

# The Gentle Seer: The Unappreciated Prescience of Egon Bittner's 'Objectivity and Realism in Sociology'

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Jesus said unto them, A Prophet is not without honour, save in his own country, and in his own house.

(Matthew 13:57)

## INTRODUCTION

If the hallmark of a great paper is that whenever you return to it, it speaks directly and wisely to current concerns, then Egon Bittner's 'Objectivity and Realism in Sociology' is a truly great paper. In a few brief but perceptive paragraphs, Bittner predicts the course of the very contemporary (and still unresolved) debate over the need for reflexivity in ethnographic field research and diagnosed the sources of much wider ranging confusions which have arisen from treating the polarity of 'objective' and 'subjective' as constituting an essential problematic for sociology. In particular, he points to the way in which the adoption of a certain attitude to 'qualitative method', one he calls an 'abortive phenomenology' (Bittner 1973: 123), could well have as distorting an effect on the understanding of social life as does an acceptance of positivist doctrines/dogmas. This attitude will, he suggests, culminate in laudable and brave attempts to forge new methods based on a rejection of positivism. But, as he acutely observes:

Pluck is a virtue, especially in this case, but it does not set all things right.

(Bittner 1973: 118)

Bittner suggests that the rejection of the aspiration to 'objectivist', 'view from nowhere' descriptions grounded in the operationalisation of meaning and the

proceduralising of formal reason, ideas which were in place and tending toward dominance in American sociology in the immediate post-Second World War period, will lead to another, equally elusive and illusory aspiration, namely that of authentic descriptions grounded in subjective experience. In this case, the key misconception rests (at least partly) on the assumption that it is necessary to replace objectivity with subjectivity. A second element, deriving from forcing the issue of choosing between quantitative or qualitative methods would lead to the consequence that the 'subjectivity' that would become prized would actually be that of the sociological researcher.

It was the search for 'authenticity' which was the primary requirement of the proposed shift to the subjective, where authenticity was assured by immersion in and engagement with the setting under view. The primary aim, therefore, was the presentation of social reality as seen from within rather than from without. In Bittner's view, this would become an objective which, in practice, would be frustrated by the fact that the sociological researcher would enter the field bearing a burden of preconceptions drawn from Sociology. The end result would inevitably be that setting up a substantial disparity between the experience of inhabitants of the social setting and that of the visiting researcher would become integral to the methodology's practice. In this way, the search for authenticity, though inspired by Phenomenology, would become a distortion, or even abortion, of the phenomenological project.

For Bittner, then, rushing to embrace the fullest form of subjectivity is likely only to bring its own troubles. First, there is the risk that what will dominate investigative interests are the enthusiasms and/or preconceptions of the investigator. Even if this is avoided, the desire to present an account of reality from the point of view of the actor must 'return' as Bittner puts it to an 'objectiveness' but one that this time is grounded in intuitions gained through 'being there'. But this warrant, this being there, can only come at a cost.

The greater the effort to enhance the adequacy of observation on counts such as acceptance, transfer of trust, subtlety, perspicacity, open-mindedness, patience and scope, the less likely that serious, searching questions will be asked about that which has come to view by means of all this loving care.....It is not whether he observes well or poorly that matters but the circumstance of his being an outside observer with all the consequences issuing from it.

(Bittner 1973: 119)

## THE REFLEXIVE CONUNDRUM

Although, since ‘Objectivity and Realism’ first appeared, its insights have been applicable to many different controversies, we think they are particularly telling in regard to the ongoing struggle that many who practice Qualitative Sociology are having with the issue of ‘reflexivity’, for reflexivity is itself just one kind of misconstrual of what a turn to the subjective might entail. As Michael Lynch (2000) has astutely catalogued, reflexivity refers to a variety of ideas, not all of them equally muddled. However some do make the mistake that Bittner points to, namely of assuming that because of the intervention of the sociologist as an observer of the social setting and the social and cultural distance between the sociological observer and the members of the society under study, reflection on the researcher’s own experience must be a central and critical concern when understanding the social life of some setting. To use the image that is most often deployed when explaining why this must be so, without an understanding of the lens through which the observations have been gathered, there is no possibility, or so it is asserted, of compensating for any partiality or distortion of the sociality under view.

