

Organisation and Workplace Studies: Talking About the Work of Egon Bittner¹

Rod Watson
Télécom ParisTech

Maria Wowk
Manchester Metropolitan University

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MW: When I read ‘The Concept of Organization’ in Roy Turner’s collection,² that was where I first came across him and I don’t think I ever realised at the time how unusual it was for that text to be in the collection.

RW: Bittner was a very classical phenomenologist in the sense that he was a Schützian. Now I know that Schütz wasn’t entirely classical but Bittner was very Schützian and I think in that paper he made one very big Schützian point that when we are talking about organisations we ultimately don’t have a theorised vocabulary to analyse that organisation; in fact the organisation is something that presents itself to the workers and incumbents, and their conceptions and their language for their organisational work is what we should be focusing on, and looking at it in a much more specific way by looking at how they perform. He was into how language was used in particular scenes of action within the organisation and I think that is something that of course has massive Schützian relevance.

MW: It does but it, it’s a bit strange that he uses those terms, the ‘terms and determinations’. They don’t come from ethnomethodology.

RW: No they don’t. I think they espouse something like an ethnomethodological mentality. Of course it was written in the early Sixties published in 1965, so it’s a long time ago, and I wouldn’t have thought that the vocabulary of ethnomethodology was that established back then. You look at Garfinkel’s ‘Trust’ paper,³ it uses concepts that never appeared again in ethnomethod-

ology and that was 1963. So I suppose it was just symptomatic of its time. 'The Concept of Organization' was originally published in a journal called *Social Research*⁴ which was the house social science journal or sociology journal of the New School for Social Research, which welcomed Jewish émigrés from the Frankfurt School because of Nazism, and it developed a very specific view of what social science was – it was largely either hermeneutic or phenomenological. He certainly had associations with people there. Remember that Garfinkel was at Harvard for a while and had links with Aron Gurwitsch and Alfred Schütz, so he had his links with the New School too, where Schütz was.

MW: Oh was he?

RW: Yes Schütz was there, he had had to flee from Austria and that's the style of work that Bittner had, the sort of work that was pursued at the New School for Social Research, and er that would probably in my opinion have been the only journal in organisational sociology that would have touched that article with a bargepole.

MW: And so if Bittner was asked to contribute to a book called *Ethnomethodology* is it clear that he would have known what that was all about then?

RW: Oh definitely yes. I think that at that time he would have regarded his work as having been at least ethnomethodologically-informed and probably simply ethnomethodological insofar as the term had been invented then, or at least it might have been invented but insofar as the term was current as a characterisation of that sort of rather broad variety of approaches that we call ethnomethodology.

MW: And Garfinkel was a supervisor on Bittner's Ph.D.?

RW: He was something like a second supervisor. Cressey I believe was the main one.

MW: Is there a difference between something being ethnomethodology and being ethnomethodological?

RW: Well that's very interesting because I think that his work is actually both. Some of his work to me is simply ethnomethodological (or of the ethnographic type) pure and simple. Other parts of his work are indeed eth-

nomethodologically-informed in my opinion, and my view of Bittner is that people see his work as being more fragmented than it actually is. For instance he wrote a paper called something like ‘The Structure of Psychiatric Influence’³ which is very much to do with his thesis.⁶

MW: OK

RW: and he was talking about Eighteenth Century America doing what we would call constructive-analytic, making constructive-analytic observations about (law), that in Eighteenth Century America a new profession really emerged that was the legal profession, taking over from the clerical profession. And he said that in that time the law became ubiquitous and autonomous in America. In other words it’s a bit like the stereotype of ‘health and safety’ today, it informs everything we do and that people began to think of things in legal terms in every area – family and all this sort of thing – and of course this affected psychiatry too he said, or what we now call psychiatry. And it looks like this is more of a cultural studies approach than an ethnomethodological one saying that kind of thing

MW: Yes

RW: but then you begin to see what he defines as the law developing this autonomy and ubiquity, and he says that he considers the law as having become detached from what he calls an interpretive framework that links it to particular situations and he calls this a sort of background framework. And he begins to define what he means by the autonomy of the law and the autonomy of the legal profession in that kind of sense. So even there there’s an ethnomethodological characterisation of sorts of the autonomy of the law. So I think even this very constructive analysis statement still has that ethnomethodological dimension.

