The future of sociology: extinction, stagnation or evolution?

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The future of sociology: extinction, stagnation or evolution?

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One likely first response to the title of my lecture is: ‘Why even bother to ask that question?’ Such a response is completely understandable. Nonetheless, there are several reasons why it is appropriate to speculate, in a critical frame of mind, about the future of sociology. In the first place, it is a sensible ontological presumption that we should not take the continuing existence of anything for granted. As humans, and despite everything that we know to the contrary, we tend to live our lives as if the world that is presently around us is a more-or-less permanent state of affairs; but nothing is actually permanent. This is the shared lesson of all the academic disciplines: everything changes. Nothing persists in an unchanged state indefinitely.

In the second place, and accepting the first premise, we cannot predict the future. We can make educated probabilistic judgments, on the basis of the best available evidence and our theoretical understandings of how things work. On the basis of those ‘predictions’ we can take interventionist action in order to shape future events. But we cannot know the future (although we do expect - within limits, and as long as we are alive and conscious - that there will be one). This discomforting aspect of the human condition is further exacerbated by one of the few sociological laws with any universal application: whatever can go wrong will go wrong, and everything can.

These are rather general propositions. It might be argued that they have no specific or necessary application to sociology (other than by virtue of the fact that they apply to everything). There is, however, a third reason for questioning the future of sociology that is both specific and necessary: there are causes for realistic concern about the present state of sociology and its future as a sustainable intellectual and academic enterprise. This is the theme of this lecture, which I shall examine through the lenses of the three logical alternative disciplinary futures: extinction, stagnation or evolution.

**Extinction?**

This is an appropriate place to start this discussion, if only to remind ourselves that we should take nothing for granted: what once was made can be unmade. Compared to many other academic subjects, sociology is a relatively new discipline. The word itself derives from Auguste Comte in 1838, and something called sociology only became institutionalised as a mainstream university discipline - defined somewhat imprecisely as a teaching activity that, in one form or another, can be found in a majority of universities - during the second half of the twentieth century, after World War Two. This suggests that, as a newcomer to the intellectual landscape, sociology’s place in the educational firmament may still be insecure. Are there any reasons to think that this might in fact be so? The answer to this question is yes, there are.
Institutionally, we do not occupy a secure niche: other disciplines, in one way or another, can lay claim to some, or even much, of our intellectual project and inheritance. There are those subject areas that have, at least in apart, emerged from sociology. Cultural studies is just such an offspring of sociology - although it had other parents, too, particularly in the literary field - and shares with sociology intellectual ancestors, substantive concerns and qualitative methods. Something the same is true of media studies, and both can be said to have returned the favour by cross-fertilising sociology, often under the signs of postmodernism or ‘the cultural turn’. The broad field of business, organisation and management studies developed by standing, *inter alia*, on the shoulders of the sociology of organisations. This is a cautionary tale: the sociology of organisations, despite its distinguished place in the intellectual history of sociology, has declined over the last three decades or so, as business schools have become essential to universities’ strategies for achieving ideological acceptability, enhanced impact ratings and increased public and private sector funding.

Then there is a set of cognate social science disciplines that historically developed more or less simultaneously, on parallel tracks, and have much in common with each other. Human geography is one such: it shares a good deal of its theory and methods with sociology, to the degree that some human geography is pretty much indistinguishable from sociology (or *vice versa*, if you prefer). The historical and intellectual relationships between social anthropology and sociology are even closer; trained as a social anthropologist but with a career in sociology, I personify this relationship. The two disciplines emerged at the same time in Europe and the United States, shared common intellectual ground during those pioneer years, and developed qualitative research styles side-by-side. The biggest, most obvious, difference between the sociological ethnography of the Chicago School or Whyte, for example, and the anthropological fieldwork of Malinowski or Mead (Margaret, that is) is that between pool halls and hustlers, on the one hand, and spears and loincloths, on the other; between ‘street-corner’ societies and ‘primitive’ or ‘savage’ societies. As anthropology has increasingly turned its attention to urban, industrial and western societies, even that contrast has increasingly been less important (although anthropology has sought, somewhat anxiously perhaps, to distance itself from sociology with respect to theory: Freud’s remark about the narcissism of small differences comes to mind).

