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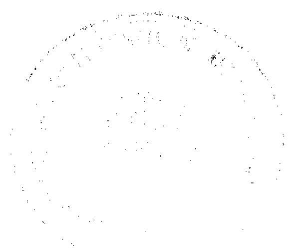
Central Problems in Social Theory

Action, structure and contradiction in social analysis

Anthony Giddens



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The Prospects for Social Theory Today

In this concluding paper, I shall try to place some of the issues discussed earlier in the book in the context of an overall analysis of the current prospects for social theory. The logical starting-point for such an analysis is the state of disarray that characterises social theory today – a matter of common awareness to anyone working within the social sciences. The past decade or so has seen the revival of traditionally established forms of theory (such as hermeneutics), the emergence of seemingly novel perspectives (including especially ethnomethodology), and the attempted incorporation within social theory of various approaches claimed to be drawn from formerly separate philosophical endeavours (the philosophy of the later Wittgenstein, ordinary language philosophy and phenomenology). To these we can add the important resurgence of Marxist theory. The latter however cannot always be clearly distinguished from trends in non-Marxist social science, since most of the same divisions appear, even if in rather different form, within Marxism: the contrasts between the various sorts of 'phenomenological Marxism', 'critical theory', 'Marxist structuralism', etc. are often as pronounced as those outside Marxism.

Now we must recognise that there are still fairly distinct 'national sociologies' – or, more accurately, types of intellectual tradition associated with major language communities, such as English, French and German. The degree of prominence of the different theoretical parameters indicated above varies between these communities: my remarks in this paper will be primarily directed to social science within the English-speaking world.

The orthodox consensus

In English-speaking sociology, the immediate origins of the disarray of social theory can be quite readily discerned. During the post-war period, up until at least the late 1960s, there was something like a consensus that held the 'middle ground' of sociology. This was not, to be sure, an unchallenged consensus; but it provided a focus of debate both for those who supported it and for those who were critical. This consensus, or so I would argue, involved two connected strands: sets of ideas whose antecedents can be traced back well into the nineteenth century, but which became elaborated in novel forms in the 1950s and 1960s. The first of these concerns what I have referred to generically as the theory of industrial society.[1] Those who contributed to the theory of industrial society – authors such as Lipset, Bell and Parsons in the United States, and Aron and Dahrendorf in Europe – held a range of broadly similar views. By opting for a bipolar contrast between 'traditional society' and 'industrial society', they were able to conclude that no form of socialist society could be distinctively different from a capitalistic one; socialism and capitalism are at most merely two partially distinct sub-types of industrial society. Such authors held to the view that, with the maturity of the industrial order, class conflict loses its transformative potential. Acute class struggles, they agreed with Durkheim,[2] are characteristic of the strains created during the early phase of development of industrial society; once class relations have become normatively regulated, class conflict becomes accommodated to the existing order. The 'institutionalisation of class conflict', which meant both the normative regulation of class struggles and at the same time their confinement to the separate spheres of industrial negotiation and political mobilisation, also supposedly entailed an end of ideology: Marxism, and other forms of radical socialist thought, were regarded as ideological expressions of the same strains which produced intense class conflicts in the initial stages of the formation of industrial society.

These views, which were developed in a political context of progressive liberalism, during a phase of relatively stable economic growth in Western capitalism, now appear almost archaic, following a period of heightened political and economic conflict. Indeed, they may now be interpreted as a cautionary tale of the perils of

overgeneralisation in social analysis: a period of not much more than a decade or so was taken as evidence for the most general assertions and projections about profoundly rooted trends in 'industrial society'. (The salutary nature of this lesson should not be ignored by those who are prone to treat falterings in the smooth economic growth of the Western economies in current times as a basis for a reversion to a dogmatic type of orthodox Marxism.) The theory of industrial society has today probably lost most of the support it once enjoyed among sociologists and political theorists: even some of its most enthusiastic advocates have had second thoughts about their earlier views.

Since the theory of industrial society, as elaborated in the 1950s and 1960s, was closely bound up with certain interpretations of political and economic changes in the early post-war period in the West, some of its shortcomings can be quite easily identified in the light of subsequent developments in the advanced capitalist societies. (One may take as an illustration the wholesale expansion of higher education, which only a few years ago was made into a long-term trend deeply entrenched within 'industrial society'.)[3] Such is not the case with the other strand in the erstwhile consensus in sociology, which was of a more abstract character, and involved an overall appraisal of the logical form and likely achievements of the social sciences. We can distinguish in turn two features of this second strand of orthodox or mainstream sociology: the prevalence of *functionalism* and *naturalism*. It is these perspectives that I shall be concerned with in this paper.

Each of these features has had a long-standing association with the theory of industrial society: the traditions of thought which run from Comte and Durkheim through to Parsons and modern American sociology have been of primary importance in sustaining this connection. Functionalist thought, which has always been strongly associated with unfolding models of change based upon metaphors of biological growth or evolution, has in general accorded well with the theme of 'progress with order', a Comtean *motif* that has been echoed in some version or another by all proponents of the theory of industrial society.[4] 'Functionalism', of course, is only a loosely associated body of doctrines. Several related versions have been developed in this century: the 'anthropological functionalism' of Radcliffe-Brown and Malinowski, the 'normative functionalism' of Parsons, and the 'conflict functionalism' of Merton. It is not relevant here to attempt a direct characterisation of the principal traits of functionalist thought. But it is worth emphasising that functionalism has normally been closely associated with the idea that biology provides the proximate model for sociology, since both disciplines, it is argued, deal with systems rather than aggregates. I have tried to show elsewhere that models of biological systems, especially those tied to a notion of homeostasis, will not suffice to illuminate some of the key issues posed by the analysis of social systems.[5] This has also been in a certain sense acknowledged by Parsons, who has turned to cybernetic models of information control in his more recent writings.[6]

