12 Epilogue: The definition of alternatives: Some sources of confusion in interdisciplinary discussion

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In refuelling ships at sea the actual transfer of fuel from one vessel to another can be a minor and relatively simple part of the whole operation. The tricky part can come in getting the ships aligned and connected, then keeping them there whilst the tranfer is made. This seems a good metaphor for the relations between the various approaches within the human studies and, particularly, for the problems involved in reciprocal criticism amongst them. The actual criticisms are, of course, the nub of the matter, but their relevance and effect depend very heavily upon the ways in which the approaches are aligned for comparison, the way the context for criticism is set up.

In this epilogue we will try to discount much criticism of Conversation Analysis, that sort which is intended not just to discredit or revise any of the findings of its particular conversational inquiries, but which is designed to show that the whole strategy of investigation is entirely wrong headed or, if not quite that, then fundamentally flawed.

Reciprocal criticism within the human studies is very difficult to make adequate and effective, and not simply because the various standpoints involved are rather diffuse and thus insulated against criticism in the way of which Popperians so fiercely disapprove. The main obstacle to serious interchange (at this stage of things at least) is the problem of mutual understanding amongst different points of view, since the arguments are not between rival theories or hypotheses so much as between alternate 'modes of analysis', and the understanding of these really involves getting the hang of how these ways of thinking, investigating and interpreting, work. Getting a grip on one mode of analysis is hard enough, but really forceful criticism often requires that one be able to get inside more than one. Not only is it difficult enough to get a grasp on one way of thinking, it is also often the case that the ability to get inside a second one is inhibited by mastery of the first. True entry into that second standpoint may require the abandonment of the entire apparatus of conceptions making up the first.

Criticism of Conversation Analysis is clear testimony to the fact that many people do not see its point. Much of the criticism of Conversation Analysis is of a kind common in the human studies, which perhaps results from the kind of difficulties to which we have been pointing. It is the kind of criticism which does not suggest a better way of doing the things that have been attempted, but which casts doubt on the value of attempting such things at all, calling for a different kind of study. Thus, criticism of Conversation Analysis (hereafter CA) is rarely of the kind which suggests different or superior ways of examining conversation's organisation and more typically of the sort that complains that CA has not solved the problems that the *critic has in mind*.

We do not aim to provide a counterblast to such critics, arguing that their own kinds of inquiry leave much to be desired, but propose, instead, to engage in some exposition of CA, though in a manner rather different to that which its practitioners usually do, which is that which has been used in the previous chapters in this collection. CA makes something of a principle of presenting itself through its work, showing the kind of inquiries that it makes and the kind of conclusions that emerge from them, a strategy which has on certain assumptions much to recommend it but which in our experience often does very little to increase the comprehension of the true nature of CA's efforts amongst those with doubts. As often as not, doing that just confirms, or even amplifies, their reservations. They can see from CA's studies, certainly, the kinds of materials that CA examines, the kind of things that it says about them and the way in which different studies under the auspices of CA perhaps complement and elaborate each other but they cannot see, from those studies, what underlies and motivates them. They

cannot, most basically, see what the 'problematic' is and, of course, without a recognition of that, many of the moves appear quite arbitrary. We shall be mainly concerned with the kind of criticisms of CA which are made from points of view dominated by 'linguistic' concerns and can note that the fundamental objection which emanates from these sources is that CA is unsystematic. Our basic aim is to show just how wide of the mark that complaint is, and that it is unlikely one can find a more systematic exercise anywhere in the human studies than CA, but it is, we think, likely to be in the forefront of objections because the critics are unable to recognise the character and extent of CA's systematicity.

We will, therefore, be concentrating attention on those things which underlie CA inquiries and which hold them together, and our exposition will not be of the particular results of CA investigations nor of their cumulative structure, but of the *elementary study policies* which motivate and direct these studies. Our hope is that some clarification here will remove some crucial misunderstandings and obviate much irrelevant criticism. It is our conviction that the misunderstandings amongst the approaches to the human studies are more important than the disagreements (at the present time). There are disagreements aplenty, we have no desire to minimise that fact, but parties to controversy often seem to be mistaken about the nature of the differences dividing them. Further, their misunderstandings are often over basic and elementary questions rather than over developed and sophisticated issues, and it is for this reason that we think that attention to some of the most simple and primary considerations may be more useful than attempts to build on the sophisticated and complex arguments and analyses that have gone before.

We start from some issues relating to the place of CA in sociology and then turn to discussion of its relations with linguistics, specifically that area designated as 'discourse analysis'.[1] The issues pertaining to CA in sociology are germane in a double fashion: there are many misunderstandings about CA in sociology and we might be able to contribute to clearing up some of those. Second, insofar as the argument is directed to people whose main background is in linguistics, then they may find a much greater awareness of what CA is 'up to' if they have some sense of the ways in which it bears the marks of its origins in sociology.

The Basis of Social Order

One problem which unites a good many sociologists is that of 'social order'. They seek to understand how activities in society possess such

orderliness as they do. They disagree about the answer that is to be given to this problem. However, they often agree about the form that a solution to it should take. They agree, that is, that what is sought is some *general principle* which provides for social order. They disagree on what the principle is. Thus, there are those who think that the general principle which assures order in society is that of harmony or agreement. There are others who could not disagree more about the nature of the general principle, and think that it is 'power' or 'control' which should be identified as this. The back and forth between these two broad points of view is long standing and continues. The purpose of much sociological investigation is, then, to show the relevance of the preferred general principle, to show that the principle *is* general and that it permeates social life, that this or that activity exemplifies the working of harmony or, alternatively, control. It is not our intention to question the legitimacy of that conception of what sociologists do, but only to doubt the supposition that inquiries are *definitive*, identifying the *only possible* conception of how sociologists might think of the problem of social order. They are not. There is at least one other conception.[2] This is one which seeks to understand the *practical production* of social order, which seeks to understand how activities fit together into stable or changing patterns, with how activities *make up* patterns of activity and how, through their interrelation, they produce and reproduce the activities they compose. We appreciate that presenting deep, dense arguments with this degree of condensation does not help to elucidate them but will in the articulation of the discussion do this in a way which should make much clearer just what we are saying here. At this point, suffice it to say that the task of seeing how activities relate under the rubric of some general principle is replaced by that of seeing how the activities 'dovetail' with one another. Thus, where the usual concern with social order is primarily about the relations between groups, organisations, institutions and persons, the interest, in this 'alternative' conception is primarily on the relationship between *activities*. Rather than thinking of society as a system of groups, institutions, positions etc. it thinks of it as a system of activities (insofar as it is a system at all).

Perhaps it begins to be apparent why we have stressed the problem of 'getting the hang' of the various approaches in the human studies. If the kind of outline we have given of the options is at all accurate, then one can see that the move from the one to the other is a shift in the way in which phenomena are to be viewed: one is being invited to look at social organisations as a system of activities rather than a set of interpersonal relations. That, surely, is a significant change which must ramify through all the things one does. How it does so is not, of course, at this point, at all clear: it is far from clear what is involved in looking at something as a system of activities.

An investigation conducted under the auspices of the practical management of social order does not and cannot provide an answer to the question 'what general principle provides for social order?' If examined from the point of view of an interest in that question then such an investigation will appear to lack an answer to it. However, such a study does not *fail* to produce an answer, since it does not *try* to answer it. It has withdrawn that question and substituted another one.

What is involved here is a perfectly legitimate step in the work of theorising, namely that of varying the givens. Any approach to inquiry has its boundaries, the matters which stand outside the reach of its inquiries, the things which must be taken on board without examination. Sociology, thus, typically takes it for granted that there is a 'world of daily life', that the commonplace affairs of life-in-society are regularly carried on and are possessed of an orderly and (relatively) stable arrangement, the object being to identify the conditions which engender and assure the occurrence of such arrangements. From sociology's point of view it is simply a given that the members of society *somehow* organise their activities to comprise the affairs of daily life, that they *somehow* co-ordinate their various doings to make up such commonplace matters as the provision of meals, the delivery of mail, the election of rulers, the holding of sporting events etc. etc. Insofar as this is considered problematical, then it will be treated as (characteristically) posing a problem of 'general principle': do people stick at the affairs of life because of – in general – an attachment of shared values and common rules or because of the dull grind of economic compulsion?