This group of historically cognate disciplines has at least three other members. Social psychology has an interesting relationship with sociology, in that symbolic interactionism, in particular, happily identifies itself as a school thought within both psychology and sociology; and with (G. H.) Mead as an intellectual ancestor in common, why should it not? Moving on to the study of politics, as an empirical rather than a philosophical pursuit this shares much in the way of
survey methods with quantitative sociology; more ominously, perhaps, political sociology has declined in importance - in much the same way as the sociology of organisations - as political science has waxed in significance and influence. Finally, there is social policy and social administration (often known as public policy in the United States), perhaps also including the professional training of social workers. Here the relationships are sufficiently close that in many universities sociology, social policy and sometimes social work co-exist under one departmental roof: this is the case here at Aalborg, and also in my own department in Sheffield.

So sociology has intellectual overlaps with all of the above - and I could easily have mentioned other disciplines, such as social history or development studies - and competes with many of them for students. But why should this be a problem for sociology in particular? After all, it might be thought that the overlaps mean that they all have a similar problem in this respect. In fact, this is not the case, because sociology is the only one that overlaps with all of the rest; this is because it is the widest ranging and most general of these intellectual enterprises. This is a great strength in my view, in that in principle it encourages a synthesising and holistic view of the human world that is without rival. But it also means that, in addition to competition for students and resources, there is a very real boundary issue with respect to disciplinary identity. Does sociology have a strong enough sense of what it is and what it does? And do other people have a strong enough sense of what is distinctive about sociology? I shall return to these issues.

As if the situation is not bad enough - if my argument above is correct - there are other institutional and political problems. With respect to student demand, which is, after all, the bottom line, two such problems are conspicuous at the moment. One is simply the likelihood of an overall downturn in student numbers as a consequence of demographic change as smaller appropriate age group cohorts feed into the higher education system. The other is the increasingly vocational nature of student choice-making in a world in which simply being a graduate is no longer a guarantee of significant labour-market advantage (or, more to the point perhaps, the relationship between the costs of being a student and the returns on being a graduate are under scrutiny as never before).

That these threats apply to most humanities and social science disciplines should not make us complacent, because sociology may not be in quite the same situation as some of those subjects. For example, sociology arguably lacks cultural capital and social status, compared to either ‘traditional’ disciplines such as history, the languages or philosophy, or a ‘modern’ discipline such as political science, with its links into the elite worlds of government and the media. Inasmuch as sociology is in direct competition with everyday common sense, as a source of understanding of the human world, it is vulnerable to criticism as ‘not a proper discipline’, ‘easy’, or ‘soft’. And finally, the
political hangover from the 1960s and 1970s lingers, in that sociology is seen by some as left-wing, and is easily confused by the ill-informed with socialism.

However, all is not gloom and doom, because total extinction seems to be an unlikely prospect. If for no other reason, we should not neglect the power of institutional inertia: organisations, particularly public-sector organisations, are fundamentally conservative, and any established activity is part of the organisational status quo. What’s more, axing an activity involves a set of costs that should not be underestimated. However, institutional inertia cannot be relied on, either: there are too many examples of institutional restructuring - affecting sociology - to permit complacency.

More significantly, once an intellectual Pandora’s Box is opened, it is difficult, if not impossible to close, as long as the ideas in question continue have some application to the world as it is known at the time. Ideas may diminish in visibility or significance, but they are difficult to exterminate; intellectual disciplines may shrink, but they are hardy. Disciplines such as divinity, philology and classics, which were absolutely central to nineteenth-century universities are still with us, even if they have become rare or even apparently endangered species.

