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# History and Sociology: A Twenty-First Century Rapprochement?

*Abstract*

This article reflects on the significance of re-analysing material from social-science archives in the context of John Goldthorpe's critique of the use of data from the *Affluent Worker* project. Drawing on my own role in elaborating this approach, most comprehensively in my book *Identities and Social Change*, I defend the value of re-analysis both as a means of bringing out previously unknown popular testimonies, and also in reflecting on the way that social scientific research has itself been a significant force for social change in recent decades. I consider how the practice of re-analysis can be defended even when social-science protocols regarding replication cannot be used, and reflect more broadly the significance of the *Affluent Worker* study in shaping understandings of social change in Britain.

John Goldthorpe's critical essay resonates with me in manifold ways because in a very personal, as well as professional sense, I cut my sociological teeth on the *Affluent Worker* project. Goldthorpe and Lockwood's foundational study has been a beacon in navigating my own intellectual journey, and it is important to introduce my long-term engagement with it to contextualize my comments in this response.

Like Goldthorpe, I moved from undergraduate history into postgraduate sociology. The fact that debates on social class played a central role in both disciplines was an important anchoring point for me in this transition. Here, the *Affluent Worker* project played a central role. My doctorate exploring how the local contingencies of labour market, household, neighbourhood, and gender dynamics drove working-class formation and political mobilization in early twentieth-century Preston, deep in the heart of industrial Lancashire, was inspired not only by debates in labour

and social history, but also by Lockwood's classic paper on 'working class images of society'.[1] My first extended academic post, as a post-doctoral fellow in urban studies at the University of Sussex from 1985 to 1987 led me into the study of contemporary urban change. I rapidly had to learn the sociological trade, rather than the documentary and archival research on which I had previously relied. My first fieldwork was as an interviewer on Peter Saunders' project examining the impact of owner occupation on working-class communities in Slough, Derby, and Burnley. This was a formative experience for me, entailing many weeks of intensive[2] and absorbing household interviewing. It turned out that my socialization into the sociological craft drew on a similar use of a 'critical case study' perspective to that which led Goldthorpe and Lockwood to Luton 25 years before. I was especially fascinated by Slough, which intrigued me because it seemed so different to the declining industrial towns of north-west England which I had studied previously. As an industrial enclave in the heart of the booming 'M4 corridor' which was being hyped as Thatcher's equivalent of California's Silicon Valley,[3] Slough had echoes of Luton. Dominated by its 'Fordist' trading estate, by the 1980s it displayed many of the characteristics of a solidly 'working-class' town, even though it was actually a product of industrial development in the inter-war period. Its high ethnic diversity and relatively feminized labour force meant it was very different from the kind of white male working-class community which was enshrined in the scholarly literature.[4] Goldthorpe and Lockwood's argument that working class had not faded out of history, but was being remade, struck an obvious chord with me.[5]

John Goldthorpe may think that the influence of the *Affluent Worker* has 'been unduly long-lasting', but I would argue that he and Lockwood brilliantly excavated a theme that has enduring significance for reflecting on post-war social change in Britain. Theirs was the first rigorous study

[1] Mike Savage, *The Dynamics of Working Class Politics: The Labour Movement in Preston, 1880-1940* (Cambridge, 1987); David Lockwood, 'Sources of Variation in Working Class Images of Society', *Sociological Review*, 14 (1966), 249–67.

[2] Here and in later passages I will use the term 'intensive' rather than qualitative or in-depth interviewing so that I can include 'largely structured' alongside semi- and unstructured interviews in my reflections.

[3] Indeed, this was the focus of my first published journal article: J. Barlow and M. Savage, 'The Politics of Growth: Cleavage and Conflict in a Tory Heartland', *Capital and Class*, 10 (1986), 156–82.

[4] One spin-off of this work was my historical research on Slough to complement Saunders study, which led to my paper 'Trade Unionism, Sex Segregation, and the State: Women's Employment in "New Industries" in Inter-War Britain', *Social History*, 13 (1988), 209–30. Slough was also the focus of an important discussion straddling history and sociology. M. Glucksmann, *Women Assemble: Women Workers and the New Industries in Inter-War Britain* (London, 1990).

