a critique of formal analysis, by asking what it could mean to say of formal analysis itself that it is members’ work. After all, Garfinkel is clear that the aim of ethnomethodology is not to judge the adequacy of members’ methods, but to examine activities as phenomena in their own right. Like people doing other things, sociologists do what makes the doing of it (sociology) analysable as such. How? A reading that collapses at this point collapses entirely. And a reading that depends on a critique of formal analysis collapses at this

point. There is something to criticise in the vicinity, say, of juror's ways of making decisions: methods used by sociologists to make historic decisions analysable. But there is nothing to criticise in the vicinity of formal analysis except formal analysis itself. If ethnomethodology is not to be found unintelligible in the end, it must be possible to examine formal analysis as members' work, i.e. as something competently and teachably done by members.

My aim in this chapter, then, is to present a particular species of formal analysis as members' work. People doing analytic philosophy of language are also and *eo ipso* members. This follows from Garfinkel's programmatic statements. But it remains for us to see what this specific subject matter might be: analytic philosophy of language as something competently and teachably done by members.

In Section 1, I make a case for a purely descriptivist interpretation of Garfinkel's basic initiative. I also suggest that a Winchian interpretation (Hutchinson, Read & Sharrock, 2008) might serve as foil. In Section 2, I illustrate Winch's critical distinction between internal and external identity criteria with an example close to home. A beginner in philosophy may be prone to use criteria external to philosophical inquiry to identify and describe the teacher's actions. In Section 3, I argue by means of another example that Winch's distinction is relative to subject matter. In this case, Brandom's account of default entitlement is external to the setting of a class on jazz improvisation, but is internal to the setting of philosophical inquiry. In Section 4, I express dissatisfaction with the view that ethnomethodology is an eliminative critique of formal-analytic sociology. In Section 5, I present an example of the work of teaching philosophy, designed to illustrate my descriptivist interpretation: people doing philosophy subordinate interest in their own practical actions to interest in some particular problem; the ethnomethodologist subordinates interest in the problem to interest in those same practical actions. We are left with a radically occasionalist conception of ethnomethodology, according to which there can be nothing on offer like a special logic of members' work.

1. Ethnomethodological Descriptivism

Practical actions are ways of doing things: these (whatever) particular ways of bringing a conversation to a close, walking down a busy street, making a scientific discovery, and so

on. Garfinkel does not deny that practical actions have formal properties, i.e. that “they exhibit upon analysis properties of uniformity, reproducibility, repetitiveness, standardization, typicality, and so on” (Garfinkel & Sacks, 1970, p. 346). But he takes the view that they cannot be abstracted from practical actions and considered on their own. This could mean either of two things: (1) that there are no invariant formal properties of practical actions, since they are features of particular social settings and vary across social settings; or (2) that accounts of formal properties of practical actions gloss over significant differences between these (whatever) particular ways of doing things.¹¹ Either way, Garfinkel (1967) holds that formal properties of practical actions “obtain their guarantee” from “ongoing accomplishments” of some particular social setting, i.e. from things competently and teachably done by members. He writes:

[Ethnomethodology] is directed to the tasks of learning how members’ actual, ordinary activities consist of methods to make practical actions [...] analyzeable; and of discovering the formal properties of [practical actions] “from within” actual settings, as ongoing accomplishments of those settings. The[se (whatever)] formal properties obtain their guarantee from no other source, and in no other way. Because this is so, our study tasks cannot be accomplished by free invention, constructive analytic theorizing, mock-ups, or book reviews, and so no special interest is paid to them aside from an interest in their varieties as organizationally situated methods of practical reasoning. (p. viii)

According to Garfinkel, it is only as members that we can hope to discover formal properties of practical actions, in the course of learning to do these (whatever) particular things: only as jurors that we hope to discover formal properties of jurors’ ways of deciding cases, only as coroners that we can hope to discover formal properties of coroners’ ways of determining cause of death, and so on. To repeat, this could mean either of two things: (1) that formal properties of jurors’ practical actions differ from those of coroners’ practical actions, which differ in turn from those of sociologists’ or philosophers’ practical actions, and so on; or (2) that accounts of formal properties of practical actions, *no matter who is doing what or who produces (or uses) accounts of what*, will inevitably gloss over significant difference between particular ways of doing things.

¹¹ Compare the idea of semantic variety: an abstract analysis of the semantics of an expression E glosses over significant differences between possible uses of E, i.e. differences that bear on an account of conditions for the truth of E on a particular occasions for speaking.

Garfinkel and Brandom may seem to be opposed on this point. Note that “uses of language” are practical actions of a special kind: these (whatever) particular ways of doing things with words. For Garfinkel, their formal properties “obtain their guarantee” from things competently and teachably done by members. For Brandom, they “obtain their guarantee” from a condition of adequacy for a theory of content.¹² What is the difference? Consider the fact that co-conversationalists make practically significant distinctions between utterance-types. As analysts of various examples of REQUESTS and DEMANDS, we might be persuaded that there are conditions of felicity common to REQUESTS and DEMANDS, and conditions of felicity that distinguish one class of speech acts from the other. An example: ‘This (whatever) particular way of using language is a REQUEST or DEMAND only if it involves at least two speakers, one of whom wants the other to do X. It is a DEMAND, but not a REQUEST, if the other is put under some kind of severe pressure to do X.’ Ethnomethodologists are not content to analyse utterance-types in the abstract. They ask how co-conversationalists actually produce the order of various utterances-types, occasionally even “formulating” (Garfinkel & Sacks, 1970) their own utterances, as when A asks B, ‘Are you asking me (to do something) or telling me (to do it)?’ By contrast, Brandom asks what *we* (language-users) must at least be capable of doing for our expressions to have meanings of different kinds. This is to blend transcendental inquiry with analysis of actual practices. That our expressions do have meanings of various kinds is given. According to Brandom, our capacity to do what is, or what instantiates, a particular practice – the practice of giving and asking for reasons – is a necessary condition for the possibility of these (whatever) particular meanings, to be explained now in social-practical terms. There is indeed a difference here between formal analysis and ethnomethodology. But is Garfinkel opposed to this or any other species of formal analysis?

The ethnomethodologist and the Wittgensteinian are in broad agreement. We should not expect to be able to reduce this great diversity of language-games to a single model of meaning, or be able to reduce ways of doing the most ordinary things to a theory of some particular social phenomenon. Accordingly, the ethnomethodologist will be inclined to criticise Brandom’s pragmatism on Wittgensteinian grounds. For Brandom commits the

¹² It must be possible to describe a practice P that would suffice to confer meanings explained by a theory of content on expressions caught up in P in suitable ways.

cardinal sin when he assumes, contrary to *Philosophical Investigations* §64, that there must be something common to language-games, something that makes us use the same word for all. But Garfinkel differs from Wittgenstein in this respect. He does not extrapolate criticism of formal-analytic sociology from the idea of an ethnomethodological study. Instead, he reminds his readers that even formal-analytic sociology is members' work. See, for example, how he continues here:

The[se (whatever)] formal properties obtain their guarantee from no other source, and in no other way. Because this is so, our study tasks cannot be accomplished by free invention, constructive analytic theorizing, mock-ups, or book reviews, and so no special interest is paid to them aside from an interest in their varieties as organizationally situated methods of practical reasoning. *Similarly, there can be nothing to quarrel with or to correct about practical sociological reasoning, and so, because professional sociological inquiries are practical through and through, except that quarrels between those doing professional inquiries and ethnomethodology may be of interest as phenomena for ethnomethodological studies, these quarrels need not be taken seriously.* (p. viii; emphasis added)

The thought that jurors are doing “practical sociological reasoning” is fairly clear. Their decision-making is, of course, an example of reasoning. It is practical in the straightforward sense that a decision has to be made. It is sociological in the sense (and here the ethnomethodologist may have Wittgenstein in mind) that an account of a decision will depend for its intelligibility on pre-theoretical knowledge of a social setting. This, perhaps, is what the sociologist denies, that a finding of social science depends for its intelligibility on such knowledge. But Garfinkel's point is more radical. He makes a comparison between the specific subject matter of a sociological study and the sociological study itself. Social science is just another example of practical sociological reasoning. There can be nothing to quarrel with even here, not unless the aim of ethnomethodology is after all to criticise and reform members' methods.

The Wittgensteinian sees philosophy as a purely descriptive activity. “Philosophy may in no way interfere with the actual use of language,” says Wittgenstein in *Philosophical Investigations*: it “can in the end only describe it” and properly “leaves everything as it is” (§124). Does the ethnomethodologist see Garfinkel's basic initiative in the same way? The difference here is that the Wittgensteinian can be unreservedly critical of an alternative conception of philosophy, and propose to avoid the errors of philosophy as it is usually and incorrectly done. The ethnomethodologist cannot afford to be so straightforward, since the

alternative conception of sociology, that of the principle that the objective reality of social facts, is also itself a possible ethnomethodological subject matter. The Wittgensteinian makes a distinction between (a) our ordinary ways of explaining meanings and (b) pathogenically philosophical ways. The ethnomethodologist may want to make an equivalent distinction between (a') members' ways of explaining their own actions and (b') sociologists' ways of discovering, demonstrating and explaining social facts, but must not forget that (a') includes such things as rules of sociological method. It is true that formal analysis obscures members' work, since it reduces an immense diversity of ways of doing things to a single model. But what is the relationship between formal analysis and ethnomethodology? Is it primarily the relationship between alternative conceptions of sociology? Or is formal analysis in the first instance just another example of members' work? Though these positions are not mutually exclusive, Garfinkel's programmatic statements imply the latter. He proposes to regard even quarrels between ethnomethodologists and their colleagues as "organizationally situated methods of practical reasoning," to be examined with indifference even by those passionately engaged in them. Garfinkel's basic initiative is inclusively descriptivist. To make sense of it, we must be able to give up the support of a critique of formal-analytic sociology.

Wittgenstein's descriptivism has critical implications for the philosophical mainstream (Baz, 2012). It is not clear that Garfinkel's descriptivism can have critical implications for Durkheimian sociology, which it regards both as its opposite and as a particular domain of problem-solving activity. Can the Wittgensteinian regard analytic philosophy of language as just another collection of language-games? If so, the motive of such descriptivism may still be to criticise at least some analytic practices. Consider Wittgenstein's famous remark in *Philosophical Investigation* about the philosopher's uses of language:

Naming appears as a *queer* connexion of a word with an object. – And you really get such a queer connexion when the philosopher tries to bring out *the* relationship between name and thing by staring at an object in front of him and repeating a name or even the word "this" innumerable times. For philosophical problems arise when language *goes on holiday*. And *here* we may indeed fancy naming to be some remarkable act of mind, as it were a baptism of an object. And we can also say the word "this" *to* the object, as it were *address* the object as "this" – a queer use of this word, which doubtless occurs in doing philosophy. (§38; original emphasis)

The Wittgensteinian will want to show that language occasionally “goes on holiday” in philosophy, i.e. that there is sometimes only the fantasy of a practice in the philosopher’s mind. There is a criticism to make. The target of criticism is not, for example, the imagined naming of an object, but a way of thinking that leads the philosopher to search for something – “*the* relationship between name and thing” – in various uses of a word. So far from ignoring problems of formal analysis, the Wittgensteinian regards them as symptoms of a deeper confusion. “The [Wittgensteinian’s] treatment of a question is like the treatment of an illness” (§255). By contrast, the rule of ethnomethodological indifference is either (1) to ignore problems of formal analysis, since they play no part in people’s actual activities, or (2) to consider the analyst’s own practices as phenomena in their own right.

Ethnomethodology must be clear in what it says about its double relationship to formal analysis. In the next two sections, I am going to press this issue by considering a distinction made by Winch in *The Idea of a Social Science and its Relation to Philosophy* (1990). Winch argues that criteria for the identity (meaning) of an action are “necessarily relative to a rule” (p. 78). The rule may be internal to the action up for analysis, as in the case of the rule ‘Add 2’ (a rule of arithmetic) and examples of the action of adding 2 (arithmetical actions). It may also be external to it. This distinction might be thought to imply that there are no invariant formal properties of practical actions. To quote Bogen (1999):

Once language-games (and, by extension, “forms of life”) are conceived as internally ordered, autonomous spheres of linguistic and cultural practice, the problem rather naturally arises of how it is we come to know and understand the operations of one kind of language-game from within the assumptions, practices, and rules of conduct of another. It is this conception of language-games, together with a willingness to extend this model to all other areas of social life, that lies at the heart of Winch’s famous “incommensurability thesis”: that different cultures are based in fundamentally different orders of belief and social convention, and thus, operate with standards (of reason, cogency, order, and the like) that are, strictly speaking, incomparable. (pp. 10–11)

On this reading, Winch takes the view that what is reasonable (cogent, orderly) or not in an action can be judged only from within a particular form of life (cf. Okrent, 1984; Taylor, 1982; Sharrock & Anderson, 1985). If this is so, the sociologist should refrain from explanation, since explanation applies the rules of one language-game (sociology) to the moves of another. Sociological explanation can be compared to the foisting of a picture of the

essence of human language on diverse language-games. “A *picture* held us captive,” says Wittgenstein (§115). In this case, what held us captive was the sociologist’s idea of what is reasonable (cogent, orderly) or not in an action, as distinct from an idea commonly employed by members of the actor’s society and constitutive of the meaning of the action. This is to be read, of course, as a criticism of sociology as it is usually done.

There is another way of thinking about, and thinking through, the abstractions of formal analysis. We can begin with idea that a members’ own accounts of “what people do” are accounts of formal properties of practical actions. Sociologists’ accounts are continuous with members’ own actions, which already gloss over significant differences between these (whatever) particular ways of doing things. What may sound like a criticism of sociologists’ accounts is no criticism at all. It applies to members’ accounts of their own actions, which are not to be judged as reasonable (cogent, orderly) or not except in terms of members’ actual activities. According to Hutchinson, Read and Sharrock (2008), ethnomethodology is the form of sociological inquiry “least exposed to Winch’s criticisms” (p. 91). On this alternative reading, Winch’s criticisms need not be taken seriously except insofar as they may be of interest as phenomena for ethnomethodological studies.