This mistake underlies the conundrum of how the researcher is to offer an analysis which both respects the view of social reality as seen from within and is recognisably and properly sociological? How can you be both inside and outside at the same time? How can you capture and represent *their* interpretations within the framework of *your* interpretation? How do you treat their point of view with respect without sliding into cultural relativism or an interminable regression? Thrashing back and forth, looking for footholds, handholds, pathways out of what Finlay calls the ‘marshy swamp’ (the latest efforts to do this being visual and sensory ethnographies) has served only to cause us to sink further.

It is the task of each researcher, based on their research aims, values and the logic of the methodology involved, to decide how best to exploit the reflexive potential of their research. Each researcher will choose their path – a perilous path, one which will inevitably involve navigating both pleasures and hazards of the marshy swamp. For all the difficulties inherent in the task, to avoid reflexive analysis altogether is likely to compromise the research. The swamp beast still needs to be confronted as MacMillan’s (1996: 15) reflexive poem captures so eloquently:

Reflexivity, like hypnotherapy, has various levels.  
Some dabble near the surface,  
dipping into reflexive moments, flirting with the images evoked in the reflection,  
before returning to the safety of the mundane.

Others attempt to confront the fear of the monster lurking in the abyss  
 by descending into the deeper realms of reflexivity. It is those who confront the  
 beast  
 who will truly know what is there, in the dark beyond...

(Finlay 2002: 227)

Bittner acutely foresaw that attempts in the name of subjectivity-as-experience to correct positivism's misrepresentation of society would induce comparable, though substantively different, distortions. Positivist objectivism sought to access social reality through faithfulness to methods designed to depersonalise inquiry. The inversion of that position envisages access to social reality through the personalising of inquiry, through faithfulness to the subject. Neither approach encapsulated what Bittner considered the genuine, phenomenologically appropriate orientation of *faithfulness to the object*, which in this instance would be to social reality as experienced from its midst. For Bittner, 'objective' and 'subjective' are not to be counterposed and polarised. Therefore we are not forced to choose one or the other. Rather, the challenge is to achieve greater clarity about their relationship – that is, the proclaimed objectivity of social reality as it is present in social settings and intelligible to us, that is those who inhabit those settings. The aim of inquiry conducted in this way is not to seek to persuade anyone that social reality is really only the subject's motile artefact any more than it is to demonstrate that determination of the real structures of social life is obstructed by layers of subjective and misconstrual. The need, as Bittner saw it, was to do justice to the patent *and overwhelmingly unquestioned* objectivity that social structures have in our daily lives. In this, Bittner was drawing upon Schütz' characterisation of the natural attitude in everyday life

By the everyday world is to be understood that province of reality which the wide awake and normal adult simply takes for granted in the attitude of common-sense. By this taken-for-grantedness, we designate everything which we experience as unquestionable; every state of affairs is for us unproblematic until further notice.

(Schütz 1973: 3-4)

## THE OBJECTIVISM OF REFLEXIVITY

As we have noted, Bittner is clear that Phenomenology was the inspiration behind the turn to the subjective. However, the elaboration of what this entails has led to the outcome mentioned earlier. The phenomenological project was built on the assumption that the life world, the world of everyday social life, is available to