[5] This theme around how class analysis needs to place class formation at its heart came to be an enduring theme in my work. See for instance M. Savage and A. Miles, *The Remaking of the British Working Class, 1880-1940* (London, 1994).

to explore empirically what might be termed the 'after-life' of a particular heroic conception of the 'industrial white male working class' using (for the time) state-of-the-art interview methods. Very similar issues to those that preoccupied Goldthorpe and Lockwood have reappeared, in different guises, for every generation since then. Peter Saunders believed that the Conservative Thatcher government in the 1980s was creating a Tory-aligned popular bloc on the basis of council house sales, privatization and the proliferation of new consumer durables such as video recorders and hi-fi. During our regular evening chats (since we conducted the fieldwork in intensive week-long bursts with several research staff decamping to Slough, Derby, and Burnley and we routinely met over dinner to discuss the day's interviews), the *Affluent Worker* study was a regular point of comparison. Our research methods, involving a structured interview schedule with some opportunities to write down more open-ended responses was very similar to theirs.[6] I vividly recall Peter Saunders telling me that the *Affluent Worker* thesis was ahead of its time, and that even though Goldthorpe and Lockwood may have been correct to question its relevance for Luton in the 1960s, their critical arguments would no longer stack up by the 1980s—affluence and social change had indeed deconstructed the working class. By contrast, I was firmly supportive of Goldthorpe and Lockwood's scepticism that class divisions had been fundamentally eroded. The fact that I had played a role in identifying the case studies, with the fieldwork located on three industrial towns with strong histories of Labour Party identification, rather than metropolitan or suburban areas, led to light-hearted (but also rather pointed) banter. I remember at one point Peter telling me and Mark Bhatti, the dedicated post-doctoral fellow who worked on the project, that we 'had taken him to the only three places in Britain where his arguments did not apply'.[7]

Even though I played no role in writing up this project, the experience inspired me to explore the remaking of class relations in late twentieth-century Britain, through studies of middle-class formation and of gender

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[6] In retrospect, this was probably one of the last major interview-based projects carried out before the development of lightweight recorders in the 1980s meant that the mechanical recording of interviews became ubiquitous.

[7] This is an anecdote from memory and not a recorded quote. Saunders' *A Nation of Homeowners* (London, 1990) is an under-appreciated classic. He was himself an inspiring and generous academic mentor, and although political differences amongst us become increasingly apparent as he was drawn to 'new right' politics whilst the research team was allied to different versions of Marxism and feminism, this is not to decry the intellectual energy and significance of the research. As well as for myself, this project provided formative fieldwork experience for numerous 'early career' social scientists who have gone onto leading academic careers including Mark Bhatti (later at the University of Brighton), James Barlow (Imperial College), Susan Halford (Bristol), and Lisa Adkins (Sydney). It is interesting that both Lisa Adkins and myself have in recent years emphasized the significance of asset accumulation, including in owner-occupied housing, which might also now suggest that Peter Saunders was ahead of his time, being prescient about the rise of wealth inequality in a decade when this was still only nascent.

divisions in career mobility.[8] By the later 1990s my historical interests had tapered off and I imagined that I had fully become a sociologist. In 1999 I began a major project with Gaynor Bagnall and Brian Longhurst which also contained echoes of *The Affluent Worker*. We spent many weeks interviewing 182 residents of Cheadle, Chorlton, Rambsbottom, and Wilmslow in the Manchester area to consider how globalization and social change were affecting contemporary lifestyles. We interviewed significant numbers of manual and routine manual workers, especially in Cheadle and Ramsbottom. This project led us to elaborate yet another version of the '*Affluent Worker* revisited' motif—that despite the blandishments of globalization and affluence, territorial identities were re-inscribed, and persisting identities of class cross-fertilized with 'whiteness' and a revived nationalism, with a distinctively northern English flavour.[9]

Our interview material provided a new empirical springboard for revisiting debates on working-class consciousness and identity which had largely run aground in the 1960s. We became fascinated by the ways in which many of our interviewees talked about class—with people articulating high levels of awareness of the depth of structural class inequalities yet also being loath to identify themselves as belonging to a class position, preferring to articulate ambivalent and uncertain class identities.[10]

It was at this juncture that I was made aware of the existence of the Qualidata archive and came to see a way of renewing my historical interests through revisiting its qualitative social-science sources. I want to emphasize in the context of Goldthorpe's critique how exploratory and provisional I saw my inquiries, which I framed as 'feasibility studies' to assess whether this could be a promising line of inquiry. When I submitted my proposal for a Leverhulme Major Fellowship (to provide me with the dedicated time to work on this project), I was aware of the important work of Foucauldian scholars, especially Nikolas Rose on the imprint of the psy-sciences in Britain but was struck that there was little or no literature exploring the significance of sociological studies.[11] There seemed to be an arresting opportunity to scope out uncharted new ground, but I had few pre-conceived ideas about what I might find.

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[8] M. Savage, et al., *Property, Bureaucracy and Culture: Middle Class Formation in Contemporary Britain* (London, 1992); S. Halford, M. Savage, and A. Witz, *Gender, Careers and Organisations: Current Developments in Banking, Nursing and Local Government* (Basingstoke, 1997).