From Comte to Durkheim to modern American sociology, functionalism has been closely connected with a naturalistic standpoint in social philosophy, if naturalism is understood to refer to the thesis that the logical frameworks of natural and social science are in essential respects the same. No more comprehensive interpretation of this standpoint has been offered than that formulated by Comte, and I want to point to at least one important residue of the Comtean position that remained an integral element of mainstream sociology in the post Second World War period. Comte's 'hierarchy of the sciences' was intended to be applied both analytically and historically. That is to say, it provided a logical exposition of the relations between the sciences, including that between biology and sociology: each science is both dependent upon those below it in the hierarchy and yet has its own rigorously autonomous factual sphere of investigation (a notion that was later strongly reiterated by Durkheim). But if understood laterally rather than horizontally, the hierarchy of the sciences provided an historical understanding of the progression of scientific development – in combination, of course, with the 'law of the three stages'. Science develops first in relation to those objects and events furthest removed from human involvement and control. Mathematics and physics are hence the first fields to be established on a scientific basis; the subsequent history of science is that of approaching nearer and nearer to human society itself. Human conduct is most refractory to the scientific understanding, since it is most difficult of all for human beings to look at their own behaviour in a scientific light. Sociology is thus the last science to come into being. Now the significance of this general conception is that it ties a naturalistic formulation of the logical form of sociology *to an account of its youthful character as compared to the natural*

sciences. Sociology is a 'late arrival', the completion of the extension of the positive spirit to the explanation of human social conduct.

The notion of the youthfulness of sociology, as compared to biology, but particularly to the fields of physics and chemistry, survived as an important element of the mainstream consensus. Its significance is precisely that it connects presumed logical features of social science to a specific self-understanding of the history of the discipline. If there appear to be certain differences between the natural and social sciences, in respect of such matters as the establishment of a set of precisely formulated laws of a universal character, such differences can be explained as resulting from the relatively limited amount of time that sociology has been established on a scientific footing. The thesis of naturalism is sustained by the assumption of a lag between the respective development of natural and social science.

The 1950s and 1960s saw a partial reunification, especially in the context of American sociology, of functionalism with positivistic philosophies of science, the latter as formulated by such authors as Carnap, Hempel and Nagel: this conjunction formed a major means whereby the naturalistic standpoint of the orthodox consensus was formulated. Many sociologists embraced such positivistic philosophies, which were essentially liberalised forms of logical empiricism,[7] with a fervour that blinded them to the fact that the logical empiricist view of science represents only one possible philosophy of science among other available philosophies. The logical empiricist philosophy of science came to be seen simply as what natural science *is*, and as showing what sociology should become. If the empiricist philosophers from their side were less hasty to consummate the union, and were for the most part sceptical of the logical status of functionalism, they none the less came to accept that functional analysis, as the shared concern of both biology and social science, could be made to conform to the exigencies of scientific method.[8]

Current dilemmas

The dissolution of the orthodox consensus has been succeeded by the Babel of theoretical voices that currently clamour for attention. One might distinguish three prevalent reactions to the seemingly

disoriented situation of social theory. The first is a reaction of despair or disillusionment. There are some who are prone to argue that, since those concerned with the more abstract problems of social theory cannot agree even about the basic presumptions with which the study of human social conduct should be approached, such problems can effectively be ignored in the continuance of the practice of social research. Many of the issues dealt with as 'social theory', it is claimed, are really philosophical rather than sociological in character: the squabbles of 'social theorists' can hence be ignored in favour of a concentration upon the doing of social research. But such a stance does not withstand close scrutiny. Quite apart from the untenable character of the positivistic conception which holds that questions of philosophy can be clearly distinguished from the main body of social theory, we must insist that theoretical considerations cannot be without potential impact even upon the most sheerly 'empirical' types of social investigation. A second reaction might be described as a search for security at any cost: a reversion to dogmatism. This is surely the case for some of those who have turned back towards orthodox Marxist positions. There are clear senses in which such positions share similar perspectives to the erstwhile consensus in mainstream sociology; and are equally barren when confronted with issues raised by other theoretical standpoints.

The third response to the theoretical disarray of the social sciences today is almost exactly the opposite of the first. Rather than a reaction of despair, it is one of rejoicing: the diversity of theoretical perspectives is welcomed as testimony to the inherent fruitfulness of social theory. We cannot attempt to achieve a closure of this diversity, nor should we seek to. Even some of the principal figures once involved in the orthodox consensus have now come to veer towards such a view.[9] And it is a view which, appropriately expressed, has a good deal to commend it. For it can plausibly be argued that chronic debates and persistent dissensus about how the study of human social conduct is to be approached express something about the very nature of human social conduct itself; that deeply established disagreements about the nature of human behaviour are integral to human behaviour as such, and thus necessarily intrude into the heart of the discourse of philosophy and social theory. Admitting the significance of this point, however, should not entail advocating the desirability of creating as many divergent abstract

perspectives upon human social behaviour as possible. We may acknowledge the likelihood of continuing disagreements about basic issues in the study of human action, while still stressing the importance both of establishing connections between divergent positions and of attempting to transcend them.

I therefore wish to reject each of these reactions to the theoretical Babel, and to propose instead that social theory stands in need of systematic reconstruction. I make this claim not in the anticipation of substituting a new orthodoxy for an old one, but in the hope of providing a more satisfactory ground for the discussion of central issues in social theory than that provided by the erstwhile consensus, or permitted by the hermetic isolation in which the diversity of current theoretical standpoints tend to exist. I want to argue that the orthodox consensus cannot be quietly forgotten, or dismissed as merely an ideological reflex of welfare-state capitalism, but that its weaknesses have to be identified if we are to declare its abandonment justified; and I want to say that these weaknesses can now be discerned without too much difficulty. I also wish to make the further argument that a diagnosis of the shortcomings of the pre-existing consensus indicates the necessity of theorising – of making a focus of theoretical analysis – issues that were ignored within that consensus. I propose to list five such shortcomings, or sets of shortcomings, that characterised the erstwhile consensus.

The origins of 'sociology'

The first I have briefly alluded to above: it is that mainstream sociology incorporated a *mistaken self-interpretation of its origins vis-à-vis* the natural sciences. As I have also mentioned above, this point has a twofold aspect: it involves assertions about the past development of social science, but also concerns logical implications that are drawn from that development concerning contrasts between the social and natural sciences.

