

# History is Not Bunk: Why Comparative-Historical Sociology is Indispensable When Looking to the Future

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**Abstract:** *Henry Ford famously remarked that 'History is bunk'. Too often, sociologists seem to have believed him. Although in its origins our discipline largely was comparative–historical sociology, the decades since the Second World War have witnessed what Norbert Elias called a 'retreat of sociologists into the present', resulting in a social science that is mainly 'hodiecentric' (to use a word coined by Johan Goudsblom). 'Historical sociology' has come to be regarded as just one empirical specialisation among many – as reflected in the ISA's organisation into the watertight compartments represented by its 56 Research Committees, 3 Working Groups and 4 Thematic Groups. I argue, however, that the neglect of the past has jeopardised sociologists' ability to look forward intelligently to the future. Indeed, the hostility to Marxism that marked a generation of Cold War thinkers such as Karl Popper led some social scientists to link distrust of predictions of the future with distrust in the study of the past.*

**Keywords:** *Historical sociology; physics envy; process theories; documentary research*

## Introduction

Durkheim asserted that *all* sociology is comparative: 'comparative sociology is not a particular branch of sociology; it is sociology itself, in so far as it ceases to be purely descriptive and aspires to account for facts' (1964 [1895]: 139). I want to argue that, equally, all sociology needs to be historical. In its origins, sociology arguably *was* historical sociology: the Holy Trinity of Marx, Weber and Durkheim were all historical sociologists as well as comparative sociologists, and the same goes for many of the lesser deities, the angels and archangels in the celestial pantheon of sociology, from Montesquieu in the eighteenth century through Tocqueville in the nineteenth century. Until about the middle of the twentieth century, understanding the *development* of human society was the central concern of sociology – and of cognate disciplines such as anthropology and political science. It no longer is. In the modern neo-liberal university, money flows to research that is present-centred in character (or 'hodiecentric', to use Goudsblom's (1977: 7) term), which politicians, policy-makers and administrators believe to be useful – a belief in which some mainstream sociologists seem to find it advantageous and lucrative to share. Both sides may also share the common belief that, because the modern/postmodern/digital/globalised world is changing and is so new in character, studying the past is irrelevant. [\[1\]\[#N1\]](#)

It was Henry Ford who said that 'History is bunk'. To be exact, what he said (in the *Chicago Tribune*, 25 May 1916) was: 'History is more or less bunk. It's tradition. We don't want tradition. We want to live in the present, and the only history that is worth a tinker's damn is the history that we make today.' Too often, sociologists seem to have believed him. The nearest echo of Henry Ford in sociology was perhaps John Goldthorpe's

(2000 [1991] essay 'The uses of history in sociology'. Goldthorpe argued, in an essay that was strongly critical of some prominent 'grand' historical sociologists – notably Barrington Moore – that we should leave history to the historians. Curiously – but on reflection not very surprisingly – for someone whose first degree was in history, Goldthorpe took a very Eltonian – even Rankean – view of historiography. [2],[#N2] That is, so to speak, the equivalent of old-fashioned positivism in sociology: he stressed that historians dealt with 'relics' – documents that have survived from the past, and whose representativeness of all the documents that once existed is not to be taken for granted. The task of sociologists, he contended, should be to 'create their own relics', by gathering quantitative data about present-day society, principally through rigorously representative sample surveys. In a way he was right, because – to give one example – his own massive data on social mobility in Britain (1980), gathered at huge public expense in the 1970s, have now become historical evidence, 'relics', in their own right.