[9] M. Savage, G. Bagnall, and B. J. Longhurst, *Globalization and Belonging* (London, 2004).

[10] Mike Savage, *Class Analysis and Social Transformation* (Milton Keynes, 2000); M. Savage, G. Bagnall, and B. Longhurst, 'Ordinary, Ambivalent and Defensive: Class Identities in the Northwest of England', *Sociology*, 35 (2001), 875–92.

[11] N. Rose, *The Psychological Complex: Psychology, Politics and Society in England 1869-1939* (London, 1985).

Goldthorpe's critical engagement with my work on the *Affluent Worker* archive (as well as that of the historians he discusses, on which I will leave them to reply for themselves) means, I suppose, that this line of inquiry has indeed become an established repertoire of inquiry, certainly in history (though in sociology there has been far less interest in using archived qualitative sources). Previously, I have benefitted from exchanges with Elizabeth Bott Spillius[12] and Ray Pahl,[13] and naturally I am now pleased to respond to John Goldthorpe's reflections. I thank him for the extent to which he has critically and seriously engaged with my writings. His interest—although critical—is surely telling evidence that the use of archived qualitative data has now become mainstream to the intellectual project of understanding social change and it is in this spirit that I now turn to reflect more specifically on the points that he raises.[14]

I firstly reflect on his discussion of the 'provenance' of the *Affluent Worker* study before secondly considering his more specific objections to my analysis of the data. In a long conclusion I broaden my reflections to consider how my re-analysis of the *Affluent Worker* material informed my reflections on disciplinarity, and especially the relationship between history, sociology, and anthropology.

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[12] Elizabeth Bott Spillius made some very generous and supportive comments on a draft of my paper 'Elizabeth Bott and the Formation of Modern British Sociology', *Sociological Review*, 56 (2008), 579–605. I subsequently invited her to take part in a special issue of the *Sociological Review* which commemorated the work of Ronnie Frankenberg and considered the relationship between sociology and anthropology. See E. B. Spillius, 'Anthropology and Psychoanalysis: A Personal Concordance', *Sociological Review*, 53 (2005), 658–71.

[13] Pahl disputed my interpretation of his work in *Identities and Social Change*, Oxford, 2010, and I encouraged him to write his criticisms into a book review which appeared in the *Sociological Review*. I reflect further on this encounter in my consideration of his major study on the Isle of Sheppey in my 'Postscript' to G. Crow and J. Ellis, eds, *Revisiting Divisions of Labour* (Manchester, 2017).

[14] Goldthorpe's paper contains several assertions about my supposed misreading of his study which in my view are inaccurate or partial readings. In order not to clutter up my text, I will use this footnote to draw attention to a few which don't form part of my broader reflections. (1) I explicitly recognize that the *Affluent Worker* project was not original in turning from the social problem agenda (Goldthorpe, 3), where I attribute a much more significant role to Elizabeth Bott. (2) I consistently point out that the *Affluent Worker* project was a response to the debate on Labour's electoral failures (see Goldthorpe, 3-4)—e.g. 'To be sure, a political problem—whether the working class electoral base of the Labour Party would decline as a result of affluence – had been identified by political commentators (especially ABRAMS 1960). GOLDTHORPE and LOCKWOOD were clearly keen to engage with this public debate about the significance of the affluent workers for class alignments and political change, but on their own, explicitly sociological, terms' (Savage, 'Revisiting Classic Qualitative Studies', *Historical Social Research/HistorischeSozialforschung*, 2005, p 118-139, cited on p129), or my comment about the study '[o]riginating as an inquiry into the argument made by Abrams and Rose (1960) that the electoral weakness of the Labour Party was due to the growing embourgeoisement of sections of the working class' (Savage, 'Working Class Identities in the 1960s: Revisiting the *Affluent Worker* Study', *Sociology*, 39 (2005), 929 - 946, 931). (3) I acknowledge that Goldthorpe's 1970 paper elaborated his account of class consciousness (see his n. 34 where he claims it is disregarded) in Savage, 'Working Class Identities in the 1960s', 932.

## The 'Provenance' of the *Affluent Worker* Study

One of the intriguing features of Goldthorpe's paper is to modify his well-known position differentiating the purposes of the historian and the sociologist in their relationship to data, since the former was forced to rely on 'relics', whilst the latter had the capacity to collect new data specially devised to address their research purposes.[15] He retains this argument in his current critique, noting that historians (and by extension, sociologists such as myself with historical interests) are indeed free to use data—as relics—without feeling constrained by its originating purposes: 'Insofar as they believe that such material is relevant to their purposes, it is of course entirely appropriate that they should exploit it.' However, Goldthorpe also opens up a new angle which I have not previously seen from him. It seems that he still wishes these sources to be used in a way that was consistent with his own training in history which he picked up as an undergraduate student at UCL in the 1950s. To my knowledge, although Goldthorpe has regularly intervened to argue for what he sees as the best mode of sociological inquiry, he has always been scrupulous in not telling other disciplines how they should operate, recognizing that this is outside his purview. The fact that he is now telling historians how to go about their business is therefore an intriguing development, testifying that the boundaries between history and the social-sciences disciplines may be rather more blurred than he has previously imagined.