2. Internal and External Identity Criteria

Consider the claim that the subject matter of a sociological study is a particular form of human activity. What members do may be of interest to the sociologist as an instance of some very general phenomenon, to be explained along with other instances (along with other forms of human activity) by a theory of the phenomenon in question. Or it may be of interest *as* a particular form of human activity. Accordingly, we can make a distinction between (1) a way of explaining some very general social phenomenon and (2) a way of understanding a particular form of human activity.

Either (1) or (2) may be the goal of a sociological study. Winch (*op. cit.*) argues for the priority of (2) on Wittgensteinian grounds:

An explanation is called for only where there is, or is at least thought to be, a deficiency in understanding. But there has to be some standard against which such deficiency is to be measured: and that standard can only be an understanding that we already have. Furthermore, the understanding we already have is expressed in the concepts which constitute the form of the subject matter we are

concerned with. These concepts on the other hand also express certain aspects of the life characteristic of those who apply them. (pp. x–xi)

Before we can ask whether *this* action calls for explanation, we need to be able to judge *what* action it *was*. Since (1) is expressed in concepts too general for this, we need to begin by mastering concepts in which (2) is expressed.

An example close to home. The beginner in philosophy may be struck by the fact that philosophers take positions, seeing only *that*, not *why*, they take the positions they do. To see *why*, the beginner will need to learn some philosophy. Suppose, for example, that the teacher tries to explain the difference between conceptual analysis and ordinary language philosophy. The teacher's preference could seem as arbitrary to the beginner as a choice of clothes. This, then, might seem to the beginner to call for explanation: the fact that the teacher takes a position.

The beginner generalises. Philosophy is not the only activity in which people take positions. There may be something to learn about this very general social phenomenon, something that would illuminate instances of position-taking in philosophy. What explains the fact that people choose between positions A and B? This question is all the more compelling in the case of philosophy since there is scarcely any consensus among philosophers. Nagel (1987) warns the beginner that philosophy "is a somewhat dizzying activity" and tries to explain why "few of its results go unchallenged for long" (p. 5). But is the explanation peculiar to philosophy? In his Postscript to *Distinction: A Social Critique of the Judgement of Taste* (1984), Bourdieu takes a different view of things. According to him, "the philosophical way of talking about philosophy de-realizes everything that can be said about philosophy" (p. 495). It may be more useful to adopt a sociological way of talking about philosophy, by considering the action of taking a position in this field of cultural practice from a sociological point of view.

The beginner extracts a theory of position-taking from Bourdieu's book. From an anthropological point of view, cultural competences are of equal value. Being able to eat with a knife and fork is in itself no better or worse than being able to eat with chopsticks. Being able to play or appreciate jazz is no better or worse than being able to play or appreciate punk or progressive rock. So the fact that taste is normative calls for explanation. Bourdieu holds that a person's normative evaluation of a cultural competence is a function of rela-

tions of power between social classes. A conception of legitimate culture (of what cultural competences a person *ought* to have) is a means of securing or maintaining power. It confers value on certain relatively rare cultural competences to the advantage of those with a monopoly on them.

The beginner now considers the teacher's defence of ordinary language philosophy, arguing as follows. The teacher speaks highly of Wittgenstein's writings. Wittgenstein's writings are philosophical writings. Philosophy is legitimate culture. Ordinary language philosophy is a way of doing philosophy. So ordinary language philosophy is a way of producing and consuming legitimate culture! For example, it is a way of producing and consuming critical commentary on Wittgenstein's writings. It confers value on the teacher's ability to "read" Wittgenstein. Compare Bourdieu:

The radical questionings announced by philosophy are in fact circumscribed by the interests linked to membership in the philosophical field, that is, to the very existence of this field and the corresponding censorships. The field is the historical product of the labour of successive philosophers who have defined certain topics as philosophical by forcing them on commentary, discussion, critique and polemic; but the problems, theories, themes or concepts which are deposited in writings considered at a given moment as philosophical (books, articles, essay topics, etc.), and which constitute *objectified philosophy*, impose themselves as a sort of autonomous world on would-be philosophers, who must not only know them, as items of culture, but recognize them, as objects of (pre-reflexive) belief, failing which they disqualify themselves as philosophers. (p. 496; original emphasis)

This passage explains the action of position-taking in philosophy as a phenomenon of economic life. Professional philosophers will have an economic incentive to prefer one way of doing philosophy to another, ultimately depending on their social trajectories. The teacher's preference for ordinary language philosophy is a case in point.

Bourdieu's account of tastes in philosophy may seem plausible to the beginner. It may even be true.¹³ But it is too general to be regarded as an account of this particular form of human activity. If it is true of philosophy, it is not true in virtue of anything peculiar to philosophy. It applies to any field of cultural practice. How, then, is one form of human activity to be distinguished from another?

¹³ Whatever it might mean to say so.

Winch works with the idea of the identity of an action. Something is meant to have happened again, an action with certain formal properties: the taking of a position. And it is meant to have been explained by a sociological theory of the normativity of taste. But before we can judge whether *this* action has been explained, we need to judge *what* action it *was*. The expression “taking a position” is too general for this. Consider the variety of actions that might be said to be tokens of this action-type: voting in an election, preferring Hume’s analysis of the idea of causation to Kant’s, not caring that white wine is meant to go with fish, and so on. To describe an action as the taking of a position is not yet to judge *what* action it *was*, since perhaps almost any action can be described as such. More is needed to bring philosophy plausibly and informatively within the scope of Bourdieu’s theory (cf. Sharrock & Anderson, 1985, p. 120).

Identity criteria are needed. The claim that these are “necessarily relative to a rule” is potentially misleading, as Heritage (1984, p. 105) is careful to point out. In some cases, actions will be oriented to rules in obvious ways, as when we speak of rules of etiquette, rules of play and rules of method. Heritage gives the example of a rule of greeting exchanges: after A greets B, B greets A. It is probable, as Heritage suggests, that “most members of our society [will] have received explicit training (or at least some prompting) in this rule, for example by being rebuked for not saying ‘hello’ back to a family friend or relative” (p. 106). In this sense, almost any activity may be said to involve rules, inasmuch as it will occasionally involve prompting, correcting, advising, instructing, and so on. Having lunch involves rules in this advisedly vague sense, and stands alongside first-order predicate calculus as a “rule-governed” activity. However, it might be better to amend Winch’s formula and say more simply that identity criteria are necessarily relative to a form of human activity.

Winch makes a critical distinction between internal and external identity criteria. There are two activities: one serves as subject matter for a sociological study; the other is the sociological study itself. Identity criteria are necessarily relative to a rule. But the rule may be internal or external to the action up for analysis. Consider the difference between (a) criteria used by sociologists to identify an action as the reconversion of cultural capital into symbolic or economic capital, as when philosophers lecture or deliver papers, and (b)

criteria used by philosophers to identify an action as, for example, a misreading of Wittgenstein. Which criteria, (a) or (b), should the beginner learn to use? Winch is clear:

The concepts and criteria according to which the sociologist judges that, in two situations, the same thing has happened, or [that] the same action [has been] performed, must be understood *in relation to the rules governing sociological investigation*. But here we run against a difficulty; for whereas in the case of the natural scientist we have to deal with only one set of rules, namely those governing the scientist's investigation itself, here *what the sociologist is studying*, as well as his study of it, is a human activity and is therefore carried on according to rules. And it is these rules, rather than those which govern the sociologist's investigation, which specify what is to count as 'doing the same kind of thing' in relation to that kind of activity. (p. 81; original emphasis)

The beginner's first task is to learn to do what philosophers do, so as to be able to judge, not only *what* action an action *was*, but also *whether* an action calls for explanation. Do we need Bourdieu to understand the teacher's defence of ordinary language philosophy? On Winch's view, we should begin by trying to understand philosophy (a particular form of human activity) in terms of what matters to people doing philosophy. For example, we should begin by reading Austin's "Plea for Excuses" or the first 125 sections of Wittgenstein's *Philosophical Investigations*. If this proves inadequate for an understanding of philosophers' actions, we can try turning to Bourdieu's writings instead. But until we have learnt to do philosophy, we should presume against the need for a sociological theory. Or perhaps it would be better to say that philosophy *is* a theory of a philosopher's actions, a sort of "wild sociology" (O'Neill, 1975). Without any training in professional sociological inquiry, philosophers do perhaps attempt to explain behaviour: not incessantly, as though explanation had always to precede ordinary human understanding, but whenever an action stands out against the backdrop of philosophy as philosophically strange. McDowell's (2009) explanation of Brandom's (1994, pp. 18–21) reading of Wittgenstein might serve as an example, along with Baz's (2009) critique of the prevailing conception of meaning in the philosophical mainstream.

In time, the fact that philosophers "take positions" will stop seeming strange to the beginner. This is not to suggest that philosophers have a theory of this or any other very general social phenomenon, a theory only expressible in peculiarly philosophical concepts. Their explanations may be sensitive to the occasion of an action, requiring no general theory of anything at all. It is important to keep a Wittgensteinian insight in mind: a sen-

tence used on one occasion to describe something may have been used on another to do something else. An example: 'Pedestrians walk on the pavement.' Must we now explain a fact about society, the fact that pedestrians walk on the pavement, or perhaps the fact that they tend to obey this rule? As Wieder (1974) shows, there are indefinitely many ways in which a rule of conduct might be used. There is no good reason to regard as primary its use as a description of a social fact. Imagine 'Pedestrians walk on the pavement' or 'Philosophers take positions' as criticism, instruction or friendly advice. Baz may be described in plain English as taking a position. (He defends ordinary language philosophy.) But to say he does is not necessarily to give an example of a social fact.

Generally speaking, if we want to understand an action typical of a social setting (the Azande's use of witchcraft, the philosopher's use of Peter Winch), we should not begin by consulting the sociological literature. We should spend time with parties to the setting and learn to join in with them in doing things. In Wittgensteinian terms, we should learn some language-games of an ethnographically relevant kind: activities consisting of accounts of actions and actions into which they are reflexively woven. This must take priority over explanation for explanation to be meaningful at all. Or as Winch says (p. x; in context above): "[T]here has to be some standard against which [a] deficiency [in understanding] is to be measured: and that standard can only be an understanding that we already have."

3. The Relativity of Winch's Distinction

A further point is not so easy to extract from Winch's classic study. *Any distinction between internal and external identity criteria must also be relative to a form of human activity.* By way of illustration, I am going to offer a Brandomian account of the following exchange:

Four students demonstrated their phrases. After each, Harris smiled warmly and nodded in approval. At a fifth student's performance, however, he shook his head and remarked, "No, you wouldn't do that in this music." Stung by the rebuke, the student defended himself: "But you said follow the rule you gave us, and this phrase follows the rule." "Yes," said Harris, "but you wouldn't play a phrase like that." "But give me one good reason why you wouldn't," the student protested. "The only reason I can give you," Harris replied, "is that I have been listening to this music for over fifty years now, and my ears tell me that the phrase would be wrong to play." (Berliner, 1994, pp. 248-9)

The Brandomian is now the beginner, intrigued or puzzled (or reminded of something) by the teacher's predicament. In my previous example, the beginner looked in a book by Bourdieu for an explanation of position-taking. In this one, the beginner looks in a book by Brandom for an explanation of the teacher's entitlement to say what he does, that the student's phrase is inappropriate for the music being played.

Brandom's aim in *Making It Explicit* is not to explain speakers' actions, but to elucidate semantic and intentional concepts in terms of the normativity of language. He relies on the idea of a social practice. A social practice has a normative dimension. It is like a game. It is constitutive of what players do that there are rules of play. It is constitutive of what practitioners do that there are social norms. Brandom makes a second analogy between rules of play and theories of meaning. Theories of meaning are constitutive of what language-users do if they make norms of specially linguistic social practices explicit. To advance this claim, Brandom makes a distinction between two levels of normative activity. One is the level of the grasping of a rule. The other is the level of implicit acknowledgement of a social norm. He reads Wittgenstein as making the same distinction in *Philosophical Investigations* §201:

The important point is that there is a way of grasping a rule that is not an interpretation. This should be talked about, as Wittgenstein at least sometimes does, in terms of *practices* – grasping a rule without interpreting it is grasping it in practice, rather than by substituting one expression of a rule for another. Most cases of understanding explicit claims and obeying explicit orders should be understood in this way. Practices in this sense are the primitive sort of acknowledgment that performances are governed by norms. But according to this way of regimenting the idiom, not all practices are graspings of rules. There are practices that involve the acknowledgement of norms without involving rules at all, except in the sense that others, looking on, may be able to state rules – whose expressions are not available to the practitioners. (p. 65)

Brandom uses Wittgenstein to depict the work of semantic analysis as analysis of actual social practices. If meaning is use, an interpretation of an expression E in a language L can be read as an analysis of norms implicit in the meaning-constitutive social practices of speakers of L. Explanations of meaning succeed or fail in making socially instituted linguistic norms explicit. Such, at least, is Brandom's guiding idea. It should be clear that it falls short of Winch's standard of adequacy. Explanations of meaning are not internal to the practice up for analysis. They are foisted on linguistic phenomena, as we will see more clearly with the example of the teacher's predicament.