John Goldthorpe has always been an explicit and committed follower of the philosopher Sir Karl Popper; indeed, when he inspiringly taught me as an undergraduate in Cambridge half a century ago, Goldthorpe transmitted his enthusiasm for Popper, temporarily as it proved, to me. [3],[#N3]

Now, I am never quite sure how extensive Popper's influence was outside Britain, but certainly in Britain his books *The Poverty of History* (1957) and *The Open Society and Its Enemies* (1945) instilled in many social scientists a feeling that it was ideologically unsound to study the past and even more suspicious to attempt to predict the future. Elsewhere in the world, notably in the USA, a general anti-Marxist influence – see for example Robert Nisbet's book *Social Change and History* (1969) – probably had much the same effect as Popper had. Whatever the exact influence, in the post-war decades, the majority of sociologists conducted a strategic 'retreat into the present' (as Norbert Elias called it). The reason may not have been entirely abstractly intellectual: the fact that quantitative research on contemporary social problems attracts far larger funding has been, and continues strongly to be, a consideration. No matter what the cause, sociology has become largely today-centred.

## Physics envy and scientism

Part of this syndrome is 'physics envy'. Popper was not alone among philosophers in taking physics as the model of what all 'science' is, or ought to be. This was broadly true of all the logical positivists and similar philosophies. One consequence was that the 'covering law' model of a theoretical explanation became the ideal against which social scientists measured their own theories. This has gone furthest among economists, especially those of the modern Chicago, neo-liberal persuasion. It is not unknown for them to compare the 'law of supply and demand' with Newton's laws of motion.

But it has affected sociologists too. Physic envy, or what I also like to call 'white-laboratory-coat-syndrome', is alive and kicking in sociology. By way of illustration, a friend and colleague of mine recently succeeded in having an article published in one of the highest-prestige and maximum-impact American general journals in sociology. But the editors insisted on some terminological amendments. She had used the term 'interdependence', which is an absolutely indispensable sociological concept. True, in my experience, very few American sociologists are able to distinguish clearly between 'interdependence' and 'interaction'. Of course, countless people are interdependent with each other without ever interacting face-to-face. The much more important concept of 'interdependence' takes us straight to the heart of sociology: the unequal and fluctuating power ratios between people and groups who want and need things that other people and groups can supply – whether the things are economic resources, political power, status, or information and orientating beliefs. Well, the editors of the *ASR* did not like the idea of people being 'interdependent' with each other. They

suggested – and indeed successfully insisted – on the ‘interdependence’ being replaced by what they said was a more technical and precise term: ‘feedback loop’. Quite apart from the fact that this pseudo-scientific term loses all reference to power ratios, it is a pathetic reversion to ideas that were fashionable in my distant sociological youth. Back in the 1960s, there was a passing infatuation with pseudo-scientific cybernetic analogies. One can find it in the later and more bizarre writings of Talcott Parsons (1977: 9, 28, 49–50, 234–6, 260–1), or in a book like Walter Buckley’s *Sociology and Modern Systems Theory* (1967). But the editors may have favoured it because ‘feedback loop’ has become adopted in the worlds of public relations and ‘human resources’. What a lot of baloney – and old-fashioned baloney at that! This is a good example of how sociological jargon passes into ‘lay’ use, and then sometimes drifts from there back into sociology.

Most vulnerable to physics envy are quantitative sociologists, especially the high positivistic establishment, notably in American sociology and in what Goldthorpe calls ‘the sociology of the research institutes’ (in contrast to the ‘sociology of the university departments’). But there is also a risk that it may afflict the very numerous ‘qualitative’ sociologists too, albeit in a different and negative way. Easily recognising that their own activities do not meet up to the physics model, they may be tempted to swing to the opposite pole and reject ‘scientism’ altogether – which is also a mistake. Furthermore, partly because qualitative sociologists find it more difficult to adhere to the quantifiable measures, outputs and outcomes that the performance-managing funding bodies demand, the rules for the funding game continue to be based upon pseudo-scientific criteria. The qualitative mainstream may thus come to suffer funding envy even if they resist physics envy.

There really is no reason for sociologists of any variety to suffer from physics envy. Although it often lingers on as a sort of ideal yardstick, we all know in our heart of hearts that the simple ‘covering law model’ of theory and explanation does not fit the social sciences. We may *know* that, but we are not always sure *why* it is not appropriate. As the historian Bruce Mazlish wrote in his book *The Uncertain Sciences* (2001: 15), the traditional philosophy of science ‘is highly formalistic ... frequently more concerned with logical principles than with immersion in the human sciences’. Instead, he advocates that in attempting to understand scientific method, we should approach the subject ‘first and foremost from a historical perspective, seeking to understand its emergence from the actual practices and changing thoughts of those working in the disciplines’.

