Comparative Sociology, Laic and Analytic: Some Critical Remarks on Comparison in Conversation Analysis

La sociologie comparative, ordinaire et analytique : quelques remarques critiques sur la comparaison dans l'Analyse de Conversation

Rod Watson

Rod Watson
Centre d'Anthropologie Linguistique et de Sociolinguistique, Institut Marcel Mauss, Paris

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1.   Introduction: First Considerations Concerning Conversation Analysts' Assembling of Data Collections and Adducing Comparisons[1]

This paper is only a first move towards a more developed critique. It is still piecemeal and contains *lacunae*. It will be devoted to an initial consideration of the implications of the making of collections of conversational data instances (or, more tellingly, 'data excerpts'), within the approach of Conversation(al) Analysis (CA).

'The' Comparative Method is very much the method of CA's alternate, Formal-Analytic (FA) Sociology. FA comprises the range of classical and orthodox sociologies that are committed, by and large, to some version of neo-Kantian epistemology, and an associated commitment to a project of literal description. Indeed, 'the' Comparative Method has often been taken as the core or even sole method of FA sociologies, their *sine qua non*. One question, then, concerning the making of comparison in CA is: can these be effectuated without concession to FA? Similar questions may be posed for CA practitioners in linguistics, given a virtually equivalent commitment to FA (and, certainly, Formalism), along with a concomitant commitment to comparative work, in that discipline. To these practitioners, however, I must say that the original relevances both of CA and EM are sociological. It

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is to these constraints that CA must respond, despite its appropriation by linguistics and the attempt to judge it on linguistic constraints.

In this Introduction, however, I shall seek to eschew the usual sheer opposition of one epistemological stance against another. I intend to bring Ethnomethodology (EM) into a more explicit relation with contemporary CA, partly by issuing a reminder about their common origins. In so doing, I shall espouse the analytic mentality held in common by EM and CA practitioners to reject straightforwardly philosophical oppositions (e.g. correspondence *versus* coherence epistemologies) as well as purely theory-driven, theory-formed approaches and substantive stipulations. All too often, such straight philosophizing leads to sociologists treading unsteadily and unwisely in the domain of professional philosophers, frequently producing boy scout versions of the real thing. Such versions of philosophy have bedevilled sociology throughout most of its 20th-century existence as is shown by von Wiese and Becker's acerbic comments, made back in 1932, on 'near-phenomenology' (v. Wiese and Becker, 1932, p. 679): sadly, their remarks still pertain today.

Instead, I intend to abide by the analytic mentality of EM and (at least, early) CA in transforming epistemological considerations into methodological ones. One of the few philosophers (along with some Pragmatists) that can assist in practical ways in the bringing about of such a transformation is Abraham Kaplan (Kaplan 1964). His book, significantly sub-titled *A Methodology for the Social Sciences*, certainly did not provide a direct line into the EM approach but nevertheless proved very influential in early British discussions about that approach—particularly in the Manchester EM group a few years later.

Kaplan (1964, p. 180–2 and *passim*) considers the often unreflective analytic practice of 'equivalence classing', which lies at the very heart of contemporary CA—as well, of course, as, for instance, FA sociology and linguistics. Establishing equivalence relations in equivalence classing is the 'building block' for subsequent practices such as collocation and constructing collections, or systematically comparing like with like, and like with unlike, and so on—what we might term the 'bread and butter' practices of CA (*inter alia*).

With these primary considerations in mind, the structure of this paper falls into place. First, some elaboration both of my approach and of equivalence classing will be undertaken. Then, equivalence

classing as a laic, i.e. ordinary members' practice based in mundane reason will be examined. As well, I shall make recommendations for the non-ironic analyses of these 'naturally-occurring, naturally-situated, naturally-organized' equivalence classes and of the ordinary activities of collection, co-selection, comparison, etc. of which these classes are placed in service. Finally, I shall consider equivalence classing and the derivative activities for which it is the precondition, as practiced, implicitly or explicitly, in professional/analytic sociology.

In another book that was very influential in Manchester, Cicourel (Cicourel, 1964, p. 24), claims that equivalence classing is precondition for counting and measurement and that language uses are inevitably involved in such classing—language uses both of laic and professional status (where the latter are variously, but inevitably, embedded in the former). Cicourel, too, observes that equivalence classing is an indispensable 'building block' for analytic practices such as counting, measuring, collecting or collocating, comparing, contrasting, identifying 'deviant cases', etc.—and to sociology we may also add that there is no exemption for linguistics from these considerations. Some methologically-radical critiques of some practices of CA will be made.

Whilst EM constitutes the basis for several of the concerns I shall bring to bear (though not all; again, Kaplan's work does not fall under the *aegis* of EM, though it has been invoked in some EM), I do not regard my position as subjecting CA to the rubric and constraints of EM *per se*. To simply subordinate CA to EM or to drive EM as what Crews (1988, p. 159–78) calls a 'master transcoding device' through CA is to risk the very theoreticism that EM disavows and against critics such as Crews so trenchantly inveigh.

Instead I wish to propose a strictly initial move in a debate that I trust will ensue. I do hope that this move, prefatory as it is, will help us move toward a 'bullet-proof' conception of intersubjectivity in action when it comes down to cases (as Al Capone said it all does). Not all these praxeological considerations of intersubjectivity will come from EM: after all, John C. Heritage has vividly referred to local conversational sequencing as furnishing an 'architecture of intersubjectivity', and gives a particularly convincing exhibition of that characterization with regard to adjacency-paired utterances (Heritage 1984, p. 254–60). That is to say, my aim is to address an analytic conception of intersubjectivity-in-action that does not, for instance, impose

an external standard or otherwise stipulate, does not commit logical or methodological errors such as conflation, category-mistakes, the fallacy of unwarranted extrapolation or is not otherwise intersubjectively problematic. In addition to their shared beginnings, EM and CA have each made too many major contributions to these issues to simply subject one to the other.

The continuity between elements of EM and CA derive in large part from the commonalities in their origins, and consist in such as the following: i) CA's detailed concern for the local organization of talk-in-interaction, which in my view is a very important 'take' on EM's recommendation—largely empirically unfulfilled by EM itself—that members' mastery of the natural language should be a (perhaps *the*) core concern (*viz* Garfinkel and Sacks, 1973, p. 342–5; reprinted in Garfinkel 2007, p. 463–40; the fact that this is a conjoint paper is most significant), and ii) EM's concern with the analysis of practices, including conversational practices, as naturally—indexically, reflexively—embedded in an inextricable way within a local *gestalt* contexture (*viz*: Wieder, 1974, p. 186–94). The notion of a *gestalt* contexture has been around ethnomethodology from the moment of its inception in the 1950s, as a result of Garfinkel's conversation with Aron Gurwitsch whilst he (Garfinkel) was a Visiting Fellow at Harvard University.

Establishing equivalence relations is integral to our procedures for attributing, finding or enhancing order in a set of phenomena or an array of phenomenal details, be these procedures laic or professional. Indeed, a central preoccupation of EM is the relation between laic and professional apprehensions of social orders, and the ineluctable embeddedness of the latter in the former (though such embeddedness is, arguably, increasingly being overlooked in some contemporary EM worksite studies dealing with expert knowledge and expert systems). These 'embeddedness' issues potentiate such questions as: does this or that approach in professional sociology ground its analysis in laic categories and/or turn them into a topic for explication on their own behalf? Or does a given professional sociologist seek to privilege their own apprehension of order as against those of members? Do they, wittingly or otherwise, establish a disjunction between the two, do they even seek to establish a competitive stance between the two such that practical, commonsense apprehensions are debunked (Berger, 1966, p. 51–8), undercut, downgraded, downranked, relativized or even

ignored altogether (all of which are what sociologists term 'method-ological irony' and what EM and, in principle, CA, radically oppose)? Indeed, I shall comment below on the decontextualising practices that create such disjunctions. In doing so, I shall also be suggesting that CA is beginning to risk regression to the methodological ironies of FA.

As Kaplan states (*ibid.*, p. 181), the practice of establishing that two or more objects are equivalent to one another involves the imputation of identity, equality and synonymy. To these, Cicourel (*ibid.*, p. 26) adds the property of reflexivity (in a broadly symbolic interaction-ist sense of this term), given the *de facto* social-interactional nature of sociology's data. The relations between objects deemed equivalent are—to employ Kaplan's terms—symmetrical and transitive, the clas-sic instance of transitivity being, of course, that if a is equivalent to b and b to c, then we can pass over b to state that a is equivalent to c. Of course, as Kenneth Burke (1965, p. 97) claims, we must be careful to distinguish between equivalence and similarity, but to pursue the potentially significant implications of this for at least some CA falls beyond the purview of this paper, given space and time limitations. A strong definition of CA collections can by adapted from a general formulation by Kaplan (*idem*, p. 185), namely that a collection is an equivalence class of isomorphs—or what CA treats and transcribes as isomorphs: again, this could be one object of an EM worksite study of CA practice (*viz*: Sharrock and Anderson, 1986, Ch.6).