MW: But which came first

RW: I think that the Schützian approach is the key to understanding everything about Bittner really. I think the Schützian stuff is what came first – I don’t know if it came first historically, biographically. I think it probably did. But it certainly had primacy in the way that he thought about things. So even the most constructive-analytic things you can think of in his statements still have to me that ethnomethodological dimension. And I think that thing about the Eighteenth Century switchover is a rather constructive-analytic

point it seems but yet he conceives of it in terms of an interpretive framework

MW: Yes

RW: Now I know that these days the term ‘interpretive’ is problematic in ethnomethodology but in those days it wasn’t so much, but you can see that that’s the kind of thing that informs even his seemingly constructive-analytic work. So in that respect I wouldn’t say that statement is ethnomethodological, it’s just ethnomethodologically-informed. But I would say that, especially in the terms of that time, ‘The Concept of Organization’ is an ethnomethodological piece as is his work on the police.

MW: oh right. Let me push that a little further because (and this might be related to the development of ethnomethodology and the stage it was at then), what’s the bit that makes it not – beyond phenomenology – but ethnomethodology?

RW: OK. I think at that time ethnomethodology was very, very Schützian, it emerged out of the Schützian argument – out of Garfinkel’s interpretations – which Bittner must surely have been privy to, so I don’t think there was the big distinction between ethnomethodology and the Schützian approach that there is today. Because especially in Britain today we have the Wittgensteinian line, over there then it seemed to be phenomenologically-inspired, and particularly Schütz’s phenomenology because Schütz was very explicit in applying philosophical precepts to social science.

MW: So it’s actually about the state of the field as it was then?

RW: Yes, as it was then yes. I mean even when I was starting out I wasn’t reading Wittgenstein so much. I was reading some Wittgensteinians like Winch but the person that I was reading with Ted Cuff and Mick Atkinson and the others was Schütz, and the thing that we kept coming back to was that sociologists as theorists very often had arrogated ordinary members’ conceptions of their world, or of organisations or whatever it happened to be, and it was an arrogation. So those theorists’ work – although it addressed theoretical relevances of course – still had this massive common-sense input into it and an unexamined common-sense input

MW: Hmhm which is where the John Lee et al. paper⁷ on meetings comes in, Atkinson, Cuff and Lee.

RW: Absolutely without a doubt

MW: So when even now where we're talking about whether he was ethnomethodological you can't really judge that from a distance, you've got to see it in the terms of the state of 'the field' or whatever you want to call it

RW: You do yes – at that time – yes. I think that 'The Concept of Organization' paper still has massive relevance now. I think it's an extremely – it's an unbelievable paper and his police stuff is extremely good and he's written stuff on 'objectivity and realism' and so on in relation to phenomenology, where he's very, very tough on what he regards as 'misbegotten' phenomenological approaches. But what I would say is that some of his papers are definitely ethnomethodological to me. Others are ethnomethodologically-informed but where you get Bittner you always get an ethnomethodological dimension – sometimes up front, sometimes in the background, but it's there.

MW: So, I know this is not about ethnomethodology as it is conceived now but he never embraced CA did he?

RW: No not at all he was mainly an ethnographer if anything. When he was beginning to get into this CA hadn't been invented. It would be in a few years but it wasn't then ... when Bittner was doing his work in the early Sixties and maybe even before. So I think that it really it wasn't – I mean he could have tape recordings of police interrogations (similar to how I did it) but I think that really he was a radical naturalist in the ethnographic sense rather than in terms of the retrievable data sense. CA came along a little bit later I would say. I think that these things are in some respects structured by one's biography aren't they? If anything it's a biographical accident and by the time CA came along in any mature sense (really the late Sixties was the beginning of mature CA) Bittner had then undergone this formation as a deeply committed phenomenologist. So his methodological concerns, as in the 'Objectivity and Realism' paper in the Psathas volume,⁸ are really to do with, how do we bring phenomenology to bear on observation? In other words he's thinking of radical ethnography as ethnomethodology rather than CA.

