

## DISCOURSE STUDIES

### Comment

#### In support of Conversation Analysis' radical agenda

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**DRAFT: 19 April 2016**

#### **Abstract**

This comment provides an overview of the four articles by Lindwall, Lymer and Ivarsson; Lynch and Wong; Macbeth, Wong and Lynch, and Macbeth and Wong, which make up the kernel of this Special Issue of *Discourse Studies* on Epistemics, and it also examines the reasons for the assorted difficulties the authors of those articles have with the Epistemics Program being proposed for Conversation Analysis. The legitimacy of their concerns is underscored by showing that the charge the EP makes, which is that the conversation analysis developed by Sacks, Schegloff and Jefferson gives an incomplete account of linguistic interaction, is founded on a number of mistakes. The first mistake is the idea that conversation analysis cannot effectively address so-called action formation. The second is a misunderstanding of the idea of 'proof procedure' in CA. The third mistake is that conversation can better be understood through an abstract analytic construction, rather than an emphasis on participants' analysis. And the fourth is that it is necessary to rely upon an imputed cognitive machinery that is asserted to underlie conversation. The systematicity and generative power of Sacks, Schegloff and Jefferson's radical work on turn-taking in conversation is emphasised and a question that begs to be answered arises, which is how well the EP compares to that earlier, and still-radical work, something that the kernel papers address in their return to key transcripts used in the EP.

#### **Keywords**

Conversation analysis, ethnomethodology, knowledge, sequential organisation, turn-taking in conversation

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The four papers that make up this special issue of *Discourse Studies on Epistemics* are all deeply worried by, and are critical of, the turn that is being proposed by what they call the Epistemic Programme (EP), for the field of *Conversation Analysis* founded by Harvey Sacks and Emanuel Schegloff, and which the founder of *ethnomethodology*, Harold Garfinkel, once described as the jewel in the crown of ethnomethodology. Ethnomethodology and Conversation Analysis (EMCA as they are referred to in the papers) were intended as radical methodological departures from social sciences' constructivist ways of accounting for social matters (Garfinkel and Sacks, 1970). To inevitably oversimplify Garfinkel and Sacks' interests we could say there were two radical characteristics to their proposals. First, they made available for study the organised properties of members' methods of practical common-sense reasoning and understanding. Second, 'constructive analysis' (social science) was shown to be parasitic on, and marbled through with members' methods, which were essentially assumed and left un-investigated. The current papers are concerned that EP is itself a departure from the radical thrust of ethnomethodology and conversation analysis and is a reversion to constructive analysis. These papers all announce their worries and develop their critiques in two major ways.

First they return to the joint works of Sacks and Schegloff on the systematics of the turn-taking system for conversation, and later work by Schegloff, to show that far from being a significant development for Conversation Analysis that some commentators such as Drew (2012) have claimed (though we note some hesitation in his enthusiasm), the Epistemic Programme in Conversation Analysis, actually, undermines the very radicalness of that pioneering work. One feature of this radicalness was that Sacks and Schegloff offered an exceptionally refined and systematic exploration of what is often called 'the actor's point of

view' — in Sacks and Schegloff's hands this is manifest in a concern with the very *doings* actors engaged in. Their inquiries were centred on the *participants'* analysis of their co-participants' actions; it placed analysis in the hands of members, so to speak. In as much as the Epistemic Programme is concerned with measuring the knowledge possessed by participants through the analytic attribution of relative knowledge according to a generalised and abstract analytic schema, and imputed normative entitlements, the Epistemic Programme emphasises the role of the professional analyst over that of participants. In that respect it is such a departure from Sacks and Schegloff's work as to make questionable that it is CA at all.

Second, the papers are not polemical, they make their points through CA's robust practice of grounding its descriptions in actual instances of conversation. Not only does CA ground its description in actual talk, it is through the actual instances that CA description can be judged and accounted for. In this respect, by looking to instances that proponents of the Epistemic Programme have used to question Sacks and Schegloff's work, and the work of others in their tradition, all of the papers compare the epistemic analysis with an analysis that can be produced by CA's emphasis on turn-taking and the sequential organisations that can be laid on top of it. They set out to display the more systematic nature of conversation analysis compared to epistemic analysis.