It should be remarked here that FA's aims have long included that of equivalence classing. Indeed, one of the high priests of FA mathema-tization of social objects, Hubert M. Blalock Jr., expresses the aim of establishing equivalence across settings—though even then he has to build into this exercise a 'disturbance variable' to identify cross-setting variation, (Blalock, 1982, p. 265). This 'disturbance variable' hedge attests once again to Garfinkel's observations on sociologists' attempts in vain to create freestanding objective expressions from indexical ones. For us, though, the telling issue is that CA's analytic operations of col-lecting, comparing and contrasting, involve the selfsame establishing of equivalence across settings. One is led to wonder just how far CA has emancipated itself from FA's ways. Maybe this is one source of what commentators such as Lynch and Bogen identify as the 'scien-tism' of CA.

The point that will be made in the early part of this paper is one

made by Cicourel (see above) and, even earlier, by Rose (1960) namely that ordinary language and reasoning are redolent with 'common-sense equivalence classes' and one major locus of these is in what Harvey Sacks (1992, *passim*, vol. 1) termed 'membership categories', 'membership categorizations', standardized relational pairs (of membership categories)', 'naturally-occasioned category co-selections', etc. Of course, as Rose shows, these laic equivalence classes operate in and influence professional social-scientific uses, too, in ways that are still largely unexplicated. The laic equivalence classes are both primordial to and formative of the professional ones, and it is with these that we shall therefore begin, by examining the practices involving membership categories in effectuating ordinary-language comparisons.

Cicourel (*ibid.*, p. 26) pertinently notes that the establishing of equivalence classes involves reification. One such form of reification, among many others (see my observations on Smith, below), involves the presentation of the isomorph as a bounded object by cutting it off from its immediate context. Thus, an isomorph is an assembled object, one that is put together by removing it from its differentiating context, a context of distinctively-identifying detail, or, at the very least, applying a *mutatis mutandis* rider along with the *ad hoc* practice that Garfinkel terms 'et cetera' (Garfinkel, 2007, p. 77-80 and p. 144–7). One argument concerning CA is that in making collections and comparisons it is, in some of its areas and in some significant respects, increasingly involving itself in just such reifying operations. Of course, detail differences within the equivalence class perforce remain and cannot be expunged; recall again Garfinkel's comments (*idem*, p. 55–8) on FA sociologists' attempts in vain to make 'objective' analytic expressions from 'indexical' ones. We might say that, as ever, 'equivalence' is equivalence only when subject to these limiting and qualifying procedures, these aspects of what we can, with Kaplan, call the 'logic-in-use' of equivalence classing. We might call this 'equivalence for all practical purposes', be these purposes laic or analytic. Whilst this argument might seem to be referring largely to FA sociology, we might suggest that it applies to FA linguistics too: as the customary English expression has it, "If the cap fits, wear it."

It would be a methodological irony to treat ordinary society-members' (henceforth: 'members') equivalence classes as the 'poor relations' of the professional ones. After all, equivalence classes of each status

are subject to similar *ad hoc* procedures that render, for their various practical purposes, each isomorph an undiscriminated object amongst its fellows. Whatever its presumed status, equivalence classing is, ineluctably, a contingent accomplishment.

A note on the term 'laic.' The term has been EM currency for approximately fifteen years or more, and I use it here in strict accordance with Eric Livingston's definition and extended specification (Livingston, 1995, p. xvii and p. 121–9). I have applied it here to Livingston's intended referent, namely the making and reading of textual objects—in the present case, transcripts of naturally-occurring, naturally-analysable oral interchanges, I have also extended it—again, in keeping with Livingston's specification—to ordinary phenomena such as the making of naturally-analysable collections in ordinary oral interchanges. As with the term 'lay', no religious meaning is implied. One useful advantage of the term 'laic' over the term 'lay,' not noted by Livingston, is afforded by its relative strangeness. Through this strangeness it can occasion a 'look again' attitude to the perhaps over-familiar phenomena it describes.

Now to more specific CA issues, with the reminder that without establishing equivalence there can be no systematic comparison.

2.   The Origins of the Concern for Comparison in CA

In the work of the founder of CA, Harvey Sacks, the issue of comparison comes up with some frequency. In his *Lectures on Conversation* (published in 1992, though the lectures were given in the 60's and early 70's), virtually all his analysis concerning comparison, equivalence classes, contrasts addresses these as laic phenomena—that is, as members' practices deployed at, and oriented towards, at the common-sense level. Thus, comparison, equivalence and contrast are analysed as society-members' own ordinary practices conducted through mundane reasoning. He referred to laic comparison and equivalence classing (Sacks, 1992: vol. 2, p. 77, p. 569 and vol. 1, p. 431–9), standards of comparison and members' measurements (*idem*: vol. 1, p. 439), compliments protected against comparison, (*idem*: vol. 1, p. 788–9), membership categorisations and comparisons (*idem*: vol. 2, p. 457–9). He refers to the "standout" property of contrasts (*idem*: vol. 2, p. 171–2), attention to and optionality of contrasts (*idem*: vol. 1, p. 681–2

and vol. 2, p. 315–6), contrast as a sequential technique (*idem*: vol. 2, p. 558), verbs and verb tenses in relation to contrasts, (*idem*: vol. 1, p. 787–8), how contrasts are found and what work they do (*idem*: vol. 1, p. 799–801). Often, Sacks writes not so much about mere contrasts but about contrast *classes*, and this is his approach to comparisons, too: equivalence and difference are at the heart of these concerns.

Parenthetically, we can also note that laic contrasts have also been studied by those whose analytic persuasion approximates considerably more towards very early EM. For instance, Dorothy E. Smith explicates a variety of two-part and three-part contrast structures in a discourse in which a party to the discourse claims the person being talked about through these structures is mentally ill, (Smith, 1978: p. 40–47). In a sense, Smith writes about how these structures work through an "instructed hearing" of the contrast, where the contrast in turn furnishes an "instructed seeing" of the person who is the object of the conversation as being mentally ill.

All the above references are explications of lay members' own comparative or contrastive work as an entirely mundane practice. All of them, one way or another, attest to Garfinkel's (1967: p. 28–9 and Garfinkel 2007, p. 88) famous comment on the indistinguishability of *what* was said and *how* it was said. Thus:

> . . . what parties talked about could not be distinguished from *how* they were speaking. An explanation of what the parties were talking about would then consist entirely of describing how the parties had been speaking; of furnishing a method for saying whatever is to be said. . . . Then the recognized sense of what a person said consists only and entirely in recognizing the method of his speaking, of *seeing how he spoke*.   (Garfinkel, 1967: p. 28–9)

Garfinkel's comments seem to me to excellently express the basis and justification of CA as well as formulating common cause with EM: it is also my warrant for my furnishing an EM-based conception of CA and *vice versa*. It is important that Garfinkel is not arguing that "what" aspects of the conversation should simply be analytically converted into "how" aspects, that the content or substance of conversations be transformed by the analyst into "something else", another separately-bounded entity, namely one or more conversational procedures. Rather, Garfinkel is saying that the "what" in conversation is

entirely coterminous with the "how". What is compared *consists in* how it is compared. It is this and other aspects of the EM "take" on CA operations with which I shall be concerned in this paper.

3.   Comparison in Members' Practices

It is instructive to consider the above comments on laic comparison and contrast with Sacks' and his colleagues' somewhat later article (originally Sacks, Schegloff and Jefferson, 1974: my references will come from the slightly modified, updated version in Schenkein, 1978). In the excerpts below, an elderly woman is telephoning a crisis intervention centre, threatening to kill herself, declaring her problems and attempting to negotiate a visit from one of the centre's personnel.

Telephone call to a Crisis Intervention Centre
Co = telephone counsellor and C1 = caller

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Excerpt D

 D.249          I am going to tell you this, I am only one in a million, but I'll
 D.250          spread it about, what the church can do for you it can do nothing,
 D.251          the Catholics do more everyday in every week than what the
 D.252          Protestants do I think it's disgusting I do really and truly.
 D.253    Co:  do you--have you spoken to your vicar or to anyone at church? do -
 D.254          they know that you're infirm?....
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(This excerpt continues with materials on the vicar's/anyone at church's need and right to know about the caller's infirmity.)