The danger is that, if we simply reject the model of physics, the discipline that underlay the old-fashioned ‘philosophy of science’, and go directly into seeking a model of knowledge derived from observing how the social sciences actually proceed, we risk jumping straight from the one pole of ‘philosophical absolutism’ to the opposite extreme of ‘sociological relativism’ (Elias, 2009 [1971]: 16). We have seen the wilder social theorists fall into that pit from time to time, most notably in the fashion for ‘postmodernism’ that set in during the 1990s. Shortly before his death, Ernest Gellner (1995) referred to ‘the carnival of cheap relativism which threatens to swamp the coming *fin de millénaire*’.

How, then, to steer between the Scylla of philosophical absolutism and the Charybis of sociological relativism? (Elias, 2009 [1971]: 16). Underlying both, says Elias, is a shared assumption. They both reduce everything that people know to two diametrically opposite states: *either* to a state of absolute dependence of knowledge on the situation of the groups where it is used or produced, *or* to a state of absolute independence from it. Most scientific knowledge, he points out, contrary to the older philosophical suppositions, has the character of ‘a structured flux’, and, on the other hand, modern ideologies have absorbed a good deal of factual sociological and economic knowledge compared with the ideologies of earlier ages. There are no zero points – forms of knowledge have to be located along the continuum. In order to see this, one needs not to contrast physics with sociology, but rather to climb step by step up the ladder of Auguste Comte’s (1830–42) hierarchy of the sciences.

As one climbs the ladder, methods of investigation developed at one level are seen to lose their usefulness, and become at most auxiliary tools. The general 'law' as a model of a scientific theoretical explanation becomes less and less useful, because one of the tacit assumptions underlying the search for 'laws' is that the entities one conceptualises, observes and manipulates are connected in a necessary and unchanging pattern, and do not change their properties irreversibly if they are cut off from their connections with each other. Regularities best suited to expression in the form of an invariant, timeless 'law' are those where the relationship between entities is impermanent though it has a permanent pattern: it can start and stop innumerable times without affecting the behaviour of other constituents of any wider nexus. Classical physics dealt with loosely-integrated congeries, in which processes were timeless and reversible – Boyle's Law, relating the pressure and volume of gases, is an example. Theories of that type were essentially two-dimensional, cause-and-effect ones. This is, in other words, the case of 'billiard-ball causality'. Further up the ladder, however, three-dimensional 'structure theories' (seen even in nineteenth-century chemistry, for instance in understanding the differences between various forms of the sugar hexose, all of which share the quantitative formula  $C_6H_{12}O_6$ ) become more useful than law-like theories.

Beyond that, argues Elias, *process theories* are necessary. In the biological sciences, explanations and theories were *never* static and three-dimensional – they always involved irreversible processes of development, and a fourth explanatory factor of *time*. Today, even physics deals with *processes*, in which time is an essential component, and much of what cosmologists observe is not reversible (not yet, anyhow, or we are in trouble from a contracting universe!). Process theories embody *four-dimensional* models they involve both the spatial dimensions and time. They are models of *structured processes of change* over time, exemplified already in nineteenth-century science by the theory of evolution of species.

What about the social sciences? Our data are all essentially *developmental*, not static, and I would argue that all good sociological explanations involve time (as also has Andrew Abbott, 2001, 2016). But there is something else, beyond the fourth dimension of time. There is also *experience*, which Elias identified as the *fifth dimension* in sociological explanations: our data include how people *experience* the social processes and social interdependencies through which they live. And thus, experience must form a component of our five-dimensional theories. More technically, sociological explanations need to involve not only the directly visible 'behavioural' aspects of human activity in the four dimensions of space and time, but also the 'experiential' aspects of human thinking, feeling and psychological drives. While these experiential aspects are not directly accessible to observation in the same way as bodily movements, they are nevertheless accessible to human observation through the examination of linguistic and other symbols carrying meaningful messages from one person to another (Elias, 2011).

