

[coulw16.001. Tr: BJF. 2 September 90: Edited version] Transcription conventions can be found on the last 2-3 pages of each transcript.

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WITTGENSTEIN AND ETHNOMETHODOLOGY

Meeting 16.: 24 September 87

TOPIC: THE GRAMMATICAL/LOGICAL VERSION
OF ETHNOMETHODOLOGY.

Including discussion of: Insertion sequences,
pre-sequence expansions, hearable presuppositions,
Speech Act Theory

Jeff Coulter: Ok! Welcome to the logicist seminar!

Ok, we were talking about Conversation Analytic work last time, and we were looking at adjacency positioning and the repair apparatus in respect of the argument that, first of all, sequential organizational conventions operate on illocutionary act types, like question-answer, greeting, invitation, request, offer, etc. And, indeed the most elementary organizational structure in Conversation Analytic work is the adjacency pair. The properties of which are properties OF illocutionary actions and their organization. It's very important that you realize that the conventions are specified OVER illocutionary act types.

So, then we started to note that, obviously, to take for example the Q-A sequence, obviously it's the case that you can have occasions in which

you have, at least as an initial observation, something that starts as a question, and then a next thing that is also a question. Where, rather than now use that fact as a basis for undermining the claim that questions are first-pair-parts and answers are conditionally relevant nexts, instead of using like an empiricist criterion for undermining a conventional claim, what one does is to examine the case in which it seems as if there is a row of questions, one after the other with alternate speakers, and see whether that characterization of the illocutionary force must now be maintained, or has to be changed. And as we saw in the case of the apparent three question: Speaker A: Question 1, Speaker B: Question 2, Speaker A: Question 3 - what looks like a row of three questions can in some cases be seen to be an instantiation of quite a different set of activity-types. So whilst it may be that the first one is a question, say, "What made you do it?", the next one: "Huh?", for example, now can be found not to be a question, but something like a repair initiator. And the next thing, "Why did you take it?", is not a third question, but is a reformulation of the repairable turn.

Then we have other kinds of variations off of the Q-A sequence that also do not undermine the power of the notion of the adjacency positioning conventions, or the adjacency pair structure and its properties. And this would be where you have something like: one question that gets another one in next position, so here it seems as if you have two questions, with alternate speakers. And we find that, again, we can maintain the integrity of the adjacency pair analysis as a basis for seeing some yet new phenomenon.

Ok, so the argument would be something like this:

A: What's the time?

B: Why do you need to know?

To alter the case, requests characteristically get grant or denial, according to the adjacency pair way of characterizing their organization. You could get something like a request followed by a question:

A: Can I borrow the sugar?

B: Do you need it this very minute?

So you can get instances in which, with perfect preservation of intelligibility – there's absolutely nothing unintelligible about these cases in which first-pair-parts get radically different kinds of nexts than the adjacency positioning convention would have led you to believe. There's nothing about those cases that destroys intelligibility. On the contrary, they alert us to the fact that we are dealing with new sequential phenomena.

So, in the case of the question that then gets a question, or the request that then gets a question, what we're on the verge of discovering – following Sacks, of course, and Schegloff, who did make the discoveries – we're on the verge of noting the existence of "insertion sequences", or "insertion expansions". Where you get, for example, something like:

A: What's the time?

B: Why do you need to know?

A: I've got a class!

B: Oh, it's 4 o'clock.

Where "What's the time?" is an adjacency pair first-pair-part, and "Oh, it's 4 o'clock." is adjacency pair second-pair-part for the FIRST adjacency pair.

But inserted between the first-pair-part and the second-pair-part of the first adjacency pair, or of adjacency pair 1 [AP1], you have tucked in there a second adjacency pair [AP2,] inserted, embedded, in a mirror structure.

		A: What's the time?
!		
!		B: Why do you need to know?
AP1!	AP2!	
!	!	A: I've got a class!
!		
!		B: Oh, it's 4 o'clock.

So, "What time is it?" – "It's 4 o'clock" has built into it another adjacency pair: "Why do you need to know?" – "I've got a class". So, we have, let

us say or I'll call it for now, the 'first' adjacency pair first-pair-part getting its second-pair-part only AFTER the inclusion of another adjacency pair stuck in the middle. And so it's in that kind of way that you get these sorts of things running off.

Another example where it would be request as a first-pair-part:

A: Can I borrow your sugar?

B: Do you need it this very minute?

A: No, it can wait I guess.

B: Ok, I'll send it down later.

Another example. And these ramify through the literature. [Tr: omits] The "Can I borrow your sugar?" example is out of Sacks. He claims it's a real case, but it doesn't matter whether it's real or not. You can see the point.

The argument is with first-pair-parts, it sets up a second-pair-part as conditionally relevant. For 'question' it sets up 'answer', for 'request' it sets up 'grant' or 'denial'. Remember that adjacency pairs are dichotomous with respect to: there's one set of adjacency pairs that make conditionally relevant ONE next, and then there's the other set that makes conditionally relevant one of two alternate nexts, and with a preference ordering over the alternate nexts. So 'invitation' can get 'accept' or 'reject', but prefers 'accept'.

[Tr: omits]

Where you find out about "insertion sequences" is in Schegloff. [Tr: omits] See: Schegloff, E. A. (1972). Notes on conversational practice: Formulating place. In D. Sudnow (Ed.) STUDIES IN SOCIAL INTERACTION (pp. 75-119). New York: Free Press. - and - Schegloff, E. A., and Sacks, H. (1974). Opening up closings. In R. Turner (Ed.) ETHNOMETHODOLOGY (pp. 233-264). Harmondsworth, UK: Penguin. [Tr: which is a slightly abridged version of the same paper from SEMIOTICA, 1973, 8, 289-327.] There's also some reference to insertion expansion, and the distinction between insertion expansion and side-sequence. And the major difference between insertion expansion and side-sequence is that side-sequences do not preserve the adjacency pair embedding structure,

whereas insertion expansions do. That's a technical differentiation that's made. And that discussed in Gail Jefferson's paper "Side Sequences" in David Sudnow's STUDIES IN SOCIAL INTERACTION [Tr: 1972, pp. 294-338, Free Press].

Ok, "Pre-sequence expansions" are where you've got tandem adjacency pairs, first-AP and second-AP. Classic is the pre-announcement sequence:

A: Guess what?
or
Heh?
or
I want to tell you something.

any of those things.

B: What?

which is the "solicit". Which then gets the "announcement"

A: I got an A on my paper.
which then gets the "assessment"

B: Terrific!

So you have the "pre-announcement sequence" and then the "base announcement sequence".

/A: Guess what?	PRE-ANNOUNCEMENT
PRE	
\B: What?	SOLICIT
/A: I got an A on my paper.	ANNOUNCEMENT
BASE	
\B: Terrific!	ASSESSMENT

And you've also got lots of other kinds of adjacency pairs that can take "pre's", that can take "pre-expansions". For example "pre-invitations":

A: Are you doing anything tonight?

B: No.

A: Would you like to go to the movies?

B: Fine.

The "pre-request":

A: I really need some help with my homework

B: Oh yeah?

A: Yeah. Could you give me half an hour?

B: Oh, no problem.

The "pre-offer":

A: We finally got some of those cookies you like.

B: You did?

A: Want one?

B: You bet!

That's the sort of way in which these work off. So 'offers', 'requests', 'invitations', 'announcements' can have pre-expansions. There are probably many others. There's also 'pre-closings' from the "Opening Up Closings", pre-closings of conversation [Tr: see Schegloff and Sacks (1974/1973) cited above.]