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Excerpt E

 E.415    C1:  no, beggars do that, I haven't become one of them yet (I'll never
 E.416          live to see a beggar's day, your) prayer, I'd sooner put my head in
 E.417          that gas = gas oven, I would (sobs).
 E.418    Co:  um
 E.419    C1:  the Jewish people, the Catholic people are all better looked after
 E.420          than (me), there's only one that isn't that's Protestants I can tell
 E.421          you that, on the phone, I do know that (.... it is) my husband's
 E.422          death, god forbid that I should say such a thing but the (....), no
 E.423          charity begins in the church (doesn't it) (that) my church, what
 E.424          they've done for me I'm very thankful for, nothing at all.
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We can find many elements of Sacks' work on comparisons (see above) in the above data sequence, not least the categorial organisation of this comparison. It shows how the conversational activity of comparing is implicated in at least one other activity, that of complaining. We can see the way in which *relevant* comparisons are made, i.e. within

a given conventional collection of categories ("membership categorisation device"), in this case the collection "types of religious faith". The relevant co-selection of categories for comparison is conducted according to a generic rule for co-selection, the consistency rule—a rule that provides both for members' effectuation and recognition of such relevance. In short, the hearer's version of the rule is that i) if some second membership category is produced proximal to some first membership category, and ii) if the categories can be heard as coming from the same membership categorisation device, then iii) hear them (or make sense of them) that way. Thus, a standard of comparison is that of categories from the same device,—in our data excerpt, from "churches"/"types of religious faith". We see also a treatment of each church (the Protestants, the Catholics) as what Sacks (1992, vol. 1, lecture 8) refers to as a locus of rights and obligations, in this case "charity". We see the comparative ease of membership categories in the proclamation of what we might term a 'moral profile'. The normative imputations that are made on the basis of membership categories are highly significant and have been attested to since Sacks' earliest writings on calls to the Los Angeles Suicide Prevention Centre. Derivatively, the role of categorization work in social control activities such as persuasion can hardly be gainsaid.

Of course we also see that a contrast is introduced through this comparative method, where Protestants are made to stand out in contrast to Catholics (lines D 251–2) and also Jews (lines E 419–20): indeed the caller herself strongly marks the contrast, ("... there's only one that isn't ..."). In lines 419–20 the contrast is set up in terms of the Jewish people and Catholics meeting a standard that the Protestants fail to meet. This, then, is a three-category comparison incorporating a third category contrast, where lines 251 show a two category contrast but with the same basis of comparison (types of religious faith/churches). Given a *mutatis mutandis* qualification, we see a 'similar' two-part contrast in the following:

Telephone call to a Crisis Intervention Centre

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Excerpt F

       F 243     Cl:   I have not asked anything, it was a bit of
       F244            company I wanted (and) I can't even have
       F245            that in this country, there's black people
       F246            coming into this country and get more than
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      F247              what (I), than what white people get, (no
      F248              good), I am not against blacks, everybody
      F249              has got to live but I've never in my life
      F250              known how hard it was ... [continues].
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Again, we get a basis for comparison from within a given member-
ship categorisation device (race) with a contrast worked up on that
basis. Of course, that operates through the invocation of a different
context than the previous excerpts, where "this country" is invoked—
a duplicatively-organised category where there is an implied contrast
between "recent arrivals" and the "established residents". Note in
passing there are two items ("I have not asked anything", "I am not
against blacks") that work to authorize this account (Smith, 1987*8*,
p. 33–5) by—perhaps I over-extend Sacks' term here—protecting it
from induction, or at least cutting off inferences that the caller is 'ask-
ing for something' or is being 'racist'. Such protections can shore up
comparisons and (especially) contrasts (see also Watson, 1978, p. 110–
112, and for a fuller data sequence).

A third example, this time from a police interrogation of a murder
suspect, gives us another type of basis for comparison out of which a
contrast is worked:

Police Interrogation of Murder Suspects

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Excerpt G

20      Officer:   Well I understand thet uh:: he tried to uh:::::: stay
21                 pretty close tih Go:d uh::: by:: singing in the
22                 churchiz 'n so forth e n:::uh,
23                       ((door squeaking))
24      Suspect:             °He's       °He's a hyp ocrite.°
25                          (2.0)
26      Officer:   Pard'n?
27      Suspect:   He's a hypocrite.
28                          (0.3)
29      Officer:   A hypocrite?
30      Suspect:   Yes.
31      Suspect:   Hypocrites owna sahd a' God. Dass one thing ah cain't
32                 (see) I am not a hypocrite. ·h (1.0) Dey say dey so much
33                 diss b't den then go out (.) outsi:de'n do wro:ng de:n.
34                 ·h (0.5) °'T's what they do.°
35      Suspect:   But ah:'m not 'f ah don:'t (0.2) believe'n it ah'm
36                 nah gon' (.) try tuh do sump'n (di:vh'rnt). (0.3) Yu-
37                 Ah try tuh p- prac (.) preach tih you den you look
38                 aroun'n see me doin sump'n else thet ah told you not
39                 t'do,
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40     Officer:   ·t
41                        (0.2)
42     Officer:   W'l then yer: telling me now thetcher a man'v honor.
43                right?
44                        (0.3)
45     Suspect:   Yeh ah'm a man'v honor.
46                        (0.8)
47     Officer:   Awright?
48                        (1.5)
49     Officer:   Ah you honorable enough:? (0.7) et this ti:me tuh
50                tell me:, (0.8) what (.) motivatedju?
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Here, on lines 20–2 we get the police officer's description of the suspect as "staying pretty close to God by singing in the churches", thus making inferentially available a membership category such as "a man of virtue/honour". Such an inference is undercut by the suspect who instead categorises the victim as a "hypocrite".

Here, we see another comparative basis—a given mode of conduct is, by inference, used as the base for one inferred category by the officer and a contrasting one, "hypocrite", by the suspect. This is worked by the suspect (lines 35–9) into yet another type of comparative basis for contrast, that of rightful claim to a given categorisation such as "man of honour" as opposed to "hypocrite". This also works on the basis of an activity contrast between "telling others to do something" to others and "doing something else ..."—activities which taken together are bound to the category "hypocrite". This is a complex case of what Sacks terms "category-bound activities", where a given single activity, e.g. "crying", is treated by interlocutors as conventionally bound to a single category "baby" (Sacks, 1972, *passim*). In our datum, though, the activities "practicing what one preaches to others" and then "not doing something else" are only *when combined together* treatable as tied to the category "man of honour"—a membership category to which the suspect affiliates when it is proposed by the officer.

In all these instances we can see ordinary interlocutors as "practical comparative sociologists", making comparisons of categories or activities and working up contrasts on those bases. We have here a remarkable variety of comparative and, derivatively, contrastive work. In a sense we can see that these instances are so 'procedural' or 'structural' (Smith, 1978, p. 40–47) that it can appear formulaic, but the detail differences are important.

Two points can be made about this laic comparative and contrastive work. First, it is not the analyst's task either to endorse or refute those comparisons. To do so would be to effectuate a methodologically-ironic stance, to set up "in competition with" or to assert authority over interlocutors' laic conceptions. The analyst's task is, instead, simply to explicate the procedural (culturally-methodic) bases for the comparisons made in interlocutors' verbal utterances. This is the way in which both CA (and the EM from which it originated) works— explication, not irony. For instance, there is no warrant in CA or EM for simply "grounding" a set of analytic comparisons in laic ones: one can not become a "members' mouthpiece" (to use Jeff Coulter's term) in this or any other way. Instead, CA and EM look at the practical reasoning involved in ordinary comparisons and contrasts as these are expressed and incarnate in, (say): conversational actions. To do otherwise would be to conflate two distinct levels—the laic/ordinary and the analytic. This, as Schütz pointed out, is a logical error.

Secondly, whilst there are clearly some family resemblances between the excerpts above, there are also many differences. Some of the contrasts are consistency rule-based, but amongst these some work on the basis of two categories, others on the basis of three. Some work through activity-descriptions with categories being made inferentially available and some do not. Some involve disavowals of a givens category-incumbency and others do not: and so on. Some of the examples are constituents of an activity such as "complaining" (with accusatory elements) and others are constituents of self-justifying self-categorisations, working on claims to incumbency of a category. Such self-referential work is a frequently-found feature of accounts. Not least, some of these examples are visibly part of a negotiation for help whilst others are to do with putative reasons for murdering this particular victim, a "hypocrite". Moreover, the context invoked by interlocutors themselves differs: e.g. one categorial claim made by the caller to the crisis intervention centre works by the invocation of "this country" as a context, whilst other categorial claims do not rely on this.