'Three cheers for Max Weber and *Verstehen!*', I hear you say. This is really what sociologists have been doing since the origins of the discipline; it just needs to be recognised, and emancipating ourselves from 'physics envy' and the 'white laboratory coat syndrome' is necessary for that recognition. As always, though, there is a snag: too often *Verstehen* has been discussed as if it were just *one* specific methodological approach; this has sometimes led on to the dafter versions of constructivism and a preoccupation with 'discourse' – which is fine if no one believes that discourse is all that there is, the unique and exclusive source of social scientific data.

## Back to Methods

There remains a widespread and persistent identification of 'real science' with the collection and manipulation of quantitative data. Of course, that is and always will be an essential part of sociology, certainly the most methodologically sophisticated – and probably always the most lucrative. But I want to warmly

welcome the recent turn towards ‘mixed methods’. I have never understood how students could be taught to think of ‘*the quantitative method*’ and ‘*the qualitative method*’ as somehow polar opposites between which one had to choose. In my own research, long before anyone told me that ‘mixed methods’ were fashionable, I always just gathered both sorts of evidence, whatever was relevant. But certainly, my own work has to a considerable extent also employed historical and documentary evidence. That, too, is part of ‘mixed methods’, and I am delighted that under the Presidency of Nina Baur, the ISA’s Research Committee 33, Logic and Methodology in Sociology, is attempting to bring back historical and documentary research into the canon of research methods.

It is worth reflecting that there is always more relevant information already out there than can ever be gathered even by the largest-scale survey. What someone, somewhere, knows already – even if they do not yet know that they know it and how to interpret it – always vastly outweighs each new addition to our total stock of knowledge. We all know this really: it is the point which, for example, the arch-positivist Popper (1972) was pointing to when he advanced his idea of a ‘World 3’ of knowledge. (‘World 3’ included all the stock of knowledge stored in libraries and archives, even if no one currently alive had read it.) But whether we always take account of what this implies for sociological research methods is another question. Sometimes sociologists seem content with an academic division of labour that consigns sociology to the task of gathering *new* data about currently living people by means of surveys, interviews and so on.

But remember that information gathered for one purpose may prove useful in a quite different explanatory context. That is one reason why I have always argued – in a remark designed to horrify John Goldthorpe – that historical evidence gleaned even from *secondary* historical sources may prove useful when redeployed to sociological purposes, when we see it from new perspectives and in the context of different theories. Nuggets of information unearthed by an historian for the purpose of, for example, writing a biography may, when viewed through the lens of a sociological theory, prove valuable clues to much broader social processes – which the sociologist can then go on to investigate, probably through ‘mixed methods’ including statistical data, interviews, and *documentary evidence*. Yet we scarcely ever see the need to train students of sociology in the handling of historical and documentary evidence. I know of only one specifically sociological textbook on this – John Scott’s *A Matter of Record* (1990). Scott is especially revealing in his discussion of a wide range of ephemera – down to and including bus tickets. Now, there is an extreme example of a document created for one purpose that can be used for a quite different purpose of sociological evidence!

Sad to say, I have encountered colleagues – quite senior colleagues – and certainly students, who confused documentary evidence with a ‘literature review’. The difference hardly needs stating, but let us state it nonetheless: documentary evidence is sociological data; a literature review is about what earlier sociologists have had to say on the topic of one’s research.

