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THEORETICAL / PHILOSOPHICAL PAPER

On *The Social Construction of Reality*: Reflections on a Missed Opportunity

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Abstract The paper recalls my response to Berger's and Luckmann's book on reading it shortly after its initial publication. It seeks to convey why it was that I failed to make use of the book at that time, even though I recognised it as an outstanding contribution to my intended field of research, and how later I came to see that this may have been a lost opportunity. The story touches upon diverse important issues including the relationship between epistemology and the sociology of knowledge; the epistemic authority of the natural sciences; the relevance of causal accounting as topic and resource in sociology; the importance of Durkheim in the sociology of knowledge; and the great value of Berger's and Luckmann's book as a corrective to the undue individualism that has long been a feature of the social sciences in the English-speaking world. Even so, the paper is more recollection than analysis, and unreliable recollection at that, after many decades in which there has been time to forget, or even to reconstruct, a very great deal.

Keywords Peter Berger · Thomas Luckmann · The Social Construction of Reality · Sociology of knowledge · Sociology of scientific knowledge · Epistemology · Phenomenology · The institution of causal connection · Individualism · Sociality

Introduction

The Social Construction of Reality was first published half a century ago, in a year that marked the onset of interesting times for Europe, just as its anniversary year seems likely to do. The book was published in Britain a year later, in 1967, the year in which I took up my first academic appointment, in the Science Studies Unit, then

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a part of the Science Faculty at the University of Edinburgh. And as the sociology of knowledge was something I was hoping to work on and perhaps to teach, it was not long before I read it. I was much impressed. Although an essay in social theory and by no means long, the book was packed with illustrative materials: empirical findings, inventions, anecdotes, all described with a clarity and lightness of touch that at once made the book readily accessible and ensured that its basic argument was never obscured. Its account of the sociology of knowledge, which amounted to a far-reaching reconceptualisation of the field, included both fundamental claims I was happy to accept and a number of additional suggestions that may or may not have been right but were certainly interesting. The heuristic value of some of these suggestions can be confirmed by a glance at subsequent research. As to the fundamental claims, I continue to find them convincing half a century later: indeed I find it difficult these days to grasp why they are still occasionally found problematic.

What though were these fundamental claims? There were three, which I shall set out without attempting to stick precisely to the terms used by the authors. As I now see it, their *key claim* was that human beings are interacting knowledge-carriers. This is how humans are invariably encountered, and why they invariably need to be studied from the standpoint of the sociology of knowledge. Of course, individual humans arrive in the world possessing little or nothing in the way of knowledge and this prompts a *second fundamental claim*: humans are born with a 'predisposition toward sociality' (Berger and Luckmann 1967: 149) that results in their acquiring knowledge in the course of becoming members of society. More specifically, neonates acquire their initial stock of knowledge from carers and immediate others as they become members of particular knowledge-carrying collectives located in particular situations, and there are many such collectives, wherein members share and sustain different knowledges, and employ them, the authors tell us, to construct 'multiple realities'. Berger and Luckmann rightly insist that knowledge cannot be understood as an individual possession. Humans, we might say, are conscious creatures: they know with others; their knowledge only exists and persists as something they share, and what it consists in must be identified and sustained collectively. This brings us to the authors' *final fundamental claim*: knowledge is collectively constituted and sustained in social interaction; without such interaction there would be no knowledge. And of course, given their second claim, we much accept that some of this interaction is inter-generational and that members' knowledge, even as it changes, is thereby passed down across the generations and always initially acquired from the ancestors.

There are of course other perfectly defensible views on what should count as of fundamental importance in the book and what not. I merely set down my own opinion, conditioned no doubt by the setting I found myself in at the time. These three claims were important 50 years ago in the English speaking world for calling attention to features of human beings and their knowledge producing activity that, whilst indeed fundamental, had hitherto often been ignored or else disparaged. And the contribution they made toward remedying this, and opening new paths for the social sciences in doing so, is more than enough to justify giving recognition to the achievement of the authors at this time.

