

## Concluding remarks

### History, sociology, theory and the fallacy of misplaced abstractness

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Although I have taught joint courses with historians,<sup>2</sup> I have usually had to exist in a sealed container labelled »sociology«. At my present university, the very mention of the word historiography by a sociologist is enough to send the historians screaming for the exit door. Looking back, I think all along I have been a historian *manqué*. That has certainly been true since my work on food more than two decades ago and my more recent work on America (Mennell 1985; 2007). People often say my work reads more like history than sociology. I deny that, but it is symptomatic of an endemic difficulty. Since the Second World War, sociologists have »retreated into the present«,<sup>3</sup> preoccupied with hodiecentric,<sup>4</sup> static empirical studies of contemporary society, often with immediate short-term questions of public policy in mind. All sociology, in my view, needs to be historically informed. The absence of a broader historical perspective means that sociological research too often has a very short shelf life. Historians, on the other hand, have often pursued detailed

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1 This is a revised version of the transcript of substantially impromptu remarks at the close of the conference.

2 Notably at the University of Exeter, UK, in the 1980s with Colin Jones, now President of the Royal Historical Society, and at Monash University, Australia, with Graeme Davidson.

3 Norbert Elias, The retreat of sociologists into the present, in Elias 2009a: 107-126, originally published in German in 1983, and in English in 1987.

4 Hodiecentric: a useful word meaning present-centred, coined by the Dutch sociologist Goudsblom 1977.

archival research – in many cases for similarly short-term periods, though in the past rather than the present – loudly proclaiming themselves practical empiricists to whom »theory« is irrelevant. One is tempted to recall Keynes’s famous remark that »Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist«. »Atheoretical« historians are usually using some theoretical assumptions of which they are unaware. For that reason, much historical research too has a short shelf life, and apparently needs to be rewritten frequently, in the light of whatever each wave of contemporary preoccupations brings along (Elias 2006a).

In this Annual Seminar, the main lines of debate seem not to have been between history and sociology at all but between different theoretical slants and, to some extent, different substantive interests among sociologists and historians. What most worried me was that many participants were showing signs of anxiety in their use of »theory« in their research. Of course, I welcome the idea that theory is useful in the writing of history and sociology. A good theory is rather like a road map: it shows you how things are connected, how they are related to each other. But a theory, like a map, has to be capable of modification in the light of changing empirical evidence. A theory is not just a conceptual scheme. I did not intend to make this an autobiographical article, but I came to the conclusion that maybe I had better talk a little about my own intellectual journey through sociology and history.

### **Some quasi-autobiographical reflections**

I am within weeks of hitting the age of retirement, and perhaps that turns one’s thoughts back to the beginnings of one’s academic career. Old men forget, but they don’t forget very much. I took a degree in economics at Cambridge and then immediately won what was called a Frank Knox Fellowship to Harvard. It gave me the run of the university, though in practice I settled down in the old Department of Social Relations. It was an utterly star-studded department in the mid 1960s. What had drawn me there was the remarkable reputation of Talcott Parsons, who dominated the world of sociology in a way that no sociologist, no

sociological theorist, has dominated it since. I actually came to know Parsons reasonably well, albeit for only a short period. He was a very nice man, but he did not have a lot of small talk; he found that I had a great deal, so he gravitated towards me. I became the resident expert on Parsons's theory among my cohort of graduate students. Yet in spite of the fact that I found it beautiful and aesthetically pleasing and all that, I had this nagging feeling, what's it all for? I could not really make any connection between it and any sociological research that I might actually want to undertake. Yes, I could see how I could write a book about Parsons, or about sociological theory in general, and indeed that is what I soon did (Mennell 1974).<sup>5</sup> And of course that is the way sociology has developed, with »theory« as a self-contained speciality – may I say as an »autopoietic system«? – in its own right, with no relation at all to what most sociologists are doing most of the time.

Towards the end of my year at Harvard, this problem was crystallised for me in an incident that deserves to be better known. The German sociologist of religion, Rainer Baum, who was a few years ahead of me, was very much an adept, an acolyte, of Parsons, and the story was that he went to see Parsons and he said »Professor Parsons, isn't it true that your system of four functional exigencies – Goal Attainment, Adaptation, Integration and Pattern Maintenance – can be used to analyse any system of any kind of social system, personality system, or cultural system?« »Yes,« replied Parsons, »it can«. »Then« asked Baum, »what about the Holy Trinity? It's only got three bits.« Parsons, so I heard, invented the fourth bit of the Holy Trinity to make it fit his system. Jesus was in the Adaptation box, the Holy Spirit in the Integration box, and God the Father in the Pattern Maintenance box. »The fourth bit«, said Parsons, was »the Human Spirit«, which he fitted into the Goal Attainment box. So Parsons's theoretical framework proved capable of fixing the mistakes in the Nicene Creed. You will think this is a joke; actually I thought it was a joke. It was one of those gossipy things that went around among

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5 My wife and I also translated from French Guy Rocher's excellent *Talcott Parsons and American Sociology* (Nelson 1975).

the graduate students. I thought someone must be a really creative satirist. Then, a couple of days later, Parsons toddled into the room and said, »I've had a new idea« and out it came: Parsons's doctrine of the Holy Quadrumvirate. Years afterwards I found he'd actually published this idea.