Of course, historical and documentary evidence needs to be weighed and evaluated, and sometimes sampled, using rigorous methods. Since the days of Elton, let alone Ranke, there have been advances in historiographical techniques that sociologists need to be aware of. All this needs to find a place in the undergraduate and postgraduate ‘methods’ curriculum. They also need to be taught that the sociological use of historical and documentary evidence needs to be critically assessed just as the interpretation of large data sets is routinely assessed. Just to give one example from my own specialist area, Patrick Murphy (2015) has recently given detailed critical attention to Norbert Elias’s interpretation, in *On the Process of Civilisation*, of drawings from the *Mittelalterliches Hausbuch* (Elias, 2012a [1939]: 199–209). [4].[#N4]

It should be borne in mind that, although historians are less prone to disciplinary navel-gazing than are sociologists, they too have methodological debates from time to time. The two disciplines could interact more than they generally do, to both sides’ benefit. An instance of where clear sociological thinking could clarify an

historical dispute is the debate about the teaching of history that has been running in various forms in Britain since the 1960s. It can be roughly described as the dispute about ‘top down’ versus ‘bottom up’ history. The historians associated with the History Workshop were caustic about the kind of ‘kings and queens and battles’ history that had tended to prevail in schools. They advocated ‘bottom up’ history, focusing on the lives of ordinary people rather than the powerful elites. But it is a false dichotomy. The outcome of the struggles for power among elites shapes and shifts the power ratios between ordinary people too, so that ‘everyday lives’ are affected by the battles between kings and queens and prime ministers and presidents and dictators. And *vice versa* too, of course, as a good process theory would show. [5][#N5]

## The Problem of Historical Analogy

None of this is to deny that there can be serious problems with over-generalisation in historical sociology, especially in what Goldthorpe called ‘macrohistorical sociology’. Goldthorpe did score some direct hits. A theory as ambitious as Marxism can lead to oversimplification. Barrington Moore (1967) probably did offer too many hostages to fortune in his generalisations about ‘lord and peasant in the making of the modern world’, drawing analogies as he did between ‘modernising’ revolutions in England, France, America, China, Japan and India. On the other hand, Michael Mann, in his even more ambitious multi-volume study of power and its transformations throughout human history (1986–2013), seems to me largely to avoid such problems by means of his careful, scholarly, Weberian weighing of evidence. Not all historical sociology involves large-scale historical generalisations; not all of it involves what Charles Tilly (1989) called ‘big structures, large processes, huge comparisons’. Norbert Elias expressed his research strategy in more modest terms. He said he did not think that he had a *methodology*, but perhaps he had a method. In a note to himself he described ‘Die Eliassche Methode’ as ‘Makrostrukturen durch die Untersuchung von Mikrostrukturen sichtbar zu machen’. [6][#N6] This is more or less the modest strategy that I followed in my book *All Manners of Food* (Mennell, 1985), in which I sought to explain through the use of documentary evidence how England and France – although in close contact with each other for centuries – had come to have very different ‘culinary cultures’.

Historical analogies do, however, sometimes over-reach themselves, and this is not a problem just for academic historians and social scientists. It enters too frequently into the rhetoric of politicians and the decisions they take. A well-known example is that of British Prime Minister Anthony Eden’s disastrous decision in 1956 to conspire with France and Israel to launch a military invasions of Egypt to seize back control of the Suez Canal, which had been nationalised by President Nasser. Eden had been a valiant opponent of the appeasement of Hitler’s Germany in the 1930s, and it seems clear that he erroneously saw Nasser as another example of the species ‘fascist dictator’. Or another example, much more recent, from the 2016 referendum on whether Britain should cease to be a member of the European Union: that of Boris Johnson, described as ‘a keen classical scholar’. He argued that the past 2,000 years of European history have been characterised by repeated attempts to unify Europe under a single government in order to recover the continent’s lost ‘golden age’ under the Romans. ‘Napoleon, Hitler, various people tried this out,’ he said, ‘and it ends tragically. The EU is an attempt to do this by different methods’ (*Daily Telegraph*, 15 May 2016).

This betrays a curiously ahistorical conception of history – such a statement reveals no awareness of how the structure of global society and the complexity of interdependencies has changed, since Hitler, let alone since Napoleon (or Charlemagne, or the Romans!). Probably Johnson was thinking – as so many people continue to do – in terms of some unchanging ‘human nature’.