As to their further suggestions—including those concerning objectification, typification, reification, and sacralisation; and those involving the distinction between primary and secondary socialisation and its use in understanding the division of both physical and intellectual labour—there was little in the discussion of these to detract from my positive evaluation of the book, and a good deal that reinforced it. Notwithstanding this, however, I was never to make extensive use of the book, or other works of the two authors, and it was some considerable time before I looked in proper detail at the work of Alfred Schutz, whose thought shone forth from its pages as strongly as that of the authors themselves. Even today I am insufficiently equipped to offer scholarly commentary on the book, enriched by proper appreciation of its context and background, and can contribute little more to the present volume than memories deriving from the fortuitous circumstance that I was around 50 years ago, pursuing academic interests that were broadly similar to those of the authors. It may also be, however, that my slightly unusual background at this time, which had left me initially detached from many of the contemporary debates and controversies in the social sciences and philosophy, will give what I manage to recall some additional interest.

Everyday Knowledge, Sciences, and Scientific Methods

I'll begin with my initial encounter with Berger's and Luckmann's book, and my failure to make active use of it. When I took up my post in 1967, it was as someone with many years of training in the natural sciences and just a single year's hectic induction into the social sciences; and one of my aims was to understand what natural scientists did, including what I myself had earlier done, as human activities. Their book was, on the face of it, just what I needed and provided what appeared to be an excellent starting point for the sociological study of scientists and their knowledge. But the authors themselves were strangely discouraging. They stressed that their own concern was with 'the reality of everyday life' and suggested that sociologists would learn more from focusing on the taken-for-granted knowledge of 'the man in the street' rather than on that of specialists. They even implied that enough attention had already been given to the knowledge of specialists—something I found hard to accept given the difficulty I was then having in the urgent task of finding material on how scientific knowledge was actually generated and sustained. So I'm inclined to identify a combination of immediate pressures and discouragement as what led to my leaving further engagement with the book for another day, although I say this on the basis of dim and distant, and highly untrustworthy, memories, and aware that all kinds of other long-forgotten contingencies would also have been salient.

The serious question of course is why that other day never came—why over a period of years, when I spent many hours perusing far less rewarding material, did I not re-engage with the book? Here the formidable power of the human mind to forget becomes less of a worry; memories may be trusted a little more perhaps, in that they are memories of what has endured over time. And what I recall is that whilst I was impressed by the explicit verbal exposition of the sociology of

knowledge in Berger's and Luckmann's treatise; this was accompanied by a vague unease, a wariness, the basis of which I shall now attempt, not to justify, far from it, but to make to some degree intelligible.

I have mentioned the authors' discouragement of the study of specialisms. Read in context it dampens the force of an inspiring and uncomplicated general vision: "… the sociology of knowledge must concern itself with whatever passes for 'knowledge' in a society regardless of the ultimate validity or invalidity (by whatever criteria) of such 'knowledge'" (Berger and Luckmann 1967: 15). It does indeed detract from the inspiring simplicity and breadth of this vision to be told that there are some places best not to go, and that best of all would be to stick to the study of everyday life, wherein 'what everyone knows' is for the most part addressed unreflectively in reified form (Berger and Luckmann 1967: 107) and reality has a consequent immediacy and solidity. Even so, the general vision was nowhere retracted in the authors' cautionary remarks. And words very close to theirs were to become something close to a slogan in the course of the later development of the sociology of scientific knowledge, not least at Edinburgh.[1]

Verbal identity does not, however, imply full functional and semantic equivalence. In their book, as later at Edinburgh, these words set out a policy of what Berger and Luckmann call 'epistemological bracketing,' but the functions of the 'bracketing' could scarcely have been more different. At Edinburgh, it came to be used as part of a naturalistic and monistic frame, wherein knowledge and its carriers were to be made the focus of empirical sociological curiosity and studied along the same lines whatever the claimed basis for the acceptance of the knowledge might be and whatever its claimed epistemic status might be. In Berger and Luckmann, on the other hand, bracketing was used as part of a dualist frame, to draw a boundary between a sociology that studies whatever people reckon to be knowledge and a philosophy that deals with the epistemological question of whether that 'knowledge' actually is knowledge. Fortunately, however, this neither qualified the fundamentals of the sociological project they set out nor narrowed its scope: pro forma, sociology retained full freedom to roam, as it were. They merely insisted that something extra needed to be done, somewhere else, by someone other. A comparison might perhaps be made with the current position of evolutionary biology in relation to the Roman Catholic Church, wherein it has quite recently been accepted as a legitimate source of knowledge, but only of knowledge still subject to further evaluation and/or interpretation at any point by a spiritual authority located elsewhere.