There is no room here to undertake a proper documentation of the thesis that social science is a relative newcomer as compared either to biology or to the other natural sciences: the idea that 'sociology' has been the last discipline to be put on a scientific footing, breaking with speculative philosophy and the philosophy of history. But we have good reason to be sceptical of these claims if we

consider how often they have been lodged: in fact, members of each generation of social thinkers since at least the early part of the eighteenth century have been inclined to assert that they were initiating a newly scientific study of man in society, in contrast to what went before.[10] Vico conceived himself to be founding a 'new science' of society. Montesquieu and Condorcet made similar claims, and held they were breaking with what went before. Comte said much the same thing in his time, acknowledging the contributions of these forerunners, but largely relegating them to the prehistory of sociology, which was only coming to be placed on a scientific basis through his own efforts. And so it continues: Marx argued much the same in respect of Comte; Durkheim in respect of Marx; and yet another generation later, Parsons of Durkheim and others. The fact that such claims have been lodged so persistently by successive generations of social thinkers does not in and of itself show that they cannot be sustained, but it does justify regarding them in a sceptical light. At any rate, I shall assert at this point that the notion that sociology is a newcomer as compared to the natural sciences is an error, whose source is to be found in the accepting of the declarations of one or other of these generations of authors (usually either Marx, or the '1890–1920 generation' to which Durkheim belonged) at their face value. Social science is as old as natural science is; both can be dated back to the post-Renaissance period in Europe, as recognisably 'modern' in form.

Of course, different sectors of both natural and social science have developed unevenly. To forestall possible misunderstandings, I must emphasise that rejection of the thesis of the youthful character of the social sciences does not involve denying that progress has been achieved within them; or that there have been important ruptures or dislocations between different phases of their development, as well as between rival intellectual traditions. Moreover we have to be careful with terminology: the invention of the term 'sociology' by Comte, and its subsequent successful propagation by Durkheim (who however regarded it as a 'somewhat barbaric term') has had more than a certain amount to do with the view that the 'great divide' in social thought can be located somewhere from the middle to the late nineteenth century. 'Sociology' meant something close to what became the orthodox consensus – ***'progress with order' in respect of the maturation of industrial capitalism, naturalism in respect of the logical framework of social science,***

and functionalism. The term 'sociology' is thus a heavily compromised one, and I continue to use it only in the acknowledgement that it is today in such wide currency that there is no hope of substituting a more appropriate term.

Problems of nomology

If the idea of the youthful nature of sociology cannot be sustained, neither can the implications that are drawn from it to explain the apparently rudimentary level of development of social science as compared with the natural sciences. Sociology is not in the process of taking the first steps along paths already successfully trodden by the natural sciences (to say this, as I should strongly emphasise, is not to say that the achievements of the natural sciences are irrelevant to the social sciences).

The most characteristic difference between social and natural science, which has inevitably preoccupied the advocates of naturalism, is the apparent lack, in the former, of sets of precisely formulated laws that are generally agreed upon by the members of a professional community. Various qualifications obviously have to be made in approaching this matter. The natural sciences are not a unity; some disciplines, and sectors of disciplines, are more nomologically 'advanced' than others. Nor are the social sciences, if that term be interpreted to include economics, all of a piece either. Those who work in the social sciences are probably prone to underestimate the prevalence of profound disagreements among physical scientists over quite fundamental problems within their areas of endeavour. None the less, the contrasts between even the less 'advanced' fields of natural science and the most 'advanced' fields within the social sciences, in respect of nomology, are clear and demonstrable.

Rejecting the thesis that social science is a latecomer means also rejecting a 'lag' interpretation of this difference. What, then, are we to make of the issues of the existence and logical form of laws in the social sciences?

I want to propose that laws certainly do exist in the social sciences, if 'laws' be understood in a comprehensive sense to refer to generalisations of a causal character; there only appears to be a dearth of laws in social science if such generalisations are dismissed

as unimportant or wanting by comparison with those to be found in certain areas of natural science. (This should not be taken to imply that the establishing of laws is necessarily the sole concern of either natural or social science.) But there are two principal reasons to suppose that social scientific laws, even in those areas where quantification is most feasible, will be differentiated from those characteristic of the various fields of the physical sciences. One does not refer to a logical contrast and, although not trivial, I shall treat it as essentially uninteresting; the other type is logical in character, and more significant for purposes of my present discussion.

The first concerns the underdetermination of theories by facts. It has become a well-established principle of the philosophy of science that theories are underdetermined by facts: that no amount of accumulated fact will in and of itself determine that one particular theory be accepted and another rejected, since by the modification of the theory, or by other means, the observations in question can be accommodated to it. There is good cause to suppose that the level of underdetermination of theories by facts is likely to be greater in most areas of social science than in most areas of natural science. The factors involved are well-enough known, and there is no need to elaborate upon them at any length: they include difficulties of the replication of observations, the relative lack of possibilities of experimentation, the paucity of 'cases' for comparative analysis with regard to theories concerned with total societies, etc.

The second reason is more important, at least to the present discussion, because it concerns a deep-rooted difference in logical form between laws in the social sciences as compared to those found in natural science. Although the character of natural scientific laws is still controversial and much debated, there is little basis to doubt that most such laws are putatively universal in form within the domains of their application;[11] all laws operate within certain boundary conditions, but the causal relations that they specify are immutable given the occurrence of those conditions. This is however not the case with laws in the social sciences in which, as I have tried to show elsewhere,[12] the causal relations involved always refer to 'mixes' of intended and unintended consequences of reproduced acts. Laws in the social sciences are *historical* in character and in principle *mutable* in form. All forms of regularised social conduct, as I have argued earlier, can be analysed as involving typical sets of connections between the unacknowledged conditions of action, the

rationalisation of action in the context of its purposive reflexive monitoring, and the unintended consequences of action.[13] The boundary conditions involved with laws in the social sciences include as a basic element the knowledge that actors, in a given institutional context, have about the circumstances of their action. Change in typically established connections tying unacknowledged conditions, the rationalisation of action, and unintended consequences, into modes of social reproduction results in potential alteration of the causal relations specified by a law or laws: and such alteration can stem from coming to know about such a law or laws. Once known – by those to whose conduct they relate – laws may become applied as rules and resources in the duality of structure: the very double meaning (and origin) of 'law' as both precept of action and generalisation about action draws our attention to this. To say that all laws in the social sciences are historical and in principle mutable is not, of course, to deny that there may be laws of universal form concerning physical aspects of the human organism which might be relevant to the study of social conduct.

