

## Integration von qualitativen und quantitativen Forschungsansätzen: Arbeitstagung, 13.-17.7.1981

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INTEGRATION VON QUALITATIVEN UND  
QUANTITATIVEN FORSCHUNGSANSÄTZEN

ARBEITSTAGUNG

13. - 17.7. 1981





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## EINLEITUNG

Die Lage der empirischen Sozialforschung ist durch die Herausbildung unterschiedlicher "Lager" gekennzeichnet, die mehr durch die Bemühung um eine Abgrenzung nach außen gekennzeichnet sind als den Versuch, Vorzüge und Nachteile einzelner Verfahrensweisen jeweils spezifisch auf eine konkrete Forschungsfrage hin zu evaluieren. Diese globale Feststellung gilt sowohl für die U.S.A. als auch die Bundesrepublik - auch wenn die Fronten im einzelnen anders abgesteckt sind. In den U.S.A. ist eine zunehmende Verengung des Begriffs "Methodologie" auf komplexe statistische Auswertungsverfahren (insbesondere Strukturgleichungsmodelle) zu konstatieren. Dies dokumentiert sich sowohl in den "Methodologie"-Sitzungen auf den amerikanischen Soziologentagen wie in den offiziellen Jahrbüchern "Sociological Methodology" der American Sociological Association. Diese statistischen Verfahren setzen naturgemäß standardisierte, quantitative Daten voraus, die vorzugsweise mit dem Instrument der Umfrage erhoben werden. Die einst dominierende "Chicago"-Schule der dreißiger Jahre mit ihren stärker auf Beobachtung und Feldinterview abhebenden Techniken ist in eine Randposition gedrängt, ebenso wie andere "qualitative" Ansätze, wie etwa die Ethnomethodologie. Daß mit William F. Whyte ("Street Corner Society") und Erving Goffman nun nacheinander zwei prominente Vertreter der "anderen Richtung" zu Präsidenten der ASA gewählt wurden, zeugt vielleicht von einem gewissen Unbehagen an der dominierenden quantitativ-statistischen Richtung, ändert aber nichts an der prinzipiellen Rollen- und Machtverteilung.

In der Bundesrepublik ist das Unbehagen und die teilweise durchaus berechtigte Kritik an der Umfrageforschung - als Prototyp der "mainstream"-Sozialforschung - schon früh auf fruchtbaren Boden gefallen; genährt von einer traditionell eher sozialphilosophischen Orientierung ("Frankfurter Schule") und den im Zuge der Studentenbewegung formulierten politischen Ansprüchen. Wo die empirische Sozialforschung nicht gänzlich abgeschrieben wurde, war zumindest das

Bemühen dominant, sich von einem sogenannten "normativen Paradigma" abzugrenzen und sich einem "interpretativen Paradigma" zu subsumieren. Diese von Thomas P. Wilson in der American Sociological Review im Jahre 1970 vorgeschlagene begriffliche Trennung wurde - und wird - vielfältig gleichgesetzt mit der Verwendung bzw. Nichtverwendung bestimmter Techniken und Verfahrensweisen (etwa quantifizierende Repräsentativumfragen vs. Tiefeninterviews in Einzelfallstudien). Wilson ging es jedoch in dieser Unterscheidung nicht um eine solche Dichotomie von Techniken als vielmehr um das prinzipielle Verhältnis von Forscher und Daten. Einen ähnlichen Gesichtspunkt hat Jürgen Kriz in seinem vor kurzem erschienenen Buch über "Methodenkritik empirischer Sozialforschung" - bezogen auf quantitativ orientierte Untersuchungen - ausführlich dargelegt.

Mangels einer Tradition von Feldforschungen ethnographischen/kultur-anthropologischen Typs in Deutschland ist ein großer Teil "alternativer" Sozialforschung in der Bundesrepublik durch eine bloße Ausweitung der Datenerhebung (nicht-standardisierte Interviews, Gruppendiskussionen etc.) gekennzeichnet, ohne eine ausreichend klar formulierte und vor allem durchführbare Gesamtkonzeption, in der auch die Auswertungsprobleme gelöst werden. Es gibt sicher interessante Ansätze, wie etwa die Methode des narrativen Interviews (Fritz Schütze) oder die "objektive Hermeneutik" (Ulrich Oevermann), die jedoch vor allem unter pragmatischen Durchführungsaspekten noch weiterer Ausarbeitung bedürfen und nicht repräsentativ für die "alternativ" verfahrenen Forschungen sind.

Als Motiv der Abkehr von traditionellen Forschungstechniken kann vielfach der Wunsch nach einer "lebensweltlichen Anreicherung soziologischer Forschung" gesehen werden (vgl. hierzu auch jüngst Martin Kohlis Bestandsaufnahme des "biographischen Ansatzes" in der Zeitschrift für Soziologie). Dieses Anliegen ist ernstzunehmen, unabhängig vom politischen Standort und gesellschaftlichen Weltbild des Forschers. Das Unbehagen, die Unzufriedenheit auch vieler potentieller Abnehmer soziologischer Forschung nährt sich nicht zuletzt aus

der Tatsache, daß die Sozialforschung zu oft nur ex post-Erklärungen für gesellschaftliche Prozesse (etwa die Studentenbewegung) zu liefern in der Lage war, nicht aber Veränderungen im status nascenti aufzuspüren vermag. Oder anders formuliert: Die Sozialforschung hat ihr Ohr zu oft nicht am Puls des Volkes.

Auf diesem Hintergrund ist der ZUMA-Workshop mit einem zweifachen Ziel konzipiert worden:

- auf der Ebene allgemeiner Methodologie sollten Kriterien der wissenschaftlichen Güte spezifisch für bestimmte Verfahrenswesen definiert und diskutiert, der Aspekt der gegenseitigen Ergänzung von "quantitativen" und "qualitativen" Techniken herausgearbeitet werden;
- auf der Ebene praktischer Methoden der Ansatz (ethnographischer) Feldforschung amerikanischer Tradition und dafür besonders nützlicher Einzeltechniken dargestellt werden; schwerpunktmäßig anhand von konkreten Beispielen aus der Forschungspraxis.

Die Auswahl der Hauptreferenten orientierte sich an dieser Zielsetzung. Mit Thomas P. Wilson (University of California, Santa Barbara) konnte der Vater des Begriffs "interpretatives Paradigma", der in der Bundesrepublik durch die deutschsprachige Fassung seines oben erwähnten Aufsatzes im Reader der "Arbeitsgruppe Bielefelder Soziologen" weite Verbreitung gefunden hat, gewonnen werden. Ausgewiesen durch eine Reihe quantitativ-statistischer Arbeiten hat Wilson in den letzten Jahren verstärkt über die wissenschaftstheoretische Begründung "interpretativ" orientierter Sozialforschung und insbesondere das Verhältnis von Soziologie zur Ethnomethodologie gearbeitet. Seine Zielvorstellungen hat er in der Formel einer "ethnomethodologisch informierten Sozialwissenschaft" zusammengefaßt.

Don H. Zimmerman (ebenfalls von der University of California, Santa Barbara) ist zwar von seiner Forschungsorientierung her als Ethnomethodologe im engeren Sinne zu klassifizieren, hat aber die Strategie der Abkapselung - die viele Vertreter dieser Richtung auszeichnet - nie geteilt und in Aufsätzen wie konkreten Forschungsprojekten eine fruchtbare Verbindung zur traditionellen Sozialforschung hergestellt.

Ein Großteil des Programms der Arbeitstagung wurde von den beiden Referenten aus den U.S.A., Thomas P. Wilson und Don H. Zimmerman, bestritten, die zur Vor- und Nachbereitung des Workshops einige Wochen als Gäste bei ZUMA weilten. Kurze Beschreibungen gegenwärtiger Arbeitsschwerpunkte und -interessen, die jeder Teilnehmer vorab zur Verfügung stellte, dienten sowohl zur Kommunikationsförderung unter den deutschen Teilnehmern als auch dazu, es den Hauptreferenten zu ermöglichen, die Präsentation ihrer Arbeiten auf den Hintergrund der deutschen Gesprächspartner abzustellen.

Neben den beiden den thematischen Bereich absteckenden Positionspapieren "On Qualitative 'versus' Quantitative Methods in Social Research" und "Field Work as a Qualitative Method" fand insbesondere eine ausführliche Präsentation von Don H. Zimmerman zum Thema "Analysis of Talk in Field Settings" großes Interesse. Grundlage dieses Vortrags sind jüngste Arbeiten Zimmermans im Rahmen eines größeren - schwerpunktmäßig relativ konventionell verfahrenen - Forschungsprojekts über "Gewalt in der Familie"; ein Thema, das - nicht zuletzt gefördert durch die Frauenbewegung - zumindest in den U.S.A. breiteres öffentliches Interesse gefunden hat. Zimmerman widmet sich hier dem Teilproblem, wie die Polizei - als Instanz sozialer Kontrolle - entsprechende Telefonanrufe (Notrufe) behandelt und damit "Gewalt in der Familie" als öffentliches Problem mitkonstituiert oder auch zu konstituieren verhindert. Die Analyse derartiger, thematisch spezieller Telefonkonversationen erfordert als Vergleichsbasis die Analyse von Anrufen bei der Polizei generell, und noch einen Schritt weiter gedacht die Beschäftigung mit

Kommunikation via Telefon allgemein. Dies verdeutlicht die Brücke, die sich zwischen vermeintlich nur esoterisch-versponnener Konversationsanalyse ethnomethodologischer Prägung und der Untersuchung handfester sozialer Probleme spannt.

Die Präsentationen der Hauptreferenten wurden ergänzt durch die Vorstellung verschiedener Projekte der deutschen Teilnehmer. Breiteren Raum nahm hier vor allem ein von Elmar Weingarten (FU Berlin) geleitetes Projekt über Intensivstationen im Krankenhaus ein, das von seinem Forschungsdesign her (ausgedehnte Feldbeobachtungen und -interviews durch das Forschungsteam selbst) der von Wilson und Zimmerman vertretenen Konzeption sehr nahe kommt und von daher besonders gute Möglichkeiten zu vergleichender Diskussion bot. Weitere Projekte stellten u. a. Manfred Auwärter/Edit Kirsch (Starnberg), Ulrike Martiny (Hamburg), Werner Nothdurft (Institut für Deutsche Sprache), Charles Kaplan (Universität Frankfurt und Institute for the Study of Human Issues, Philadelphia) sowie Friedrich Schrecker (Wiesbaden) vor.

Einen größeren Platz nahm - als immer wiederkehrendes Thema - die Diskussion von angemessenen Kriterien wissenschaftlicher Güte ein. Die traditionellen Kriterien wie Repräsentativität, Validität und Reliabilität sind für Forschungen, die stärker auf die Feinanalyse von Prozessen ausgerichtet sind als auf die notwendig gröbere Bestimmung von Gesamtverteilungen, nicht oder zumindest nur modifiziert verwendbar. Zu dieser Problematik hat Wilson in seinem Positionspapier ausführlicher Stellung genommen und alternative Vorschläge unterbreitet.

In diesem ZUMA-Arbeitsbericht sind die beiden oben erwähnten Positionspapiere abgedruckt, die damit allgemein zugänglich gemacht werden. Während das Papier von Don H. Zimmerman gegenüber der auf der Arbeitstagung verteilten Fassung nur geringfügig modifiziert wurde, ist die nun abgedruckte Version des Wilsonschen Papiers eine geschlossene Darstellung der Argumentationen, die Wilson in verschiedenen Sitzungen der Arbeitstagung vorgetragen hat.

Die recht ausführlichen Literaturangaben in beiden Papieren ermöglichen eine gezielte Auseinandersetzung mit den hier vorgetragenen Sichtweisen auch für Interessenten, die an der Arbeitstagung nicht teilnehmen konnten.

## FIELDWORK AS A QUALITATIVE METHOD

Qualitative social research, and in particular, participant observation, is an established tradition of research in American social science, the ascendancy of quantitative analysis as the "scientific" mode of inquiry within the profession notwithstanding. The tradition has produced its classics, major and minor, and graduate students are usually held responsible for having read them. Survey courses of research methods mention field techniques, at least in passing, and the "methodology" of fieldwork is subject to periodic attempts at codification, emerging as textbooks or manuals of qualitative research.

Despite the fact that qualitative research is in some sense an "established" mode of inquiry, and even though it seems possible to recognize good exemplars of the tradition, there remains an air of informed vagueness concerning just what techniques or strategies to recommend to those colleagues brave enough to venture into some setting, notebook or taperecorder at the ready. Practical lore exists, to be sure, and the literature can be made to yield both the detailed memoirs of past fieldwork and attempts to develop recipes for research in this mode. Many of these accounts are useful in that they sensitize the would-be fieldworker to a range of practical issues--access to the setting, rapport with informants, note-taking, etc.--but the features of social settings differ sufficiently such that the actual management of these problems awaits experience in the setting itself. Thus, it is appropriate to critically examine the aims and practices of qualitative research, particularly those lines of inquiry addressed to what Wilson (1981) has termed "situated action," that is,

...actions in particular concrete settings as these actions are produced and are available to the participants for their own recognition, accounting, and use as bases for further inference and action within those settings.