This is a good illustration of theory for theory's sake, of the »social theory« industry, of forcing reality into one's abstract categories. »Social theory« is a specialist activity with great prestige. Among sociologists, the »theorists« are the members of the House of Lords, speaking metaphorically (or now, in the case of Tony Giddens, literally).

Nor, coming back to Parsons, was the Holy Trinity episode an isolated aberration, as can be seen if you look at his two more historically-orientated small books, one of which had just been published when I was at Harvard, while the next one was being circulated for comment among us (Parsons 1966; 1971). There you will find again how Parsons thought: »So we've got to think about world history. Let's fit it into our four boxes«. So, yes, you've got it: Russia goes into the Adaptation box, America goes into the Goal-Attainment box, and so on. It's nonsense. The moral of this story is that you will never understand the doctrine of the Holy Trinity, or the development of human societies, by fitting them into abstract boxes.

Parsons was very fond of referring to Alfred North Whitehead's concept of the »fallacy of misplaced concreteness« (Whitehead 1997 [1925]: 51). That is the fallacy of believing that because we have a word or a concept, there must be something real »out there« corresponding to our concept. But Parsons never seems to have considered the opposite fallacy in concept formation, the fallacy of misplaced abstractness.<sup>6</sup> The fallacy of mis-

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6 If Whitehead will always be credited with labelling the former fallacy, it is less clear who coined the latter term. I have been using the term for many years, without any clear notion of where I acquired it. The earliest use in print that I have found is by Daniel Bell in his reply to a review by Peter Berger of Bell's *The Coming of Post-Industrial Society*; see Reply by Daniel Bell, *Contemporary Sociology* (1974) vol. 3, issue 2: 108n.

placed abstractness may be defined as the assumption that we can know in the abstract, in the general, what we do not know in the particular. I think that is probably valid in physics, in Heisenberg's »Uncertainty Principle«, but I am not convinced that it is a relevant idea in the social sciences and history.

Let me give you a little concrete example of over-abstraction. At this conference I have been carrying out a modest observation project in the style of Erving Goffman. Just go around the corner behind me and look at the little abstract symbol on the Gentlemen's toilet. It's not very clear at first glance which sex it means, and first of all I hesitated. I went a few steps further on, to look at the sign on what proved to be the Ladies', which did look like a female. I then watched several other men do the same thing: go to the Ladies', see the symbol on the Ladies', then go back because that clarified the meaning of the abstract symbol on the Gents'. Now, the point is if a symbol is so abstract that it doesn't even tell you what door to go through to the loo, it isn't a lot of use.

But back to my autobiography: that year, 1966-1967, we first-year graduate students spent a whole seminar discussing the so-called macro-micro problem. The macro-micro problem concerns the relationship between macroscopic sociological theories on the one hand and microscopic or interactionist social theories on the other, and the fact that there seems to be a gap between them. Of course, this problem is quite obviously also just a variant of several other common chicken-and-egg static conceptualisations in sociological theory: »action« *versus* »structure« in Parsons's day, or »agency« *versus* »structure« as it later became in the hands of people like Giddens, or »individual« *versus* »society«, and so on. At Harvard we did not discuss only Parsons; we also spent a lot of time discussing a then brand-new book by Peter Blau, *Exchange and Power in Social Life*, which was actually one of the foundation stones of what later became »rational choice theory« in sociology (Blau 1964). As a recent graduate in economics I thought it was pretty much nonsensical; I knew enough economics to think this was not remotely convincing as economic theory. When Blau tried to bridge from his essentially interactionist rational choice theory at the micro level to the macro level – as

he claimed to be able to do – in practice it drove him back into the arms of the tired old ideas of shared norms and values, back to Parsons. Thirty years later, in 1997, I went to the American Sociological Association Annual Meeting in Toronto, and the overall theme chosen by that year's president was the macro-micro problem. I can tell you that the American sociologists had not advanced one inch in the three decades from 1967 to 1997.

George Homans, another of the great Harvard stars of those days, once said to me, rather provocatively, »Talcott is a great empirical sociologist, but he's no good at theory«. The same went for Peter Blau, who did great empirical work. I came to the same conclusion as Homans, and my early exposure to Parsons's theories served me as a kind of vaccination. I had a built-in resistance when other »system builders« in the same tradition came along later: Giddens's »structuration theory«, or even Habermas's »theory of communicative action« – although I would admit that Habermas's enterprise is on an altogether higher intellectual plane than Giddens's. As a prominent British sociologist said about Giddens, »There *is* no theory of structuration, he merely re-describes the problem using a different set of jargon.« As graduate students you must beware: you have to be on guard against such nostrums. Think of Thorstein Veblen's *Theory of the Leisure Class*, with its account of conspicuous consumption and competitive status display. Something similar is involved in the domain of »grand theory« in the social scientific world. »Theory« carries great prestige, and to have a bigger and better theory is a badge of very high status. It seems to me that a lot of theory building in sociology has been driven by that kind of social mechanism of competition. My own mentor, Norbert Elias – of whom more in a moment – had a nice image concerning the relevance of theory, both in sociology and history. He used the Greek myth about Hercules fighting the giant Antaeus. Hercules repeatedly struck great blows, knocking Antaeus to the ground, but each time Antaeus picked himself up and fought back, until finally it dawned on Hercules that Antaeus regained his strength through his feet touching the earth. And so Hercules picked up Antaeus, holding him up in the air with his feet clear of the ground, until Antaeus' strength ebbed

away. The analogy is that theories that are so far up in the air lose their power and become less useful and forceful (Elias 2008a: 77; see Graves 1955: 509).