As a more pertinent example, even when sociology was apparently extinguished by ministerial decree in Denmark in the late 1980s, sociologists did not, of course, vanish. They sought intellectual asylum in research institutes and other disciplines - a situation in which the overlaps discussed above were useful - and the resurrection came in time (and actually rather quickly): today, sociology may even be expanding in Denmark. One of the main reasons for the survival of Danish sociology is that the academic world has always been international and today is even more so, thoroughly globalised (even if national intellectual traditions do, thank goodness, survive). The discipline of sociology continued elsewhere: as long as it did, its return to Danish universities was almost inevitable.

These arguments do not, of course, mean that the discipline of sociology must survive. There is no inevitability about this, and we should not be complacent. Instead, they raise another important question: what kind of sociology will survive, and how will it survive? Which immediately suggests another question: does it really matter whether something called ‘sociology’ survives? The answer is that it does not - or at least not much - and that for several reasons. First of all, some perspective and some modesty are necessary here: sociology may be important as an aspect of the collective reflexivity that is intrinsic to modernism, but it is only one aspect of that reflexivity. Sociology could vanish and most people would not even notice its absence; what’s more, there are many more significant and more pressing global issues than whether or not sociology has a future.
To continue on a modest vein, it’s not the disciplinary name, or an institutional presence, that matters anyway, it’s the activity: that most, if not all, of what sociology currently does could carry on under different disciplinary umbrellas is one of the lessons of the overlaps discussed earlier. This returns the discussion to the discipline’s boundaries, because much of what sociology does is carried on under different disciplinary umbrellas: sociologists work in many different intellectual contexts, which are not called sociology. In addition, another aspect of the boundary issue is that many sociologists have some or all of their background in other disciplines. Sociology may, in fact, be one of the very few disciplines in which it is possible to work - in my case as a full professor - without ever having been trained in it. The fuzzy osmotic boundary is not merely organisational or institutional, it is also intellectual. And a boundary issue need not necessarily be a boundary problem: the uncertainty about what is and is not sociology may be creative, enriching sociology intellectually as well as in terms of personnel.

So the biggest potential problem is not that sociology will vanish. As I hope I have shown, this prospect is not something that should keep us awake at nights; at least no more than the possibility of an asteroid hitting the Earth should. Rather, the biggest potential problem for sociology and its future health is that it will survive in its present form. This leads me on to the next possible future scenario: stagnation.

**Stagnation?**

At this point it may be appropriate - indeed a little overdue - to ask, ‘What is sociology?’ I shall not rely on well-known normative definitions of what sociology ought to be, such as those offered by Durkheim or C. Wright Mills; I shall look at what sociology ought to be, later. Instead I want to look at what it is, in the here and now. There are some problems with adopting this approach, most notably the fact there are different national or regional traditions. In practice, sociology may be a somewhat different discipline in different places. Allowing for that caveat, some defensible generalisations can be offered.

At its most fundamental, sociology is the study - by which I mean the theoretically informed analysis of systematically assembled empirical material - of the collective or non-individual dimensions of human life, however those are defined; whether as patterns of behaviour, social trends, institutions, interpersonal interaction, or shared ideas and concepts. These are typically summarised by the somewhat metaphysical notion of ‘society’. Two shared themes run through this study of the human world: the relationship between the individual and the collective dimensions of
life, and social change. What have been the most important patterns and trends in this activity known as sociology since it first appeared as an infant university discipline at the turn of the 1900s?

Sociology split relatively early into schools of thought that often owed - and indeed still owe - as much to politics and ideology as anything else. It also split into quantitative and qualitative sociologies, divorced to a degree which means that there are today very few sociologists who can convincingly claim dual methodological citizenship. Although I know that I would say this - as a qualitative researcher, in the main - but quantitative research has arguably become primarily a matter of excellence in technique, a triumph of form over content. It has also, in the US for example, become a localised yardstick of sociological status and reputation. Alongside methodological fission, and related to it, the development, early on in sociology’s history, of specialised sociologies of ‘this and that’ has created intellectual bunkers and silos, between which communication rare and generally stilted.