Brandom argues that a particular practice is necessary and sufficient for content: the practice (or game) of giving and asking for reasons. Assertion is central to what practitioners do. It is constitutive of an assertion of p that the speaker is capable of using p as the antecedent or consequent of an argument. In some cases, this will mean that the practitioner is obliged to demonstrate entitlement to use p as a reason by giving a reason for p . But it is obvious that a practitioner cannot always be obliged to give a reason for p . Otherwise, a reason for p , which is just another assertion, would be no better than p , but would always oblige the practitioner to give a reason for the reason.

The teacher's predicament can be viewed as an example of this very general socio-semantic phenomenon. He is entitled to p but incapable of discharging his obligation to demonstrate entitlement to use p . He simply asserts p . Brandom has this to say:

Often when a commitment is attributed to an interlocutor, entitlement to it is attributed as well, by default. The *prima facie* status of the commitment as one the interlocutor is entitled to is not permanent and unshakeable; entitled to an assertional commitment can be challenged. When it is *appropriately* challenged (when the challenger is *entitled* to the challenge), the effect is to void the inferential and communicative authority of the corresponding assertions (their capacity to transmit entitlement) unless the asserter can vindicate the commitment by demonstrating entitlement to it. (pp. 177–8; original emphasis)

The teacher's predicament is a presumptive instance of "default" entitlement. The student challenges Harris to demonstrate entitlement to use of p . But there is no need for Harris to respond, except (for practical purposes) to say so. The student has no reason to challenge Harris. So Harris is entitled to p by default.

In fact, Brandom claims that there are two kinds of default entitlement. Some assertions will be "treated as 'free moves' by members of our speech community" (p. 222). Their status is intelligible in terms of their position in a web of beliefs (cf. Quine, 1951). To deny them would be to deny too much by implication for denial to be appropriate. In B's company, A may thus be entitled by default to say that we evolved by a process of natural selection, that both of them will one day die, that the external world exists, and so on. The teacher's assertion serves as an example of the second kind of default entitlement. It expresses perceptual knowledge, like an utterance of the sentence "This necktie is green" in Sellars' (1997) example. Hearing a phrase, the teacher asserted that the phrase was inappropriate for the kind of music being played. He did not mean that it went against the rule

provided. That would have been to give a reason for p , which he could not do, except to say that the phrase sounded inappropriate. The Brandomian spots a real-life example of a reliable non-inferential report. Compare Brandom's story about a reporter of hornbeams:

Suppose that Monique has been trained reliably to discriminate hornbeams by their leaves. As a result of the training, she is often disposed to respond to the visibility of leaves of the right sort by noninferentially reporting the presence of a hornbeam. She understands what it means to claim that something is a hornbeam and, in circumstances appropriate for such reports, actually comes to believe that there is a hornbeam present. She may still be uncertain of her discriminatory capacity long after she has in fact become reliable. In such a situation she may have a true belief that there is a hornbeam in front of her, yet be completely unable to justify that claim (for instance, by citing features distinctive of hornbeam leaves), and even deny that she is a reliable noninferential reporter of hornbeams. (p. 219)

Brandom explains Monique's entitlement as a status conferred on her assertion by other members of her community, who provide her with a reason (their justified belief that her non-inferential reports are reliable) for reporting the presence of a hornbeam. The teacher reports the presence of an inappropriate phrase. Like Monique, he is incapable of saying why he believes what he does, except to repeat in a stuttering inference that he perceives what he does. His entitlement is a status conferred on his assertion by other members of his community. They believe with justification that Harris has been trained reliably to discriminate inappropriate phrases by some (who knows what) general feature. And he believes of himself what they believe of him. His justification is the same: "If this ear says it's inappropriate, then it probably *is* inappropriate." He has this reason to believe what he has said.

It should be clear from this example that external and internal criteria identity may indeed differ. In this case, criteria for the identity of the teacher's action belong to a certain philosophical literature. Indeed, they belong to an exchange between McDowell and Brandom on the nature of perceptual knowledge (see Sellars, 1997; and McDowell, 1995, 1997, 2002; and Brandom, 1995a, 1995b, 1997, 1998). The real-life example ticks certain boxes. For example, it illustrates the difference between the two kinds of default entitlement. Brandom's distinction between platitudes or commonplaces and reliable non-inferential reports serves in turn as way of filling out the details of the exchange. It is a kind of rendering device, a means by which to identify a documented action as a token of an action-type. This distinction is external to the setting of the action itself. By contrast, a distinction made

by jazz musicians between musical idioms (“I have been listening to this music for over fifty years now [and] you wouldn’t play a phrase like that”) is internal to the same documented action.

It should also be clear that any such distinction is relative to the subject matter of a sociological study. Consider Brandom’s (playful?) tendency to cast himself and his readers as discursive deontic scorekeepers. To give just a few examples:

Competent linguistic practitioners keep track of their own and each other’s commitments and entitlements. They are (we are) *deontic scorekeepers*. (1994, p. 142; original emphasis)

McDowell contents himself with making his *commitments* explicit in passages such as those I have cited, without showing just how he would propose to show himself *entitled* to them. But suppose we grant these three claims: the normativeness of representational relations, the rational or inferential articulation of concepts on which the critical assessment of their credentials depends as essential to the contents of those concepts, and the dependence of the idea of a state’s so much as seeming to be about some objective feature of the world on the liability of that state to critical examination of its credentials. I think in fact they are all three both true and important, and that the promissory notes concerning our entitlement to them that McDowell is implicitly offering can be redeemed. (1995b, p. 370; original emphasis)

Addressing this [sic] topic requires making a series of choices of fundamental explanatory strategy. The resulting commitments need to be brought out into the open because they shape any approach to the conceptual in such important ways. Making this background of orienting commitments explicit serves to place a view in a philosophical space of alternatives. (2000, p. 2)

Perhaps it will be clear at this point [sic] how it is that Kant can take it that the systematic obligations of philosophers are merely the explicit form of the very same obligations that are implicitly incumbent on rational knowers and agents as such. (2009, pp. 36–7)

Should we say that Brandom’s analytic categories are external to these (his own) actions? On the contrary, they are available to Brandom and his readers as ways of judging *what* action an action *was*. Brandom says of McDowell that he makes his commitments explicit without demonstrating his entitlement to them. On Brandom’s view, this is to be regarded as neglect on McDowell’s part: not, then, as an instance of entitlement by default. Unlike Harris and Monique, McDowell is obliged to demonstrate entitlement to use *p*. Brandom apparently believes what he says about the “systematic obligations” of philosophers. He has undertaken an obligation to explain meaning in terms of use. He is making it explicit in the book called *Making It Explicit*, and articulating reasons in the book called *Articulating*

Reasons. These self-observations are not a beginner's initial observations in an unfamiliar setting, but a respected philosopher's ways of talking about philosophy. They are a member's own ways of making actions analysable.

Winch's principle applies. If we want to understand analytic philosophy of language (a particular form of human activity), we should not turn for guidance to the sociological literature. Instead, we should try to acquire an understanding philosophers already have. For example, we should learn to play this peculiar language-game of Brandom's, this activity consisting of a model of assertion and inferential practice and assertions and inferences into which it is reflexively woven.

4. Ethnomethodology as Eliminative Critique

I am arguing here for a descriptivist reading of Garfinkel's basic initiative. On this reading, there are no general ethnomethodological findings. In other words, there are no findings that are not already available to members, none that call for a peculiarly ethnomethodological competence. For example, there can be nothing on offer like a specific logic of members' methods, to be set against some other conception of practical action. Indeed, the principle here is to presume against the existence of any unitary phenomenon of social life, not least of all the phenomenon of order as such.

With a view to defending this reading, I want now to begin to consider what difference there could be between ethnomethodology and any of its subject matters. It is clear that sociology differs from philosophy. People doing philosophy, though their actions may be considered as instances of social facts, do not seek to explain their actions *as* instances of social facts. The sociologist does. By contrast, the ethnomethodologist must regard philosophy simply as such: not as something standing for something else (some very general social phenomenon), but as just some particular form of human activity. This imperative may lead us to think of ethnomethodology as an ongoing eliminative critique of sociology, consisting entirely of negative responses to constructive sociological work. I am going to argue for another way of taking Winch's point.

What could be the aim of an ethnomethodological study of philosophical inquiry? An answer comes pat. The general aim of ethnomethodology is not to explain practical actions by presenting them as instances of social facts, but to examine methods members use to

make their own “practical actions” accountable. Since people doing philosophy are *not* doing other things, this is already *not* to regard philosophical inquiry just *as* philosophical inquiry. Like people doing other things, people doing philosophy are making their own actions analysable. Still, there is meant to be no general procedure for doing *that*: for making “practical actions” analysable. Members’ methods are always unique to some particular form of human activity. Along with the rule of ethnomethodological indifference, Garfinkel (2002) lays down “the unique adequacy requirement of methods,” which has a weak and strong prescriptive use (p. 175–6). According to its weak use, philosophical inquiry can only be described by a person competent in philosophy. According to its strong use, it can only be described by a person involved here and now in the doing of philosophy. Either way, ethnomethodology will involve a kind of pointing out. “This was (is) a way of doing philosophy,” the ethnomethodologist will say, intending to leave philosophical inquiry as it is. But this is also what the philosopher will say, intending to criticise, instruct or reform. Where is this difference to be made out except in a corpus of ethnomethodological studies, with analogies, metaphors and programmatic statements external to philosophy? There is a genuine difficulty here. How can the ethnomethodologist, having selected philosophy as a subject matter, distinguish ethnomethodology from philosophical inquiry? It is not enough to cite chapter and verse. If ethnomethodology is a purely descriptive enterprise, how can it claim to say anything at all about this or any other particular form of human activity? What exactly does the ethnomethodologist, *qua* ethnomethodologist, have to say? I will try to answer with another real-life example.

Brandom says of his theory of content that it is “firmly rooted in actual practices of producing speech acts” (p. xiii). By “actual practices” he means either (1) performances which are, or which instantiate, the practice of giving and asking for reasons or (2) the practice itself. Where he refers in the plural to “social practices of giving and asking for reasons” (p. xiv), he means either (1) performances which are, or which instantiate, an invariant practice or (2) elements of “a simple system of [invariant] practices” (p. 157). Performances are tokens of a practice-type: of *the* practice, or of this or that element of *the* practice, of giving and asking for reasons. We should always be able to see a difference between documented performances and tokens of a practice-type. It will be comparable to Durkheim’s “radical disjunction” inasmuch as accounts of a practice-type may be read as

analytic gloss. But unlike a social fact, a practice-type is something people can be said without absurdity to have done. I have already suggested that Brandom's account of a content-conferring practice is external to one particular form of human activity (jazz improvisation) and internal to another (analytic philosophy of language). My example is designed to reveal a difference between philosophical inquiry and a study of philosophy as members' work. It cannot be understood so straightforwardly in terms of Winch's distinction, which will need to be related specifically to this subject matter.

Between Saying and Doing: Towards an Analytic Pragmatism is the published version of Brandom's John Locke Lectures, which he delivered at Oxford in 2006. It appeared in 2008. In the Preface, Brandom discusses a six-day conference devoted to the project of these lectures. It was held in Prague in April 2007, and was "attended by more than two hundred philosophers from twenty-seven countries, including a host of specially invited people" (p. xx). Video recordings of the sessions are available online.¹⁴ The first session begins with a welcome address from Pirmin Stekeler-Weithofer, who chairs proceedings on the first day. Brandom then delivers his first John Locke Lecture. He is followed by John McDowell, who reads a critical piece on Brandom's project. Brandom is given an opportunity to respond before Q&A. Then come four questions. The third, from Friedrich Kambartel (FK) to both Brandom (RB) and McDowell (JM), introduces the theme of the embeddedness of language in a practice. Kambartel declares that he does not read Wittgenstein as a quietist. Wittgenstein assumes that a problematic vocabulary must come together with a practice, even though its embeddedness in a practice may be unclear. This is a position from which to criticise and reform philosophy. Do Robert Brandom and McDowell agree? And if they do, could this way of reading Wittgenstein serve as a mediation of their positions?

McDowell is first to answer. Unlike Brandom, he regards philosophy as problem-driven activity. It seems wrong to him that interest in Brandom's project does not hinge on an antecedent intellectual difficulty, but is self-generated by the project itself. This is not how philosophers of the analytic tradition did philosophy. In response, Brandom calls attention to McDowell's presumption against the genuineness of philosophical problems.

¹⁴ See Brandom's university webpage: <<http://www.pitt.edu/~rbrandom/>>

On McDowell's view of philosophy, problems are always simply misconceptions of language. Finally, Brandom concedes that he pictures philosophy as abstract curiosity, not requiring problems (real or only felt) for motivation. Philosophical understanding is understanding for its own sake.

FK When listening to both of you, to Bob and John McDowell, I got the idea that there could be a sort of mediation, which perhaps you don't have in mind; and that is the only reason for my remark now. I mean I'm not an interpreter of Wittgenstein which thinks that he is a quietist of some kind. But if something would survive the quietistic interpretation of Wittgenstein, I would say it is that a vocabulary, or language expressions, come with a practice together, jointly. Now, if this is OK, if we buy this from Wittgenstein as a fundamental insight, which I would do, then I think we could say that if we have a vocabulary, let's say classical semantic analysis vocabulary, which has so to speak got off the ground, the practical ground, which can't make clear its embeddedness in a certain practice which comes with it, that then this vocabulary is a problematic vocabulary, in a sense I think that John McDowell used the word problematic vocabularies. So at least there are problematic vocabularies, even in the Wittgensteinian perspective. And then the question is, if handling this analytically in some sense, handling this problematic vocabularies, would mean to give them back some embedding, some embeddedness in a certain practice. So that, in my listening to you, must be bought by both of you, as far as I can see. And then the problem is, how do we do that? How do we retribute to those problematic vocabularies their, by Wittgenstein assumed, grounds, practical grounds? How do we do this? That would be one thing. And the other thing would be, if we don't succeed in doing this, is this a very severe reason to say, and I think Wittgenstein had something in mind of that kind, do away with this vocabulary? So that's what I want.