The orthodox consensus was familiar with the mutability of laws in the social sciences in the form of 'self-fulfilling' and 'self-negating prophecies'.[14] But here the relation between the reflexive appropriation of knowledge and the conditions of action is apprehended, first, only as a 'problem' confronting the social investigator; and second, only as affecting the mobilisation of evidence for generalisations, rather than as broaching epistemological issues relevant to the very character of those generalisations themselves. Self-fulfilling or self-negating prophecies, in other words, are seen as predictions which, by the fact of their announcement or propagation, serve to create the conditions which render them valid, or alternatively produce the contrary consequence. The 'problem' they pose is that of marginalising the noxious effect which such nuisances have upon the testing of hypotheses. But if the mutable character of all social scientific generalisations is acknowledged, we must conclude that such a standpoint is quite inadequate. Rather than attempting to marginalise, and treat purely as a 'problem', the potential incorporation of social scientific theories and observations within the reflexive rationalisation of those who are their 'object' – human agents – we have to treat the phenomenon as one of essential interest and concern to the social sciences. For it becomes clear that every generalisation or form of study that is concerned with an

existing society constitutes *a potential intervention within that society*: and this leads through to the tasks and aims of sociology *as critical theory*.

Ordinary language and social science

The second set of shortcomings that characterised the erstwhile consensus concern its reliance upon a now *outmoded and defective philosophy of language*. As I shall try to demonstrate, the implications of this point link directly with considerations I have just discussed. Orthodox sociology took for granted an old-established view of language: an old-established view, however, that received a new impetus from the work of Russell, the early Wittgenstein, and subsequently from logical empiricism. According to this conception, language is above all a medium of describing the world (physical or social). Language should be studied as a medium of descriptions, and an isomorphy can be discovered between the structural form of language, or certain basic features of language, and the object-worlds to which language gives access. The most developed and sophisticated version of this standpoint is to be found in Wittgenstein's *Tractatus*, according to which the basic units of language are 'pictures' of corresponding units in reality.

Wittgenstein's own rejection of his former views is only one element in a convergence of philosophies otherwise quite distinct from one another: ordinary language philosophy, Schutzian phenomenology, and contemporary hermeneutics. All these have come to the view that it is erroneous to treat language as being most aptly characterised as a medium of descriptions. Description is only one among many other things that are carried out in and through language. Language is a medium of social practice, and as such is implicated in all the variegated activities in which social actors engage. Austin's famous example is still as good as any to illustrate the point. The words uttered in a marriage ceremonial do not constitute a description of that ceremonial: they *are* part of that ceremonial. In another equally well-known example: language has as many uses, and therefore as many facets, as the tools in a tool-box.[15]

Since the orthodox consensus accepted the traditionally-established view of language, those working within it dismissed the

relation between ordinary language – the language employed in the course of day-to-day conduct – and the technical metalanguages of social science as of no particular interest or importance. The object of concepts introduced or invented by the sociologist, they presumed, is to improve upon or correct, where necessary, the inadequacies of ordinary language. Ordinary language is often fuzzy and imprecise: these deficiencies can be overcome by moving over to metalanguages that embody clear and precisely formulated concepts.[16] But the assumption that the relation between ordinary language and the technical languages of social science poses no questions of any particular interest or difficulty cannot be maintained if we understand the significance of the newer philosophical conceptions of language. Ordinary or lay language cannot be just dismissed as corrigible in the light of sociological neologisms, since lay language enters into the very constitution of social activity itself.

This point has been recognised by those working from the standpoint of post-Wittgensteinian philosophy, as well as by phenomenological authors. At least two distinct interpretations of how the connections between ordinary language and the technical concepts of the social sciences should be grasped can be discerned in the literature. One is that formulated by Schutz in relation to what, borrowing a term from Weber, he calls the *postulate of adequacy*. Schutz holds that the relevances presumed by the concerns of the social scientist are different from those of lay actors in their day-to-day behaviour. In social science, we are interested in generalised, context-free knowledge; the stocks of knowledge employed by social actors in social life, on the other hand, are forms of 'cookery book knowledge', in which the emphasis is upon the practical mastery of the demands of everyday activities. The concepts invented by the social scientist thus may differ from those employed in ordinary language, because two different orders of relevances are involved. But the former have to meet a criterion of adequacy in respect of the latter. Schutz's various formulations of the postulate of adequacy are not wholly unambiguous. He seems, however, to assert that concepts of social science can only be declared to be adequate in so far as they can be translated in principle into the everyday language of lay actors.[17] If this is in fact what Schutz means, it is hardly a defensible viewpoint. In what sense does the notion of 'liquidity preference' have to be capable of translation

into the ordinary language concepts of actors engaged in economic activities? There seems no reason to suppose that an evaluation of the adequacy of the concept to economic theory has anything to do with whether or not such a translation can be effected. The shortcomings of Schutz's view are also indicated by considering the behaviour of very small children, to which we might very well want to apply technical terminologies of action; if the children in question are too young to have mastered more than rudimentary linguistic skills, there would obviously be no possibility of testing the adequacy of such terminologies in terms of a translation process.

Schutz's postulate of adequacy is therefore not a satisfactory mode of approaching the connections between lay language and the concepts of social science. An alternative view is set out by Winch, and I shall suggest that this is more nearly correct. Winch holds that there is a 'logical tie' between ordinary language and the specialised languages of the social sciences, and indicates that the nature of this tie is the reverse of that entailed by Schutz's postulate of adequacy. It exists not because sociological concepts have to be capable of transposition into lay ones, but rather the opposite: because the concepts invented by the social scientist presume mastery of concepts applied by social actors themselves in the course of their conduct. Winch does not spell this out satisfactorily, and does not make it sufficiently clear that such lay concepts are typically only partially available discursively to actors; nor does he really explain why distinctive social scientific languages are needed at all, as Schutz tries to do.[18] But his main point is clear enough, and valid: a term like liquidity preference only applies to the behaviour, and consequences of behaviour, of actors who have mastered, in the sense of practical consciousness, notions like 'risk', 'profit', 'investment', etc.: notions embedded in the contexts of use of ordinary language.