Perhaps the most ruinous split, however, is the chasm that has opened up between empirical sociology and sociological theory. There are a number of observations to make here. To begin, although empirical sociologists draw on theory as a frame of reference and a discursive lexicon, as the necessary ornamentation that is required if they are to publish successfully, they rarely test it in any rigorous fashion. Paradigms are chosen and worked within, and generally confirmed. While it may be understandable that meta-theory is generally taken for granted (it is probably not meant to be empirically tested, after all), the same is often depressingly true of what Merton called ‘theories of the middle range’, which are explicitly intended to guide research and to be tested. This situation is almost a caricature of Kuhnian ‘normal science’.

The next thing to point out is that the teaching and elaboration of research methods has become a specialist institutional bunker in its own right. As a career strategy it may sometimes be more useful to know about research methods than to do practical social research: armchair methodologists whose specialism is teaching methods rather than doing research are not unknown. This situation may have developed, at least in part, because something called ‘social research’ now spans a number of disciplines: there are no specialist sociological methods.

Finally, theoretical sociology has become part of a broader enterprise called ‘social theory’, a self-contained intellectual pursuit that brings together philosophy, politics and other disciplines as well as sociology. This is meta-theory - what Mills called ‘grand theory’ - and not only is it empirically untested but, as suggested above, it is probably, given the ways in which its propositions are formulated, untestable. It is, however, striking that, although questions about the nature of social phenomena might appear to be central to sociological meta-theory, ontological
questions are rarely addressed these days. Particularly since the ‘structuration debate’ ran into the buffers, the view seems to be either that these issues were solved long ago, or that we should agree to differ and get on with other things (a ‘normal science’ paradigm effect). So rather than attempting to clarify ontological fundamentals, social theory has instead become focused on the contemporary diagnosis and prognosis of modernity.

Most damagingly, this social theory of modernity has become almost completely divorced from the constructive constraints that might be provided by a close relationship with empirical research. The void is filled by speculative essay writing, decorated by anecdotes and context-free informational nuggets and passing for the serious social critique and interpretation of modernity: Bauman on liquid modernity, Urry on mobilities, Calhoun on cosmopolitanism, Giddens on self-identity and the transformation of intimacy, Habermas on the public sphere, the list could be very long. Even those social theorists, such as Bourdieu, who have explicitly attempted to resolve this divorce between theory and empirical research have arguably failed, perhaps because of the seductions offered by the intellectual and professional kudos that are attached to being ‘a theorist’.

In Bauman’s terms social theory has ceased to be ‘responsible speech’: it is accountable to nothing. Armchair theory matches armchair methods.

As part of this process, social theory has become a self-referential - and extremely self-important - conversation with itself. Or, rather, social theorists talk to each other more than they talk to anyone else. And they talk about each other, rather than about the empirical world of human experience. Social theory has become a self-perpetuating and self-validating enterprise, and a space within which nonsense can - at least for a while, and if it is applauded - seem to make sense. Hans Christian Andersen’s story about the Emperor’s New Clothes resonates as loudly today as it did in the nineteenth century.

The tax-paying wider public, and our institutional and political paymasters, are systematically excluded from this conversation - and one is almost tempted to be grateful that they are: what would they make of it, after all? - by the language in which it has become almost de rigeur to write serious social theory. The denser and the more difficult the text the better; impenetrability has become an index of an author’s serious intent; worse, pharisaic argument has become an essential element of any claim to political or social radicalism; and if the poor, stupid readers cannot understand it, the problem is, of course, them and their incapacity or incompetence. Despite - or actually because of - the linguistic hedge of thorns that surrounds it, social theory has become the arena par excellence in which big sociological reputations are made. It has also become a prime source of professional cultural capital - and even social capital - for the followers and disciples of the great theoretical
prophets. Apropos nonsense, part of this has to do with the status that may accrue to those who appear to understand really difficult esoteric doctrine.

Sociology does not deal in dramatic empirical discoveries: there is never going to be a sociological equivalent of the Higgs boson, or penicillin, or a new species of feathered dinosaur, or even new archival material casting a key historical event in a new light. So, in the absence of the discipline provided by the need to respond to and understand empirical data, the main motor of apparent theoretical change has become fashion and fad. The fate of postmodernism, and the host of other post-isms that prospered during the 1990s, is an instructive case in point: the sociological herd grazed...and then moved on to theoretical pastures new.