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RW: [CA] was a particular take at that time on ethnomethodological sociology but it wasn't a take that Bittner had much of an affinity with. For a start I think at the beginning of the Sixties Chomsky was very much in the air. If you were studying anything to do with language as Sacks did you had to deal with Chomsky and I think that Sacks' early notions of apparatus and mechanisms, etc., and this mechanistic vocabulary that he had, actually comes from Chomsky, and in his Ph.D. thesis⁹ he says that he wants to do something akin to Chomsky and that is create a machinery that generates conversation

MW: Yes

RW: Now that seems to me to be quite inimical to the way that Bittner saw ethnomethodological studies as being pursued. And I think that what he did was relevant to symbolic interactionism, there's no question about it, and I think that's partly the influence of Cressey and partly because symbolic interactionists have a very good eye for a good study, a good ethnographic study. People like Phil Strong in Britain used Bittner a lot. He was a great proponent of Bittner's work. Other symbolic interactionists, including Sheena Murdoch and Tom Weinberg, in his fabulously detailed analysis of addiction studies,¹⁰ have used Bittner's work and see it as not unrelated to symbolic interactionism but very much espousing the SI spirit. Edward Rose doing the Ethno-Inquiries which was somewhat distinct from ethnomethodology, well Bittner worked with Rose on Rose's Skid Row study, *The Unattached Society*.¹¹

MW: Yes

RW: He came in to work with the police to look at how the police toured around skid row in their cars or on foot and looked at police conceptions of skid row people and that was in the Skyline area of Denver. The particular street was Larimer Street, that's where all the homeless men were (nearly all men), and Bittner worked with Rose on that. His report on that research that he did is Report no. 32 of the Bureau of Sociological Research Reports at the University of Colorado at Boulder.¹² And his report is there.

MW: Yes

RW: In the main report, Bittner is listed as an assistant co-author or something like that. Rose was the main author. So he clearly was relevant to a lot of people in addition to ethnomethodologists.

MW: OK

RW: I think you might say that some of his work has a functionalist element to it, a normative functionalist element to it. Indeed I think he has one book called *The Functions of the Police in Modern Society*.¹³ But again it is always ethnomethodologically-informed.

MW: But is it that we are all wanting to claim him, or is it how he saw himself?

RW: I have no idea because I didn't know him well enough. I met Bittner just once, in his only visit, a fleeting one, to the Sociology Department of the University of Manchester where he gave a lecture. He always credits Garfinkel and of course Schütz so I think that what I am trying to do is claim him when other people have forgotten him

MW: Aha, yes

RW: People don't claim him enough. The workplace studies people, some of them are very, very keen to claim him; some of them have never heard of him or of his work in detail and yet I think if they did study 'The Concept of Organization' for example it would stop them making a lot of mistakes that they make in workplace studies.

MW: Alright then so if we can talk a little more about 'The Concept of Organization' paper?

RW: I think that Bittner always insisted on looking at organisations from the incumbents' point of view as they were involved in particular tasks within the organisation, and that to Bittner *made* the organisation rather than it being this great reified entity. I think that these days there is a little bit of falling away from that. You see descriptions of work-sites and very often you can ask does this have any grounding in how incumbents of organisations actually see their organisational life and organisational activities? I think that there is creeping into (some) workplace studies if you like an analyst's imposition, which is as John Lee used to call it 'subjectively problematic' from the point of view of the incumbents. I think that if

people knew Bittner's theme on organisation and looked at that in conjunction with his 'Objectivity and Realism' paper in particular, they would realise that it's easy to produce a 'misbegotten phenomenology' of these things, which looks more phenomenological than it is, and that that 'misbegotten phenomenology' is misbegotten because it doesn't always capture members' orientations to their organisation.

So I think we could in a sense use Bittner's 'Concept of Organization' and the 'Objectivity and Realism' papers as a sort of measuring rod, as a set of criteria for workplace studies. I mean having said that it's not like modern workplace studies which relate to Garfinkel's 1970¹⁴ respecification of what ethnomethodology was, but there is in Bittner's work what I might call a set of cautions for contemporary workplace studies.

MW: OK that's interesting. I know we talked a little bit on and off about 'The Concept of Organization' paper – is there anything more you want to say about that?