Consequently, the procedural equivalences (use of categories, consistency rules, category-bound activities, etc.) between some of the instances do not stretch to all the instances. Even where there are similarities, seeming equivalences at the analytic level, the context differs. Thus these instances **as** instances can not, in any straightforward way,

be aggregated into a single collection. Instead, these instances do not show a finite number of procedural features which could be used as definitive analytic criteria for the addition with no further decision of additional instances for the collection. Consequently, the conditions for a "mechanical CA" do not here exist. As we shall argue again below, we can only write of these apparent equivalences if we invoke a *mutatis mutandis* rider: we must pay all due attention to detail differences— differences that count for the interlocutors themselves. Note that a *mutatis mutandis* rider does *not* in CA license the disattending of such detail differences, let alone the tearing of these claimed equivalences from their varying orientational contexts. In this respect, any clear-cut comparison and contrast at the analytic level is beset with difficulties so complex as to seem, to many, to be intractable. Analytic comparison and contrast, whether or not licensed by lay members' own comparisons and contrasts, do not present themselves as possibilities without being vulnerable to some major objections.

It must be said that CA practitioners do take care to elaborate the detail of each instance, irrespective of its status in a collection, as Heritage (1984, Ch.8) observes: and their observations are some of the most acute and rigorous to be found in social science. However, as Heritage (*idem*, p. 258) indicates, such elaboration is ultimately addressed to the identification of generic procedure, of typicality or reproducibility, or of some systematic feature—which, of course, apply to more than the single and singular instance (for another e.g., see Drew, 2004, p. 76, where he writes about the recurrent, systematic aspects of an extract from a telephone call). This, of course, provides the basis of and rationale for collections in CA. Despite CA practitioners' invocation of context, there remains an "elephant in the room": the question that still looms is, "For CA, just how freestanding *is* conversation?"

To sum up what has been said so far: we can have no objections to ordinary interlocutors' own practices of comparison or contrast. Indeed, our remit is to analyse them as topics. However, the fact that members compare and contrast does not license CA practitioners to straightforwardly set up homologous contrasts and comparisons at the analytic level: and, indeed, Sacks himself, particularly in his lectures on MCD analysis, frequently and even predominantly employed single instance analysis, though it must be said that there is considerable variability here. Sacks, too, often saw his single instances as 'typ-

ical' or even 'invariant'—a very strong claim indeed. Sometimes this refers to a laic conception of 'typical' (i.e. interlocutors produce some categorized person as 'typical' of the category), but sometimes he suggests typicality at an analytical level. Thus, his single instance analysis can operate as a prolegomenon to the making of a collection, perhaps to be compared with other collections.

Moreover, Sacks' use of terms such as 'apparatus', 'machinery' or even 'device' in respect of interlocutors' methods of speaking together also suggests that the making of collections might be a next move, e.g. collections of instances 'generated' by the machinery. Sacks proposes a set of rules in his single instance analysis, rules that 'reproduce' his initial data (Sacks in Hill and Crittenden, 1968, p. 41–2) and it is clear that these rules, in order to *be* rules, must be applicable to more than one case. Of course, as Coulter opines (Coulter, 1995, p. 330), Sacks may be using terms such as 'apparatus', 'reproducible', etc. as a vivid metaphor or it may just be that Chomsky's notion of generativeness was still in the air at the time, still defined the terms in which language phenomena were discussed. Nonetheless, the point stands that Sacks' single instances were often analysed in such a way as to potentiate collections, comparison and contrasts—ultimately a 'systematics', in fact.

4. Comparison as an Analytic Practice in CA

Why, then, do practitioners of CA seek to build collections for the purpose of analysis? It seems that the answer to this is that formalisations are being arrived at and displayed. That is, there is a preference for formal generalisations, though attempts at statistical generalisation are not unknown, (*viz*: Schegloff, 1993; Heritage and Greatbatch, 1986). Perhaps a modified version of Cicourel's arguments on the procedures involved in the equivalence classing and rate-producing processes might be the EM "take" on the latter. It is in this area that we can begin to locate the potentialities and pitfalls of comparison in CA work. We shall return to this issue below.

In what way has CA aspired to do comparative work as an analytic commitment rather than just studying comparison as a laic phenomenon? The clearest example takes the form more of an advisory rather than any substantial delivery, though one may find shadows of

it in the area of the "Institutional Talk Program (ITP) of CA". This is the recommendation of Sacks *et al.* (1974, and in Schenkein, 1978, p. 45–7): that CA take on the comparative study of speech exchange systems. As Sacks *et al.* put it regarding the envisaged trajectory of CA:

> suffices to suggest a structural possibility: that turn-taking systems, at least the class of them whose members each preserve 'one party talks at a time' are, with respect to their allocational arrangements linearly arranged. The linear array is one in which one polar type (which conversation instances) involves 'one turn at a time allocation'; that is, the use of local allocational means, and the other pole (which debates instance) involves 'preallocation of all turns' and medial types (which meetings instance) involve various mixes of preallocational and local allocational means.

> That the types can be so arranged permits them to be compared directly, in relevant functional terms. Thus, one pole (local allocation of turns) permits maximization of the size of the set of potential speakers to each turn, . . . whereas the other (preallocation of all turns) is designed to permit the equalization of turns . . . which it does by specifying . . . next speaker.   (Sacks *et al.,* 1978: p. 46.)

It is very clear from the above quotation that the authors are proposing a comparative method, one which refers to analysts', not necessarily members' work of comparison *per se*. Nowhere in the section on the comparative study of speech exchange systems is members' laic comparative work even mentioned: no wonder, since one does not routinely hear or observe members organising two or more speech exchange systems into a "linear array" or continuum. What we have instead is a set of *analytic* terms and criteria for the comparison of speech exchange systems without any direct reference to members' own intersubjective conceptions of any such comparison or criterion.

Thus, there are two modalities for comparative work in CA. The first is the study of comparison as a lay members' conception and practice. The second is the study of comparison as an analysts' conception and practice without an explicit, central concern for laic comparisons. This is sometimes done in an unannounced manner, *viz* Schegloff's often implicitly comparative approach to data instances (Schegloff, 2006). The remainder of this article will argue that, of course, the CA researcher has every reason to take seriously as a topic members' laic

comparative work but that the comparative study of speech exchange systems conceived as a purely analytic exercise is a questionable one. Some possible resolutions to such problems will,—highly tentatively— be proffered: however, we must first work towards them, both diagnostically and therapeutically.

5.   Methodological Issues in CA's Use of Comparison as an Analytic Practice

The very first, and from an EM point of view, highly positive thing to observe about CA's use of comparisons as an *analytic* practice is that it refuses the usual conceptual devices that sociologists use to draw comparisons. For instance, Weberians' use of ideal types is clearly subject to the strictures Sacks adduced in his paper 'Notes on Methodology' concerning what he called "hypotheticalised-typicalised models" of social action (Sacks, in Atkinson and Heritage, 1984). Not only do these models comprise analytic impositions on the data but also, as Sacks claims, they are in no wise descriptively adequate to their object. As Atkinson and Heritage say, (1978: p. 2–3) such models might well lead to artifactual characterisations and conclusions based on—and here I quote—"intuitive idiosyncrasy, selective attention or recollection, or experimental design". The use of naturally-occurring, tape/video-recorded data is recommended instead: and these data are the very basis of CA analytic practice, providing for subsequent comparative and other analytic operations, usually transcript-based too.

An initial question, then, is this: given that intuitive or imagined data instances and analyses are renounced (although still sometimes used) by CA, and that natural data are then preferred in order to characterise and analyse social (in this case, conversational) actions, how does the making of collections get to be done? How does a series of natural data instances come to be produced as a "recognisable collection"? How many instances is enough? It seems clear that the activity of equivalence classing is going on: the collections are seen in prepositional form—collections *of* something (e.g. of repair). Comparison, then, is involved in the most basic sense in the initial assembling of a collection ("instance one can be compared with instance two"), as well as the collection itself coming to be used for subsequent comparisons.

Very often (though not of course always), the collections in the published research literature in CA are of the kind n=2. Only two cases of the class are presented, though we are apparently sometimes meant to infer that they are only two (presumably perspicuous) representatives of a larger collection held by someone, somewhere, or that they express a 'systematics' for a very large number of instances. We might say that a collection of two data instances suffices (this is reminiscent of Sacks' remark to the effect that one is an instance, two is a series), though of course the number can be proliferated. It is notable that collections are subjected to "instructed readings", though of course such readings are not restricted to collections. Those readings may involve section introductions, e.g. Schegloff *et al.'s* lead-in to a two-item collection in their canonical paper on the normative preference for self-correction, i.e. (Schegloff *et al.*, 1977: section 6.2).

> Section 6.2: "As noted above, the *Y' mean X*? form may be used to modulate an other-correction. But it may be used, quite apart from that, to check understanding, i.e. for a check by recipient-of-a-turn of his understanding of the turn—as can forms other than *Y' mean X*? E.g., (continues)."