In this paper, I consider how such a conception of the nature of the subject matter bears upon the practice of qualitative research and how, following upon this, canons of "good work" might be developed so that this research tradition could be subject to more explicit critical appraisal by its practitioners. I proceed by first critically examining Herbert Blumer's<sup>1</sup> "naturalistic inquiry" which furnishes a conception of the subject matter of sociological study and proposes what is a fundamentally qualitative approach to it. After considering a number of criticisms of Blumer's "naturalistic inquiry," I introduce a brief characterization of an ethnomethodological perspective on the subject matter of inquiry and use this to develop one view of the issues of qualitative research and the criteria by which it may be evaluated.

#### The Symbolic Interactionist Framework of Blumer's "Naturalistic Inquiry"

Blumer proposes three simple but central premises upon which symbolic interactionism rests: (1) persons respond to objects and events in terms of their meaning; (2) meaning emerges from social interaction; and (3) meaning is managed and transformed through an interpretative process (1969:2). The "interpretative process" presumably figures in the construction of individual or collective social acts (cf. Blumer, 1969:50ff.). The methodological implications of these premises are fairly straightforward.

First, given the notion that people act on the basis of the meaning objects in their environment have for them, it follows that sociologists are constrained to understand these objects as members understand them. Thus, "failure to see their objects as they see them, or a substitution of (the sociologist's) meanings of the objects for their meanings, is the gravest kind of error that social scientists can commit" (Blumer, 1969:51). To achieve

this understanding requires the researcher to acquire familiarity with the world of the member. This means, among other things, placing "oneself in the position of the individual or collectivity" in question, and to assemble a body of relevant observations...in the form of descriptive accounts from the actors of how they see the objects, how they have acted towards the objects in a variety of different situations, and how they refer to their objects in their conversations with members of their own groups" (Blumer, 1969:51). The suggestion is quite plain: our subjects can tell us what various objects mean to them--reliance on informants is a common enough research strategy --and we can overhear conversations between members in which the various objects that populate their symbolic environments are discussed in terms that we can record and report.<sup>2</sup> As field-workers, moreover, we can observe and report both our subjects' and our own behavior in a setting, as well as our introspected internal states (cf. Denzin, 1971:168,180), deriving from these records additional materials for fashioning our account of the meaning of objects and events from the point of view of the member.

#### "Naturalistic Methodology": Some Objections

Blumer's "naturalistic methodology" has not escaped criticism. Recently, McPhail and Rexroat (1979) have chastized Blumer for what they argue to be fundamental departures of Blumer's symbolic interactionism from Mead's social psychology. The central issue of their critique is not the suspect credentials for Blumer's claim to the Meadian tradition

but the poor showing of his "naturalistic methodology" in comparison to Mead's scientific rectitude. They locate Blumer's difficulties in his naturalistic ontology, that is, his assumption that the social world in all its varieties is already ordered by the people who make their home within it. In contrast to Mead's position that "science is not concerned with the nature of reality; rather science is concerned with 'ordering the events it observes' (Mead, 1936:275)" (McPhail and Rexroat, 1979:457), Blumer calls upon the investigator to search

out the order already present, and to hold concept and theory formation accountable to that order (Blumer, 1969:23-24). In discussing the ontological basis of Mead's conception of scientific procedure, McPhail and Rexroat propose that:

Mead's (1936:275) ontological perspective dismisses any concern with the 'nature' of observed events. Rather, the investigator is required to provide directions which will occasion the convergent responses which order observed events. Blumer's ontological perspective, if anything, requires the opposite. He directs investigators (1969:39,45) not to assign but to discover the nature of their concepts. They are directed not to impose relationships upon their analytic components but to discover them through intense scrutiny of the empirical world...The consistent respect in which Blumer holds 'the nature of the empirical world' and his lack of attention to investigators' convergent responses and the interactions and the interactions which produce them, give us the ontological assumptions upon which his naturalistic methodology is constructed (1979:458-459).

They go on to suggest that Blumer's conception of an indigenous order to social life poses a paradox within the framework of symbolic interactionism. If, they argue, "human actors impose meaning and order upon their own and others' activities," how can Blumer consistently apply this premise to the social world under investigation and simultaneously deny that the activity of research--a human activity presumably a part of the same social reality--imposes meaning and order on its subject matter? (McPhail and Rexroat, 1979:459). Their own view is expressed by their formulation of the notion of "domains of meaning," by which they indicate that social life as understood by participants can differ from the understanding of the same arena of activities achieved by researchers. "Neither domain," they write, "is any more natural or less artificial than the other. One or the other, or both, may be more useful depending on the purposes at hand" (McPhail and Rexroat, 1979:462-463).

Clearly McPhail and Rexroat mean to imply that the investigator should exercise complete analytic authority over the field of data, that is, impose a conceptual framework constrained only by its utility in achieving "convergent responses" from other professionals. This option is often elected by

sociologists. If, however, as they concede, human actors construct a "domain of meaning" of their own, then they must develop "convergent responses which...establish shared meaning, common understanding, and socially constructed lawfulness..." (McPhail and Rexroat, 1979:463). This surely implies that it is possible to talk about a "natural" order to be discovered in social activities, and it would be odd, to say the least, if such an orderliness was not available to systematic study.

McPhail and Rexroat, then, have succeeded only in raising serious question concerning Blumer's claim to be following in Mead's footsteps--an uninteresting issue from my point of view --rather than delivering a death blow to Blumer's "naturalistic methodology." Their quarrel with Blumer's insistence on respecting the point of view of the actor may, if we may speculate, be rooted in the suspicion that this brand of "naturalism" leads to mirroring social reality rather than to its analysis.

This concern is echoed, although in different form, by Huber (1973:280) who notes with reservation Blumer's insistence on "faithful reportorial depiction and analytic probing (and his demand that the investigator remain) in close and continuous relation with the natural social world." Assuming that in a given study the sociologist has faithfully reported what informants have divulged about their social world, one can wonder, as does Huber (1973:280) in a slightly different context, how "to distinguish the findings of sociologists from the findings of anyone else" and be concerned with how the practitioners of this approach could avoid having their reports...confused with mere journalism" (Huber, 1973:282). Leaving aside the relatively conventional objections to Blumer's (1969:28-31) wholesale rejection of accepted research procedure, e.g., hypothesis testing, operational definition, etc., the concerns just examined do require attention. If the "natural social world" is to be faithfully reported, to what sociological purpose are such reports to be put beyond the painstaking description of social reality? A satisfactory answer to that question will also provide a means for distinguishing a "naturalistic" study from journalism or an aimless empiricism. To approach an answer to this question, a number of issues must be raised and briefly discussed.

Issues in Seeking the "Point of View  
of the Member"

First, the notion of placing "oneself in the position of the individual or collectivity" (Blumer, 1969:51) requires elucidation, particularly with respect to its implication for research strategy. This I shall attempt when I discuss a field procedure called "tracking" (Zimmerman and Wieder, in press).

Second, short of a thoroughgoing behaviorism (cf. Wilson and Zimmerman, 1979/80), grasping "the point of view of the member" and thus obtaining an intimate view of a particular social world, requires sharing (or coming to share) a common language and, beyond that, being instructed in that language on the matter of how to respond to and talk about the various activities or events that make up that world in a situationally sensitive, and hence appropriate, way. These resources will be employed by the observer to fashion a description of what was seen or heard in the setting. Descriptions furnished by informants may be called "natural language accounts" of social activities.<sup>3</sup> What is problematic is that such accounts are indigenous to the setting they depict, and reflexive upon it (cf. Garfinkel, 1967; Wilson and Zimmerman, 1979/80:57-60).<sup>4</sup> The upshot of this is that the embeddedness of talk in and of the setting of its occurrence may mean that the matters thus described from the point of view of the member may not be fully understood by the researcher, or worse, may be misunderstood.

Thus, in Zimmerman and Wieder's (1978) study of marijuana smoking (which will be considered in more detail later), seemingly contradictory informants' accounts of marijuana use turned out, under further scrutiny, to be consistent and warranted depictions of socially organized smoking practices in that youth sub-culture. Capturing the "point of view of the member" is not simply a matter of listening to what they say or asking them to explicitly state the meaning of things. It involves locating members' accounts within the organized ways of a setting, a reflexive process itself since the account furnishes the initial instruction for the search while the search subsequently informs the sense of the initial instruction. Moreover, pursuit of the

member's point of view is not merely chasing after disembodied meanings but rather the tracking down of collectively accomplished patterns of social organization, a point which leads directly to a third issue.

Concern with the member's viewpoint is often understood to be a "subjective" approach. The term "member," however, we construe in the ethnomethodological sense, that is, as referring to membership in a collectivity with its attendant rights and obligations (Garfinkel, 1967) as well as the possession of competency in society's ways, including the mastery of natural language practices (Garfinkel and Sacks, 1970). This does not deny the individual's subjectivity, i.e., the experiences of the inner and outer world gained from the vantage point of a singular life, but emphasizes intersubjectivity, the activities and accounts produced by the member for members. Clearly, the field worker interacts with individuals, and questions them concerning their thoughts, feelings and opinions. Nevertheless, one of the foremost uses of the ethnographic interview is to elicit socially sanctioned knowledge from an informant, i.e., descriptions, explanations and evaluations that would be recognizable to and socially supported by other competent members of the setting as correct and/or appropriate, and thus sanctionable. Private opinions or deviant viewpoints may be of interest, but concern for understanding the structure and social process of a setting must, in the first instance, lead to a focus on accounts which could meet the test of collective, i.e. intersubjective, validation.<sup>5</sup> The point I wish to stress is that it is necessary to assess the status of an informant's remarks lest field work become merely a passive reflection of members' undisciplined musings about their own affairs. Respect for the "point of view of the member" should not entail the transfer of analytic authority to the subjects of our research.<sup>6</sup>

Fourth, and finally, what guidelines might be established to discipline the organization and analysis of ethnographic data collected while respecting the constraints of "naturalistic inquiry"? That is, is it possible to specify criteria in terms of which data of this sort may be sifted, arranged, and made to tell a sociologically interesting story without forcing an arbitrary

conceptual framework upon it? We shall consider responses to each of these issues in turn.

### The Problem of Positioning<sup>8</sup>

To speak of placing oneself in the "position of the individual or collectivity" would seem to require a translation of what is, in the first place, a spatial metaphor, into a practical plan for marshalling opportunities to observe and question research subjects. To begin with, of course, a basis for the investigator's presence in the setting must be established, i.e., some acceptable rationale for the intrusion of the observer into the social space of the target group must be devised. For example, Whyte, in his study *Street Corner Society* (1943/1921), solved this problem through establishing a relationship with Doc, a leader of a group of "Corner Boys." There are other solutions to the entrance problem, depending on the nature of the setting in question and the strategy of observation adopted.<sup>9</sup> While the problem of entry can sometimes perplex the would-be observer, a more serious problem looms when entry has been successfully effected.

This problem may be termed the "positioning" problem, the "getting into the thick of it." Even relatively simple social settings exhibit a diversity of behavior patterns distributed in time and space and across different categories of persons. Solutions to the problem of positioning the observer in such a way as to accommodate this diversity will inevitably have consequences for the nature and scope of information that can be gained, given that the observer cannot be everywhere at once nor can he expect to achieve the same degree of rapport with all potential subjects. The ideal, never reached in practice, is that of the omniscient, omnipresent observer, free to travel in space and time, invisible to those observed, and possessed of the ability to read minds. Short of such talents, the ethnographer must devise means to get to where he needs to be, see and hear what he can, develop trust between himself and his subjects, and ask a lot of questions.

One way of accommodating this diversity of behavior in settings may be formulated as a general ethnographic strategy we call

tracking. Prior to discussing tracking in any detail, we need to take a closer look at the problem of the diversity of behavior in settings with which tracking, as a method of "positioning" the observer, can deal. Some of the most fundamental dimensions of this diversity may be illustrated by considering the problem of observing an American university.

- (1) Roles and other person-related differences. The observer must come to terms with the fact that different people and different types of people do different activities and that they also behave, feel, and think in typically different ways. These differences are often organized into different roles or social categories, e.g., age, sex, race, etc. The observer generally decides to focus on some specified roles rather than observing everyone he happens to confront on a "catch-as-catch-can" basis. In observing the university, the observer would be likely to select some roles from the wide range of roles (from deans to students to gardeners) which are potentially available to him.
- (2) Diversity within roles. The extent to which the ethnographer must concern himself with differences in behavior, feeling, and thought among incumbents of the same role depends heavily on exactly how he circumscribes his subject matter. For example, if he defines his problem in terms of the role of the student in the university organization as such, the fact that students differ in many ways may not be relevant to his study. It may be sufficient for the ethnographer to set out to observe all the different ways that students can act on campus as students (what the range of possible behavior is while enacting the student role) and what they must do as students (what they must minimally do if they are to maintain their status as students as compared with what they could do to get themselves dismissed from the university). Other definitions of the topic of interest, however, will require that the ethnographer be more deliberate in his sampling of the persons whose behavior and talk he observes, perhaps even requiring a representative sample. An interest in the variety of student life styles, for example, is likely to require such a sample.
- (3) Temporal diversity. The ethnographer must also concern himself with the fact that behavior, feeling, and thought vary temporally in terms of (1) daily, weekly, seasonal, etc., cycles, (2) in terms of phases of activity, and (3) more or less idiosyncratically. The university, for example, presents a very different scene for observation at twelve midnight as compared to twelve noon. The first day of classes looks different from classes

in midterm. The behavior of one student who engaged in the practice of "cramming" might involve his not studying during most of the semester. Observation might also show that when he did study prior to examinations, the way in which he behaved changed progressively as the hour of the examination approached. That is, one could observe that his pre-examination behavior had definite phases to it. Furthermore, the temporal arrangement of some of his behavior might appear idiosyncratic. The same student might appear outgoing and happy during some weeks of the semester and withdrawn and depressed during other weeks, without these mood shifts being associated with other temporally organized features of his existence. Nevertheless, his behavior, especially vis-a-vis other students, might be markedly different during these periods. If the ethnographer observed only one of these periods, his observations would be skewed. Somehow, the ethnographer needs to take into account the cyclical character of the observational scene, the differences between various phases of activities that may go on within that scene, and the possibility of idiosyncratic temporal differences.