RW: The other thing is that Bittner's view of organisational rules is very, very important indeed. For a start it gives us a view of rules in action rather than rules as these abstract things that just underpin this reified entity. In other words he's looking at rule use – hence the 'terms and determinations' emphasis – *very* early, very early that one and I think it probably informs Zimmerman's¹⁵ and Wieder's¹⁶ early stuff on rules. Now my view of this is that there are some fantastic insights there, and the classic – as you probably remember from your organisational sociology at Manchester – the classic stuff that comes out of Weber really is that we have the formal rules of the organisation and then we have people's informal practices. So you get formal culture within an organisation and an informal culture, very often the workers' culture and all that. Well Bittner says that's really not the way to think of organisations. If you think of it in terms of formal and informal what you are doing is using a particular situated conception of organisation, such as a manager's conception, people who are producing organisational charts, flow charts, etc., using their conception of what the organisation is, which is only one situated conception, and you're giving that priority: judging what's informal in relation to this thing to which you have accorded priority. There's a real mistake being made there, again from a philosophical point of view. He always goes back to the phenomenological standpoint.

MW: Can you elaborate the relationship of that to rules?

RW: Well it's often conceived in terms of rules isn't it? The formal rules of the organisation as opposed to the informal rules, things like goldbricking restriction of output type rules and that sort of thing

MW: so the importance of the emphasis on rules in action?

RW: Yes – in action – and not giving any set of rules the priority over others but just looking at whatever rule is used. Let's see how it gets used in relation to the organisation's work and seen by the incumbents, not by the sociologist operating so-called objectively and independently when really it is neither of those things

MW: Always coming back to what I think of as phenomenology I suppose

RW: Yes absolutely. I think that was pretty much the only game in town so far as the sort of wellsprings of ethnomethodology are concerned at that time

MW: So erm I think you said that we can see that paper very closely related to the 'Objectivity and Realism' paper. Do you want to say how you think they are related?

RW: Well I mean I think that the 'Concept of Organization' paper is like how-to-do-a-Schützian phenomenology of organisations. The 'Objectivity and Realism' paper has many themes but one of these is how people think they are being phenomenological or at least naturalistic when really they are not, when they fall far short of that. So basically, that if you want to find out the kind of mistakes that are made in a so-called phenomenology or naturalistic approach to organisations then don't just read 'The Concept of Organization' paper but also read this other one, which is almost a guide-book to what goes wrong.

MW: Another way into this may be to talk about his police studies.

RW: Well I think it's all part and parcel of the same thing. You see one of the lines which I would like to take which I think addresses again this issue of fragmentation is that I think that people seem to read Bittner, understandably of course, article by article rather than trying to see the crossovers. Now to me there's a crossover between 'Objectivity and Realism' and 'The Concept of Organization'. To me there's also a crossover between 'The

Concept of Organization' and the police work because the police are an organisation. They're producing conceptions of for instance the homeless people in Larimer Street or in San Francisco (where Bittner also did field-work). They're producing conceptions which are at least partly organisationally-relevant because, of course, they have to address matters of the law. On top of that they have – and we have to be careful not to say informal conceptions here – they have working rules as well which are in some ways continuous with the legal rules where they typify these homeless people. There's one paper in a book by Anselm Strauss, *The American City*¹⁷ where he published one of Bittner's papers – well a bit out of one of his ethnographic notebooks – about how the police tend to work according to a criterion which is in a sense legally-given – and that is, which people are liable to be trouble makers in Larimer Street or the San Francisco skid row – and who are not. And really even with the troublemakers the police try to keep the peace rather than arresting people and putting them through the sausage machine.

MW: I read this paper a very long time ago

RW: There are two more of them. One is 'The Police on Skid Row',¹⁸ the other is on 'Police Discretion in Emergency Apprehension of Mentally Ill Persons'¹⁹ – the discretion that is used. There are three main typifications that you find that the police use. The first is 'solid citizens'. People who may not have any money but they've pretty much got a place to live. They may get drunk, too drunk occasionally but by and large they're just OK. But 'solid citizens' can slip into being what the police officers call 'the hard luck group' – people who've for some reason lost their regular place to live, drinking too much and all this kind of stuff. Now these people are a source of trouble.