Goldthorpe's reflections on the historical method are to some extent uncontroversial, and indeed his emphasis that it is necessary to unpack the 'detailed consideration of the *provenance* of documents' is entirely consistent with the approach I adopted.[16] Thus, I did not approach the various archived sources as 'blind data' which could be re-analysed afresh, as if the data was not embedded in the plans and procedures of the originating research team. I was highly attentive to the principles of collection, analysis and publication which sociologists developed, and made the intentions of the various researchers central to my own interpretation. I therefore welcome Goldthorpe's concern to clarify the provenance and purposes of the *Affluent Worker* study, and his paper contains some fascinating vignettes in this regard. Indeed, his contextualization of the study in terms of wishing to differentiate sociological research from the 'gentlemanly' anthropological tradition resonates with the argument I made in

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[15] J. H. Goldthorpe, 'The Uses of History in Sociology: Reflections on Some Recent Tendencies', *British Journal of Sociology*, 42 (1991) 211–30.

[16] Though, as I discuss below, the provenance of documents extends well beyond understanding the motives and intentions of those who solicited them. Historical analysis is replete with outstanding studies which draw out issues that the originators of sources were unaware of. Indeed, an awareness of the role of unintended consequences in social change is another issue that sociologists and historians (as well as anthropologists, and indeed scholars across the humanities and social sciences) have in common.

Chapter 5 of *Identities and Social Change*, and that '[i]n the context of the rapid expansion of the discipline of sociology in the 1960s, the study acted as a template as to how the nascent discipline of sociology could comport itself and carve out a distinctive role vis-à-vis its better established neighbours in the social sciences.'[17]

However, recognizing the 'provenance' of historical documents requires going beyond the intentions and motivations of their originators. Any kind of critical research depends on the adroit analysis of sources to reveal issues which historical actors themselves may be unaware of. Goldthorpe's paper is fascinating, but we don't also have David Lockwood's account (or that of other team members who also helped shape up the project). Goldthorpe's more recent advocacy of quantitative sociology committed to the value of survey analysis, and his scepticism towards qualitative research, may also affect recollections of his earlier work, through the normal process of selective memory and recall bias.

One of Goldthorpe's main critical points is that 'the *Affluent Worker* study was not, and was never designed to be, a community study in the conventional sense' and 'our interest in Luton *qua* community was limited to those features of it that were relevant to what we did wish to have: samples of manual workers who could be studied as a critical case in appearing to be the most promising candidates for *embourgeoisement*'. However, I have never claimed that the *Affluent Worker* project was intended to be a community study 'in the conventional sense'. Indeed, Chapter 6 of my *Identities and Social Change* explicitly argued that agreed conventions for community studies failed to be established during the 1960s. This indeed led to the decline of the field and the increasing hegemony of nationally representative survey research. What I argued was that the study itself, as it took shape during the 1960s, turned away from the diffuse tradition of conducting fieldwork in community settings to articulate a different model of sociological expertise which championed a nationally oriented study of social change. Goldthorpe's comments here are testimony to exactly my point.

Building on this, let me follow Goldthorpe's advice to revisit his 1963 paper with Lockwood which he emphasizes was central to setting out the aims of the *Affluent Worker* study. In insisting on the need to address the 'relational aspect' of class relations, they clearly situated the need to place the project within some kind of community frame. They observed that

> It does not appear to have occurred to those who argue that the worker is being merged into the middle class that this implies that within communities, neighbourhoods and associations long-standing social barriers are being broken down and that manual workers and their

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17 Savage, 'Working Class Identities in the 1960s', 129.

families are being accepted as equals into status groups from the which they, or their kind, were previously excluded. This is all the more serious because such an implication would not be supported by the relevant evidence at present at hand. A variety of studies carried out in different parts of Britain over the last ten years or so have pointed to a marked degree of status segregation in housing, in informal neigh-bourhood relations, in friendship groups, in the membership of local dubs, societies and organisations and so on.[18]

This quotation clearly identifies a lineage to traditions of community-based research, and indeed the research design of the *Affluent Worker* study, which conducted interviews at work *and at home*, was entirely con-sistent with this concern to draw out the 'relational' community-based aspects of the lives of affluent workers, rather than to construe the study specifically as focused on industrial and employment relations alone.