But, I am not exploring the properties of pre-sequences now for their own sake, I'm trying to make some other points. The issue then is that none of these orderly variations, insertion sequence, pre-sequence expansion, and repair - the canonical structure of repair, repair has a lot of other properties - none of these orderly variations in any way affect, so to speak, the logical status of the adjacency positioning convention. Nor does the logical status of that convention affect the logical status of any of these others.

The argument is that what is being worked out here is NOT a kind of claim about empirical regularity, but rather, the way I interpret the point of the work, and the way, I think, we can also develop then a criterion for distinguishing between interesting work and less interesting work, is to see it as a contribution to the analysis of the combinatorial logic of activities, primarily communicative activities. And, as I was arguing last time, the conventions, the concept of

preference, are, in my view, best construed as, if you like, in some general sense, logical claims, claims about the logical properties of these things, rather than seeing them as quasi-statistical arguments, or as probabilistic arguments, or as regularity claims. And in that sense, once a sequential structure has been identified, it holds A PRIORI. That is to say, it now has an incorrigible status in virtue of the way it is set up. Because, if you'll remember, the ways in which ACTUALLY Harvey Sacks, for the most part, and to some significant extent as well Manny Schegloff, though there are some variations in the way in which he formulates things, the way in which these conventional claims, these organizational claims, these sequential-organizational claims are set up is under the "IF-THEN" idealization.

It's NEVER said, it's NEVER argued, and has NEVER been proposed that, for example, when someone asks you a question you "should" reply to it. Or when someone asks a question, what TENDS to happen next is that somebody gives an answer to it. OR, there's a "higher probability" of an answer being the second turn to a question than, let us say, a whole bunch of other illocutionary activities. It's never set up that way. It's set up under the IF-THEN construction, just as principles of logic are. In this case it goes: IF something can be heard to be a question, THEN it makes conditionally relevant the production of an answer. AND, IF something can be heard to be a question, and then something is said in subsequent turn, IF it can be heard to be an answer to that question, THEN hear it that way. Those are the ways in which the apparatus is described. It is a, as I have argued somewhere in a paper, it is the postulation of a logical optionality system, and NOT an argument about the empirical propensities of actual speakers, OR the probability of occurrence of certain kinds of conversational objects. And if that doesn't exhaust the universe of characterizable possibilities for this, then I would be very interested. Because it seems to me that it's got to be one of these kinds of things.

Mike Lynch: Are these mutually exclusive?

Jeff Coulter: I suppose you could say that having found a set of a priori conventions, or combinatorial tolerances for an activity-type, you might then be able to say something like: "Well, it's commonly the case that...". As in "Ordinary Language Philosophy", as in Wittgenstein, as in various other places, one talks about ORDINARY USE,

the conventions of use. And there a sense, of course, of 'regularity' in there.

Caesar Mavratsas: But you always get the exceptions every once in a while, but that doesn't mean anything. Right?

Mike Lynch: Well, the exceptions, at least in that paper by Schegloff, the exception is turned into the exception that prove the rule. That's the way he tries to work with it. Because the exception has a very powerful character with respect to what he wants to do with it, because, given that he's going after 'invariance', ANY exception can destroy the claim of invariance. So, he has to work with that exception in such a way as to reclaim it as part of the system that he is explicating.

Jeff Coulter: But is it invariance?

Mike Lynch: Well, that's what they used to- In the Purdue Symposium [Tr: See Hill, R. J., and Crittenden, K. S. (Eds.) (1969). PROCEEDINGS OF THE PURDUE SYMPOSIUM ON ETHNOMETHODOLOGY. Lafayette, IN: Purdue University Press], Sacks says very specifically that they are going for invariance. {JC: That's true!} Garfinkel, if I remember from that, dis-affiliates from that a bit, but Sacks is very clear, that they are going for empirical invariance, which, I think, converges with your notion of a logical-grammar because there would be no way of distinguishing the two. If it's an a priori logic, it should show up in terms of empirical invariance.

Jeff Coulter: Except it's not 'substantive' invariance. It's not like saying: for the universe N of cases, whenever a question is asked, it gets an answer. {ML: That's true.} That would be a different form of invariance. {ML: Yes.} In the sense of a priori relation invariance, Yes. FORMAL invariance.

Mike Lynch: Ok, right. But they did hold themselves to the criterion that if it doesn't get an answer, there should be an "account" as to how the parties worked with that 'absence' to make it an accountable absence, rather than just an exception to a general rule, that nonetheless holds 75 percent of the time.

Jeff Coulter: You see - yeah - Ok - but I think what I'm trying to argue is NOT so much that- Let's take the Schegloff case. [Tr: See

Schegloff, E. A. (1968). Sequencing in conversational openings. AMERICAN ANTHROPOLOGIST, 70, 1075-1095] It seems to me that it's not that he wants to argue that ALL telephone openings have a specific set of substantive properties, but rather that there can be now an incorrigible a priori sort of set of rules that provide for, and can encompass, the empirical variance that he discovers. The paper starts out with an invariance claim, the "distribution rule". On making a call to someone: Answerer speaks first. {ML: Sure} That's set up in the first part of the paper as an INVARIANT distribution rule. Then he finds from a corpus of 500 calls ONE case in which caller speaks first. NOW, what do you do? If it were an empirical invariance claim, the claim is defeated, because you have a case that shows it's NOT INVARIANT.

Caesar Mavratsas: Unless you account for it.

Jeff Coulter: No, it shows it's not invariant, whatever you do with it. If you get a case where answerer does not speak first, then the claim that "answerer speaks first" is an invariant distribution rule is defeated. Whatever you may say about it subsequently, it's defeated it. The tact that Schegloff takes strikes me as methodologically IDENTICAL - and I've argued this in a paper, and I think he's come to see it, his methodological treatment is identical to the way in which a logical grammarian treats a case of an oddity of use, or a deviant case. Which is: instead of proposing that the principle is completely defeated, the principle is now subsumed under a larger principle that can encompass this as a particularization. The way in which he does it is, of course, brilliant and classic. He says: Let us now try to give an account for the case in which caller speaks first. So, he DOES a sort of deviant case analysis, and he says: The way in which we can characterize what was occurring when the caller spoke first was that the following chain of events were in process: The telephone rang. It was picked up and the answerer did not speak. The caller, therefore, spoke. And then the answerer replied. NOW, IF one treats the ringing of the telephone as a "summons", as, if you like, a mechanical analogue of "HEH", or a call, then you can find that the failure of the answerer to speak first occasions a re-cycling of the summons. As we know independently from summons-answer sequences, in ordinary interaction, independent of telephone calls:

A: HEY, JOE.

(silence)

A: JOE.

Right? So, the telephone ringing is treated, now, as a sort of an analogue of the summons. The one called, not speaking, is now treated not as a violation of the answer speaks first distribution rule, but as a violation of the summons-answer sequence, which now occasions its re-cycling. So that the caller speaking first is now seen as a re-cycling of the summons-answer sequence. So what we get now is a way of treating summons-answer sequences as the broader organizational structure subsumable under which is the case in which the answerer did not speak first. Where that is seen now as a case of a "noticeable absence" in an adjacency pair. So, summons occasions answer. If it doesn't get answer, then that adjacency pair can get re-cycled. So, invoking that, the trick was to see that the ringing of the telephone is itself a summons, rather than something independent of the interaction between the parties.

Caesar Mavratsas: So you still have an invariance claim. Once you get the second structure {JC: Right}, you have invariance.

Jeff Coulter: But it's not the same kind of invariance as the answerer speaks first claim. {CM: No} It is an invariance. It is a FORMAL invariance, in the sense that- Well the way it is set-up is like this: Summons make Answers conditionally relevant nexts. The failure to produce an answer as a conditionally relevant next can provide for a re-cycling of the adjacency pair/first-pair-part. That's the way it is set up. That is an a priori structure now that's incorrigible with respect to any next case.