MW: Mhmm right

RW: but the third typification is the greatest source of trouble and that is what they call 'predators'. The predators are pimps, prostitutes, con men, people who are jack rollers (people who roll drunks in other words), muggers, all these kind of people

MW: undesirables

RW: yes undesirables that is how the police see them, these are the major source of trouble. Now the hard luck group can slip into becoming predators,

travelling from skid row to skid row. They're not always in the same place and according to the typification that is, the typical predator has these kinds of predicates, that they are people on the move, they prefer to remain anonymous and all this kind of thing. So these people – the hard luck group can sometimes slip into predators but very often the hard luck group are the victims of the predators, so that's also a problem that the police address, but by and large the predators tend to predate on each other as well. So a predator at one point can become a victim the next, and later become a predator again, so there's all that business. So, the police are always working at what we would see as an extremely complicated typificatory enterprise. With predators they very often settle for peace-keeping rather than arrest. In other words () if there's a mugger attacking another predator perhaps or another hard luck person and there's a fight going on, the victim is resisting and all the rest of it, the police sometimes will come along and will calm it down and try to prevent the thing from escalating so that other people won't join in and so on and they'll keep the peace, just keeping the lid on as it were partly because of course arresting people is an extremely long bureaucratic process, the police are thinking of their interests as well as the interests of the community. And so now all of that relates to two things. First of all to Garfinkel's notion of ad hoc-ing, ad hoc rules and ad hoc practices. In other words the police don't think of these things in general they think of it specific case by specific case. And one case isn't necessarily the same as the next and they all have their particular details. So what the police do is produce ad hoc actions. Now ad hoc really means for special purposes and so that's what the police do – what we might call recipient-design stuff to a certain extent. So that's the first sort of thing – it relates to some of Garfinkel's early precepts in ethnomethodology and I'm sure that he was deriving these at about the time when he was teaching Bittner. Secondly, it relates to Bittner's own work on things like stylistic unity and corroborative reference which is part of the 'Concept of Organization' paper. Although you're dealing with things case by case an overall pattern can emerge – an overall peace-keeping pattern rather than an arrest pattern. And this is kind of interesting because you get food and shelter, the police have to resist people who are volunteering to be arrested and perhaps actually making trouble in order to be arrested and have a square meal. So in that respect we might say that there is a stylistic unity to what they do, how they handle that situation. In a way we're talking about stylistic unity in the management of skid row and things like corroborative reference. So, I think that one of the things that we have not really done is

apply the ‘Concept of Organization’ analysis to the police as a study in organisations, because the police do belong to an organisation

MW: Is that not what Bittner was trying to do?

RW: Yes I think those same concerns run right across these papers yes but they’re not that explicit, he doesn’t invoke the ‘Concept of Organization’ paper too much at all, and in relation to the police we need to do that and I don’t think we do. We tend to take these papers one by one and treat them as self-contained but in fact there’s a very significant crossover here, certainly in terms of the ways rules are used – in this very flexible, case-specific way. All of that can be seen to come out of the terms and determinations sort of stuff. So in that respect for any legal rule you can have a peace-keeping determination or an arrest determination – you’re still doing legal work one way or another. So in that respect I feel that there is much more of a crossover than he’s often credited with because he himself doesn’t draw the parallels, but you can see the same concerns cropping up again and again as you can with Garfinkel

MW: That’s interesting can you suggest why he didn’t bring that out more?

RW: I think he was what I call a radical ethnographer – not applying some a priori stipulative theory to what’s going on which is, for instance, the way that critical criminologists carry on. He didn’t do any of that. He was very much involved in what I have called maximising his fidelity to the phenomenon. I think that when he was studying the police he was studying *the police* as far as he was concerned not looking at organisational work *per se*.

MW: He’s taking the approach of the later Garfinkel (quiddity and haecceity for example)

RW: Absolutely yes, the distinctive identifiability of *this* setting as opposed to this next setting and all this kind of thing. But I think the fidelity to the phenomenon meant that he didn’t quote his own work as much as you’d have thought he did. So I do feel that he is one of those people whose work gained much by *your* exercise in applying it to some other study that he did. I regard ‘The Police on Skid Row’ as an early canonical workplace study.

MW: Can you elaborate on his relation to workplace studies?