The question of why social scientific metalanguages are necessary at all I shall leave to the next section, since answering it involves looking at issues to be discussed there. We cannot, however, leave matters where Winch leaves them in respect of the relation between lay language and the terminologies of social science. The 'tie' between the two is not only logical in character: it has *practical implications* which relate to the significance of reflexivity, introduced earlier. It is not just that the social analyst is dependent upon 'mutual knowledge' – founded in ordinary language categories – in

order to generate characterisations of his field of investigation. There is a two-way relation involved between lay language and the language of social science, because any of the concepts introduced by sociological observers can in principle be appropriated by lay actors themselves, and applied as part of 'ordinary language' discourse. Thus it may happen that terms (a good example is the term 'economic') are appropriated by technical specialists from lay discourse, new meanings given to them, and these new meanings later returned to lay discourse. Such a phenomenon is not just of interest to the history of ideas. Again it opens out to issues that cannot effectively be marginalised in social science, in the manner normally suggested within the orthodox consensus. For the latter typically assumed an instrumental connection between sociological findings and practical 'applications' of them, a connection presumed to be logically the same as that pertaining between natural science and technology.

Revelation, mutual knowledge, common sense

To carry this discussion further, however, there is another source of weakness in the erstwhile consensus which must be analysed. This I shall identify by saying that orthodox sociology relied upon an *oversimple revelatory model of social science*, based upon naturalistic presumptions. The essentials of this model are as follows. The findings of natural science, it is assumed, are revelatory or demystifying in respect of common-sense beliefs about the physical world. What science does is to 'check up' upon common-sense views of and attitudes towards the world, showing some of them to be mistaken, and using others as a point of departure from which to develop more detailed and profound explanations of objects and events than were available in lay knowledge. The progress of science punctures the delusions of customary habitual beliefs. Now under some circumstances, it is pointed out, the findings claimed by scientists are resisted by those who choose to cling on to their established beliefs or conceptions. Those findings are rejected or ignored, either because of vested interests that are threatened by them, or because of the inertia of habit or prejudice. There are those who continue to hold that the earth is flat, no matter how conclusive the evidence to the contrary might appear to others to be.

In the orthodox consensus, this view was transferred *en bloc* to sociology. There are strong reasons to suppose (following particularly the arguments developed by Husserl in *The Crisis of European Sciences*) that this is an inadequate approach even in respect of the relation between natural science and 'common sense'; I shall be concerned here, however, only with its implications when transposed to the social sciences.

According to the revelatory model just described then, 'resistance' to the findings of social investigators takes the same form as that found in respect of some of the claims of natural science: a 'refusal to listen' in favour of an obdurate affiliation to pre-existing beliefs or ideas. But anyone who works within the social sciences is likely to be familiar with a quite different form of resistance to the claimed findings of social science. Far from resisting the findings of sociological investigations because they convey claims that people are disinclined to want to know about, such findings are resisted *on the basis that they are already well known and familiar.* Sociology, it is often argued, simply tells us what we already know – albeit often wrapped up in esoteric jargon, such that it might initially appear to be something novel. This could be called the *lay critique of sociology*.

Now sociologists are not prone to take the lay critique of their claims seriously, usually attributing it to the influence of engrained habits of thought, or prejudice. Resistance to the findings of social science they see as including resistance to the very idea of studying human social conduct in a scientific manner. But lay objections to social science are so prevalent that a more plausible defence against them is needed; and this can also be found by an elaboration of the revelatory model. The object of sociology is to check up on common-sense beliefs. Where social research reveals that what actors believe about the conditions of their own action, or other features of their society, is in fact the case, its findings will necessarily appear banal or unilluminating. It is just such instances, it is proposed, that are fastened upon by the lay critics of sociology. But there will be other instances in which social analysis will show that common-sense beliefs are in fact invalid; in such circumstances, social science will appear revelatory.

If the practitioners of social science have not been inclined to give serious attention to the lay critique of sociology, some philosophers have done so. Louch, for example, has argued that the concepts of sociology are 'unnecessary and pretentious'.[19] To explain human

social activity, we need only to inquire into the reasons actors have for acting as they do. Once we have ascertained what those reasons are or were, which can be done in ordinary language, there is nothing more that can be asked. While anthropology, Louch says, can provide for us a collection of 'travellers' tales', sociology is a redundant exercise – indeed, worse than that, since the introduction of technical concepts in place of ordinary language terms can serve to obscure what was formerly evident enough to everybody, and hence can be used by the powerful as a means of dominating the less powerful. Winch flirts with the same conclusion, although he evidently regards anthropology as of more importance than Louch does. Given that he does not clearly explicate the role of technical concepts in social science, and that he precludes the possibility of formulating causal laws in respect of social conduct, it is not easy to see what a 'Winchean sociology' would look like.[20]

I wish to suggest that we should indeed take the lay critique of sociology seriously, even if in the end it cannot be sustained. For it is correct to claim that every member of society must know (in both the practical and discursive modes) a great deal about the workings of that society by virtue of his or her participation in it: or, more accurately put, that such knowledge is incorporated as an element in the production and reproduction of that society via the duality of structure. It is not at all as easy as the orthodox consensus presumed to puzzle out the conditions under which the social sciences can deliver enlightenment to the members of a society that is made the subject of study. Explanation of human social conduct in terms of reasons can certainly not be ignored by sociologists: the rationalisation of action is the fundamental component of social activity that orthodox sociology discounted. On the other hand, it should be emphasised just as strongly that the rationalisation of action is always bounded, in every sort of historical context; and it is in exploring the nature and persistence of these bounds that the tasks of social science are to be found. As I have proposed in an earlier paper, there are three types of circumstance relevant here: unconscious elements in action, practical consciousness, and the unintended consequences of action, all of which combine within the reproduction of social systems.

One further aspect of these issues must be mentioned. It is a notable feature of the 'rediscovery of ordinary language and common sense' that it has frequently eventuated in a sort of *paralysis of*

the critical will. Having come to see that ordinary language and the world of the natural attitude cannot merely be disregarded or corrected by the social analyst, some authors have been tempted to conclude that no kind of critical evaluation of beliefs or practices is possible where such beliefs and practices form part of an alien cultural system. The debate surrounding Winch's discussion of Zande sorcery is, of course, well known, as is that stimulated by Garfinkel's principle of 'ethno-methodological indifference'. In these controversies, I want to argue, both sets of proponents have right on their side: but each has failed to make a vital distinction. It is right to claim that the condition of generating valid descriptions of a form of life entails being able in principle to participate in it (without necessarily having done so in practice). To know a form of life is to know a language, but in the context of the practices that are organised through the 'common sense' or tacit presuppositions against the background of which discourse is carried on. In this sense, hermeneutic tasks are integral to the social sciences. But it does not follow from such a conclusion that the beliefs and practices involved in forms of life cannot be subjected to critical assessment – including within this the critique of ideology. We must distinguish between *respect for the authenticity of belief*, as a necessary condition of any hermeneutic encounter between language-games; and the *critical evaluation of the justification of belief.* Expressed in less cumbersome fashion, we must differentiate what I call 'mutual knowledge' from what might simply be called 'common sense'.

