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Towards a New Theory of Organizations

Edited by
**John Hassard and
Martin Parker**



Towards a New Theory of Organizations

First published in 1994, the essays collected in this book explore the impact and current status of the ideas put forth in David Silverman's *The Theory of Organizations*, and how they relate to future directions in organization theory. After opening with a chapter by Silverman himself, the subsequent chapters investigate key issues in the study of organizations, including structure and agency, the politics of organization theory, and the meanings of post-positivist organizational analysis. Contemporaneous debates on postmodernism, the emotions, gender and structuration are discussed in the context of the development of organizational theory in the preceding twenty-five years — providing insights into the continuities within organizational theory and provoking thought about future directions.



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Routledge
Taylor & Francis Group

First published in 1994
by Routledge

This edition first published in 2017 by Routledge
2 Park Square, Milton Park, Abingdon, Oxon, OX14 4RN
and by Routledge
711 Third Avenue, New York, NY 10017

Routledge is an imprint of the Taylor & Francis Group, an informa business

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A Library of Congress record exists under LC control number: 9343226

ISBN 13: 978-1-138-23724-7 (hbk)

ISBN 13: 978-1-315-30011-5 (ebk)

ISBN 13: 978-1-138-23731-5 (pbk)

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London and New York

First published 1994

by Routledge

11 New Fetter Lane London EC4P 4EE

Simultaneously published in the USA and Canada

by Routledge

29 West 35th Street, New York NY 10001

© 1994 John Hassard and Martin Parker

Typeset in Times by Intype, London

Printed and bound in Great Britain by

Mackays of Chatham PLC, Chatham, Kent

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British Library Cataloguing in Publication Data

A catalogue record for this book is available from the British Library.

Library of Congress Cataloging in Publication Data

Towards a new theory of organizations / edited by John Hassard and Martin Parker.

p. cm.

Includes bibliographical references and index.

ISBN 0-415-09539-5

1. Organizational sociology. 2. Silverman, David. I. Hassard, John, 1953- . II. Parker, Martin.

HM131.T66 1994

302.3'5--dc20

93-43236

CIP

ISBN 0-415-09539-5

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Introduction

Martin Parker and John Hassard

This volume was conceived following a conference entitled 'Towards A New Theory of Organizations' held at the University of Keele in April 1991. All the contributors to the book presented papers at the conference but they have been revised or re-written for publication. The conference was inspired by the idea of holding a twenty-first anniversary celebration for David Silverman's book *The Theory of Organizations* (1970). In our opinion this was a volume that captured a particular turn in European organizational theory and was deserving of re-evaluation at a time when organizational theory seems to be searching for new ways of relating to its subject matter. In recent years the currents of cultural theory, postmodernism, gender, critical theory, literary theory and so on have provoked a number of attempts to re-theorize organization and organizing (Morgan 1986, Hearn *et al.* 1989, Turner 1989, Hassard and Pym 1990, Alvesson and Willmott 1992, Hassard and Parker 1993). This collection attempts to complement that work by using David Silverman's writing as a starting point from which to consider the theoretical and methodological issues he first raised over twenty years ago.

We begin with a revised version of the keynote address that David Silverman gave at the conference in which he outlined his understanding of how *The Theory of Organizations* (henceforth *TO*) became a key text in the development of post-positivist organization theory. He charts the move from Parsonian functionalism and abstracted empiricism to a theory of organizing that was informed by the notion of social action. Rather than seeing action as an epiphenomenon of system needs, Silverman saw social actors as reflexive about their strategies to achieve their own goals within the context of a formal organization. Effec-

tively this meant that organizational actors were not necessarily pursuing the ends and means officially legitimated by the formal organization. Silverman notes that he might now be accused of downplaying structural constraint in favour of a kind of romanticized subjectivism. Furthermore, this subjectivism was underpinned by a combination of Weber, Schutz and Garfinkel which, though suggestive, did not provide a clear methodological guide for exposing and accounting for social action.

Silverman then proceeds to give an account of his more recent work in medical sociology and shows how it amends some of the deficiencies in *TO*, particularly with reference to facilitative formulations of power in organized settings. He shows, using two research studies, that the power to speak or not to speak is an outcome of a particular interaction. There is no sense in which 'democratic decision making' is a situation which is not brought about by and through power relations. These power relations can be interactionally constituted but are also shaped by broader institutional constraints such as finance, media representation and competition between the organized professions. Silverman then moves on to consider some of the methodological strategies that might link actors' talk with structural constraint. In broad terms he suggests that both subjectivist and objectivist strategies tend to deflect attention away from the local organization of the phenomenon under scrutiny. This happens either by focusing on supposed causes and consequences (the objectivist) or the actors' perceptions and responses (subjectivist). Silverman suggests that the object/subject polarity is hence a false one for social science; instead we should look for practices in order to see what actors do, not simply what we/they say they do.

The chapter then proceeds to analyse a short section of talk to illustrate the kind of practices he wishes us to focus upon and the methodological strategies that might be employed. Silverman notes the problems of generalizability but suggests that the method of analytic induction, a form of grounded middle-range theorizing, allows for hypotheses to be generated and tested on naturally occurring data. These hypotheses are not solely based on the *a priori* assumptions of the researcher or those of the researched and can therefore be used to guide practical applications of the research in terms of guides to social policy and practice. Silverman concludes that the programme he began in *TO* is still intact, though modified towards an ethnomethodological

ethnography and he concludes by cautioning us not to adopt any ready-made labels. Methodology and theory are guides, not ready made rule books, and need to be seen in a far more pragmatic light.