- (4) Spatial or locational diversity. Persons may behave, feel, and think in typically different ways, depending on the setting or sub-setting in which they locate or find themselves. On the same university campus, one may find a more-or-less standardized way that students act and even feel and think within classrooms which contrasts with the typical ways that they behave, feel, and think in the gymnasium, at a rock concert, in the student union, in the library, and in professors' offices. The ethnographer must position himself relative to these various sub-settings and know the relationships between them if he is to make sense of what he observes.
- (5) Interactional diversity. How persons behave, feel, and think may systematically vary depending on whom they are interacting with and who is present. For example, students may talk in one fashion when they are in a discussion group that is led by a faculty member and in another fashion when they are together in the student union without faculty present. The ethnographer must either position himself in such a way as to permit him to observe these interactional differences or he must, at least, be aware that there are systematic forms of behavior that he is unable to observe.

In keeping with the tenets of "naturalistic observation," the investigator must of course be concerned to glean from such observations the meaning of events and activities to participants. Obviously one resource for this is available in talk people do. The observer may find that the scenes of interest are organized in such a way that the participants are constantly

commenting on the meaning of their activities. For example, in some universities, one may find that "what classroom activities are all about" from the point of view of students is the principal topic of discussion between students in the student union. An observer who sits with students in the student union might hear all that he needs to know. On other campuses, however, students' conversations in the student union might be largely devoted to talk of weekend and nighttime activities as well as the playing of bridge. An ethnographer of such a campus might have to form relationships with students which permitted him to interrogate them about the meaning of classroom activities. We should also note that the ethnographer who found students freely talking about the classroom might also decide that additional and private interrogation was also necessary for him, since the way students talk in public about lectures, faculty, and the like might be a partial view of what these things mean to them.<sup>10</sup>

The ethnographer, then, is faced with the task of (1) encountering and describing the repetitive events and activities that make up the routines of the group he is studying. He will attempt to position (and systematically reposition) himself in such a way as to take into account role and other person-related differences, temporal and spatial differences, and interactional differences. He also has the task of (2) positioning himself socially so as to obtain information about the meaning of these activities from the point of view of those who engage in them. That is, he must either be in a position to overhear and be a participant in naturally occurring conversations in which the meaning of routine events is discussed by the participants and/or form relationships with the participants which permit him to freely interrogate them.

I suggest, then, that to pursue an adequate investigation of social conduct in situ requires making observations on behaviors presumed to be distributed across roles, varying within roles across incumbents as well as through time and by location, and varying in response to the interactional combinations of roles and/or persons in the given setting. In effect, these dimensions represent five basic sources of variation in conduct,

at least in the sense that the observer should look to determine whether or not some activity is uniform or variable over such features.

It should be clear that in offering these dimensions of diversity we obviously assume that a given setting exhibits some degree of social organization, and that members' words and deeds, and the meanings infused in them, stand in some orderly relationship to that organization, e.g., accomplishing or reproducing its features. Moreover, attention to such dimensions of variation requires information about the setting which the investigation itself is expected to generate, creating a "bootstrap" situation. This situation provides one insight into the difficulty of fully rationalizing a research design prior to acquiring actual experience in the field setting.

### Tracking

The features of social life outlined above pose a challenge to the ethnographer who proposes to describe some particular social scene. We view tracking as one response to this challenge. We did not, of course, invent this strategy, for many of the features of tracking outlined below are to be found in the work of a number of participant observers (e.g., Bittner, 1967a,b; Black and Reiss, 1970; Piliavin and Briar, 1964; Skolnick, 1967; Sudnow, 1967; and Zimmerman, 1969).

Tracking, first of all, provides a way for the ethnographer to overcome the sometimes frantic urge to be everywhere at once, while at the same time allowing him to approach this goal. This is done by systematically following the routine of a succession of different role-incumbents over some time period. By periodically switching from one subject to another, the observer is eventually able to encounter many of the patterned activities within the setting from a number of different role-bound perspectives.

In tracking, the ethnographer stays as close to his subject as he can manage, attempting to see what the subject sees as well as noting what he does and what he says. Skolnick (1967:33) provides an example of tracking police:

I spent eight nights with...patrolmen, mostly on weekends, on the shift running from 7 pm to 3 am. All of this time was spent interviewing and observing, talking about the life of policemen and the work of the policeman. I understood my job was to gain some insight and understanding of the way the policeman views the world. I found that the most informative method was not to ask predetermined questions, but rather to question actions the policeman has just taken or failed to take, about events or objects just encountered, such as certain categories of people or places of the city.

Skolnick then shifted his attention to the vice control squad. While he does not describe much of the actual sequences of his observations there, what he does describe gives us an impression of the observer's closeness to his subjects in "tracking" work (1967:35-36):

Under direct observation, detectives were cooperative. They soon gave permission to listen in to telephone calls, allowed me to join in conversations with informants, and to observe interrogations. In addition, they called me at home when an important development in a case was anticipated. Whenever they went out on a raid, I was a detective as far as any outsider could see...I looked enough like a policeman when among a group of detectives in a raid for suspects to take me for a detective...Even though I posed as a detective, however, I never carried a gun...As a matter of achieving rapport with the police, I felt that such participation was required. Since I was not interested in getting standard answers to standard questions, I needed to be on the scene to observe their behavior and attitudes expressed on actual assignments.

Tracking provides a systematic solution to the problem of positioning the observer vis-a-vis the diversity of behavior in settings.<sup>11</sup> As is evident in the examples above, tracking in the hands of Skolnick and others provides a way of positioning the ethnographer so that he may observe not only what people do and thus be able to infer or develop evidence concerning what these matters mean to the participants. Furthermore, by systematically tracking the occupants of one role (e.g., a sergeant in the police department) and then tracking another (e.g., a lieutenant in the same division), etc., the ethnographer is able to incorporate into his research plan and description the

the fact that persons in different roles act differently. If his definition of his research problem demands it, the ethnographer may, if he chooses, track a number (even a representative sample) of incumbents of the same role. By tracking the same individual over a sufficient period of time, he is able to take temporal variation into account. Because he follows one individual over the full course of that individual's day, he is also able to observe the differences in the individual's behavior which are related to (or caused by) the various settings in which he acts and the various persons with whom he interacts.

Tracking, of course, is a strategy based on the overt observer role. It presupposes the ability of the investigator to move across social boundaries in a given setting with some degree of freedom and the ability to ask questions and engage in conversation with a wide range of participants. Unless the social setting and its activities are tightly encapsulated and delimited in time and space, such as the "tea room" setting in which anonymous, impersonal sexual activity is performed largely in silence (cf. Humphreys, 1970), occupying a pre-existing role in some setting constrains the covert observer to the range of behaviors appropriate to that role, and what Merton (1957:368-70) calls role set.

The discussion of tracking above obviously does not provide a precise algorithm for research design. It is, however, intended to provide guidelines for developing a systematic plan of observation given a reasonably clear picture of a setting's organization. This picture may not begin to emerge until after entry into the field, or it may be partially revealed by preliminary inquiry, e.g., interviews with knowledgeable people, analysis of documents, etc.<sup>12</sup>

Use of the notion of tracking, in that it encourages a systematic attempt to encounter a range of persons and situations, provides a means to discover the array of local identities in the setting, and to become informed of the relevancies that are tied to particular scenes. Insofar as such relevancies and identities are embedded in routine activity, they may be difficult to elicit from disembodied interviewing. The

ethnographic interview--asking questions on the actual occasion of some activity or event the meaning or purpose of which is unclear to the investigator--is an essential element of the optimal use of tracking as a field procedure.

To conclude this section on the issue of "positioning," we should point out that this view of observational strategy is constrained by a conception of social life as methodically accomplished by members. Among the features of this accomplishment are the "social fact" properties of everyday life: the repetitive, routine, standardized, transpersonal and trans-situational character of patterns of social activity from the point of view of the member. Thus, it may diverge from the symbolic interactionist conception of social reality, such as that advocated by Blumer, from which premises this discussion began. The central premises of symbolic interactionism affirm again and again the human capacity to delicately shade meaning, to take into account many varied aspects of objects, fashioned from even the most routine and standardized social occasion. This celebration of these human skills is the platform from which critics, such as Gonos (1977:856ff.), attack. He is especially critical of the symbolic interactionist emphasis on situations. He writes:

Symbolic interactionists have considered it their theoretical calling to describe the rich texture of everyday social life, a quality highly valued by them and, allegedly, totally ignored in the functionalist portrayal of social reality, against which they define their movement...To remain true to human experience, they have insisted on study at close range, as participants, and, from this vantage point, the situations that make up everyday life have been seen as idiosyncratic...The reward for research activity would seem to be an endless wonderment (1977:856).

However, if we are to speak meaningfully of a member-of-society's "construction" of a situation, or more loosely of the "construction of the social world", it must be kept firmly in mind that if indeed the situation is "constructed," it is constructed methodically, using common cultural resources which permit not only the production but the recognition of such

construction. Detailed understanding of members' methods for the production and recognition of social objects, events, and activities, while an important goal in its own right, also serves to impose a discipline of analysts of social activity who would speak on behalf of the point of view of the member. Only by knowing how members construct their activities can one be reasonably certain just what those activities are. An approach to the investigation of how members "construct" their activities will be discussed later in connection with an examination of the analysis of talk as an ethnographic procedure.

### Grasping "The Point of View of the Member"

Earlier, it was suggested that grasping "the point of view of the member" might pose problems, even when "explained" by the member to the researcher. An example from an ethnographic study of marijuana smoking practices was used to illustrate this point. It will be useful here to examine this example in more detail.<sup>13</sup>

#### An example: marijuana smoking

A useful illustration of the points we have been making is provided by the study by Zimmerman and Wieder (1978) in a student community. Because of space limitations, we cannot discuss this study in detail here but must assume that the reader is familiar with the paper itself.

Zimmerman and Wieder conclude their analysis with the following general remark.

It should be clear by now that we proceeded by employing our informants' remarks--in the context of what we already understood about the community we were studying--as guides to further inquiry, instructions, as it were, for assembling disparate observations into a coherent pattern of activity. In turn, the emergence of pattern further deepened our grasp of how our informants' commentaries articulated with social arrangements in the setting. Thus what we heard from the people we studied was both resource and topic and yielded a picture of social organization sustained by our independent investigation but consonant with our subjects' accounts of its consequences. Our informants were not simply surrogate ethnographers furnishing disinterested descriptions of their activities, but neither were they dupes fooled by

collective prejudices into thinking their activities were of one sort when they "really" were of quite another. The problem was to discover in what way to regard what was said.

In the final analysis, the talk that goes on in and about social settings cannot be regarded as distinct from the setting, as something that can be detached and examined at leisure apart from the lively context of its occurrence. The contours of socially organized settings (and the internal structure of the organization itself) often require an instructed gaze provided by indigenous accounts; but it is the working of that social organization which invests the accounts with specific sense.

Thus, "grasping the point of view of the member" poses a challenging analytic task that goes beyond mere reportage. It requires careful observation, the integration of varied sources of information, an appreciation of the intimate tie between setting and account, and a willingness to exercise a "sociological imagination," that is, to reserve to the investigator the analytic authority to transcend the viewpoint of the subject while nevertheless employing it as the basic datum.