But answerer speaks first, in the first part of the paper, where that's set up as like the finding from the study of the corpus, that was set up as an empirical distributional invariance. And now finding that there was a case in which answerer did not speak first... If it was set up as something like: It is preferred that answerers speak first, or it is the convention that answerers speak first, then to find a case where an answerer doesn't speak first is neither here nor there, in a sense, because a convention and a rule can always be broken. You don't refute a rule by finding a counter-example, necessarily. You can simply say

someone's broken the rule, but the rule is still there to be broken. But the reason why it was a crisis in the research, so to speak, was that, I think-- and it was a productive crisis, because it generated a different conception of the way to describe the machinery. That's how I read it anyway. Otherwise it would be the most-- One case in 500. Ok, so sometimes the rules is broken. No, no. It becomes an issue insofar as now it is to be accounted for in terms of the a priori conventionality structure that can encompass it, that can provide for it, that can preserve its intelligibility, and provide for the orientations of persons in telephone conversation, communication.

Dave Bogen: One thing that, if I remember correctly, then that, in this instance, at this deviant case: caller's first utterance would have to be something then like "Hello?" [Tr: with rising intonation] [Tr: omits] The sort of powerful thing about it is that the whole thing stands or falls on this one deviant case not being "This is Joe calling for..." in that next. That wouldn't provide for that sequence. So, the thing stands or falls on just that particular production, in just that instance.

Jeff Coulter: No, no. Because the thing is like an intuition pump. To find that it was a "Hello?", to find that it had a summoning character was an intuition pump for Schegloff. It got him to see that it had the character of a summons. That was now a summons type, "Hello?", which led him, by a stroke of insight, to see that the telephone's ringing was itself a prior summons that hadn't gotten an answer, a response, that now could generate the possibility for 'caller speaks first' as an instantiation of a re-cycling of the S-A sequence. That's all that's being said. Now that's not to say that that now provides for a way of incorporating any possibly deviant case. Like some kids can pick up the telephone, or somebody maybe hard of hearing, and the telephone call won't work properly as a matter of empirical fact.

Dave Bogen: I was saying something different. {JC: Ok, go ahead. I'm sorry I missed the point.} Maybe it wasn't that interesting a point and that's why it went off the rails, but of course that serves as sort of guidance for this further adjustment, or reformulation at a more abstract level of what it's doing. But the interesting thing to me is the way in which that ONE deviant case, if you want to call it, provides the

intuition pump, it's that click that happened there, and you can see it.

Jeff Coulter: YES! And you only needed one!

Dave Bogen: And, if the next case comes along, and you find your second deviant case from this distributional rule, and you can't find any other reason for it, yet the person comes in with this "Hi, this is Joe calling", then that's possibly cause for, again, reformulation. {JC: Right} That's all that I was pointing out. So that the formulation itself, although highly abstract, is sensitive to the particular case. That's its structure of invariance. It has to be sensitive to each particular case that might then again count as a deviant case, subject to reformulation. {JC: Mmm mmh}

Mike Lynch: I think that makes this whole notion of a prioristic structure problematic. I've never heard Sacks or Schegloff talking about going for an a prioristic structure. {JC: No, they didn't use the term.} This is your claim about it. {JC: yes} They would be claiming to go for, at least in some of their work, to be going for invariance as discoverable invariance. So, that they would be vulnerable to any next exception to whatever invariant organization you've mapped out at the present. So that that case from Schegloff - I think he presented it in the way he wrote up that article, his strategy writing up that article, as a demonstration of how they were going after invariance. Whereas the exception was not just something to write off in terms of a statistical law.

Jeff Coulter: Empirical, sort of, distributional invariance is a strange goal.

Mike Lynch: Well it 's not distributional in the statistical sense. It's organizational. But it's discoverable at the same time, it's not something that-

Jeff Coulter: This business of invariance perhaps I'm not getting very clear about. The way in which the summons-answer sequence is now to be characterized as a result of the work done on the deviant case, is it's now to set up as one of those adjacency pairs in which if the second-pair-part is not produced, it CAN occasion its recycling. That is the way I've understood it. Now, if what's being said is: When you get a summons you get an answer, or you generally get an answer, but if you

don't get an answer, then you WILL get, or INVARIANTLY get a recycling, IF that's what they're saying, IF that's what Schegloff was trying to say, then I think he's sunk himself unnecessarily. Because it's either one or the other, right? Am I missing a subtlety in this?

Lena Jayyusi: Isn't this where the notion of programmatic relevance enters? The idea that certain actions make programmatically relevant certain others.

Jeff Coulter: Oh, it's conditional relevance, not programmatic relevance, in Schegloff.

Lena Jayyusi: In Sacks, isn't there the notion of programmatic relevance?

Jeff Coulter: Yeah, but that's around categories. They're not interchangeable. Conditional relevance is only a sequential organizational concept.

Lena Jayyusi: Yeah, but I understand them as sort of logically the same kind. {JC: Yeah, they are} And the point is that what we're talking about is systematic relevances. If you don't get one thing which is relevant in this position, then other things that would occur in the second-pair-part are HEARABLE by virtue of that which was in the first place relevant and expected.

Jeff Coulter: Hearable, relevant - these are a prioristic.

Lena Jayyusi: Exactly, that's the point.

Jeff Coulter: Right. It's not that they WILL be heard, or they MUST be heard, or they will invariantly be heard, or invariantly be so produced.

You see, I can't see, Mike, what the concept of invariance is unless it's either the notion of a set of rules that now described is invariant now. In other words you don't change the formulation of the rules. OR, you mean by invariance the actual empirical occurrences invariantly occur, so to speak. IF there is some other sense of invariance, I've missed it.

Mike Lynch: I guess I'm confused about what you're saying when you say "a prioristic".

Jeff Coulter: Oh, that you can derive an a priori claim, i.e., one that is true prior to further investigation. It's not the analytic sense of a priori. {CM: It's discoverable} Yes, in other words, there are discoverable a priori's. Like, for example, given the rules of chess, it's a priori true that a king cannot be mated by, and then are two pieces - I forget which they are - bishop and knight - I don't remember which they are. It's a priori true. For any game of chess that could ever be played, given the rules...

Eileen Crist: It's by definition, though.

Jeff Coulter: Well, I don't want to say 'by definition', you see, because that will conflate the notion of a prioricity with the notion of analyticity.

Caesar Mavratsas: Can you say something about the difference.

Jeff Coulter: Like it's analytically true, if you like, that '1' is a number. But it's discoverably, a priori true that this proposition about chess that the king cannot be mated by... The point of the argument is to say that you could derive an a priori truth about the organizational possibilities of communication from ONE case. Supposing Schegloff had hit on that one case FIRST, using as his just background intuition that fact that when I pick up the phone, normally the answerer speaks first - but BANG, he's just hit on that one case where it doesn't. He needn't wade through now the $n + 1,000$ th cases to undermine the a priori structure for summons-answers, that it is now a conventionally provided for possibility, a priori. Given a summons, answer is conditionally relevant. Failing answer as conditionally relevant next production, S-A sequence may be recycled. That stands. That's a priori true. No new information can undermine it. All that can be done now is you can ADD to it, or spin off new structures from it. Just as finding that you can have three questions in a row doesn't undermine the adjacency positioning convention. That's incorrigible as formulated. Rather now when you find apparent violations of it, seek for specifications of new phenomena. Because you'll know that it is not an adjacency pair you're any longer dealing with.

[Tr: pause]

You're looking terribly pained. People are looking awfully pained.