Mutual knowledge is a necessary medium of access in the mediation of frames of meaning, and brackets the factual status of the tacit and discursive understandings shared by an observer with those whose conduct he or she seeks to characterise. It is largely because the bracketing involved in the application of mutual knowledge is usually itself tacit mutual knowledge, employed in a routine fashion, that the need to respect the authenticity of belief is not always apparent to sociological investigators. But the difference that such an orientation makes is easily seen in circumstances where it is absent. Thus according to 'physiological' views of schizophrenia, the utterances of schizophrenics are often to be regarded as merely meaningless babble. If Laing is right, however, the language of schizophrenics is meaningful, so long as we see that some of the notions that are taken for granted by the majority of the population are questioned or expressed in quite different form by schizo-

phrenic individuals. The development of dialogue with schizophrenic persons, as a hermeneutic endeavour, is only possible if we accept that their utterances and behaviour may be treated 'methodologically' as authentic. To treat such utterances and behaviour as authentic means to hold in abeyance their possible validity or falsity.

What I earlier called the 'rediscovery of ordinary language and common sense' is in these terms *the discovery of the significance of mutual knowledge*: mutual knowledge is not corrigible to the sociological observer. It is only the methodological bracketing mentioned above that separates mutual knowledge from what I want to suggest can be called 'common sense'. By 'common sense' I refer to the un-bracketing of mutual knowledge: the consideration of the logical and empirical status of belief-claims involved (tacitly and discursively) in forms of life. Common sense is corrigible in the light of claimed findings of social and natural science. The distinction between mutual knowledge and common sense can be illustrated by allusion to the Zande witchcraft controversy. Winch is right to hold that accurate characterisations of the beliefs and practices connected with Zande sorcery are 'rational' – in so far as that term means in this context that there exist internally coherent frames which both a sociological observer and the Azande draw upon in generating descriptions of witchcraft. But he is wrong in so far as he seems to infer from this that acknowledgement of the 'rationality' or authenticity of Zande witchcraft and oracular divination precludes critical evaluation of the beliefs and the activities thus characterised or identified. Mutual knowledge is the necessary medium of identifying what is going on when a sorcerer places a malicious spell upon an individual in order to procure that person's death. But this is no logical bar at all to critical inquiry into the empirical grounding that can be marshalled to support the validity of the belief-claims held in relation to this practice, or into their possible ideological ramifications.

Of course, I do not want to say this provides a solution to problems of 'rationality'; it would be more accurate to say that it is where those problems begin, in respect of the rational justification of belief. However, this is not the point in the particular context of my argument here. Winch and others have demonstrated entirely convincingly the naïveté of the views of the orthodox consensus about the revelatory character of social science. I am only concerned at this juncture to propose that we should not therefore succumb passively to a paralysis of the critical will. There are many threads involved in connecting the rational justification of belief to critical theory, and I shall attempt to provide a detailed discussion of these in the book to follow this one. However, I do want to make what seems to me an important logical point, which I think shows that the critical evaluation of beliefs and practices is an inescapable feature of the discourse of the social sciences. This is that the critical assessment of common-sense beliefs does not just logically presume drawing upon mutual knowledge; *the reverse is in fact also the case.* For any characterisations of beliefs or practices made by a sociological observer logically presuppose the possibility of their justification, offered in response to the potential critical evaluation by others of the accuracy or appropriateness of those characterisations themselves.

The theory of action

As a fourth type of shortcoming of the erstwhile consensus, we may say: *orthodox sociology lacked a theory of action.* But I also want to claim that this was directly linked to a failure to make questions of power central to social theory. The lack of a theory of action, by which I mean a conception of conduct as reflexively monitored by social agents who are partially aware of the conditions of their behaviour, is first of all to be attributed to the dominance of naturalism as a philosophy of social science. In the cruder versions or applications of naturalism in sociology, conduct is explained merely as the outcome of social causes. As the most thoroughgoing attempt to produce a synthesis of a theory of action with functionalism, Parsons's 'action frame of reference' has justly been the most influential overall theoretical scheme in (English-speaking) sociology. Critics have often pointed out that, in spite of the action terminology which Parsons uses, recognisably human agents seem to elude the grasp of his scheme: the stage is set, the scripts written, the roles established, but the performers are curiously absent from the scene.[21] But the critics have not always recognised why this is so. In *The Structure of Social Action*, Parsons identifies action theory with 'voluntarism', by which he refers primarily to the purposive character of human conduct, and to the capability of actors to

choose between different goals or projects.[22] Voluntarism is interpreted against the backdrop of the 'Hobbesian problem of order', as posing the question of how purposiveness or a diversity of wills is compatible with 'order'. The reconciliation of the Hobbesian problem and voluntarism thus becomes the main issue that the action frame of reference is called into existence to resolve, and this reconciliation is achieved through regarding values simultaneously as the basis of social consensus and as the motivational components of the personalities of members of society. Apart from the difficulties raised by this thesis in respect of the nature and significance of values – and of 'order'[23] – this approach does not serve to draw attention to the importance of reasons in human conduct: that human beings reflexively monitor their conduct via the knowledge they have of the circumstances of their activity. Although Parsons separates 'cognitive' from 'cathectic' symbols in his scheme, *his social actors are not capable, knowledgeable agents.*

Of course neither naturalism nor functionalism reigned unchallenged in the post-war period. Within American sociology, those writing from a perspective of symbolic interactionism have diverged significantly from the emphases of the orthodox consensus, especially in respect of being concerned with the theory of action, as I have specified it above. But 'symbolic interactionism' – Blumer's term for a diffuse set of influences emanating from G. H. Mead – has from the beginning been hampered by an inadequate theoretical grasp of problems of institutional analysis and transformation. The importance of Mead's conceptions of the development of reflexivity, of the gesture and symbol, overshadowed the fact that his treatment of society, as represented by the 'generalised other', is a rudimentary one. Mead's social philosophy (like Piaget's developmental psychology) lacks an understanding of the broader society as a differentiated and historically located formation. Moreover, although Mead successfully places reflexivity at the centre of the concerns of social philosophy and social theory, the origins of the 'I' in the dialectic of 'I' and 'me' remain obscure and unexplained. The major part of his concern is with the emergence of the 'me', or social self. Hence it is perhaps not surprising that, amongst some of his followers, the reflexive relation of 'I' and 'me' largely disappears from view in favour of a concentration upon the social self. Once this movement has taken place, given the dearth of an adequate conceptualisation of institutions and institutional change, the way is

opened for the idea that symbolic interactionism and functionalism can be helpfully conjoined to one another. The former is held to deal with 'micro-sociological' issues to do with small-scale social relationships, while 'macro-sociological' issues, concerned with aspects of the institutional character of society, are left to functionalism.