#### The Issue of "Subjectivism"

The preceding section has in passing addressed the issue of subjectivism and the related concern that naturalistic inquiry (or observational research more generally) is prone to stick too closely to informants' or subjects' reports and employ them in an uncritical fashion. This concern is founded on the notion that social reality is hidden from the individual, or at best only partially available; and that the individual's accounts are distorted, subject as they are to prejudice, ignorance, special interest, and a common-sense outlook. The systematic, objective approach of (usually quantitative) social science is thus mandatory, while close intercourse with subjects' mere accounts constitutes one or another brand of subjectivism. This view--and I acknowledge that it is drawn perhaps too starkly here--in turn rests upon a view of the member as essentially passive, as acted upon by social forces rather than being an active participant in their creation. Consistent with this outlook is the correlative tendency to view subjects' remarks as

propositional in character, and thus literal. This ignores the tie between account and setting developed in the marijuana study. It thus seems appropriate at this junction to introduce a somewhat truncated characterization of the ethnomethodological stance that informs my conception of qualitative research.<sup>14</sup>

Jeff Coulter (1979:20-25) has recently provided a formulation of ethnomethodology that is directly concerned with the problem of subjectivity. ( In drawing upon his work to illuminate the issues, we construe his concepts and terminology within the context of the approach to ethnomethodology found in Wilson and Zimmerman 1979/1980<sup>15</sup>). Coulter proposes that the aim of ethnomethodology is "to work out analyses of the reasoning structures and conventional member-orientations involved in various empirically observed courses of social interaction" (p. 21). The "reasoning structures" to which Coulter refers centrally involve the exercise of common-sense knowledge. One can understand common-sense knowledge either in the sense of "knowing that"--common-sense knowledge as comprised of differentially distributed, situated and often inconsistent if not dubious propositions about the social world--or in the sense of "knowing how." It is this latter view of common sense that is crucial. Coulter writes:

The aspects of what can be called 'commonsense' which interest the ethnomethodologist are those which enable anyone possessing it to perform their ordinary activities in ways that are recognizably appropriate, rational, intelligible, proper, correct or reasonable for all practical purposes...commonsense amounts to a set of culturally-furnished abilities. Such abilities constitute the doing of any mundane activities, such as transmitting information in various contexts, recommending something to someone, persuading someone about something, enumerating, grading, complaining, insulting, warning, apologizing, thanking, promising, ascribing statuses, and countless other practical actions...To say of someone that he is able to do such things means that he knows how to do them, and this practical knowledge forms the central core of what is here being described as "commonsense knowledge of social structure" (1979:21-22, emphasis in original).

What one knows how to do, in short, is to methodically produce, in concert with others, the varied events and scenes of everyday life. And, what is important here, the orderliness of everyday settings is available to us as observers for the same reason that is available for the "use and appreciation" (Schegloff and Sacks, 1973:29 ) of members, i.e., by the systematic assembly and recognition of social activities made possible by the practices of common-sense reasoning. An important component of this achievement is found in the intimate "tie between (commonsense reasoning) and mastery of natural language" (Coulter, 1979:21-22; cf. Garfinkel and Sacks, 1970: 341-45). As Coulter notes:

...an overwhelming number of our ordinary, everyday activities are performed in and through speaking, and most of the rest presuppose linguistic abilities...The commonsense competence in which the ethnomethodologist has an interest, then, is in large measure co-extensive with natural-language competence; the one varies with the other insofar as they are mutually constituted. We learn a language and a common culture and pari passu, and we discover, through speaking with others, where, and to what extent, that common culture of a natural language fragments and where it is sustained between us (1979:22).

Thus, the ethnomethodological concern to analyze practical reasoning and the exercise of common-sense knowledge also involves the study of natural language practices. The ethnomethodologically inspired but distinctive tradition of work known as "conversation analysis" has opened this particular subject matter to sociological inspection and has already recorded significant advances in our understanding of the structure of conversational interaction.

Some further remarks on natural language pertinent to the topic of our concern are necessary here. The study of natural language involves the investigation of the systematics of producing utterances, sequences of utterances, and other expressions both verbal and non-verbal which can in turn provide the framework within which it can be ascertained (a) how these

conversational "gestures" achieve a particular meaning or delineated range of alternative meanings in some local environment; (b) how they contribute to, establish, negotiate or expose a "definition or definitions of the situation"; or (c) express and warrant assertions or statements concerning one's or the other's "state of mind," "motive," "feeling," and so on. These activities are seen as situated accomplishments of the use of "natural language" and are of interest as they can lead to a fuller understanding of how the system that produced them works. Ethnomethodology, then, is not concerned with interior goings-on of the acting subject, nor is it occupied with rendering a description of the particular meanings attributed to situations by participants' analyses. The concern is instead: insofar as members recognize and respond to such objects as "state of mind" or "motive" or "the meaning of a situation," then the ethnomethodologist takes it that such objects are methodically produced and appreciated by members, an achievement itself in need of description and analysis in its own right. Moreover, as Coulter has put it:

Nothing in (ethnomethodology's) program commits us to a view of human conduct as beyond the categories of the public, social world; reasoning structures are cultural and the abstract categories of ethnomethodology consist in categories alien to psychologism. There are no 'egos', no irreducible 'impulses', no 'subjective meanings', no 'interior states', no individualizing ontology and no interest in 'the private domain', whatever that could mean. There is no uncontrolled intuiting, even though the exercise of commonsense, reasoned intuition forms a necessary first-order step toward getting analysis off the ground (1979: 24; emphasis in the original).

To summarize, the term "methodology" in the compound word "ethnomethodology" points to a commitment to understand the member-of-society's collective use of systematic procedures to assemble and assess social activities and events. The use of these procedures is itself an activity, not a conception or an idea in someone's head or anyone's head. If any social activities are publicly observable, the activities that comprise the exercise of common-sense reasoning and natural language competencies are publicly observable.

As I hope is evident, I am employing a conception of ethnomethodology to generate what is termed an ethnomethodologically informed sociology" (cf. Wilson and Zimmerman, 1979/80), that is, to attempt the application of ethnomethodological insights and findings, including those of conversation analysis, to the more conventional concerns and topics of sociology. I take Bittner's (1967a,b) studies of police practice; Emerson and Pollner's (1976) research on psychiatric emergency teams, Zimmerman and West's (1975) study of male dominance in conversation, Maynard and Wilson's (1980) inquiry into the reproduction of bureaucratized judicial settings, and Maynard's (1979) research on the conversational basis of "plea-bargaining" as examples of such an ethnomethodologically informed sociology. We also engage in the application of the ethnomethodological framework to the development of appropriate research strategies for the collection and analysis of empirical observation. This leads me to the final issue to be considered in the critical appraisal of field work: guidelines for the analysis of observational material, e.g., field notes and ethnographic interviews.

#### Working Criteria<sup>16</sup>

Ethnographic procedures yield a large amount of detailed and heterogeneous information. It is the investigator's task to transform this fund of data into a defensible representation of the social phenomena of interest. In simpler terms, the investigator attempts to take a large amount of detailed information and make it into a "factual" and general story. The pertinent question here is how this can be done and justified on grounds other than the researcher's intuition, based on familiarity with these details. While many ethnographic reports are formulated in terms of the researcher's general familiarity with the data, analyses of ethnographic materials can be subjected to relatively specific constraints which make the analysis more methodical and rigorous.

The problem facing the ethnographer is essentially this: such social phenomena, e.g., socially organized patterns of behavior, etc., have the character that no single observation

(without the context of other observations) is, in itself, clearly identifiable as an observation of the phenomenon of interest. For example, the normative character of a norm partially resides in its repeated use. A pattern of behavior emerges as a pattern only through repeated observation. And, for example, the idea of personality or character structure as used in clinical psychology and in the field of culture and personality "refers to some observable consistency in behavior. An unrepeated type of action is not in itself made the basis for establishing a dimension of type of personality..." (Turner, 1961:58). As we shall see, repetitiveness is only one of many possible constraints upon the researcher which demand some particular organization for his observations.

The form that the phenomena of interest should have--as those phenomena are conceptualized within some discipline such as sociology--can be "translated" into guides or instructions for the researcher's observations, analyses, and theorizing. While guides or constraints should be derivable from any fundamental social scientific conception of some phenomena, we will illustrate these matters with the fundamental conceptions of social organization and culture which define a very general stratum of sociological phenomena. The proper use of these two concepts, in effect, informs the investigator's sense of what he is looking for and looking at. How these conceptions can operate as constraints on, or guides to, observation and theorizing can be seen by considering each of them in turn.

### Social Organization

"Social organization" refers to regularities in conduct (alternatively, stable patterns of social interactions) which are systematically interrelated. The concept is rather broad and is defined in different ways according to the particular perspective adopted by the investigator. In general, the notion is used to talk about a specifiable population of actors who are linked together through a network of social relationships and who engage in interdependent activities of a distinctive character within a bounded social territory. Under the auspices of

this notion, ethnographers attend to such particulars as the scheduling and sequencing of activities, the use of titles and other forms of address, the ways in which interactions are initiated and terminated, which actors can request or demand what of whom, and so forth (see Garfinkel, 1956). Such details constitute the basis of talking about more global patterns of conduct and their interrelationships.

What must an investigator observe if he is to claim that he has detected some form of social organization conceived of in this fashion? How can he use this basic sociological conception in the actual practice of his observational work? How can he claim to have detected particular patterns of organization in the reported performances and associated commentaries of subjects? The criteria we suggest are derived from explicit and implicit statements of classical sociological theory. Durkheim's (1962) conception of social fact provides for many of these criteria, but they are not unique to his work (cf. Wieder, 1974:31-37; Wilson and Zimmerman, 1979/1980).

Description: To begin with, any analysis requires that the phenomenon of interest be circumscribed and defined in some fashion. Sociologically interesting phenomena may arise from theory or from the researcher's empirical acquaintance with his subject matter. In either case, some statement of the phenomena, couched in the "language of observables," must be made. In survey research, such statements tend to be operational definitions. In ethnographies of all forms, the phenomena may be circumscribed by the ethnographer's attempt to describe some typical social form and then testing that described social form against further data.

How ethnographic or qualitative data such as interviews might be used to develop evidence that a particular pattern of social organization has a typical form may be illustrated by some materials drawn from the marijuana smoking study discussed earlier. I illustrate this and other criteria with the practice of marijuana smoking as a unit of social organization, i.e., the use of marijuana as a socially constrained activity requiring a particular kind of interaction between participants (Zimmerman and Wieder, 1971:42-72). It should be noted that this particular

practice is offered as an example, not in the expectation that it is news, but by virtue of its simplicity. Through the use of diaries (Zimmerman and Wieder, 1977), as well as other information (including limited first-hand observation), a description was obtained of one phase of marijuana use, the practice of "passing the joint" (when more than one person is involved).

Having arrived at a description of a unit of interest, it was possible to inspect diaries for references to "smoking occasions," note the features recorded by the diarist, question the diarist further in the interview, and compare this information with that available from the diaries of other informants or field notes based on direct observation.

The process of searching the materials for instances of the practice thus defined affords the opportunity to confront the occurrence of the practice in a different form. The object of the search is to provide the ground for asserting that the practice, when it occurs, typically occurs in the way specified.

Recurrence: The search for reported instances of the phenomenon of interest in order to gain confidence in its description is inseparably linked to the process of accumulating evidence that the phenomenon recurs as described. For example, it is well known that marijuana smoking is a widespread activity among college-age youth such as those who served as subjects in our research. The point, however, is that we wished to make the claim (trivial though it may appear when viewed in isolation from other aspects of marijuana use) that the practice or social form not only recurs, but that it recurs in a standardized fashion.

Distribution: The criterion of recurrence is also tied to the issue of distribution and cannot be fully explicated without reference to it. The phenomenon must be examined with respect to its distribution relative to the types of situations and types of actors making up the interaction setting of the group in question. Thus, the search of our material focused on the types of occasions in which the practice of "passing the joint" occurred and the composition of the participants. Our conclusions were not surprising: the matter was reported by

virtually every diarist; it was widely distributed with respect to different types of social occasions wherein it was possible to smoke without detection or effective action by authorities; and it was almost universally engaged in by the individuals in question.

Transpersonality: The matters of recurrence and distribution lead us to another criterion: the independence of the practice or social form from the personalities of the individuals involved. In our research, the issue here was whether or not "passing the joint" was a custom that was passed on to each succeeding cohort of persons adopting the life style, since this was central to the determination that the pattern under scrutiny was a feature of the social organization of the community rather than a feature attributable to the makeup of the individuals in the community. Also involved here is the notion of relative persistence, i.e., distinguishing between a transitory phenomenon (a "fad" or "fashion") and something more enduring. The observation that persons are socialized to a pattern is one kind of evidence for its transpersonality.

Diary materials and other data, to no one's amazement, indicated that "passing the joint" was indeed transpersonal in the sense specified above. Just as persons are taught to experience the effect of marijuana as pleasurable (Becker, 1953), they are also taught--largely by example--to "pass the joint," and in addition may acquire certain common rationalizations for the practice, e.g., "it is more economical" or "it's a way of increasing a solidarity," etc. I might add that this practice is by no means unique to the community studied and that others besides counter-culture youth also "pass the joint."

### Culture

Culture is conventionally conceived as a more-or-less integrated system of norms and values which define the desirable ends of action and the appropriate means of achieving them. Sometimes referred to as "designs for living," culture is one basis for the actor's definition of the situation. The relationship between culture and social organization is of critical

interest in ethnographies. Social organization, within this scheme, is the result of norms and values being systematically translated into conduct: the fundamental patterns of social organization are culturally sanctioned. One consequence of this proposal is that the ethnographer must construct a description of "basic" social organizational patterns that is congruent with the actor's view of social reality. Since the notion casts the actor in the role of sanctioner or cultural agent at least some of the time, it must be supposed that instances of the pattern in question are recognizable to him as an instance of the way things should (or should not) be done.