JM I think I agree with everything you said; I'm just not sure it amounts to what you offered it as: a mediation of what Bob wants to do with Wittgenstein and what I think's actually there in Wittgenstein. Surely it's right that the very idea of a vocabulary with a significance is inextricably bound up with the idea of a way of using that vocabulary. And that yields a possibility of conclusive, if you can make it out, ways of representing candidate vocabularies as problematic. I mean if you can say of some supposed meaningful, some attachment of a meaning to a word, "Look there's only a fantasy in the area where there would need to be a real viable practice of using of a word in such a way that it has that meaning," well there's something badly wrong with the thought that [*trails off*]. But that's not what Bob is doing. That's, as I hear it, beginning on sketching a programme for a general description of a mode of philosophical activity – a very general divergence here between Bob and me – a mode of philosophical activity that's driven by problems, or maybe one should say by supposed problems. Philosophers don't, for no reason out of the blue, fall into fantasies of the attachability of meanings to words; they do

so in response what they perceive as problems. The large difference between Bob and me as I see it is that he wants to not just describe but engage in a mode of philosophy that isn't problem-driven. The spirit in which one engages in it is something like, "Let's do this, maybe something interesting will come out." Yeah, with luck something interesting will come out. But the interest of the envisaged result doesn't hinge on there having been antecedently some intellectual difficulty that the result will answer to. So the interest is self-generated by the project itself. Whereas my paradigm philosophical activity is problem-driven, and then I say supposed problem-driven. It's fine for Bob to do what he does, but I don't think he ought to represent doing what he does as going on doing what problem-driven philosophy was already doing. There's a cleaner break from the classical analytic tradition, and the pragmatic challenge to it if that's going to be personified by Wittgenstein, in the kind of activity that Bob is recommending – especially in his responses after the lecture – than I think he confesses to.

RB Well I think John is right that one of the big differences between us is that he thinks that philosophical activity is, and by rights ought to be, driven by felt philosophical problems. I mean maybe in the interests of full disclosure he should mention that he thinks those problems are always misconceived, and that the result will always be a diagnosis according to which you should not in the end have been puzzled and, you know, will be brought back to something like where you were before only now being comfortable being there. Certainly my picture is much more abstract curiosity about how things work that I don't think needs to be driven by thinking that we're going to be in deep trouble if we can't, you know, understand how this works in these other terms. Understanding is a matter of knowing your way about – poke over here and see how it looks from here – and it doesn't seem to me that that needs to be driven by the sort of urgency John is talking about.

Before considering this exchange from an ethnomethodological point of view, we should examine some specifically philosophical details of Brandom's response to McDowell.

Brandom does "take a position" in saying what he says. He rejects a Wittgensteinian conception of philosophical inquiry in favour of a sort of meta-philosophical inferentialism. Compare *Philosophical Investigations* §133: "It is not our aim to refine or complete the system of rules for the use of our words in unheard-of ways. For the clarity we are aiming at is indeed *complete* [original emphasis] clarity. But this simply means that the philosophical problems should *completely* [original emphasis] disappear." Brandom rejects the idea that philosophical problems are symptoms of an illness, driving the suffering philosopher to find piecemeal therapies. But he comes close to Wittgenstein in his idea of clarity in philosophy. See, for example, *Philosophical Investigations* §122: "A main source of our

failure to understand is that we do not *command a clear view* [original emphasis] of the use of our words. – Our grammar is lacking in this sort of perspicuity. A perspicuous representation produces just that understanding which consists in ‘seeing connections’. Hence the importance of finding *intermediate cases* [original emphasis].” If we take §122 alone, inferentialism is compatible with this conception of understanding, and with Brandom’s view of philosophy as curiosity. We are to imagine a web of inferential connections in which our words are caught up, meaning what they do in virtue of their roles in indefinitely many possible moves in the game of giving and asking for reasons. For Brandom, the point of philosophical inquiry is not to relieve the philosopher’s suffering, but simply to see what gives a word its meaning. He proceeds in the hope that his work will make at least some inferential connections perspicuous, those that may be said to confer a meaning (this or that meaning) on the word. So he does not begin with *felt* problems, to be cured by a therapy appropriate to them, but with the most general experimental aim of discovering such connections.

Brandom’s way of commencing is distinctly rationalist. A thought is simply entertained, a sentence simply said. Nothing is asserted. But a possible assertion is brought into play. A possible assertion is a point at which various lines of reasoning intersect. It is an antecedent or consequent of indefinitely many arguments. It contains words which have a meaning (this or that meaning) in virtue of their various inferential roles. Simply entertained, an arbitrary assertion serves as a starting for a philosophical inquiry.

In his Afterword to *Between Saying and Doing*, Brandom recalls his time as a student at Princeton, where he was struck by the example of David Lewis:

David Lewis propounded a view of philosophy that was inspiring to me when I was his student, and inspires me still. He thought that what philosophers should do is lay down a set of premises concerning some topic of interest as clearly as possible, and extract consequences from them as rigorously as possible. Having done that, one should lay down another, perhaps quite different set of premises, and extract consequences from them as rigorously as possible. The point was not in the first instance to endorse the conclusions of any of these chains of reasoning, but to learn our way about the inferential field they all defined, by tracing the many overlapping, intersecting, and diverging paths through the terrain. That is how we learn what difference it would make, in various contexts, if we were to endorse some claim that figures as a premise in many of the inferences, and what might entitle us to a claim that shows up as a consequence in many of the inferences. Actually

plumping for and defending any of these theses is then a subsequent, parasitic, and substantially less important stage of the process. The principal aim is not belief, but understanding. (pp. 225–6)

Wanderer (2008) picks up on this. He asks why Brandom is concerned to elucidate semantic and intentional concepts in terms of a normative pragmatics. We can expect a philosopher to want to elucidate a puzzling concept. But Brandom shows no sign of being puzzled. He is already comfortable with the vocabulary of content (reference, inference, truth, etc.) and with the vocabulary of thought (intending, desiring, believing, etc.). Why, then, does he strive in *Making It Explicit* to describe a practice in the context of which the content of these concepts is clear? Wanderer suggests that Brandom is motivated by “a kind of genuine intellectual curiosity” (p. 38). Following the example of David Lewis, he begins by laying down two basic premises: (1) There is a practice P that is necessary and sufficient for content. (2) P is the practice of giving and asking for reasons. In *Making It Explicit*, (1) and (2) are nowhere asserted but only ever entertained. The point is not to show that there *is* a practice that is necessary and sufficient for content, or *what* that practice must be. The point is to use (1) and (2) to discover connections that elucidate semantic and intentional concepts. This, of course, is just what baffles McDowell: “[T]he interest of the envisaged result doesn’t hinge on there having been antecedently some intellectual difficulty that the result will answer to. [It] is self-generated by the project itself.” Why bother? Brandom’s answer has a scientific ring: “[M]y picture is much more abstract curiosity about how things work.”

So much for the exchange in April 2007 between Brandom and McDowell. Obviously, that it occurred in April 2007 will be of interest to the student of philosophy only insofar as the date marks a point in the development of Brandom’s thinking. (*Making It Explicit* first appeared in 1994, etc.) For the ethnomethodologist, that it occurred when it did as the event it was is of interest in itself. That there *was* something to document is of interest. It serves as an example of what Garfinkel first called a “quiddity” (1988) but later preferred to call a “haecceity” (1991). Its having been just this event provides an opportunity to ask how members made their own actions analysable.¹⁵

Winch’s point is corrective on an analogy with Wittgenstein’s critique of philosophy, and is consistent with Austin’s (1961b) critique of formal-analytic theory of action. It

¹⁵ Or, if these terms are preferred, how members produced the order of this particular exchange.

makes no sense to abstract an action-type from a particular form of human activity and ask what it means to members. I daresay that a student of philosophy would be confused by the instruction to pick out an action from the transcript reproduced above. In the context of what activity might I pick out an action? In what language-game? This is what the sociologist does, not what a member does. I am not “picking out an action” when I say that Brandom endorses a particular view of philosophy, as though something (an action) that has a certain meaning for members (that of an endorsement of a particular view of philosophy) had certain formal properties of its own. Like the beginner, the sociologist is prone to abstract an action-type from philosophical inquiry. It is an error to do so. For it is part of what the sociologist picks out that it has a meaning. This is not to say that what a person did (what an action meant) is never uncertain to members, or that their accounts of what a person did are incorrigible, but that questions of meaning cannot be settled at the level of a philosophical or sociological theory of action.

Consider what I described above as a concession to McDowell: “Certainly my picture is much more abstract curiosity about how things work,” etc. Was I right to describe Brandom as making a concession? Could he not have been boasting or hedging his bets? Should we even call this exchange between Brandom and McDowell “philosophical inquiry”? Is it not really a kind of theatre, even philosophical pantomime? For that matter, might the beginner not be right to mention Goffman or even Bourdieu in this particular case?

These are questions of meaning. Such questions do occasionally arise for members. But they are bound to arise for the sociologist who *begins* with the idea of the doing of an action as such. To *begin* with this idea is to place a taboo on precisely what we need to settle questions of meaning: an account of the practical circumstances in which a person did an action in a particular way. Bourdieu (1984) calls this taboo the “objective epochē” of reflexive sociology, to be followed by an epochē of another kind, one that includes sociology itself in the domain of sociological phenomena. Giddens’ (1976) “double hermeneutic” is similar in design. Winch’s broadly Wittgensteinian point is that the sociologist who *begins* by bracketing “the understanding we already have” (p. x; in context above) thereby *generates* a problem of meaning, one that completely disappears when we see its methodogenesis. In this case, the understanding we already have is a philosopher’s own understanding of philosophical inquiry. Brandom “did an action” that any member would recognise as prac-

tically significant. He conceded something to McDowell. Later, in his Afterword to *Between Saying and Doing*, he recalls the example of David Lewis and defends a view of philosophy according to which abstract intellectual curiosity should suffice to motivate inquiry. Winch's point applies: uniquely philosophical identity criteria are indeed internal to this action and these (correct or incorrect) "formulations" of it; they cannot be subtracted from this token of some very general action-type.

What are the implications of this fundamental insight? In negative terms, it begins to seem that there is "no such thing as a social science" (Hutchinson, Read & Sharrock, 2008). Where "explanations" explain anything at all, they depend for their intelligibility on a member's understanding of a particular form of human activity. This must be shown, not once or twice, but again and again. Sociology is a kind of metaphysics. We are always at risk of being led astray by superficial resemblances between actions. It is necessary to rehearse the same basically Wittgensteinian arguments in endless context-specific eliminative critique of professional sociological inquiry, finding ad nauseam that the understanding we already have is a necessary condition for the possibility of explanation at all and is presumably sufficient for explanation in particular cases.

Are there any positive implications? Is there, then, another way of studying social life, a programme not exposed to Winch's criticisms? That there could *be* such a programme is by no means obvious. Arguably, the person who wishes to understand philosophy should approach this task neither (1) as a sociologist nor (2) as an ethnomethodologist, but simply (3) as a student of philosophy. Ethnomethodology only complicates things. Philosophical inquiry, selected by the sociologist as a subject matter, is now to be studied as a phenomenon in its own right. It is not to be regarded as a manifestation of something else, to be explained by a theory of some considerably more general social phenomenon, but is instead to be regarded simply *as* philosophy, by means of the formula that activities are identical with methods members use to make their own actions analysable. This is *not* to do what Winch recommends. On Winch's view, if we wish to understand a particular form of human activity, then we must, and presumably need only, join in and become competent in the doing of it. This will be a difficult view for anyone who wishes in addition to publish research. But it is not a difficult view *per se*. The ethnomethodologist is never primarily concerned to understand a particular form of human activity. Philosophy is to be done, not

for the sake of philosophy, but the for the sake of ethnomethodological research. It is a particular means to a very general end, a way of making the case against formal-analytic sociology. There is a genuine difficulty here.

The ethnomethodologist is not to depart from the subject matter of a sociological study. If the subject matter is philosophical inquiry, the “asymmetrically alternate” ethnomethodological study must be grounded in an understanding philosophers already have. Coleman (1968) objects that this limits findings to what is already obvious to ordinary members of society, and that Garfinkel hides the sociological banality of ethnomethodological studies in sheer verbosity. It is easy to understand this perhaps rather common (incorrect) reaction to ethnomethodology. Thus, where Brandom responds to criticism from McDowell, and we wish to do what Winch recommends, we may find ourselves saying of “people doing philosophy” that they are parties to a particular form of human activity, that one of them did an action, that it *was* the action of responding to criticism, that in responding to criticism a person *was* doing philosophy, and *was* found by other members to be doing philosophy, and that the doing of philosophy, like the doing of any other particular form of human activity, is a phenomenon in its own right. What have we said? Just that Brandom responded to criticism from McDowell. But this is not banal. If ethnomethodology is eliminative critique, the point of these “descriptions” is once again to criticise sociology on broadly Wittgensteinian grounds. They remind the sociologist that there is never anything more to say about the doing of an action than members are already capable of saying. The rest is an attempt to solve a problem generated by sociology itself. It only obscures this (whatever) particular language-game. On this view, the point of ethnomethodology is not to make discoveries of its own, but (as Brandom puts it in the transcript) to offer a diagnosis according to which we should not have been puzzled in the first place.

I do not feel happy with this interpretation of ethnomethodology. In the remainder of this chapter, I am going to suggest another way of understanding Garfinkel, also derivable from Winch’s distinction between internal and external identity criteria.