However, why make such a fuss about epistemics? After all, it does follow up on precedents that are common in the CA literature, that deal with the 'epistemic' features of interactions in various ways. CA, though, has tended to address epistemic features in isolation from one another, as separate topics, for example, news announcements, and speakers' activities of sometimes under-telling and over-supposing. Consequently, what can be wrong with an Epistemic Programme that is attempting to bring these assorted topics

together, and trying to provide a coherent framework for treating them? Why are the papers making such a fuss about EP if the criticism of it mainly turns into disputing the fine details of transcript interpretation? Certainly the instances analysed by EP may not be definitive examples of the matters they are concerned with, the distribution of knowledge between the parties. However, no one is going to doubt that those phenomena are *sometimes* manifest in *some* conversations. Therefore, it is entirely reasonable to suppose that even if many of the instances presented by EP to date are questionable, that other instances better fitted to the epistemics framework can certainly be found. It might be said of the papers making up this special issue that the most their accumulated critiques show is that the issues about instances presented in transcripts can mostly be addressed through more persistent application of standard CA practices of sequential analysis, albeit applied in more circumspect ways.

To a certain extent this might well be the response that those the papers take issue with might make. They may say that what they are doing is to build on the legacy of Sacks and Schegloff, addressing matters not taken up by them and thus strengthening and developing the programme of conversation analysis. Indeed, they may claim that the current papers are presenting a fundamental mis-reading of their work, distorting their intention and framing them as critical of Sacks and Schegloff's CA, when what they are attempting to do is to extend CA by enabling it to address matters hitherto only gestured at and, at the same time, making CA perspicuously relevant to the range of disciplines that concern themselves with epistemic matters. Reviewing extracts that have been studied by CA in the past in order to re-analyse them from an epistemic point of view is not intended to detract from their previous analyses, but instead to show that more is going on that requires

reference to the relative distribution of knowledge between the participants, an issue that CA, as originally practiced, does not consistently bring into play.

However, the strong critical reaction of the papers in this special issue against EP is not because of the dubiety of EP accounts of exemplary sequences as such, but precisely because it *is* advanced as a *Programme*. As a programme, it makes claims about the (un)systematic character of CA analysis and about the systematic potential of its own. It is proposing that, in important respects, EP's form of analysis does not just build on CA, it cannot, actually, be accommodated in CA as developed by Sacks and Schegloff and subsequently practiced by many others, notably by Jefferson. It is proposing, first, that CA has no answer to the question 'what drives conversation,' a question that it considers is, plainly, in need of an answer. Second, it proposes that CA is bereft of techniques for analysing what the actions done through talk are doing *as* actions. CA procedures do not involve acknowledgement that one thing which those actions are doing is managing the epistemic situation. Epistemics are programmatically considered to be ubiquitous features of conversation, or, at least, it is suggested that any utterance can be considered from EP's point of view (which seems to mean much the same as *every* utterance needs to be considered in those terms). Consequently, EP presents 'epistemic analysis' as competitive with 'sequential analysis'. The correct analysis of the cases that proponents of EP have taken from various transcripts that have figured in CA's past, therefore, need much more epistemics input since, as it is claimed, epistemic concerns can and do 'trump' other considerations in determining the identity of the actions occupying sequential positions.

This order of criticism made by EP brings to mind early criticisms of CA by sociologists who by and large had no interest in conversation and would complain that CA could not deal with the very important concerns they were interested in. The considered reply from CA was

often of the form: 'have you tried to see what you can do with CA procedures and whether, even you might turn out to be able to do the things you said could not be done, although not perhaps in anything like the form you might have dreamed of doing them?'. This is precisely why the transcripts and their analyses that are presented in the papers are central to their critical dispute with respect to EP. They are not intended to show that particular epistemic analyses have slipped up, but to show that the phenomena which are picked out as exemplary phenomena in epistemic analysis *can* be comprehensively analysed by assiduous application of CA's stock procedures.