The following chapter by Stewart Clegg provides a reading of Silverman that locates him within the social action framework originating in Talcott Parsons' early work and finding its clearest exposition in Berger and Luckmann's social constructionism. He attempts to explain the reception of the text with reference to the intellectual themes that characterized organizations as institutionalizing processes on both sides of the Atlantic in the late 1960s and early 1970s. However, against both Silverman's humanist assumption that actors are necessarily individuals and a Marxist insistence that agency is an epiphenomenon of production, Clegg suggests that agencies should instead be conceptualized as possibilities within a framework of power. Organizations then become the accomplishment of agency by many subjectivities and hence instruments of enablement and domination simultaneously. Various strategies are used to facilitate agency, including the articulation of common interests and the establishment of (human and non-human) power relations. These strategies are powerful but always contested and necessarily changing across space and time, an analysis which Clegg draws out of a Foucauldian or Weberian attempt to conceptualize organizational power as a form of differentiated social practice or rationality. The author concludes, prefiguring the subsequent chapter, that any sociology of organizations is hence a social and cultural theory – organizations must be seen as the sites of the production and reproduction of power and knowledge claims.

Clegg's evaluation of the historical and theoretical importance of Silverman leads into the next section – four chapters primarily concerned with agents and/or structures and the relation between the two concepts. Chapter 3, Richard Whittington's 'Sociological Pluralism, Institutions and Managerial Agency' suggests that the insights of Giddens' structuration theory have yet to be fully realized in organizational analysis. While acknowledging the influence of the US Carnegie School on formulations of agency in organizational analysis he argues that this group of theorists lacked an emphasis on structural constraint which made them ahistorical and over-individualistic. A more promising opening is found in Silverman's *TO*, an account which potentially linked

meaning and structure but which, Whittington claims, was not developed in the author's later work – though Silverman would clearly contest this interpretation. Whittington then provides an account of structuration theory, stressing the way in which states, organizations, patterns of association and so on form rich enabling and constraining patterns through which agents structure their worlds and are reciprocally structured by them.

Whittington argues that organizations themselves can act as agents, breaking or modifying the structural pressures that they structure in the polity, economy, society and culture. He suggests that empirically this is reflected in several studies that move towards a new 'institutionalism' (a term Clegg used in the previous chapter), but without any particular theoretical clarity to bind them together. Operationalizing Giddens provides a clearer conceptual foundation and also means that actor or structure bias can be avoided by envisaging organizations as sites of multiple cross-cutting rules and resources which actors can draw upon to further their individual or collective projects. Whittington concludes that questions of organizational strategy and agency must hence be clearly articulated within a framework that recognizes other senses of social identity, community, class, profession, gender, nation and so on. In other words – organizational action reflects social structure.

Focusing more directly on the 'micro' level, Chapter 4 by Stephen Fineman takes as its starting point the recognition that organizations do not operate 'without hatred or passion' but instead are sites for the deployment and production of human emotions and meanings. Yet, on a managerial level, while the Human Relations tradition has now developed into full blown Human Resource Management and organizational culturalism, they still rely upon the masculinist and managerialist assumptions that a 'rational' approach to studying and controlling emotions is necessary. Fineman suggests an alternative view based on post-Silverman actor-level social constructionism in which organizations are viewed as inter-subjective processes. Emotion can then be formulated as an element in the reflexive 'glue' that enables actors to formulate their subjectivity in orientation to some and against others. It is also a strategy that we use to present a desired representation of self to others with different power to impose their emotions on us and to manage our own emotional labour. The author concludes by suggesting that an actor-focused

approach to the organization of emotion is epistemologically and politically a vital step for organization theory – though it should not be seen as a panacea for humanizing capitalism.

‘Bringing Agency (Back) into Organizational Analysis’ by Hugh Willmott is an attempt to (re)assert the importance of studying the ‘open, reflective and intentional’ quality of human beings within organizational theory. Against what might be described as the ‘bourgeois humanism’ of many of the other authors (Clegg perhaps excepted), Willmott seeks to use a combination of Heideggerian existentialism and Foucauldian poststructuralism to demonstrate that the agent need not be conceptualized as a self-conscious, sovereign, unified subject. In developing his analysis he subjects Silverman, Burawoy and Clegg to critical scrutiny and suggests that they all, in different ways, fail to theorize the historical development of modern senses of what it means to be a person. The underdetermination of identity in modern societies leads to pressures to adopt a socially valued identity – citizens must make something of themselves, must use their ‘freedom’ within an ever-changing set of social conditions. Autonomy is predicated upon insecurity and vice versa. Like Fineman, Willmott suggests that within organizations the management of the person is hence increasingly internalized as Taylorism gives way to culturalism. He goes on to propose that radical postmodern understandings of the self suggest the possibility of a form of de-subjection that subverts the subject/object, mind/body dichotomies constituted in modernist and hypermodernist thought. Moving beyond modernity means moving beyond both Foucault’s nihilism and Habermasian critical theory and towards the development of new forms of subjectivity that may be both philosophically and politically more satisfactory.

David Knights’ and Glenn Morgan’s ‘Organization Theory, Consumption and the Service Sector’ (Chapter 6) begins by suggesting that the strength of organization theory is that it is ‘parasitic’ on other disciplines. One of the main potentialities of this situation is ‘intellectual anarchism’ and the authors suggest this is to be strongly encouraged. Following this line of analysis they construct a Foucauldian argument regarding the historical construction of subjectivity within and without organizations which parallels that of Willmott and draws on a similar range of literature. Through the use of evidence from one case study the authors then show how market changes in a particular sector resulted in

new attempts to manage the subjectivity and emotion of workers and customers in ways which were centrally related to class and, in particular, gender identities. In theorizing the production of consumption-related identities Knights and Morgan move from political economy to gender subjectivity and convincingly demonstrate the dialectical relationship between the two. In addition they achieve this in a way that demonstrates how important the specificity of a particular organization is in mediating structures and agents. Whether they escape the humanism that Willmott identifies and how closely they follow a version of structurationist analysis is for the reader to judge.