observation and understanding *prior to* the production of any scientific scheme for its further examination. Moreover, phenomenological inquiry is needed for the clarification required as a propaedeutic to the ‘understanding’ of social reality through the adoption of some apparatus of scientific (or at least theoretical and methodological) principles. Thus, amongst other things, it would set standards which fix what ‘understanding’ is to be. Phenomenological investigation is, then, *prior to* the understanding of social life by means of the adoption of and operationalisation of some set of methods and/or principles. The risk for any objectivist approach is that, without such secure determination of correct standards and ways of understanding, social reality will remain unknown. For phenomenology, on the other hand, the risk is that, should the form that social inquiry must take be stipulated *a priori*, the social reality that is the site and setting of the inquirer’s own inquiries might itself be *lost sight of*. An approach to the study of social life chosen *a priori*, might simply fail to recognise that the understanding of social reality is ever present in social settings and available to those resident there. It is this understanding that the affairs of everyday social life actually run. As the struggles with it make abundantly clear, the conundrum of reflexivity confronts us only because of the assumption that the social researcher is seeking a special, primordial understanding of social reality. Bittner diagnosed the importance and consequences of this assumption well before reflexivity became the *topic du jour* that it has now become. We can see just how deep it runs and what its consequences are by looking at possibly the most influential source of and contribution to the debate over reflexivity, namely Latour and Woolgar’s *Laboratory Life* (1979).

Harold Garfinkel recommended that sociologists should treat social life as ‘anthropologically strange’. What he meant by this was that they should *pay the same attention* to things which sociologists are hugely familiar with, by virtue of belonging to their own society, as is paid by anthropologists to the ways of some society which are strange both to us and, initially, the anthropologist too. The Latour and Woolgar approach is entirely different. For them, the adoption of the ‘sociological standpoint’ is an analytic sleight of hand whereby familiar things *are made to seem strange*; a view which is a long way from the notion of faithfulness to participant experience, which is itself an attempt to remain faithful to the object of experience. The environment to which Latour and Woolgar apply their *legerdemain* is not one, however, which is familiar to either the sociological researchers themselves or their readers. It is already a strange world, at least to most of Latour and Woolgar’s readers, that of a laboratory working at the cutting edge of its discipline. This world is not strange in the sense that what is going on is bizarre or, if presented in explicated detail, seems to lack intelligibility. It is strange in the sense that it is not a world where the sociological researchers or

their readership can be at home. It is not a world where things are familiar to them and they could say with respect to innumerable particulars of the site precisely what is going on there. At the same time, of course, it *is* a world which is familiar to those who are there and leading the laboratory life, one in which (at least *presumptively* for those who are strange to it) the parties are at home, familiar with the endless detail of the scene and their own and – by extension – others' doings.

Because they remain attached to the idea of sociology as a methodologically distinctive pursuit, Latour and Woolgar focus on the disparity between, on the one hand, themselves and their readership and, on the other, those whose working world is this laboratory. Overcoming this disparity, they suppose, must be an epistemological problem. What methods are to be used to know this world? The methods chosen are precisely those which distance the understanding gained by the sociologists from that of those living within the world. They are ones which construe their doings in ways that foreground an alternative understanding to the one which would be required to live and work in the world of that laboratory. It is not an election which is made by way of a point for point comparison of the laboratory doers' understanding of the work in hand with the sociologists' purportedly alternative understanding.

The disparity between Latour and Woolgar and those living the laboratory life is not really an epistemological gap but an organisational one. An understanding of what is going on in the laboratory is available since it is manifest in even the most detailed specifics of activity to be found there. However, this understanding is only accessible to someone equipped with the competences to live the life of the laboratory. Understanding what is going on in that setting cannot be just a matter of the standard sociological strategy of being on the scene and witnessing the events; legitimation through 'being there'. The sociological competences do not include those that are commonplace in the laboratory and make up the likely preconditions for understanding of what is being done before the sociologist's eyes. Often enough, it is the researcher's challenge to see that anything is being done at all. As we have seen, Bittner is quite clear about this (and it is a view he shares with Garfinkel). In the laboratory as in any other social setting, the problematic issue is not how social reality is to be disclosed to the sociological researcher, but how to pay attention to the way in which it is already disclosed as an objective setting of conduct to those who encounter it as such; those for whom it comprises the most ordinary conceivable matters. It is not, of course, the case that sociologists turning up and hanging around with working scientists can't come to understand anything about the initially opaque scientific stuff going on in the laboratory. With patience (exercised as much by those playing host as by the visiting sociologists) simple observation can make some features of laboratory

work accessible to the latter and this can, in turn, be presented as a description of laboratory life. The contrast between an understanding of social life as accessible to competences, however, is in stark contrast with the idea that it is available to specialised methods such as that of making it ‘anthropologically strange’.