As the authors of the articles in this special issue see it, epistemics' challenge is not to CA's methodology of analysing the organisation of turn distribution in talk in terms of understanding the multifarious ways in which the shape of turns produced in conversation incorporate the 'systematics' of turn taking into their production *and* reception. The EP objection is seen, rather, pitched against the way in which the analyst's own competence as conversationalists contributes to understanding the actions that are done in/by turns, something which EP considers to be a key failing of Sacks and Schegloff's CA. If this is what EP is proposing, then it would involve some serious misunderstanding of Sacks and Schegloff's reasoning by proponents of EP. In this respect, Lindwall et al. (2016) argue that EP's treatment of 'action formation' supposes that CA possesses only inadequate means for identifying what a given utterance is doing in the specific sequential place it occupies. CA, allegedly, is restricted to reliance on a proof procedure, which is effectively, endorsing the identification that the recipients of an utterance make of it. This allegation that Lindwall et al. (2016) find in EP makes CA's policy sound decidedly constructionist in tone: the identity of a given action is decided by the reaction that participants other than its producer manifest toward it.

However, the quotations in Lindwall et al. (2016: 4-5) from Levinson and from Heritage may index a deep misrepresentation of CA by EP critics. CA's identification of actions done in conversation are first formulated in the currency of the vernacular, of common-sense categories of action such as 'greetings', 'questions' and the like, not in the technical vocabulary of linguistic technologies with their disciplinarily-generated definitions and criteria. However, we could ask, what else is there other than commonsensically recognisable actions? Even the analytic scaffolding of linguistics is based upon the common-sense recognition of an action, as the thing it is. For example, in speech act theory the definition of how to do a correct promise trades on our common-sense knowledge and understanding of what comprises promises in the first place. To be sure, Sacks' reference to 'greetings' in his lectures (Sacks, 1992) and Schegloff's (1968, 1979) work regarding 'conversational openings' and 'opening sequences' takes for granted their ability to recognise a greeting when they encounter one, just as anyone being greeted 'knows' they have been greeted, and as the one doing the greeting 'knows' what they are doing. However, Sacks and Schegloff do not, in Levinson's terms, just 'intuit' a greeting and proceed on that basis, or, in Heritage's terms, just develop a greeting in a 'transparent' or 'ad hoc' manner.<sup>1</sup>

It is not the case that Sacks and Schegloff proceed on only the basis of their intuition or ad hoc characterisation, for as Jefferson (1989) makes perfectly clear in her reprimand of Pomerantz, CA moves from this initial characterisation into grasping the organisational machineries through which this characterisation is account-able. Thus while Schegloff uses the vernacular term 'greeting', he then moves to placing that characterisation within a detailed consideration of the interactional organisation that provides for the action(s) it accomplishes, and in those accomplishments makes the characterisation appropriately

visible. Thus, we use what we know, by being in our culture, to be a greeting term (e.g. 'Hi') or gesture (e.g. a wave) but we use it in particular sequential places in conversation which provide for the recognisability of the action it accomplishes, in the turn initial position of a conversation, or interaction, or following an initial greeting. What Sacks and Schegloff's analysis, which may well start off from a common-sense characterisation of an action that is available to any competent person, aims to do is make visible what it is we know—what we display as what we know—when we do a greeting or respond to a greeting. Thus when Sacks or Schegloff use a common-sense term, a greeting for example, to describe an action, in describing how it is done, and done recognisably, they provide for the method used to produce it, and also the method used to respond to it, and thus the method used by themselves to determine that it is a greeting. In other words, their analysis explicates the common-sense knowledge we all display of what greetings are in our shared cultures when we greet someone and when we use that knowledge. And they do this most systematically, for as we know the analysis of greeting sequences differentiates different types of action done in a greeting such as an initial greeting from a returned greeting.

An extract quoted by Jefferson (1989) from one of Sacks's lectures might be instructive for Levinson and Heritage in respect to their criticisms:

Put kind of straightforwardly, what I figure is going on is that ... Portia thought then and there that Kate might have done something that embarrassed, maybe angered, annoyed Carl. She then proceeded to tell Carl how good a person Kate was, in some aid of Kate.

Now that's an altogether informal, unproved, perhaps unprovable, perhaps irrelevant-to-prove-it, characterisation of what took place. And it's just the sort of observing that, when it appears in a student's paper, we thoroughly discourage.



However, it is one legitimate and fruitful way to approach materials, for the initial observations themselves, and in that that sort of sophisticated lay observation of a scene is one way that you come to find items that can be extracted and developed quite independently of the observations one initially made; where the initial observations need not, then, be presented. One needs to see if those sorts of observations that sort of a discussion, can lead to something that could perhaps transcend it and turn it into some sort of serious statement, other than the statement I offered, and which perhaps Portia herself could offer. (Sacks 1970, Lecture 7, page 3, quoted in Jefferson, 1989: 428)

EP's criticism of CA as developed by Sacks and Schegloff with respect to the so-called problem of 'action formation' therefore involves a misunderstanding of the idea of 'proof procedure'. EP has it that CA turns to next utterances to 'prove' what the prior utterance was. This criticism over-inflates the idea of 'proof' in CA, and in so doing misses that all it is, is a practical tool. The 'proof' procedure is not a means for initially establishing the identity of a turn in conversation, it only supplies further confirmation of a determination that the analyst has already made as to what a given utterance might be – if, e.g. the analyst figures some utterance is a question and, apparently, fellow conversationalists respond by engaging in answering, this shows that the identification does not manifest the analyst's idiosyncratic reasoning but, seemingly, the same kind of reasoning on the part of the conversationalists whose talk is being analysed. The CA analyst's initial identification of any given turn is characteristically premised in considerations affecting 'utterance design', involving an extensive array of particulars potentially relevant to asking of a turn the central and genuinely ubiquitous query at the heart of the turn taking system: *why that now?* In these respects It needs to be emphasised, again, that what Sacks and Schegloff are addressing is

*participants'* understandings. Conversation analysis has always been about making visible the practices that *parties to the conversation* engage in as part of organizing their conversation. *That* is why Sacks and Schegloff emphasize next turns, not as a method to establish for the conversation analyst what some prior action was, definitively, but to address how something was heard in the course of the interaction of which it was a part, and how it could be heard as that thing.

In Lynch and Wong's (2016) paper, it is asked whether, then, EP deliberately *reverts* to a traditional constructive-analytic method for addressing social action in terms of associating the action with 'who' has done it, where the nature of 'the who' is determined by some social theory. In the terms of Lynch and Wong's paper, the 'who' in EP is determined by the *cognitive states* of what we could describe as 'one who knows', and 'one who does not know' something.

There have been many significant determinations in the social sciences of the identity of 'the who'. What someone does is done because of their *status* with respect to others, their *power relationships* with respect to another, their *class* position with respect to another, their different *gender* with respect to others, as examples. These, and other identities have traditionally been used in the social sciences to account for people's doings, but rather than examining 'the doings', they examine 'the doer' (Schegloff, 2010). It is to 'the doer' that EP turns, for what is seen to drive interaction is the relative distribution of knowledge between participants. Thus what someone does is done because of the distribution of knowledge between themselves and another. EP simply solves the so-called problem of action formation by reverting to a traditional constructive-analytic social science method of imputing identities to the speakers, the identity of someone who 'knows'

something with respect to another, or someone who does not know something with respect to the other.

However, EMCA emphasizes that structures of practical action (Garfinkel and Sacks, 1970), including those of conversational turn-taking, are cohort independent, a point reiterated by Sacks, Schegloff and Jefferson (1974) in their turn-taking paper. Identities are assigned within the turn-taking system, they do not frame its organisation. The issue of cohort independence brings into sharp focus the divide between traditional social science, on the one hand, and EMCA on the other, so a move by EP to feature cohort dependence would be a significant move, but not one deriving from analytical necessity. As Sacks' work on membership categorization (Sacks 1972) makes clear, persons can be described or identified in many ways, and the question is 'how is it that describing them as an "x" as opposed to a "y", as a man as opposed to a teacher, for example, is made relevant for what they do and is provided for?.' The warrant for the social science attribution of omnirelevant identities to people as grounds for understanding what they do resides in the particular social theory being articulated. However, Garfinkel, Sacks and Schegloff break with traditional constructive-analytic social science; the radicalness of their pioneering work, resided in placing the warrant for invoking the identity of the doer as relevant for their doings in the actual doings themselves. That is, the warrant for invoking the gender identity of a person resides not in an all encompassing social theory of gender or gender relationships, in the fact that the person is, for example, a man, but that it is possible to see in what he does, that in how he does what he does, his identity as a man rather than, e.g., a teacher, is relevant.

This reversion by EP to constructive theory and methodology has been considered by Schegloff in discussions of work done by others associated with CA. Zimmerman and West

(1975) and West (1979) emphasized gender in order to understand conversational actions. Their two papers examined 'interruptions' and 'silences'. However, their concern was not so much with the organization of interruptions and silences in talk, as such, but their organization in relationship to the talk occurring between people of different genders. However, Schegloff (1991) re-emphasized the need to understand how interactional occurrences like interruptions are organized, irrespective of the imputed identity of the people involved, and the imperative to show, if indeed it can be shown, the relevancy of an identity for the doing of the interaction in the very terms of how that interaction, in this case 'interruption' is organized.<sup>2</sup>

Schegloff's point is that it is not so much the persons involved, the actors, that are of concern in CA but rather the impersonal organizing practices and methods of the action and the interaction; it is these, not 'the actors' (which should not be confused as denying an interest in 'the actor's point of view'), that CA strives to address. Schegloff returns to this issue and amplifies it in his response to Stivers and Rossano's (2010) descriptions of sequential implicature in conversation. They describe this in terms of the way in which actors seek to impose *normative obligations* on others, or in EP's terms, imply '*normative entitlements*'. However, CA, as Schegloff (2010) describes, is not concerned, as we mentioned above, with what people do as actors but with *how* they do what they do, and with how it is possible to gain an understanding of *how* they do what they do through an examination of what they do. Much turns, for Schegloff then, and he makes this abundantly clear, on the difference we mentioned above, between describing things in terms of the '*doer*' or '*the doing*'.

Thus, it is not an actor that exerts a normative obligation on an interactant; rather there is an organization to the action and interaction that stands apart from any particular

actor. Actions, as noticed by Garfinkel, are done so as to be account-able; that is they are done so as to be recognizable for what they are, and this recognisability derives from the way action is put together, not from personal characteristics of the turn taker. Although this organization stands outside of any particular interactants it obviously requires them to turn it, once again, in and for this occasion, which requires that they understand a particular organization, into something they display in the doing of the action and interaction, but it is not dependent upon them. In their turning of it, they may well build into it, for particular occasions, the relevance of their identities, and then as we have mentioned above, Schegloff makes clear that this may warrant the invocation of that identity. But its occasional relevancy does not provide for it as omni-relevant. It may well be that the identify of someone as 'the one who knows' is made relevant in the organization of an action, for example in this News Announcement: "I know something that you don't know, Don and Beth are getting married." But that occasioned relevancy does not license the omni-present relevance of 'knower' and 'not knower' for the organization of action in general. Schegloff sums up his point concerning CA's emphasis on the doing rather than the doer by paraphrasing Goffman, 'not persons and their moments, but the organization of those moments' (Schegloff, 2010: 41).<sup>3</sup>

The discussion of 'oh' in two parts in the current collection (Macbeth et al., 2016; Macbeth and Wong, 2016) reflects the authors' insistence on the remarkably minimalist character of CA's methodology with respect to the matters discussed above, that CA is highly restrictive with respect to the invocation of matters external to the 'turn taking' arrangements. The range of data extracts previously used in CA papers that have been re-examined within EP have been used to argue that understanding the actions in question requires reference to matters external to the interactional, sequential organization

previously provided by the original papers that employed the extracts. However, Macbeth et al. question the need to relax CA's minimalist requirement; indeed, they make visible that by so doing—by invoking extra-interactional matters—the EP actually distorts what is being done in the actions considered. Both papers demonstrate that extra-interactional resources are not required because they are able to develop more effective analyses of the same extracts by simply making visible the interactional (sequential) context within which the actions occurred. They make visible that, when placed in their interactional, sequential context, which Macbeth et al. (2016) do by revealing more of the transcript before the extracts in question, the actions can be accounted for in terms of matters internal to the turn-taking organisation and do not require extra-interactional and extra-sequential matters to be invoked.

It may perhaps be that it is just this relaxation of minimalism that is attractive about EP to some within CA, and those in other disciplines who, while not wholeheartedly embracing CA, nevertheless, have understood its implications for their disciplines. With respect to the first cohort, the fact that EP allows the extension of the domains of inquiry and the forms of analysis available to conversation analysts may be attractive to those in EMCA who wish to demonstrate its relevance to the consideration of traditional concerns within social science at large. The initial white heat of hostility shown by social science to EMCA has perhaps burned down to the ash of indifference, and some within EMCA have attempted to rekindle at least a warming glow by showing how its concerns can relevantly, rather than nihilistically, contribute to sociological theory, statistics methodology, and communication studies, for example. EP, thus, may appeal in as much as it orients CA to a new multi-disciplinary environment. Those who have recognized CA as having relevance for their own fields but have struggled with embracing CA entirely, lest it undermine their prime

concerns, may equally be enthralled with EP in that it allows them to draw from CA (now EP) in ways familiar within their home disciplines.

Importantly, all of the papers in this Special Issue do not represent an attempt to exclude any (relevant) topics which may be thought to have been significantly 'neglected' by CA, and there is certainly no attempt by them to prohibit investigation of epistemic aspects of the organization of interaction. After all, as we have mentioned above, these have long been of interest to EMCA.<sup>4</sup> However, what all of the papers attempt to show is that if epistemic matters are to be addressed there is need (a) to develop a systematic method for picking out 'epistemic features' as distinct from sequential organisation and (b) to develop a systematic application 'of epistemic structures' sufficient to allow determination of how conversational exchanges are embedded in the epistemic order and just how it is that the relevance of specific epistemic features are incorporated into turn-taking arrangements. Fulfilment of these by no means undemanding requirements would put the relevance of EP beyond question. However, the papers are not convinced that the EP as so far set out is capable of fulfilling these requirements, given that as Macbeth et al.'s papers argue, it was founded on quicksand in the first place.

We will conclude by noting that if those who only have a passing acquaintance with EMCA, or a second hand version through the various summaries and interpretations of the work in that field,<sup>5</sup> turn to Harvey Sacks' lectures, his notebooks (*if they can get them*), his other early writings, and his collaborative writings with Emanuel Schegloff and Gail Jefferson, as well as Garfinkel's *Studies in Ethnomethodology* and his unpublished materials (*if they can get them*), they will be struck by the divergence between the practices of social science at large and EMCA. We introduce no caveat here such as 'social science at the time they were written', for despite multifarious change in the social sciences since they were

written, such as the tsunami of works in social studies of science and technology, of constructionism more generally; the advent of post-modernism; the proliferation of deconstructionist techniques; the surging enthusiasm for ethnography and the expansion of feminism, all of which might, on the face of it, contrast with the predominance of 'positivism' at the time of their writing, little has changed in the way in which the social sciences go about the practical business of inquiry into the social order. All of these 'new' developments share with the 'positivism' of old the attempt to render descriptions of social matters through disciplinary owned methodological apparatuses such as theorizing, generalized schemas, re-purposed terms, and the like.