It is not, we repeat, a complaint that sociologists take for granted the fact that people stick at everyday affairs sufficiently to make happen, to put together, the round of diversified activities: given their problematic there is no reason why they should open this up to inquiry. However, if someone wants to ask about that which sociology usually takes for granted, namely that there is an orderly round of everyday affairs available *in the first place* and asks how do those ordinary activities put themselves together then they are free to do so. Note, though, that the question concerns not origins but constitution: what do the affairs of daily life consist in?

There is an issue between these two approaches to social order. It is that this latter question cannot be raised within the framework provided by the more usual conception of the problem. There is an issue of *access to phenomena*. The complaint is: one cannot, from within the framework of the traditional conception of order, organise inquiries into the ways in which everyday affairs are constituted, how they make themselves up as the everyday affairs that they are.

Notice, the claim is not that one *must* abandon the received frame of reference and take up the new problem, but only that if one wants to take up the problem *then* one will be compelled to step outside the received framework. The claim is, first, that there is a *bona fide* and unresolved problem for sociological inquiry which is that of the organisation of everyday affairs as such, and second, that this problem cannot be examined from within, or by simple modification of, the established framework of investigation. In order to open up to investigation the organisation of commonplace activities one needs a new framework, for the conduct of inquiries into social order conventionally depends upon taking these things as given, the accepted form of inquiry *requires* that these issues be treated as givens.

Within sociology proposals such as those we have been sketching are met with objections, many of which are motivated by assumptions about the way in which all sociological investigations must fit within some 'master scheme' and which complain that the proposed studies do not fit comfortably within some envisaged comprehensive framework. Thus, for example, proposals to open up the world of daily life for examination in its own right, as an organised arrangement of activities, is characteristically treated as the basis of a complete and general conception of what sociology is and can do. Hence, it is objected that we cannot take 'everyday life' at face value and that we cannot allow that to be the ultimate locus of sociological inquiry, that we must 'go outside of it' or 'behind it' to really understand it. Thus, the conflict is made to appear as one between those who envisage the world of daily life as the ultimate end of all inquiries and those who (with philosophical and scientific precedents on their side) want to insist that it cannot be. However, this is to treat a proposal to open up a problem for examination as though it comprised a master plan for the future of all inquiry, as though a commitment to consider an (allegedly) neglected topic was an attempt to circumscribe, finally, the whole range of sociological possibilities. Someone proposing to examine the availability of the world of daily life and to shift the way that the problem of social order is viewed might hope, even expect, that this move would have ramifications *throughout* sociological thought, but it would be a remarkably prescient person who could see if it would have far reaching consequences and what these would be.

Criticism, thus, is characteristically of and between projects. We have before us a battery of proposals as to how we might go about sociological inquiry. Sociology is a heavily programmatic pursuit, its main and dominating elements being plans for investigation, actual investigations often being 'toy' versions of projected investigations, exercises to show what we might do if we began to follow a programme through fully seriously. However, the conversion of sociological programmes into a successfully accumulating

collection of investigations often proves more than a trivial and technical problem and argument is, consequently, more characteristically about the promised than the proven success of plans for research. Much criticism, therefore, consists in allegations of constitutional limitation, claims that one approach will be constitutionally incapable of doing what another approach can do, without too much concern to ask whether it is *relevant* to a given strategy that it be able to do the things that (at least allegedly) it cannot do. Thus, the kind of criticism to which this alternative proposal is subjected are prevailingly to the effect that it will not explain this issue or be able to take account of that one, criticisms issued on the assumption that the proposal outlines and envisages a comprehensive scheme for the treatment of all the problems which might conceivably fall within the domain of sociology. However, as we have outlined it, the proposal does nothing of the sort, and the question of whether it can explain this matter or take account of that one can only come after it has been decided whether it *needs* to provide an explanation or an account for these, whether its proposal is faulted by offering less than a complete scheme for sociology (or even, on less immodest scale, for the whole of the social sciences). *If* it were to be established that such an approach did need to attempt explanation of this or accounting of that, it would then need to be established what it would take and how such limitations as there are to the proposed strategy are to be determined: how are we to be able to tell, with any confidence, whether a proposed strategy will *eventually* be able to tackle this, that or the other problem? What a strategy *might* achieve is really only something to be tested out in practice by applying it and seeing how far it will go.

There is a deep divergence of attitude, here, and it is a divergence which makes a difference to one's judgement as to whether certain matters are worth arguing about. Not, that is, because one does not think them important, but just because argument about them — at this point in time — is not going to make any useful progress. An attitude which can be taken is that which we can call 'living in the investigative present'. This is not, we think, the prevailing attitude of the human studies, where one of 'living into the envisaged future' is vastly more common. Much work in the human studies is governed primarily by the need for a sense of direction, by the conviction that we must be going somewhere and that the need is to take us to our destination as quickly as possible. Thus, and in the most classic instance, it is supposed that we are moving towards becoming like one of the natural sciences, and we must therefore do that which, in the present, promises to make us most expeditiously like the natural sciences. Current moves are, therefore, judged first and foremost in terms of where they are likely to let us end up. Thus, for example, CA will be criticised because it does not seem to

enable us to adopt that sort of 'systematic' (i.e. sometimes quantitative) method that we shall need to employ if we are to become more like natural (even genuine) sciences. 'Living in the investigative present' is an attitude which may be adopted in response to the suspicion that judging present moves in terms of an envisaged future requires the kind of prescience which is simply not available. If, after all, a persistent problem for *all of us* is that of moving from outlining a programme of inquiries to actualising it, then this means that we are all very far from being able to see *if* things will work out, let alone *how* they might do so. No one is in any good position to judge which strategies of inquiries are likely to pay off best in the longer term, and we might as well then, consider what we can do now without prevailing concern for where we might be when and if we manage actually to do the things we are now attempting.

From the point of view of 'living in the investigative present' it looks as though there is a great deal of 'writing off' of problems and strategies as insignificant, worthless, pointless, ineffective etc. where there can be no real understanding of what it is that is being written off. A great many judgements just look very ill founded and vastly premature.

For example, one of the things which is subject to recurrent debate amongst sociologists is the possibility of an 'interpretive' sociology.[3] People argue about whether sociology is destined to become a genuine 'hard' science or whether it must always remain an interpretive (and therefore 'soft') one? Are interpretive methods sufficient to comprise adequate inquiries or must they be supplemented by (or even be supplements to) much more 'objective' forms of inquiry? These debates continue *ad nauseam* but one thing which is clear about them is that those who join them have little clear idea of what an interpretive sociology might actually be.

An opposition to 'programmatic' discussion can result. One need not oppose programmatic discussion as such, but one can object when it acquires the character that it takes in sociology, that of *endless* programmatics. There is little point in continuing the discussion in that way. If an 'interpretive sociology' is an arguable possibility for sociology, then the best way to find out if it is a viable one is to get on with contriving one, with trying to *work out* what one might be accepting or rejecting if one goes for an interpretive sociology.

Of course, one is not promising a quick solution to the arguments. It cannot be supposed, if one is attempting to move beyond the realm of programmatics, that one will rapidly be able to say whether an interpretive sociology will be, first, a viable strategy and then whether it might be the optimal one. *Seriously* attempting to put together such a thing as an 'inter-

pretive sociology' cannot be undertaken in the expectation that such a thing can be produced at a stroke, that one's initial efforts will indeed result in anything that is clearly indicative of the final shape of the whole. Living in the investigative present means that the construction will have to be a step-by-step matter, with only the most limited capacity to see where present steps will take us and, very often, leaving us without any capacity to say right now whether we shall, sooner or later, be able to answer a given question or tackle a problem. Developing an interpretive sociology is not like following out a prepared blueprint to construct something whose structure and properties can be anticipated, it is rather a matter of discovering what such a thing might be as one goes along trying to turn a vague idea into something specific.