5. Ethnomethodology and Ordinary Human Understanding

What is the difference between philosophical inquiry and a study of philosophy as members’ work? A Winchian criticism of Brandom’s pragmatism provides a clue. Brandom

glosses over significant differences between various ways of using language, seeing all as instances of invariant formal properties of meaningful speech. We can apply the same formula to members. Generally speaking, members gloss over significant differences between their own practical actions, seeing all as instances of certain invariant formal properties. For example, philosophers tend to see these (whatever) particular ways of doing philosophy as instances of just a few familiar items of philosophy: as instances of familiar problems, familiar approaches to problems, familiar positions on ways of doing philosophy, and so on. In short, they tend to assimilate their practical actions to just a few peculiarly philosophical models of what people (philosophers) do. Ethnomethodology differs from philosophy as an inquiry into the lived work of such assimilation. Like other members, philosophers are “not interested” and are even “specifically uninterested” in that work, i.e. in their ways of making their own actions analysable *as* instances of just a few familiar items of philosophy.

This interpretation does not imply a global criticism of philosophical inquiry. If the ethnomethodologist has a criticism to make, it can only be a member’s own criticism, and can only be a criticism of some particular assimilation. Recall Wittgenstein’s initial point about Augustine’s model of meaning. It does illuminate *some* ways of using language. We err in expecting it to illuminate all. Likewise, the philosopher errs *on some particular occasion for speaking* in expecting just a few familiar items of philosophy to be illuminating. The point of picking out what is unrepeatable in a documented action is to question some such active expectation.

I am going to give an example from my own practice as a teacher of philosophy. It should come as no surprise that I am guided in what I do *as* a teacher of philosophy by a broadly Wittgensteinian conception of philosophical activity. I think of philosophy, not as a body of knowledge, but as the activity of detecting and addressing certain conceptual difficulties. I expect students to have trouble answering textbook questions, but do not require that a picture hold them captive. That would be absurd. My view of things as a teacher of philosophy is that a picture probably *is* holding them captive if they have trouble answering.

This outlook is severely compromised by the bad science of good practice in teaching today. According to the likes of Geoff Petty (1997), the teacher’s task is always to facilitate

the production of evidence of learning. Teaching begins with formulations of SMART learning outcomes: philosophical insights must be specific, measurable, achievable, realistic and timed. In my opinion, there can be no such thing as a SMART learning outcome in philosophy. Otherwise, Socratic method would be trivially ironic. But this will not be heard by the leading lights of evidence-based learning, who implicitly reject Wittgenstein's view of philosophy (to say nothing of Garfinkel's view of professional sociological inquiry).

To be clear, my aim here is not to criticise the prevailing conception of good pedagogical practice. Instead, I want to present an example of what might be called formal-analytic foreclosure. By this I mean an overwhelming tendency on the part of a member to ignore significant differences between particular uses of language – not for the sake of an explanation of some very general social phenomenon, but for the sake of whatever concerns members.

The example comes from an introductory course on philosophy for first-year undergraduate philosophy students. Ironically, I had been inspired by the ethnomethodological literature (Livingston, 2008; McHoul, 1982; Watson, 2009) to use recordings of classroom exchanges to teach the course. I made audio recordings of seminar discussions and video recordings of students' responses to specially prepared questions. The questions often anticipated the topic of the following week, which allowed me to present a passage from a course reading as commentary on an exchange. For example, I recorded students answering the question "Do you *know* the sun will rise tomorrow?" before we had studied Hume's analysis of the idea of causation, which students subsequently used to identify and describe common features of their responses.

As the end of term approached, I selected and transcribed approximately 90 minutes of recorded material to be used as questions in an extended matching questions test.¹⁶ I envisaged a test of basic competences of academic philosophy (cf. McCoubrie, 2004), and distinguished the following four. Students should be able to identify (1) some "isms" of philosophy, (2) some views of philosophers, (3) some principles of philosophy and (4) some errors of reasoning and/or rhetorical ploys. Answers for each theme of the test were lists of course content. For example, candidates were asked to decide whether a speaker (one of them) was

¹⁶ See appendix.

endorsing empiricism, rationalism, methodological scepticism, realism, idealism, relativism or behaviourism.

Inasmuch as activities were always designed to enable learning, teaching/learning can be compared here to “protoprofessionalization” in psychotherapy sessions (Hak & de Boer, 1996). Candidates would be asked to identify their own remarks as instances of these (1–4) philosophical things. The high-scoring students would be those who were able to hear in their own remarks what a competent philosopher would hear in them, like patients hearing their own remarks as instances of denial, unhealthy rumination or awfulisation. It was my conscious intention to prepare them in this way for more advanced courses.

I have come to regard the test as an ill-conceived attempt to exploit the existential dimension of members’ work. Compare Garfinkel (1967):

It is suggested that students of decision making may find it profitable to reconsult Cassirer’s laws that describe the ways that human situations are progressively clarified. Cassirer’s “law of continuity” states that each outcome is a fulfilment of the preceding definition of the situation. His “law of new emphasis” states that each outcome develops the past definition of the situation. These “laws” remind us that persons, *in the course of a career of actions*, discover the nature in the situations in which they are acting, and that the actor’s own actions are first order determinants of the sense that situations have, in which, literally speaking, actors *find* themselves. (pp. 114–5; original emphasis)

My absurd idea was to bring my students to the brink of an existential discovery, leaving them to find for themselves what was determined in advance: that their own actions had been first-order determinants of just a few familiar items of philosophy. In the meantime, I was ironically “not interested” in the work of my own hermeneutically heavy-handed assimilations. My efforts were directed to the task of having successfully taught X, Y and Z. This is an example of what I have suggested calling formal-analytic foreclosure.

Consider this example. In a session on language, I had used a clip of a documentary available online to introduce students to some of the difficulties of Brandom’s question. What must a noise-making creature be able to do to qualify as a speaking being? The clip shows animal psychologist Irene Pepperberg demonstrating a parrot’s ability to use language. Its name is Alex (short for Avian Learning Experiment). Pepperberg presents Alex with a tray of objects and asks, “How many green block?” Alex gets the answer right. The presenter explains in voiceover: “This is an elementary logic problem. Alex can’t just count up all the green things and he can’t just count up all the blocks. Alex has never been trained

with this particular collection of things.” There is a cut to an interview in which Pepperberg adds: “One of the things Alex doesn’t have is a knee-jerk response to the type of objects you present him. He can look at two objects and answer several different types of questions about those objects. Or he can look at a novel collection of items and answer questions about that collection. What this shows us is that he really understands what those questions mean.” At one point, Pepperberg interprets Alex as asking for water simply to interrupt the demonstration. Here is the transcript of the exchange:

01 P How many?
 02 A Two
 03 P That’s right. You’re a [good boy
 04 A [(Can I) go back? (I want to) go back
 05 P No you can’t go back yet
 06 (1.5)
 07 P You’ve [got to
 08 A [(I want) water
 09 P Alright
 10 ((P gets some water and offers it to A))
 11 P Do you want some water or are you just asking to interrupt?
 12 A (2)
 13 P You’re just asking to interrupt. I [know
 14 A [Go back

Does Pepperberg “formulate” the silence at line 12? Not, perhaps, if it takes two to formulate. She interprets Alex as having done the action of not drinking water (cf. Lynch, 2001, p. 132; Sacks, 1992, p. 293). My transcript encourages the same interpretation, since it represents a silence of two seconds as a turn. In the context of Brandom’s inquiries, it would beg the question to identify this silence as the second item of an adjacency pair (Schegloff, 1968). For that would be to assume what Pepperberg’s demonstration is designed to establish. These remarks are to bring the problem of the following exchange into clear view:

01 S1 No I was just thinking that as you said if it’s routine then maybe it happens
 02 the same every time and he goes through the charade of it. Also [he asks for
 03 water
 04 T [Charade?
 05 S2 But when she offered him water he didn’t [want it

06	S2	[That's the thing like, I was going
07		to say
08		H::e (.) He <u>asks</u> for the water. He gets [given the water
09	T	[<u>Did</u> he ask for the water?
10	S1	He <u>said</u> water
11	T	But did he <u>ask</u> for water?

What does the teacher do at lines 11? At line 09, by putting stress on the auxiliary verb, he indicates the problematic nature of the assumption that Alex had “asked” for water. At line 10, the student offers as a reason for answering in the affirmative the equally problematic observation that Alex “said” water. At line 11, by using a discourse marker and putting stress on the main verb, the teacher calls attention to the circularity of the student’s response. Followers of Winch might object to these thin descriptions of conversation analysis as an obstruction to proper understanding (cf. Sharrock & Anderson, 2011), since they do little to illuminate the matter at hand. Did Alex do what the student interpreted him as having done? The teacher’s point (as he recalls or reconstructs it) was that it would be circular here to claim that Alex had “said” anything at all.

This was one of the exchanges that found its way into the extended matching questions test.¹⁷ The answers (only four) were the following “maxims for doing philosophy”:

1. Avoid vague and ambiguous expressions.
2. Never take a crucial assumption for granted.
3. Do not change the subject.
4. Do not introduce irrelevant considerations.

Candidates were to decide which maxim the student had violated at line 10. Looking back, I wonder why I omitted my response at line 11.

Now for some comments on this example. We were “doing philosophy” in the most ordinary sense of that phrase. However, it embarrasses me to admit that I cannot say what the “most ordinary sense” of that phrase is. I could mention the fact that the exchange took place during a seminar of a philosophy course, that the student was enrolled on the course, that the teacher was employed by the university to teach it, and so on. But there is obvious-

¹⁷ Section D, Question 8.

ly nothing to prevent a non-philosophical exchange from breaking out under the same circumstances. (“I’ve lost my bag,” etc.) I am familiar with various conceptions of philosophical inquiry, two of which I have presented in this chapter. But I think of these on an analogy with competing conceptions of a rule. They suggest to me that there is something, philosophy, which they are indeed conceptions of. What good would it do to pick one or introduce another as a standard? We would be left with the same problem: whether and in what way the exchange was an instance. For these reasons, I am inclined regard the phrase “doing philosophy” as a piece of linguistic equipment. I have a feel for its use, acquired by considering very many intelligible uses of it. I would not expect to find something common to all uses, but only a network of similarities overlapping and crisscrossing in various ways. In any case, it could hardly be the aim of an ethnomethodological study to define philosophy. We take an activity carefully documented and properly glossed as “doing philosophy” and ask how that phrase (or its cognates) were reflexively embedded in these historic actions. In short, we examine a Lebenswelt pair.

A similar point. My responses to the student, and her responses to me, were obviously not scripted. Yet there was something the student wanted to say. Call it the Speaker’s Point. Where the transcript ends, the Speaker’s Point remains to be seen. The reason is not that transcript ends where it does. It is typically the case in philosophy that the Speaker’s Point remains to be seen. Socrates comes to mind. This may be why analysis leaves the (false) impression of a quasi-improvised achievement.¹⁸ On the other hand, there may be nothing special about philosophy in this respect. For example, the Speaker’s Point typically remains to be seen at the end of a psychotherapy session. Indeed, it may not be going too far to say that it remains to be seen at the end of a person’s life. Thus, though this exchange took place several years ago, the student (perhaps now a philosophy teacher) may still be working out the Speaker’s Point.

Next. Though there may well be something to learn about philosophy in advance of a seminar, there is nothing in general to learn about members’ work. There is no fixed logic to any of this. What happens is this: with some vague sense of the matter at hand, or perhaps with some relatively precise sense, we respond to one another as seems appropriate to

¹⁸ The locally co-produced and endogenously accountable production of order.

the situation. Obviously, the sense of what is happening now may depend on what has yet to happen, leaving parties to activities inclined in some cases to keep semantic options open. But it is no part of a logic to say that vagueness is sometimes, though not always, actively or routinely preferred, despite an official line that says the opposite. That is not always so. Nothing, perhaps, is always so.

The last comment concerns the idea of a member. The term “member” refers neither to a particular participant role (philosopher, jazz musician, sociologist, etc.) nor to the occupant of a role. It should be understood as a limit concept on analogy with Kant’s thing-in-itself. No one can come forward as a member in the ethnomethodological sense. No one can come forward as competent to do this or that as a matter of fact, since it is not a feature of a person or office that the person or office-holder is competent. Membership is a matter of practical concern for people in the process of doing something. Let Monique’s colleagues and neighbours say what they will. There may be someone here today whose judgment is reason to doubt hers. More realistically, it is always possible that the beginner in philosophy (and Kant was once a beginner in philosophy) has greater insight than the teacher. For all I know, S1 merely lacked the erudition to make herself clear, and would *in time* have changed my mind about Alex’s abilities.

The basic point is that a person who lays claim to competence in philosophy lays claim to competence in something, and may fairly be asked: “Competence in what?” This question brings us back to square one.

In light of this example, it is easy to see the appeal of the idea that meanings are conferred on actions by other actions. A meaning is conferred on what the student does at line 10 by the action of the test. The idea is this: it was neither true nor false of the first action (what the student does at line 10) that it *was* the action of taking a crucial assumption for granted; that was what it was identified as being by high-scoring students. To extrapolate, it is neither true nor false of any action that it *is* the action of doing such-and-such. This conclusion is doubly fallacious. Suppose we do have an example here of a meaning conferred on one action by any other. It is fallacious to assume that this feature of some “meanings” is common to all. It is also fallacious to infer absence of meaning (the mere doing of an action) from indeterminateness of meaning. I would hazard a diagnostic remark in this connection about Brandom and his readers, where they ask what speakers

must be able to do for their expressions to be meaningful at all. If the meaning of an action is a sentence saying what an action means, then there *was* absence of meaning. For there were no sentences saying what our actions meant. These came later as answers in the test. But these sentences depended for their intelligibility on an understanding we already had.

Features of my example should make it possible to see a simple difference between members' work and a study of members' work. Compare:

- (1) Interest in a problem.
- (2) Interest in these (whatever) ways of doing things.