Thus far, I may have given the impression that I am simply anti-theoretical, like old-style historians. Nothing is actually further from the truth. I consider myself a theoretical maximalist in the writing of history. All history – including traditional narrative history, concerning political events, for example – needs to be informed by sociology, and equally all sociology needs to be informed by history. It is not just a matter for historical sociology, social history, and perhaps intellectual history. History and sociology are equally relevant for the traditional narrative to political and diplomatic history. I found myself thinking of another amusing story, a famous remark by British Prime Minister Harold Macmillan (Prime Minister 1957-1963). When he was in retirement, an interviewer asked him »Mr Macmillan, what was the greatest difficulty you faced as Prime Minister?« Macmillan replied »Events, dear boy, events«. The point of the story is that if we are to write convincing narratives of the past we need to be able to explain why events were unforeseen at the time, and theory is essential if we are to have the 20:20 hindsight for which historians and sociologists tend to be famous. We are essentially proponents of retrospective wisdom. You can't be wise without having some kind of theory; the question is, what kind of theory?

### **Elias's critique of sociology and history**

Well, to continue the autobiography simply as a way of joining up a theoretical argument, I returned from America and started teaching sociological theory at a British university, the University of Exeter, where I was still worrying about the macro-micro problem. I read Georg Simmel on »the significance of numbers in social life« (Simmel 1950 [1908]), and he seemed to be pointing in a certain direction, but I couldn't actually get very much further by myself. About four years later, quite by accident, I came into contact with Norbert Elias. He was then virtually unknown in Britain, and he was only just beginning to be famous in Germany and the

Netherlands. International celebrity came to Elias very near the end of his life.

Here is a strong connection with Bielefeld. Norbert Elias was Permanent Fellow-in-Residence at the Center for Interdisciplinary Research (*Zentrum für interdisziplinäre Forschung*, ZiF). Indeed he remains the only person to have had permanent status of this kind at the ZiF. As I recall, Elias was quite a significant intellectual presence here on the campus at Bielefeld. But why am I telling you this? – why is Elias relevant for the problems of history and sociology that are now being taken up anew by the Bielefeld Graduate School in History and Sociology?

I met Elias because by chance I was asked to translate Elias's little book *Was ist Soziologie?*<sup>7</sup> At first I couldn't really see what the book was about. It seemed very eccentric and obscure. It certainly wasn't the accessible introduction to sociology for first-year undergraduates that the publishers seemed to be expecting. And then I reached chapter 3, entitled *Spielmodelle*, or »games models«, and suddenly the penny dropped. Elias doesn't even call it a theory; he just says that this series of models are didactic or heuristic models. But that modest little chapter seems to me to be worth more than ten tons of philosophoidal writing on the macro-micro and agency-structure problems. Obviously I can't summarise the argument in full, save to say that the series of models shows how the interweaving or *Verflechtung* of people's intentional actions produces processes that none of them has intended. This tendency towards the production of the unintended consequences through the interweaving of intentional actions is increased as the number of participants increases – that is the insight that Simmel was groping towards. But it also increases as the power ratios or balances of power between the participants – individuals or groups of individuals – become relatively more equal. The more relatively equal the balances of power, the more there emerge structured but unintended and unplanned processes. Elias shows how it becomes more difficult for participants to put together a realistic picture, to have a map of the game in which they are involved. This links

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7 For a fuller account of this chapter of accidents, see Mennell 2006.



through, then, to problems of knowledge and ideology. Models that are couched in terms of the intentions of individuals tend to be associated with right-wing ideologies. Models that say we are all subject to the force of history tend to be on the leftward side of the ideological continuum. If you haven't read Elias – and I get the feeling that, in spite of him having been a major presence for some years in Bielefeld a quarter of a century or so ago, he is not now much remembered among historians and sociologists here – I recommend that any historian or sociologist to start with chapter three of *Was ist Soziologie?* And then you should pay some attention to the more profound development of these ideas in more difficult books like *Involvement and Detachment* and *The Society of Individuals* (Elias 2007; 2010). Of course you can also read the more famous *Über den Prozess der Zivilisation* and *Die höfische Gesellschaft* (Elias 2000; Elias 2006b). But they are *empirical-theoretical* investigations, the subject of legitimate debate among historians and sociologists concerning actual empirical evidence as well as the theoretical explanations they offer. For understanding the place of theory in history and sociology, I think the other books that I mentioned are actually more important.