This passage is immediately followed by a presentation of and by a two-paragraph explication of the two instances: and, again, n=2.

It is interesting that Schegloff *et al.* explicitly present the commonalities as "forms" and, indeed, we shall return very shortly to CA's methodological emphasis on forms. For the moment, however, let us arrive at a conception of collections, which form the basis of so much comparison in CA: we refer here both to intra-collection inter-collection comparisons. Again, the Schegloff *et al.* paper on conversational repair comprises a classic example: intra-collection comparison of three different positions for self-initiations and inter-collection comparison of self- and other-initiated repairs: of course, intra-collection comparisons can yield sub-comparisons (*viz* Schegloff *et al*, 1977, section 3).

A collection might, then, be referred to as (i) at least two (transcribed) empirical instances, adjacently or proximally arranged, and (ii) the "lived work" of collecting, in this case the textual work of describing the collection and the work of comparison, i.e. identifying the similarities and differences between the instances. A col-

lection, then, is not just a set of instances *tout court.*[1] Indeed, following Garfinkel and Sacks (1970), we may refer to the instances as the "accountable texts" for which the surrounding textual explication furnishes the account, provides for an instructed reading of the instances taken together: indeed, the account provides for what "together" might mean in the first place so far as these instances are concerned. This lived work thus *constitutes* the collection *as* a collection, as an equivalence class of isomorphs or partaking of isomorphic properties.

Of course, we also need to look at CA actual methodology, its 'logic—in—use (Kaplan, L, 1964, p. 3–11) not least that of transcription practices, worksite practices through which the data instances, severally and together, are initially furnished and are transformed from the primary data-site, the audio or video- recording. CA analysts often work by preference from these primary sites, but, often, all that we have in printed media such as journals or even in online journals are the transcribed transformations. There, the transcripts stand as 'the data', though Garfinkel has described them as 'docile texts' when compared to the witnessably lived work of the primary field. One might suggest that the methodological project can be characterised as the production of formal statements from these data sources, or perhaps of sociological descriptions of conversational forms. As the philosopher Per Segerdahl (1998: p. 282–3) claims in a perceptive, if sometimes off-target, critique of CA's theoretical basis:

> A conversation analysis, such as the analysis of turn-taking performed by Sacks *et al.* is characteristically a *formal* analysis of features of sequentially represented conversations. This draws conversation analysis nearer to linguistic disciplines such as formal semantics and pragmatics. The claims attached to formal analysis are, however, different from those of formal semantics and pragmatics. Under the influence of ethnomethodology, a formal analysis is not conceived of as a theoretical model of the underlying nature of a phenomenon—recall Garfinkel's conception that there is an inevitable gap between theory and phenomenon—but as a description of empirically-found formal procedures that conversationalists employ to produce conversations, and to recognize, and display their understanding of, organizational fea-

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1. Here I draw upon Eric Livingston's most suggestive analysis of textual and other kinds of lived work in his books *An Anthropology of Reading* and *The Ethnomethodological Foundations of Mathematics*.

tures of conversations, for instance, that a particular conversationalist ought to speak but is silent.

His claim accords, in the main, with that of Garfinkel and Sacks' back in the days when EM and CA were not so sharply differentiated (Garfinkel and Sacks, 1970: p. 345, reproduced in Garfinkel 2007, p. 441):

> Ethnomethodological studies of formal structures are directed to the study of such phenomena, seeking to describe members' accounts of formal structures wherever and by whomever they are done. . . .

The comparative establishing of equivalence and difference are very much part of this project of formalisation, or at least the project of describing forms. In principle if not always in practice, CA involves one strand of formal analysis, that of a phenomenological rather than a neo-Kantian sort[1]. There is a stress on the accountability of formal properties to members, to formal properties as phenomena for members. The former can be elucidated through the work of Gurwitsch. This kind of formalism is to be distinguished from the neo-Kantian kind such as that of Simmel, with his "form"-"content" distinction and an implied or incipient distinction between the phenomenon as apperceived and the thing in itself.

The similarities and differences fielded by comparative analysis in CA are, then, done through formal analysis in the elemental sense that they comprise *forms*, e.g. self-versus other-initiated repairs, self-versus other-repair outcomes, etc. This is definitely not to accuse CA analysts of producing crude overviews or overweening rubrics. As Segerdahl vividly puts it (*ibid.*, p. 282), with reference to Sacks' formal analysis of membership categorisation:

> The spirit that Sacks emphasizes is not that of a thick-skinned logician led away from the concrete world by fascination of the abstract subtleties of a formal calculus, but that of a highly sensitive scholar who is able to get on the fine track of a shy animal.

Would that this analytic mentality were more consistently in evidence in much contemporary CA. Our question concerning formal

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1. On this distinction, see D. Martindale (1961), p. 220–30, 233–81 and *passim*.

analysis becomes: through what moves is such a formal analysis produced by the CA researcher? If we can answer this we might be able to see the problem with CA as a formal project (of which comparative analysis is part).

6.   Some Major Reservations about the Difficulties of Comparison in CA

There is something of an ambivalence at the heart of the CA project. CA is interesting and important in its conception of contextualising instruments in conversation (membership analysis, circumstance analysis, recipient design, etc.) but CA practitioners' wish to arrive at a formal analysis, i.e. an analysis which identifies the forms these instruments take, leads them into a curious position, one which bears a few resemblances to Dorothy Smith's (1974) "three tricks" of analytic extraction, decontextualisation and re-contextualisation. These are (in my admittedly selective précis): i) separate what people say they think from the actual circumstances in which it is said, ii) rearrange those ideas and iii) constitute them as a distinct entity (say, 'systematics').

In their synoptic article of early CA, Sacks *et al.* (1974 and in Schenkein, 1978, p. 9–10) verge on some of these analytic "tricks". Hence:

> Reason began to appear for taking seriously the possibility that a characterization of turn-taking for conversation could be developed that would have the important twin features of being context-free and also capable of extraordinary context-sensitivity. . . .

> First of all, a problem for research is that conversation is always 'situated', always comes out of, and is part of, some real sets of circumstances of its participants. But there are various reasons why one does not want to have to know or characterize such situations for particular conversations in order to investigate them. And the question then becomes: What might be extracted as ordered phenomena from our conversational materials which would not turn out to require reference to one or other aspect of situatedness, identities, and particularities of content or context.

It seems an odd position that one has to decontextualise in order to study contextualisation. Sacks *et al.* furnish plausible reasons for

this analytic election, but a major reason to which they do not explicitly refer is that this procedure of lifting conversational organisation out of the distinctive and specific circumstances of its use on particular occasions, out of its particular phenomenal field—and, as a consequence, downplaying its content too—operates as a "machinery for creating formalisations". The "rearrangement" referred to by Smith then works in the case of CA as extracting and selectively focusing (e.g. in transcripts) on sequential features of conversational organisation. This, in turn, provides for a "rearrangement" such as the making of collections where each "similar" instance is shorn of the *gestalt* contexture which comprised its provenance so that it can be aggregated with other instances similarly treated and inspected largely for sequence. Thus, a first step in formalisation can lead to a second step— the arrival at formalisations that emerge from considering the collection of instances "as a whole". The operations of comparison and contrast, of establishing equivalence, variation and difference take place within that nexus of formalisation. Of course, other analysts of natural language use may vary in their orientation to the centrality of formalism and to its intersubjective status, but those practising 'high church' CA seem to have explicitly espoused formalism from the earliest times, as I have indicated above—hence their conceptual vocabulary items such as 'apparatus' and 'systematics'. (I am not inclined to be so indulgent as Coulter concerning CA's intent in using these terms). This being the case, we can interrogate the intersubjective status of the formal properties adduced.

The similarities and differences that are, variously, the condition for and/or the outcome of comparative analyses are, then, formal ones established across a range of substantive materials raised from their distinctively-identifying *in vivo* contexts and qualified through a *mutatis mutandis* rider. Put this way, the pristine naturalism of CA as described by Atkinson and Heritage in their quotation cited above (p. 10) seems more artefactual than it first appears.

Zimmerman (1988: p. 419; also cited in Lynch and Bogen, 1994: p. 81, with whose arguments my own bears significant points of contact) expresses very honestly and well the role of comparison in CA relative to the making of collections of recorded, transcribed natural data:

> To be sure, initial purchase on some phenomena may be gained on intuitive grounds, but this is merely the beginning. From this point on, the phenomenon is 'worked up' by searching across many conversations, resulting in increasing empirical control and a more general understanding of the process that generates it. When applied to new cases (which, of course, could undercut the formulation and force its revision) such empirically grounded formulations furnish a warrant for the identification of particular conversational events. Indeed, the cumulative results of conversation analytic research should permit a defailed understanding of particular, singular conversations.