[Tr: general laughter]

Dave Bogen: Well, no. There are several issues in there that I find really difficult to either, on the one hand, understand, or accept. One of them is using the chess analogue. Chess seems to be more of a self-contained kind of... Conversation is not a game of chess. I'm not sure the analogue succeeds in that sense. That is to say - and I think I'm picking up on the way Mike is expressing the point about invariance-

Jeff Coulter: But there are rules.

Dave Bogen: I definitely have no problem with that. But the notion of invariance there, at least the way Mike is expressing it, sounds like it's serving as a sort of analytical principle, or limiting case. That is that the limit of your formulations is something like coming up with an intuitionistic formulation that would capture all cases. So, the turn-taking formulation seems to capture all cases. If turns are taken, then they have to go this way, and intuitively you can see that there are no alternatives. And in that sense then it's incorrigible. {JC: Yes} On the other hand, the status, in Schegloff's piece, the status of the formulation, and the way in which the piece is written, is to show you that something that was taken as an incorrigible formulation, stands in need of reformulation, not just in terms of 'adding to' or in terms of 'expansion of the concept' exactly, but a real reformulation. And it would lead you to believe that-

Jeff Coulter: ONLY if it's construed as an empirical invariance claim. There is nothing wrong with saying that conventionally answerer speaks first.

Dave Bogen: No. The formal statement stood in need of- What we first thought there, and what we thought would handle all the cases stands in need of reformulation given this deviant case. And the question is: What's the logical relationship between that first formulation, and the next? And then to recognize that likewise this now taken as incorrigible, a priori formulation COULD, in confrontation with another set of cases that you haven't seen, serving as elaborate reminders, want another reformulation, a radical reformulation of sorts.

Jeff Coulter: No, I don't see it that way.

Mike Lynch: So, you are claiming that - we should get the paper out, because I haven't had this in a long time, but - Schegloff makes characterization 1, that you claim is just an empirical regularity, which is confronted with a counter example. And then he not only revises the formulation of empirical regularity, but he goes to a new kind of {JC: conceptualization}. Ok. Now, I never understood the paper that way. I don't know you may be right or wrong. I had always understood it from the way that David just explicated that, in a sense, Schegloff is doing a demonstration of the NON-incorrigibility of Conversational Analytic findings, or abstract formulations, the formal analysis, and that it is essentially defeasible in the face of further instances that can be discovered.

Jeff Coulter: But you can't defeat the summons-answer sequence argument as it's finally set up. How would you defeat the following argument: If - {CM: It's got to be impossible} - Yeah, it's a question of an ordered array of logical possibilities - If you hear something as a summons, that makes answer conditionally relevant next, failing the production of a conditionally relevant next, the adjacency pair may be recycled. Nothing in the world can destroy that. That's a structured set of logical options.

Mike Lynch: But if you find-

Jeff Coulter: It's just like interrupting you now doesn't in any way violate the, or lead to some sense of the corrigibility of the 'current speaker selects next', or 'speaker self-selects' rules.

Mike Lynch: Well, let me get back to something David said earlier about the specific instance that in the case that he discussed counts as a counter-example. It's not just that it was logically in conflict with rule, but that the instance in its internal detail was hearably oriented to the absence of a summons. So that when he lays out the summons-answer sequence, if you were to find in further inspection of tapes, cases that do not fall in line with that set of formal rules and which have no hearable orientation to the absence of the expectable actions, then you're in trouble.

Jeff Coulter: No, then you've got a new sequence structure.

Mike Lynch: Then you've got a critique of Conversational Analysis.

Jeff Coulter: No, I've got a critique of the way in which it's self-understood, or self-misunderstood, perhaps, according to some practitioners. That's all.

Remember the case of the adjacency pair, and then coming across the three questions as Sacks sets it up in this lecture. He's talking about Q-A chaining, which is like the recycling of the adjacency pair, question-answer, then you get next question-next answer, etc. And then he said: Well what about this? Bang! And you've three questions in a row. Then he starts talking about canonical organization of repair: Repairable item, targeting or non-targeting repair initiator - though he's not using those terms at that time, he's using something like 'correction elicitor' - then correction, or repair.

So, you use an apparent variation off of, let's say, the canonical openings of telephone conversations NOT to destroy the S-A, the summons-answer logical optionality system, but rather now that's used incorrigibly as a basis for seeing what this new thing is, in its detail, as a new organizational phenomenon.

So, the notion of incorrigibility, I think, is preferable to invariance here. Now, how Schegloff got this, perhaps malgre soi, against himself, in spite of himself, was because he was tacitly treating the rule 'answerer speaks first', in the domain of telephone conversation, as tacitly treating it - perhaps not self-consciously, but tacitly treating it - as IF it were an invariance claim. And I think that's a helpful way of talking about it. Finding now the deviation, it's NOT necessary, you see, to be worried about one deviation, IF you haven't already set the thing up as an empirical invariance claim. So, it would make no real difference if he had been operating on a logicist program explicitly, so to speak, from the start to say: Here's a rule, here's a convention: answerer speaks first in the domain of telephone conversation. Of course there are some exceptions. But it just so happened that his misunderstanding of the status of rules, so to speak, in the first part of the paper, as I see it, led him to a reformulation, not just of the

empirical corpus, but of the logical STATUS of the claim itself. Now it's possible world status stuff, it's 'may's', 'might's', 'can's', 'could's', rather than 'answerer speaks first' - which could have been set up as 'answerer conventionally speaks first', or 'answerer may speak first, is entitled to speak first'. If it had been set up that way, there would have been no issue.

At the end of the paper, the thing is set up "Modally", in the sense of modal logic, i.e., in the sense of not necessity but using the subjunctive conditionals: may, can, is entitled to. Or deontic actually, modally and deontically. And indeed the logic of Conversation Analytic claims are both modal and deontic. Normative, in other words, it uses the 'may' of permission, not the 'may' of 'may occur'. 'It may happen', 'it may rain' is not 'it's entitled to rain'. There's deontic 'may' and the 'might', and the 'could' and the 'can', and then there's the modal, or the subjunctive.

I see the work as a contribution to combinatorial logical analysis of practical action. And I think that that way of construing it enables us to escape once and for all from the empiricist constraints that some of the practitioners want to put on the field, which would include such things as that you can only make assertions about the logical properties of conduct based upon what is endogenous to the data construed as a transcript or a tape-recording. That's number one. Number two: that your argumentation is now to be thought of not in terms of like the production of theorem-like claims, or logical a priori's, but rather ENDLESSLY corrigible, even trivially corrigible, observations of regularities. I don't find it in any way as powerful a way of construing what Ethno. studies of communication can be like to set it up as an empiricist program into the sheer contingent regularities of conversation. As Harvey Sacks once said to a class, he said: Look there are billions of these things. There are billions of conversations in the world, simultaneously, historically. There are billions of them, trillions of them, an indefinite set of them. Therefore the way in which we speak about the rules, or the principles is in one of two ways: Either it's got to be subject to statistical verification, and he did toy with that, I'm sure he did, and I think that was under the impact of Schegloff at one point, but that's a speculation - OR, it's got to be seen as connected with the traditions of the logic of language, which he was

well aware of: Wittgenstein, and the rest of it.