The politics of organization theory are the topic of the next three chapters which vary widely in their suggested programme for the development of organization theory. Humanism is defended in Chapter 7 by Jean-François Chanlat, which provides a French North American perspective on organizational theory. Chanlat argues that the technocratic and functionalist character of the discipline is under increasing challenge from a holistic and complex view of people within and without organizations. Our use of language in the creation of organization is therefore central to understanding institutions as symbolic processes. A theory of organizations that takes symbolism as its starting point therefore functions as an implicit critique of the 'universe without hope' which has resulted from the dominance of instrumental and scientific theories and practices of organization. In this project, time, space, subjectivity and meaning become key categories for the practice of an inter-disciplinary anthropology of organizations which covers the dialectical relationship between individual/interactional and social/global dimensions. Chanlat concludes with the hope that his manifesto will provide a new basis for allowing the 'renaissance of human life in organizations'.

The next chapter, by Lex Donaldson, presents a controversial assessment of the influence of *TO* on British organization theory and contributes to a debate on the status of organization theory which has been hotly contested since the publication of his *In Defence of Organization Theory* (1985). The core of his argument is that Silverman and his disciples have contributed towards the critique of positivist organization theory because of a mistaken assumption that positivism results in an unquestioning defence of oppressive organizations. Donaldson argues that this is an unfortunate path for organization theory to follow because

organizations are not simply constellations of actors and meanings. He also notes that no coherent body of empirical research has followed from this line of enquiry. Citing the Aston studies as a founding example, he argues that positivism has insights which have simply been eclipsed in the name of a critique which has no clear means or ends. Putting forward a clearly structuralist line he asserts that what he terms 'illiberal' and subjectivist organization theory is out of touch with the reality of the modern liberal democratic revolution. In short, Marxism has failed but many organization theorists do not like to admit it. Instead he suggests an approach which relies on the critique of methodological individualism inherent in functionalism and acknowledges the inherent superiority of liberal market mechanisms of governance. Whilst many of the other contributors to this volume would disagree with Donaldson's diagnosis, it serves as a valuable counterpoint which clarifies the, often hidden, assumptions of much contemporary sociology of organizations.

Tony Watson's chapter is similarly impatient with theoretical approaches that fail to find application but diverges significantly from Donaldson in epistemology and politics. Watson takes as his starting point the necessity to bridge the gap between the worlds of practising managers and theorizing academics. With particular regard to the latter he suggests that much of organization theory appears to be written entirely for other academics with no attempt to engage with the problems of actually existing organizations. In this regard, Watson acknowledges the importance of *TO* for setting the conditions for the development of a non-positivist and non-managerialist organization theory. The latter position is defined as a non-partisan attempt to describe actors' definitions in a way that could be used by management or workers to further the sectional interests of either but that privileges the experience of neither. Watson argues that the theories or descriptions consequent from such an approach are hence not intrinsically political, but the uses to which they may be put are. To illustrate, he then offers an account of what may be described as a piece of interpretive action research which relies on the creative appropriation of sociological theory to explain the problems organizational actors are engaged in. Watson's use of the term liberalism is more interpretive than Donaldson's but both appear to share a formulation of organization theory making

existing capitalist organizations work better for their stakeholders, even if they differ as to exactly who these stakeholders might be.

The final three chapters of the volume turn to methodological issues, most particularly the anti-positivist methods given such prominence by *TO*. The chapter by Barbara Czarniawska-Joerges looks at gender, power and strategies of interpretation in the context of some of David Silverman's later work. Her interruptive interpretation (a term synthesized from Silverman and Torode) relies on a critically hermeneutic understanding of language as referring to other language and not some 'truth' about the world. Using stories about power elicited from students she builds an account of how gender and power are articulated in talk about organizations. Yet her account is not merely about that talk, it is also about the other discourses that are assumed or missing in the context of a particular organization and a particular culture – Poland, Finland and Italy – and the general context of theories of organization. Czarniawska-Joerges concludes by suggesting that organizational theory needs gender theory, not only to right a balance but also to upset our limited conceptions of what organizations are and might be. Neither interpretation nor organization are neutral, both are gendered and historically mediated.

In Chapter 11 John Law begins by arguing that much of the 'un-learning' about organization that has taken place since the 1970s has resulted in what he terms a 'bonfire of the certainties'. He suggests that a credible response is not to rescue modernism but, like Willmott, to celebrate the postmodern multiplicity of de-centred narratives. This way we can seek to uncover what has been said as well as what has been suppressed – the Other. Law does this by drawing upon his background in the social study of science as well as a case study from which he draws four narratives which were characteristic of one particular organization. These stories generated social and technical arrangements which in turn reflexively reinforced narratives of a particular kind. However, these stories are not isolated – organization is made and remade through the creative interaction of different narratives. There is no centre, or final truth, to this process but a continually shifting terrain in which the necessary incompleteness of one narrative is partially repaired by the incompleteness of another and so on. Law concludes by provocatively suggesting that the outcome of this kind of approach is that organizational analysis might begin to do without the teleological assumptions of strategy.

If we no longer assume that actors or analysts necessarily know where they are going then our stories about social organization must become very different from what they are at present.