To be sure, it may have been David Lockwood rather than John Goldthorpe who was most keen on this orientation. Lockwood's famous 1966 article made his debt to anthropology very clear. In terms which echo the Manchester school of anthropology inspired by Max Gluckman, Lockwood wrote that

for the most part men [*sic*] visualise the class structure of their society from the vantage points of their own particular milieux, and their per-ceptions of the larger society will vary according to their experiences of social inequality in the smaller societies in which they live out their daily lives. This assumption that the individual's social consciousness is to a large extent influenced by his immediate social context has al-ready proved its usefulness in the study of "images of society" and it has been stated most clearly by Bott.

In elaborating the differences between different 'working-class images of society', Lockwood was at least as attentive to the significance of com-munity relations as he was to the nature of employment.[19]

It may well be that Goldthorpe himself, with his background in indus-trial sociology, was always less committed than Lockwood to this 'rela-tional' orientation, and it is revealing that after the *Affluent Worker* project Lockwood took a different approach, continuing to focus on how issues

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[18] J. H. Goldthorpe, and D. Lockwood, 'Affluence and the British Class Structure', *Sociological Review*, 11 (1963), 133–63, 138.

[19] Lockwood, 'Working Class Images of Society', 249. See also on proletarian images of society: 'These primary groups of workmates not only provide the elementary units of more extensive class loyalties but work associations also carry over into leisure activities, so that workers in these industries usually participate in what are called *occupational communities*"' (251); or '[B]asically, the pecuniary model of society is an ideological reflection of work attachments that are instrumental *and of community relationships* that are privatised.' (256; italics mine)

of solidarity and cohesion could be linked to the analysis of stratification and differentiation.[20] However, it would be mistaken to write out those aspects of the *Affluent Worker* project which can trace a lineage to this influential body of community-based research.

## Re-Using the Archived Data of the *Affluent Worker* Study

Goldthorpe's main objection is to my (and to various historians') use of the archived data held at the Qualidata archive. He claims that our use of the data is insufficiently rigorous: 'If the "interpretive qualitative methodology" that [Savage] invokes does exist—which I take leave to doubt—it does not, at all events, appear to provide its exponents with any principles that they can set out on how the selection of snatches is to be made. "Pick and mix" would seem entirely permissible.' Goldthorpe is especially concerned about the use of the interviewer notes which were not part of the research material of the study and the inappropriate use made of the free comments made in the specific question asked about class consciousness.

Goldthorpe's concerns are entirely legitimate and require a proper response. I will make four linked replies.

First, I want to be clear that I was not using the data to test or dispute the conclusions he and the research team took. I am puzzled by Goldthorpe's assertion that I, like the historians he criticizes, 'have then taken their analyses as a basis for critical commentary on what they believe to be the conclusions reached in the original research both on class imagery and more widely, and in particular as regards the prevalence of, and the significance of, "money" models of class over either "power" or "prestige" models'.

I am clear in all my writings that *in the terms they themselves set*, Goldthorpe and Lockwood's arguments are not undermined by my re-analysis. Precisely because qualitative re-analysis cannot replicate or test the arguments of the originating scholars, in the way that is now conventional for quantitative data, it follows that re-analysis cannot somehow adjudicate over the original claims drawn from it. Qualitative data are generally not fit for these purposes, being imperfectly collected, partial, and located in a specific historical and geographical context. It follows that if the purpose of qualitative re-analysis is to mimic replication along the lines of survey-based analyses, it falls short, and has limited, if any, value. This is a major theme of my article reflecting on the methodological implications of qualitative reanalysis.[21]

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[20]  D. Lockwood, *Solidarity and Schism: 'The Problem of Disorder' in Durkheimian and Marxist Sociology* (Oxford, 1992).
[21]  Savage, 'Revisiting Classic Qualitative Studies'.

However, this also means that re-analysis does not need to be constrained by the terms of reference set by the original researchers. As I stated, 'whilst we can say that the data probably is consistent with GOLDTHORPE and LOCKWOOD's interpretations, this does not mean that their interpretations are necessarily the best ways of interpreting the data. There is the potential here to use other research strategies today to suggest alternatives.'[22] Thus, I sought to draw out the qualitative aspects of issues which might be of further interest in the context of the recent studies such as those that I had conducted in Manchester in the 1990s regarding the ambivalences and nuances of class identification.