There are, of course, some systematic patterns of action which are indirectly related to norms and values and are, in and of themselves, not specifically culturally sanctioned. That is, it is possible to formulate patterns of behavior that are recurrent, distributed, and transpersonal which are not in any direct sense culturally constrained although they may be derived from more fundamental patterns which are, e.g., differential crime rates, rates of residential mobility, motor vehicle accidents, etc.

From this general sociological perspective there is, then, the additional criterion for developing an empirically warranted description of social phenomena which goes hand in hand with the criteria for describing social organization: the researcher must find evidence that the behaviors constituting the pattern of social organization are sanctioned and enforced within the group in question. In alternative language, the task is to show that such patterns constrain the conduct of the individual, either through internalized dispositions to comply, or institutionalized expectations of compliance enforced by sanctions (Wilson, 1970). I should also point out that since the relationship between culture and social organization appears to be causal, the reader may wonder why we treat cultural features as criteria for an empirically warranted description of social phenomena. In brief, the causal connections between cultural elements, e.g., norms, and the patterns of behavior making up social organization is illusory. Partially formulated cultural elements frame the observations the ethnographer makes of patterns of behavior,

Similarly, partially formulated observed patterns of behavior lead the ethnographer to see that certain utterances of his informants are statements of and about their culture. Culture and social organization are thus aspects of the same phenomenon. (Cf. Wieder, 1974; Wieder and Zimmerman, 1976).

In the example of "passing the joint," one could well raise the question whether the form in which it so regularly occurs is, in fact, culturally sanctioned, i.e., whether deviations are noticed and corrective action applied. No clear-cut instances of deviation were apparent in our materials, so we were required to employ hypothetical examples in certain diary interviews to explore this dimension as well as to examine closely those cases where certain participants refrained from smoking. While I cannot go into detail on our findings in this respect, our analysis suggested that (1) failure to pass a joint, should it occur, would be sanctioned; (2) persons are expected to share a joint in this fashion if they are going to smoke at all; and (3) the ritual takes precedence over other concerns, e.g., the communication of disease (Zimmerman and Wieder, 1971:52-55).

Procedurally, the determination that a pattern is culturally supported rests on (1) encountering reported instances of sanctioning (e.g., a negative remark such as, "He's really on a power trip," or reports that a particular person is being avoided or is subject to derogatory comments by virtue of (a) a particular act or series of acts or (b) the view that he is the kind of person who will persist in certain undesirable behaviors; and (2) incidents in which persons are being explicitly instructed to behave in a certain way.

In concluding this section, I should point out that the patterns or regularities reported by a particular ethnographer (or, for that matter, any investigator, regardless of the method employed) are always more-or-less recurrent, distributed, etc. The quantitative or qualitative specification of these properties is always relative to the sophistication of the available observational and measurement procedures and the feasibility of their use in the particular study, as well as the level of precision required by the problem under investigation. For most purposes, the properties obtained as indicative of stable patterns

of culturally sanctioned social organization can serve as a set of guideposts for directing and evaluating ethnographic descriptions of social phenomena, whatever the scope of observation or level of measurement.

### Conclusion

In this paper I have discussed some issues in qualitative methodology in connection with field research on systems of situated actions in natural settings. I have attempted to deal with two general kinds of issues. One set of questions concerns quite practical matters of technique. The other concerns issues in the basic logic of inquiry. The point of contact between these two levels is the problem of the researcher attempting to develop an empirically grounded and theoretically cogent analysis of a segment of social reality. The central theme around which the discussion has revolved is that the phenomena of social life are produced by the activities of members of society as they pursue the routine and not-so-routine activities of everyday life, while at the same time these activities are available to members as understandable and coherent only within their social organizational context. Thus, from the point of view of both technique and logic of inquiry, the researcher is compelled to discover how social life is systematically organized for the members themselves, not so much in the subjective sense but rather as a matter of intersubjective reality for rendering their interactions with one another coherent and intelligible. It is on this basis that my specific recommendations and general reflections are put forward.

NOTES

1. I have chosen to discuss Blumer's perspective because, first of all, his conception of the subject matter of sociological inquiry is, at one level, similar to ours. He has, moreover, been a consistent advocate of our approach to inquiry which, if not in principle qualitative, has in practice been so very often. The perspective taken by Glaser and Strauss, and by Norman Denzin, are related and similar.

2. In a similar vein, Denzin tells us that the practitioner of naturalistic inquiry

respects and takes seriously those he studies. Indeed, he cultivates close relationships. Hoping to be taken seriously by the subject, he recognizes that alert, observant participants know more than he ever will about the realities under investigation (see Blumer, 1969: 41). Such persons serve as natural resources and checks on emerging theory. Acting as a panel of judges, they collectively and singly evaluate and help construct valid and viable theories of their social worlds...Native persons serve, too, as methodological consultants and field guides...On other occasions the native can coach the observer on new field techniques, suggesting important modifications in existing research strategy (1971:168-69).

3. This is not to say that the description constructed by the researcher is not a "natural language account." When in the form of a research report or article, it is an account delivered within a different setting, that of the practice of social science. We cannot explore the implications of this suggestion here.

4. Wilson and Zimmerman (1979/80) write:

...The concept of reflexivity holds that context (here read: social setting) and particular (here read: a detail or details of an account, or the account itself) are mutually elaborative rather than being analytically independent terms. This relation of...mutual elaboration figures in the specific determination of meaning on a given occasion, and it is the simultaneous seeing of particular and context in their reflexive relation that constitutes the transparency (i.e., readily evident meaning) of displays.

5. It might well be the case that some collectively validated accounts of how things work are merely myths. Such an eventuality is one of the motives for eliciting "private," "personal" or "off-the-record" opinions of a setting's workings. Informants may furnish an "official account" in the hearing of others, and a different version in confidence. The finding that other informants reveal similar unofficial versions in private could thus illuminate the structure of "insider's" versus "outsider's" knowledge and permit a fuller understanding of both ideology and the organization of activity in the setting.

6. Similarly, inquiry cannot be confined to faithful witness to the member's understanding of some setting; sociological interest requires more than a chronicle of everyday life.

7. The materials in this section (pp. 8-15) are adapted from Don H. Zimmerman and D. Lawrence Wieder, "The Diary--Diary Interview Method," in Robert B. Smith, ed., Social Science Methods, Vol. I, forthcoming. I would like to thank Professor Wieder for permitting the use of these materials. Any reference to this discussion should include an acknowledgment of his contribution.

8. Our discussion of the "positioning problem" assumes that the investigator has opted for the overt participant-observer strategy. One can, of course, distinguish between types of observer roles. For example, the overt participant observer declares himself to his subjects and openly assumes the role of sociologist in the midst of the ordinary social activities characteristic of the setting he is studying. As an overt participant observer, he intervenes in the routine of his subjects. Intervention may be minimal, involving conversations or even formal interviews, or he may assist with or join in the activities of his subjects. The covert participant observer is something like a spy. He pretends to be something he is not--an ordinary participant--and operates with an ulterior motive, namely, to report on the activities he observes. In some instances the masquerade is limited and involves the exploitation of very standardized conventional roles, e.g., bar patron (Cavan, 1966). A little further down the line is a type of infiltration,

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e.g., Humphrey's (1970) assumption of the role of "watchqueen" in his study of sex in public restrooms. At the extreme is the manufacture of a biography, status characteristics and even physical appearance to gain access to attitudes and actions otherwise difficult to observe. There is a unique and controversial study by a research team working for the U.S. Air Force in which an Air Force officer was converted into a basic trainee to study the morale of recruits (Coser, et al., 1959; Sullivan, et al., 1958; cf. also Erikson, 1967). The overt non-participant observer is, obviously, known to his subjects as a research scientist, but remains in the background and does not interact with subjects. It is difficult to locate a clear instance of this type outside the laboratory setting. The covert non-participant observer mode is exemplified by research done by Sudnow (1972) in which surreptitious photographs of individuals are employed as data for the analysis of "glance behavior." For this line of research, it is neither necessary or even desirable for the observer to declare himself or participate. In part, this is due to the nature of the social activities of interest which are assumed at the outset to be highly standardized behaviors which are frequent and common and which can be found in most public places where the observer has access as a member of the public. For more detailed treatments of various classifications of the observer role, see, for example, Denzin (1970:185-218); Junker (1960:35-40); Lofland (1972:93-116); and Schatzman and Stauss (1973:52-56).

9. An obvious expectation of the ethnographer is that the setting he enters is organized in definite ways

(Humphreys, 1970:24-26). From the outset, the would-be observer is anticipating the constraints imposed by the cultural and social organization of the group he has chosen to study. The ways these constraints operate pose the problem of initial access to the setting, which can be quite varied. For example, formal organizations are available as a research site contingent upon the approval of those in authority, and it is characteristic of such settings that there is a definite

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structure of authority. In contrast, settings like bars and public restrooms are different kinds of possible research targets and are characterized by different internal organizations. Cavan (1966) exploited the fact that there are readily available roles which she, as an adult, could easily assume, and thus her access problem was trivial. She also discovered that certain bars were largely patronized by men, and her sex thereby became a liability. She was also constrained in her observation by the limits of normal bar-patron conduct. Humphreys' (1970) investigation of sexual activity in public restrooms capitalized on the fact that these behaviors were also organized into definite role relationships, among them the role of "watchqueen" or lookout who, by fulfilling the necessary function of warning those engaged in sex of the approach of "outsiders" could also observe what was going on in the restroom. Humphreys, by assuming this role, provided for his covert sociological purpose, observation, while at the same time avoiding full participation himself. Both Cavan and Humphreys opted for the covert participant-observer approach. Most sociological field studies are undertaken in the overt participant-observer mode, in part for ethical reasons, in part for the reason that the covert mode constrains the observer to the confines of the indigenous role he or she assumes, whereas the creation of the observer role within a given setting typically allows greater latitude of observation.

10. In line with earlier considerations, such "private" views should be assessed with respect to their possible character as shared-if-covert student views (e.g., students know that most of their peers dislike their classroom experiences but keep quiet about it) or as merely personal preferences held in private, etc.

11. There is another sense in which tracking may be even more systematic. While Skolnick permitted the relevancies of his observations--the questions that he asked himself and his subjects about what they both were observing--to emerge out of the apparent concerns and interests the subjects had in the developing events, e.g., first they were worried about this, then they were pleased about that, et., there is another, more structured form of tracking which Reiss, one of its advocates, calls

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"systematic observation of natural social phenomena" (Reiss, 1971). In this method, the observer records the observations he makes while tracking in a pre-structured manner. The standardized recording form instructs the observer to note certain predecided and, hence, standardized features of the episodes that he witnesses.

12. In one of my field studies (Zimmerman, 1969, 1970a, 1970b), the general organization of the research site, a district office of a welfare bureaucracy, was provided by a training class for new welfare workers which I attended. The design of the subsequent observational study was based on this information.

13. The discussion that follows is adapted from Don H. Zimmerman and D. Lawrence Wieder, "You Can't Help But Get Stoned: Notes on the Social Organization of Marijuana Smoking," Social Problems 25: 298-207. We would again like to thank Professor Wieder for allowing us to use this material. Reference to this section should include an acknowledgment of Professor Wieder's contribution.

14. The discussion that follows (pp. 18-20) is adapted from Candace West and Don H. Zimmerman, "Conversation Analysis," forthcoming, in P. Ekman and K. Scherer (eds.), Methods of Research in Non-verbal Communication. We would like to thank Professor West for her permission to use this material. Reference to this discussion should include acknowledgment of Professor West's contribution.

15. Coulter's own framework is heavily indebted to a Wittgensteinian approach to the philosophy of language, whereas it is not apparent that our approach (Wilson and Zimmerman, 1979/1980). requires such commitments. Coulter's discussion of subjectivity is cogent from either point of view, and so these exigetical questions are irrelevant here.

16. This section (pp. 21-23) is adapted from Don H. Zimmerman and D. Lawrence Wieder, "The Diary--Diary Interview Method," in Robert B. Smith (ed.), Social Science Methods, Vol. I, Irvington Press, forthcoming. We would like to thank Professor Wieder for permission to use this material. Reference to this section should include an acknowledgment of Professor Wieder's contribution.