So, what is the essence of Elias's critique of sociological concepts? Again, it is useful to explain this biographically – that is, to trace the roots of Elias's intellectual stance in his own early career. His views on concepts and theory go all the way back to a profound disagreement that he had with his supervisor as a graduate student of philosophy in Breslau, the neo-Kantian Richard Höningwald. The disagreement centred on the Kantian idea that the brain is, so to speak, hard-wired with fundamental categories: space, time causality, the categorical imperative in morality, and so on. Elias thought that this was entirely implausible and argued, even as a twenty-one year-old Dr. phil. student, that these categories of thought could only be understood as the outcome of the growth of knowledge over many generations – a long inter-generational learning process, as he would later call it. Höningwald refused to give him his doctorate unless he tore out the last three or four pages of his thesis, and they are lost. The thesis exists – it was rediscovered in the library of what is now the University of Wrocław, but it is minus the crucial last few

pages (Elias 2006c [1922]). The formulation found in the early doctoral thesis is perhaps a little rough and ready, but you can see which way he is heading, and indeed he developed these ideas in progressively more refined versions for the rest of his life. His argument is that the mainstream of western philosophy has worked with an assumption of what he calls *homo clausus*, the model of an individual as a closed person. It can be seen very clearly in Descartes's *cogito ergo sum*. Elias's interpretation is that *homo clausus* is not merely a philosophical error; rather, it corresponds to something real: it was a mode of self-experience that was becoming more common in the European Renaissance. That intellectuals like Descartes had begun to think in this way was a symptom of something real happening in society. Something important and enduring too, for it found its way into the dominant stream of philosophical thinking right down to the present day – and philosophers are influential among intellectuals in general. But the sense that one is a *homo clausus* is not a universal mode of self-experience. One way of looking at Elias's *magnum opus*, *Über den Prozess der Zivilisation*, is to see it as showing in a very empirical way, through the development of social standards of manners and feelings, *how* people came to think of themselves as isolated individuals. He speaks of an

invisible wall of affects which seems now to rise between one human body and another, repelling and separating, the wall which is often perceptible today at the mere approach of something that has been in contact with the mouth or hands of someone else, and which manifests itself as embarrassment at the mere sight of many bodily functions of others, and often at their mere mention, or as a feeling of shame when one's own functions are exposed to the gaze of others (Elias 2000: 60).

And yet, coming back to the philosophical mainstream, this is really the point: the image of the human being as a *homo clausus*, which runs from Descartes through Kant to the present day, is the starting point for endless circular discussions on agency and structure and the individual and society and the macro-micro problem and so on. It is also, I would say, the root from which spring such dichotomous distinctions as that of language versus practice – one of many »static dualisms« as Elias calls them.

As Elias himself puts it in one of his essays »Action theorists mentally dissect social contexts into seemingly independent actions of many seemingly independent single people. The utility of statistical investigations is based on this fictitious dissection« (Elias 2008b: 93). I actually disagree with him to some extent about the value of statistics, but there is sometimes an uncritical assumption of *homo clausus* underlying statistical work.

What I especially want to argue is that Elias represented quite a radical break. The break with the neo-Kantian or mainstream tradition, on the part of Elias and a small number of others, stands in contrast to Talcott Parsons, who in his intellectual autobiography quite explicitly says that the foundations of his theory were laid in his study of Kant at Amherst as an undergraduate (Parsons 1970). Jürgen Habermas is also quite clearly in the Kantian tradition, along with Karl-Otto Apel<sup>8</sup> and many other people who have been influential in the area of social theory. Giddens doesn't actually say he is a Kantian, but Hermínio Martins has described him as the ventriloquist of the *Zeitgeist*, and the ventriloquist of the *Zeitgeist* is not likely to make a bold and radical departure from the mainstream. (Such a ventriloquist is more likely to make a lot of money out of articulating the mainstream just before people realise it needs articulating!) The whole phenomenological stream in sociological theory – including Berger and Luckmann on the one hand and the ethnomethodologists on the other – constitute another manifestation of the mainstream. And, paradoxically, latter-day systems theorists like Luhmann, despite apparently having little place for »the individual« in their thinking, seem to me to stand in the same tradition simply because they cannot escape the static dualism of »individual« versus »society«.

Only a minority of theorists reject that central Kantian tradition. Besides Elias, Pierre Bourdieu must be mentioned; he begins his famous book *Distinction* with an »anti-Kantian theory of the judgement of taste« (Bour-

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8     Apel and Elias were fellow members of the research group on utopias at the ZiF in 1980-1981, cf. Norbert Elias, Appendix I: Note on Kant's solipsistic doubt, in Elias 2009b: 288-9, especially the note on p. 289.

dieu 1984). And the work of Bruno Latour in social studies of science would be another example. The point is, to put it simply, that it is necessary always to think in terms not of *homo clausus* but of *homines aperti* – not a »closed person« but »open people«, in the plural. One has to start from interdependent people because factually there is no such thing as an isolated human being severed from interdependences with other human beings.

I did not intend to deliver a lecture about Elias any more than I planned to give you my intellectual autobiography. Yet, listening to discussions of »theory« in this conference, I could not help but find Elias's critique of concept formation in the human sciences highly relevant (Elias 1978). His critique of sociological concepts has two main elements. One target is *homo clausus*. His other target is *Zustandsreduktion* – a term which, after lengthy arguments with him, I translated as »process reduction« rather than »state reduction«, because he is arguing that our normal way of thinking – if you like, our default setting – is to look at a process of change but then try to reduce it to static concepts. He bases this partly on a famous theory associated with the anthropologist Edwin Sapir and the linguist Benjamin Lee Whorf, the so-called Sapir-Whorf hypothesis (Whorf 1956). The argument in a nutshell, is that what Whorf calls the »Standard Average European« (SAE) languages have a tendency to indicate process by the use of a noun, a static term, plus a verb. So we have little verbal tics like »the wind blows« – but of course as soon as we stop to think about it we know that there is no such thing as a wind without the blowing. The wind *is* the blowing. And it is easy to think of sociological ideas where people try to use a static concept and then with the help of a verb make it move. It's very tricky to avoid this, actually. I'll give you one example of a vacuous concept that everyone uses – modernity. I think it is totally unusable, but it is very widely used. Even as an undergraduate I was told to be careful of even *modernisation* let alone *modernity*. Similarly, concepts like »fields« and »spheres« and »systems« and »sub-systems« all smack to my mind of *Zustandsreduktion*. Sociologists, like little boys, seem to enjoy playing with a Meccano set of concepts that they can bolt together in various ways to form buildings and