Secondly, in justifying my extensive use of quotation and vignette, I can't do better than cite Goldthorpe and Lockwood's own programmatic arguments for the *Affluent Worker* project. In criticizing the use of simplistic market research categorizations of class, they make clear their objection to crude quantification:

In the first place, it is known that responses to such a question as 'To which social class would you say you belonged?' may vary significantly depending on whether the respondent is given a pre-determined choice of class categories or whether categorisation is left 'open'. Secondly, it is also known that where pre-determined categories are used (which is usually the case), wide variations can again be produced in the pattern of response according to the particular class designations which are offered—for example, according to whether 'lower class' is used instead of, or in addition to, 'working class' or not at all. Thirdly, and perhaps most importantly, *it has become evident that responses to questions on class identification which are nominally similar, and which will thus be grouped together by the investigator, may in fact have very different meanings for the various persons making them. . . .* It is our view, then, that *if questions of class identification and class norms are to be at all usefully investigated through interview techniques, the pollster's overriding concern with easily obtainable and easily quantifiable results must be abandoned and an effort made to do justice to the complexity of the issues involved.*[23]

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[22]  See also the similar statement in Savage, 'Revisiting the Affluent Worker Study', 942: 'it is worth reiterating, once again, the impressive nature of the study itself. The authors were not wrong: Goldthorpe et al.'s refutation of claims that work relations and technologies determined workers' actions is in many respects underscored by my re-study. My emphasis about the power of individualist notions of class can be closely allied to their own stress on instrumentalism and the "action frame of reference". However, with respect to their arguments about class identities, I have a critical rider to add. Goldthorpe et al.'s findings about the nature of class identities and the dominance of money models of society only stand up in the context of the analytical categories they deployed, namely the distinction between money, power and status models of society. Although the data can be interpreted in these terms, this unhelpfully obscures the close association between these axes in the minds of the respondents.'
[23]  Goldthorpe and Lockwood, 'Affluence and the British Class Structure', 143, 145 (emphasis added).

Goldthorpe and Lockwood rightly point to the problems of imposing ex post facto typologies onto data and in ripping accounts out of the context in which they were gathered. I contend that my provision of contextual detail through the use of extended quotations, as well as my reflections on the nature of the research process itself, is entirely consistent with their own stated wishes, and that reducing my analysis to counts and frequencies would not do justice to this orientation.

Thirdly, however, these first two points do not provide an adequate defence to Goldthorpe's objections that the data are being used selectively, and that there needs to be clarity about my own principles for selecting vignettes and passages for quotation. Goldthorpe is concerned about my use of 'phrases such as "most", "nearly all", "many", "a few", "very few", "some", "nearly always", "often" and "rarely"', rather than more exact counts. However—as he also emphasizes—because of the semi-structured nature of the questioning at this point, as well as the varied styles of the interviewers, any lack of reference to a term or issue does not mean that it was necessarily absent, but rather that it was not recorded or inquired about. When I was confident that questions were asked and recorded systematically, I did give counts, for instance around the specific number of classes that respondents identified. Thus, I noted the larger proportion of Luton workers identifying an upper class compared to those in Richard Brown's Wallsend study.[24] However, to have given strict frequencies for every issue that interested me would have given spurious exactitude, given the qualitative nature of the data.

This observation does not mean that 'anything goes'. The approach I adopted is consistent with Goldthorpe's own advocacy of the 'critical case study' approach. Recognizing the diversity and complexity of the material recorded in the notes, I deliberately focused on the testimony of those respondents who, on the face of it, talked extensively about money, but then went on to show that they also frequently linked this to power and status. A more fully qualitative reading of the accounts of those who on the face of it seemed to endorse Goldthorpe and Lockwood's conclusions could thus reveal that talking about money was not necessarily exclusive of power and status considerations.[25] This allowed me to query the ideal types differentiating these terms, which Goldthorpe and Lockwood had projected onto their findings, and to suggest alternative interpretative frames.

This is my fourth point. I became increasingly interested in the way that 'individuality', 'nature', 'society', and 'authenticity' were being articulated. These were not topics that seemed possible to render

[24] Savage, *Identities and Social Change*, 218–19.
[25] There is no doubt that if the coding decisions used by Goldthorpe and the research team were available, this would allow an even fuller consideration. However, I never came across such notes in the archive.

meaningfully through numerical counts. What would I be counting? More than this, given how rarely the actual lay terms used by manual workers have been recorded in historical sources, I did indeed want to render them fully and in qualitative richness, rather than treating them as needing to be put in their place by expert social scientists.