Some definitions. A problem is an incentive to do things. The incentive may be active or at rest: their problem but not ours, our problem but not theirs.¹⁹ It is active where practical actions – these (whatever) ways of doing things – are reasonable, cogent, orderly or not *given* the problem. Where an incentive *is* active, people will naturally take an interest in their own practical actions, but only for the sake for the problem. People doing things subordinate interest in their ways of doing them to interest in the problem. They subordinate (2) to (1). Garfinkel's basic initiative is to reverse the direction of interest. Ethnomethodology subordinates (1) to (2). Selecting a problem as a subject matter, we refrain from acting in response. It is their problem, not ours. We still take in interest in it. But instead of becoming absorbed in an activity oriented to the subject matter, we ask how people use the subject matter to produce order. This is our problem, not theirs: to show that the orderliness (analysability, intelligibility, reasonableness, cogency, etc.) of an activity is there in the very doing of it, not brought in as a meaning conferred on one action by another.

¹⁹ Cf. Husserl, 1970, p. 136: "Within [the transcendental] epochē, however, neither the sciences nor the scientists have disappeared for us who practice the epochē. They continue to be what they were before, in any case: facts in the unified context of the pre-given life-world; except that, because of the epochē, we do not function as sharing these interests, as co-workers, etc. We establish in ourselves just one particular habitual direction of interest, with a certain vocational attitude, to which there belongs a particular 'vocational time'. We find the same thing here as elsewhere: when we actualize one of our habitual interests and are thus involved in our vocational activities (in the accomplishment of our work), we assume a posture of epochē toward our other life-interests, even though these still exist and are still ours. Everything has 'its proper time', and in shifting activities we saying something like: 'Now it is time to go to the meeting, to the election', and the like."

I have related myself in three ways to a particular problem. How should we receive the claim that Alex said something? I was concerned as a teacher of philosophy to point out some of the difficulties of this question. An incentive to do things *was* active for “members” *then*. I am now concerned as a PhD candidate to assess Brandom’s way of answering it. An incentive to do other things *is* active for “members” *now*. But I am also concerned to present this question as a possible subject matter for an ethnomethodological study. So I have tried in this chapter to refrain from acting in response. How should we receive Brandom’s claim that a practice P is necessary and sufficient for content, so that Alex said something if and only if Alex was doing P in making a sound (the sound of the word ‘water’)? Selecting *this* as a subject matter, I have tried to subordinate (1) to (2). Instead of allowing myself to become absorbed in the intellectual difficulties of Brandom’s pragmatism, I have tried to show what it could mean to say that certain documented ways of doing things were reasonable, cogent, orderly or not *given* those very intellectual difficulties. In other words, I have tried to show that the question I have refrained from trying to answer was “internal” both to Brandom’s response to McDowell (first example) and to T’s response to S₁ (second example).

Contrary to the more conventionally Winchian reading of Garfinkel’s basic initiative, I have also tried to show that there need be nothing “external” to members’ work for us to be able to examine it as such. In other words, a study of members’ work need not involve criticism of formal analysis. This is crucial if problems of formal analysis are not to be given a special status. And Garfinkel is clear on this point. Problems that arise for people doing formal analysis are to be regarded with indifference as possible subject matters for ethnomethodology. To make sense of this instruction, we need to think of Winch’s critical distinction as an only occasionally useful instrument of ethnomethodological research. People doing sociology subordinate interest in their ways of doing things to some particular problem. Winch objects that the sociologist’s identity criteria are external to actions up for analysis. His arguments bring sociology into view as just another particular form of human activity. The point of ethnomethodology is not to show that Winch was right about sociology, but to study what Winch was wrong or right about, sociology, as members’ work. It is part of the problem we now select as a subject matter that Winch *was* wrong or right about sociology. We now refrain from responding to this problem: from agreeing or disa-

greeing with Winch. Instead, we take a radically altered interest in the problem of identity criteria, asking how members subordinate interest in their own practical actions to interest in Winch's arguments.

Winch's distinction falls away. We are left with a clear view of philosophical inquiry as such, unobstructed by the urgency of a philosophical problem. We see Brandom responding to McDowell and "taking a position" on the nature of philosophical inquiry. We see a teacher responding to a student and "taking a position" on her understanding of a philosophical problem. This is material for another Winchian critique of formal-analytic sociology. For example, where Bourdieu (1984) presents philosophy as legitimate culture, we object on broadly Wittgensteinian grounds to the sociologist's procedures. We offer another critical methodogenesis of a problem of formal-analytic sociology. But there is no need to do so. Members' work makes a claim on our attention as a phenomenon in its own right – as sociology's fundamental phenomenon. Considered as such, philosophical inquiry is subordination of interest in these (our own) practical actions to interest in some particular problem. It will be part of the problem, whatever it is, that the sense of the phrase "doing philosophy" remains indeterminate. In other words, we will be using some such phrase as "doing philosophy" (or "doing analytic philosophy of language") as analytic gloss. Ethnomethodology is subordination of interest in that same problem to interest in these (our own) practical actions, which indifference brings into clear view. There is no need to bring another problem into play, except to say by way of clarification that it is *not* ours. This, I want to say, is the meaning of the emphatic *not* of contrastive definitions of ethnomethodology. It should not be interpreted as implying criticism of formal-analytic sociology, and *cannot* be so interpreted where the ethnomethodologist has selected a problem of formal-analytic sociology as a subject matter. In such cases, there is nothing in the vicinity to criticise except members' work.

We are also left with an attractively austere conception of ethnomethodology. What is the basic aim of an ethnomethodological study? It can only be to understand what people said or did on particular occasions. There can be nothing on offer like a special logic of members' work: nothing, then, to compete with Brandom's account of invariant formal properties of practical actions (norms implicit in any "specifically linguistic" practice). On offer is just some unobstructed understanding of another human being, where there could

have been, or where there was, a deficiency in understanding. Thus, ethnomethodological descriptivism goes hand in hand with an occasionalist view of problems of meaning. On this reading of Garfinkel's basic initiative, Garfinkel agrees with Austin and Wittgenstein that problems of meaning do not arise, and cannot be settled, at the level of a theory of content, but only at the level of historic uses of language.²⁰ But he agrees in a way that makes it possible for us to refrain from philosophy even here, by studying philosophical inquiry as members' work. For the ethnomethodologist, the question is emphatically *not* whether Alex said something, or whether the student was wrong to say that he did, but whether we have grasped the Speaker's Point on this particular occasion. There is a difference. And ethnomethodology in general can be understood as way of being concerned with such a difference. It is just a way of trying to understand.

6. Summary

I have rejected a Winchian interpretation of ethnomethodology in favour of the view that ethnomethodology is purely descriptivist. On the Winchian interpretation, Garfinkel takes a critical view of formal analysis. He makes a distinction, comparable to Winch's, between methods used by sociologists to explain social facts and methods used by members to make their own actions analysable. He objects to the identification of practical actions as instances of social facts, arguing like Winch that criteria used by sociologists to identify instances are external to things members do. I have been led to reject this interpretation by the idea that people doing sociology are also and *eo ipso* members. To make sense of this idea, we need to give up the interpretative support of an eliminative critique of sociology. Where we have taken a problem of sociological inquiry as our subject matter, its critical elimination is the elimination of what we have decided to study: sociology as members' work. So if it is true that ethnomethodology is the form of inquiry "least exposed to Winch's criticism" (Hutchinson, Read & Sharrock, 2008, p. 91), this should be seen an accidental feature of a selection of subject matter. For example, though Garfinkel's (1967) account of jurors' deci-

²⁰ I am invoking the Austin of "A Plea for Excuses" as distinct from the Austin of conventional speech act theory (Sadock, 2004; Searle, 1969), and the Wittgenstein of Ebersole (2001) and Baz (2012) as distinct from the Wittgenstein of mainstream analytic philosophy, whose name is synonymous with various influential theses about language and meaning (Pitcher, 1968).

sion-making may (or may not) be less exposed than Kalven and Zeisel's (1966) explanation of judge-jury disagreement, it was not his aim to criticise the Chicago Jury Project.

I have defended an alternative interpretation of ethnomethodology by means of comments on some these examples: (1) the beginner's view of philosophical position-taking, (2) a presumptive case of default entitlement in the teaching of jazz improvisation, (3) Brandom's response to McDowell on the occasion of the first session of a conference on Brandom's work, (4) the design of an extended matching questions test for purposes of summative assessment and (5) my response as a teacher of philosophy to a remark made by one of my students. These examples help us see that Winch's distinction between internal and external identity criteria is relative to a particular form of human activity. They also show that critical elimination of "formal analysis" may be nothing less than the elimination of an ethnomethodological subject matter. In the case of Brandom's pragmatism, the point of Garfinkel's basic initiative is not to criticise the project of *Making It Explicit*, but to show that some such phrase as "the project of *Making It Explicit*" is analytic gloss for indefinitely many ways of doing things. In the vicinity of example (2), there was something to criticise in the Brandomian analysis of a teacher's respond to a student: formal analysis of a documented action. In the vicinity of example (3), there was nothing criticise in Brandom's response to McDowell except that response itself. Winch's distinction falls away, leaving an unobstructed view of members' work: Brandom's response to McDowell.

Finally, I have offered a simple and demonstrable account of the relativity of Winch's distinction. Members subordinate interest in their own practical actions to interest in some particular problem. Ethnomethodology subordinates interest in the problem to interest in those same practical actions. In some cases, this will be to subordinate interest in Winch's distinction (e.g. Okrent, 1984; Bogen, 1999; Hutchinson, Read & Sharrock, 2008; Sharrock & Anderson, 1985; Taylor, 1982) to interest in the practical actions of people doing philosophy and/or sociology. Here as elsewhere, the point of ethnomethodological subordination (or respecification) is simply to understand.

CONCLUSION

Ethnomethodology and the Pragmatic View

The main body of this thesis fell into two parts. My aim in Chapters One and Two was to make an objection to Brandom's pragmatism. To this end, I began by introducing an analogy between Durkheimian sociology and analytic philosophy of language. Both are examples of formal analysis, i.e. analysis of constructed models of people's actual activities. Brandom has his own version of Durkheim's "first and most basic rule" of sociological method. He considers facts about a content-conferring practice as things: not, then, as practical achievements, but as attitude-transcendent constraints on any use of language. My aim in Chapters Three and Four was to clarify the place of criticism in ethnomethodological reflection on practices, using the following criticism as an example: in the light of Garfinkel's revisionary reading of Durkheim, Brandom's response to Wittgenstein's critique of philosophy turns out to be circular. But what of the "procedural policy" of ethnomethodological indifference? Were we meant to be using it to criticise examples of formal analysis? If not, what is the place of criticism in studies of members' work?

In the company of ethnomethodologists, I want to admit that I sometimes feel a "craving for generality" in response to ethnomethodological studies. It is not ultimately plausible to dismiss this feeling of dissatisfaction or insufficiency as a symptom of infatuation with science. It is elicited by talk of what is somehow recurrently there in numerous (perhaps quite varied) practical actions. Consider even such an intuitively straightforward case as, so to speak, the queuehood of queue. This is the craving: "Why, if there is not something common to all queues, do we use the same word for all?" It is hard not to be impressed by the idea of a mathematician's model of what we do, where it explains the typical length of a familiar queue.²¹ Even to speak of a familiar queue – not, then, of numerous historic queues

²¹ To give an example, I remember the almost daily predicament of having to decide whether to go to one side of a supermarket, to a single fewer-than-five-items checkout, or whether to walk all the way over to the other side, where the main checkouts were located. Was the queue long enough for it to be worth the risk of walk-

– is, or seems to be, implicitly to endorse a separability thesis, the thesis that there is something common to it and other instances. The point I have wanted to make is that this intellectual reflex – this craving for generality – must always come late. It can come only after we see what Wittgenstein invites us to stop and consider: a complicated network of overlapping and criss-crossing similarities. Formal analysis is a response elicited by a similarity. We are naturally inclined to ask *what* it is that A and B have in common. Ethnomethodology is not the study of *what* an account glosses over, but of ways in which an account has been intelligibly spoken. It does not aim to define, which would be formal analysis. Instead, it aims to see how definitions get their sense from uses of them. It makes a policy of the idea of semantic variety in uses of definitions (and, of course, in uses of accounts of other kinds).

Suppose, then, that ethnomethodology is already being done. For example, it is already being done by way of the question: “Just how did a teacher respond to the student?” It encounters a hindrance: a craving for generality. Ethnomethodology was already being done. But now its path is blocked by formal analysis. It is blocked, for example, by a Brandomian analysis of default entitlement. The teacher’s way of responding has been so defined, identified as an instance of this very general socio-semantic phenomenon. It *is* default entitlement. But conceding that it *is* default entitlement, we can still ask what is being glossed as such.

Ethnomethodological indifference is indifference to members’ practical concerns. In this case, it is indifference to the teacher’s concerns in teaching, which are now to be regarded, not as pressing practical matters, but as accounts of practical actions woven into those same practical actions. An obstruction has been placed in the way of the study, that of a Brandomian analysis of default entitlement. The ethnomethodologist, facing this obstruction, can hardly be indifferent to problems of formal analysis. An argument is needed to clear away the obstruction: not to motivate ethnomethodology at all, but simply to remove the obstruction. Call criticism of this kind “reparative” rather than “motivating” criticism, on analogy with conversation repair.

ing over to the other side? A mathematical model might have revealed the logic of variations of queue-length and helped me decide.