machines. I realise I'm sticking my neck out to have it chopped off in this centre of Luhmaniac affiliations, but I really don't think this is a very profitable way of writing history in particular.

Finding and using process concepts can be tricky, because we only have SAE at our disposal. Even if we use process terminology, SAE constantly exerts a pressure towards turning them into something static. Thus, for instance, in the hands of Kant, the word *Zivilisation* denoted a process. And Elias wished to retain that sense when he spoke of *der Prozess der Zivilisation*. But already, by the time he wrote his book, in everyday usage civilisation had become a noun, something static and finished. Yet there is no such thing as an uncivilised society, and there is no such thing as a perfectly civilised society. There is no zero point, and no end point; all you can do is think in terms of vectors. All societies have some rules about the things that Elias was discussing – eating, spitting, undressing and going to the toilet and so on. All that can be observed is whether and in what direction the rules – the social standards prevalent at any given time or place – are changing.<sup>9</sup> That is what I mean by thinking in terms of a vector. Alternatively, in mathematical terms perhaps it is a little bit like thinking of the first differential in calculus, looking at the rate of change.

What do process theories look like? Well, there is one major category of process theories in the human sciences, those stemming from the Marxist tradition. I suppose that reflects Marx's original intellectual debt to Hegel as opposed to Kant. But even the Marxist tradition has been subject from time to time to this kind of process-reduction and systematisation. Louis Althusser's theories seem to have amounted to Parsons with the sign changed from positive to negative. Although few people have read Althusser since he murdered his wife, it certainly is still worth reading that wonderful essay by Edward P. Thompson, *The poverty*

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9 Which is why the five volumes of Hans-Peter Duerr's polemic against Elias, published under the overall title of *Der Mythos vom Zivilisationsprozess* completely misses the point because it is just a random display of static evidence, cf. Goudsblom & Mennell 1997.

*of theory, or the orrery of errors* (in Thompson 1978: 193-397), in which he compares Althusser's system to a mechanical device with the planets circulating around the sun, and moons moving around the planets; you turn a handle and everything revolves in predestinate orbits. Edward P. Thompson seemed to me to be in many respects an excellent model of the writing of history from a processual point of view. He fully recognised the process-reducing inclinations of modern sociologists:

Sociologists who have stopped the time-machine and, with a good deal of conceptual huffing and puffing, have gone down into the engine-room to look, tell us that nowhere at all have they been able to locate and classify a class. They can only find a multitude of people with different occupations, incomes, status-hierarchies and the rest. Of course they are right, since class is not this or that part of the machine, but *the way the machine works* once it is set in motion – not this interest and that interest, but the *friction* of interests – the movement itself, the heat, the thundering noise [... C]lass itself is not a thing, it is a happening (Thompson 1978: 85, italics in original).

The twin critiques of the *homo clausus* assumption and of process-reducing concepts offer an escape route from the endless circularity of agency-structure debates. They also point to a key point of processual thinking that Goudsblom sums up in one sentence: »The unintended consequences of people's intentional actions become the unintended conditions of further actions« (Goudsblom 1977). So, there is a need for a theory, yet I've just launched a very rude attack on a whole tranche of famous theorists. What kind of theory is useful to sociologists and to historians?

### **The need for theory**

I think I can distinguish between two kinds of historical sociology or the use of sociological ideas that are useful in the writing of history. One of them I call »the sociology of the past«, which involves taking a modern sociological idea and using it in interpreting historical evidence. An example is the use of Stan Cohen's famous book *Folk Devils and Moral Panics* (Cohen 1972). It was a study of the »Mods« and the »Rockers«, who

were two groups of young people who rode Vespa scooters and motor-bikes respectively in an outbreak of juvenile exuberance in the 1960s; the Great British Public thought that the end of civilisation had come just because two different gangs were fighting in seaside resorts. But the idea of the »moral panic« has since been applied quite widely in historical writing, being applied notably in understanding the Salem witch trials in late 17<sup>th</sup> century Massachusetts. Another example is Keith Thomas's use of modern anthropological ideas about magic in his famous book *Religion and the Decline of Magic* (Thomas 1971). A third example is Kai Erikson's *Wayward Puritans* (Erikson 1966), in which he used what was then the latest thinking in sociological theory about »deviant behaviour« to interpret evidence of the life of the early Puritan settlers in New England. This book has dated somewhat, because »deviancy theory« at that time was heavily influenced by Parsons, taking for granted assumptions about »shared norms and values« from which people deviate. The fact that Erikson was using deviancy theory as it stood then in the 1960s does actually weaken the durability of the historical writing, which is itself still quite interesting. This points to the danger that theories often date more quickly than the historical evidence they are used to explain. (That being said, it should be acknowledged that Erikson's book also made a contribution to sociological theory; there is always a two-way flow yielding insights of value both to history and to sociology.)