See, one thing that you can now start to rule-out by using these criteria for the establishment and adjudication of analytical claims, one thing you can start to rule-out are those weak inductivist, empiricist papers in the field. Where somebody has - and I would include a part of my own 'arguments' paper in this condemnation, not the whole of it, but at least a chunk of it - where what you find is people have some set of data, like 7 cases, 8 cases, 3 cases, or 500 cases of, say, a particular activity system, like argumentation, or service-encounters, or doctor-patient interaction. And now what they are trying to do is to INDUCE regularities of sequential organization that they then propose are particular to the broader set. That is an empiricist, inductivist program, and can't possibly work, because the set is indefinite. And you cannot have a probability where you do not know the universe of possibilities. So, probabilistic articulation is ruled out. You certainly can't have empirical invariance claims, and so at best, you end up with these weak empirical regularity claims, that are sort of endlessly defeasible with respect to any next case. That is not where I find the power. So, I would argue that the structures like adjacency pair, pre-sequence expansion, insertion expansion, repair, and the turn-taking apparatus constitute the collection of a priori structures that have been discovered, that have been elucidated. AND if CA is to go any further, we need to elucidate a priori structures of that power. It's of no interest to me to read that: for some set of instances, people tend to complain like that to each other. Because in any next case you might find that they don't; they complain like THAT to each other. And I don't know the universe of cases, and neither does anybody. But if you are able to generate - and you could generate on the basis of just ONE case - a powerful a priori theorem, or principle, then that seems to me to be what we're after in elucidating the logic of practical action.

Caesar Mavratsas: So, why couldn't we work without empirical data though?

Jeff Coulter: This I'm not sure of. {CM: Why not?} Here's the rub: It depends on what your problem is. The fact of the matter is that our memory, and our own raw, unaided intuition is sometimes not going to be sufficient to give us what we need. We need to be reminded of the ways

of the world in detail. And the only way to be reminded of the ways of the world in detail is, I think, still through the use of transcription of naturally occurring phenomena. But that doesn't mean that for some kinds of arguments about the logic of practical action, or the properties of it, that you are barred from a hypothetical scenario. I don't have to wait around for ten years for some REAL case to pop up when I have my tape-recorder on to hear somebody saying something to someone else that instantiates a property I'm interested in, because I know damn well that I've heard it before, and I can come up with a case of it. It depends on the purpose of analysis. For the more detailed things, for the more subtle things, it's clear our own intuition and our own memory isn't sufficient. But I don't think there is an absolute barrier to it; I think it depends on the point.

Mike Lynch: I think the big issue too is that what Sacks, and Schegloff, and Jefferson, and so forth, were going for is not a monological structure that we can dredge out of our own linguistic intuition, but these sorts of structures that are occasioned by somebody saying JUST THIS, and somebody else responding to JUST THAT in real time, and in which case just how long a pause might be, just how something is intelligible, and so forth, would be an occasion for something following it. That these are things that don't seem to organize themselves very well in terms of our reflective intuitions about what kinds of sentences are sensible, and how you might formulate a request, and things like that, which all come from a kind of a stable social intuition that develops over the life-time. That these are things that transact persons, and in a sense, they transact memories too.

Caesar Mavratsas: Yeah, but if you accept that there are a priori structures, you could in principle discover them without any empirical data. No? {JC: No}

Mike Lynch: Well I don't accept that because I don't understand what an a priori structure could be on that grounding.

Jeff Coulter: Right. Let's have a break for five minutes, and I'll bring in some further examples, and try to convince you about a prioricity, which you're confusing with analyticity. Because your conception of a prioricity is that it can be derived from pure ratiocination. Now, some a priori claims can be,

but Kripke and others, and I think it's shown clearly in Conversation Analysis above anything else in Sociology, and the behavioral sciences anyway, shown that some a priori relations, a priori properties really cannot be derived from pure ratiocination. You NEED the cases to see.

Caesar Mavratsas: But what is the difference? I'm not clear about the difference. What's an analytic claim, and what's an a priori?

Jeff Coulter: An analytic claim is something that is, so to speak, true in virtue of the meanings of words, or by definition, or that can be derived purely from our pre-existing experience, without the necessity of actually looking observationally at the world to get there. I don't need to go around looking at marriages to know that, or any $n + 1$ th case of marriages, to know that under circumstances x , y , and z , when someone says "I do", he or she married somebody.

Caesar Mavratsas: So in cases of a priori claims you have to...

Jeff Coulter: Let's leave it for a moment. I'll run this through more systematically when we come back.

Caesar Mavratsas: Yeah. I don't see the difference though.

[Tr: brief break]

Caesar Mavratsas: Is there any difference from the way Kant was using the terms?

Jeff Coulter: I can't remember.

Caesar Mavratsas: Because you have a priori analytic, and a priori synthetic.

Jeff Coulter: That's true. Yeah, I don't remember off hand, Caesar.

Well, whatever Kant had to say, and I think these are Kantian distinctions {CM: Yeah}, or rather the way I'm talking about them is the way Kant would talk about them, but I'm not really all that bothered whether I've got Kant right. This is the way I'm thinking about it.

Analytic is something like: It is analytic to the concept of promising that when you promise someone, you place yourself under an obligation. I was making an analyticity argument last week about invitations and the preference for acceptance. Those were just mentioned. It's analytic to the concept of invitation that it prefers acceptance, i.e., it's part of what it means to invite someone. And that if you are 'inviting' somebody, knowing and with the purpose of having it turned down, then you're not really inviting them. You APPEAR to be inviting them, but you're not. But you're doing something that is parasitic upon inviting.

Now, a priori here I'm using to mean: something belongs to the realm of a priori knowledge IF it can be deduced from a given set of arrangements, or phenomena, and then is impervious to transformation in respect of ANYTHING ELSE IN THE WORLD.

Caesar Mavratsas: But doesn't that hold for an analytic claim?

Jeff Coulter: Yes it does. But it's not true the other way round. It's not that all a priori claims are claims of analyticity.

Caesar Mavratsas: But can you give an example?

Jeff Coulter: I will give you an example.

Suppose that while watching a game of chess I see two Pawns of the same color standing in the same column. Then I say: "One of them must have taken an opposing piece in a previous move." How do I know this? Is it sufficient to say that in all chess games we ever witnessed this correlation held? No, GIVEN THE RULES OF THE GAME, the relation holds A PRIORI; the contrary is not something unusual or unlikely: [Given the rules of the game - JC] it is inconceivable. Nor is this an analytic connection in the Kantian sense of the term: [I'm quoting from Vendler here, so he's actually talking to this point - JC] any given position on the board is perfectly comprehensible without historical data (think of chess puzzles). One might never realize the connection, but once it is noticed, [and this is the key -JC] one sees that it

cannot be otherwise. [It is incorrigible - JC] [Tr: Vendler, Z. (1971). Linguistics and the A PRIORI. In C. Lyas (Ed.) PHILOSOPHY AND LINGUISTICS. New York: Macmillan. pp. 255-256 -- quoted in Coulter, J. (1983). Contingent and A PRIORI structures in sequential analysis. HUMAN STUDIES, 6, 361-376. See p. 368.]

Now, the question arises as to the relationship between an a priori proposition about, say, the organization of interaction, and an a posteriori one, one that's based upon looking at cases. And here I'm drawing upon Kripke:

[Some philosophers] think that if something belongs to the realm of A PRIORI knowledge, it couldn't possibly be known empirically. This is just a mistake. Something may belong to the realm of such statements that can be known A PRIORI but may still be known by particular people on the basis of experience. To give a really commonsense example: anyone who has worked with a computing machine knows that the computing machine may give an answer to whether such and such a number is prime. No one has calculated or proved that the number is prime. We, then, if we believe that the number is prime, believe it on the basis of our knowledge of the laws of physics, the construction of the machine, and so on. We therefore do not believe this on the basis of purely A PRIORI evidence. We believe it (if anything is A POSTERIORI at all) on the basis of A POSTERIORI evidence. Nevertheless, maybe this could be known A PRIORI by someone who made the requisite calculations. So "can be known A PRIORI" doesn't mean "must be known A PRIORI". [Tr: Kripke, S. A. (1972). Naming and necessity. In D. Davidson and G. Harman (Eds.) SEMANTICS OF NATURAL LANGUAGE. New York: D. Reidel. p. 261. Cited in Coulter (1983), cited above.]