The final chapter, 'Recreating Common Ground' by Stephen Ackroyd, is an attempt to show the limitations of theory and the importance of grounded empirical research for organizational analysis. It starts with a review of the recent history of the field which Ackroyd interprets as the breakdown of a broad consensus about what organizations were and how they were to be studied. He argues that the fragmentation of the field was largely a result of a theoretical backlash against positivism and a privileging of meta-theoretical paradigm construction over the 'underlabourer' activity of empirical data gathering – a position, he argues, that Silverman implicitly encouraged. However, against a 'random mosaic' response to paradigm-based theory Ackroyd proposes four broad areas which he feels might sensitize us to new questions about organization whilst not losing the broad contours of the organizational sociology project. He suggests these areas as problematics, not theories, and stresses the importance of empirical studies to demonstrate their possible fruitfulness. Like Silverman at the beginning of the book, Ackroyd insists that theory will not solve problems once and for all, but is instead a guide to the problems and politics of the contemporary. Whilst this disqualifies us from asking for final truths it does not prevent us from being involved in Giddens' 'double hermeneutic' – a reflexivity about who we are producing ideas for and why. In that sense Ackroyd asks for more emphasis on empirical data collection and analysis and not theory construction for its own sake – a middle-range grounded theory that provides resources for the problems the analysts and their audience define as important.

There is no sense in which these twelve responses to the *New Theory of Organizations* title can be described as a coherent programme. Certain theoretical resources that were not current at the time of *TO* find application, postmodernism and structuration for example, but in no sense do the contributors gather under the same banner. That being said, three key debates seem to be rumbling on without any particular consensus on their resolution, and hence underpin much of the analysis. The first is the relation between actors and structures. In recent years the terms of this debate have changed, actors are not necessarily individuals and structures are things that are performed, but the

problem of exactly how we articulate the relation between subjects and objects clearly remains contested. The second is the question of who organization theory is for. Putting the problem simply, if it is to be useful to managers and/or workers, how can we make them listen? If it is to be critical, what does that mean in practice? The gap between Donaldson and Willmott, for example, suggests that these are issues that will still be around in another twenty-one years' time. The final issue is one of method. Even if the centrality of some form of interpretive method is accepted, there are many diverse forms this could take. While Silverman's ethnomethodological ethnography, Knights' and Morgan's Foucauldian analysis, Fineman's social constructionism, Czarniawska-Joerges' critical hermeneutic and Law's postmodern explication of narratives share a commitment to understanding meanings, they all rely on very different epistemological and ontological assumptions in order to operationalize their strategies. Clearly, interpretation is not just one thing; even if we like the sound of an action frame of reference we still need to decide exactly what it is and what to do with it.

To conclude, this volume reflects the continuing interest in the sociology of organizations. That there are so many unanswered questions, and so many new ways of posing the old ones, is not in any sense a cause for intellectual despair. A balanced consideration of the importance of *The Theory of Organizations* means acknowledging both its strengths and its shortcomings, both what it said and what it left unsaid. That so many organization theorists still treat it as a seminal text, whatever their theoretical orientation, is reason enough for all this collection and a tribute to the work of David Silverman himself. Finally, the editors would like to thank Ian Atkin, Louise Mc Ardle and Penny Tyldesley who all enabled the original conference to run smoothly. Our gratitude also to Debbie Pointon for typing the bibliography and Rosemary Nixon and Laura Large at Routledge for their editorial advice and support.



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Chapter 1

On throwing away ladders: re-writing the theory of organizations¹

David Silverman

One of the sociological processes that is of some interest to me is the one that is known as the re-writing of history (Garfinkel 1967, Silverman 1975). And I suppose that is what I am doing in this chapter, as I look back on a book that was written twenty-two years ago and try to make sense of my later work in terms of it. Now if I were to do that in front of a job selection committee, I'd have to pretend that everything I've done since then logically followed from what I wrote over two decades ago. Fortunately, to what is, I hope, a friendly readership, I can be more honest and point out the ways in which my work has diverged over that period.

I want to begin by outlining where I started in organizational analysis more than twenty years ago. I am somewhat reluctant to do this for three reasons. First, it reminds me of how old I am. For instance, many of my present students were not even born when that book appeared. Second, I presume that most readers are already familiar with the arguments presented in *The Theory of Organizations* (henceforth *TO*). Third, as post-moderns, accustomed to 'the death of the author' (Barthes 1977), the autobiographical reflections of an ageing writer are less a guide to the inner mechanisms of 'his' work than yet another text awaiting deconstruction.

Despite this reluctance, I offer these reflections in order to set up the tale that follows. Like all tales it is deeply moral and invites interruption (Silverman and Torode 1980).

Back in the late 1960s, the dominant theoretical tradition was 'Functionalism', largely imported from the United States through the work of Talcott Parsons. The prevailing framework asked us to see social action in the context of normative and system

constraints and adopted an organic analogy in its explanations of social order and change.

The other stream which was dominant in social thought in the late 1960s was what the American sociologist C. Wright Mills, quite aptly, I think, called 'Abstracted Empiricism' (Mills 1953). I think what he meant by that was work that wasn't Functionalism or Marxism or any other kind of 'ism' but that conducted research simply by counting what could be counted and sometimes what couldn't be counted. As a graduate student in the United States in the 1960s, I saw how such empiricism was expressed in survey research studies which included almost any variable as long as it could be operationally defined and measured. Like a self-fulfilling prophecy, statistically significant correlations could always be attained provided you included enough variables. And theorizing became largely *ex post facto* as you speculated about the reasons for such correlations.