What Bittner’s approach makes us sensitive to is the character of the understanding that results from such methods. In an important sense, it is a dependent and delimited understanding. It is not the sociologist’s autonomous understanding of what this or that instrument is doing but an understanding of the instrument via what has been told to the sociologist by the working scientists. It is dependent because the sociologist comes by it through dependence on what the scientists understand. In the Latour and Woolgar approach, furthermore, this dependent understanding gets distorted and obscured in their attempts to articulate the relationship between the object which they are studying and their sociological position. In addition, such understanding is not only a dependent one but, compared to the working scientist’s understanding, a highly delimited one. The scientist’s understanding enables a whole range and diversity of activity within the site as part of the minute by minute and day by day production of a fluently continuous round of situationally cogent conduct. The sociologist’s understanding is the basis for hardly anything more than a very circumscribed emulation of a few aspects of the working scientist’s performance.

The mode of investigation advocated by Latour and Woolgar leads, therefore, to an irony; but it is one that is different to their usual ironical take on the sociological analysis of social life. The project to create a Sociology of Science was originally intended to reflect that the view Sociology is a science and the Sociology of Science was the realisation of a *science of science*. The idea that Sociology is a science (at least in anything other than a very trivial way) is, of course, not so widely held thirty to forty years after the new sociology of science was initiated. The idea that sociology has an independent form of understanding of science, however, remains strong. It is a surprising fact well worth reflecting on, then, that after more than four decades of well funded effort by a significant cohort of investigators, the Sociology of Science still understands very little about the science that scientists do (though in a way its announced ambition to show that the content of science was ‘socially determined’ was a misdirection for the actual project, which was to challenge science’s place in society). To the extent that it concerns itself with the content of science the title ‘Sociology of Science’ should, perhaps, be understood as meaning something like ‘science for sociologists’ (rather akin to the *Science for Dummies* series); a genre in which sociological reports offer understandings of fragments of scientific life shaped in forms easily recognisable to and digestible by sociologists, rich food for their prejudices about ‘science’.

Whilst Bittner was offering a version of ethnomethodology as a non-abortive phenomenology, with the task of descriptively exploring the forms in which social settings are, from within, found to be objective, Melvin Pollner was soliciting a very different understanding, the irony being Pollner's was pretty much the conception that Bittner aimed to dissociate from, affirming the rights of subjectivity against the social order's pretensions to objectivity. The apparent objectivity of social scenes is a product only of subjective conviction – in effect social order is being produced to preserve that subjective conviction. What Bittner was urging with respect to the sociological task was much greater diligence in determining in what senses social settings are objective for those within them, whereas Pollner begins with an *a priori* conception of everyday experience (or, as he calls it, mundane reason) marbled with a strikingly foundationalist streak. For Pollner, it is a given that the world as experienced is a product of a naive sensibility – naive in the derogatory sense, that is. He sees a counterposition between applications of ethnomethodology's procedural stipulation that activities be viewed as accomplishments, on the one hand, and applications of the practices of mundane reason on the other. The former is not a means of opening up the mundane reason for more sensitive and searching exploration but rather is a basis for confronting and challenging it. The idea that activities are 'accomplishments' is purportedly at odds with mundane understanding of them. This reproduces the very conventional sociological disparity between the way in which social affairs appear to those dwelling within them and the way in which they really are (as this reveals itself to the distanced standpoint of the sociologist). The disparity is essential to Pollner because the two understandings – the social world as objective and the social world as accomplished – are incongruous in ways that imply that recognition of their accomplished character erodes the sense that those very accomplishments produce – matters of fact are not genuine facts for they are *only* accomplishments. At least, it would be subversive were it not for mundane reason's self-preserving properties, ones which (like the secondary elaborations in Evans-Pritchard's Azande story) inhibit recognition of mundane reason's 'failures'.<sup>1</sup> Hence Pollner's insistence that, when wielded by ethnomethodology, reflexivity has radical potential, revealing the shakiness of the foundations upon which mundane experience rests (see what we mean about the residual but heavy dose of foundationalism). As Pollner recognises, social construction-style versions of ethnomethodology are not going to shake up worldly practices anywhere (except in sociology, perhaps) so that the actual radicalism that Pollner sees is a matter of pressing at the limits of reason, an exercise whose futility is assured by the very reflexivity it advocates, for any version of social-life-as-really-only-an-accomplishment is, as Pollner also acknowledges, a fair recipient of a *tu quoque*. What else is there for anything to be? Which is, of course, one reason for withdrawing the