Fifty years ago, of course, academic boundaries were fought over more fiercely than they are today, and indeed the authors' own account of reification and sacralisation can help in the understanding of what was involved. But there were obvious problems associated with the boundary they proposed here. It doesn't make for peaceful co-existence to accept the right of sociologists to analyse whatever passes for knowledge in philosophy as well as the exclusive right of philosophers to evaluate the 'knowledge' sociologists produce in the course of doing so. Even the authors recognised that erecting a boundary between empirical enquiry and

[1] It is indeed strikingly similar to the postulate of 'symmetry' eventually set out in Bloor's (1976) 'strong programme' for the sociology of scientific knowledge.

epistemological enquiry created difficulties: empirical fields, we were told, constantly generate epistemological problems. They list sociology itself, along with psychology and biology, as one of the major 'epistemological troublemakers' (Berger and Luckmann 1967: 25) among the empirical sciences, all of which can be troublesome to some degree by throwing up epistemological problems that they themselves are wholly incapable of dealing with and only philosophers are equipped to address. The point is not exactly tactfully made: "…the philosopher is driven to differentiate between valid and invalid assertions about the world. This the sociologist cannot possibly do" (Berger and Luckmann 1967: 14). But whilst I found myself irritated by these words, it was not from any imagined insult offered to sociologists: that the capabilities of sociology were unclear and problematic was part of what had drawn me to the field. The irritation arose because the claim extended to all empirical fields. And it was not tactlessness but what I took to be lack of judgement that gave rise to it. (If the words quoted above don't seem at all ill-judged, try the effect of replacing 'sociologist' with 'biologist' or 'chemist'.)

The problem that empirical scientific fields are said to face, and to lack the capacity to deal with, is that of validating their own knowledge. Why do we bother with scientists if they lack this vital capacity? They come expensive after all. Are we to allow them to get by somehow or other, just as 'the man in the street' is said to do, with 'knowledge' that isn't knowledge? Or should we urge them to call in philosophers every month or so, to check out their latest methods and findings? As to the philosophers themselves, if nobody else can differentiate between valid and invalid assertions how is the existence of this rare capability of theirs to be known of more widely? And how is it to be decided where in philosophy the capability resides, given that philosophy has always been a sea of disagreement? Indeed, so difficult did I find it to make any coherent sense of the claim being made here that I was led to wonder whether it might have been made tongue in cheek, or perhaps out of expediency.

It is intriguing how merely to turn over possibilities of this sort in the mind can colour the reading of a text:

> *What is real? How is one to know?* These are among the most ancient questions, not only of philosophical enquiry proper but of human thought as such. The intrusion of the sociologist into this time-honoured intellectual territory is likely… to enrage the philosopher. It is, therefore, important … that we immediately disclaim any pretension … that sociology has an answer to these ancient philosophical preoccupations. (Berger and Luckmann 1967: 13)[2]

What is to be made of these words? Are they a call to limit the scope of a field out of fear—fear of the rage of reason perhaps we should call it? Or might they be an expression of wry humour? Or are they nothing more than part of an argument in

[2] This quote is condensed more than might be thought desirable, but the full quote runs two themes in parallel, and would generate an additional set of queries and gratuitous complexity. Besides, the idea floated in the full quote that the 'man in the street' would be found standing alongside the enraged philosopher with eyebrows raised seems wholly implausible to me, even a long time ago and in another country.

support of 'philosophical enquiry proper,' put together a little too quickly? Presumably, they don't represent an all-out attack on the credentials of empirically oriented fields—the authors were writing a treatise on one such field after all, both of them were practising members of it, and there are many passages in their book where their respect for empirical knowledge whether in their own field or elsewhere is clear and evident. But beyond that what more is there to say?