The other kind of theoretically informed history writing, in which I myself try to indulge, is what may be called the »sociology of the long term«. I think it is quite difficult to do, and rarely yields a nice compact research topic for a PhD sociologist. And historians have tended to be frightened off this by the writings especially of Sir Karl Popper, who denied the possibility of a theoretical history and identified the attempt to formulate one with incipient totalitarianism.<sup>10</sup> Many notable social scientists, such as my teacher John Goldthorpe in Britain or Hartmut Esser in Germany,

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10 Popper 1945; 1957. See also Norbert Elias's critique of Popper, *On the Creed of a Nominalist: Observations on Popper's »The Logic of Scientific Discovery«, and »Science or sciences? Contribution to a Debate with Reality-Blind Philosophers«*, in Elias 2009b: 161-211.

became convinced Popperians, and came to distrust all uses of history in sociology. One problem is that Popper set up physics – classical physics – as a model for all sciences, and this gave many social scientists an inferiority complex. They suffer from what has been called »physics envy« (by analogy to Freud's »penis envy«).

Yet that is surely nonsense. It is certainly possible to discover patterns in long-term historical processes. Most obviously is the case of the division of labour, a very long-term process that has experienced some fluctuations, but has basically continued in a steady direction throughout human history. Other examples, in which long-term trends are subject to more marked fluctuations, are civilising processes on the one hand and state formation and the dissolution of states on the other, both of them investigated (and related to each other) in Elias's *Über den Prozess der Zivilisation*.

In the classical philosophy of science, prediction can be divided into two parts: *prodiction* and *retrodiction*. *Prodiction* actually means prediction in the ordinary sense: predicting the future. *Retrodiction* means the intellectual prediction of what we might find in the evidence of the past when we come to investigate it. It is possible to predict – on the basis of our present knowledge and our theory – what is going to be found in historic archives, for example. Popper was ideologically opposed to any attempt at large-scale *prodiction* of the future. I think we can sometimes do that to a limited extent. The science fiction writer Arthur C. Clarke was famous for predicting some scientific breakthroughs long before they happened, such as television broadcasts from geo-stationary satellites; he did not just guess at random, but rather knew a great deal about science, and that together with a novelist's imagination enabled him to make some strikingly bold *prodiction*s. But sociologists and historians are on rather stronger ground when it comes to *retrodiction*; we are often accused of having retrospective wisdom. Even that may be extremely useful, however. Take the example of the current banking crisis. History, sociology and economics (if economists are so inclined) can help to explain to politicians how we got to where we are and why we went wrong. There are links to be made to different kinds of speculation and different



episodes. The knowledge that we generate ought to enable people to avoid making the same mistakes again – though of course it often does not. I have recently been reading Galbraith's *The Great Crash 1929*, which is a best seller once again. It is quite gripping, because the parallels between the idiocies of the late 1920s and the even grander idiocies of today are really very obvious. Our present masters of the universe have discovered much more sophisticated ways to make the same mistakes. Galbraith, in a preface written for a new edition in 1975, rightly claims that there is merit in keeping alive the memory of the greatest cycle of speculative boom and bust since the South Sea Bubble. For a generation after 1929, politicians and businessmen remembered the crash and avoided the mistakes. But, by the 1960s »this memory had dimmed« and »almost everything described in this book had reappeared« (Galbraith 1975 [1954]: 9). By the first decade of the 21<sup>st</sup> century total amnesia had set in among the people with the power to cause economic catastrophes. One must, of course, always ask the classic question *Cui bono?* There are reasons for forgetting. A lot of people made a lot of money out of forgetting the lessons of 1929. As Karl Deutsch remarked, power is »the ability to talk instead of listen [and] the ability to afford not to learn« (Deutsch 1963: 111).

It may only be a satisfaction to us as historians and sociologists, but it does give some kind of intellectual satisfaction to be able to explain where they went wrong, what the players in this disaster did not know. Probably the bankers do not want to know it, but it is some sort of intellectual satisfaction to us to be able to point out that Adam Smith was fully conscious of the need for government and aware that the markets did not entirely work on their own: the hidden hand operated within boundaries set by law and government. But the extreme laissez-faire reading of Smith has come to dominate. Much more recently, but still more than half a century ago, Karl Polanyi in his book *The Great Transformation* made some points that now seem utterly relevant (Polanyi 1944). He demonstrated vividly that markets are always embedded in wider social arrangements. He showed how the Gold Standard was a convention, a tacit agreement between governments and bankers to fol-

low a system of rigid rules, which imposed a kind of intense self-restraint upon them, but threw the burden of adjustment to the trade cycle on to the mass of the workers through vicious cycles of unemployment.<sup>11</sup> Polanyi's book was on the reading list when I was an economics student more than forty years ago, but people didn't bother to read it because Polanyi seemed to be telling them things that everybody knew. Well it now turns out that not everybody any longer did know, and *The Great Transformation*, like *The Great Crash*, is attracting new readers today.