In 'Agency, Structure' I have argued that the successful introduction of a theory of action into sociology cannot be achieved without a complementary re-working of the idea of structure. Such a re-working is immediately relevant to questions of ordinary language and the lay critique of sociology. The notion of the *duality of structure*, which I have accentuated as a leading theme of this book, involves recognising that the reflexive monitoring of action both draws upon and reconstitutes the institutional organisation of society. The recognition that to be a ('competent') member of society, every individual must know a great deal about the workings of that society, is precisely the main basis of the concept of the duality of structure. The thesis that the notion of human agency cannot be adequately explicated without that of structure, and vice versa, necessarily connects with the claim that temporality has to be treated as integral to a conceptual grasp of the constitution of social life. Whatever the incompatibilities between structuralist thought and history, one of the specific contributions of structuralism from Saussure onwards has been to illuminate the temporal ordering of social reproduction. The social totality cannot be best understood, as in functionalist conceptions of the whole, as a given 'presence', but as relations of presence and absence recursively ordered. The strictures of structuralist authors against philosophies and forms of social theory that accord primacy to the subject may be readily understandable against the background of Cartesianism; and it is essential to grasp the importance of the thesis that we have to reject any conception of a subject that is 'transparent to itself'. But here we also approach the limitations of structuralist theories, which are riddled with dualisms inherited from Saussure. One of these dualisms is that between structure and event, usually overlapping directly with that of unconscious/conscious. The prominence of these oppositions has effectively foreclosed the possibility of generating a satisfactory account of human agency from within structuralist thought. The supersession of the dualism of structure and event, within sociological theory at any rate, is most readily approached –

or so I want to claim – by introducing a distinction between system and structure, the former being ordered in terms of the reproduction of spatially and temporally situated events, the latter being both the medium and outcome of such reproduction. This is immediately connected to the rejection of the polarity of unconscious/conscious, since a theory of agency must recognise the basic significance of practical consciousness in social reproduction. Practical consciousness is not 'consciousness' as ordinarily understood in structuralist theories; but it is also easily distinguishable from the unconscious in any sense of that term.

Structuralism and functionalism betray their common origins in respect of concepts of power with which they have often been associated. For many authors working within those traditions, if a concept of power has been developed at all, power is regarded as a phenomenon of a society or collectivity confronting the individual. This was already clear in Durkheim, who in the places where he addressed problems of power, tended to do so in terms comparable to those in which he sought to analyse the constraining influence of social facts.[24] Those authors writing within the philosophy of action, on the other hand, have either regarded power as the capability of an individual agent to accomplish his will, or (especially in the literature influenced by Austin and the later Wittgenstein) have largely ignored issues of power altogether. In this regard, there is a point of direct contact between the philosophy of action and 'normative functionalism', *each of which, in rather different ways, have tended to treat norms or conventions as exemplifying 'the social'.* Weber's definition of power, as the chance of an agent to secure his will even against the resistance of others, has probably been the most frequently utilised in the literature. I criticise this in two respects. On the one hand, it reflects Weber's subjectivist methodological position, and leads to the dualism of action and structure that I have insisted has to be overcome; on the other, considered solely from the point of view of the connection between power and agency, it does not bite deeply enough. For the notion of human action logically implies that of power, understood as transformative capacity: 'action' only exists when an agent has the capability of intervening, or refraining from intervening, in a series of events so as to be able to influence their course. The introduction of a theory of action into sociology thus entails regarding power as just as essential and integral to social interaction as conventions are.

But the same considerations which apply to the theory of agency generally also apply to power: we have to relate power as a resource drawn upon by agents in the production and reproduction of interaction to the structural characteristics of society. Neither aspect of power is more 'basic' than the other.

The social and natural sciences

The fifth type of shortcoming of the orthodox consensus has been much discussed in recent years, but I want to claim that its implications cannot be adequately understood apart from the issues I have referred to in the preceding sections. This fifth point, which connects back to themes I introduced at the beginning of the paper, is: orthodox sociology *was closely tied to a positivistic model of natural science.* The term 'positivism' has become so indiscriminately employed[25] that it is important to point out that, in the context of the ideas informing the erstwhile consensus, it can be used in a fairly definite sense: to refer to what some philosophers have labelled the 'received model' of natural science. The received model was strongly conditioned by liberalised versions of logical positivism, as worked out by Carnap and others; but it was further consolidated and elaborated by members of the 'Berlin group' (especially Hempel), and by indigenous currents of American philosophy (as represented, for instance, by Nagel).

I have already pointed to the important, if never entirely happy, conjunction that was effected between this approach to the philosophy of science and functionalism. But the influence of naturalistic standpoints, of course, has stretched considerably more broadly than this: many authors who have been either sceptical about, or directly critical of, functionalism have presumed that the received model of natural science is appropriate for sociology. Logical empiricist conceptions of natural science, particularly the hypothetico-deductive method as originally advocated by Hempel and Oppenheim, achieved widespread acceptance.[26] Such conceptions were employed to suggest that social science should aim towards the (admittedly distant) goal of formulating deductively related hierarchies of laws; and that explanation in both natural and social science consists in the deductive subsumption of an observation or event under a law.[27] But the first cannot be regarded as an

appropriate general interpretation of the nomological form of the natural sciences, and has even less relevance to sociology, given the historical character of laws of human social conduct: the laws of the social sciences are in principle open to the 'environment' to which they refer. In the light of these considerations, the emphasis that explanation is the deductive relating of an event to a law appears as peculiarly dogmatic and restrictive – even if applied in respect of the natural sciences, but particularly within the sphere of the social sciences.