Because the everyday empirical issues for which an analytical framework is required do not change that much, this non-cumulative theoretical instability leads to the regular, enthusiastic, and usually completely unacknowledged - perhaps even un-noticed - re-invention of this or that conceptual wheel. As an example, take the theoretical proposition that ethnic identity, far from being primordial and imperative may, depending on the situation, be relatively flexible and negotiable: Everett Hughes proposed this in the late 1940s, Fredrik Barth, apparently independently, said the same thing in the late 1960s, while postmodernism - surprise, surprise - trumpeted this as a radical new insight in the 1990s. That this was all done in good faith would be entertaining if it wasn’t so depressing.

Claims to theoretical novelty are also often achieved through the simple device of neologism. New names, and novel presentations, are given to long-established favourite dishes on the sociological menu: ‘intersectionality’ and ‘intersubjectivity’ are merely two of my least favourite bugbears of the moment, but I could easily highlight others. The pursuit of superficial intellectual novelty - without which the market in reputations that governs the social theory field would not work - masks a perpetual intellectual merry-go-round in which the dead fall off their horses and are replaced by fresh-faced recruits armed with new words, but old ideas. The collective intellectual amnesia that results from, and feeds, this relentless and restless danse macabre is among sociology’s greatest vulnerabilities.

This is perhaps enough on this topic. Stagnation is in the eye of the beholder, I am aware, but there is a normal science paradigm here that has become settled and staid; that lacks substantial innovation while, only seemingly paradoxically, being in thrall to the trumpeted appearance of novelty; that rewards elitist and exclusionary intellectual obscurantism rather than intellectual democracy; and that serves to distance sociology, and worse sociologists, from those citizens whose taxes underwrite the whole enterprise.
I do not, however, wish to suggest that there has been no innovation at all since the ‘foundational age’ of pioneer sociological theory and methods. This era lasted until the 1950s, perhaps into the 1960s, during which the discipline achieved its present shape and content in most important respects. So I want to draw attention to perhaps the only outstanding genuine example of innovation - with respect to both theory and method - that has occurred during the last fifty years. This innovation had ideological roots in early twentieth-century politics and the social movements of the 1960s, and gathered pace during the 1970s. It was stimulated by post-1945 changes in participation in higher education and policy throughout the western world, and by scientific, economic and social change during the 1960s. It was rendered utterly irresistible by the relentless logic of a simple empirical fact, that half of the human population was effectively being ignored by a discipline that claimed to study ‘society’ in all of its aspects.

The second-wave feminism of the women’s movement demanded the recognition of women as adult actors in the human world, and hence, for sociology, as research subjects and as colleagues. It is no exaggeration to say that this shook discipline to its foundations, intellectually, academically and professionally; it has certainly never been quite the same since. Recognising the ‘other half’ of the human world was, what’s more, a qualitative as well as a quantitative change and it affected both theory and method. It is also true to say, however, that feminist sociology - and interdisciplinary ‘women’s studies’ more generally - has become institutionalised as its own normal science. It has generated its own *hermetique* of social theory, some of which is altogether incomprehensible, if not nonsensical, and much of which is written in thoroughly exclusionary language.

Despite that somewhat sour note, the welcome lessons of the feminist critique of sociology, in terms of method and theory, are that change is possible, but that it is not theory that drives that change: rather, intellectual change in sociology is the result of events in the social world and the sociological collision with stubborn empirical facts. This brings me on to my final prospect for sociology: evolution.

**Evolution?**

It is obvious from the previous section that I think that sociology needs to move on; that it actually needs to change for the better. So in what respects do I think that this evolution needs to happen? I shall begin with research methods, not least because I think that empirical research is, or should be, the foundation of the discipline.
It has become conventional wisdom to pay lip service to the proposition that quantitative and qualitative research methods need to be reunited and that thorough training in both should become the disciplinary norm. Well, it is time to move beyond the lip service. What we can do to achieve these goals, however, beyond increasingly loud and shrill exhortation - which we know won’t work, anyway - is not wholly clear. Nonetheless, we can give the matter some thought.