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QUALITATIVE "VERSUS" QUANTITATIVE METHODS IN SOCIAL RESEARCH:

An Essay on Theory and Method<sup>+</sup>

The controversy over qualitative and quantitative methods is perhaps the most enduring one in social science. According to one extreme view, we can acquire objective knowledge of social life only through classifying, measuring, tabulating, and using statistical methods. Other procedures can contribute, if at all, only by suggesting ideas for hypotheses that can then be tested by rigorous and objective quantitative methods. Moreover, according to this view, talk of "qualitative methods" is pernicious since it tends to encourage indulgence in undisciplined subjectivism. At the other extreme, according to the radical qualitative position, quantitative methods impose a structure and form inherently alien to the vital texture of social life, which can be grasped only in its complex detail and wholeness. Statistics might be useful to organize superficial facts wanted for administrative purposes, but they cannot reveal anything significant about the basic nature of social life. On this account, the notion of "quantitative methods" is at best mischievous, for it leads to a mindless empiricism that, in its very claims to objectivity, is fundamentally misleading about the nature of social life.<sup>1</sup>

The issue of the status of quantitative methods is an old one, and in fact it seems to be responsible for the work "sociology": apparently Comte coined this barbarous neologism when his earlier term, "social physics," was purloined by Quetelet to describe statistical studies of distributions of individual behavior, an approach Comte regarded as totally inappropriate (Hayek, 1952:177). From a contemporary perspective, Comte's rejection of quantitative methods seems incongruent with his commitment to the natural sciences as the intellectual model for social science, but it is perhaps explicable in light of his equally strong commitment to radical methodological holism and failure to understand that statistical methods can deal

with aggregate data. Be that as it may, since that time, the quantitative position has tended to become aligned with unquestioning acceptance of the natural science model, in part no doubt because of the wide-spread belief that the essence of the natural science approach is nomothetic explanation through formulation of deterministic or probalistic laws holding universally within their domains of applicability. Correspondingly, the qualitative position has become associated with rejection of the natural science model and insistence on the relativity of knowledge of a social phenomenon to the context of that phenomenon. This context is sometimes construed as the prevailing historical and cultural milieu, as in macro-historicist positions, and sometimes as the immediate concrete interaction situation, as in micro-historicist or situationalist approaches, but in either case the method is ideographic, denying the importance of causal generalizations and emphasizing instead the elucidation of the full concrete detail of the phenomenon and the meaningful relations of these to each other and the whole of which they are parts. Thus, the question of quantitative and qualitative methods has been confounded with polemics over "scientism" versus "historicism" and the contemporary tendency to identify objectivity and rational empirical inquiry with what are taken to be the methods of the natural sciences. The person who rejects the hegemony of quantitative methods seems thereby to be rejecting the quest for objective knowledge and championing some form of radical historicism, and one who rejects the natural science model as inappropriate for the social sciences is seen as rejecting any use of quantitative methods whatever.

Most persons engaged in social research probably incline to a moderate position between the extreme quantitative and qualitative views in their day-to-day work, particularly when they are not pressed to display their scientific credentials. While perhaps favoring one method or another in their own research, they are prepared to accept other approaches as necessary and make use of studies employing quite different methods in order to supplement and interpret their own data. Thus, as a practical matter, social researchers

in fact tend to take for granted the interdependence of quantitative and qualitative methods in the course of actual research. Nevertheless, the polemics issuing from the extreme positions and the uncompromising tone in which they are expressed render dispassionate assessment of the real methodological issues difficult if not impossible. Consequently, what is probably the predominant methodological attitude in practice lacks systematic formulation and, more importantly, clear foundations transcending the extreme quantitative and qualitative positions. The purpose of this paper is to examine these issues in an attempt to move beyond this increasingly sterile and unrealistic controversy and establish rational grounds for the integration of quantitative and qualitative approaches.

#### A Point of Departure

It is evident that one's ideas about appropriate methods are interconnected with one's conceptions of the nature of society and what one can say about social phenomena. However, for all the talk of sociology being an empirical science, the question of the fundamental nature of society tends to be addressed in abstract a priori terms, and issues of methodology tend to be discussed on the basis of philosophical assumptions about scientific method. One is reminded in this connection of the Methodenstreit at the turn of the century in Germany, in which the issues of nomothetic versus ideographic approaches was debated from empiricist, idealist, and neo-Kantian positions. More recently, in the nineteen thirties in the United States we find the controversy over statistical versus case studies informed by logical positivist and pragmatist conceptions of scientific method, and in contemporary debates opposed philosophical premises are no less evident. But, it may be suggested, the interminable nature of the controversy results precisely from these appeals to philosophical doctrines. For, arguments from a particular philosophical position will be persuasive only to those who already hold the same philosophical commitments. Thus, resort to particular philosophical principles, whether those of phenomenology, neo-positivism,

marxism, Popper, Feyerabend, or whatever, cannot be expected to convince persons not sharing that philosophical persuasion. To make progress, then, we must approach the issues in some manner other than on an abstract level of first principles.

The central proposal I want to make is that the nature of society is a question that has some essentially empirical elements to it, and consequently one cannot choose a methodological position completely freely in accord with tastes, prejudices, philosophical convictions, or intellectual tradition. That is to say, methodological positions imply assertions about the nature of society that are subject to empirical examination, and if some of these assertions turn out to be false, the methodological positions from which they follow cannot be tenable. Put in a positive way, methodological doctrines, while underdetermined by the facts, must nevertheless respect the facts. We can of course ignore empirical reality and adopt ideas and methodological canons at variance with the facts as the basis for our attempts at social science. But to the extent that we insist on such methodological principles at the expense of attending closely to the phenomena we study, our results will be pretentious, sterile, uninteresting, and misleading. And this is not an unfair characterization of social research, whether qualitative or quantitative, that emphasizes its methodological purity.

### Situated Action

What, then, are the facts that any methodological discussion must respect? We begin by noting that in a concrete situation, the participants recognize and describe what they are doing in such a fashion that the action of one person can reasonably be seen as attending to and following upon the action of another, and that these accounts serve as warranted grounds for further inference and action not only in this situation but in subsequent ones as well (cf. Mills, 1940, and Garfinkel, 1967). Situated actions, then, are produced in particular concrete situations and are available to the participants

for their own recognition, description, and use as bases for further action. This characterization holds for virtually all social interaction, whether between four-year old preschool children, ordinary adults, atomic physicists setting up some apparatus, or behaviorist psychologists discussing the results of an operant conditioning experiment. In each case, the participants attend to and take seriously what the others say and do.

The central point now is that for the most part the social sciences define their subject matter in relation to situated actions. Note, for example, that one is not confined to the categories used by the actors, for it is perfectly possible to employ second-order constructs that are not used by the people whose actions are being studied, provided these constructs are grounded in those actions. Thus, a community may have no arrangements for tabulating crimes and calculating crime rates, nor indeed even any conception of a rate; but this does not prevent an observer from gathering the data and studying crime rates provided a notion of crime is institutionalized within the community. Here a second-order construct, "crime rate", is unknown to the members of the community, but it is grounded in activities that are indeed recognized by them, namely crimes. Observe also that historians concern themselves with documents and artifacts that are the products of action within concrete settings, and the routine questions of historiography begin with, Who produced it? In what context? For what audience? For what purpose? With what effects? Further, the classical anthropological and sociological ethnographies have been explicitly directed to describing situated actions and their social organizational contexts. And insofar as economics deals with commodities, prices, media of exchange, and the like, it deals with the products of situated action. In short, much of what is conventionally regarded as social science is concerned with a social world constituted by the situated actions of members of society, whether this be investigated from a "micro" or a "macro" point of view.

The only way to avoid defining social phenomena in terms of situated actions and structures of them is to treat situated actions themselves as epiphenomenal and hence of no real significance. One tradition taking this view is biological reductionism, which defines behavior as a physiochemical response to physical and chemical events in the environment of the organism, or as the working out of genetic inheritance independently of environmental stimuli. Another tradition that treats situated action as epiphenomenal is idealism, especially prominent in the German historical tradition of the last century and represented in contemporary form most notably in the structuralist approaches of Levi-Strauss, Althusser, and their followers. While both biological reductionism and idealism have generated some controversy and attracted coterie of adherents, they are open to a number of well-known objections and have had only marginal impact on the main traditions of Western social science, and we will not address them further.

The phenomena of interest to conventional social science, then, are constituted by situated actions. Consider now three rather obvious features of such actions.<sup>2</sup>

(1) The objectivity of social structure

Members of society tend to treat social categories, customs, norms, recurrent patterns of events and the like as existing "out there" and independently of any particular individual's doing. That is, social structure has an apparent objective character in relation to individuals and their actions and however it is encountered it is a fact of life to be taken into account or ignored at one's peril.

(2) The transparency of displays

Within a particular social group, it is in most instances plainly evident to the members what others are doing. Thus, one is generally able to see at a glance that another is chopping wood rather than baking bread, or is saying that the post office is to the left rather than straight ahead.

Gestural and verbal displays, then, are transparent in the sense that members can usually apprehend directly the concrete, situated actions being performed.

(3) The context-dependency of meaning

The meaning of a gestural or verbal display depends on the context of its occurrence, so that physically identical displays can have different meanings and different displays the same meaning, depending on the situation.

Note that these three features of situated action are essential properties of the social world for the member. Thus, it is a matter of basic competence that the objectivity of social structure, the transparency of displays, and the context-dependency of meaning be recognized and, if need be, enforced. From the point of view of the members, the social world is already objectively out there, transparent at the level of everyday words and deeds, and determinate in sense if context is consulted. The person who attempts to treat social categories and norms as mere caprice or fantasy, who resolutely insists that what others are doing is unintelligible, or who demands literal rather than contextually-informed interpretations is quickly defined as too dangerous or incompetent to be allowed normal unrestricted rights within the group.

In these terms, the extreme positions with respect to quantitative and qualitative methods selectively emphasize different features of situated action and neglect others. The radical quantitative view focuses entirely on the experienced objectivity of social structure and transparency of displays while treating the context-dependency of meaning as merely a technical nuisance to be dealt with in specific research situations but without theoretical or methodological importance. This assumption is the logical underpinning of the view that natural science is the appropriate intellectual model for social science, and it paves the way for the presumption that quantitative methods are inherently superior to qualitative approaches. In contrast, the radical qualitative position emphasizes the context-depen-

dency of meaning but neglects the objectivity of social structure and the transparency of displays. This not only provides grounds for rejecting the natural science model, but also encourages denial of the importance of any regularities in patterns of situated action and a narrow focus on the full detail and complexity of any form of social phenomenon.

The central problem here is an inadequate understanding on all sides of the nature and implications of the context-dependence of meaning, with the result that the importance of context is either dismissed as uninteresting or elevated to the status of a single over-riding principle. However, while this points to the source of difficulty, it does not get us very far toward a more adequate methodological position. It is one thing to acknowledge the facts that social structure has an apparent objective character, that situated displays are by-and-large transparent in meaning, and that meaning depends on context, and quite another to take adequate account of these commonplaces in our methodological reflections. For the latter we require in addition some empirical understanding of the nature of context embeddedness. Consequently, we digress in the next two sections to consider briefly the results of some relevant empirical research concerning context embeddedness of situated actions and its relation to social structure.

#### Context-Embeddedness of Meaning: Indexicality and Reflexivity

The most extensive empirical investigations of the context-embeddedness of meaning have been carried out by ethnomethodologists. Consequently, to obtain a foundation for considering the basic methodological issues with which we are concerned, we consider the results of ethnomethodological studies of social interaction. In this section, we introduce several concepts important for the remainder of the discussion.<sup>3</sup>

### Particulars

We use the term "particular" to denote anything that can enter into the constitution of meaning or sense on some occasion. Thus, a word, a gesture, or an action is potentially a particular. Particulars are collected as clues at the scene of a crime, artifacts at an archaeological dig, mementos of special occasions, indications, signs, warnings, trends, or proofs. Particulars are used to fashion an account or description of activities, events, occasions, and the like.

### Indexicality

The expression, "indexical particular" is used to indicate that such particulars depend on context for their identity and meaning. As we have already suggested, the assertion that the particulars with which we fashion our depiction of the world around us are context-dependent is a common place: persons in everyday life acknowledge the indexical character of particulars and hence of the accounts they constitute. What is not commonplace is the further suggestion that a given context invoked to disambiguate a particular is itself indexical (cf. Garfinkel, 1967:10). In order to deal with the indexicality of particulars, one might consider not a particular in isolation but rather a particular in conjunction with its context. The force of the proposal that contexts are also indexical, then, is to block this move: indexicality is irremediable in the sense that any attempt to disambiguate a particular requires invocation of a context of further particulars that are themselves indexical.

At this point it is crucial to recall the fact of the transparency of displays: the implied ambiguity of a display of particulars is in fact resolved in a routine, taken-for-granted basis by members of society. Members of society do not experience a giddy swarm of indeterminate particulars, but rather a relatively stable, coherent social world. Not only do we, as a practical matter, come to terms with the irremediably indexical character of talk and action, but as an equally practical matter, we hold each other accountable

for the reasonable, timely, and largely effortless accomplishment of the for-all-practical-purposes mastery of indexicality, that is, for the accomplishment of the transparency of displays.

### Reflexivity

However, matters are still more complex. Consider the following question: if a particular is to be given a determinant meaning in relation to some context of other particulars, how are those particulars-as-context selected? The very item one is attempting to disambiguate provides the key to the assembly of a context, but at the same time, it is that context which gives the particular its specific identity in terms of which a relevant context can be constructed. In short, the relation between a given particular to be disambiguated and any particular in its disambiguating context is reflexive, for the given particular is part of the context needed to disambiguate the particulars in the context. Thus, instead of an infinite regress of successively broader contexts, we find a reflexivity between the particulars constituting the given concrete situation at hand. In this sense, moreover an account, which is a particular, is reflexive in that it is an event in the self-same order of affairs that, in explaining and describing, it renders orderly, (Garfinkel, 1967:7-9).