We have been pointing to some things which make relations between the kind of strategy we have been outlining and that from which it deviates and showing it is not that these lead to disagreements on specific points, but that it produces divergent judgements as to how far some matters can be discussed at all (in present circumstances). CA has certainly taken an attitude of living in the investigative present, which means it must be simply unresponsive to much criticism. Much argument is about whether it is worthwhile following out CA's line of inquiry but such argument invites claims and counter claims as to what CA might come to be, how its inquiries might finally turn out, but CA is in no position itself to say this and hence less than likely to be impressed by critics who seem, somehow, to know this.

The Organisation of Social Actions

We have subsumed CA under the heading of sociological approaches but have tried to indicate that its relation to other approaches is likely to be an uneasy one, not because of direct rivalry, but just because they are, so to speak, at a tangent to one another, in some way related and marked by common concern, but in other respects the vehicles of very different policies. We should expect that the relations of CA to some approaches in linguistics may be no less uneasy and for much the same reasons.

Sociology, as we have seen, pervasively makes the assumption that the world of daily life is an orderly, predictable place. It assumes that the world of daily life 'makes sense'. When we say this, we are not proposing anything more drastic than that (say) when we see someone in a store handing over money and being given a loaf in exchange that we are seeing someone buying a loaf of bread, or when we hear someone say to someone else 'Take out the

garbage' that we are hearing someone being told to take out the garbage. Ordinary, everyday activities are readily recognisable for what they are, visible as the commonplace occurrences that they are. For the kind of alternative we have outlined above, the fact that these ordinary events are *recognisably* commonplace occurrences is not something which is detachable from their commonplace character. Being readily recognisable is an *essential* feature of their commonplaceness and so, naturally, the issue of how ordinary activities can be recognised for what they can become is a central question. How do those who inhabit it make sense of their everyday world? Thus, one way in which an 'interpretive sociology' might be initiated is by examining the ways in which people 'make sense' of their everyday environment, how they 'see the sense' of the most ordinary activities.

Seeing the sense of ordinary activities means being able to see what people are doing and saying, and therefore one place in which one might begin to see how making sense is done is in terms of the understanding of everyday talk. It should not be imagined that one is going to set the understanding of people's sayings *against* the understanding of people's doings, such that we shall then have to face problems as to how sayings relate to doings. One focusses on how people make sense of talk as a way of getting access to the examination of the way people make sense of each other's activities, and one sees that making sense of each other's talk is integral to, and often identical with, making sense of each other's doings. It is not as if we have (say) social activities on the one hand and linguistic matters on the other, but that we are undertaking to look at talk as an organised social activity.

One of the ways in which CA goes about this engenders objections. CA characteristically works with tape and transcript of verbal exchanges, showing little acquaintance — if any — with the circumstances under which the recordings were made or the way of life from which they were extracted. Thus, CA meets with the 'enough data' question right from the start. Does it have enough data to be able to say anything? The answer to such questions must, of course, depend on at least two things: What the data is, and what it is being used to license talk about? From CA's point of view we cannot say what the data is until we have examined it. Can we say anything about social organisation from the contents of tape recordings and transcripts of same? Well, what is there on a tape recording: How can we say unless we listen to it and characterise what is recorded on it (and not just characterise it in some gross and general way, but in terms of its detailed and specific features). If we do that, then we are examining and *analysing* the talk. From CA's point of view, it is not as if we can determine what data is independently of the analysis of it and thus CA's investigative exercise is to determine the charac-

ter of the data, to discover what the contents of tape and transcript are, and coterminously, to discover what kinds of things can be said about and on the basis of that data. The point is certainly not to suppose that any and all questions can be answered on the basis of such materials but to try to work out, through inspection of them, just how many and what sort of questions can be solved. After all, the value that data can have is not a function of its intrinsic character but of the questions that are put to it, and the ways in which they are designed to extract maximum legitimate value from the materials available. In many ways, then, CA adopts a reverse strategy to that followed by most forms of sociological inquiry. It takes a 'data driven' approach, making its question not 'what data do we need to answer this question?' but, by contrast, the very different question 'what questions can this data answer?' It may, of course, be that work in conversatonal analysis does (as work in any area can do) exceeds its legitimate brief and claims things which it cannot legitimately claim, asserts things that could only legitimately be said if, say, one knew a great deal about the immediate circumstances or general milieu of the data collection but this would be faulty CA, not evidence of the intrinsic inadequacy of its materials or methods.

At the very least should not audio recordings be complemented with video recordings? Are not the paralinguistic and kinesic environments of verbal interaction absolutely essential to the interpretation of the verbal component? Is it not even likely that the verbal component is much the less important part of the communicative process.

Once again we are in the position in which people are making judgements as to the relative importance of things which they have not examined. How important is talk to 'the communicative process', is it more or less important than the non-verbal component? Who is to say? No one really knows what 'the communicative process' is nor what parts the verbal and non-verbal components play in it since neither have been analysed and described in any systematic way. It was, thus, partly and importantly, in reaction against what can only be called *prejudicial* judgements about the nature of the communicative process that CA formed its character: it was not prepared to accept that the verbal was much less consequential than the non-verbal channel when no one could say what went on through the verbal channel. Consequently, it set itself the task of taking a good look at the 'verbal component' to see in what that consisted, *considered as a socially organised matter.*

It is probably important to stress that CA's programme is not designed to reverse the judgement on the relative importance of verbal and non-

verbal communication, to claim that the verbal is what matters at the expense of the non-verbal. It is designed to identify and describe the organisation of the verbal exchange as such, to see how the *talk* is organised *as talk*, a task which can be conducted quite independently of the examination of non-verbal communication. At least, it is possible to undertake the examination of the organisation of the verbal exchange and to identify some of its organisational features without having also to take *systematic* account of non-verbal phenomena.

It is, of course, tempting to think that one *must* examine verbal and non-verbal phenomena in conjunction because they are clearly related phenomena, but the issues pertaining to the way in which phenomena are to be examined, whether simultaneously or independently, must depend crucially upon the conception of the sequenced character of operations making up the programme of analysis, the *kinds* of relationships one is looking for.

One may, for example, be looking for 'correlational' connections between verbal and non-verbal behaviour, seeking to find if the two 'move together'. Thus there is the predominant concern with the relationship between speech and gaze direction, one which is designed to show that there are predictable points in the course of a speech exchange at which parties to it will direct their gaze at each other. Such a method of investigation is not designed to identify the structure of *either* the verbal or the non-verbal elements of behaviour. It makes no attempt to work out in a thorough or systematic way of what *either* channel of communication consists, how they are made up and how their respective structures might be integrated.

From CA's point of view the examination of the relationship between the verbal and non-verbal aspects of communication is not necessarily to be sought by looking for some correlations between particular features of the two 'components' but, instead, of looking for interconnection of their respective structures: only if one has specified what the structure of the verbal and non-verbal channels respectively are can one start to work out how those structures are interconnected. Thus, though there might well be points at which the analysis of the organisation of the verbal exchange cannot proceed without consideration of the relevance of some 'non-verbal' occurrence there is no reason to suppose that one cannot begin to examine the organisation of the talk without simultaneously examining non-verbal activities.

What is it to determine the structure of the organisation of talk? Such a question cannot be given some general answer, but must always be relativised to a point of view. From CA, to determine the structure of the verbal

exchange is to identifhy its properties as an organised system of activities, which means, at its most rudimentary level, to specify what conversation consists in as a succession of actions, and to see how those actions are related one to another, how they build up the conversational sequence.

This, however, transforms the character of the problem, away from that of the connection between dimensions of a communicative process and into that of saying how the connections between actions are generated. The primary characteristic of the utterances that CA deals with is often less that they are *verbal* actions, but that they are *actions*. There is, thus, no need for CA to insist that verbal actions can only relate to other verbal actions for they may relate, as well, to non-verbal ones. One feature of the interrelation of actions to which CA pays great attention is that of pairing, the relation of 'first' and 'next' action. It is entirely possible for first action to be a verbal action and for 'next' to be a non-verbal one, as for example a first, spoken greeting can be returned with a smile or other gesture, as a spoken offer 'help yourself' can be responded to by doing just this. CA is not, then, precluded from looking at 'non-verbal behaviour' except by its own 'one thing at a time' strategy, though if it is to extend its inquiries from talk as such to other aspects of the 'communication process' it will be constrained to do this by looking at such behaviour to see what kinds of actions constitute it or how it relates to the organisation of verbal actions. It will have the first task of doing, for non-verbal behaviour, something analogous to that which it has done for verbal behaviour, namely trying to identify the actions constituting it (which is not, for example, what gaze directions studies try to do).