Let me be clear here as I do not want to be wrongly pigeon-holed as some kind anti-quantitative sociologist who does not believe in the value of statistical research methods. Especially now that the re-reading of the *Affluent Worker* archive has gathered such interest, it could be valuable to provide a more extended, formal textual analysis than I conducted. At the time of my study, the notes were not digitized and, given the large amount of handwritten material that existed, I could only selectively type notes during hurried day trips from Manchester. Although I was systematic in my note-taking, I was able to extract only a small selection of the available textual material during these visits, and it is certainly possible that I missed important material, and that my own preferences got the better of me in deciding what to note down. Furthermore, my Leverhulme Fellowship, constructed to reflect on the feasibility of numerous social-science archives being re-used to examine social change, meant that I could not dedicate myself to the *Affluent Worker* material alone.[26] Readers will need to judge for themselves whether my endeavour to synthesize across a variety of sources, the extent of which is most fully captured in my book *Identities and Social Change*, has come at the cost of providing insufficiently comprehensive accounts of each of them. I was certainly well aware of this trade-off throughout my studies and ultimately made a judgement call which I might have got wrong.

Anyway, this is now by the by: given that the feasibility of restudying this kind of qualitative social-science data seems to have been well and truly established, then a more systematic analysis involving the importation of all the text into machine-readable form and using qualitative software and appropriate coding schemes and analytical techniques would surely be of great interest and I would be entirely supportive of this project. The findings of such analysis might indeed lead to a challenging of my earlier arguments.

## Conclusion: Disciplinarity in the Twenty-First Century

I finish by broadening my discussion to draw out the wider stakes of this debate, which is not just a specific discussion of the work of myself and

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[26] A full account of the range of different sources that I consulted is included in *Identities and Social Change*, Appendix. In particular, balancing my research in the vast Mass Observation archive at the University of Sussex along with the Qualidata material mostly at the University of Essex proved a real challenge especially as child-care responsibilities meant that I could rarely have overnight stays in either Brighton or Colchester.

selected historians. Goldthorpe's critical account needs to be interpreted in the context of his long-term concern to define and maintain and police disciplinary boundaries, in which he defends a specific mode of socio-logical analysis, one which he increasingly sees as bound up with quanti-tative demography and with little or no role for qualitative inquiry.[27] Perhaps, one of the reasons for his criticisms of my work is that I don't sign up to this vision of capital-S Sociology, and indeed my re-analysis of the *Affluent Worker* project was formative in leading me in other directions which emphasize the vital need for interdisciplinary investigations that are thoroughly historical in orientation. I see this as exactly the direction of travel which we need to champion, for much the same reason that the economist Thomas Piketty makes central to his account of inequality trends.[28]

I have already noted that when I began my studies of historical social-science archives in 2002, I wasn't really clear how I would use the mater-ial, or what light it would shed on the arguments Gaynor Bagnall, Brian Longhurst, and myself had elaborated in Greater Manchester as part of our study of globalization and belonging. To be sure, I had my own ama-teurish take on Walter Benjamin's critique of historicism and wanted somehow to put the past in dialogue with the present in a more active way than simply seeing past social-science archives as recording the for-bears of what was to come. But other than that, I was surely rather naive.

Goldthorpe is quite right that one of the revelations of the material was the interviewer notes, which contained such rich evocative accounts of the fieldwork experience at the very moment when it was becoming established as a legitimate research method. The same kind of evocative material also turned up in numerous other sources that I studied, such as those of Elizabeth Bott, Brian Jackson, or Ray Pahl. There was a sense of the melodramatic encounter between young (generally male, invariably in these sources white) social scientists and the working-class 'other' whose lives, attitudes, and values were somehow being opened up and exposed through the novel use of intensive interview methods. Goldthorpe thinks that my use of the interviewer notes is somehow im-proper. Of course, I fully appreciate that it was never used by the researchers in their analysis of the data, and it was only from my own perspective that this seemed so interesting—for this material struck a chord because of the similarities and differences from my own experience as a fieldworker.

Compared to the more 'choreographed' interview experiences that I had become aware of in my fieldwork from the 1980s into the early 2000s,

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[27] J. H. Goldthorpe, *Sociology as a Population Science* (Cambridge, 2016).
[28] See Thomas Piketty, *Capital in the Twenty-First Century* (Cambridge, MA, 2014), and *Capital and Ideology* (Cambridge, MA, 2020). This issue is a major theme of my *The Return of Inequality: Social Change and the Weight of History* (Cambridge, MA, 2021).

the *Affluent Worker* interviewer notes seemed to hark back to more pro-
tean kind of encounter. Neither interviewer nor interviewee understood
the 'rules of the game', leading to interviews of different lengths, inter-
viewers being invited to stay for dinner, sometimes even overnight, and
things were said—and written down—which in the context of twenty-
first-century sensibilities look entirely inappropriate for recording. I was
certainly aware that—as Goldthorpe confirms—one of the interviewers
was especially prone to these melodramatic encounters but this was still
intriguing, especially as it was sometimes clear that they were sometimes
discussed amongst the wider research team. Struck by the contrast with
my own fieldwork experience, I then reflected back to my own interview
experience in Slough, Burnley, and Derby in the 1980s and saw how my
experiences here still retained some elements of the ambiguities which
were evident in 1960s Luton. Some interviewees weren't sure what to
make of me, and there seemed more frankness than was evident in my
later fieldwork.