The idea of reflexivity can be illustrated by the well known face/goblet illusion. Seen one way, the picture is of two faces in silhouette with a white space between them; seen another way, it is of a white goblet against a black background. The essential point is that, either way the picture is seen, each part of the contour separating the white and black areas takes on an identity only in the context of the other parts and the whole. Thus, for example, one portion of the contour is a nose only in the context of another portion that is a chin, and vice versa. Moreover, these can be a nose and chin only in the context of the face as a whole. But a face can be seen at all only because of the presence of a nose and chin. And

clearly the picture and its parts are not intrinsically any of these things since the picture can be seen alternatively as a goblet, in which case the very same parts of the contours become completely different things. Here, then, we cannot conceive of the whole as assembled out of pre-existing parts, nor of the parts as determined by some pre-existing whole. Observe, moreover, that when the figure is seen as a picture of two faces, it is clearly and definitely that, and the fact that the picture can be seen otherwise does not disturb this definiteness and clarity.

The concept of reflexivity holds, then, that context and particular are mutually elaborative rather than being analytically independent terms. This relation of reflexive mutual elaboration occurs in the specific determination of meaning on a given occasion, and it is the simultaneous seeing of particular and context in their reflexive relation that constitutes the transparency of displays.<sup>4</sup>

The result of numerous empirical studies is that reflexivity is a universal feature of situated action. Research in diverse institutional settings has forced the conclusion that in every case the meanings of actions, rules, norms, and social categories depend on the context of the particular occasions on which they occur or are used.<sup>5</sup> Thus, the context dependence of meaning is not merely an occasional lapse on the part of more-or-less naive members of society but rather an inherent feature of situated action. This conclusion, it must be emphasized, is an empirical one rather than merely a theoretical or philosophical speculation. We must, then, take it seriously if we want to have a realistic approach to the study of social phenomena.

Context-Free, Context-Sensitive Mechanisms of Interaction

We have noted several times that, from the point of view of the competent member of society, it is an obvious fact that the meanings of gestures and talk depend on context. One result of ethnomethodological research has been to establish this indexicality as a universal feature of social interaction, and hence to force attention to the reflexivity of situated actions and their contexts. However, we can go further than merely documenting the obvious. Research on the detailed structure of interaction has begun to suggest how reflexivity is built into social interaction in such a fashion that the social world appears as an objective reality that for the most part is transparent as to the concrete meanings of situated actions.

One concept that appears to be extremely useful in organizing the results of these analyses of social interaction is that of context-free, context-sensitive mechanisms (Sacks, Schegloff, and Jefferson, 1974). The leading idea is that any particular concrete episode of interaction is organized in the course of its development by the participants employing quite general mechanisms to deal with interactional problems that must be solved if the interaction is to continue, and that to be effective these mechanisms require exploitation of sequential and social organizational contexts. That is to say, the mechanisms are context-free in the sense of being available for use by any participant in any interaction situation and context-sensitive in the sense that competent use requires implicit or explicit invocation of or reference to the place of the current action in the developing sequence of interaction and the social context and identities of the other participants. It is this context-free, context-sensitive character of the mechanisms by which interaction is organized that provides for social structure being an objective feature of social interaction, not in spite of, but rather by virtue of reflexivity.

Perhaps the most fundamental of these context-free, context-sensitive mechanisms is the process described by Sacks, Schegloff, and Jefferson for turn-taking in conversational interaction, that is interaction in which there are no prior institutional arrangements prescribing (1) the order in which people speak, (2) the lengths of their turns, or (3) the specific content of what they say (Sacks et al., 1974). This mechanism can be formulated as a sequence of options available whenever the question of speaker change arises:

- (1) the current speaker may select another party as the next speaker;
- (2) if the current speaker does not select the next one, then another person may select him or herself as the next speaker;
- (3) if no other party selects him or herself as next speaker, then the current speaker may continue;
- (4) and finally, if the current speaker does not continue, the option to speak cycles back to (2) (1974:703-706).

Here a turn consists not simply of a segment of talk by one person bounded at each end by the speech of others, but rather of a period of time during which a person has the right and obligation to speak. Turns are constructed by the speaker out of unit-types, which in English may be single words, phrases, clauses, sentences and perhaps longer segments. On gaining a turn, the speaker has an initial right to produce one such unit, and the terminal boundary of a unit-type, such as the end of a sentence, is a possible transition relevance place, at which a transfer of the turn from one speaker to another may properly occur, and at which options (1)-(4) become available in the indicated sequence (Sacks et al., 1974:702-706). It should be noted that speakers project the structure of their utterances in such a way that the listener can anticipate an up-coming transition relevance place such that if turn transition occurs, it can do so with minimum gap or overlap (Sacks et al., 1974:702-703; Schegloff, 1980). Moreover, the phenomena of turn taking discussed by Sacks et al., are

oriented to by the participants in the sense that if the mechanism fails on a given occasion, this creates a situation requiring repair, e.g. a situation of more than one party speaking at a time, that is noticed by participants and which somehow must be resolved (Sacks et al., 1974:723-724). And, it is assumed, there is available to the participants a set of repair mechanism for addressing the interactional problems in this new situation. (See also Jefferson, 1972, 1975; Sacks and Schegloff, 1974; Schegloff, 1972; Schegloff, Jefferson and Sacks, 1977). Finally, the turn-taking mechanism is context-free in that it applies whenever a transition-relevance place occurs in a conversation and is usable by any set of participants irrespective of the setting, the size of the group, and the social categories that distinguish them (Sack et al., 1974:699). And the mechanism is context-sensitive in that what constitutes a transition-relevance place is a contextual matter and it is usable by participants to manage such things as changing topic (Maynard, forthcoming) or to weave exogenous factors such as the situated identity of the speaker into the course of an actual conversation as, for example, when a subordinate "speaks only when spoken to" or a dominant party interrupts (cf. Zimmerman and West, 1975; West and Zimmerman, 1978). The context-free, context-sensitive character of the mechanism means that the process by which turns are allocated and regulated is the same in all conversations and invariant to the particular motives, characteristics, and circumstances of the participants. Yet how that mechanism is used by participants depends on the particular setting, the situated identities of those present, and their purposes at hand.

Turn-taking is one of a family of interrelated mechanisms for social interaction that includes, for example the following: adjacency pair structures such as question-answer, request-response, and insertion sequences (Schegloff, 1972); the organization of story telling (Jefferson, 1979; cf. also Sacks, 1974); negotiation sequences (Jefferson and Schenkein, 1977; Schenkein, 1979; Maynard, 1979); agreement-disagreement sequences (Pomeranz, 1975); the opening and closing of conversation (Schegloff, 1968; Schegloff and Sacks, 1973);

locational formulations (Schegloff, 1972); and repair systems (Schegloff, Jefferson and Sacks, 1977; Jefferson, 1975). In each case, we have a context-free, context-sensitive mechanism through which the participants construct their actual conversation turn by turn. Social interaction, then, is constructed within actual situations by actual participants. It is managed over the course of particular turns at talk, and the conversational events thus constituted are securely lodged in a sequential and social organizational context, displaying transparency of meaning except in anomalous instances needing corrective work.

### The Reproduction of Social Structure

Social interaction takes place through mechanisms which are context-free in that they are available in any setting or situation but are context-sensitive in that their appropriate use at any particular point depends on the sequential and social context. However, there appear to be the makings of a paradox here, since social structural categories are, on the evidence, as context-dependent for their meanings as any other features of social interaction. Let us turn, then, to a more detailed look at the way social structure is related to situated action.

For this purpose, we make use of the notion of social category as it has been refined in Sacks' discussion of membership categorization (1972a, 1972b). A membership categorization device contains a collection of categories that may be applied to the members of a given population. For example, a prominent device in contemporary society is "occupation", where the categories applied to various members include "mechanic", "student", and so on. A related idea is what Sacks terms a "category bound activity", which is a specific activity unputedly done by members of given categories. Thus, attending classes is an activity bound to the category "student", as is being late to classes, while making investment decisions is an activity bound to the category "banker", as is embezzlement.<sup>6</sup> The key point

now is that membership categorization devices and the categories and category-bound activities they contain are the means by which members make use of institutional structure in the course of their interaction to make what they are doing intelligible, and at the same time it is through such use that social structure is reproduced as a real thing in the social world of the members. Thus, as Giddens (1977:118) has pointed out, social structure is simultaneously a resource for and a product of social interaction.<sup>7</sup>

Let us consider an example, analyzed for somewhat different purposes in Maynard and Wilson (1980, pp. 299).

1. George: 'R you takin' Sosh or what. Or
2. Laura: Yea:ah. (.) Sosh two

For George's question to be meaningful, he must take it for granted that Laura is a student at X University (where the conversation was recorded), and that taking sociology courses ("Sosh") is one of the things such students do, that is, it is an activity bound to the category "Student at X University". Note, further, that for Laura's response to be heard as an answer to George, she also must take it for granted that she is a student at X University and that taking sociology is an appropriate category bound activity. Here, plainly, George and Laura are employing elements of what, from a sociological point of view, we would call the institutional structure of the university to render their interaction intelligible and coherent. Observe also that from a technical conversation analysis point of view, this exchange is a simple question-answer sequence, and the context-sensitivity of such a structure consists precisely in the fact that what can be heard as a sensible question and what can be heard as an answer depend on the context.

So far, the argument is a fairly standard conversation-analytic one. The additional sociological point to be made now is that in employing these categories in this way, Laura and George in fact are reproducing the institutional structure of the university. For, in

using the categories and category-bound activities in this fashion to render their interaction coherent and meaningful, and at the same time treating each other's talk as warranted grounds for further inference and action, the speakers are presenting the university structure as a real object.<sup>8</sup> However, it must also be emphasized that social structural categories employed in this way in social interaction are no more exempt from the context-embeddedness of meaning than are any other particulars. Thus, what a student is and does are not matters that can be determined independently of particular interaction situations. Hence the social structure that is reproduced in social interactions is constituted through those interactions.

As another example, consider the following interaction in a courtroom. The participants are a judge, a public defender (P.D.), and a district attorney (D.A.) regarding a defendant who had earlier pled guilty to a charge of second degree burglary. The defendant was also present but did not speak. The question before the court is that of sentencing.

1. P.D.: Your Honor, we request immediate sentencing and
2. waive the probation report.
3. Judge: What's his record:
4. P.D.: He has a prior drunk and a GTA (Grand Theft Auto).
5. Nothing serious. This is just a shoplifting case.
6. He did enter the K-Mart with the intent to steal.
7. But really all we have here is a petty theft.
8. Judge: What do the people have?
9. D.A.: Nothing either way.
10. Judge: Any objections to immediate sentencing?
11. D.A.: No.
12. Judge: How long has he been in?
13. P.D.: Eighty-three days.
14. Judge: I make this a misdemeanor by P.C. article 17 and
15. sentence you to ninety days in County Jail, with
16. credit for time served.

This transcript is analyzed in some detail in Maynard and Wilson (1980), but for our purposes it is sufficient to draw attention to two major points. First, observe that at each turn the talk is understandable only by invoking the criminal justice system as an institutional context, and in employing the categories of the criminal

system in this way, the participants are reproducing that structure. Second, note that this exchange departs from a conversation in that the judge here has preemptory rights to interrupt, ask questions, and in general direct the course of interaction. However, these features are notable precisely because of their departure from the turn-taking model for conversation, and they are intelligible only by invoking the institutionalized role of the judge. Thus once again we see the reflexive relation between interaction and social structure, for in using the category "judge" to render the interaction coherent, the role and its institutional framework are reproduced.

To summarize, the social world is constituted through situated actions produced in particular concrete situations and which are available to the participants for their own recognition, description, and use as warranted grounds for further inference and action on those same occasions as well as subsequent ones. Situated actions are produced through context-free, context-sensitive mechanisms of social interaction, and social structure is used by members of society to render their actions in particular situations intelligible and coherent. In this process, social structure is both an essential resource for and a product of situated action, and social structure is reproduced as an objective reality that partially constrains action. It is through this reflexive relation between social structure and situated action that the transparency of displays is accomplished by exploiting the context-dependence of meaning.

#### Methodological Implications

Let us return now to our main concern, the question of the relation between quantitative and qualitative methods and the issues with which this has become surrounded. It is evident that on empirical grounds the context-embeddedness of meaning cannot be disregarded as a technical nuisance, nor can it be used to justify denial of the importance of regularities in patterns of situated action. The metho-

dological situation of the social sciences, then, is quite different from what has been assumed in most current discussions, and it is this that leads to the gap between formal methodological canons and much actual research practice. To move toward a more realistic approach, we must in fact abandon four central assumptions underlying most formal discussions of social science methodology: the utility of the nomothetic-ideographic distinction, the notion that the basic methodological problems of quantitative and qualitative approaches differ in kind, the assumption that quantitative and qualitative approaches are genuine alternatives, and the idea that objectivity is a property of knowledge that derives from adherence to specified rules of procedure.

#### Untenability of the Nomothetic-Ideographic Distinction

The distinction conventionally drawn between seeking to explain social phenomena in terms of transhistorical universal laws on the model of the natural sciences and attempting to understand them in all their concrete individuality and complexity is fundamentally misleading. This dichotomy informs virtually all methodological discussions in the social sciences, but it profoundly misrepresents the nature of social reality by supposing that one can have either genuine nomothetic explanation of social phenomena or purely ideographic understanding when in fact neither is possible.