To look at quantitative methods training first, perhaps we need to start from somewhere other than the present common presumptions that (a) this stuff is really difficult and (b) students are resistant to learning how to do it. If it is presented as complicated and abstruse, something that only special beings with higher maths skills are any good at it, resistance will be created. But how many quantitative methods trainers start out by telling their students that quantitative research is not so difficult, and can be great fun? That’s got to be an approach that’s at least worth trying. Part of the problem here is that quantitative and qualitative sociological research have been allowed to become almost separate disciplines, with the latter seen as the elite, minority pursuit. However we do it, we need to demystify quantitative research and make it more accessible (because most of it is not that difficult).

On the other hand, qualitative research training needs to progress beyond reflexive story-telling (necessary as an experience-based approach is), to become more ambitious in the range of approaches that it embraces, and more demanding in terms of the imagination and sophistication we expect from students (and, it must be said, from many professional practitioners of the qualitative arts). The semi-structured interview that seems to have become the ‘industry standard’ is only one approach among many, and not the most exciting. So qualitative research needs to be de-routinised and its full spectrum of options restored as practical alternatives, rather merely something to read about in order to pass the Quals module.

Finally, with respect to both quantitative and qualitative research, at the more advanced level we need to resist the current shrinking and deskilling of PhD research, which has been shoe-horned into a standard straitjacket model, dictated by financial constraints and bone-headed, if well-intentioned, political interventions (in the shape of completion targets). We cannot expect our postgraduate researchers to produce exciting, challenging work if they are working to a template of what a thesis should look like - two chapters of literature review, three chapters of findings and one of discussion, if current British practice is anything to go by - and a minimalist yardstick of how much research, in terms of number of interviews or questionnaires, or length of observational time, for example, is sufficient.
In principle, all of the above are achievable (although whether they are likely is a different matter). Changes in the realm of theory are more problematic, however.

Most fundamentally, social theory needs to be redefined as the servant of empirical research, informed and shaped by empirical research, rather than being allowed to continue as an insulated high-status sphere of self-absorbed activity in its own right. If this is interpreted to mean we should get rid of ‘social theory’ as such, so be it. The intellectual stratosphere is not the best home for sociology: our mission involves observational engagement with the hurly burly of everyday life, documentation in detail of middle-range patterns of behaviour, and the identification and interpretation of emergent secular trends in institutionalised processes and outcomes. We need to get our hands empirically dirty, and the fastidious intellectual preoccupations of social theory as it is currently practised are ill-suited to this task.

Change of this kind presents a very difficult prospect: eminent people do not cheerfully surrender their eminence, institutional inertia is hard to overcome, a university that has just spent a small fortune to attract an internationally renowned social theorist is not likely to be sympathetic, and to judge from publishers’ catalogues and journals there appears to be a thriving market for this stuff. Why that market persists might make an interesting PhD topic. A disciplinary debate about this is certainly required (although it is also tempting to ask whether, given that C. Wright Mills couldn’t change this, there is anything else that can be done: we cannot say that we do not already know about this issue).

To compound the situation, another problem presents itself. If I am correct in suggesting that sociology does not really advance by radical empirical discovery - such as might be produced by new experiments, or new investigative technologies for studying the world - how does, or should, it move forward? Realistically, contemporary sociology adds to itself, and to the stock of knowledge, by either examining stuff in the human world that it has previously ignored (atheism is a good example of a significant social phenomenon waiting for its sociology), or by observing, and engaging with, changes in the human world (of which globalisation is a good recent example). This has not always been the case, however. For the first sixty or so years of their histories, sociology, social anthropology and the like experienced a period of fertile theoretical and methodological innovation and development that consolidated these disciplines as practical intellectual enterprises. This period arguably ended in the 1960s. At the risk of being controversial, and with the notable exception of the impact of feminism, the period since the ’sixties has seen the regular reinvention of the theoretical wheel that I have already described without anything particularly new or radical appearing.
If we want to get away from retreading the past in the pursuit of superficial and illusory novelty, on the one hand, and the self-absorption of empirically disengaged theoretical grandeur, on the other, perhaps we need to become comfortable with a more modest idea: that we have an existing toolbox of basic concepts and methods with which to document and analyse the human world as it changes. In this prospect, sociology becomes an intellectually acute and methodologically sound analytical (and critical) commentary on the human world as it changes, and a peerless contribution to the future’s historical record. This will not be everyone’s glimpse of a desirable future, but it would at least be distinguished by the virtues of intellectual good faith and practicality. It might also encourage us to develop better public engagement than we currently manage.