This quotation is notable because CA practitioners' explicit reference to how their analytic collections are made are very rare (and we may wonder why). Zimmerman's quotation also aptly identifies the data-gathering/analytic procedure followed by CA practitioners, and the role of data collections in that procedure. However, it also, perhaps unwittingly, identifies the pitfalls involved for at least a pristine CA in following a collection-based or comparative procedure. This is that the cumulative—we might say emergent—properties of the collection "as a whole", as analytically established, are not only used to characterise each member of the collection but are also used to approach the characterisation of some new data instance that is being considered as a candidate for the collection. This is equivalence classing, full-blooded, *à la* Kaplan. Perhaps, even, it constitutes a move, however slight, towards what is now called 'toy models' of the 'just so' kind. Plainly, not only are there pitfalls in this "absorptive" analytic use of collections, but also there are fundamental problems involving members' laic, *in situ, in vivo* designations of an instance of speech exchange as opposed to the technical designations of that instance effectuated by professional practitioners of CA. Indeed the notion of a (formalistic) 'systematics' is a case in point. We should note that the idea of a 'systematics' of social action has been an ideal of FA sociology for most of the last century; von Wiese and Becker set out a protocol for "the systematics of action patterns", focalizing linguistic actions, back in 1932 (v. Wiese and Becker, p. 715–30 and Part 3).

Some of those pitfalls and problems can merely be sketched here. The overall problem seems to be the conflation of members' and analysts' standpoint on each data instance. Put more specifically, the latter comes to stand for the former. That is, members' orientations

are more or less displaced, to be replaced by those of the CA practitioner. There is a clear danger of analytic stipulativeness and imposition here, but there is also a deep intellectual confusion. Not least, all this imputes a rather flat stylistic unity to CA,—a "monochrome plain", we might say—where each study tends to have an insistently similar pattern to every other, such that variant forms of the analysis, (for instance, where studies indicate that conversational organisation owes its nature to something in addition to sequence on a given occasion), are not afforded corpus status: only canonical sequential analyses will be admitted to the fastness that is CA.

Even 'deviant case analysis' is, ultimately, not to the single instance approach that it seems to merit. Schegloff's (1968) canonical and indeed inventive analysis of a single deviant case amongst approximately 500 others ended up in the absorption of that case with the other 500 and into a single analytically-described equivalence class, that of summons-answer sequences.

What we get, then, is a situation where the emergent properties of collections, as derived by professional analysts, tend to come to stand on behalf of lay speakers' own *in vivo, in situ* orientations to each case. As Lynch and Bogen point out, (Lynch and Bogen, 1994: p. 82–3, 91–2), what has evolved from this is a corpus of properties that forms part of a professionalised, scientistic culture of analysis: they are speaking here of scientism, not science. This culture is where the natural accountability of a phenomenon is subsumed under, even supplanted by, an increasingly aprioristic technicised characterisation based on a corpus of precedents, i.e. prior findings that are part of the working culture of CA,—and, particularly, perhaps, of those CA practitioners who work within what some call the ITP; the 'Institutional Talk Programme' in CA. For some observations on this, see Lynch and Bogen (1994, p. 78ff.). Thus, a set of precedents may be drawn on from this culture in order to furnish the practical meaning of some currently-considered sequence(s). Of course, such considerations yet again take us into the domain of CA as a worksite practice.

Transcription practices at the workface comprise a particular focus, though there has been relatively little analytic attention paid to the transcribing process and even less to transcript reading. Transcript reading contains rules of use such as '/' for silence. The rule provides that the symbol stands unquestioned—it directs the reader to the ref-

erent: the sign does not simply stand for (the bounding of) a silence but <u>constitutes</u> that object, (this point is my re-working of Pack 1984, p. 144–5).

In turn, the phenomenological conception of formalism comes to recede somewhat in favour of the neo-Kantian one, with all its attendant problems. Chief among these, as indicated above is the intersubjectively problematic status of the actual formal properties that are adduced—clearly a problem for any form of CA that recognises any remaining resonance with its EM roots. This does not mean that CA becomes another example of classical sociology—that would be going much too far—but it does mean that in some cases it incorporates some of the conceptual (an element of neo-Kantianism) and methodological devices (e.g. quantitative techniques) that characterise some classical sociologies. This might, at the very least, be said to introduce disjunctive elements into CA practice.

It was not always thus,—at least, not to such an extent. Though, as I have observed, the seeds of the current situation were always present in CA, especially from 1974 onwards, CA at one time was less impervious to the conception of conversational organisation as intersubjectively available from the commonsense standpoint of the ordinary person,—so much so that CA was seen by some, even as late as 1984, as revealing (again) the "architecture of intersubjectivity" (*viz*: Heritage, 1984: p. 232). CA was seen as a topic, referring to ordinary speakers' ongoing analysis of the conversations in which they were involved. As Schegloff and Sacks put it a decade previously, there was a laic intersubjective warrant for the identification of a turn at talk, and this warrant was revealed by "next action analysis" in CA. This is particularly marked in the case of adjacency pairing of utterances, i.e. in utterance types that are conventionally paired such as question-answer, invitation-acceptance/declination, etc. This is how Schegloff and Sacks, in an impressive paper (Schegloff and Sacks 1974: p. 240), exhaustively define the intersubjective warrant as operating with reference to such adjacency pairs.

> What two utterances, produced by different speakers, can do that one utterance can not do is: by an adjacently positioned second, a speaker can show that he understood what a prior aimed at, and that he is willing to go along with that. Also, by virtue of the occurrence of an adjacently produced second, the doer of a first can see that what he

intended was indeed understood, and that it was or was not accepted. Also, of course, a second can assert his failure to understand, or disagreement, and, inspection of a second can allow the first speaker to see that while the second thought he understood, indeed he misunderstood. . . . Wherever then there is some reason to have the appreciation of some implicativeness to be made attendable, 'next utterance' is the proper place to do that. . . .

Here, we can see how "next utterance" is crucial in "intersubjective alignment",—that is, the identification and checking by the interlocutors themselves from within the same utterance sequence in the same conversation—of the specific, putatively shared, sense of the first utterance. ("Sense", here, consists in what that first utterance was specifically *doing*, as a conversational action—again, the "what" and the "how" coincide.)

The Schegloff and Sacks quotation also shows how specific intersubjective alignment is a jointly-achieved matter, done through the "fine tuning" of conversational activities, reciprocally done. Whilst the quotation adopts a formal approach, the formalism involved is, clearly, sensitively attuned to co-conversationalists' activities with regard to a given sequence. Thus, whilst a question may be identifiable as such through its interrogative format, intonation contour, etc., one intersubjective warrant for its being a question (rather than, e.g., an invitation) is that it is recognised, i.e. treated as such in the immediately succeeding utterance. This kind of consideration is why the title "Conversation(al) Analysis" was devised. It was initially intended as designating a *topic* for analysis rather than, say, a professionalised research technique. "CA" designated lay interlocutors' *own* commonsense analysis of the conversation in which they were involved, moment-to-moment.[1] It is, I feel, no accident that contemporary CA has gradually backed off from insisting on 'next action analysis.

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1. Of course we must issue the *caveat* that, in another strand, the Schegloff and Sacks' quotation may indeed be taken in general terms, terms which do provide for the making of collections.

7. Implications for Professional Conversation-Analytic Practice

What, then, is to be done about CA with regard to comparison? Can the practitioner continue to partly or wholly derive the meaning or specific sense of given utterance sequences through a comparison with a) prior, putatively "similar", instances, or b) fellow instances in a current collection? Or do we need to re-specify CA in order to maximally capitalise on its parallel tendency—most evident in earlier studies and in Sacks' lectures (Sacks, 1992)—towards locating meaning as *interior* to the *particular* interactional sequence under consideration?

If we choose the latter,—and I propose that we do—we tend to move away from an analytic warrant for a particular imputation of sense toward a topicalising of the laic production of sense entirely from *within* a given sequence. This would serve to re-position CA away from one tendency, i.e. where analysts, partly on the comparative basis of precedent and other general considerations, arrogate the meaning of an utterance, toward another tendency, that of focalising interlocutors' own *in situ, in vivo* sense-production activities as deployed within a particular sequence in its distinctive phenomenal field. The latter option would move toward an emphasis on the examination of single, indeed singular, instances and of starting again with each instance rather than drawing one's analysis from some precedent or some collection—which operation surely involves the importation and imposition of what Garfinkel has called an "external standard", a set of "meanings" that are exogenous to the specific, distinctive activities and singular setting currently under consideration. After all, *falsus in uno, falsus in omnibus*, as the legal phrase has it.