Baz's (2012) defence of ordinary language philosophy is reparative in this sense. It begins with a provisional reply to the standard charge against classic ordinary language philosophy, that ordinary language philosophy confuses meaning and use. The reply is indeed provisional, as Baz is careful to say. (He calls it an "initial characterization" [p. 21] of the conflict between ordinary language philosophy and its detractors.) In addition, Baz needs to give some examples of what ordinary language philosophy is good for. He goes on to consider some examples of historic uses of language, actual utterances that made little sense. These are not invented examples in which an imagined Jack and Jill spoke about an imagined situation. On the contrary, they are real examples of what philosophers have said about a philosophically troublesome word. Baz picks the topic of knowledge. He gives examples of ways in which things "can be wrong and go wrong" (Austin, 1974, p. 14) in philosophical talk. His aim is not to motivate ordinary language, but to reply to a circular charge and resume work.

A second charge may be made, also circular: the charge that we learn nothing about knowledge (or anything else) from ordinary language philosophy. Austin and Wittgenstein warn against theory-building in philosophy. It is not, as Brandom says of Wittgenstein in particular, that they leave theory-building to others, but that they acknowledge that models of meaning may be disparate. The point, then, is not to build a theory of knowledge (or anything else). So it is no criticism to say that we learn nothing about X from ordinary language philosophy, if by this is meant that no theory of X is forthcoming (no theory of knowledge, consciousness, meaning, know how, or anything else).

Then what *is* the point? My defence of ethnomethodology is just that it helps us understand what a person said or did. The critic of ethnomethodology demands something else: a theory of a very general social phenomenon. In philosophy, the critic of ordinary language philosophy demands a theory of meaning. Brandom is just such a critic. He separates two supposedly Wittgensteinian commitments: pragmatism about norms that govern linguistic performances (pp. 20–21) and theoretical quietism (pp. 29–30). He repudiates theoretical quietism but endorses pragmatism about performance-governing norms. Why does he repudiate theoretical quietism? In his Preface to *Making It Explicit*, he laments the fact Wittgenstein's writings "have discouraged his admirers from attempting to work out the details of a theory of meaning or, for that matter, of use" (pp. xii–iii). What do we learn

from Wittgenstein about the relationship between semantics and pragmatics? This is to ignore, or even deny, the motivating insights of ordinary language philosophy.

The requirement to construct a theory is an obstacle to studies of members' members. Ethnomethodology was already being done. But its path has now been blocked by a circular objection, according to which there is nothing to be learnt from these studies of language-games and lived work. How can we respond? One way is to rehearse the argument from the indeterminateness of imagined cases and intuitive findings. Travis (2008) provides a particularly elegant version of this response. He defends what he calls "the pragmatic view" (p. 87). Like Brandom, Travis concerns himself with the relationship (or with the idea of a relationship) between semantics and pragmatics. He begins by offering two pairs of alternative characterisations, which I shall simplify slightly:

Pragmatists:

- P1 Pragmatics is the study of linguistic phenomena left untreated by semantics.
- P2 Pragmatics is the study of properties of words that depend on the circumstances under which, or ways in which, they were spoken.

Semantics:

- S1 Semantics is (by definition) the study of relations between words and the world, especially those on which the truth or falsity of a sentence depends.
- S2 A workable theory of the semantics of language L would give the meaning of any sentence in L. Semantics is the project of constructing such a theory.

Semantics is standardly defined as the study of the literal meaning of a linguistic expression, and pragmatics as the study of its pragmatic enrichment or modification. On this view, semantics is an autonomous discipline, requiring no input from pragmatics, but leaving something over for pragmatics to explain. For example, the sentence 'Business is business' means what it does in English, that business is business, but may implicate something else on a particular occasion for speaking. The pragmatic view follows from a claim due to Austin (1961a), about what can properly be held true or false. Austin argues that questions of truth do not arise at the level of expressions considered on their own, but only at the level of historic uses of language. We properly hold true what a person said about a particu-

lar situation, not the sentence by means of which a person said it. And to decide what a person said, it is not enough to consider the sentence. We need to consider the circumstances of a particular occasion for speaking, and what was done with a sentence by means of which a person said something.

To illustrate, Travis considers a sentence containing no words with pragmatic properties in the sense of P2: 'The leaves are green.' (No indexicals, no ambiguous expressions or structures, no ellipsis on which the meaning of the sentence crucially depends.) He says what any speaker of English could say about the meaning of the sentence itself. The meaning of the sentence itself is that certain leaves were green at the time it was spoken. This is not to concede that there are literal meanings to be explained by a theory of the semantics of the English language. If there were literal meanings in the sense of S2, it would be possible to state conditions for the truth of the sentence up for analysis without considering a speaker's way of using it in the circumstances of a particular occasion for speaking. According to Travis, it is easy to see that it is not possible to do so. He invites the reader to consider the following example:

Pia's Japanese maple is full of russet leaves. Believing that green is the colour of leaves, she paints them. Returning, she reports, 'That's better. The leaves are green now.' She speaks the truth. A botanist friend then phones, seeking green leaves for a study of green-leaf chemistry. 'The leaves (on my tree) are green,' Pia says. 'You can have those.' But now Pia speaks falsehood. (p. 89)

There is no need for pragmatics in the sense P2. (No indexicals, no ambiguous expressions or structures, no ellipsis on which the meaning of the sentence crucially depends.) What Pia says by means of the sentence up for analysis is just that the leaves are green. But she says it truly of the same leaves on one occasion of speaking and falsely on another.

What do we learn from Travis' example? Suppose the critic were to object here that we learn nothing about the relationship between semantics and pragmatics. This objection would be circular. If semantics essentially concerns itself with truth conditions (S1), in terms of which it individuates and explains meanings (S2), then pragmatics (P2) just *is* semantics. This means two things. First, the idea of pragmatics on the standard view is wrong, since the idea of a literal meaning is wrong. In the case of Brandom's pragmatism, the error is to make a distinction between inferential practices of two kinds: semantic inferential practices (which explain literal meanings) and pragmatic inferential practices (which

explain enrichments or alterations of literal meanings). Second, a theory that provides conditions for the truth of a sentence may be illuminating on one occasion for speaking and unilluminating (even misleading) on another. Travis calls this phenomenon “semantic variety” (p. 89). It calls Brandom’s entire project into question. In light of the demonstrable fact of semantic variety (“don’t think, but look!”), what is point of describing a practice that suffices for content? What do we learn from *Making It Explicit*? Why does the issue of a content-conferring practice matter? The tables are turned.

Travis’ argument demonstrates, by way of illustration, the indeterminateness of a theory of meaning (S₂). In this way, it repairs a conversation badly affected by an intellectual reflex. It is not a motive for doing ethnomethodology (ordinary language philosophy), but simply a way of clearing an obstacle from the path for ethnomethodological research. The intellectual reflex is to abstract an expression (‘The leaves are green’) from historic uses of language (Pia’s first and second utterance of it) and ask for the meaning of the expression itself. Conversation about historic uses of language is badly affected by this reflex: by a requirement to ignore properties of the historic totality of an act of speech, with a view to explaining the meanings of specimen expressions.

What does Garfinkel add to any of this? In the case of Brandom’s pragmatism, he adds a revisionary reading of Durkheim’s aphorism. This reading applies to the project of *Making It Explicit* where Brandom relies on his own version of the basic principle of Durkheimian sociology, that of the objective reality of social facts. He makes a distinction between practices (performances) that exhibit a particular structure and the structure they exhibit. He abstracts the structure from historic uses of language and asks (1) whether it suffices for content and (2) whether speakers are necessarily able to produce it. Garfinkel helps us see this manoeuvre for what it is: the same bad intellectual reflex. Brandom recoils from the idea of semantic variety (Wittgenstein). Garfinkel embraces it. His advice here would be to (mis)read Brandom’s account of the practice up for analysis as the first segment of a *Lebenswelt* pair. To deny semantic variety (the meaning-constitutive embeddedness of an account of a practice in the doing of it) is to prevent an account of what we do from having any determinate sense. This is just what Travis’ argument shows. So if Brandom’s account ever did have some determinate sense, its having that sense must have involved an ad hoc

suspension of his own analytic rule. It is hard to find a better way of making this point. We need Garfinkel and innumerable clarifications of the ethnomethodological literature.

The reparative argument does not involve the contradiction of critical indifference to problems of formal analysis. For it is not an argument against members' practices, but only a response to the critic of ethnomethodological research. It does not require examination of the critic's own practices, except perhaps for the emphasis of an ironic failure on the critic's part to make them explicit. Nor do we require the reparative argument as a motive for ethnomethodology. The motive is ordinary human understanding, so badly affected by the intellectual reflex of formal analysis. Ethnomethodology was already being done, before the objection of a familiar rhetorical question. What do we learn from ethnomethodological studies? The reparative argument shows what is wrong with the objection that we learn nothing new about content, consciousness, rational action or even, come to think of it, methods members use to do some specific kind of work (astronomy, psychiatric consultation, jazz improvisation). It is a circular objection.

Have I subordinated ethnomethodology to a quarrel of contemporary philosophy of language? I think not. Ethnomethodology does help us see the circularity of Brandom's denial of semantic variety. But it was never the point of ethnomethodology to settle philosophical scores. Nor was it the point of ordinary language philosophy to motivate ethnomethodological research. The relationship is closer. At the level of motivating insights, there is no discernible difference between ethnomethodology and ordinary language philosophy.

One further objection may have occurred to the reader. I have relied on "historic" actions (compare Brandom's "actual" practices) to clarify the basic injunction of ethnomethodology. This injunction is similar in some ways to Husserl's, which Garfinkel uses to reread Durkheim:²² to return from the abstractions of formal analysis to the things themselves (practical actions). Garfinkel (1991) speaks of the "radical phenomena" of ethnomethodological studies, and understands their "objective reality" in terms of what allows

²² See e.g. Garfinkel, 2007, p. 16: "[We should] praise Durkheim and Husserl for having established sociology's distinctive study of social life. Each author worked to identify and fill a gap in the peer reviewed literature of sociology's distinctive phenomena of social order."

itself to be documented (observed and reported) on the occasion of the doing, *this* doing, of a practice. Consider his use of tense:

[T]he reported phenomena are only inspectably the case. They are unavailable to the arts of designing and interpreting definitions, metaphors, models, constructions, types, or ideals. [...] They were discovered. They are only discoverable and cannot be imagined. (p. 16)

The things of interest to ethnomethodology “were discovered” and “cannot be imagined” by the armchair theorist. I have presented this injunction as the conclusion of an argument of ordinary language philosophy. It follows from the indeterminateness of imagined cases and intuitive findings. It tells us to put away the things of formal analysis (definitions, metaphors, models, constructions, types, ideals) and produce ethnographies of member’s methods: careful descriptions of how, on particular occasions, people did produce, maintain and repair social order (practices). Ethnomethodological ethnographies are emphatically *not* imagined cases, designed to make a philosophical point. The reported phenomena were discovered, etc. In that case, how should we receive Travis’ story of Pia (not to mention Austin’s own examples of speech-acts or Wittgenstein’s descriptions of sometimes quite extraordinary language-games)?

Compare Bloor’s (1983) reading of Wittgenstein, where he demands that Wittgenstein’s invented scenarios should be replaced by real examples. According to Cerbone (1994), Bloor misses the point of a “grammatical” investigation. Imagined ethnographies suffice “as devices to aid in recovering the naturalness and familiarity of our concepts” (p. 159). They are gestures toward a limit of conceptual analysis, the limit of what we do. Wittgenstein’s aim is not to describe our actual practices, but show what kind of nonsense can result from formal-analytic abstraction. It is an error to read his examples as making do for ethnographies.

Does Cerbone’s criticism of Bloor apply to my reading of the relationship between ethnomethodology and ordinary language philosophy? Phil Hutchinson²³ has made an interesting suggestion about the role of invented scenarios in reflection on ethnomethodological phenomena (practices as ways of doing practices). The semantic theorist asks how an expression can be shown to mean what it does: asks, in other words, for a theory of the

²³ In personal communication.

meaning of some philosophically troublesome word, or for a theory of the semantics of a natural language. Invented scenarios show either (a) that the theorist's demands can only produce confusion, since they deny the naturalness and familiarity of our concepts, or (b) that the theorist's findings will always be iffy, since they ignore the circumstances of particular occasions for speaking. There is no need for ethnography here. However, we should see Garfinkel's breaching experiments as analogous with invented scenarios. They enact the limit of what we do, making it visible in and as the confusion of a demand for a self-standingly intelligible rule of conduct. Or they enact the indeterminateness of an account of a practice, which falls short of an account of any doing of the practice (the second segment of a *Lebenswelt* pair). These enactments allow Garfinkel to make a point that does not require reflection on real cases, a point he could have made by means of invented scenarios. However, if we want to understand what it means to say (or could mean to say) that a person did something, or what some expression means (or could mean), we do need to reflect on real cases. For that, we do require ethnomethodological ethnographies. This is not to read Wittgenstein or Travis as making do with invented scenarios, which might well be replaced by ethnographies. Cerbone's criticism of Bloor does not apply.

My own suggestion is to be careful in placing criticism of formal analysis in ethnomethodological reflection. Criticism can only occur at the point of an intellectual obstruction to ethnomethodological research. Ordinary language philosophy is ethnomethodology reflecting on the obstruction. Invented scenarios were never meant to settle questions of meaning. They serve to remind the reader of a limit of conceptual analysis (what we do) and the indeterminateness of formal analysis in philosophy, and by extension (Winch) in studies of social life.

To sum up. The motivating insights of ordinary language philosophy are ethnomethodology's own insights. This is to say neither (1) that ethnomethodology leans on ordinary language philosophy nor (2) that it places ordinary language philosophy in the broader content of a critique of formal analysis, but (3) that ethnomethodology and ordinary language philosophy, at the level of their own motivating insights, are essentially the same project.

What do we learn from these studies of historic uses of language? We learn how people produced a practice that "suffices for content" on particular occasions: what they did with

words in the context of a practice and what their words meant. The point is to understand our actual practices and see more clearly what these expressions of ours allow us to do. That is why the ethnomethodological issue matters: why it is better to resist temptations of formal analysis, including those of Brandom's pragmatism. There can be nothing on offer like a theory of order production. But if this is spoken as an objection, the ethnomethodologist can ask in reply whether understanding is not after all enough.

Appendix

I discuss the following extended matching questions test in Chapter Four. The questions for each “theme” are transcriptions of exchanges that took place in the classroom, redacted for purposes of the test. The answers are philosophical items drawn from course content.

Section A: Philosophical Isms

Theme A speaker is endorsing one of the isms a–g. Which ism?

- Answers**
1. empiricism
 2. rationalism
 3. methodological scepticism
 4. realism
 5. idealism
 6. relativism
 7. behaviourism

Answers may be used once, more than once, or not at all.

1. A Things like facts and like pieces of information, we can't really know as such without any sort of backup. But then **most backup is technically flawed.**
B And what kind of backup are you thinking?
A Like evidence, like empirical evidence, or you know various different arguments, deductive, inductive, that sort of thing. They're all flawed in their own particular way, which leads me to think that we can't really KNOW – like the word 'fact' doesn't really mean much.
2. Well knowledge is something that everybody seems to think that human's lack. So therefore, well not everybody, but a wide part of people say that knowledge is objective and it's based upon perception. So it's not a set thing. So **technically knowledge would be something that humans don't have,** which is defining knowledge by what it isn't. Due to my atheism nothing may have knowledge, but some may say that God does, or whatever God they believe in.
3. I don't think knowledge has to be sort of objective or infallible, because **knowledge is just based on what we can perceive from the environment around us,** so it's always going to be contextual. Because someone can say the [people of] the Amazonian rain forest would know things, that are very different from what WE know, but still it wouldn't be discounted because they think about it. So that's what knowledge is, just what's in your head.

4. A It's when I was a little boy, and I tried to get some cakes out of the oven and I burnt my hand, because I put my hand in the oven. And I got taken to hospital to cut the blisters out. And then two weeks later it healed and I did it again. Exactly the same thing. So that's probably when I learnt what causality is.
- B The second – it took you two shots?
- A **It did.**
5. A So you think the sun is going to rise tomorrow?
- B Yes.
- A Participant C, do you want to change your mind?
- C **Well I'm HOPING it rises tomorrow.**
- A OK. Participant A, Could you tell participant C why you think it will? Give him cause to believe, reason to believe.
- B Because it just does. It's just going to happen whether you like it or not.
- A And why but why do you believe that?
- B I don't know. If I say 'experience' then someone is going to moan.
5. A So you think the sun is going to rise tomorrow?
- B Yes.
- A Participant C, do you want to change your mind?
- C Well I'm HOPING it rises tomorrow.
- A OK. Participant A, Could you tell participant C why you think it will? Give him cause to believe, reason to believe.
- B **Because it just does. It's just going to happen whether you like it or not.**
- A And why but why do you believe that?
- B I don't know. If I say 'experience' then someone is going to moan.
7. A I think to have knowledge you have to have a belief. And I don't think a parrot can hold a belief. Like an animal can't hold a belief.
- B Why not?
- A **To hold a belief you have to have some kind of reason behind that belief, some kind of process of cognition.**
8. A He's only responding to the green, so I think it's quite possible that he may have knowledge of green, because he's experienced green, but everything else may be not green. There's no other colours to him: it's green or not green.
- B So it could just be mechanical?
- A **Yeah.**

9. He knows how to differentiate between words. Maybe like we were saying maybe not through the meaning of the words as in like, if you said like for instance to a dog like 'Good boy' he knows that the sound of what you're saying means reward, whereas if you say 'Bad dog' it, the sound, it's more of the sound more than the meaning. So with Alex's case maybe like saying the word 'Block' maybe like is obviously the sound of the word more than the actual meaning behind the word.
10. A Why don't we just say that Alex has acquired a conditioned response? Isn't that all we're saying, is that Alex ordinarily a parrot doesn't go 'Green' when it sees green, but in this case it does. Why? **Because it's been trained to do it.**
- B But through that conditioning has he thus gained knowledge?
11. A So the question is this. What more do we need than just having a learnt response to some thing to count as knowing something?
- B We have to have proof to prove that that thing is green. **WE can see it's green, so then it's an ELEMENT of proof.**
- A But I can tell you the iron is hot. The thermostat registers that it's hot and the light goes off. And then I touch the iron and I go, 'Actually yeah that IS hot, I'm not going to touch that again.' It IS hot. The thermostat has just been proved by an outside source to have knowledge.
12. Well I don't think it's anywhere near – I don't think it's possible to be sure of the mind, because hallucinations and stuff. That's all in the mind. It's not a perception: it's a perception within yourself; and if you can't be sure of your mind, then you can't be sure of anything.

Section B: Thinkers

Theme A speaker is expressing a view similar to one held by one of the thinkers a–e.

Which thinker?

- Answers**
- a. Descartes
 - b. Hume
 - c. Kant
 - d. Nietzsche
 - e. Freud

Answers may be used once, more than once, or not at all.

1. Our view of the external world is based on the impression we get from our senses; but since our senses can't always be trusted or even proven, the only thing we can truly rely on is the information that's going through our own mind about our thoughts.

2. There's also stuff like hypnotism, where a guy can hypnotise you to do something and you don't remember any of it. So how can you be sure of your own mind when you don't even remember it?
3. I think that knowledge is the fact how things work, the properties of things, the physical world based on our perception of reality. And will vary from person to person. But I think if we link knowledge and logic, and we think that they're one and the same, we can be logical and we can rationally draw conclusions from premises of what's real around us based on our perception. And that doesn't necessarily have to be wrong, or the opposite of knowledge. **It's a sort a construction over what we perceive.**
4. A When you're a baby and you cry and your mother gives you what you want, so you thereby learn that if you cry you get what you want.
- B OK, now listen to this. Listen. Do you, on an occasion like that, do you think that on an occasion like that the child learns the concept of causality, the idea of a cause and effect relation, in the same way that it learns what a duck is when the thing is pointed out?
- A You learn how these things relate to each other, so you learn that when you do something, something else happens. 'Cause and effect' and 'duck' are all just arbitrary words for these things. If you looked at a duck, then you'd learn what a duck is but you wouldn't know it was a duck. So **until you go to school and somebody teaches you the English language properly, then that is when you actually learn what cause and effect are.** But you learn what they actually are, as in terms of the world, when you experience them.
5. A We can analyse ourselves and why we feel the way we feel, and why we react in a certain way, more or less extensively. If we go deep into it, then we can try and understand our actions and our perceptions and how we analyse – how we PROCESS the information that we receive from sense data.
- B And entering into myself, what does that mean?
- A **Analysing.**
6. When he says that the self, if it existed, would be a simple and continued thing, and that he is also a bundle of perceptions, the flux and the movement, you can just be looking at it from one perspective, your perspective. By yourself, in his vision. You don't have to change just because all these perceptions and all this sense data is passing through you and you're feeling it. **You can look at it from the same point of view, from one person's point of view, and that can be a constant thing.** Your way of dealing with it can be a constant thing, and your way of processing. Because even the most stable people are the people who are most continued, continuous in this sense. They still have endless sense data moving through them.

7. Maybe there isn't a purpose. Maybe these things don't follow a logical pattern because we, what we WANT to interpret might not be there. And the fact that we try and define these things, maybe there IS no definition to be had. We're just looking at it from a human point of view.
8. A So do YOU believe that the sun will rise tomorrow?
 B **No, technically.**

Section C: Philosophical Principles

Theme A speaker is EITHER endorsing OR challenging one of the principles a–c.
 Which principle?

- Answers**
- a. We know the contents of own minds always, instantly and without having to do anything.
 - b. Our knowledge of the external world depends on our knowledge of the contents of our own minds.
 - c. We ought to be able to explain the meaning of a word in terms of what it refers to.

Answers may be used once, more than once, or not at all.

1. We can often be fooled by a stick in water. It appears to be different in water than how it actually is. Or even hallucinations: things aren't actually there when we think they are there. So we can only really carry on relying on what's in our mind instead of what's in the external world and around us.
2. There's also stuff like hypnotism, where a guy can hypnotise you to do something and you don't remember any of it. So how can you be sure of your own mind when you don't even remember it?
3. A When you're a baby and you cry and your mother gives you what you want, so you thereby learn that if you cry you get what you want.
 B OK, now listen to this. Listen. Do you, on an occasion like that, do you think that on an occasion like that the child learns the concept of causality, the idea of a cause and effect relation, in the same way that it learns what a duck is when the thing is pointed out?
 A You learn how these things relate to each other, so you learn that when you do something, something else happens. 'Cause and effect' and 'duck' are all just arbitrary words for these things. If you looked at a duck, then you'd learn what a duck is but you wouldn't know it was a duck. So until you go to school and somebody teaches you the English language properly, then that is when you actually learn what cause and effect are. But **you learn what they actually are, as in terms of the world, when you experience them.**

4. So anything your mind's kind of created, even if it can be proved false by your – you know, false perceptions; if it's there, then you can be personally sure of it, because your mind's created it, and it's the only thing you've got to kind of assure yourself of anything.
5. I think it's not just that you can't perceive what the actual concept [of causality] is, but it's like with the duck thing – the first time you were told this is a duck you may not necessarily remember that this is a duck. After being told it multiple times **you'll kind of see what all of these things that you've been told is a duck have in common, and compile that as what a duck is, almost like forming a schema**. So with the idea of cause and effect, gradually as you go through the years from about say being born to the age of ten, you're going to see if this happens, this happens. And eventually figure out that these things being connected like that is a relationship that IS cause and effect.
6. Six is saying that the self would be a simple and continued thing. Seven is saying that I am a bundle of perceptions in a perpetual flux and movement. They seem contradictory, unless there's a difference between what he's identifying as the self and what HE is. The difference between self and 'I'. **Unless he's saying 'I' as his physical body and his self as something else**.
7. When he says that the self, if it existed, would be a simple and continued thing, and that he is also a bundle of perceptions, the flux and the movement, you can just be looking at it from one perspective, your perspective. By yourself, in his vision. You don't have to change just because all these perceptions and all this sense data is passing through you and you're feeling it. **You can look at it from the same point of view, from one person's point of view, and that can be a constant thing**. Your way of dealing with it can be a constant thing, and your way of processing. Because even the most stable people are the people who are most continued, continuous in this sense. They still have endless sense data moving through them.
8. A Why do you trust those laws [of nature]?
 B Because it's happens for however many years, and it wasn't, I mean it was still happening even before somebody had said, 'THIS is the law whatever'. **It was still happening**. It doesn't need to be given a title and . . .
 A OK.

Section D: Maxims for Doing Philosophy

Theme A speaker is violating one of the maxims a–d. Which maxim?

- Answers**
- a. Avoid vague and ambiguous expressions.
 - b. Never take a crucial assumption for granted.
 - c. Do not change the subject.
 - d. Do not introduce irrelevant considerations.

Answers may be used once, more than once, or not at all.

1. A If someone is hallucinating, they're not perceiving what they think they're perceiving.
B People with auditory hallucinations and stuff are quite common, aren't they? But they're still perceiving it, they still know that they can hear it.
C And they can still be sure in their mind of what they're experiencing, whether it's true or false – **the argument wasn't being sure of reality, the argument was not being sure of what's in your mind.**

2. I think the point was that you can't be sure of anything in the external world apart from what's in your mind. And so if you see a green dragon on a tower, then you're not sure of it in the same way that you're not sure of the fact that there's a tower there in the first place. **But the fact is that you have an emotional reaction to that.** You see it, you perceive it, you think about it. Those are the things that are concrete.

3. Rather than proving to anyone else that you saw a green dragon. **It was about the validity of your mind not what goes on inside it.**

4. A Well **empiricists base the world on empirical evidence,** or what you can see.
B They base the world?

5. It's kind of like, when no one's at the forest and the tree falls down, **did it really** collapse and whatnot. I didn't see it, **so I don't know** if it did get chopped off or if it just happened by itself.

6. A What's wrong with saying that we know from experience that the sun is going to rise again tomorrow?
B It's not the only reason. It depends what you mean by experience, but what the sun is, what it's made of and our relationship to it mean it's going to appear to rise tomorrow.
A Why do you think so?
B Because it's a giant ball of plasma. It's not going to vanish.
A But why do you think it's not going to vanish.
B Because the only reason it could do is if another universe comes through ours, which COULD happen, but is so incredibly unlikely that theoretically . . .
A And what does 'unlikely' mean here? How do you judge how likely something is?
B **It hasn't happened yet.**

7. A Why do you think parrots don't, cannot have beliefs?
B Because I think you have reason to hold a belief. I don't think a parrot has a reason.
A You don't think a parrot has reason?
B No.

- A Why not?
- B Hmm. Well, I was going to say because of language. **But then it can speak, can't it?**
So then I'm confusing myself.
8. A I was just thinking that as you said if it's routine then maybe it happens the same time every day and [Alex the parrot] goes through the charade of it, but also he asks for water.
- B But when she offered him water he didn't want it.
- A That's the thing. I was going to say. He asks for the water, he gets given the water.
- B DID he ask for water?
- A **He SAID 'water'.**
9. The internal is personality, sense of self, choice, perspective, versus external. But it's not necessarily about being FORCED to do something you don't want to do. **Sometimes you can be encouraged to change.** And my partner said it could be a mix of both as well.
10. A So the question is this. What more do we need than just having a learnt response to some thing to count as knowing something?
- B We have to have proof to prove that that thing is green. WE can see it's green, so then it's an ELEMENT of proof.
- A But I can tell you the iron is hot. The thermostat registers that it's hot and the light goes off. And then I touch the iron and I go, 'Actually yeah that IS hot, I'm not going to touch that again.' It IS hot. **The thermostat has just been proved by an outside source to have knowledge.**

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