‘only’ rider from ‘accomplishment’ at the very start. Pollner is haunted by nostalgia for a transcendent rationality, one that is elusive and almost subliminal, for it is only when measured against the requirements of that quite traditional (mis)conception that the ways of mundane reason appeared ‘flawed’. Otherwise, they are just the ways of mundane reason.

Pollner’s reiteration of the call for ‘radical reflexivity’ was motivated by ethnomethodology’s settling for a place in the suburbs of sociology, moderating, if not abandoning, what sometimes seemed to be a totalistic hostility to (all) other forms of sociology, only to turn inward, effectively accommodating the to institutional and intellectual structures of the ongoing wider discipline. Is the idea that ethnomethodology is now more settled than it once was only an optical illusion? It would certainly be false to say that ethnomethodology has been robustly included in the intellectual life of sociology at large, despite occasional gestures from both ‘sides’ toward alignments with the preoccupations of other sociologies. Since, on the whole, ethnomethodology is only very dimly understood outside its own sociological circles, it was and remains unlikely that any very dependable links are going to be forged in respect of what, after all, Garfinkel insisted were ‘study policies’, quite unlike the usual clutter of theory and method that frames sociological thought. Sociology *has* (of course) changed enormously since ethnomethodology first appeared, and many sociologists have given up the litany of logical-empiricist precepts that *still* hold sway in some areas of the discipline, but very few of those changes have involved moves to positions less retrograde than the logical-empiricism left behind (it’s worth remembering the animosity that ethnomethodologists sometimes manifest toward conversation analysis) – there is little interest anywhere in topicalising social order as practically enacted. Whilst many of ethnomethodology’s themes were shaped by the then contemporary situation, rather dominated by logico-empiricist doctrines and attempts at their realisation in the social studies, those themes have by no means been outdated by the dwindling, but persistent, influence of the logical-empiricist constituency. As Michael Lynch (*inter alia*, 2000) has repeatedly shown, for the field of social studies of science, which is animated primarily by hostility to the logical-empiricist programme, this does not serve to bring other positions in that area any closer to ethnomethodology. Viewed from ethnomethodology, the situation of ‘the international social science movement’ (as Garfinkel was apt to call it) is not much changed from that which Bittner identified in another of his outstanding papers, 1965’s ‘The Concept of Organization’:

In general, there is nothing wrong with borrowing a common-sense concept for the purposes of sociological inquiry. Up to a certain point it is, indeed, unavoidable. The warrant for this procedure is the sociologist’s interest in exploring the

common-sense perspective. The point at which the use of common-sense concepts becomes a transgression is where such concepts are expected to do the analytical work of theoretical concepts. When the actor is treated as a permanent auxiliary to the enterprise of sociological inquiry at the same time that he is the object of its inquiry, there arise ambiguities that defy clarification.