We can believe something which is a priori true on the basis of a posteriori evidence. For example, Cantor when he generated the theorem that for any

number the sum of the first n odd integers is n squared.

Caesar Mavratsas: He had to find it. He had to discover it.

Jeff Coulter: He had to find it; he had to discover it. But he wasn't like discovering a new species of beetle. It was in a sense already there, i.e., it's a priori true that for any n , the sum of the first n odd integers is n squared. And if he'd been an unbelievable genius, he could have perhaps worked it out in his head. But, no, what he had to do in order to derive that theorem was to put numbers on the table, and play around with them. Literally mess with the numbers. But the permutations of numbers on the pieces of paper that he was playing with served him as a kind of quasi-empirical data. I say quasi-empirical because I treat CA data as quasi-empirical in the sense that by implicit contrast to the ways in which empirical materials, or data, in at least some canonical version of what scientific evidence is about, is used in different ways. Here data is used in the form of elaborate reminders, as I put it in this article. That's the best I can do. It's not wholly satisfactory, but data seems to-

Caesar Mavratsas: So what would make that claim about the first n odd integers an analytic one as opposed to an a priori one? If somebody discovered it just like that, I mean if somebody woke up one morning and said that's the case.

Jeff Coulter: Well, I don't think too much hangs on it except that one might want to say it's not derivable from the meaning of 'number'.

Mike Lynch: Well is it possible to have a discovered a priori modify your analytic concept, or maybe even overthrow it? To conflict with it?

Jeff Coulter: What sort of concept do you mean Mike, I'm not quite clear?

Mike Lynch: Well, you're talking about analytic which, things which hold true by definition of a concept. {JC: Yeah} So let's say we have a notion of what a request is by definition, and that we could explicate these analytically. {JC: Properties of request. Yeah.} And maybe we would disagree with each other on some points, but... If you were to do Conversational Analysis as you've described it, and come up with a priori's on the basis of looking at recognizable

requests, is it possible that you could run into a conflict between what you discover about requests, and this previous analytic notion of what a request was?

Jeff Coulter: Well, maybe with my defective notion of what a request was, but you can't overturn what a request is in the language.

Caesar Mavratsas: What's the difference between the analytic claim- I mean if you start by defining what a request is, you can expect-

Jeff Coulter: Well, we'd give examples rather than define it. {CM and ML: Yeah} The logical grammar of requests. {CM: Yeah}

Mike Lynch: But then what is it in the language if you have defective ideas of what it is? Is it that that's a thing for which you cannot have an analytic concept then, if we all have defective ideas about it.

Jeff Coulter: Oh, but we don't. The only way in which a logical grammatical analysis of a concept can be overthrown is if you got it wrong. Unless you're denying that concepts have logical grammars, which I think is a tall order.

Mike Lynch: Well, I think that one of the claims in Conversational Analysis is this whole idea if you can't imagine it, Sacks used to complain Goffman made up some wonderful scenarios on the basis imagined access to ordinary settings of life. And it was very plausible. Right? And they had that analytic character to them, although I don't think they were expressed quite that way. And Sacks would complain that when you looked at the data, it wasn't like that at all. That was a gloss, or worse; it was wrong in detail. And he would use that term: It was wrong in detail. So what I'm asking about is: How does that bear on this notion of analyticity versus a prioricity?

Jeff Coulter: Well, Speech Act theory deals with the, I suppose, if you like, the analytic, or the logico-grammatical.

Mike Lynch: Right. And that would be another one where the Conversational Analysts take them on, and say they are wrong in detail.

Jeff Coulter: Ok. Let me make this claim then: Given a certain reworking of, say, Searle's work on promising, I believe it to be incorrigibly

a priori true. And, if you like, it's in this discussion a revelation of the Kantian analytics of promising. Though, look, we've been doing Wittgenstein. Let's talk about the logical grammar of promising. The problem with the way Searle sets it up is he sets it up as a production-recognition model. And so do many Speech Act theorists set up their logico-grammatical elucidations of concepts as if they were production-recognition models. So, what he says is: Here are a set of necessary and sufficient conditions for producing and recognizing promises. No! Production and comprehension are really not quite what's at issue, except at a most abstract sense, I suppose. Since you certainly couldn't hear ANYTHING accountably, rationally as a promise, unless what you have heard was a promise by the criteria of the logical grammar of promising. One would want to say that! Because you can't hear any-ole-thing rationally as a promise. So it has a logical grammar. But Searle sets up his specification of the logical grammar of promising as if here's a set of necessary and sufficient conditions that we sort of USE in producing and comprehending. Because he's trying to marry Wittgenstein and Chomsky. This is where Searle goes wrong, he's all the time trying to marry Wittgenstein and Austin with Noam Chomsky. So he wants production-recognition things.

Now, Conversation Analysis can provide us with lots and lots of detailed cases of persons making promises, and we can find out a lot about the organizational properties of promising as a course of action and interaction, but to figure out that these are cases of promising in the first place requires that we draw upon the concept of 'promise'. And the concept of promise has a logical grammar, so not anything goes.

And this brings us to the question of the identification of the turns in a transcript. The very selection of a case as a case of an 'x' requires that we already have the concept of an 'x'. And I have seen some people trying to do the kind of thing you were posing as a hypothetical where they say: Here's some cases in which people are inviting people with the intention of having it refused, say. We saw one the other night, and we've got it right here. No! That's not an invitation. Now, how come? And I'd have to make an argument. But the argument would at least rest in part upon the logical grammar of invitation. Which in turn, of course, the specification of the logical grammar of a concept in ITS turn can only be done as an elucidation of canons of proper use.

Caesar Mavratsas: So it's a priori and not analytic - the logical grammar of a concept.

Jeff Coulter: I don't want to use the notion of analytic, because what analytic means in canonical form in Kant is true by the definition of the meanings of words. We know from Wittgenstein that the meaning of words is not givable, certainly not invariantly, by definition. And when it is given by definition - only for certain purposes. So meaning and definition aren't the same. So, let's delete that and talk about the logical grammar. Unless you want to make a reactionary argument for Kant versus Wittgenstein. Let's talk about the logical grammar.

Now, the issue is what is Speech Act theory doing? Speech Act theory IS extending logical grammar to the analysis of illocutionary concepts. It's an analysis of the logic of illocutionary concepts. It seems to me that: so far so good. And a lot of Speech Act Theory is completely undefeated by CA. Where you can start making inroads - and this is why I'm dying to see the Searle-Schegloff debate - because Searle - I know in advance because Sharrock saw a little bit of it - is accusing Schegloff of empiricism. And I think he's got a point. So I'm dying to see this debate. It's going to be a hot one. Because one of the things I think it's fair to say about Searle, he sets up the apparatus, so to speak, the rules for promising in a way that blinds us to what in fact he's doing. So also does Sacks, Schegloff and Jefferson, quite often, set up the principled analytic claims in the wrong way, thereby blinding the whole field to what we're doing. We are doing socio-logic. I take Ethnomethodology to be a development and an extension of a certain conception of logical analysis of conduct. It's NOT empiricIST, but, of course, it's always got an empirical base. The empirical base operates to expose for us the taken-for-granted, to give us the detail that we might not otherwise recollect properly, to operate as an intuition pump. It does all of those things. But it does not serve as a SAMPLE of anything. It's not like we're sampling the world. It's not like we're producing probability frequencies for the occurrence of things. And I don't think it's an empirical invariance program either. And I can't think of anything else it could be. Now if there is something else it could be, other than a prioristic in the Kripkean sense, other than empirical regularity searching, then I'm sunk.