RW: I think that workplace studies emerged as a kind of package deal – part of a package deal of Garfinkel’s respecification of ethnomethodology around the 1970s (initially with Sacks in ‘On Formal Structures’²⁰ and afterwards) where Garfinkel placed more emphasis than before on ethnomethodology looking at order in the concrete rather than order in the abstract. In other words ethnomethodology looking at the identifying phenomenal detail of each setting which he initially called ‘quiddity’ and later ‘haecceity’ (since Quine had already booked the term ‘quiddity’ for other purposes he had to move on) and I think that for all the abstraction in ‘The Concept of Organization’ there is this insistence on determinations of *specific* determinations, specific uses of particular rules for particular cases in point and stuff like this. So, and I think that relates to Garfinkel’s point about jurors’ work when he said, do you remember, that Robert Bales was a small-group analyst yet he was trying to use Bales’ interaction analysis – on Fred Strodbeck’s project – and Bales always asked what makes the jury a small group and of course Garfinkel reversed that – really what’s interesting is what makes this small group a jury? In other words instead of looking at the abstract properties of small groups we need to look at the particular work that’s done by juries that they regard as the identifying work of the jury and which therefore makes them jury-members as they conceive themselves.²¹

MW: Yes

RW: So in that respect I think that that very concern for specific tasks and so on came out of the quiddity issues in Garfinkel and you can find an early precursor of that in Bittner’s ‘The Concept of Organization’ and a few early case studies in his police stuff. The two or three papers on mentally ill, discretion, study of peace-keeping, this business in Strauss, so I think in a way it was a precursor of things that were developed later on by Garfinkel mainly. In that respect I think that Bittner’s work is foundational in a way that isn’t always fully recognised

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RW: Some workplace studies people *do* recognise Bittner’s foundational influence but others do not and I think that there is something of an obsession with what is happening recently, which is important but it’s not the only

important thing. I do feel that if workplace studies extends far beyond ethnomethodology these days into areas like distributed cognition and different kinds of ethnography, often very quick forms of ethnography, in that respect Bittner's work is a bit of a caution because I think that many of these workplace studies that go beyond ethnomethodology begin to bring in again the sociologist's view of things without really much grounding in the regular person's view of things, that is the organisational incumbent's. And I think that one thing that Bittner's article says and shows is that this really is a no-no. You just shouldn't go there. You should always ground what you're saying, whatever the term grounded means (it's an ambiguous thing) but in some respects your work *always* address and topicalise this business of how incumbents themselves see their work in the organisation and how they actually go on with that work – how they conduct it. And I think that in some things that go beyond ethnomethodology in workplace studies that would be a very useful thing

MW: to come back to

RW: yes because sometimes in going beyond ethnomethodology some people use ethnomethodology and some use non-ethnomethodology. It doesn't always fit that well together. I think that there's a problem of logical disjunction. It's a bit like trying to put together two jigsaw puzzles. You don't get a bigger picture you just get incoherence. And I think that Bittner warned against the pitfalls of all that really and I would say that's a caution to ethnomethodologists always – we've all got to watch that kind of thing. It's a methodological constraint for us but it is of a particular relevance for those workplace studies that use a bit of ethnomethodology but also go beyond ethnomethodology in a variety of ways and try to bring in various kinds of conceptual cross-currents. So that's what I mean by being a caution

MW: OK so what you want to say is that he did a lot of the groundwork for what has now become workplace studies

RW: Mm yes oh absolutely, that's the way to put it because you wouldn't say that any of his stuff is a modern type of workplace studies

MW: No

RW: the phenomenological ethnographers are about as close as you could get to that I think, but I would say that the elements which became workplace studies largely are present in Bittner's work, and that's why I regard it as foundational

MW: OK right are there other aspects of ethnomethodology in Bittner's work that we haven't touched on?

RW: I think that's it. I think to get back to the typifications that's part of what he dealt with particularly in the phenomenological work, typifying people as solid citizens or that kind of thing. That's a typification with lots of predicates and that kind of thing. In other words 'what they're like' and all that stuff. I think that one unacknowledged thing that comes out of his participation with Edward Rose ... on Larimer Street ... is that he gives us a sense of typifications that perhaps Berger and Luckmann's *Social Construction of Reality*²² type of approach does not. For all the talk on social construction in Berger and Luckmann they tend to look at typifications as relatively enduring and all that – some of them are, gender typifications being quite constraining and long term and all the rest of it – but I think that Bittner's work with Rose in this case – because Rose is very strong on this – is that typifications can be sort of fleeting and constantly transmuted, so that for Bittner a 'solid citizen' can become a 'hard luck type' and depending on the circumstances they can go back to being a 'solid citizen'. But even more radically *The Unattached Society* report (to which he contributed) tells us that for instance sometimes, people – (this is the trouble with interviews and especially statistically-based interviews where you're trying to look at modalities and incidences and stuff) – you interview somebody who calls himself 'a drunk', never 'an alcoholic'. They don't see themselves as 'alcoholics', that's a middle class term that they abjure, they don't like it. But they will call themselves drunks. And so you go down in the stats as 'a drunk' (or as 'an alcoholic' because statistical sociology doesn't bother much with how people call themselves) but what Rose found, and probably Bittner too I guess because they were working together, was that a guy can call himself 'a drunk' on one occasion, interview him again three days later he regards himself as being completely sober and regards himself as being 'a solid citizen', not drinking all the time and then next time he's 'a drunk' again. So there's a sense in which typifications are this 'in and out' thing in Bittner's work, and certainly in Rose's work. And of course that is a big problem if you can't count somebody as being 'a drunk' because you interviewed him once next time you would