I would argue that historians and sociologists, working together, could provide a useful critique for policy makers. I don't mean »critique« in quite the sense that the Frankfurt school meant critique – not an ideological critique, although that might come about as well – but a practically useful critique, a sort of social historical equivalent to psychoanalysis perhaps. That could be far more useful, but less comfortable, than much of the current limited and myopic social research for which governments will pay large sums of money – and thus determine our intellectual agenda for us. (That is a *cri de coeur* from the sociologists probably more than from the historians.)

### **The maximalist programme for history and sociology**

I said at the beginning of this talk that I am an advocate of the maximalist programme for history and sociology, a proponent of the univer-

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11 A certain parallel can be seen in the consequences of the Bretton Woods agreement, which are now attracting renewed and widespread interest. Contrary to popular myth (at least in Britain), it was not John Maynard Keynes's proposal that was accepted, but rather the American model, which was perhaps intended primarily to avoid the terrible traumas of the Great Depression ever occurring again in America. It made the dollar the world's reserve currency, and the USA the world's banker. In the medium term, that brought enormous benefits to the living standards of Americans, but, like the Gold Standard, threw the burden of adjustment to trade cycles on to the poor: the poorer parts of the world in this case. In the longer term, it appears to be highly destabilising for the USA, see Liaquat Ahamed, *The Future of Global Finance*, *New York Times*, 20 September 2009.

sal relevance of sociology and theory – at least if it's the right kind of theory – for the writing of history, as well as the relevance of history for writing sociology. I recently glanced again at John Hall's book *Powers and Liberties*, in the introduction to which he tells a story about Oxford history as it was in the 1960s. He relates:

Whilst an undergraduate at Oxford studying history, a close friend was asked to write an essay on the social origins of the Third Reich. He spent a week reading about unemployment, working class authoritarianism, inflation, reparations, lower middle class anti-communist attitudes and the like and produced an essay with these factors very much in mind. His tutor pounced, delighted at this deliberate hoax, designed to teach a lesson, delighted that it had worked so well. What was the lesson? There *were* no social origins of the Third Reich, the tutor insisted, merely particular moves made by specific actors, especially Von Papen, and these political manoeuvrings were the real cause for Hitler's accession to power (Hall 1985: 1).

Of course, there's no need to explain to this audience what nonsense that is, but there is a grain of truth in it as well, because social processes, social factors are ultimately the product of the interweaving of the plans and intentions of many people. Again, to quote Elias »underlying all intended interactions of human beings is their unintended interdependence«. <sup>12</sup> So, obviously, whatever theory we are using there is a need to explore factual interdependences, the power balances that link people and groups in whatever time and place we are studying. Sometimes we may be able to explain how there arise the unanticipated »events«, to echo Macmillan's aphorism. Again, a couplet that I like from Elias:

From plans arising yet unplanned  
By purpose moved yet purposeless (Elias 1991: 64).

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12 Norbert Elias, *Sociology and psychiatry*, in Elias 2009a: 175.

So we need to study the power ratios, go back to the game models and the proposition that the more equal the power ratios and the longer the chains of interdependence, the less planned the overall process is – yet it can be made intelligible.

Actually, what I have just said is probably obvious to historians; historians do it all the time without necessarily conceptualising it in those terms. That's what history consists of, but sociologists don't always see these things so clearly, particularly if they read too much contemporary social theory. We also need to look at knowledge and assumptions and perceptions. Knowledge: the theme of one of the workshops at this conference (although the convenors wondered whether it was quite the right term – but it will do for the moment). Obviously, the further back we go in time as historians the more important it is, and the more difficult it is to understand what people knew. The less can it be taken for granted that we know what people knew. That is clearly the case in medieval and early modern European history, for example, where increasingly the need for the ethnographic skills developed by anthropologists for the study of non-European societies are seen as relevant. But Darwin's bicentenary in February 2009 reminds us how difficult it is now, looking back to what seems like only yesterday, the mid-nineteenth century, for us to understand why even the Victorians thought as they did. Why were theological modes of thought and knowledge, and the use of them against natural scientists, so much more marked than we can conceive now (except among the very numerous American hillbillies, of course)? Or take the whole question of »rational choice« theory. It really is the greatest nonsense, but it seems to work in the short term for so many things. Yes, human beings always had the intellectual equipment to pursue their goals rationally, but the bigger problem is to understand what they wanted and why they wanted it in the more distant past.<sup>13</sup> For

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13 Another example from Elias is relevant: his discussion in *The Court Society* of »court rationality«, which from the point of view of rational bourgeois rationality looks irrational. They spent money that they didn't have, they spent – rationally in their terms – in order to maintain a rank and to consume in relation to their rank rather than trimming their expenditure to their resources.

all its glitter, it seems to me that rational choice theory is still vulnerable to Lord Macaulay's jibe that what the theory really amounts to is the proposition that »a man had rather do what a man had rather do« (Macaulay 1889 [1829]: 180). Translating that into more modern English: a person will do what he wants to do. The historical and sociological task is to explain why and what »he had rather do«, and that is not something to be taken for granted.