Mike Lynch: Well what about this issue of singularity? I mean to take this example from Judy Davidson [Tr: see footnote 4, page 126 in Davidson, J. (1984). Subsequent versions of invitations, offers, requests, and proposals dealing with potential or actual rejection. In J. M. Atkinson and J. Heritage (Eds.) STRUCTURES OF SOCIAL ACTION (pp. 102-128). Cambridge, UK: Cambridge University Press.]

Jeff Coulter: Yeah. Could I do another example first before we do that?

One of the places you can take this argument, if you're persuaded by it, is to note that - and it's interesting to see Schegloff trying to do it contemporaneously - though for many a year Schegloff used to chasten characters like me for not using a big enough collection of cases - now he's into single case analysis. The key thing is HOW you use a single case to extrapolate a theorem or a quasi-theorem. Let me show you. I believe that, for example, you could say something like, from my own materials:

A: Dr. K asked us to take you to hospital.

B: I'm alright as I am.

This is real stuff; it really was said. It's a psychiatric social worker talking to a prospective patient. It really was said, though I don't care whether it was really said or not.

From just that [Tr: two utterances above], looking at that, one can derive the following theoremic claim, namely that: The production of a rejection of an offer, or the counter-mandling of an offer, or of an order, can work at the level of presupposition, rather than at the level of assertion. So, for example, taken just purely formalistically according to the canons of propositional logic, "Dr. K asked us to take you to hospital" might sound like an announcement of a task. "I'm alright as I am" might sound like simply a personal state disclosure. We come to hear them as something like 'offer, quasi-order' - it's an offer that can't be refused, so it's kind of an odd sort of offer, in virtue of the category auspices of the speaker - "I'm alright as I am" is NOT heard as an unsolicited personal state disclosure. He's not arbitrarily telling you how he feels. He is, in some sense, rejecting that [Tr: "Dr. K asked us to take you to hospital."].

It's a kind of thing I call a "concessionary rejection". Now from this simple case, and I don't need any more cases, though you could flood me with millions of them, I don't need any more cases to now have discovered, or be able to elucidate the following property of rejections of offers, namely that, they can work at the level of presupposition, where the assertion provides the reason for the rejection that is visible only at the level of presupposition.

Mike Lynch: That would be a very peculiar claim to make in Conversational Analysis since there's no talk of the level of presupposition.

Jeff Coulter: Yes, Ok. But let me say this: To determine that that is a rejection [Tr: "I'm alright as I am."], after all CA may not be interested in theorems of that kind, or principles of that kind, but they have to be interested in the identification of turn-types, even heuristically. To find that that's a rejection, as distinct from an unsolicited personal-state disclosure, IS to tacitly assign to it- To find that it is a second-pair-part, and not a completely anomalous remark that is out of sequence, to find that that indeed is a second-pair-part, you have to figure, you have to find that things like rejections can operate at the level of presupposition. Now, all I've done is make explicit one element of the tacit apparatus that enables a CA analyst, for example, and the rest of us as mundane analysts, to find that that is a rejection. That's all. I'm not making any great claims. There are more interesting principles and rules that can be had out of this stuff. But it only needs one case. And it does bear upon the question because the identifiability of something as a case of a Speech Act type, or an illocutionary act, involves more than simply, clearly, more than simply sequential location. And it may involve such things as the tacit assignation of presuppositions, the determination of- Well, for example, what one would now go on to say are more interesting theorems that you could derive from this would be to say: Assignable presupposition is sequentially sensitive, i.e., that that is what is presupposed by that [Tr: "I'm alright as I am".] is a function of its sequential location, which in its turn is dialectically elaborated by finding that to be possibly presupposable. In other words, take that right out of this sequence, "I'm alright as I am", in some other sequential position does not have that presupposition. So, assignable presupposition is contingent upon sequential location. That would

be another theorem. And you could perhaps derive some more theorems out of this stuff which it might be useful to look at additional cases to see that it really seems to hold. But all you're doing is you're checking your intuition which might be quite satisfiable by one case, and one case alone, if it's analyzed correctly. And it seems to me that the mythology about the use of collections of cases is an empiricist mythology, which is not to say one should never use collections. But it's not that by getting a collection of things that you are, in some sense, beefing up the analysis, or making it more credible. On the contrary, if your first analytic insight is wrong, it doesn't matter how many you've got.

Like I don't need to have seen $n + 1$ games of chess to realize that when I see two pawns of the same color standing on the same column, I derive the a priori synthetic truth: One of them must have taken an opposing piece in a previous move. Though I never saw that happen. Given the rules of chess. Given the rules of conversation, the assignability of presupposition is sensitive to sequence and vice versa. The assignability of something as a sequentially appropriate object may be contingent upon what it hearably presupposes. So we've got all of that stuff working. And it's all a matter of the interplay between logical grammar and sequential organization.

Mike Lynch: So why would it be that then to conduct an analysis in Conversational Analysis, to say that that works out as what you said it was {JC: an a priori} - yeah - because it works against, or works on what it hearably presupposes. That certainly would be rejected as a formulation of Conversational Analysis. Right?

Jeff Coulter: More's the pity. Because whilst if it is so rejected, then they have rejected an element of the Ethnomethodological programme as I understand it, which is that one is to try to elucidate how things have the properties they have. And one of the things that they never really seem to be interested in, except at the most elementary level of sequential position, is how it is that you can figure what the turn might be doing. Granted questions can occasion answers as conditionally relevant nexts. What sorts of other things might militate against the hearability of something as an answer when it is in the sequentially appropriate position? That will require matters such as the invocation of assignable membership categories, it may involve

analysis of assignable presuppositions and implications, it may involve matters of utterance design - about which Sacks did say some things. One can't boil down the whole of the logic of communicative interaction in the way it's been done.

Mike Lynch: I think the issue about presuppositions would be that the attribution of a presupposition to a speaker, or to both speaker and hearer in this case, is a bit of analyst's work. Then the question would be well what sort of status does a presupposition have in a conversation? How do we specify further? Given that almost anything can be a presupposition, we invent it analytically for the speakers and hearers - how do we provide for its accountability within the particular conversation that we are analyzing?

Jeff Coulter: I'm not quite sure that I followed that. But let me start out by saying that first of all notice I'm talking about "assignable" presupposition. {ML: By who?} By anyone. And the presupposition is assignable not to the speaker, but to the utterance. That utterance, in that position, in that sequence hearably presupposes {ML: What?} the rejection of the offer.

Mike Lynch: Well, you could say it hearably accomplishes that.

Jeff Coulter: Hearably accomplishes, Ok.
Then: How so?

Let's take another example:

A: Would you like a ride home?

B: My car's right outside.

"Would you like a ride home?" - invitation, offer - not clear. How come it's not clear is actually something worth thinking about, but we'll leave that for now. "My car's right outside" could be found to be simply an assertion of the geographical location of his car. What it assignably presupposes there is, of course, that since his car is outside, he can get in it and drive it home, or implies, if you prefer. The assignable implication or presupposition - and I'll use them interchangeably for now, though I think there are some differences - is a function of its sequential position in the slot in which it could be hearably either an accept or a reject. Finding that it is a

reject of the invitation only works by figuring out what it presupposes, or implies. It's not on its face, as an assertion, as a remark, as an utterance, it doesn't on its face reject the offer.

Mike Lynch: Oh, I would say it would. It certainly does. It rejects as clearly, as hearably, as evidently as, say, "No way".

Jeff Coulter: In virtue of what?

Mike Lynch: Well, in virtue of its sequential production in the context of that prior utterance.