interview him (if you ever came back and of course Rose says sociologists typically don't come back, it's just one interview and that's it) he wouldn't be in that statistical cohort any more he'd be another one, 'solid citizen'. So I think that there is a rather different view of typifications that Bittner had in the Rose study. And I would say that this is a collective view in which Rose was very important too, whereas very often a lot of people think of typifications in terms of gender typing, and/or racial typing, which are fairly constant (not as constant as we think but fairly constant). And so I think that it's very, very interesting to look at these interactional transformations in typificatory work and what typification is arrived at.

MW: It is and this must be where the crossover with symbolic interactionism comes in

RW: Without a doubt yes, without a doubt because (some) symbolic interactionists tend to assume that what they call labels are very, very enduring and you cannot get out from under them, and that is sometimes true but it's by no means always true. And very often your 'label' can change and change back again. So there is this notion of – something that ethnomethodology would eschew – as a concept of a sort of negotiated order in all of this

MW: Yes so the Becker kind of labelling theory would not have been related to Bittner's kind of work

RW: Not really. I guess when you look at Pragmatist symbolic interactionism and so on, very often it runs in parallel with ethnomethodology – what we call 'typifications' they call 'labels' and all this kind of thing – and behind those different terminologies is of course a theoretical backdrop, which sometimes overlaps and sometimes is different. It's very difficult to look at the relation(ship) between the two but I do think that in what the symbolic interactionists (not the ethnomethodologists) call 'the negotiated order' there is the possibility of seeing how labels transform themselves if you're a symbolic interactionist or how typifications do if you're an ethnomethodologist. In other words, I think that in the Rose study there is an emphasis on the fluidity of these typifications that perhaps you don't quite get from some other understandings of what typifications are – particularly in my opinion the Berger and Luckmann stuff.

MW: Typification is not something that's continued as a big issue in ethnomethodology

RW: Not really

MW: and not in workplace studies I don't think

RW: Not really, this is partly I think because workplace studies is quite strongly influenced by CA. The term 'typification' never really worked there

MW: No, membership categorisation

RW: took over from the typification thing to a certain extent – but that is eschewed anyway now by CA. So I think that with some but not total success they're trying to disassemble the notion of typification into various forms of personal reference. I think that sometimes that works and sometimes it doesn't in CA, but no you don't get much on typifications now. If you do get stuff in workplace studies on typifications it's liable to be membership categorisation-type stuff, but even that is relatively rare and I suppose I can understand why. In the sense that very often what Bittner says about police work is this – although there are typifications there (the 'hard luck group' and all that kind of thing) he says that ultimately what the police do is experience situations, situation-by-situation and person-by-person. And knowledge of the person is just as important as the overall typification. In other words it's not enough just to see this guy as a member of the 'hard luck group', you've got know a bit about his background and biography as well in order to manage the situation appropriately.

MW: But that will be part of knowing what the typification is?

RW: Yes, many typifications become very highly personalised. As Schütz says, as we know the person better the typification recedes in the sense of being an exclusive characterisation of that person, and we do have this personal knowledge as well and the police do have this personal knowledge of their area, the beat that they walk and all this kind of thing. So it's not just typifications but as you say the personal knowledge is often organised in terms of those typifications – there's a reflexive relationship there

MW: But then actually I'm wrong because workplace studies will refer to personal knowledge