So that, more or less, was where I saw Anglo-American sociology as it was in the late 1960s. I was dissatisfied. I was trying to develop a theoretical, non-functionalist basis for research. In particular, what I wanted to argue was that we could look at organizations analytically without assuming that what happened in social life arose from some supposed needs of the social system.

What I tried to do in *TO* was to develop a version of organizations, not as some clear, fixed reality, but as a set of legitimated rhetorics. For instance, there seemed little doubt that people often talk about the goals of the organization when they are pursuing their own sectional interests. One clever thing to do, when you want to achieve something politically, is to pretend that it is in the general interest: and one way to do that is to say 'what I'm doing here serves the goal of the organization'. Hence the goals of the organization work not to determine people's behaviour but to serve as a way of justifying or legitimating it. Later these ideas were to get a more sophisticated treatment in work on organizational charters (e.g. Dingwall and Strong 1985).

Out of this, I came to develop what *TO* called 'A Social Action Model'. This model was not meant as a theory of how things happened but more as a set of questions to be asked if you were interested in how organizations function. And the questions I suggested we should ask, you may recall, were as follows. First, who are the principal actors involved in the setting? Second, what are the goals they are trying to achieve, perhaps using

the organization's charter as a legitimation? Third, what kind of involvement or attachment do they bring to the organization: what kind of commitment or interest do they have there? Fourth, what sort of strategies and tactics do they use to achieve their goals or ends in the light of their attachments to the organization? Do they engage in open conflict or do they work by stealth in the proverbial smoke-filled room? And, finally, what are the consequences of their actions for each other and for the pattern of interaction when actors pursue particular goals, using particular strategies in particular situations?

Now this seemed to me an important re-balancing of work on organizations away from assuming the determining character of some sort of more or less biological set of needs. To look much more closely at the kinds of political strategies used in organizations and the ends that people were actually pursuing rather than the officially defined ends. And, as you know, I used as a model for this sort of approach an early book by Alvin Gouldner, *Patterns of Industrial Bureaucracy* (1954).

LOOKING BACK ON *TO*: REFLECTIONS AND MODIFICATIONS

Trying to critique a position from a different historical context is an example of the worst kind of history of ideas. Nevertheless, albeit reluctantly, it may be helpful to the reader (as well as a tactical pre-emptive strike from my point of view), if I briefly outline how I now see the main limitations of *TO*.

There was an obvious danger in looking at organizations purely from the point of view of the definitions of actors. Clearly, as Gouldner's work had shown, historical and structural factors are important in understanding activity in organizations. There was a danger particularly in terms of how the approach could be seen as atomistic or individualistic.

In a way, this was the last thing I intended. The emphasis was always on social action. You may remember that Weber is very clear, in his definition of social action, that he is not referring to individual states of mind but to the way in which action is defined in relationship to other actors and their intentions. However, there was a danger that, in reacting against structural constraints as explanations, I'd pushed the argument too far in an apparently individualistic, anti-structural direction.

The second kind of related problem I have called 'romanticism' (Silverman 1989a and 1989b). This is difficult to convey briefly but I will try. I worry about those approaches in sociology which imply that feelings or experiences are the most authentic data about the social world. I worry about that because, as we are all aware, experience is never unmediated but is always structured by particular cultures and by settings. Often, perhaps when we think we are expressing our true selves, we are merely reproducing a script that everybody works to.

Let me give you a brief example of this. The mass media in most societies seem to be fascinated by accidents and disasters. Indeed, I believe there is (or used to be) an Australian TV station whose news consisted entirely of reports of the latest road accidents. In such situations, what usually follows is that there is a media person present with a microphone to interview people who have been involved, or sometimes their relatives. They haven't yet interviewed the pets of people who have been involved in disasters – but that's coming I promise you.

Now, if you look at such interviews, as I've started to do, you can see something very curious happening. The usual question asked is 'How do you feel?' In response, people produce accounts which point to heroes, villains and so on. Their descriptions and explanations are amazingly similar. For instance, if someone has died in a tragedy, we look for the heroic qualities of that person. Maybe they weren't very nice after all, but we can count on people producing an heroic account which works as a kind of eulogy.

In a similar way, accounts of sporting or academic success or failure reproduce predictable forms (See Mulkay 1984, Emmison 1988). Only occasionally do sportsmen and women resist their depiction as heroes or villains. For instance, the British decathlete, Daley Thompson, was well known for nonplussing the media by producing the 'wrong' account – claiming he was 'over the moon' when he had failed and 'sick as a parrot' when he had won. Again, in this vein, a British boxer was recently termed 'arrogant' by a reporter because he had refused to engage in the usual pre-fight slanging-match with his next opponent. This, of course, is the irony. The media aim to deliver us immediate 'personal' experience, yet what they (we) want is simply repetition of familiar tales. Perhaps this is part of the post-modern condition. So

we feel people are at their most authentic when they are, in effect, reproducing a cultural script.

I worry about approaches in sociology which to some extent take on board the media's approach and imply that people's experiences are individually meaningful and authentic. Because then we have to ask a further question: from where do these experiences derive? If you can see uniformity in even the most intimate kind of accounts, I think there we see a job for the sociologist or the anthropologist. I will return shortly to the methodological implications of this phenomenon.

The third kind of problem with *TO* is perhaps that its theoretical basis was a bit unclear. I refer many times to Weber. I also refer to the phenomenologist Alfred Schutz and, in an unexplained way, to ethnomethodology and Garfinkel. It wasn't altogether clear where I was coming from in that book. The reason, in short, was that in 1969 I didn't know where I was coming from or at least where I was going to. It was a very early book and I had yet to formulate very clearly the work that I was going to be centrally concerned with in future.