Now, you may say, the meanderings of one mis-educated buffoon – even if he is now the United Kingdom’s Secretary of State for Foreign Affairs – are of no great scientific importance. But that prominent people can

still use rhetoric of this kind is symptomatic of something that has worried me for a long time: the discipline of sociology's failure to penetrate the popular consciousness. Auguste Comte, who invented the very word 'sociology', aspired to the new discipline shaping the way human beings understand the societies in which they live. Yet today, I fear, the man and woman in the street still tend to think *psychologically* about the *motives* of particular individuals. This is commonly linked to attribution of blame as a means of orientation (Bentham van den Bergh, 1978). They do not seem easily to think in terms of lengthening chains and denser webs of interdependence between people, groups, economies and states – nor in terms of the fluid, fluctuating power ratios that are always involved in such webs of interdependence. [7]. Johnson's remarks imply a romantic entanglement in the notion of 'sovereignty', and that he cannot perceive the UK's entanglement in far wider constraints. Nor, it would seem, had he noticed the extent to which not just Europe, but the whole world has been unified under American hegemony via such organisations as the WTO, IMF, OECD, capital markets, credit rating agencies, NATO and so on.

This failure of sociology to penetrate the popular consciousness with its central insight of social interdependence is not to be lightly dismissed. It is a matter of practical importance. The rigid polarity between 'scientist' and 'layman' is a false dichotomy. As Mazlish wrote:

The community that is willing to accept the knowledge acquired in the pursuit of the human sciences and that is prepared to act on the basis of such acquisition ideally has to be humanity at large; only then can the sciences whose subject matter is evolving humanity go forward. ... Given my thesis about the necessity of a scientific community composed of much or most of humankind, it is only fitting that I write both for the general public and for specialists prepared to think outside their own field. (Mazlish, 2001: 3, 6)

What we may over-dignify as the 'Borisonian fallacy' is also linked to a problem common both to social scientific and lay thinking, the problem of what Norbert Elias (2012b [1970]: 106–11) calls *Zustandsreduktion* or 'process reduction'. By that, he means the tendency to form concepts in a static mode. Drawing on the Sapir-Whorf hypothesis (Whorf, 1956) about 'Standard Average European' languages, he writes:

We say, 'The wind is blowing', as if the wind were actually a thing at rest which, at a given point in time, begins to move and blow. We speak as if the wind were separate from its blowing, as if a wind could exist which did not blow. This reduction of processes to static conditions, which we shall call 'process-reduction' for short, appears self-explanatory to people who have grown up with such languages. (Elias, 2012b [1970]: 107)

This mode of concept formation contributes to hodiecentrism in sociology as well as to idiocies of popular thought such as that I have illustrated with reference to Boris Johnson.

## Conclusion

By way of conclusion, I want to stress that I do not oppose the use of historical analogies, whether by social scientists or politicians. But I do advocate that they be sociologically informed analogies, and better supported by historical understanding too. Former British Prime Minister Tony Blair has said that he wishes he had read history rather than law at university (Schama and Blair 2007) It is (I hope) hard to believe that he and US President George W. Bush, or at least some of their very powerful advisors, would not have acted differently

had they had better historical understanding when they embarked on the actions that led to the catastrophic destabilisation of the Greater Middle East. The distinguished journalist Robert Fisk habitually refers to Blair as ‘Lord Blair of Kut Al Amara’, a reference to the defeat and surrender of a whole British army to the Ottoman forces in 1916, during the First World War. He strongly implies that Blair knew nothing of earlier British military interventions in the Arabian region, let alone of the rebuff of three British incursions into Afghanistan (1839–42; 1878–80; 1919) long before the recent defeats suffered there by the Russians and the Americans. On the other hand, it has to be admitted that in that case, the people who shaped the policy *did* infer that there had been structural changes that made historical precedents irrelevant. They seem to have decided to ignore, or never to have thought about, the large literature in political science and sociology on ‘the social foundations of democracy’ (see Mennell, 2017, in this issue). One reason for that may be that in the period of American triumphalism after the collapse of the Soviet Union some writers – most prominently the philosopher Francis Fukuyama (1989 [1992]) – convinced themselves (and, unfortunately, some of the powerful) that liberal democracy was now the default setting to which human society would ‘naturally’ revert if given the chance by the forcible removal of market imperfections such as authoritarian rulers.

What an indictment of sociology that it seems to have less influence on world events than – for a time at least – did one philosopher’s musings on another philosopher’s Hegelian musings! [8][#N8]. The philosophers have been performing their ancient round-dance for more than two thousand years, without saying anything very empirically grounded. Sociologists seem content for their theoretical–empirical investigations to have some marginal influence on social services and education policy, rather than upon thinking about global events. If only historically-informed sociological voices were able to make themselves better heard!

I have argued that all sociology needs to be historical as well as comparative. We have to know where we are coming from – our direction of travel, and other people’s direction of travel – before we can make intelligent predictions about or chart the way forward towards.

## Notes

1. A shorter version of this paper was delivered on 12 July 2016 at the International Sociological Association’s Forum held at the University of Vienna, the overall theme of which was ‘The Futures we Want: Global Sociology and the Struggles for a Better World’. It was a contribution to the ‘Common Sessions’ on this theme, as President of the newly-created Research Committee 56, Historical Sociology. (It is remarkable that until 2016 the ISA never had a section on Historical Sociology.) A longer version was delivered as a plenary lecture at the ISA’s Research Committee 33, Logic and Methodology in Sociology, 9th International Conference on Social Science Methodology, 11–16 September 2016, Leicester, United Kingdom.  
I should like to thank Stephen Vertigans for his valuable comments on an earlier draft of this paper. ♣ [ #N1-ptri ]
2. Leopold von Ranke (1795–1886), is generally regarded as the pioneer of rigorous study of documents and archives, reliance on primary sources, and emphasising narrative history; Sir Geoffrey Elton (1921–94) is among the most well-known defenders of the Rankean tradition, especially against Marxist and postmodernist approaches. ♣ [ #N2-ptri ]
3. See Dunning (1977) for a specifically Eliasian response to Goldthorpe’s advocacy of Popper. ♣ [ #N3-ptri ]
4. This is not the place to discuss the much more wide-ranging critique of Elias’s use of historical evidence that was launched by Hans Peter Duerr (1988, 1990, 1993, 1997, 2002). Shortly before his death, Elias (2010 [1989]) himself replied to Duerr, and counter-critiques of Duerr include Goudsblom and Mennell (1997), Hinze (2002) and Górnicka (2016: 23–25). ♣ [ #N4-ptri ]



5. For a more theoretical insight on such processes, see Norbert Elias, *What is Sociology?* (2012b), chapter 6, 'Game Models', pp. 66–98. [\[#N5-pttr1\]](#)
6. 'By investigating micro-structures to make macro structures visible', quoted by the editors in Norbert Elias, *The Genesis of the Naval Profession* (2007: 18). René Moelker found this remark on a slip of paper in the folders containing Elias's typescripts on the naval profession, but it may have ended up there by accident and not have any direct connection to that research. Nevertheless, in that book, Elias did show how the structures of naval command on board ship were related to the class structure of early modern English society ashore. [\[#N6-pttr1\]](#)
7. It is true that the phrase 'unintended consequences' has entered the political vocabulary, but the fact that politicians most often refer to 'the law of unintended consequences' demonstrates that they do not understand the concept. If it were a law, the consequences would not be unforeseen and unintended. Such politicians seem just to see the unplanned consequences of their action as inevitable, rather than as something to be foreseen and avoided; thus their use of the concept becomes a mere synonym for 'Sod's Law'. [\[#N7-pttr1\]](#)
8. Fukuyama himself drew upon the writings of the neo-Hegelian philosopher Alexandre Kojève (1902–68), who until then had not been widely known. Kojève argued that the progress of history must lead toward the establishment of a 'universal and homogenous' democratic state – thus making a prediction of the kind that Popper denounced. [\[#N8-pttr1\]](#)

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