RW: Sometimes yes, sometimes I think so. But there's a sense in which membership categorisation can help workplace studies in ways that are now assumed rather than made explicit. So for instance, and this is transforming a study that Lorenza Mondada²³ did about surgeons and auxiliary personnel performing an operation – I can't remember it very strongly now but essentially there is a surgeon there, an assistant surgeon there, theatre nurses and often porters – a whole bunch of people and it's all duplicatively organised as a team, and that's very often not brought out. But a surgeon will cut into a muscle or something and it will bleed so he says – it's French, so he says something like – 'bru' (burn) and the assistant surgeon will come in and cauterise it to stop it bleeding and the surgeon can continue. Now of course that is a sequential thing to a certain extent of course. The surgeon says 'do X' and somebody else 'does X' but it's not the porter who does it, it's the person categorised as assistant surgeon who does it. That is his/her job as a category-bound activity, so in that respect I would argue that the surgeon doesn't look up and say burn, just says 'bru' or whatever the word is and of course the assistant surgeon knows that as an incumbent of the category 'assistant surgeon' it is his/her responsibility to produce that second pair part sequentially. That's why I think categories and sequences go so closely together. Now very often there are two assistant surgeons so there's very often a more specific division of labour within that category too, so then there's a distributional phenomenon that I think we need to look at. So in that respect I do feel that when you look at a lot of workplace studies there are a lot of these kinds of examples which simply assume the identity of a person. I am not accusing Mondada

MW: No, no, no

RW: but I'm trying to use her example as an example of the fact that submerged identity or categorisation work is happening here, submerged in terms of the workplace-studies practitioners' own conceptions of wanting so desperately to keep it sequential that these other concerns are counted upon, but not explicated. Now to me that's one definition of what constructive analysis does, or formal analysis as Garfinkel now calls it – orthodox social sciences – so I think there's a bit of backsliding there, in typifications or as we now call them, membership categorisations, and there's not a one-to-one relationship between those two things. But what I'm saying is that the room which was taken up by typifications is now taken up by this other kind of apparatus, called membership categorisation; and I think that essentially having said that the room is taken up by membership categorisa-

tion I don't think that membership categorisation is accorded enough room when it comes to looking at particular tasks.

MW: You've made that really clear thanks. I don't know if I've taken us far away from Bittner now

RW: no, no, no I think it relates back to Bittner in the sense that what we now call membership categorisation was originally called typificatory work and although the two are not coterminous – as I say membership categorisation has taken over the space from typifications – I think that what Bittner showed in his work was that, how can I put it, specific situated identities, which he called typifications, are very, very important features of workplace practice – certainly for the police officer and I would say for a lot of others as well. You look at flight decks, air traffic controllers, there's always that identity stuff going on there, and I think it's not fully teased out in terms of some of the sequential analyses that are done. So I think it's Bittner's contribution that identity work, which he might have called typificatory work, is such a rich resource for people doing their organisational work. It's that kind of thing and I wouldn't say that's fully deployed in contemporary workplace studies practices, that's why I regard Bittner's work as a bit of a caution. In other words there's a whole set of resources there that are sometimes exploited by workplace studies people and sometimes not so or inadequately so, and Bittner would have thought that it was inadequately so in some cases. I think Bittner would – again guessing at what people would think – I would say that he would be looking at, much more *explicitly* at the identity work, which is after all being done there and can be shown to be being done, but workplace studies analysts don't always go in that direction – so keen are they to look at action sequences and sequences in context (but) to me the identity stuff is very much part of that context.

MW: Yep. OK.

RW: So that's what I think is the way through from Bittner.

[END]

NOTES

1. This is an edited transcript of a conversation held in the Faculty of Humanities, Law and Social Science, Manchester Metropolitan University, on 19th January 2012. Breaks in the talk are marked with asterisks. Words unavailable for transcription appear in empty parentheses. References have been added following the conversation to allow readers to follow up points of discussion. The guest editors thank both Rod Watson and Maria Wowk for allowing us to publish this unfinished piece in an unrevised form.
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23. This refers to a data-corpus presented by Lorenza Mondada in ‘The video-analysis of surgical operations’ at the One Day Conference on Video-Analysis, École Normale Supérieure, Lyon, France, March 2008. It should be noted that membership categorisation activities were not features of Mondada’s presentation, hence the ‘transformation’ caveat. Publications using these data include the following:
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Mondada, Lorenza. 2007. Operating together through videoconference: Members’ procedures for accomplishing a common space of action. In *Orders of Ordinary Action: Respecifying Sociological Knowledge*, eds Stephen Hester and David Francis, 51–67. Aldershot: Ashgate.