A fourth problem is that, despite a last chapter which touches upon the practicalities of researching 'meanings', *TO* lacks any clear methodology. It stresses, time and time again, that what we should look at are actors' meanings. But then how do you get at an actor's meaning? After all, as Weber himself wrote, when people engage in action they are more or less unconscious of its subjective meaning (Weber 1949). If that is the case, how on earth do you understand its subjective meaning? Do you put a questionnaire under people's noses? I don't think so, at least for most sociologically interesting purposes. So how do you research 'meanings'? I don't really answer that question in the *TO*, even if I at least pose it.

And now a fifth problem – the nature of power in organizations. Earlier on, when I talked about my lack of attention to structure, I seemed to be criticizing *TO* by assuming that power is given and resides in structures. From where we are now, I wouldn't necessarily see all power as residing in structures in that sort of way – for instance, in Weber's terms, as arising within a particular historical context of actors being able to achieve their ends despite resistance by other actors. Increasingly, we see power now as involving not simply coercion (the most obvious sort of situation) but enablement (perhaps this is where Parsons and

Foucault come together?). Since Foucault, we have been encouraged to see the workings of power not simply in situations where we're silenced but also in situations where we are encouraged or incited to speak. I will return to this below with reference to my own more recent work.

I have now attempted a pre-emptive strike by seeking to anticipate some of my critics. Of course, this attempt is doomed to fail. First, my critics are bound to find other lines of attack. Second, remember that the author of *TO* is dead – only analytically speaking, I hope.

REPLIES AND RESPONSES

Let me now go on to show how I have tried to deal with some of these issues in later work. First, I tried to give a sounder theoretical base, in a paper entitled 'Accounts of Organizations – Organizational Structures and the Accounting Process' (Silverman 1975). Here I examine the large number of studies available by 1975 which looked at organizational activities from the point of view of how the actors or members display to one another the grounded rationality of their action. What these studies avoid is the treatment of people as what Garfinkel (1967) calls 'cultural dopes' pre-programmed by rules. I show the studies that have been done in courts, in police work, in social work, in juries and in selection interviews and promotion panels to demonstrate the practical rationality of actors in particular settings. This kind of work was later dubbed 'ethnomethodological ethnography' (Dingwall 1981). A key early text is Bittner (1967).

Now I move on to power. I've said already that I became increasingly interested in power as it works not always just to silence people but sometimes to give them a voice. My recent studies in medical consultations have more than ever convinced me of the force of this argument about power. For instance, in one particular paediatric setting, I found consultations to be longer and apparently more democratic than elsewhere. A view of the patient in a family context was encouraged and parents were given every opportunity to voice their concerns and to participate in decision-making. Yet this served to reinforce rather than to challenge the medical policy in the unit concerned.

The children concerned had Down's Syndrome (see Silverman 1981 and Silverman 1987) and the study (henceforth DS) emerged

as an off-shoot of research on doctor-patient-parent encounters in a prestigious Paediatric Cardiology Unit (PCU). It was discovered that consultations with the parents of DS children were longer and more 'open-ended' than others. For instance, even at a first visit, parents were given the right to choose whether their child should have the routine diagnostic test (at that time cardiac catheterization) which might set them on the path to corrective surgery. This was unlike other consultations where physicians would typically simply tell parents what was proposed at that stage (although the rider 'if you agree' was routinely added, no such parent was seen to dissent from the treatment plan). Moreover, particularly when dealing with asymptomatic children with signs of cardiac disease, physicians would usually spend time emphasizing the long-term clinical outlook with and without treatment. Although the clinical situation and outlook of DS children was clearly, indeed starkly, presented to their parents, doctors seemed to prefer to dwell upon the prospects of a happy family life which DS children could have *without diagnostic tests and surgery*.

The outcome of this way of presenting the situation was that, almost without exception, the parents of DS children opted against the cardiac catheterization that other children with the same clinical signs but without DS would routinely be given with a view to future surgery. Discussions with a senior physician at the PCU revealed that this was in accord with an unwritten policy that major cardiac surgery was not the favoured option with DS children. This was supported by evidence about the greater risks of surgery on such children some of which related to the chest infections to which they were prone. It must also be added, however, that a decision not to put them on a path towards surgery at an early stage meant that several DS children seen at the PCU already had developed severe lung damage that made surgery particularly dangerous.

The research thus discovered the mechanics whereby a particular medical policy was enacted. The availability of tape-recordings of large numbers of consultations, together with a research method that sought to develop hypotheses inductively, meant that we were able to discover a phenomenon for which we had not originally been looking. More importantly, from the point of view of our present concerns, the research underlined how power can work just as much by encouraging people to speak as by silencing

them. 'Democratic' decision-making or 'whole-patient medicine' are thus revealed as discourses with no intrinsic meaning. Instead, their consequences depend upon their deployment and articulation in particular contexts.

COUNSELLING AND ORGANIZATIONAL STRUCTURES

Finally, I want to deal briefly with the issue of structure. Currently, I'm engaged in two funded studies of AIDS counselling. Most of the work is being conducted with audio tapes and video tapes of actual counselling episodes around the antibody test and for seropositive people. Now it would be tempting, especially given some of my earlier work, to focus simply on a very close description of what professionals and clients are saying to one another. Indeed this is what I'm largely doing. So far my co-workers and I have published papers on how doctors and AIDS patients discuss prescribing and using the palliative drug AZT (Silverman 1989c), how counsellors and their clients talk about sexual activities (Silverman and Perakyla 1990, Silverman and Bor 1991) and fears about the future (Perakyla and Bor 1990), how people describe each other's experience to counsellors (Perakyla and Silverman 1991a), how advice is given and received (Silverman, Perakyla and Bor 1992) and the communication formats which are maintained in counselling (Perakyla and Silverman 1991).