Those who propose to examine verbal and non-verbal communication conjointly may do so in the supposition that they have a superior strategy to that pursued by CA but from the latter's point of view it will not appear so, only that the very phenomenon to which CA addresses its inquiries will be lost. The very things that CA wants to examine, the socially organised structure of the verbal exchange, is not something that the proposed mode of analysis is likely to be intended to capture and the things that CA needs to identify are ones it will not discriminate. For example, one may examine Beattie's *Talk* (1984) as a reiteration of just the kinds of claims about the indispensibility of analysing talk and non-verbal behaviour conjunctly, and which seem to make these claims in an unsuitably generalised manner, without any indication of whether *for some purposes* it might be the case that one indeed cannot isolate the analysis of the verbal from the non-verbal and *in yet other cases* that this might be, strategically just the move to make. Beattie's investigations are into such matters as turn taking, interruptions and (that eternal favourite) gaze direction and these are matters which are germane to the examination of conversation and to CA

but there is no interest shown by Beattie in, nor evidence that his strategy would enable him to address, those issues which are central to CA's inquiries, those involved in describing the character and organisational interrelaton of utterances, in seeing how the talk constitutes and compounds conversational structures.

Claims as to the relative superiority of this or that approach are made with respect to their adequacy in respect of such large and vague topics as 'language', 'interaction', 'communication', 'discourse' etc. with little attention being paid to specific things that a particular strategy may (and usefully) be seeking. Thus 'Discourse Analysis' (hereafter DA) and Conversation Analysis are set up as being in more or less direct competition, something which is perhaps natural if one thinks of them both as 'approaches to discourse'. If one thinks of them in that way, then it must seem that we cannot have two different and comprehensive approaches to discourse and therefore we must choose one of the two but if one sees that to treat them as candidate approaches to 'discourse' (as if each sought to give comparably general and comprehensive accounts of the same thing) is to fail to recognise that they are actually interested in discourse in very different ways, seek to determine very different things about discourse and have very different ambitions with respect to the treatment of 'discourse as a whole' then one might also see that they do not provide an occasion for choice of this sort.

Choice is, of course, inevitable. There are numerous ways in which one may set out to investigate matters in human social life, and there is a vast multiplicity of things to which one might attend. The choices which the human studies face are presented as though they were the kind that scientists in disciplines with some mature theories might face, those of choosing which of two rival theories is best but, in our submission, this is seldom actually the case. There is a need for choice, but it is more of the kind that the economist identified as 'opportunity cost': In order to do one thing one must forego the opportunity of doing another. Few of the strategies available to the human studies are very developed or of much complexity, and they are not capable of accommodating the diversity of interests and problems that different researchers might conceivably have. It is not, then, that an approach can claim to surpass another by incorporating its interests into its own, more comprehensive system, and tending to them there. They tend to claim superiority, rather, by denigrating and seeking to rule out those interests which they cannot accommodate and, as often as not, this means that these interests must be diminished on methodological grounds: one defines legitimate topics on the grounds that they can be treated by a preferred method, rather than identifying preferable methods on the grounds that they provide access to a richer range of topics.

Thus, there must be a choice between 'dicourse analysis' and 'conversation analysis' but, if we are right, it is, initially at least, simply because one cannot do everything at once nor go at things in very different ways simultaneously. One cannot look at talk both from the point of view of CA and that of DA at once: one can look at it from that of either, but not from that of both.

There are some overlaps between them, but these do not presage a close convergence or easy integration of strategies. Discourse analysis is concerned to provide a corrective to a tradition of linguistic analysis which has, in its view, been too prone to treat the sentence as an isolated object of analysis without regard for the location of sentences within a sequence of sentences (its discourse aspect) or in terms of its relation to the uses to which language can be put (its pragmatic character). What is being disregarded within the main traditions of language study is, to put it crudely, the fact that language is a social institution. The main tradition of language study knows perfectly well that language is a social institution but the fact that it is does not figure very largely in the form of analysis that is there developed and employed. It has, at best, a tacit presence. However, the attempt to develop that main tradition runs into difficulties, some of which are manifestly because it pays insufficient attention to the fact that language has a part to play in social life and verbal exchanges. Consequently it is felt necessary to give the fact that utterances occur in social contexts a much greater prominence than it has been given hitherto, but such a step is designed as a modification, *rather than a transformation*, of the frame of analysis developed by the main tradition. The exercise is still *primarily* a linguistic exercise into which 'sociological considerations' enter in a supplementary role and which, when they do, are handled in a largely *ad hoc* fashion.

As an indication of this, consider the following remarks from Brown & Yule's (1983) *Discourse Analysis*, the first of two useful and simple illustration of the extent of the problems that we have been discussing. They propose that 'from the discourse analyst's point of view' a promising approach to the study of social meaning 'is offered by a consideration of that area of conversation analysis which investigates *turn taking*.' They point to the work of Sacks, Schegloff, Jefferson and others in this area and, particularly, to the role of the notion of 'adjacency pair' in this work. They initially grant that the notion of the 'turn' as a unit of analysis is reasonable, but immediately qualify: 'However, most conversational data consists of more substantial "turns" in which several utterances can occur, or in which the basic adjacency pair organisation is difficult to determine.' They provide a data extract, shortly to be reproduced, to make these difficulties visible, and on its basis are able to suggest that some of the interrogative forms function

as both answers and questions, and that the final declarative form is not, in fact, an answer to any of the questions. This provides them with sufficient ground for overall judgement on the capacity of CA:

> 'the immediate question which springs to mind is how does the analyst determine when an interrogative form counts as a question in an adjacency pair, or as part of an insertion sequence, or even as an answer? This type of question is never really raised by those undertaking the analysis of conversational interaction, largely because little attempt is made to discuss the relationship between linguistic form and the interactive functions proposed.' (Brown & Yule, 1983: 230)

Such objections may, in terms of DA's own project, be telling ones, if what is sought is a *systematic* relationship between 'linguistic form' and interactive function. But from CA's angle the making of them amounts to no more than begging the question, for what CA has taken as its most elementary methodological presupposition is that the identification of the interactive function of an utterance cannot be determined in this formulaic way, that it is a *local and circumstantial* matter, involving the examination of the way the utterance is *embedded in the interactional sequence* to determine just what interactional role a given utterance has. There have been various arguments designed to show that from the point of view of CA many characterisations of 'linguistic form' are superficial, and do not capture the interactional character of the utterances they are designed to describe. CA *does* think about the relationship between linguistic form and interactive function, but it does not reach the same conclusions that DA does, that it would be practicable to provide 'the analyst' with a *mechanical* procedure for identifying the interactive function of a given utterance type.

Further, Brown & Yule's objections betray an apparent failure to appreciate that the priorities of CA are substantially different from those of DA at a methodological level. Brown & Yule, and DA more generally, show a concern which is characteristic of many in the human sciences, which is to have an explicit and mechanical method for processing data, where the objective of inquiry is to provide the analyst with a set of worked out categories and explicit procedures for characterising data and where it is a shortcome of the method if it leaves it uncertain or equivocal as to how an instance should be described. However, it is notable that CA does not give primacy to these policies, that its objective is not to equip the sociologist/psychologist/linguist with methods to describe events in conversation but that it is concerned to identify and describe the methods which the *participants in conversation themselves use* to order and describe conversational

events. This does not, we should stress, bespeak an indifference to the provision of rigorous control over one's inquiries, but it does mean that the considerations of what would provide the most rigorous treatment of a particular matter will be at variance with those which others in sociology, psychology and linguistics would prefer to use. There is, thus, a policy of systematic transformation of the objects of inquiries into conversational matters. For CA the relevant question is not 'can we, the conversational analyst, provide a surefire way of telling this or that' but, invariably, 'is there a conversational way of telling this or that?'

Far from being an evasion of the difficulties or of the standards of scholarship that is, we submit, a matter of pursuing, entirely consistently, one of the suppositions of the programme, namely that the observable character of social phenomena, their visibility for what they are, is not something contingently but *essentially* connected to the nature of them. It is not just a casual and occasional concern of conversationalists that they should have their co-participants recognise what they are doing for what it is, that they should see the projected sense of remarks and recognise the action implications of an utterance. Having others see what you are saying and responding appropriately is part of the very business of conversation and it is therefore always relevant to ask, in CA's terms of reference, how are the parties organising their talk to make these things recognisable amongst themselves, what conversational ways are there of seeing or showing this or that? Such questions do not suppose that there must be resolutions to ambiguities and uncertainties, for 'clearing up' obscurities, confusions, misunderstandings etc. is *one conversational task* amongst others and not always the highest priority item: What some interactional item did is something that may remain wholly unresolved. This point applies, also, to problems in the identification of turns just as much as to the identification of utterance types. The problem is not to provide us, conversational analysts, with a way of deciding whether a bit of talk comprises a distinct turn or not, but to see how parties to talk decide this. For just this reason that CA has always placed greatest emphasis on the fact that *the turn is itself* an interactionally defined unit, that it is for the participants themselves to figure out whether or not a turn was complete, no stronger solution to such questions being available to them — very often — than that it was possibly complete but this, it transpires, is often good enough for the organisation of conversation's business. Brown & Yule point out that 'most conversational data consist of more substantial "turns" in which several utterances can occur' as though this were something unknown to CA and a basic fact of which it is incapable of taking account, but CA's account of turn taking has precisely been designed to take account of this fact and to enable it, therefore, to treat as 'a

conversationalist's problem' that of getting an extended opportunity to talk in an environment regulated by a turn taking 'machinery' which favours short utterances (to put it crudely). Thus, for example, there has been attention paid (to give just one example) to the need to anticipate and notify the likelihood of a long stretch of talk coming up as in the prefacing of stories: The basic observation is that stories characteristically take more than one utterance to do (Sacks, no date, 1974).

The crucial notion, quite a central one to CA, which Brown & Yule feature in their discussion, is that of 'adjacency pair' but their judgement on its analytic utility does not begin from a concern with the role that it plays in CA. It is assumed that the notions of 'turn' and 'adjacency pair' are being offered as analytic units out of which we could potentially compound a systematic analysis of all the utterances in a conversation, but this is not the way they are perhaps best understood.

Adjacency pairs are singled out for attention in CA because they are *widespread* phenomena in conversation and are (so to speak) very useful there in resolving *some* of the problems of co-ordination which confront conversationalists (cf. Schegloff, 1968, 1972). There is no suggestion that every utterance must be part of an adjacency pair or that every sequence must be a composite of such pairs.

Adjacency pairs provide part of the answer to CA's main problem, which is 'how is it that parties to conversation are able to co-ordinate turns at talk in such ways that they provide appropriate steps in conversational sequence?' It just is not to be supposed that such problems are always solved in the same way, that something which provides *a* solution provides *the only* solution. Adjacency pairs are singled out because they provide one elementary and frequent solution to the problem of what to do next in conversation. If a party to conversation can identify a previous utterance as one of a normatively linked pair, such that its occurrence makes the production of a second, corresponding part of the pair appropriate, then that person knows (at least) what kind of utterance is appropriately produced next. Thus, if 'question' and 'answer' are paired in this way, the production of a question makes the provision of an answer appropriate. Of course, since the connection between the parts of the pair is normative, it is not assured that what *will* happen is that the appropriate second part of the pair will appear on cue, though the fact that the first part has been issued can be most relevant for organising the ensuing talk (as Brown & Yule's example shows well). The example is:

George: Did you want an ice lolly or not?
Zee: What kind have they got?

George: How about orange?
Zee: Don't they have bazookas?
George: Well, here's twenty pence + you ask him.

The fact that one question gets 'answered' with another question is hardly news to CA, and the fact that an interactional sequence does not produce something identifiable as an answer to a question is hardly likely to surprise it either. That a question is asked does not ensure that it will be answered, it does not even guarantee that it will be addressed: questions can go unanswered. CA has, to make just one point, been careful often to speak of 'candidate answers' in order to respond to some of the complexities of the situations questions create: after all, whether something is an answer to a question (sometimes) depends on its informational status, whether it gives the right information, rather than on its linguistic form.

The point about the notion of adjacency pairs is not that it predicts, given a first, there will be a next. It explains, rather, what we might call an 'orientational fact', namely that given a first, parties will be looking for a next and hence may find that such an appropriate next did not occur. Brown & Yule's own discussion displays this orientational fact: seeing a question, they start examining the following utterances to see if they can find an answer amongst them? That there is no identifiable answer is, for them, a 'noticeable absence': a non occurrence is a remarkable thing (cf. Sacks, 1972a).

If DA complains that CA does not look at the relation between linguistic form and interactive function, then the reciprocal complaint that DA does not pay much attention to the character of the actions that utterances perform is also in order. Brown & Yule do not seem to appreciate that CA's first methodological rule is to look to see what kind of *action* an utterance performs and from this point of view, George's question is of primary interest not because it asks a question but because it *makes an offer*. George is not asking Zee about her psychological state, whether she wants a lolly or not, he is offering to buy her one, even offering to get it for her. An 'offer' is also the first part of an adjacency pair, making relevant the next action of taking up or declining the offer. Now, nobody supposes that anyone who is made an offer will immediately accept or reject it, because there can be quite long gaps between making an offer and finally accepting it or rejecting it (e.g. ringing back can be involved, consulting others first etc.) However, *if* an offer is made then we shall be disposed (as ordinary speakers of the language and ordinary members of the society) to see what the recipient of the offer does next as a response to the offer, and that is, of course, how we understand Zee's first remark, as a conditional acceptance of the offer: she is

not rejecting it outright, but making it depend on what kind of lolly she can have. Thus, we find a quite routine follow up to an offer, namely questions designed to clarify its character, elucidate its conditions and so forth. In this case it turns out that George goes through with the offer to buy the lolly for Zee but withdraws on the implication that he will get it for her, giving her the money to get her own. Thus, the offer is 'dealt with' though not in terms of a simple accept/reject response, we could not see the sense in the sequence without following it in terms of the relevance of the 'accept/reject' alternative.

It is likely that Brown & Yule, again like many of their colleagues in the human studies, have at the back of their mind an idea of these studies as putatively predictive in character such that we should be developing methods which will enable us to say what will happen, to predict what the next action will be if a first one occurs. Their strategy of analysis seems, like many others, to rest upon the assumption that its task must be 'determination of outcomes', i.e. that we must seek to see what happens as a result of, and therefore as predictable from, a set of specifiable conditions. From such a point of view, a notion of adjacency pairs such as we have outlined will probably seem 'slack' since it does not predict, at all, what will happen. Given a first, an appropriate next may occur but it need not. Surely what we want, the demand often goes, is a way of saying what will happen next? There are other possible notions of what our tasks may be, however, and the kind which CA may be seen to be engaged in is rather more in fashion (given the 'new realist' philosophy of science's rising stock, with its emphasis on investigating the nature and constitution of things) than the view of the objective of inquiry as prediction (though, of course, it takes a long time for many forms of inquiry in the human studies to catch up with fashions in the philosophy of science): CA is concerned with the nature and structure of conversation, and as such one thing it is concerned to do is to capture and preserve what conversation is likely to do and it takes it that, for those involved in conversation, it does not have a definitely predictable character. Conversation is a risky business, such that one can seek to predict and control what will happen next, but one is not assured that what one projects will happen. The adjacency pair allows for just that fact, for the production of a first part of such a pair makes relevant, but does not ensure, the occurrence of a next.

We have indicated that we are more concerned to map divergences than to argue the rights and wrongs of them, and we have therefore been mainly aiming to show that Brown & Yule's criticism of CA seems premature and deriving from a lack of appreciation of the very different kind of exercise that CA is from that which Brown & Yule envisage for themselves. The

criticism has been of Brown and Yule's understanding of CA and, therefore, of misconceptions about the basis on which DA might claim superiority. Our arguments do not show that DA is not superior to CA nor does it show that there is something constitutionally wrong with DA: after all, given the nature of our complaint against Brown & Yule it would be premature to do this. If successful, we have shown that (so far) the efforts of CA are neither invalidated nor rendered superfluous by those of DA.

Forms of Systematic Inquiry

However, we have not yet touched upon what is offered as the most telling objection against CA by DA, that the former is an unsystematic exercise, and that the latter, putatively more systematic, is therefore the superior one. We shall argue, as before, not that the situation is reversed and that it is DA which is the unsystematic exercise (though we think this is probably so) but only that there is no basis for DA to allege superior systematicity. There are two different conceptions (at least) of what systematicity involves and there is no *prima facie* reason why one should be vaunted above the other.

Consider the second example from Brown & Yule, which involves citing a fragment of (invented) data by Widdowson,

A: That's the telephone.
B: I'm in the bath.
A: O.K.

and the summary of his argument that 'it is only by recognising the action performed by each of these utterances within the conventional sequencing of such actions that we can accept this sequence as coherent discourse' (quoted in Brown & Yule, 1983). Such an instance shows clearly enough that from DA's point of view, the fact that utterances perform actions is a residual one, and hence one which is given no systematic attention. Where some utterances cannot be readily interpreted as a coherent sequence *then* one may note that they are connected as actions, with one party suggesting the other answer the phone and the second indicating why they cannot do this, the first then accepting this. It is entirely legitimate for DA to treat the fact that utterances perform actions as a matter of only occasional note, since they have quite a specific concern with relations between utterances, which is to find 'coherence' between them, something which can sometimes be done by linguistic features, sometimes by reference to the kind of conventions that a Gricean analysis employs and sometimes by taking note of the kind of

connections that actions may have to one another. However, let us note that *from the point of view of a concern with the organisation of social actions* such a study policy is quite unsystematic, making no response to the fact that any utterances can be examined as performing a social action and that one can raise, as a problem to be systematically pursued, that of the kind of connections which there may be between one action and another. Such a problem, naturally enough, changes the focus of investigation entirely, replacing the search for linguistic coherence with an investigation of the *organisational* relations between utterances in a conversation and the way in which utterances are organised into conversations. Such an inquiry requires pervasive (and exclusive) attention to the action-performing role of utterances and to the investigation of their organisational role.

A further alteration ensues. DA sees itself as engaging, primarily, in the description of 'cognitive' processes, describing the understandings which enable people to interpret expressions, whilst CA sees itself is treating primarily of interactive processes. From its point of view, DA stands in much the same relation to its elected phenomena as sociology conventionally takes to its own, namely that it takes the givenness of the activities (or utterances) it seeks to examine for granted. People do, in orderly ways, produce mutually intelligible utterances and the issue is to determine how, once those utterances are produced, they are interpreted. For CA, however, it is legitimate to ask how the phenomena comes to be available in the first place, how persons are able to organise their activities in such a way as to produce mutually intelligible exchanges of utterances — how is discourse made to happen? Thus, the concern is with the production and management of a socially organised occasion, the production of a flow of activities — verbal ones, as it happens — in co-ordinated sequence. Thus, the utterances which comprise a conversation are to be inspected for the ways in which they generate talk, for their potential in bringing about further talk, and for their capacity to shape the course of subsequent talk. Thus, to revert to the topic of adjacency pairs, one of the things which makes it of considerable interest is its capacity to project further talk: the production of a first part calls for the production (by another speaker) of a next. CA is, then, overridingly concerned with talk as a *collaborative* matter and with how parties to an occasion can jointly produce an organised sequence of talk, which means that it cannot treat problems of interpretation and understanding independently of those of production. For conversationalists, seeing what an utterance says is not a retrospective, reflective, academic, theoretical matter, but one of direct practical implication. For them, the issue is, abidingly, 'what to say/do now?'. Seeing what kind of action an utterance performs and what kind of next action it implies, invites, demands etc. is the primary issue,

integral to the generation and management of the exchange within which the utterance occurs.

CA is concerned, then, with the analysability of conversation *in media res*. It is not a matter of looking for regularities which will only reveal themselves through the application of sophisticated methods of investigations but of seeking to identify those features of conversational organisation to which conversationalists themselves attend and of seeking to see how they respond to those features. The process of interpretation with which CA is primarily concerned, then, is that which is situated within the conversation itself, which is involved in following and developing the course of the talk, and the investigative strategy is, therefore, directed toward describing the way in which interpretation and understanding are articulated with conversation's organisation. Rather than looking for 'cognitive processes' involved in interpretation, CA — consistently with its policy of looking for conversational solutions to problems — is engaged in searching out *conversational practices for achieving and exhibiting understanding*. Note that understanding is regarded as an achievement. That is, it is not something which is automatic or assured and parties must therefore reciprocally design their respective remarks in such ways that the projected recipient of them will see what they are saying. It is partly because of this that CA gives prominence to 'recipient design' (Sacks & Schegloff, 1979) as a feature of the organisation of conversational utterances. Utterances in conversation are not directed towards anonymous 'speakers of the language' but toward specific others, and conversationalists therefore pay pervasive attention to the issues of to whom they are talking, what such persons may be expected to know, what they will know without having to be told, what they will be able to read into what has been said without it being put into so many words, what they will be interested in and so forth. 'Recipient design' points the investigation toward the ways in which utterances are constructed specifically so that they will be understood by *this* recipient.

Since understanding is not guaranteed and since such practices as conversationalists have for designing recipient-intelligible utterances are not failsafe, then misunderstandings are a possibility in conversation and, therefore, they provide a source of conversational trouble, there is a need, then, to describe the ways in which such troubles reveal themselves, may be detected, diagnosed and resolved.

All in all, CA is examining conversation — to use a term from ethnomethodology — as a self-explicating system,[4] a policy which can be pursued with respect to social activities and settings of all kinds. The policy involves seeing how the setting makes its own organisation visible to participants,

how its arrangements can be examined from within so that people can see 'what is happening here' and determine 'what we're supposed to do now'. Thus, when applied to conversation it means examining how the talk making up the conversation is organised so that parties to it can determine 'what has been said', 'what we are talking about', 'where we are in this conversation', 'what further course this conversation might take' and so on. The answers to such questions are to be sought by looking at *how* the parties are talking, at whether they are talking in such ways as to be (say) 'opening up a conversation' or 'preparing to bring the talk to a close', whether they are talking in such ways as to show that 'one party follows what the other is telling', that 'one party is checking out that the other has not misunderstood', that the recipient is 'seeking clarification of the teller's remarks', whether the parties are 'in the midst of talking about some particular topic', are 'searching around for something to talk about' or are 'competing to decide which of two or more preferred topics they are going to talk about'. Such things as these are 'audible phenomena' in conversation. They are not ones which can only be discovered by statistical analysis or by close timing of utterances, the pauses between them etc. (there are, of course, some features of conversation which would be discoverable only by such means). That is, if we listen to a tape recording of a conversation we can simply hear that (say) everyone involved in talking about the same topic or (alternatively) we might hear the talk as involving two parties trying to get different topics started at once, and CA's problem is: How are these phenomena made audible to us, how can we hear them in the utterances produced? What features of and relations between utterances enable us to say that they are addressed to the same topic or, alternatively, that they prefigure two quite different topics. The point is, of course, to work out how those phenomena are available to the persons in the conversation such that those hearing a particular remark can see what topic it could introduce such that they can then say something which will also be 'about the same topic' or which might cut that topic off before it develops?

Such inquiries are wholly consistent with the maintainance of CA's focus on its elected phenomena, which is the organisation of conversation as a sequence of turns at talk, and with its task of describing the relationship between turns at talk as making up organised overall sequences. All such inquiries are dominated by and organised around the topic of 'turn taking'.

In the demarcation of academic territory, takeover bids are sometimes made. DA seems prepared to make one for CA. If one is interested in 'discourse', then conversation is one form of discourse and one thing which obviously occurs in conversation and some other form of discourse is 'turn taking'. Conversational analysis has developed a fairly elaborate account of

turn taking and it seems, therefore, only reasonable to suppose that one might include the phenomenon of turn taking within the range of topics covered by a 'discourse analysis'. However, such assumptions are reasonable enough if it is thought that turn taking is CA's topic but this is not, we think, the best way to consider the matter. It is perhaps more apposite to see the examination of turn taking as *the method rather than the topic of* CA. That does not involve looking at turn taking as one aspect of conversation but, instead, the examination of conversational activities wholly from the point of view of the necessity for turn taking. The policy is to examine anything and everything in conversation to see in what ways it is affected by/responsive to the basic organisational fact that conversation is a turn taking pursuit.

Conversation is, virtually by definition, the alternation of turns at talk and, therefore, whatever happens in or is done through conversation, must 'fit in' to the environment of alternations at talk. CA's work, then, involves considering conversational materials as comprised of 'sequential objects', as phenomena which are environed by, constituted of and distributed over turns at talk, and CA aims to describe the ways in which the occurrences of conversation are organised as sequential objects. CA's strategy is, then, to see just how far the description of events in conversation relative to the organisation of turn taking can be taken, perhaps finding that matters which may appear, on first inspection or from the point of view of some other kind of analysis, entirely independent of turn taking are significantly shaped by turn taking requirements. Thus, there can be no question (from CA's point of view) of making an a priori demarcation of topics between those which can be considered in terms of turn taking and those which cannot: the only way to determine how pervasive and in what ways the requirement for turn taking makes itself felt (organisationally speaking) is to examine phenomena relative to their placement in and constitution by turns at talk.

Far from providing a treatment of one subordinate topic within 'discourse', then, CA provides a method which could be applied in the reconsideration of a very broad range of topics within that area, of looking at the phenomena that DA is intended to cover as turn taking phenomena (insofar as they occur in conversation or are located in other turn taking systems). Looking at the matters treated by DA in terms of turn taking considerations could have far reaching ramifications for the strategies and topic structure of 'discourse analysis'.

The point of this section has been to counter claims that CA is unsystematic. It seeks to be thoroughly systematic in its single minded application of a point of view in the examination of a central problem. It seeks to

view conversation as a system of activities and to see how such a system can be organised 'from within', how participants in talk can collaboratively construct orderly sequences of turns at talk and thereby accomplish routine activities of their life in society. It is persistent in its examination of conversation as an organisation of interrelated social actions and equally persistent in its examination of how those actions can be embedded in and concerted through the turn taking sequence. It is equally persistent in its exclusion of phenomena and problems which cannot be treated in such terms and in maintaining its focus on the organisation of talk *as such*.

It is this last self-imposition which is apt to be regarded as CA's most gross, obvious and grievous error. How can one possibly know what is going on in a social situation (and in the talk which comprises it) without knowing a very great deal about the situation, the characters involved in the talk, the structure of their relations, the history of their personal connections, the business they have in hand etc.? Such a question is probably best looked upon as rhetorical, meant to need no answer since possessing the implication that one cannot possibly know what is going on in a social situation without knowing a great deal about it. Simply having a tape recording of some talk cannot, surely, be enough?

'Enough for what?' is the only sensible response. Any method can be abused, of course, and it is entirely possible that people will seek to use tape recordings of talk as the basis for claims which they cannot, on the basis of such data, make (but making claims which are ostensibly licensed by those actually unsupported by one's data is hardly a failing unique to conversation analysts). The fact that a method can be abused does not mean that it is intrinsically deficient: whether or not one can rely on tape recordings alone depends, very much, upon what one is doing with them. Materials do not *intrinsically* possess or lack value (one might, for example, despise archaeology on the grounds that it involves grubbing about in the middens of lost civilisations). How much use can be made of materials will relate to the problems that you have and the techniques that you develop to treat them.

Let it be remembered, then, that CA's focal concern is with *the talk* and therefore a tape recording is a recording of the very phenomenon that CA intends to inspect. It does not commence upon the inspection of those materials and that phenomena on the basis of suppositions that it will be able to do this or that, but initially undertakes the examination just because it does not know what such data might be good for. Until it has examined them and tried to analyse them CA does not know what it will find on tape recordings, what kind of phenomena they will make available to it and what kinds of problems it will be able to pose and resolve through the investiga-

tion of such materials. The nature of tape recordings as data is something to be discovered by inquiry: What they are data of is not to be determined in advance.

Such determinations should be cautiously made. It is not a matter of making such materials answer any and every question in sociology, but of seeking to see which questions can be put to them in such ways that they can be answered through these materials. Clearly there must be issues which cannot be answered on the basis of tape recordings of talk alone and without familiarity with many 'background' matters involved in the situation and the relations between those involved there are many things one cannot definitely determine about some sequence of talk. However, the response of CA to that fact is to try to develop a mode of analysis which is largely independent of such knowledge. It is not trying to devise a form of inquiry which can substitute for the possession of local, specialised or expert knowledge in understanding some things about social situations and organisations of activities but in trying to identify those things which can be studied without reliance on such knowledge.

Reliance on the talk/transcript alone is a device for enabling discriminating analysis and specification of relations between organisational features. It is not enough to make programmatic pronouncements to the effect that there are (must be) relations between talk and social context or that talk itself is a form a social action. The need is to be able to say just which features of talk relate to just what kinds of features of the social context, to say just what kinds of social actions make up sequences of talk and how they do so. Thus, CA has sought, as its prevailing objective, to show how the social context of the occasion 'the conversation' is relevant to the production and interrelationship of utterances, to show just what the activity 'talking together' consists in, to achieve the *analytic isolation* of those features of the verbal exchange which are shaped by the fact that they are being done through conversational talk.

Such a method does not involve denying that things which happen in talk may be decisively shaped by such matters as the respective social standings of the parties involved, their organisational capacities, their personal connections, the history of their relationship etc. for, of course, it does involve (often) disregarding the ways in which this might be so in order to isolate and examine pure turn taking and conversational phenomena. CA is quite able to take notice of the fact that persons in conversation have social positions and affiliations without modification of its basic strategy. Thus, it can assume (under the principle of recipient design) that conversationalists will be attentive to just such matters in interpreting others' utterances and in

designing their own. However, it is the kind of interest that is to be taken in such matters that is at issue and CA's is, as we have already indicated, in the audible character of conversational occurrences, with determining how they can be heard in the talk.

Let it be clear that CA is not aiming to provide methods which will enable us to say with greater definiteness than a conversationalist might just what is happening on any particular occasion of talk. CA's aim is not to uncover the facts about the particular social situations that it examines, to establish for sure (say) that these conversationalists were a conjugal pair, old college chums or employer and employee. It is not indifferent to these facts either, however, for it can legitimately, and within its elected frame of reference, concern itself with the organisational consequences that they do/could have, with their *possible* import for the structure of the conversational sequence. The investigations are, then, only properly understood if they are recognised to be into organisational possibilities, and the issues which arise from this pose questions about what *possible* differences such facts would make and *just how* the memberships and affiliations of participants matter for the course and character of talk. Thus, the issue is not (say) to be certain that this conversing couple are man and wife but to be clear about what difference it would make to the interpretation of what they are saying to each other *if they were* man and wife? Such a question is not to be answered by establishing that they *are* man and wife but by examining the talk again to see which and in what ways its features are linked to social membership. The analysis seeks to identify formal possibilities rather than to pin down instances.

The common supposition is, we suppose, that it is the organisation of relationships in the social setting and social relationships which determine the course of talk and, thus (should), govern how we hear (i.e. understand) the talk. CA makes a different supposition, namely that the character of the social situation and the nature of the social relationships between the participants are audible in the talk. Give someone a transcript and they can, very often, get quite a definite sense of who the parties to the talk are, in what capacities they are relating to one another, what kind of personal relationships they have and a great deal more beside. Thus, from listening to a tape one can soon tell, say, that it is a recording of a classroom activity, what kind of class it is, which person is the teacher, which pupils are 'teacher's favourite' etc. Similarly, listening to the opening of a telephone call in which the parties play 'identifying each other by sound of voice' one can see that these are familiars, that they know each other well enough to anticipate mutual recognition by voice alone and so forth. Thus, one can entirely legitimately and without need for resort to more or different kinds of data,

raise questions about the ways in which talk can embody and constitute social relations and not by making unjustified assumptions about facts to which one does not have access. One can do this by examining the way in which social relations are 'audibly present' in verbal exchanges, seeking to determine just what it is about a sequence of talk which makes it quite audibly (say) a conversation between old friends, a student ringing a teacher at home, a member of the public calling an organisation in search of help or a service? There has been, in sum, a shift from considering how social relationships determine the course of talk to asking what social relationships consist in, considered as exchanges of talk.

Conclusion

We have been looking at some of the problems involved in the relations of Conversation Analysis and discourse analysis and have been trying to suggest that these are often matters of misunderstanding rather than of direct disagreement. We have, or course, looked at this from the side of CA, suggesting that its critics in DA show, by their objections, that they do not really see what it is about. We do not thereby imply that those from CA who criticise DA are necessarily any more perceptive in their assessment of what that is about and what might be wrong with it. Whether they are or not would require another investigation. Nor do we intend to suggest that were the misunderstandings cleared up that the prospect of disagreement would evaporate with them. No, we have no doubt that, as carried on, CA and DA are quite incongruous and cannot be fitted together (not, at least, without considerable change in one or the other). In its crudest terms, we can say that one of the difficulties is this: that, by and large, DA is motivated to idealise out the very things that Conversation Analysis wants to examine.

To say this is not to make a criticism but to point to a rigidity. DA cannot readily adapt to take into account those things (appertaining to the interactional coproduction of an ordinarily orderly exchange of talk) which are the very stuff of CA's preoccupations. It cannot do so because its investigative method depends upon, consists in, abstracting out those things. For it to take notice of CA's problem, issues and phenomena would, thus, require an extensive re-orientation of its whole mode of analysis. It cannot just take the topics CA treats and add them to the list of things with which it deals. Neither, of course, can CA just encompass the stuff of DA's inquiries since the former's methodological ideals are just such as deny to CA the capacity to adopt the kinds of idealisation and abstractions which are

the stuff of the latter. For CA to take up those would be for it to deviate from its own programme of inquiry.

The relations between things viewed from the standpoints of DA and CA, we are suggesting, is rather like that between the two components of a gestalt switch. What one is looking at is, in one sense, the same thing but what is seen is very different and the transition between one picture and another is abrupt, discontinuous.

All too often one's remarks on matter such as these (i.e. on the inter-relation of disciplines or approaches) are listened to for counsels of hope or despair. Either one is saying that there is hope for much closer and more effective co-operation between different kinds of inquiry or one is saying that there can be no hope of this, that they cannot even talk to each other. Our discussion has been conducted under the attitude of 'living in the investigative present' which we identified earlier and thus directed toward saying what the situation presently is. What that might portend for the future is something we cannot foresee but toward which we would take neither an 'optimistic' nor a 'pessimistic' stance. *At present* there are severe difficulties of understanding between alternative strategies in the human studies (the kinds of difficulties between DA and CA being the sort also found in many other areas).

One of the key difficulties which creates misunderstanding is, we think, that too little concern is given to identifying the level at which problems arise. Too often the disputes are focussed upon specific issues when, as we have tried to show in this case, the divergence is at the level of frameworks.

Divergence between frameworks is very different from disagreement within frameworks. Consequently much criticism is quite ineffective because it is made as though between parties who share the same frameworks when it is precisely on those frameworks, on the whole shape of modes of analysis, that they differ. We have, thus, tried to show how DA's criticism of CA is made as though it were of an enterprise directed to the tasks and sharing the assumptions of DA when it is not this. Within a shared framework, it is the case that one cannot have two parties saying very different things, without raising the question, which one of us is right? In a divergence of frameworks, however, the situation is not at all the same. The relationship between frameworks is often best cast in terms of relative power. If mode of analysis A can adequately handle its own problem and phenomena and can also encompass the range of problems with which framework B is concerned, then framework B becomes redundant. Situations like this require the development of modes of analysis of relatively great power, but this is what we do not (at present) have in the human studies. Mode of

analysis A does not usually have the capacity to take up and solve the problems faced by mode of analysis B so in an attempt to claim superiority it is more apt to try to 'write off' the latter's problems as unimportant, irrelevant and so on, aiming to monopolise the field by restricting the range of problems that can be acknowledged to those with which it is competent to deal.

This, perhaps, explains why method assumes the peculiar, dictating position that it does in the human studies: One finds, that is, not the insistence that we ought to develop methods that will enable us to tackle problems but, much more often, the contention that we should restrict our problems to those our preferred methods can handle.

The fact is (at the present time) that the modes of analysis available in the human studies are of limited power relative to each other, such that attempts to claim the superiority of one over another (except on some very specific point) are, at best, premature: DA seeks comparative superiority over CA but does so without any clear conception of what it is that CA is trying to do and what it (CA) would regard itself as having achieved and, hence, without any conception of what it (DA) would have to do to match or surpass that. An understanding of that would require a much more thorough understanding of the whole mode of analysis within which CA operates.

There is (we think) much more heterogeneity than direct disagreement in the human studies for there are many very different *kinds* of problems that can be raised that will require very different kinds of solutions. In setting up strategies it is largely the case that one can only get one off the ground by restricting it, by excluding many questions and disregarding many difficulties. This is not a criticism of any one approach but a suggestion that, in the primitive and partial character of our efforts, we are all very much in the same boat. There are plenty of controversies, of course, but these are (we suggest) because, guided by a preoccupation with developing sociology, or linguistics, or psychology, we treat primitive attempts to get a grip on some problems as a candidate solution to the question of what general strategy the discipline (or even the human studies) as a whole should adopt. Of course, these heterogeneous strategies cannot all provide the right general approach, only one could do that. However, the evidence of experience is that *no one of them* provides anything like a general approach. This approach opens up some phenomena and allows one to get at certain problems and provides a reasonable solution to them, but (usually) *at the price* of having to close one's eyes to various difficulties and of having to entirely disregard a range of problems: that approach, by contrast, can deal with some of those neglected problems but only by setting aside the problems that the other gives primary attention to etc. etc.

We have been making our points largely in terms of the conflict of frameworks, but they apply also to assumptions about the complementarity of strategies. The possibility of interdisciplinary collaboration is not something to be raised and pursued just because strategies are concerned with what are (superficially) the same phenomena. These relations, too, need to be considered at the level of frameworks. It is for this reason that we have emphasised CA's *sociological* character so persistently, not because we hold that it is better to be sociological than linguistic or anything like that but simply to show that what is identifying and distinctive about CA, what gives sense and force to its moves and findings, depends upon recognising its primary disposition to treat issues as 'social organisational' ones. Putting CA together with more linguistically motivated forms of inquiry in a systematic way which would not just divest the former of its *raison d'etre* would not be a simple matter of taking more notice of the fact of turn taking in conversation, but of integrating what are, at this time, quite incongruous frames of reference: some radical revision of one, or both, would be required but that (which is where we came in) can only be done if one has a strong understanding of the respective modes of analysis involved, something which has — so far — rarely been in evidence.

Notes to Chapter 12

1. 'Discourse' is currently a very popular word and there are numerous quite different strategies which project themselves as some form of 'discourse analysis'. In this discussion we have in mind that kind of discourse analysis which draws its inspiration from linguistic modes of inquiry and which finds expression in books like Brown & Yule (1983), Coulthard (1977) and Labov & Fanshel (1977). These sources are themselves fairly heterogeneous in their views of what discourse analysis actually consists of but somewhat more uniform in their conviction that, whatever that is, it is an improvement over Conversation Analysis.
2. We refer to 'ethnomethodology'. The canonical, but difficult, account of these matters is given in Garfinkel (1967). Simpler introductions are provided in Sharrock & Anderson (1986) and Heritage (1984).
3. 'Interpretive sociology' is, broadly speaking, that kind which seeks to give 'the actor's point of view' a central part in schemes of sociological analysis, and which, therefore, requires a (more or less) significant feature of its operations the description of circumstances as seen by those who must act in them.
4. Pollner (no date) gives the best account of this but a more easily obtainable statement is Pollner (1979).