Adopting such an approach would emphatically not necessitate that we merely tread water, whether theoretically or in terms of method. As a case in point, I have already suggested that issues of social ontology - about the nature of the phenomena that sociology claims as its own specialist territory - have taken a back seat in the glamorous world of international social theory. In the sociological future that I have sketched out above, we will need to revisit under-examined and taken-for-granted ideas such as ‘society’, ‘social structure’, and ‘social groups’. To take as an example ‘social structure’, one of the most fundamental and commonplace items in the disciplinary lexicon, the basic idea derives, as a metaphor, from biology or architecture. But what is ‘social structure’? What does it actually mean? One can find many answers to those questions, the overwhelming majority of which are vague and none of which arrive at a consensus. This is a remarkable - and worrying - state of affairs for a discipline that aspires to be taken seriously. Because of the ubiquity and taken-for-grantedness of the idea of social structure - vacuous and useless as it might be - it is going to be very hard to shift. Nonetheless, we need at least to initiate a conversation about how we might develop research-useable models of human collectivity that are rooted in the observable everyday realities of human life and experience, rather than in the metaphysics of ‘the social’.

Another example of a foundational sociological idea that is beginning to creak audibly, and requires some attention, is the well-worn distinction between ‘social construction’ and ‘the natural’. Social constructionism was inscribed at the core of sociology from its beginnings - Marx, Durkheim and W. I. Thomas are just three of the names that come to mind - and may be the discipline’s most significant achievement, its greatest gift to public discourse, politics and culture. If we are to defend this inheritance we need critically to examine the ideological filters that discourage us from recognising that some aspects of the human behaviour in which sociology is interested are not wholly socially constructed. If we are to proclaim the value and necessity of our social
constructionist perspective, we must also be relaxed about acknowledging its limits. This will be difficult, if only because of important political sensitivities about gender, disability, sexuality and so on, but it is not impossible. The prize is a robust social constructionism that needs neither to apologise for itself, nor turn a blind eye to inconvenient truths (about bodies in particular).

As a final example of an area in which there is still basic theoretical work to be done, we need to become more comfortable with contingency and the random aspects of human life. Theoretically and substantively we search for patterns: this is basis of the scientific enterprise (and it is also central to human cognition). But the human world may be more random than we know; it may even be more random than the natural world. The problem here is that we are expected, by ourselves and by others, to discover pattern, preferably pattern with at least some predictive power. In the process, however, we may be squeezing the human world - and ourselves, as analysts of that world - into an intellectual straitjacket.

Everything that I have just discussed requires us to engage better with our widest possible public. This is partly a matter of how we write - to say which is to turn back to C. Wright Mills again - but it is also raises another issue. Why are we content to let the psychologists, management scientists, economists and others write the books that sell in airports? That there is an audience out there is suggested by Levitt and Dubner’s best selling 2007 book *Freakonomics*; marketed as ‘renegade economics’ it could, in fact, have been better described as simplistic - and really not very good - sociology and social research. There are lots of sociologists who could have done better. What are the barriers to this kind of public engagement? Sociology may just not have the positive brand identity of psychology and economics; if so, this is strange enough to warrant some further thought, since - in terms of either prediction or offering something beyond buffed-up common sense - the track records of economics and psychology are pretty dismal. Sociology definitely needs to start marketing itself better. Or perhaps sociologists feel that they might risk their professional reputation, among their peers, by jumping the academic fence? If that is so it is a sad comment on the discipline. Or is it that we just don’t think that public engagement matters? I don’t know what the answers are, but the questions need to be asked.