If we choose the former option, which maximises rather than minimises collecting and comparative operations, we are condemned to repeat the approach of the more orthodox formalistic sociologies and much of linguistics. That approach involves the analyst setting up him/herself in competition with ordinary speakers' displayed orientations in any particular situation of speech exchange. Of course, there may be rich temptations for those practitioners originally trained in orthodox sociology or linguistics to attempt to absorb CA into their "mainstream" analytic rubrics: but this would lead to the dissolution of CA's distinctive contribution, and would reinstate just those ana-

lytic problems that CA was devised to resolve or to occlude.

What, then, might we propose, however tentatively, as a way of re-working CA so as to avoid some of the problems concerning collections—and, we might add, to the incipient scientism in CA practitioners' language in referring to and using collections? One way might be to respecify the notion of "collection", to treat it as, at bottom, a natural language term indexing—or, better, glossing—a contextually variable range of characteristics rather than indicating one or more "across the board" finite, criterial features. In other words, we might treat the term "collection" in the same way as Wittgenstein in his later work (1968: paragraphs 66e and 67e) treats that of "game":

> Consider for example the proceedings that we call 'games'. I mean board games, card games, ball games, Olympic games. What is common to them all?—Don't say: "There *must* be something common, or they would not be called 'games'"—but *look and see* whether there is anything common to all.—For if you look at them you will not see something that is common to *all*, but similarities, relationships, and a whole series of them at that. . . . Look for example at board games with their multifarious relationships. Now pass to card games; here you will find many correspondences with the first group, but many common features drop out, and others appear. When we pass to ball games, much that is common is retained, but much is lost.—Are they all "amusing"? Compare chess with noughts and crosses. Or is there always winning and losing, or competition between players? Think of patience. In ball games there is winning and losing; but when a child throws his ball at a wall and catches it again, this feature has disappeared. Look at the parts played by skill and luck; and at the difference between skill in chess and skill in tennis. Think now of games like ring-a-ring-a-roses; here is the element of amusement, but how many other characteristic features have disappeared! And we can go through the many, many other groups of games in the same way; and see how similarities crop up and disappear. And the result of this examination is: we see a complicated network of similarities overlapping and criss-crossing: sometimes overall similarities, sometimes similarities of detail.   (Wittgenstein, 1968: paras 66e and 67e)

Consequently, we may treat some first instance of what CA practitioners identify as a collection as having a range of properties that vary from, but (on the basis of contextual variation), overlap with those of some second instance, which then again departs from some

third instance but also retains some overlaps: and so on. We might observe that in our respecified definition of "collection" (a set of collected instances + the lived textual work of collecting) we do see something approaching such provision, given that much of the textual work of collecting involves a *mutatis mutandis* rider—that these instances comprise "a-collection-given-due-attention-to detail-differences" and given the "forms of life" limitation (see below). I simply propose that this *de facto* practice become an explicitly *de jure* one. In a strong sense, though, the family resemblance model works better for the analysis of descriptive terms such as "membership categorisation" (*viz*: indirectly Heritage, 1978: p. 86–95).

In this respect, we might now locate comparison (e.g. intra-collection comparison) in CA within a "family resemblance" model. However, there remains what is at least an open question, that of intersubjective transitivity. To wit: does this model avoid the problems to which we referred above, those involved in the "carrying over" of the specific, contextually-based intersubjective orientations that constitute one given instance into a characterisation of the next? This is the core problem of comparison in CA. It is far from certain whether collections of, say, sequences can be analytically respecified in this way.

As indicated above, the only way to move toward securing the "subjective adequacy", as Weber put it, of the analysis is to produce a radically respecified version of Schegloff's single instance analysis, one which does not involve residual comparisons. This, of course, also involves respecification of CA *in toto*, and rather than simply rejigging the "analytic technology" of CA, in a limited way, seems to involve a radical reorientation of the "analytic mentality" of CA, a letting go of what Wittgenstein, writing of philosophers, dubbed "the attitude of disdain toward the particular case".

Such a respecification involves a major initial move, and that is to reverse the policy that Sacks *et al.* propose in their "Simplest Systematics" article, quoted on *infra*. That policy is to lift the "apparatus" of conversation out of its occasioning context, to extract it from the particular set of very specific contextual relevances of which it is part—including features such as participants' contextual orientations to a given task, a contextually-embedded project of action, or one of a vast array of *gestalt* contexture-based considerations, e.g. collaborative working at a computer work-station. It is entirely conceivable that the

talk will have a variety of bearings on different *gestalt* contextures—
including various degrees of centrality to the context, e.g. as Michael
Lynch has observed, talk at the science laboratory bench is often nei-
ther central nor pervasive. We might observe that talk may well on
occasion be shaped quite as much by locally-specific, distinctively-
identifying *gestalt* contexture considerations as by the turn-taking or
categorial ones, (see, e.g., Greiffenhagen and Watson, 2009, for some
indicatory arguments and instance-by-instance findings relating to col-
laboration at a computer: here, the hope and attempt was also to make
some initial moves in a single instance analysis of data that were at
least recognizable to practitioners of CA).

   The same applies to the comparative analysis of the formal speech
exchange systems studied under the *aegis* of the ITP of CA. To remove
talk from its legal or medical, or pedagogic or other conventionally-
given purposes, motives, understandings or tasks—specific knowl-
edges, purposes, motives, understandings or tasks that are embedded
in the vast range of particular and distinctive worksite contexts of each
of those institutional practices—is putatively to remove from consid-
eration one major shaping element of the talk. Indeed, that shaping
element may be variably operative within the range of settings com-
prising a given institution. As for inter-institutional comparisons of
speech exchange systems, the shaping elements are located in very dif-
ferent "forms of life", to use Wittgenstein's term (Winch, 1990, p. 40–
2). Included under the *aegis* of this term are the distinctive contexts,
different complexes of conventions and rules, different constellations
of concepts, different knowledges, different informal practices, distinc-
tive ways of making professional sense—contextual sense—that per-
tain amongst different institutions such as religion, sciences, law. These
institutional differences make the 'across the board' criteria for com-
parison highly problematic. Indeed, any form of analytic transitivity
and transposition risk incoherence or logical disjunction. The Wittgen-
steinian game analogy not only gives EM and CA practitioners alike
a possible way forward (subject to the 'form of life' *caveat*) but can
also afford us a basis for a critique of formal equivalence classing in
CA. Consider the term 'run' as a unit of scoring: the term 'run' is
used in at least three sports—cricket, baseball and rounders. The term
'run' seems to be an equivalence class operating across three sports.
However, a run in cricket has a very different sense from that in base-

ball, a sense that is located in the distinctive complex of rules, conventions and moves in each game. A 'run' in rounders is closer to that of baseball but still differs in sense or meaning given the differing (if overlapping) rule-sets of the games. Thus, the term 'run' is, in other games, sometimes closer in its meaning or sense to that in baseball, sometimes further away but is never coterminous with it. To say that a 'run' in each of these games is an isomorph that can be collected with 'runs' from the other games into a single equivalence class, is observationally radically misleading, and amounts to what Ryle (1949) calls a 'category-mistake'—an error of logical category. To effectuate a comparison between different forms of ordinary talk would be to ride roughshod over these forms of life, to forcibly conflate and confuse them, to wrest these talk-shaping elements from the distinctive conventional-institutional context that informs them. Often a rather behaviouristic formalism is the only way these contextual matters can be disattended. This "slippage" between different forms of life leads to the kind of logical disjunctions and confusions that Winch (1972, 1990) so rigorously and profoundly exposes.

A somewhat parallel argument applies to the usual comparison in this domain, namely the comparison between "ordinary"—or "natural" conversation (that is, locally allocated not pre-allocated turn organization) and some form of (pre-allocated) institutional talk. The latter involves a complex of orientations and control procedures that do not pertain in ordinary conversation. In some worksites, talk itself may not be so central to the work, to task-performance and the like, see again Lynch (1985).

Of course, this approach requires a respecification of the central policies and methods of CA, and this respecification would serve to reunite it with Harold Garfinkel's recent EM—not his *Studies in Ethnomethodology* so much as his *Ethnomethodology's Program: Respecifying Durkheim's Aphorism*, (2002). EM too would profit from this reunion and would be able to evolve accordingly, especially on linguistic matters. This later study is cast in terms of Gurwitsch's notion of *gestalt contexture* far more than is the earlier one. It might be suggested that the re-emplacement of conversational organisation, conversational instruments, etc. within an array of orientations constituting a particular *gestalt contexture* would abolish the problems of comparison in canonical CA that we have discussed above—problems that

Schegloff's "single instance analysis" does not fully occlude. Such an approach would maximise our profit from the highly sensitive analytic mentality that, as Segerdahl observed, characterised Sacks' own pioneering approach and which still has echoes in some contemporary CA.