The research was partially funded by the Health Education Authority. Through workshops and training programmes, it is starting to have a practical impact on how counsellors do their work. However, it is important to realize the limitations of work that purely deals with techniques of communication. Some attention needs to be paid to the broader context in which these consultations are taking place. For instance, if resource or organizational constraints mean that counsellors only have ten minutes for each session with a client, then some of the communication problems highlighted by the research may not be able to be resolved simply by modifying how professionals communicate with clients.

One can think of a number of institutional factors that are relevant here. First, there are the mass media. In Britain, health promotion campaigns have been developed in recent years which have informed people about the risks of contracting the virus. However, the early campaigns which used images of gravestones

may well have been counter-productive. All research indicates that if you present such an image of doom, people respond by sweeping the issue under the carpet.

Another sort of context is the way in which AIDS has suddenly opened up a whole set of opportunities and resources to different health care professionals. An extreme example of this is that one fairly junior hospital doctor in London suddenly found himself a media celebrity, chased after by TV crews to give interviews. Moreover, he was working in genito-urinary medicine, an area that in the past had been accorded a relatively low-status. Many doctors had gone into it, I suspect, to get promoted more quickly. Suddenly, this became high-status medicine. Suddenly, resources were being made available not just for treatment but for research. Suddenly, people were becoming media celebrities and flying around the world (as indeed now I am) to attend conferences and to give workshops).

Finally, one has to look at the way in which this new situation has impacted on the different professions and on the relationship between these different professions. This has involved such diverse occupational groups as doctors, nurses, social workers and even stockbrokers (think of the interest in the City about which pharmaceutical company is going to produce a successful anti-viral therapy – is it any surprise that the annual International AIDS Conference is attended by hordes of gentlemen in pin-stripe suits?).

One of the issues in Britain revolves around the question of who should be doing the counselling: to take one possibility should it be nurses or should it be social workers? And different things follow from which professional group is doing the counselling. Nurses, after all, are working within a clinical framework in which they are part of the hospital hierarchy and accountable to doctors. Social workers are working for local authorities, independent of the hospital framework, and are, therefore, less in the control of doctors. Understandably then, most doctors are in favour of the counselling being done by nurses. The reasons they will produce are not necessarily the ones I have just given. They will say, and there is some argument for this, that nurses have the clinical background which is very important in counselling about HIV and AIDS. And also nurses experience in working in sexually transmitted disease clinics means that they have a better understanding of the importance of one-to-one

confidentiality whereas social workers usually work through case conferences where details are revealed to colleagues.

Different stories are also produced by nurses and social workers. If you speak to nurse health advisors or counsellors, they will say that social workers are mainly needed to obtain social security benefits for patients. Yet they tend to neglect this work in favour of counselling. If you go to the social workers, you hear a different story. They will insist upon their training in communication issues and compare it favourably to the training of most nurse-counsellors. While they are prepared to follow up such welfare matters as, say, housing benefits for AIDS patients, social workers also want to be responsible for counselling.

So this is some of the rhetoric which defines the politics of the situation. What it reveals is inter-professional conflict with real consequences. For instance, in one hospital, seropositive people are counselled by nurses in the role of health advisors. Yet, should they be admitted as in-patients for AIDS-related illnesses, they are re-allocated to social workers. At this crucial point in their illness-career, they thus have to turn to another person for counselling. The sad irony is that here is a pressing social problem that needs to be solved. But, because it's recognized as a pressing social problem, it generates resources. In turn, these resources generate squabbles among people who have access to them (see Silverman 1990).

I return to my point about the need to locate 'communication problems' in a broader structural context. In this case, we are dealing with inter-professional conflict with clearly political undertones. At other times, organizational routines, without overt political significance, create their own pressures. For instance, counselling prior to the HIV-antibody test occurs within at least two major constraints. First, it is dependent upon patient-flow. This produces sudden periods of demand (usually immediately after the latest media advertising campaigns), interspersed with relatively quiet periods. The uneven flow of patients makes it difficult to design an effective use of clinic resources.

The second problem is that pre-test counselling is expected to cover a huge number of topics – from the difference between HIV and AIDS, to the meaning of positive and negative test results, to issues of insurance-cover and confidentiality and to 'safer sex'. The consequence is that, in most English testing-sites, such counselling consists of largely stereotyped 'information

packages' and is completed within fifteen minutes (see Perakyla and Silverman 1991a). The lack of patient uptake (Silverman, Perakyla and Bor 1992) suggests that this is not very useful for clients. It is certainly a dull and repetitive task for the counsellors.

Our research has much to say about how counsellors can organize their talk in order to maximize patient uptake (Silverman, Perakyla and Bor 1992). However, without organizational change, the impact of such communication techniques alone might be minimal or even harmful. For instance, encouraging patient uptake will usually involve longer counselling sessions. Experienced counsellors will tell you that if they take so long with one client that the waiting period for others increases, some clients will simply walk out – and hence may continue their risky behaviour without learning their HIV status.

Three simple organization changes might allow counsellors to adopt new, more effective, but time-consuming styles of communication. First, central government could keep testing centres better informed of new media AIDS campaigns so that local structures can be more responsive to sudden surges of client demand. Second, testing centres might use an appointment system rather than seeing clients on a walk-in basis. Third, certain of the topics now cursorily covered in pre-test counselling might be just as well addressed by leaflets or, still better, by videos shown to patients while they are waiting to see a counsellor. Counselling might then look more like a service encounter, where the client is encouraged to ask questions of the professional, rather than a sermon.