Jeff Coulter: But how is it heard as a rejection just in virtue of its sequential position, because lots of things can be put in the sequential position that are appropriate for rejection, that you don't hear as rejection? How is it that you haven't said "No. Thank-you. I don't want to come." You've just said "My car's right outside"? How is it that that could be heard there as fulfilling the sequential function of rejecting?

Mike Lynch: Well, I'm saying that's no more problematic in a lived situation than saying "No" would be. It would be immediately, unproblematically heard. Unless there were some ambiguity in the way it was said, it would be heard as "No". It would be ambiguous to an analyst who extracts that sentence and wonders "Well, in other contexts, this sentence would mean other things". But to the participants in that conversation, it's not a problem that that sentence would mean something else in another context.

Jeff Coulter: Well, no, it's not a problem. But don't forget that a part of the methodology, as Sacks sets it out, is to suggest that what we have in utterance production is organizational solutions to members' problems - with a small 'p'. Now how are we going to work out what that is as an achievement? It's an achieved rejection. In virtue of WHAT does it do that? Surely it's got to have something to do with what it presupposes. And what it hearably presupposes is itself a function where it's said in that sequence.

Mike Lynch: You could say that of anything whatsoever, though. You could say that of "No".

Jeff Coulter: Yeah you could, but then- I've lost the track now. Why is it not an issue as to how something comes to hearably fulfill its

possible function in a sequence? Why isn't that an issue? It seems to me that that is a reasonable analytical question. It's not just in virtue of it's being in second-pair-part position. Lot's of things can be put in there. {ML: Oh, yes.}

Mike Lynch: I think where collections come in, one of the ways of working with collections is not that you are sampling the universe, but you're finding what sorts of things in a place like this can count as one of those, hearably doing 'rejection', rather than immediately going for: Well, we can think of presuppositions. "I'm all right as I am" is in reference to the mention of hospital, and what might happen in a hospital - we can sort of run along those tracks. But the alternative would be to characterize that somehow, and to build up collections of them, inspect them, and say: These are the sorts of things that can happen here. Now, this is certainly an arguable tactic, because it's making out members to be doing a kind of empirical reasoning in these slots. That in this place, this is a sort of regular occurrence - regular enough. It doesn't have to occur all the time, or half the time, or anything like that. A regular enough occurrence that happens in a sub-collection of rejections, or whatever you want to call that. And they have specifiabile properties, which I can't say what they would be because I don't have a collection of them to specify them. But that's a different way of operating on the organizational objects than to reflectively specify, or stipulate presuppositions on behalf of the utterance.

Jeff Coulter: But it's not a stipulation. {ML: Oh} Why is it a stipulation? It's not being said, you see, that the speaker somehow mentally determines an explicit presupposition in so many words. It's not a cognitivist argument. It's say the accountability, so to speak, the accountable recognition of that, or the accountable comprehension of that thing would have to be, would have to make reference to an order of sequentially controlled implication, or presupposition. That's all. And it would be part now of the logical properties of requests that they can be done not as assertions, but through assertions that operate to do the rejection at the level of implication or presupposition. And then you go on to the theorem. I don't want to sort of bandy the point too much, because it's a modest claim. What it connects with is the discussion of Speech Act theory. Speech Act theory is too cavalierly dismissed. There's no question but that a lot of Speech Act theory dealt

with first-pair-parts and no others. Like it deals with 'invitations' and never 'rejections of invitations', etc. - and very few of those anyway. But that's not to say that there is something intrinsically defective about what the Speech Act analyst is attempting to do that now stands in need of correction by Conversation Analysis. On the contrary, Conversation Analysis cannot but build off of, and go beyond Speech Act analysis insofar as Speech Act analysis is concerned with illocutionary activities, and talks about the felicity conditions that have to be satisfied for something to be hearable as something. What Sacks comes along and does is to say: Ah, felicity conditions in Austin, here is a particularization of the general notion of felicity condition: sequential location. I.e., it is indeed a part of the felicity condition for something to be hearable as an answer that it is a second-pair-part to a question. Austin never noticed that. Austin, in his general talk about felicity conditions for appropriate and intelligible production of Speech Acts, never noticed that. But now I don't think CA has any basis whatsoever for turning around and saying that because Speech Act analysis had a decontextualizing propensity in certain respects, that therefore the whole sum of the achievement of the elucidation of the concepts of illocutionary conduct is out the window. On the contrary, I think that their methodology, with suitable logical correction, i.e., the Speech Act analysts, is perfectly compatible with. Moreover, it seems to me, CA people trade, so to speak, tacitly on resources for identifying turns that they are uninterested in explicating. And it seems to me that if it's part of the Ethno. program, it is a part of the resources that at some point should be susceptible to elucidation.

I mean Mel Pollner used to say, I don't know if you ever chatted to him about this Mike, but he used to say things like Sacks used to be interested in reasoning; CA is now interested only in structure. Seems to me there's no incompatibility between the two. {ML: No}

We're way over time.

That's about it for today.

Mike Lynch: We didn't do Davidson did we?

Jeff Coulter: No, we didn't do Davidson. We'll have a look at that next time. {ML: All right}

TRANSCRIPTION CONVENTIONS

These transcriptions were prepared from an audio recording, the auditory quality of which was not always optimal. Participating members, especially Jeff Coulter, have reviewed initial drafts of the transcripts and have attempted to clarify portions that were particularly difficult to hear, and also provided assistance in correcting errors in transcription that were made during the production of the first draft of each session. The generous assistance of all those involved is gratefully acknowledged. Particular appreciation goes out to Jeff Stetson who took on the laborious task of copy-editing. Unfortunately, not all errors in transcription, and not all inaudible portions could be corrected given the time lag in producing the transcriptions. The transcriber apologizes for any remaining typographical errors.

The following are conventions employed in producing the transcriptions of these meetings:

- [x] square brackets: Text in square brackets is for the most part that of the transcriber. Such text is indicated by "Tr:" at the beginning of the brackets. It includes: 1. paraphrases of talk, 2. clarification of indefinite references, for example pointing, drawing or writing on the chalk-board, producing a behavioral display etc., 3. information about references to texts referred to in the talk, 4. other general remarks and clarifications and 5. indications of omissions and tape changes. PLEASE NOTE that there is almost always some bit of talk missing at the points of tape change. On occasion, comments in square brackets are insertions offered by those who have edited the text and are indicated as such by the initials of the person in question, e.g., [JC: xxxxxxxx]. Also, if additions or deletions are made in text quoted during the class, then these changes are noted in square brackets, with the speakers initials included within the brackets.
- { x } braces: Text in braces is the talk of persons who are commenting on or responding to the talk of another person who currently has the floor. This talk is introduced by the initials of the person making the remark and/or comment, e.g., {JC: xxxx}.
- (x) parentheses surrounding text: Text in parentheses is the

transcriber's best guess as to what was being said when there is some doubt about exactly what was said due to the poor auditory quality of the tape.

- () empty parentheses: Parentheses containing no text indicate a portion of the talk which is not transcribable due to the poor auditory quality of the tape, although it is possible to tell that something was being said.

CAPITALIZATION: Capitalization is used to indicate titles (or partial titles, e.g.: INVESTIGATIONS for the PHILOSOPHICAL INVESTIGATIONS) for books. It is also used to indicate vocal emphasis by the speaker in question, i.e., something which is spoken noticeably louder, or with more emphasis than the surrounding talk. It was not possible to underline text in the editing/text-formating system employed, which is why capitalization rather than underlining was used in the above cases. This also influenced the forming of terms in languages other than English, e.g.: Ubersicht, vis-a vis, etc, which are neither capitalized, nor underlined, although occasionally are explicated in square brackets following the text in question.