(Bittner 1974: 70)

Bittner here states what we have always taken to be the central element of ethnomethodology's critical relation with sociology, namely that it does not much examine, let alone clarify, the relationship between the common sense understandings (in another way, the mastery of natural language) upon which it essentially depends and the proprietorial structures of theory and method that it deploys. This is not necessarily a disabling objection, one which obstructs the capacity of sociological researchers to get on with, and to their satisfaction, conclude their work. It is, rather, another form of the opposition to exceptionalism, one which remains valid despite the widespread enthusiasm – perhaps now dissipating – for reflexivity, remarking the fact that sociological theorising does not make explicit provision, within its own schemes, for the vernacular resources upon which its practical intelligibility depends.

There are no substantial grounds for arguing either that ethnomethodology has been comfortably included within contemporary sociological thought (as opposed to being institutionally peripheralised – which it has been, to the extent that is often more warmly welcomed outside of sociology), nor for arguing that it has betrayed its critical potential by being, itself, less confrontational with other forms of sociology.

There are (at least) two ways of thinking about ethnomethodology's potential in relation to sociology. One, to think of it as having a mission to challenge other forms of sociology with the, to them, (putatively) embarrassing fact of their reflexive production in the hope of subverting and destabilising such sociologies (or, alternatively, in the hope of helping design fixes for them). The other certainly understands that sociological *and ethnomethodological* investigations together with their outputs are accomplishments, but this is an unavoidable fact about them. Garfinkel himself averred that ethnomethodology does not involve making things out to be better or worse than they are usually cracked up to be. There are greater complexities to ethnomethodology's relations with sociology not explored here, but one thing ought to be plain, in those terms, sociological investigations, including ethnomethodology's, are 'more of the same' in respect of the practical reasoning that gets done, themselves further episodes in the production/reproduction of social order. One can, therefore, include studies of sociologists at work, including ethnomethodologists themselves, within the ensemble of ethnomethod-

ology's studies, can equally study professional sociologists at work or ethnomethodologists' own practical reasoning as investigate what observatory astronomers, birdwatchers, office accounts keepers or the receptionists in psychiatric clinics are doing. Harvey Sacks' slogan 'order at all points' highlights the way that social order can be inspected from anywhere in social life, and to that extent *it really does not matter* what one elects to investigate – no special merit attaches to making 'reflexive studies' of sociology's or ethnomethodology's investigative practices since those do not exhibit 'reflexivity' in ethnomethodology's sense any more prominently, perspicuously, or interestingly (or any less either) than the doings of astronomers, birdwatchers and bookkeepers do. We stress that no demerit attends them either, for, to repeat, it really does not matter what the topical stuff is. From this point of view, then, the relationship to the rest of sociology has no special standing amongst ethnomethodology's investigative interests.

In 'Objectivity and Realism in Sociology', Egon Bittner warns against heeding the siren calls for authenticity and realism as a justification for a turn to subjectivity. He predicts that this will only lead to the substitution of the analysis of sociological understanding of any social setting for common-sense understandings in that setting. Over the past quarter of a century, precisely this has happened as, in pursuit of the subjective, versions of the Latour and Woolgar operationalisation of the injunction to make social life anthropologically strange have been made central to the investigative and analytic techniques promulgated by much of Qualitative Sociology. In turn, these have led to many researchers being transfixed by the conundrum of reflexivity.

## NOTES

1. There's surely something wrong with the idea that it is 'mundane reason' that is on trial in the traffic courtroom or the psychiatrist's office, one that perhaps originates in the transformation of Schütz's idea that mundane reason presupposes (until further notice) a world known in common into the idea that it is a task of mundane reason to preserve that assumption. Schütz's further specification of the assumption, that it holds provided biographical differences can be set at zero, defuses the idea that perceptual discrepancies are at odds with it.

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