Having emphasized the importance of organizational structures in counselling, I have not meant to downplay the relevance of analysing communication structure in themselves. I will now turn, therefore, to the logic of working with naturally-occurring data. Using my research on counselling (henceforth HC), I will emphasize the methodological issues about which *TO* is noticeably silent.

COUNSELLING AND THE DISAPPEARING PHENOMENON

Like the DS study, HC is based on audio tapes of consultations between patients and professionals which are then carefully transcribed and analyzed (there are video tapes available at one of

the centres being studied). Although the emphasis is on such 'naturally-occurring' data, DS also used home interviews with parents of the children seen at the hospitals, while in HC we are interviewing professionals to discover how care is organized at each centre, philosophies of care, patient turnover, etc. This has generated information about the structural context of counselling which I discussed above.

My mandate in the HC study was to examine the health education consequences of counselling particularly from the point of view of 'safer sex'. Given the nature of the problem, why study it this way? A descriptive survey of HIV counselling in the UK had been carried out earlier (Chester 1987). Why not follow that up with the development and test of clear hypotheses about the effectiveness of different forms of counselling? A brief examination of the limits of different styles of research should clarify the potential strengths of the field research methodology that I adopted.

First, let us examine the potential of a mainly quantitative, hypothesis testing research design. This could proceed, for instance, by the definition of different methods of counselling, based on Chester's earlier descriptive work, followed by an examination of their effectiveness. Two formats might be used:

- 1 An experimental design where we offer volunteers different forms of counselling and then interview them subsequently about their uptake of information (followed up some weeks later with a further interview about the effects, if any, on their behaviour).
- 2 A non-experimental design where existing counselling procedures are evaluated by a cohort of patients (with a later follow-up).

The advantage of such research designs is that they permit large-scale studies which generate hard data apparently based on unequivocal measures. However, a number of difficulties present themselves:

- (a) How relevant is information-uptake? Good information can coincide with the continuance of 'risky' practices (for instance, think of cigarette smoking and health).
- (b) How seriously are we to take patients' accounts of changes in their behaviour? Will they not tend to tell interviewers

what they think they will want to hear? May there not be a 'halo-effect'?

- (c) Doesn't study (1) ignore the *organizational* context in which health-care is delivered (relations between physicians and other staff, tacit theories of 'good counselling', resources available, staff turnover, etc.) which shape its nature and effectiveness in actual situations?
- (d) Don't both studies treat subjects as 'an aggregation of disparate individuals' who have no social interaction with one another (Bryman 1988: 39)? Yet surely sexual behaviour has a large social component where people respond to their partners and to culturally-provided versions of appropriate and inappropriate behaviour?

(e)

Are we any wiser about how the different forms of counselling work in practice? In study (1), we make a dangerous leap from descriptive work to 'operationalization'. How do we know that the counselling methods that we have created in the laboratory bear much relation to that found in the field? In study (2), we examine the effects of actual methods *without knowing* what those methods (in detail) are. Hence, if counselling in a particular centre seems to be effective, we don't know what about it is effective and so we cannot replicate it more generally. This problem has a general relevance to policy-oriented research, e.g. attempts to reduce inequalities in education by structural changes which are not based on understanding the ways in which teaching works in practice (for instance, see Mehan 1979).

- (f) If our interest is in the relation of counselling to sexual practices, does either study tell us how people actually talk about sex with professionals and with each other as opposed to via responses to researchers' questions? Hence, the apparently unequivocal measures of information-retention, attitude and behaviour that we obtain via laboratory or questionnaire methods seem to have a tenuous basis in what people may be saying and doing in their everyday lives.

An example makes the point very well. At a recent meeting of social scientists working on AIDS, much concern was expressed about the difficulty of recruiting a sample of the population prepared to answer researchers' questions about their sexual behaviour. As a result, it was suggested that the Medical Research

Council convene a subsequent meeting at which we could swap tips about how to recruit such a sample.

The irony of AIDS researchers meeting to discuss such a topic seemed lost on most of the people present. For, if we concede that our best chance of limiting the spread of HIV may be by encouraging people to discuss their sexual practices with their partners, then surely we need to study naturally-occurring talk about sexuality rather than to treat the whole issue as a technical problem of obtaining a sample?

To counter the problem (f) above, work is being carried out on understanding how people ordinarily describe their own and others' sexual activities. This currently takes two forms:

- 1 Asking respondents what they understand by a series of terms used by professionals to describe sexual activities (e.g. 'heterosexual', 'intercourse' etc.). Using this method, Wellings (1989) discovered that many of the terms used in British government health education campaigns on AIDS were misunderstood by the general public. Many people simply did not use such terms to describe their own or others' behaviour – indeed they were strikingly reticent about using very many explicitly sexual terms, at least to an interviewer.
- 2 Asking people to keep diaries about their sexual encounters where they record, in their own terms, the number and nature of their sexual activities over many months. This method is currently being used by Coxon with a cohort of gay men in Britain (see Weatherburn *et al.* 1990). The aim is to discover everyday vocabularies for describing sexual activity without researchers providing their own formulations – as in most survey research.

Despite the apparent (and real) differences between experimental designs and qualitative research like that described above, both share an unwillingness to examine how 'safe sex' comes to be constituted as a topic in naturally-occurring situations. For instance, keeping a diary about one's sexual activities is unlikely to be a routine situation for many people! This is because both kinds of research are fundamentally concerned with the environment around the phenomenon rather than the phenomenon itself. This arises within the following kind of decision-making strategy: