

Norbert Elias and the Habits of Good Sociology

Abstract

This paper explores the somewhat mixed reception of Elias's work as, in part, understandable in terms of Elias's transgression of a dominant code of 'sociological etiquette' that I have here called the 'habits of good sociology'. I explore a number of key 'habits', which include: *empirical legitimacy*, *political alignment*, and *relativistic egalitarianism* which have arguably come to dominate the discipline in recent years. I argue that Elias's ambition to develop a central theory falls foul of a prevailing sentiment in which no single perspective should be elevated over and above any other, and where epistemic relativism has become something of a creed in the teaching of sociology. In relation to this, I will explore the model of sociological practice developed in Elias's work and suggest that it is this model of the sociological endeavour – one in which considerable sociological ambition is combined with empirical humility (i.e. that handkerchiefs might be as important as, say, economic relationships) – that remains an important component of his intellectual legacy. Ultimately, my contention is that while it is probably unrealistic in the current intellectual climate to expect Elias's work to comprise a 'central theory', his approach nonetheless offers a model of sociological practice that might permit 'advances' in sociological knowledge to take place.

Introduction¹

As will no doubt be evident to people familiar with the work of Elias, the title of this paper is a play on words of a famous manners text, a nineteenth century etiquette manual that was aimed in particular at young women, *The Habits of Good Society*. It was one of the many sources the Elias drew upon in *On The Process of Civilisation* (2012) [1939] to trace overall shifts in standards of behaviour among the secular upper classes of specific European societies. Hopefully this is more than just a neat pun. In this paper, I will explore the extent to which it is possible to discern a set of *sociological sensibilities* similar to those of 'polite society', and to provide some reflections on how such sensibilities might inform the dominant practices surrounding the discipline. I do not yet have enough data meaningfully to consider whether there has been something akin to a sociological 'civilising process', but I am able nonetheless to discern some practices surrounding paradigmatic hygiene and sanitisation involved in the formation of sociological taboos. My broader intention is to explore how the prevailing tide of sociological sensitivities and sensibilities, at least as I have been able to discern it, might have affected, and might be continuing to affect, the prospects for Elias's figurational sociology. Ultimately, I intend to consider the extent to which Elias's idea of developing a 'central theory' for sociology might fall foul, might violate, some important axiomatic sociological mores.

Part of my intention in exploring this topic is to address something of an enigma. Namely, that despite his increasing recognition as one of the most important sociologists of the twentieth century, Elias's work remains very much at the margins of the discipline, largely divorced from the sociological mainstream. Despite some 'figurational centres', notably those in the UK, Holland, Germany and now increasingly Brazil, Elias's work has very largely not enjoyed the influence and recognition that it arguably deserves. There are many possibilities for why this has been the case, but one that I shall focus on here is how Elias's work has historically displayed an obstinate tendency to run counter to intellectual fashions, and more generally to transgress dominant codes of sociological etiquette.

It is of course a very dangerous enterprise to make general statements about any discipline, especially one as fragmented and diverse as sociology. It is my hope that the reflections offered here are taken in the spirit in which they are intended: as polemical

observations of particular *tendencies*, rather than discrete empirical *generalisations*. I would also add that as a British sociologist, most of my knowledge of sociology reflects that particular national context, and I think it is highly probable that there are distinctively 'British' elements to the *habits* I am about to explore. In preparing this paper, it was tempting to include a long list of observations, but I ultimately decided to look at three key tendencies that are particularly significant to a consideration of Elias's work, and to current sociology more generally. I have called these: *empirical legitimacy*; *political alignment*; and *relativistic egalitarianism*. I will consider each of these sensitivities in turn before concluding with a more general set of reflections on their significance for the prospects for Elias's figurational sociology into the twenty-first century.

Empirical Legitimacy: You Shouldn't Study That

I remember distinctly when I started my PhD research – a study of tobacco use in long-term perspective – a number of fellow PhD students at the time expressed dismay at my choice of topic. 'Why are you studying that?'. 'That's not a topic for sociologists'. 'You need to ask a clinician or a psychologist', was a common response. It probably did not help that at the time, my guiding overall research question was 'why do people smoke?'. It was not, I came to find out, a particularly fruitful question, at least not as I had first formulated it. But it was a starting point. And it did capture, ultimately, what I wanted to investigate. I later became friends with the PhD students in question. I remember one was doing some very worthwhile research on representations of childhood sexuality and paedophilia, and another working on the Jewish diaspora. I later discovered that their objections to my provisional research ideas were at least in part well-founded: the undertaking of looking for some kind of 'answer' definitively to the question of 'why people smoke' – as though there were a single, generic 'cause' of smoking – was definitely problematic, and odds with the way such issues might conventionally and productively be approached within sociology. That said, I think their comments also help to demonstrate the first of the *sociological sensibilities* that I want to explore in this paper: that there are topics which are deemed to constitute 'legitimate sociological concerns', and topics that are not sufficiently 'suitable' for sociological attention.

So what was the standard employed in the case of my work on smoking? Why was my topic not deemed to be suitable, not appropriate, and their topics unquestionably

legitimate? I think part of the principle at play was that the topic of smoking was somehow understood to be politically anaemic and perhaps not sufficiently 'worthy' or 'important'. I eventually won over my fellow PhD students by taking the moral high ground – I was able to demonstrate, for example, that there were significant gender and class differences in the uptake of smoking, and that, as I had learned, it was the single largest cause of preventable deaths in the Western world. But if I am honest, that was not what had motivated me to research the topic of tobacco use. I was at that time a smoker, following the advice of my supervisor, Eric Dunning – who in turn was reproducing the advice of his mentor, Norbert Elias – to conduct research into an area in which I had some kind of personal involvement. Eric had undertaken his research on football. I was much more of a layabout who liked smoking and drinking, so my topic was something of a foregone conclusion.

It is perhaps easy to forget how much opposition the likes of Eric Dunning and Norbert Elias encountered when embarking upon their early research into the sociology of sport in the 1960s. At that time, as Eric has recounted to me, the topic of sport was widely regarded as at best a slightly quirky, superficial, or marginal concern. His and Elias's research on the topic was initially lampooned by some, and only slowly accepted. It took Eric and other key progenitors of the sociology of sport quite some time to establish the widespread recognition that the field was one worthy of sociological investigation. Sociologists of the time were typically looking at the state, industry, stratification, socio-economic relations, and other similar suitably 'hard topics'. Against such a backdrop, a consideration of sport stood out as a rather esoteric concern. It is noteworthy that both Norbert Elias and Pierre Bourdieu stand as relatively early examples of sociologists who took seriously 'everyday stuff', variously researching such topics as sport, art, leisure, taste, and quotidian aspects of social life such as eating habits, manners, and standards of dress. Importantly, both authors were simultaneously researchers and theoreticians; both developed concepts and more general theoretical models in relation to empirically-embedded and grounded study; both lamented the divorce of theory from research; and both shared the aim of, to use a phrase of Bourdieu's '...preventing people from being able to utter all kinds of nonsense about the social world' (Bourdieu and Wacquant 1992: 53). Of course, paradoxically, the state, social power, the economy, and so forth, were precisely the kinds of concerns that underpinned

both Elias's and Bourdieu's analyses, albeit *via* reframed and reimagined theoretical objects that, most importantly, were embedded in the minutiae of human social life.

Indeed, while I have not got space here for a proper comparison of the work of Bourdieu and Elias, there are some remarkably similar aspects to their orientations towards *doing* sociology – what we might call, their 'sociological practice'. A key commonality between their work is, to paraphrase Bourdieu, their capacity to have combined: '...immense theoretical ambition with extreme empirical modesty': to have treated precise and seemingly mundane 'objects' of study in a manner which, nonetheless, permitted an engagement with 'high theoretical stakes' (Bourdieu in Wacquant 1989: 51). It is today perhaps rather less controversial to undertake research on these kinds of topic. That said, my colleague at Brunel, Sharon Lockyer, has recounted to me the disparaging sniggers she still occasionally receives when she tells people that she works on the sociology of comedy. Yet, as the work of sociologists such as Lockyer (see, for example, Lockyer and Pickering 2009) and, for instance, Giseline Kuipers (see, in particular, Kuipers 2006) serves to demonstrate, comedy can be an extremely serious business, and as an empirical field, it holds enormous sociological value and significance.

Perhaps the chief sociological 'danger' today resides not so much in the topics that we choose, but in the embrace of a counter-tendency to that observed by Bourdieu: of combining immense theoretical modesty with only empirical ambition. For example, a recent BBC documentary on the wastage of taxpayers money ridiculed an Economic and Social Research Council-funded project in the UK entitled the 'Cappuccino community: cafés and civic life in the contemporary city' (Laurier and Philo 2005). The documentary makers conducted an experiment. They asked a group of pensioners to visit a number of cafés, make notes on their experiences, and then to draw some more general conclusions in relation to the kinds of questions asked by the ESRC-funded project researchers – questions about the functions of cafés, the kinds of interactions that took place within them, the kinds of communities that formed in that context, and so forth. The programme makers then compared these general conclusions from the pensioners with those of the authors of the ESRC-funded study and found, with great journalistic alarm, a remarkable consistency between them. Despite their lack of sociological training, and for a fraction of the cost of the original study

(£200,000), the pensioners found that: Cafés facilitate sociability. They are places to hang out. They serve, in some communities, a vital social function. They facilitate the development of friendships, and promote a sense of conviviality. Such findings were indeed ostensibly similar to those of the original study which concluded:

The café is a place of social mixing quite distinct from the street, allowing a differently ordered set of encounter, fleeting gazes can be built on into encounters, the duration of proximity stabilising a sense of being with friends, familiars, strangers and others in public (Laurier & Philo 2005: 13).

The programme makers' experiment seemed to bear out an insightful remark made by Howard Becker concerning ethnographic research: that social researchers are sometimes prone to spend thousands of dollars to reach conclusions that a taxi driver could have provided them with for the price of a cab fair, or words to that effect (Becker 1998). However, of course, Laurier and Philo's research cannot be so easily dismissed. If the conclusions of a piece of research coincide with common-sense understandings of the world, does this necessarily mean the research was not worthwhile? The BBC was perhaps more interested in making an anti-intellectual point about the wastage of taxpayers' money than attempting a serious appraisal of the study on café culture. I have subsequently obtained a copy of the project write-up and, perhaps unsurprisingly, it was rather more sophisticated than the BBC had claimed. While the study made central use of sociological concepts and ideas, its authors were in fact based in a Department of Geography and Geomatics. Nonetheless, its authors drew links to a whole range of other key sociological themes: cities as social spaces; Habermas's concept of the public sphere; homeworking; leisure and consumption; 'Starbucksisation', and so forth (Laurier and Philo 2005). The project involved a more wide-ranging analysis, and a considerably more detailed set of findings than the BBC programme had reported.

Notwithstanding their somewhat over-simplified account of the research findings, the BBC documentary makers' attempt to convey the idea that the key differences between what the original researchers found and what the pensioners found related principally to *how* the findings were framed, expressed and reported serves here to highlight a pertinent concern. Without a requisite intellectual training, the pensioners, unlike the project researchers, were

unable to use the same kind of conceptual language, unable to draw links to, for example, Habermas's (1989) work on the public sphere, Blum's (2003) 'fluorescent' account of city life, nor employ a combination of representational theory with a Foucauldian archaeological and genealogical discourse analytic (see, for example, Foucault 2003; Laurier and Philo 2005). However, one might be tempted to ask, did the pensioners need such a training: particularly given that the fundamental *insights* they gleaned were ultimately not so very different from those of the original study. The documentary makers' steer towards the conclusion that the key difference between what the researchers said and what *anyone* could have said resided predominantly in *how* they said it, while not entirely fair with regard to the Laurier and Philo study, does indeed raise some important questions about practices within contemporary sociology more generally. It would be an indictment indeed if it were now correct to accuse sociologists – or to be more encompassing, those who draw centrally upon what are commonly recognised as sociological ideas – of having become a group of practitioners who have learned a particular way *to talk*, not *from talk*.

Part of the more general problem this example points towards resides, again, not so much in the use by sociologists of esoteric language, their attempts to frame their findings in relation to existing theory, but in the sociological tendencies surrounding the use of concepts and theory and the specialist lexicon associated with these. Another colleague of mine, Chris Rojek, together with Bryan Turner (Rojek and Turner 2000), have described the prevailing 'modesty' surrounding theory as comprising a 'decorative turn' in sociology. Put simply, the decorative turn involves the dominant sociological tendency to employ theory, particularly that which is in sociological vogue, in a manner which serves only to adorn or otherwise 'decorate' empirical analysis. Theory employed in a 'decorative' manner is characteristically superimposed on to research, rather than developed in tandem with it. It is used to lend otherwise quite straightforward empirical observations ostensible veracity and sociological significance – to boost their perceived sociological value. In decorative sociology, theory is more 'tasted' than 'tested'.

As Bourdieu suggests: '...the power of a mode of thinking never manifests itself more clearly than in its capacity to constitute socially insignificant objects into scientific objects ... or ... to approach a majorly socially significant object in an unexpected manner' (Wacquant

1989: 51). Bourdieu is here pointing towards a model of doing sociology which centres on moving from the mundane to the significant through the formation of 'scientific objects'. However, the crucial point is that for Bourdieu, like Elias, this necessitates a different kind of sociological practice from that of decorative sociology: one in which theory and research are consistently inter-woven, and not force-fitted through a kind of conceptual–empirical 'shotgun wedding'. Again, Elias and Bourdieu share remarkable similarity in respect of their 'social praxeology' (Bourdieu's term): their approaches to doing sociology. Each author is opposed to the inherent institutional separation between supposedly elite theoretical specialists and the supposedly 'proletarian' researchers who are set to work to 'operationalise' grand classificatory schemes – a depiction which, as Goudsblom (1977: 102) has observed, is nowhere better displayed than in Etzioni's preface to a 600 page theoretical treatise in which he states: 'The power of the propositions produced by this theory have [*sic*] to be tested in empirical research and social action' (Goudsblom 1977: 102). Thus to follow Elias and Bourdieu, empirical humility is by no means a 'bad' sociological 'habit'. We might indeed be encouraged to look at cafés, perhaps even coffee, cups, spoons and saucers, but always with considerable sociological ambition – ambition that involves considerably more than the conceptual 'decoration' and 'adornment' of mundane insights, and in which a deference to the supposedly unassailable conceptual authority of certain 'intellectual totems' (Wacquant 1989: 50) through their various guises in 'social theory', is discarded and transcended.

Political Alignment: You Shouldn't Do/Say That

I have recently completed an edited reference collection on visual methods (Hughes 2012). I mention this because while writing it I came across some interesting papers on the early years of the *American Journal of Sociology* (AJS). Before undertaking the project, I was not aware, for example, that between 1896 and 1916, the AJS published some 31 articles containing 244 photographic illustrations most of these appearing four or so years either side of the turn of the century (Stasz 1979). However, by the 1920s, articles in the AJS containing photographic illustrations had all but disappeared. Such a trend was repeated elsewhere, with sociology becoming, until recently, very much a 'discipline of words' (Ball and Smith 1992). As Stasz (1979) suggests, in the case of the AJS, this marked a shift in editorial policy: a conscious effort to move away from crude and amateurish work, and towards a quest for

professional legitimacy under the guises of establishing a more 'scientific' basis for sociological debate. The early 'politics of sociological expression' involved a denigration of the visual, in part predicated upon the scientific aspirations of the discipline at this formative stage of its development. Indeed, in the early twentieth century, the use of photographic images was increasingly coming to be considered in sociological circles to be 'pandering to low tastes', and to be employing spurious rhetorical devices instead of 'objective' and 'dispassionate' argument and evidence (Becker 1995: 9). Somewhat paradoxically, the natural sciences have had a long tradition of using photographs and other images not simply as illustrations, but as constitutive vehicles of scientific knowledge (Fyfe and Law 1988: 3). Nonetheless, this formative sociological prejudice against visual representations, which to a degree remains today, appears to echo a more general belief that images are 'involved' and text is 'detached'. Such an idea finds expression, for example, in the notion that 'serious' people read 'broadsheet' newspapers with a predominance of text, whereas those after gossip or the superficial and frivolous affairs of the day read tabloids, or magazines clad with page after page of celebrity images. These associations, again, have political dimensions: they are profoundly gendered and classed. To accuse someone of being the kind of person who 'reads books with lots of pictures in them', is to insult their intelligence – to suggest they lack education and social distinction. Even where photographs are arguably essential to the substance of a book, for example, in the case of cookery, there is an unspoken divide between populist recipe collections featuring sumptuous photographs of the food in question, and serious 'professional tomes' which contain few photographic illustrations, and only lengthy descriptions of method.

It is noteworthy that Elias's work, along once more with that of Bourdieu and of writers such as Becker, Goffman and Foucault, again ran counter to the prevailing politics of sociological expression in this respect. For example, amongst the many sources that Elias drew upon in developing his theory of civilising processes were a series of 14 images from the *Mittelalterliches Hausbuch* principally in his discussion of 'Scenes from the life of a knight'. Similarly, in his introduction to *Involvement and Detachment* Elias provided an extensive discussion of Velazquez's *Las Meninas*. Elsewhere Elias explored how the illusion of three-dimensional space *via* a more realistic sense of spatial perspective employed by painters

such as Masaccio and van Eyck, was a capacity based upon a variant of the 'detour via detachment' (Kilminster 2004: 33). Thus, rather than simply marking an avenue towards 'lowbrow' sociology, Elias saw the potential for pictorial sources and other forms of visual data to serve both as referents of the social universe, and, for example, as in the case of the 'false teeth' and 'dartboard' diagrams in *What is Sociology?*, themselves a vehicle for the expression of sociological knowledge (Fyfe and Law 1988: 6).

While, it has now become far more common to include photographs as illustrations – even the primary mode of expression – in sociological articles, the 'disappearance' and subsequent 'reappearance' of the visual, as will no doubt already be apparent, also serves here to highlight the second of the key sociological sensibilities that I wish to highlight: *political alignment*. Put simply, the historical tendency towards hesitation or even self-censorship concerning the inclusion of photographs in sociological work can be understood to be related, in part, to a prevailing unease regarding the 'power effects' of particular kinds of images, and with it, a more general hesitance among sociologists to transgress a dominant political code – that 'we' as sociologists should always distance ourselves from the interests and technologies of 'power' (and I am consciously using the term in a Foucauldian sense [see, in particular, Foucault 1980]). As Emmison and Smith (2000: 13) have suggested, an important strand of Foucauldian analysis has involved a focus upon how technologies of simultaneous surveillance, discipline, and control – from Bentham's panopticon to modern closed-circuit television cameras – might extend also to the 'relations of viewing' involved in the production and consumption of photographic images. They write:

To the extent that disciplines such as sociology, social work and social administration invariably 'study down' then the inclusion of photographic images in research accounts of subjects such as 'the underclass', welfare recipients, street kids, factory labourers and so on may be construed as a continuation of this regulatory regime' (2000: 13).

This sensitivity and unease with 'the relations of viewing' – and by extension, of adopting an analytical self-positioning which does not 'align', ideologically and politically, with dominant sociological norms – can also be seen, for example, in a number of cases of editorial and self-censorship over the use of photographic images in sociological journals (Emmison and Smith 2000: 13). Emmison and Smith cite a number of examples in this

connection. The first involves a paper by Dingwall *et al.* (1991) which compared representations of the obstetric encounter in Japanese and US birthing manuals. The article was published in the journal *Sociology*, but the authors were refused permission to include one particular image of a birth scene because, the *Sociology* editors maintained, 'some readers might consider that pictures of this kind portrayed women at a private and vulnerable moment in an objectifying and offensive manner' (Dingwall *et al.* 1991: 443 in Emmison and Smith 2000: 13). The authors noted that the image in question was already in the public domain in a popular pregnancy advisory publication, but to no avail; it was excluded from the final version of the paper. Clearly the concern here was extra-scientific: the imagined sociological audience would, the editors seem to be suggesting, be uneasy, potentially offended, or otherwise disturbed by this kind of depiction. The unstated sentiment is that a sociological audience would, and by implication *should*, be more sensitive to such depictions. Such a sentiment is predicated upon an unspoken and assumed political consensus amongst the sociological community: a profound sensibility concerning the photographic representation of, in particular, female bodies, particularly when these are depicted in anything other than powerful and assertive ways.

As Emmison and Smith note, this kind of sensibility seems also to find expression in self-censorship. Two examples are used to illustrate this point. The first is Jenefsky and Miller's (1998) *Women's Studies International Forum* paper on depictions of lesbian sex in the pornographic magazine *Penthouse*. Jenefsky and Miller's central argument is that portrayals of 'girl-girl' sex in *Penthouse* are colonised by a heterosexual gaze, and are in fact constructed to serve male gratification. The article contains no photographic illustrations, but has detailed and explicit textual descriptions of the images in question. A good empirical case could be made to include the images in the article so that sociological readers could 'see for themselves' the details of the cases in question. Of course, to do would have been to reproduce the very phenomenon that Jenefsky and Miller set out to critique. Emmison and Smith contrast this example with that of Morgan's (1989) *American Journal of Semiotics* article on dominatrix pornography. Morgan's article contains illustrative material of the phenomenon in question. The key difference, Emmison and Smith suggest, and a possible

reason for the inclusion of this material, is that in Morgan's article the depictions 'present an image of the female as empowered' (2000: 14). Thus, they write:

Where images are considered to be degrading or morally offensive, authors and editors feel uncomfortable about reproducing them. When they are considered to be progressive, liberating or neutral there is a greater likelihood of their being included. What is ironic, we suggest, is that whilst photographs are often deemed to be unacceptable by authors and editors, textually explicit descriptions of morally suspect materials are considered less so. Such a 'double standard' tells us quite a lot about the relationship of our society to the image as opposed to the text (Emmison and Smith 2000: 14).

What these standards also serve to demonstrate is a prevailing tendency towards *political alignment*. That is to say, the sensibilities expressed by the *Sociology* editors, and which appear to have underpinned the self-censorship of Jenefsky and Miller, have stemmed not from an agreed upon set of institutional safeguards deriving from a centralised intellectual consciousness or professional establishment. Rather, they appear to be based upon an unspoken and largely unquestioned set of axioms concerning where 'our' – and here I'm speaking of 'us sociologists' – political sensitivities *should* reside. It is particularly noteworthy in this respect that, in a manner similar to the shifting codes of etiquette elucidated in *On The Process of Civilisation*, such standards appear to have become increasingly internalised: they can be observed to operate as much through self-censorship as direct editorial control. Elias consistently argued against such unchecked political incursions into sociological investigations, and more generally against the bowdlerising of source material.ⁱⁱ Even today, it is difficult to discuss Elias's work on such topics as nudity, bodily functions and masturbation, without it raising eyebrows in many scholarly circles.

Relativistic Egalitarianism: You Shouldn't Engage in 'Epistemic Privileging'

On the whole, as I have argued in an earlier section of this paper, Elias remains something of an outsider from 'mainstream' sociology. However, by a few authors at a few 'historical moments', his approach has been considered to constitute something of an 'orthodoxy'. For example, in the early 1990s, Dutch sociologist, Dick Pels (1991: 178), went so far as to suggest that, in the Netherlands, 'figurational sociology' had '...risen into what is today one of

the most distinctive, prestigious and successful academic establishments'. This was probably not an over-estimation of figurational sociology's reputation in the Netherlands at the time of Pels's writing, though it is considerably less the case today.

The ambivalence regarding Elias's status as at once a sociological outsider from the mainstream, and simultaneously, as an 'orthodoxy' within discreet sociological enclaves has long characterised some of the critical commentaries on the work of figurational sociologists, particularly within the field of the sociology of sport. Sociologists who have been directly influenced by Elias have, at various times, been described as an 'industry'; sometimes as a 'sect' of 'disciples' who seek only to canonise the 'great man and his work'. Again to return to Pels (1991), such 'figuralists' are said to be typically involved in a politics of theory that entails a kind of intellectual violence based around paradigm conquest. Perhaps most revealingly, Pels charges those who have argued that Elias's work constitutes a sociological 'breakthrough' with 'intellectual immodesty' (Pels 1991: 177) – a term that Elias would, no doubt, have found sociologically interesting, and likely also amusing, given his central analytical engagement with the nexus between power, notions of 'modesty', and the development and transgression of codes of etiquette more generally. As will by now be evident, this latter point is more than simply a frivolous one. Pels was entirely correct in his depiction of Elias as a scholar who would not have been satisfied with the establishment of a 'mere school' based upon his ideas (1991: 177). But Elias's tendency in this respect amounted to considerably more than personal ambition, empire building, 'immodesty', 'shameless' self-promotion, and egoism. Indeed, it was Elias's immense *sociological* ambition – his interest in and passion for the development of sociology as a subject with an expanding knowledge base – rather than a simple preoccupation with self-interest, that arguably underpinned his entire approach. Elias understood the widespread tendency within sociology towards multi-paradigmatic conflict and the more general lack of agreement and consensus regarding the sociological enterprise itself, as themselves constituting problems to be addressed. Indeed, what are the problems of sociological modesty, particularly if such a standard leads the discipline away from any kind of attempt to build a common sociological endeavour, a basis upon which to build and 'advance' a central canon of sociological knowledge? To borrow Kuhn's (1962) terms, sociology has thus far been a mostly

'revolutionary' science, with little or no 'normal' science in its output. While paradigm specialisation, inter-school rivalry, and theoretical heterogeneity have at times been quite significantly productive and of considerable importance to the expansion of sociological knowledge, Elias's view was that these self-same tendencies have also come, particularly from a more recent standpoint, ultimately to impede the development of the discipline.

As suggested earlier, Elias's sociological ambition involved a great deal more than the establishment of a school of process or figurational sociology. His aim was to establish a sociological re-orientation in which he envisaged his own work as representing little more than a theoretical-empirical platform – one which might at best sensitise growing numbers of subsequent researchers to conceive of sociological problems in a relational and processual manner. As such, his ambition was for other sociologists, not simply for himself, to contribute towards a common programme of research-theorising which might, over time, facilitate the establishment of a set of agreed-upon standards, principles, methods, and analytical approaches, together with the development of a body of stable and relatively certain knowledge concerning the social world. Of course, the very notion of theoretical 'reconstruction', and Elias's vision of epistemic consensus-building, transgresses a dominant intellectual code within current sociology premised, to the extent that Rojek and Turner's arguments concerning the 'decorative turn' are accurate, upon the legacy of deconstruction, paradigmatic divergence, and eclecticism. In particular, Elias's undertaking falls foul of the third key sociological 'habits' and sensibilities that prevail today: 'relativistic egalitarianism'. This is a sentiment that finds expression in the promotion of liberal theoretical pluralism over paradigmatic conciliation; eclecticism over synthesis; and specialisation over the development of a common sociological enterprise. It also involves the implicit stipulation that no single perspective or orientation should be elevated or 'privileged' over and above any other, and which underpins calls for intellectual modesty and a diffidence towards building any kind of central corpus of sociological knowledge.ⁱⁱⁱ This sentiment is perhaps the most significant for the future prospects of Elias's sociology, and for the development of the discipline more generally.

Conclusion: Good or Bad Sociology?

It is probably dangerous to draw any hasty judgements concerning the habits of good sociology. As a model, Elias's work in *On the Process of Civilisation* was expressly formulated in such a way as to avoid either dismissing or embracing the shifting standards of behaviour he sought to elucidate. In the case of sociology, such extra-scientific standards are most likely inevitable, though it is arguably 'our' role as sociologists to be reflexively aware of their influence upon our work. We might then ask whether these standards make for 'good' sociology in anything more than a normative sense.

For the most part, the habits of good sociology go unchecked and unquestioned, and yet they have a profound bearing on what can be said, what can be published, what can be funded, how research proceeds, the status of methods and methodology, and the relationship between theory and research. Some sensibilities may well be conducive to the development of the discipline in a productive direction. Indeed, having some kind of agreed upon sociological 'good sense': some common training in the 'craft' of sociology might arguably even be necessary for the development of the discipline (see, for example, Wilterdink 2012) – though the point is moot. Indeed, part of the problem is that the current habits appear to have played a part in impeding the formation of any kind of agreement on what a 'productive direction' for the discipline might comprise and what 'habits' might be desirable in order to facilitate such a development.

As I have shown in this paper, Elias's work transgresses all three of the key sensitivities that I have identified, but it also transgresses many more besides. Elias's tendency to run 'against the grain' of mainstream sociology might go some way to explaining his continued status, relatively speaking, as largely at the margins of the discipline. To return to the question of the prospects of Elias's process or figural sociology in the current era, my position is to argue not so much for re-invention of Elias, but rather, for perhaps repositioning his work. It is, I would suggest, fundamentally important to take seriously the significance of *the habits of good sociology* for pursuing his more general 'scholarly credo' of doing 'our part' in carrying forth the baton of an intergenerational knowledge relay race. Loyal and Quilley (Loyal and Quilley 2004; Quilley and Loyal 2005) have argued that Elias's work