**In closing....**

The first thing to say at this point is that I have knowingly, and necessarily, been trading in sweeping generalisations. While I stand by those generalisations - and the critique of contemporary sociology that I have offered - it is equally necessary to acknowledge that if there were not many, many sociologists ‘out there’ practising their own versions of the kind of sociology which I am
advocating, there would have been no point in delivering this lecture, because the battle would already have been lost (and it isn’t).

The second thing to say is that this battle - if such it can be called - is not an all or nothing, either/or affair. For example, I have no particular wish to see the social theorists simply vanish (not all the social theorists, anyway). However, I do ask - and it may be appropriate to actually demand this - that social theory become more intellectually democratic in terms of how social theorists write and the audiences that they write for, adopt a more systematically empirical approach to the human world, and, above all, perhaps, take itself a good deal less seriously. Social theory is only social theory, after all, and most of it will vanish in the fullness of time. Empirical sociology will, however, continue to be read, even if only as a contribution to the social historical record.

Talking about history brings me to a matter that I have not yet discussed, but which needs at least to be mentioned. Sociology emerged as an attempt to understand the seismic social changes of the nineteenth century: industrialisation, mass migration, urbanisation, secularisation, etc. To put this in another way - and at the risk of committing the cardinal sociological sin of reification - sociology is modernity’s attempt to grasp and reflect on itself. Viewed in this light, social theory’s fascination with modernity is not only understandable, but also completely appropriate. However, we have gradually moved away from the intellectual foundation in history that the discipline’s pioneers took for granted. Sociology without a sense of history is bad sociology; the mesmerisation by novelty that I talked about earlier is one example of the problem. We need to stand firm as a historical discipline again; we also need to remember our own intellectual history better.

There is one other issue to discuss. Having been sharply critical of our present social-theory obsessed ‘normal science’ of sociology, I might easily be interpreted as saying, even if only by implication, that the limiting and stagnant character of normal science is the real root of the problem. On the other hand, however, I have presented the positive prospect of another, empirically focussed, normal science of sociology, that is modest in its ambitions, constructively critical in its engagement with its publics, and recognises its task as to interpret the world on the basis of systematic evidence (and in the process, perhaps, to help to change it). Thus it is not normal science per se that is the problem: as Kuhn showed us a long time ago, normal science is simply how intellectual enterprises work. Far from being an inevitable barrier to evolution normal science is probably, in fact, its precondition. That, however, is no reason for complacency: vigilance will always be necessary if we are to prevent normal science turning into self-absorbed and self-important stagnation.
To close, nothing that I have said in this lecture is new. C Wright Mills is the most obvious source. That I feel the need, fifty-four years after *The Sociological Imagination*, to re-issue Mills’ call to arms says much about my view of sociology’s problems. With Mills, I do not see those problems as intractable; with Mills, too, I see the struggle to address them as worthwhile. In fact that struggle, and those problems, may be inherent in the discipline, simply part of the business of doing sociology and a necessary part of its future.

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Because this was an inaugural lecture, I have taken full advantage of the licence that such an occasion affords, to roam far and wide without the safety net of reassuring scholarly references. Developed versions of many of the arguments advanced in the lecture can be found, supported by the references to the relevant literature, in *Foundations of Sociology: Towards a Better Understanding of the Human World* (Macmillan 2002). The argument about social structure comes from ‘Beyond social structure’, in P. J. Martin and A. Dennis (eds.) *Human agents and social structures* (Manchester University Press, 2010); contingency and the randomness of the human world are discussed in ‘Tales of the Unexpected: Doing Everyday Life, Doing Fieldwork, Doing Anthropology and Sociology’, *Anthropologica*, vol. 55, pp. 5-16. I am grateful to Tom Clark and Jenny Owen for their comments on an earlier draft and contributions to the final text; I alone am responsible for its remaining eccentricities and failings, however.