Explanation, most broadly conceived, can be more appropriately treated as the clearing up of puzzles or queries; seen from this point of view, explanation is the making intelligible of observations or events that cannot be readily interpreted within the context of an existing theory or frame of meaning. The distinction between description and explanation then becomes in some part contextual in character: the identification or description of a phenomenon, by its incorporation within a given frame of meaning, is explanatory where that identification helps to resolve a query. Such a broad notion of explanation relates explanatory queries in science quite closely to everyday queries. In neither case is there a logically closed form assumed by explanation: that is to say, *all attempts to satisfy queries presuppose a contextual 'etcetera clause'*, whereby an inquiry is deemed to be concluded 'for present purposes'.

Now this of course explains very little about the nature of explanation. In particular, it does not show what are the characteristics of a 'satisfactory' or valid' explanation of a phenomenon, as compared to others that might be judged to be defective.[28] I do not intend to take up this issue here. What I do want to stress is that the claim that explanation is contextual does not, as proponents of the orthodox consensus might suppose, imply advocating a 'soft' or humanistic version of sociology. In particular, there can be no reversion to the opposition of *verstehen* and *erklären* which, in the hermeneutic tradition, served to differentiate the tasks of the social sciences from those of the natural sciences. For it is a notable feature of the development of hermeneutics that most authors who have proposed that the social or human sciences are distinctively concerned with 'meanings' or 'cultural products', have accepted a positivistic model of natural science. Dilthey, as is well known, was strongly influenced by J. S. Mill's *Logic*, and accepted the latter's overall characterisation of natural science as a foil for his concep-

tion of the human sciences. In more recent times, Winch's account of the philosophical basis of the social science appears to rely upon the view of natural science developed by the logical empiricists; and Habermas's elaboration of his notion of knowledge-constitutive interests still seems to retain elements of a positivistic model of science, thus in some part recapitulating the *verstehen/erklären* differentiation.[29]

The main implication of the ideas I have set out in this paper is that, in the current phase of social theory, we are involved in rotating two axes simultaneously: that of our understanding of the character of human social activity, and that of the logical form of natural science. *These are not entirely separate endeavours, but feed from a pool of common problems.* For just as it has become apparent that hermeneutic questions are integral to a philosophical understanding of natural science, so the limitations of conceptions of the social sciences that exclude causal analysis have become equally evident. We cannot treat the natural and social sciences as *two independently constituted forms of intellectual endeavour*, whose characteristics can be separately determined, and which then subsequently can be brought together and compared. Philosophers and practitioners of sociology must remain attentive to the progress of the natural sciences; but any philosophy of natural science in turn presupposes a definite stance towards problems of social theory.

I. S. C. T. E.
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28. 'Functionalism: après la lutte', pp. 111–12.
29. E. E. Evans-Pritchard, *Witchcraft, Oracles and Magic among the Azande* (Oxford University Press, 1950); also Bryan Wilson, *Rationality* (Oxford: Blackwell, 1970), and numerous other contributions.
30. Pierre Bourdieu, *Outline of a Theory of Practice* (Cambridge University Press, 1977) p. 83.
31. Cf. Raymond Williams, *Keywords* (London: Fontana, 1976).
32. For a rather different view of these matters, and a variant use of the term 'historicity', see Alain Touraine, *The Self-production of Society*.
33. E. H. Carr, *A History of Soviet Russia*, vol. I (London: Macmillan, 1969) p. 88.
34. *Studies in Social and Political Theory*, pp. 14ff; 'Classical social theory and the origins of modern sociology', *American Journal of Sociology*.
35. Cf. Nisbet's criticisms of metaphors of growth, linked to 'immanent causation, continuity, differentiation, necessity and uniformitarianism [*sic*]'. Robert A. Nisbet, *Social Change and History* (New York: Oxford University Press, 1969) p. 251 and *passim*.
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37. *Studies in Social and Political Theory*, pp. 19–20.
38. *The Class Structure of the Advanced Societies*.
39. Immanuel Wallerstein, *The Modern World-system* (New York: Academic Press, 1974) p. 348.
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46. William Dray, 'The historical explanation of actions reconsidered', in Sidney Hook, *Philosophy and History: A Symposium* (New York University Press, 1963) p. 105.
47. *New Rules of Sociological Method*, pp. 153–4 and *passim*.
48. N. Rescher and O. Helmer, 'On the epistemology of the inexact sciences', *Management Science*, vol. 4 (1959).
49. Dray, 'The historical explanation of actions reconsidered', p. 108.

Chapter 7

1. *Studies in Social and Political Theory*, pp. 14–20; 'Classical social theory and the origins of modern sociology', *American Journal of Sociology*, vol. 81 (1976).

2. Cf. my *Durkheim* (London: Fontana, 1978) pp. 21–33.
3. Cf. Jerome Karabel and A. H. Halsey, *Power and Ideology in Education* (New York: Oxford University Press, 1977).
4. It is a mistake, as I have tried to show elsewhere, to link functionalism mainly to conservative standpoints in politics. 'Four myths in the history of social thought', in *Studies in Social and Political Theory*.
5. Cf. 'Functionalism: après la lutte', in *Studies in Social and Political Theory*.
6. For Parsons's views on cybernetics, see 'The relations between biological and socio-cultural theory', and other papers in Parsons, *Social Systems and the Evolution of Action Theory* (New York: Free Press, 1977).
7. See 'Positivism and its critics', ibid., pp. 44–57.
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12. *New Rules of Sociological Method*.
13. Ibid., pp. 153–4 and *passim*.
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15. Ludwig Wittgenstein, *Philosophical Investigations* (Oxford: Blackwell, 1972) para. 11.
16. See, for instance, C. W. Lachenmeyer, *The Language of Sociology* (New York: Colombia University Press, 1971).
17. The concepts of social science 'must be constructed in such a way that a human act performed within the life-world by an individual actor in the way indicated by the typical construct would be understandable for the actor himself as well as for his fellow-men in terms of common-sense interpretations of everyday life.' Alfred Schutz, *Collected Papers* (The Hague: Mouton, 1967) p. 44.
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