On the one hand, nomothetic explanation consists of showing that the facts to be explained can be deduced logically from a conjunction of universal laws and further facts about the particular situation that are taken to be given for the purpose of the explanation. For this kind of explanation to make sense, the terms appearing in the laws and descriptions of phenomena must have the same meaning no matter where or when they are applied, since otherwise one cannot claim to have used the same law to explain facts in different situations. However, we have seen that situated actions are reflexively tied to the social structural contexts within which they occur, and

these contexts vary across cultural traditions and over historical periods. Consequently, the only possibility for genuine nomothetic explanation of social phenomena is to describe them in terms that are entirely independent of the meanings of situated actions, which would require abandoning most of the topics of interest to social science. Thus, the search for non-trivial, non-metaphorical trans-historical laws of social phenomena will be as barren in the future as it has in the past.<sup>9</sup>

On the other hand, the idea that one can ignore regularities in patterns of situated action is equally misleading. For, we have also seen that social structural categories reflecting trans-situational regularities enter into the constitution of the meanings of situated actions, so that one cannot make sense of what is going here and now without reference to regularities in the social environment. Thus, understanding social phenomena cannot be limited to grasping complexes of meanings, even as the question of meaning cannot be dismissed as "metaphysical" or "subjective".

All this does not, of course, imply that general concepts are irrelevant to the social sciences, but only that their status is different from that of general concepts in the natural sciences. In the natural sciences, general concepts represent classes in the strict logical sense, whereas in the social sciences they are ideal types in Weber's sense (see Burger, 1976). Further, comparative studies across cultural and historical contexts remain fully intelligible, but their purpose cannot be reasonably construed as seeking universal generalizations but rather as locating and illuminating similarities and differences between the particular cultures or historical periods that are examined. Here, Weber's work still stands as a model.

The Homogeneity of Methods

It is evident that trans-situational regularities within a particular institutional context abound, and that understanding them is essential for understanding how a particular social system works. It is obvious, further, that quantitative methods, often highly sophisticated, are needed to unravel such phenomena. However, it is also apparent that identifying the component events making up such regularities depends irremediably on a qualitative understanding of the situated actions through which the regularities are produced. At the very minimum, the operations of recognizing and classifying events depend on such qualitative understandings, and all subsequent manipulation and interpretation of the data is intelligible only in light of those understandings.

From these observations it follows that no clear distinction can be drawn between the basic methodological problems of quantitative and qualitative approaches. For, if we leave aside purely technical matters such as sampling procedures and methods of statistical estimation and inference, we see that the gathering and interpretation of quantitative data is methodologically indistinguishable from the gathering and interpretation of qualitative data. Indeed, what we call "quantitative" data turns out on inspection to be merely repeated qualitative observations by someone, whether the researcher and his or her assistants, government bureaucrats, or ordinary members of society. Thus, all the problems of "bias", "selectivity", "subjectivity", and the like that can be raised about a field worker's ethnographic report have counterparts in the form of interviewer effects, questionnaire design, selection and formulation of specific items, and so on, that are well known to those who must deal with the practical problems of gathering data, cleaning it, and organizing it so that it can be read into the computer, though these problems are apparently easily forgotten by those who get their data already on a computer tape ready to analyze. Moreover, it is useless to pretend that these difficulties will "average out" with a large sample, for

the problem is not one of random error. There are of course practical steps one can take to attempt to deal with these problems, and these remedies are part of the lore of competent research. The crucial point, however, is that these steps are not applications of further quantitative techniques but rather depend essentially on qualitative understanding of the particular research situation.

### The Interdependence of Methods

It follows further that qualitative and quantitative approaches are complementary rather than competitive methods. Each supplies a kind of information that is not only different from the other but also essential for interpreting the other. Quantitative data reveal patterns of regularities in situated actions and give essentially distributive information, while qualitative data shed light on the concrete social processes by which particular patterns of situated actions are produced. Consequently, use of a particular method cannot be justified in terms of one's "paradigm" or preference but rather must be based on the nature of the actual research problem at hand.<sup>10</sup> Moreover, in order to avoid major confusion of mere correlation for causal connection and gross specification error in formulating statistical models, it is necessary to know something about the processes by which the regularities are produced that one is studying with quantitative methods. In short, interpretation of quantitative data is informed by the analyst's qualitative understanding of the specific social phenomena under study, just as interpretation of qualitative data is informed by the investigator's knowledge of regular patterns of which the particular events he or she is examining are parts. One can, of course, lose sight of the way interpretations of quantitative data depend on qualitative understanding, and vice versa, and present one's findings as though they were arrived at by uncontaminated use of a single approach, but this kind of self deception is neither helpful nor necessary.

## Objectivity

Finally, we observe that the question of objectivity in social research is not an abstract epistemological one that can be dealt with by mechanically following certain rules of procedure. The idea that objectivity results from adhering to a certain "methodology" derives from confusing philosophy of science with the practice of science: while philosophers may concern themselves with such questions as how we can have objective knowledge and how we can know when we have it, the problems facing empirical researchers are quite different.

Specifically, the question researchers must address whenever they encounter a report of someone else's work that is relevant to their own is whether they can take that work seriously as warranted grounds for further inference and action in their own research. Conversely, in their own research, the question is whether they can expect others to take their work seriously in the same way. The point here is not simply one of whether a particular research report is persuasive, nor is it that agreement among colleagues defines what is "fact". For, acceptance of one's work by others is not a symbolic matter but a practical one that obligates others to take account of it insofar as it is relevant to their research under the threat that their research in turn will not be taken seriously if they ignore it. Thus, if a particular piece of work happens to contain flaws unknown at the time and is accepted as adequate, then sooner or later discrepancies will begin to appear as other researchers cover similar ground, bring more powerful methods to bear, discover new data, attempt replication, and follow up implications of the original work. The situation is complicated, moreover, by the fact that few studies are perfect, and consequently, as Max Weber noted, it is the fate of almost any piece of work on an important topic to be superseded or at least significantly qualified by subsequent research. The fundamental point is that objective knowledge does not consist of propositions with certified truth claims but rather is that which a given scientific or scholarly community holds its members responsible for taking seriously as bases for their own work.

The problem, then, is not how to guarantee taking someone else's work seriously if and only if it is correct, or how to produce work that one can guarantee to others is correct. Rather, it is a question of when researchers will view acceptance of a piece of work as reasonable and failure to do so as irresponsible. Clearly, this is a matter of judgements made on a daily basis by individuals working in a particular field and cannot be reduced to a set of rules.

If we reflect on how researchers in fact procede rather than appeal to textbook idealizations of "scientific method", we find at least two major considerations entering into the assessment of a piece of work. The first is what we may call internal coherence. The question here is whether the data of the study and the methods by which they were gathered are consistent with the interpretation given to them. The issues connected with internal coherence turn out to be largely technical: is the sample appropriate, was the observer in a position to see what was reported, was the arithmetic done correctly in computing the statistical tests, did the researcher understand what was going on so that his or her observations are trustworthy, are logical inferences carried out validly, are there no crucial conceptual ambiguities, and so on. These are matters that standard textbooks on methods tend to deal with, and there is much valuable lore concerning them. However, there is another element, what we will call external coherence: do the findings cohere with what else we know about the phenomenon under investigation from other sources? For example, we might have a very tidy survey analysis that is quite coherent internally but which implies a causal structure flatly contradicted by the historical development of the situation and direct observation. Or we might have an elegant historical argument that simply can't be right because it implies a present state of affairs that isn't so. While these considerations in fact play a prominent role in our judgments concerning the soundness of one another's work, they have no sanctioned place within the standard methodological canons, with the partial exception of the emphasis

placed on problems of specification error in structural equation modeling. Instead, conventional methodological discussion confine themselves largely to problems of internal coherence on the mistaken assumption that correct method alone will guarantee sound results.

Internal coherence, then, has to do with whether the interpretation put forward makes sense in light of the data and methods actually presented in the study. We are taught early the importance of there being no blatant contradictions in the stories we tell, and so we tend to be fairly adept at satisfying the requirement of internal coherence. The requirement of external coherence, in contrast, opens our research up to critique in light of data not our own nor even gathered in our own intellectual tradition. The point, of course, is not that our interpretations have to agree with those of others, but rather that they can accomodate the empirical materials on which other interpretations are based without having to selectively suppress particular items or do too much ad hoc explaining away of awkward facts. Because of the near-exclusive focus methodological discussions have given to problems of internal coherence, the issues surrounding external coherence are often only vaguely appreciated and sometimes neglected entirely.

In summary, what we as researchers are willing to call objective work is that which satisfies the requirements of internal and external coherence insofar as we can tell at the time the work was done. Moreover, these assessments are not all-or-nothing matters: we may have confidence in some aspects of a study but not in others, and consequently we will take those parts more seriously than others. In the end, what we take as objective is that which we treat as adequate grounds for further thought and action under the constraint that, in the long run, our colleagues are looking over our shoulders.

Conclusion

It seems somewhat peculiar to be arguing at length for what may appear obvious to many social scientists: there are no privileged methods; quantitative and qualitative methods each have their appropriate uses; and in the end one cannot do without relying on both kinds of data. The occasion for this essay, then, is the fact that this common-sense view is strongly denied by the major methodological positions in contemporary social science. There is, then, a gap between formal methodological rhetoric and actual research practice. The fundamental assumption of this paper is that what is in need of correction is our formal methodological discourse. The approach we took was to consider in some detail the nature of social phenomena constituted by situated actions, relying heavily on empirical ethnomethodological studies of social interaction. This led to the view that situated action is reflexively related to its social organizational context, which must be taken into account in any methodological reflections. Finally, we drew several methodological conclusions that run directly counter to much of the received tradition in the social science methodological practice. We are, then, in a position to abandon formulations that are unrealistic and irrelevant to actual research and move toward a methodological view that reflects the nature of our subject matter.

NOTES

<sup>+</sup>Revision of remarks to the ZUMA Workshop, July 1981. I am indebted to Don H. Zimmerman and Manfred Kuchler for useful comments, and to the Zentrum für Umfragen, Methoden und Analysen, Federal Republic of Germany, for generous support.

1. For extreme expressions of the quantitative and qualitative positions, respectively, see Gold (1979) and Blumer (1969). It should be noted that qualitative approaches divide into two major categories, historical research and direct observation of social interaction. For a discussion of the latter that is congruent with the ideas developed here, see Zimmerman (1981).

2. Portions of this section and the one following are adapted with permission from Wilson and Zimmerman (1980:54-55, 57-60, 67-71).

3. For a more extended discussion and references, see Wilson and Zimmerman (1980).

4. This formulation raises two important questions: an appearance of circularity and the status of natural science. We cannot take the space to discuss these issues here but instead refer to Wilson and Zimmerman (1980:60-63) and Maynard and Wilson (1980: passim).

5. Bittner (1967a; 1967b); Cicourel (1968;1973); Cicourel, Jennings, Jennings, Leiter, MacKay, Mehan and Roth (1974); Daudistel (1976); Emerson and Pollner (1976); Garfinkel (1967); J. Handel (1972); W. Handel (1972); Hilbert (1978); Jefferson (1972; 1973; 1975; 1979); Jefferson and Schenkein (1977); Leiter (1971); Maynard (1979, forthcoming); Mehan (1971); Pomeranz (1975; 1978); Pollner (1974; 1975, in press); Sacks (1967; 1972a; 1972b; 1973; 1974; 1975); Sacks and Schegloff (1974); Sacks et al. (1974); Sanders (1977); Schegloff (1968; 1972); Schegloff and Sacks (1973); Schenkein (1979); Sudnow (1965; 1969; 1972); Wieder (1974); Williams (1977); Zimmerman (1969; 1970a; 1970b).

6. Note that social category and category-bound activity are not the same as the Lintonian concepts of status and role, but rather are more general, including disapproved behavior customarily seen as associated with the category as well as behavior that is approved.

7. However, Giddens does not attend to the context-free, context-sensitive character of the mechanisms by which social interaction is organized, and consequently his (1979) discussion follows a direction different from that taken here.

8. "Objectified" in a Marxian sense, though not necessarily "alienated" or "reified". See Maynard and Wilson (1980) for further discussion.

9. From time to time the claim is advanced that situated action can be described in a context-free manner, but without exception the claim has had to be abandoned. One example is behaviorist psychology, which has made large claims concerning the context-free description of behavior, but these are slowly disappearing (e.g., Bandura, 1973). Most recently the claim appears in the artificial intelligence literature (see Wilson and Zimmerman, 1980: 60-61 and note 9), an especially prominent example being Hofstadter (1979), who, however, is able to seem to make his case only by stretching the usually sharply defined concepts of isomorphism and chunking into metaphors so as to conceal the reflexivity of meaning.

It should be noted that the context-free, context-sensitive mechanisms by which interaction is organized are not laws of social phenomena in the sense intended here, for these mechanisms do not deal with the content of situated actions. See Wilson and Zimmerman (1980) and Maynard and Wilson (1980) for further discussion.

10. Note that the so-called "interpretive paradigm" does not preclude the use of quantitative methods. See Wilson (1970:706 note 15).

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