We realize this is a large claim, and we also realize that this is not the correct place to provide a proper justification. The point, however, that we wish to emphasize from this is that ethnomethodology and conversation analysis tend to remain enduringly at odds in their accounts of human doings with the body of social science that demands that those doings must be rendered through proprietary professional methods of accounting rather than by pervasive reference to the organizing methods embedded in the doings themselves.

Not only might this radicalness be apparent to new readers of original work, for those within EMCA returning to them can also be salutary. Thus in researching relevant material in the preparation of this paper we have returned to some of the original analyses that figure in the various papers in this volume and the papers they refer to. We were struck again with their depth, intricacy, cogency and above all their systematicity – the pinnacle of which is undoubtable Sacks, Schegloff and Jefferson's (1974) paper on the systematics of turn-taking for conversation.

## **Funding**



This research received no specific grant from any funding agency in the public, commercial, or not-for-profit sectors.

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## Notes

<sup>1</sup> It needs to be remembered that definite determination of the nature of specific utterances in recordings is not the necessary purpose of examining them – if analysts could not achieve clear or consensual determination of what was being done by some utterance then, Sacks counselled, they could examine instead other instances where they could be more confident of their determinations.

<sup>2</sup> Subsequent to this initial work, and despite the early work on cohort independence and Schegloff's subsequent intervention, gender, and identity in general, has been propelled onto the CA stage, for example Antaki and Widdicombe (1998) and Speer and Stokoe, (2011). These reversions, like EP, wander back to the old route trodden by social science.

<sup>3</sup> Interestingly the specific problem that Lynch and Wong (2016) are pointing to with regard to the EP's reversion to a theoretical apparatus that imputes underlying cognitive structures to conversation has also been acknowledged by Schegloff in his consideration of discursive psychology (Schegloff, 1997, 1998). Schegloff specifically draws out the break that CA made in its foundational work with traditional social science. As we have noted, traditionally the social sciences account for social action and interaction in the terms of a social theory, and Schegloff makes the point that in discursive psychology actual action and interaction are analytically addressed under the auspices of a theory involving the attribution of motives and other cognitive states.

<sup>4</sup> One of us, for example, explicitly addressed such matters (Sharrock, 1974).

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<sup>5</sup> Many of which mis-represent them.

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### **Authors' biographies**

**Graham Button** completed his PhD in 1975. He was supervised by Wes Sharrock at Manchester University and his thesis was the first PhD in the UK to be awarded for work in Conversation Analysis. After a sustained period working in the area of Conversation Analysis, and publishing in a variety of journals and edited collections, Graham Button began to move more into the area of Ethnomethodological Studies of Work with particular reference to the way in which ethnomethodology and conversation analysis could support the design of interactive and ubiquitous computer systems. His last position before retiring from institutional life was Pro-Vice Chancellor for Arts, Computing, Engineering and Science at Sheffield Hallam University and prior to that he was the Director of the Cambridge Laboratory and then the Grenoble Laboratory of Xerox Research Centre Europe.

**Wes Sharrock** recently celebrated his fiftieth year at the University of Manchester. His PhD was awarded in 1970 and since then he has authored and co-authored many publications, the most recent of which is *Choice*, written with Dave Randall and Richard Harper and published this year, 2016, by Polity. His publications have ranged over a diversity of topics but have remained fairly constant to his interest in the philosophical and methodological difficulties of the 'social sciences' and the way in which these show up in empirical descriptions. Since the late 1960's the work of Ludwig Wittgenstein, Harold Garfinkel and Harvey Sacks have provided invaluable resources in pursuing those interests, especially those respects in which they facilitate a deflationary conception of the social studies.