8.   Conclusion and Implications

The main thrust of this brief paper has been to argue that the equivalence classing that seems to inform CA practitioners' assembling of collections of data instances is misconceived. It involves lifting a data instance from its context, which following long-established EM practice I have called a *gestalt* contexture, and bringing it into an equivalence relationship with other data instances torn from other, potentially very different, contextures.

Tellingly, the concept of *gestalt* contexture has never been employed in CA, despite many discussions of context in that area. Perhaps this is not surprising since no naturalistic discipline cognate with CA in analysing communicative interactions uses this conception of context either. For instance, a canonical and otherwise immensely useful collection of papers on language and context, Duranti and Goodwin's edited volume *Rethinking Context* (1992), contains, to the best of my knowledge, not a single substantive reference to *gestalt* contexture in over 360 close-printed pages.

The term '*gestalt* contexture' perhaps affords us the most phenomenologically-sensitive, the most intersubjectively-adequate conception of context. In early EM it was often linked with the 'documentary method of interpretation' as a model of the back-and-forth determination of an array of indexical particulars and an imputed underlying pattern (see Wieder's 1974 study of language in the subcultural context of a parole institution, p. 184–211). This way of conceptualising the reflexive constitution of pattern and particular was later dropped by Garfinkel and he reconceptualized it in a way that accorded more closely with Aron Gurwitsch's conception (Garfinkel, 2002, p. 158 and *passim*), though it was still very much Garfinkel's development of that conception.

There is no room in this article to elaborate the extensiveness and subtlety of the concept '*gestalt* contexture' nor to do more than merely

assert its centrality to EM. Suffice it here to say that it refers, in Garfinkel's "take" on Gurwitsch, to the distinctively-identifying *in vivo* phenomenal detail composing just this setting and no other, here and now, just this particular phenomenal field. Each phenomenal detail at once gains its sense from its affiliation with a texture of other detail and lends its sense to them: and this sense emerges, develops and transforms over a texture-specific *durée* as endogenously apperceived by participants. Such a contexture may encompass items of specialised knowledge, specific motivations or norms, as well as the more obvious 'contextual' features. We may then talk of a particular conversation's reflexive embedding in its distinctive phenomenal field detail.

The point about a *gestalt* contexture is its distinctiveness: each specific phenomenal field is composed of a distinctively-identifying array of phenomenological detail, much in the way that a kaleidoscope furnishes a new, distinctive pattern after each shake. To lift an item of talk-in-interaction from such a distinctively-identifying phenomenal field is to remove it from what Garfinkel and Gurwitsch might term its very specific functional signification. To be sure, this may wrest an item of talk-in-interaction from some features of the very interaction in which it is involved. The item of talk comprises a move within that particular phenomenal field, just there and then, and to lift it from that field is to dissolve its sense as just that move, as a move-within-this-specific field (where, reflexively, the specific field gains meaning from the move, too).

Wieder's use of the notion of *gestalt* contexture in relation to utterances comprising 'expressions' of a context (the 'context' being, in part, the inmates' "code" of resistance) is nicely formulated in the following quotation (Wieder, 1074, p. 184)

> The utterances and behaviours upon which the code (or any other normative order) is based have no self-evident or self-explanatory sense in isolation from one another. Instead, they have a relatively definite sense as constituent parts of an actually witnessed, concrete setting in the way that each is a constituent within a system of functional significances. That is, situations, actions, and rules determine one another's sense as constituent parts of a *Gestalt*-contexture.

Following through on this quotation Jeff Coulter (1994: p. 692) adds of texts (in which we may include oral-aural texts):

> Texts' self-explicating features are, *inter alia*, functions of their exhibited 'respect for' the *gestalt*-contexted character of the human activities, circumstances, objects and products they conceptualize.

To this we might add, somewhat laterally, that the self-explicating features of utterances, sequences or a conversation of what Garfinkel and Sacks, (1970, p. 350ff.) call a 'self-explicating colloquy' also explicate their distinctive contextual embeddings. This explication elaborates that embedding and is, reflexively, elaborated by it: in this strong sense, utterance/sequence and context are ongoingly reciprocally embedding. This means, of course, that sequence and specific context are non-extractable from each other. This of course, might even render questionable a family resemblance model of bringing data instances together. Whether such a model can handle such highly specific, distinctively identifying variations in phenomenal detail must for now remain an open question. It may be that what we need to consider is a single instance approach: an item of speech-in-its-distinctive-context, i.e. a much more radical single instance approach than that presently employed in CA.

So, for instance, in Wieder's study, the utterance "you know I can't organize a baseball game", said by an inmate of the parole institution to a staff member who has asked him to do so, indexes a shared understanding of that particular setting, to a code of non-cooperation maxims informing that specific context. The refusal is a *move in that particular game*, played at just that time and in these circumstances. To remove it from that game and to focalize some of its formal properties (e.g. as part of the equivalency class 'adjacency pairs') is to leech much of the interactive life from it, leaving only skeletal remains. To further treat it as an isomorph and bring it into an equivalence relation with other items of speech, other alleged isomorphs, gathered elsewhere and similarly denuded, is a major conflation. After all, these other items of speech were other moves in other games occurring in other, very distinctive and very different, phenomenal fields.

Perhaps the most serious issue arising from the above is that such activities as collocation, collection and comparison in CA are thereby rendered intersubjectively problematic, since in removing them from their context they have been shorn of their specific intersubjective sense and treated as equivalent with other times, similarly shorn.

To use Dr. Samuel Johnson's famous phrase, such heterogeneous instances are 'yoked by violence together'. It is in this sense that equivalence classing in CA may be seen as intersubjectively problematic, whatever the purpose of that classing—be it comparison or anything else.

Of course, CA itself has a strong notion of context. Schegloff has written widely and often about the notion of context. (e.g. in Duranti and Goodwin, 1992, p. 191–228) and his writings on this matter have great depth and subtlety. Indeed, in their own terms they cover several (though not all) of the features of context that are highlighted by the notion of *gestalt* contexture. Similarly, Schegloff argues for an intersubjective warrant for his analysis as we have, at least indicated, above. But, with all due respect to Schegloff, even if we set aside the notion of *gestalt* contexture, we may therefore ask whether the CA practices of collocating, collecting and contrasting actually even meet his own methodological constraints and requirements concerning interlocutors' orientations.

One outcome of moving towards the notion of *gestalt* contexture is that we may be able to stop referring to 'talk' and its 'context' in submerged binary terms. Talk is not embedded in context it is inextricably enmeshed in it: indeed, talk is context and context is talk. Through the notion '*gestalt* contexture' we might begin to refer to contextualized speaking in unitary rather than binary terms, and this can become part of our EM project of abolishing binary oppositions such as 'action' and 'structure' (of which the 'talk' and 'context' binary is a close relative). It is not that such *gestalt* contextures contain no 'general' resources such as membership categories, sequences, etc., but that these skeletal resources are 'filled out' and attain very specific functional significances within each particular *gestalt*. We might say that these 'general' resources express a 'kaleidoscopic' significance with the other *gestalt* elements: if one shakes the kaleidoscope one changes the pattern and one utterly reshapes the place and expression of the putatively general resource within that new pattern. So it is with talk, which consists of an immense variety of elements that **on any given occasion** take on a very specific *gestalt* determination, specifiable only by analysis of that *gestalt* as such. The fact that some resource is 'general' does not indicate that it has a standard supra-contextual meaning, function or determination, and as a consequence does not indicate that any phe-

nomenologically meaningful comparison can be effectuated.

This article has established a distinction between laic and professional-analytic comparison in CA, and has identified a range of problems attaching to the latter. The article then proposes an EM-sensitive respecification of CA, where professional-analytic comparative operations would not figure in any focal way. The more we take seriously Garfinkel's later considerations, the more sceptical we should be about the role of comparison in the analysis of natural language use, despite the exhortations of some "sympathetic" linguists and sociologists to the contrary. Indeed, the de-sociologising of CA by some linguists seems to comprise a case of 'out of the frying pan and into the fire' in this respect (and others), with regard to considerations of intersubjectivity.

A few EM practitioners write of hybrid disciplines—an ethnomethodological physics or ethnomethodological mathematics, for example (see Livingston, 1986). We might consider that an ethnomethodological conversational analysis (to use George Adoff's phrase: personal communication, 1988) or conversation-analytic ethnomethodology might emerge from our attempts to forge a secure notion of intersubjectivity-in-action—and, indeed, more convincingly than some of the other proposed hybrids.

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