itself might constitute a partial foundation for the development of a 'cumulative science of social processes' – a 'central theory' for sociology more generally. However, under the current social and sociological conditions, the idea of positing a 'central theory' smacks immediately of something unpalatable: of paradigm 'conquest' – another example of how figurational sociologists see 'all roads leading to Elias'. Quite simply, to many sociologists, this is an offensive notion. The sentiment might be expressed thus: 'why do violence to the many other paradigms that also have something to offer'? Ultimately, a great deal depends upon what we mean by a 'central' theory. Elias himself used the term in several places. In employing it, he principally referred to the development of a body of general sociological models that avoided the disconnection and abstraction of 'grand theory'; the somewhat *ad hoc* character of 'middle range' theory; and the myth of inductivism enshrined in 'grounded theory'. To oversimplify somewhat, Elias's notion of a central theory involved the development of processual and relational models based on meticulous, detailed and sensitive empirical observation couched at a level of synthesis sufficiently high to be applicable to a range of topics yet sufficiently down to earth to be clearly related to and relevant regarding the real-life experiences of humans.

My own view is that it is probably unhelpful and counter-productive to position Elias's work as the foundation for all sociology henceforth. Rather, the real value of his work resides in the model of *doing* sociology embedded in his approach. This model is one which, as I have sought to have shown in this paper, is shared in important respects by other key sociologists, notably Pierre Bourdieu. In this respect, Elias is by no means the 'lone voice' and 'maverick scholar' that he himself, alongside several others, has sometimes assumed. His work, and that of those who share some or all of the 'ambitions' of his sociological practice, points the way towards the formation of a common sociological enterprise: a future in which implicit and unchallenged codes of sociological etiquette are increasingly replaced by carefully developed institutional safeguards; where the ratio of heteronomous to autonomous evaluations of the discipline shifts decisively away from the former and towards the latter; and where the 'decorative turn' within sociology might come to be replaced by a 'relational turn' (see Dunning and Hughes 2013). Thus, whether or not 'we' as sociologists 'sign up' to Elias's sociology specifically is largely immaterial. It is more important, I would suggest, that we

collectively subscribe to a thorough investigation of *the habits of good sociology* and begin critically and reflexively to assess the extent to which they are 'good' *for* sociology. Perhaps in tandem with this undertaking, some 'habits' might be consciously replaced, refined, developed such that prevailing sensibilities within the discipline instead favour dialogue between theory and research – not mere 'decoration'; empirical humility always combined with sociological ambition; and might otherwise promote the institutional conditions that facilitate an engagement with one another's work in a manner that permits sociological knowledge to develop such that what 'we say' might be demonstrated to have more value than what 'anyone can say', not simply in terms of how it is said, but through its value to securing small islands of certainty in the vast oceans of our ignorance (Elias 2009).

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ⁱ An earlier version of this paper was presented to the *Reinventing Elias: For an Open Sociology* conference at the University of Amsterdam, 22nd–23rd June, 2012.

ⁱⁱ Elias's sociological approach, while not overtly 'political' as such, nonetheless has a series of political 'ramifications'. For example, his critique of *homo clausus* is *ipso facto* a critique of neo-liberalist conceptions of 'society', 'liberty', and the associated politics of the right. Contrary to the common misconception that Elias was politically 'inactive', his vision of sociologists as 'destroyers of myths', his early work on anti-Semitism, his later work as part of the group analytic circle, and indeed his personal engagement with the student protests in the 1960s are but a few examples which serve to illustrate that this was by no means the case. The 'politics' of figurational sociology, and indeed, Elias's own personal politics are a topic which is beyond the scope of the present discussion. Together with Eric Dunning, I have considered such issues in some depth elsewhere (see Dunning and Hughes 2013).

ⁱⁱⁱ Such a code perhaps finds its clearest intellectual expression in the methodological relativism developed by advocates of the 'strong programme' in sociological studies of scientific knowledge; in particular, in relation to the 'symmetry principle' (see, for example, the work of writers such as Barry Barnes [Barnes & Bloor 1986], David Bloor [1991], and the associated 'Edinburgh school'). The principle, which takes as a methodological premise, the effective rejection of any engagement with the 'reality congruence' of scientific knowledge in accounting for the success or otherwise of competing 'truth claims', has been extensively and convincingly contested, perhaps most notably by Sokal & Bricmont (1998), and as part of the now infamous 'Sokal Affair'.