Let me conclude with another case study, on the question of knowledge and perception. In the last chapter of my most recent book, *The American Civilising Process*, I look at the effects of unequal power ratios on Americans' perception of themselves and the wider world. It seems to be a general principle that the more unequal the power ratio between two parties, the more the perception on the part of the more powerful party becomes distorted. You can obviously reach this principle from Hegel's famous discussion of the master-slave relationship, but I actually reached it through a study of a refuge for battered wives in Amsterdam written by two Dutch friends (van Stolk & Wouters 1983). What they found was that if you asked the wives who had been beaten up by their men to write a character sketch of their man, they could give a lot of detail about the men's personal idiosyncrasies, behaviour, what got them riled, and so on. If you asked the men to describe their women, all they could do was to speak in terms of stereotypes of »the little woman«. This seems to be a principle of wide application. For example, I am a British person living in Ireland. The Irish know everything there is to know about the goings on in their neighbouring more powerful country, Britain. Talk to a Brit, they still have old out-of-date stereotypes of Ireland – people riding around on donkeys and so on. In the case of the USA, it seems to me that Americans always see themselves as the champions of democracy, even though history shows their record to be distinctly mixed. The key element that has shaped American character in the very long term, I would argue, is that America has continuously become more powerful vis-à-vis its neighbours. The Pilgrim Fathers appear to have been briefly dependent on the Indians, but the New England settlers were very shortly fighting the Indians in King Philip's War, and there followed westward

expansion in which Native Americans were almost wiped out, and then the gradual emergence of the USA as a world great power. Now the whole world watches America – following its elections in great detail for instance – but Americans’ perception of the wider world is not reciprocal. It is as though they are sitting in a brightly lit social psychological laboratory. We, on the other hand, are sitting in a dimly lit observation room behind a one-way mirror, and when they look towards us they only see their own reflections. I give you a concrete example: why do the Iranians hate the Americans? Americans tend to be very much puzzled by that, thinking »they must be evil people if they don’t like us«. But if you ask even a highly educated American whether he or she knows anything about Mohammed Mossadeq, the answer is likely to be, »Who?« Mossadeq was the democratically elected prime minister of Iran in 1951-1953, who was overthrown in a *coup d’état* arranged by the British and the CIA, in the interests of preserving Anglo-American control of Iran’s oil-fields.<sup>14</sup> The Shah’s murderous regime was reinstalled and not overthrown until the Iranian Revolution of 1979. Ask any Iranian about Mossadeq and they know all about the coup of 1953. A similar asymmetry could be found between the United States and any number of countries in Latin America.

A further small extension of this line of argument throws light on the consequences of the end of the Cold War. The collapse of the Soviet bloc in 1989-1990 at first led only to intellectual triumphalism in America, like Fukuyama’s notorious essay *The End of History* (Fukuyama 1989). But the removal of a major external constraint upon the behaviour of the USA – just like the reduction of regulatory control over the bankers mentioned before – had consequences in gradually changing behaviour. Just like a small child, bankers or American presidents began to explore what they could get away with. Eventually, the administration of George

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14 It is symptomatic in 2009/2010 that the Wikipedia entry on Mossadeq had a notice at the top reading »The neutrality of this article is disputed. Please see the discussion on the talk page.« The talk page reveals that some American readers found the factual history of this episode unpalatable.

W. Bush decided it could get away with a war of choice in the invasion of Iraq.

What I'm saying is that this hypothesis about the connection between unequal power ratios and perceptions that lack congruence with reality is a fairly simple sociological idea – only a middle range theory – but it seems to me to be quite illuminating and of wide application. It helps to explain quite a range of things. But it is not a huge great theoretical system, a crate full of boxes within boxes within boxes.

### Conclusion

In conclusion, I'd like to make an observation based on my experience of doctoral students in sociology. My own students, particularly at the Masters level, but also at the doctoral level, now seem to think that it is uncool to commit yourself to a particular theoretical perspective. They may have picked up this idea from teachers of my generation, who remember »the war of the schools« which virtually destroyed British sociology, maybe German sociology at times as well, when departments blew up and were closed down in British universities because staff felt passionately committed to one or other »school« of theory – ranging from positivistic survey research through the 57 varieties of Marxism to phenomenology and ethnomethodology. Sometimes it went to the point where they could not speak to each other at all. Boom! – departments of sociology exploded. Thank goodness those days are past. But one consequence seems to be that students are hesitant to take a theory and explore it and see how it fits and how it can be developed and how it can be modified. Instead what they tend to do in the introduction to their thesis is say to themselves, »Oh, you've got to have a theory chapter, right?« So, whatever their specific empirical topic, they write a list: what Habermas says about it, what Bourdieu says about it, what Foucault says about it, what Elias says about it, what (if you are German) Luhmann says about it, what Uncle Tom Cobley and All say about it. And when they have written the list, they disappear into their data and that is the last you hear of any theoretical perspective. I do not think that is the way to proceed. I think you should let your empirical research lead you to a

relevant theory with the potential to gear into empirical evidence – to make connections for you. It should not be something up in the air like Anteaus being held clear of the ground by Hercules. It has to be some hypothesis, some question, some insight that has quite clear implications for what you might expect to find in your archive or other source of evidence. Then pursue the theory, develop it, use it, and if necessary reject it in due course. At least you are using theory, whereas so many of my students think it's a sort of exercise to be written at the beginning of the thesis and then to be forgotten about. It's probably not a danger here, because you all seem to be well and truly dug into the lasting influence of Niklas Luhmann. I hope I've explained why I'm sceptical about grand theory, but still think that theory is essential both in sociological and historical research.



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