In short, I realized that I might be witnessing the historical life course
of the intensive interview method itself. Subsequently, my account of this
trend came to occupy a central role in my book *Identities and Social
Change*.[29] The *Affluent Worker* project took place at the dawn, what might
be seen as a certain kind of 'golden age', of the intensive interview. By the
1980s, the interview had reached mature middle age. By the 1990s and
into the 2000s it was becoming increasingly choreographed and subject to
game playing which affected the nature of the data being generated. This
argument has only been confirmed by my later fieldwork experience.
When Andrew Miles, Jane Elliott, Sam Parsons, and I conducted inter-
views with a sub-sample of the 1958 National Child Development Study,
the choreographed nature of the interview had become very clear. By this
time, upwardly mobile men had skilfully learned to narrate their 'modest
stories' which managed to downplay their success whilst still drawing at-
tention to it.[30]

In short, intensive interview methods, just like all social-science meth-
ods, are fully part of history. It follows that rendering their trajectories as
some kind of technocratic teleological journey towards more sophistica-
tion and rigour according to the protocols of their insider methodological
gatekeepers (with the consequent ticking off of those who depart from
these) is entirely at odds with a fully historical—and also sociological—

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[29] And see also M. Savage, 'Elizabeth Bott and the Formation of Modern British
Sociology'.
[30] See A. Miles, M. Savage, and F. Bühlmann, 'Telling a Modest Story: Accounts of Men's
Upward Mobility from the National Child Development Study', *British Journal of Sociology*,
62 (2011), 418–41. This theme is also evident in many recent qualitative studies of class iden-
tities, e.g. S. Friedman, D. O'Brien, and I. McDonald, 'Deflecting Privilege: Class Identity
and the Intergenerational Self', *Sociology* (2021), online first.

understanding of what has come to be called 'the social life of methods'. Here, Goldthorpe and I clearly must agree to disagree about the value of scholarship from science and technology studies. There is a complicity of methodological expertise of various kinds in contemporary social change which we need to fully recognize. It follows that I do not think it is profitable to police disciplinary boundaries as they were established during the twentieth century, nor do I think we should be constrained by the disciplinary politics of previous eras.[31]

In pondering Goldthorpe's comments, I am struck by the fact that he and I have both worked on the boundaries between history and sociology, except that whereas he became increasingly bound up with a particular type of sociological identity and made clear how he differentiated between the scope of history and sociology, I was ultimately drawn back to the need for historical analysis, not just as a supplement, but as actually necessary for a sociological analysis of social change (which is to say, pretty much all good sociology).

The issue of disciplinarity does not just concern the relationship between history and sociology. Goldthorpe's paper is revealing on how he felt it necessary to differentiate the *Affluent Worker* project from anthropology, given the suspicions of Meyer Fortes and the gentlemen of Cambridge anthropology to the emerging sociological project. This certainly makes it clear why the *Affluent Worker* team came to prefer more formal analysis of the data, even though this may not have been prefigured in their initial intentions. More broadly, Goldthorpe was a key player at the time that the disciplinary boundaries of British sociology were bring forged, and his fascinating discussion of his repudiation of American Cold War sociology sheds much light on his resistance to American social stratification research in later decades and his strong advocacy of European collaboration. The after-effects of this formative moment of disciplinary formation can still be felt in the distance that many British sociologists keep from American sociology. However, none of this means that we still need to be constrained today by the 1960s departmental politics of Cambridge University, fascinating though this might be. We are no longer trying to build the discipline of academic British sociology largely from scratch. As I make clear, for instance in my interest in Marilyn Strathern's anthropological excavations of 'Englishness', I see the differentiation of sociology from anthropology as having little value these days. We no longer need to fight these battles in the twenty-first century if we are to fashion a critical, and historical, social science that we sorely need.

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[31] This is a theme I have elaborated at length in my discussion of the challenges of inequality in *The Return of Inequality*.

I hope these remarks are helpful in reflecting on the pertinence of John Goldthorpe's paper. I have tried to show how my use of the *Affluent Worker* data needs to be read in terms of my broader purposes, which have themselves changed over time. It is for others to judge my success and whether my methods of data extraction and analysis are sufficient for these purposes. I fully agree with Goldthorpe that there is much more that could be extracted from the *Affluent Worker* data, and I look forward to seeing this vital work flourish in the future.