In fact, there is a little more. For all their general remarks on empirical sciences, it is clear that the active interest of the authors at the time was substantially confined to those empirical fields, including social, cultural, and natural sciences, acknowledged to be relevant to the explanation and understanding of human activity. Fifty years ago, work purporting to apply the methods of the natural sciences to the understanding of human activity—whether in the guise of social science, wherein number fetishism was widespread with speculative theories sitting adjacent but unconnected, or in the guise of a natural science like genetics—was even more likely to be shot through with manifest failings than it is today. And the tendency to wave vaguely at a mythological entity called 'scientific method' instead of attempting to find out what methods and procedures were actually in use in those fields generally recognised as natural sciences was also very much in evidence.[3] So allowing for context, what the authors were saying was, if not entirely correct, then at least understandable; but it was some time before I knew enough of that context to appreciate this.

I should also mention that most of my vague uneasyness about the book was evoked by its earlier pages, where general remarks and philosophical reflections preceded the extended discussion of the sociology of knowledge promised in its title. With hindsight, I suspect that in the first instance I should have followed a suggestion made by the authors themselves (Berger and Luckmann 1967: 7) and moved directly to the sociological discussion in the later parts of the book. I should then have passed over most of their remarks on what today I should refer to as the distribution of epistemic authority, and not encountered the provocative suggestion that this authority *rightly* lay entirely with philosophers. Had they spoken in a descriptive frame here, and simply claimed that ultimate epistemic authority *in fact* lay with philosophers, it is unlikely I would have felt unease, more likely I would simply have disagreed with them. And one of my reasons could have been that the natural sciences were by then widely regarded as the final arbiters on what was valid and what not on a vast range of topics including many pertaining to human activity, to the extent that in the English-speaking world in the 1950s and 1960s several philosophers were developing epistemologies that recognised this and treated natural scientific knowledge as a paradigm of valid knowledge.

[3] It has to be admitted that one or two of the authors themselves were not entirely free from this tendency.

Renewed Acquaintance and Second Thoughts

In any event, I persisted in my interest in the sociological study of science and natural scientific knowledge for some years, drawing on insights and models from a variety of other sources, before eventually being prompted to look back and reflect that Berger and Luckmann might have served well after all as a guide to their empirical study. I guess this must have been close to a decade after my initial reading, at a point when I had at last managed to understand some of the arguments in another impressive work published in 1967, also profoundly influenced by the work of Alfred Schutz. Harold Garfinkel's *Studies in Ethnomethodology* discussed a number of methods of studying everyday knowledge which seemed to be readily applicable without modification to the study of natural scientific knowledge, one important example being 'the documentary method of interpretation,' in discussing which Garfinkel productively combined the work of Karl Mannheim (Garfinkel 1967: 77; Mannheim 1959: 53) with that of Schutz.[4] It was reading Garfinkel that led on to my reading some of Schutz' essays once more, although I shall henceforth discuss the work of these two authors as little as possible, lest it diverts attention from the subject of the present discussion.[5] It is enough to say here that renewed interest in Schutz resulted in my taking another look at part one of Berger's and Luckmann's book, which set out, ex cathedra as it were, a simplified version of Schutz' phenomenology and presented it as the frame that the sociology of knowledge should adopt in its primary task of studying the reality of everyday life (Berger and Luckmann 1967: 33ff).

Whatever my initial response to this prelude had been, it certainly soared in my estimation on this later reading. There were several reasons for this, but one was that I was now able to compare it with the only other philosophical position I had encountered systematically set out for use in the social sciences in this kind of way. Over time, I had become familiar with the individualistic rational choice theory that dominated many of the social sciences in the English-speaking world. In the 1950s, it increasingly focused on the knowledge, or rather the 'information,' on which rational action depended, and encountered a series of intractable problems to do

[4] Garfinkel seems neither to have enjoined avoidance of the study of the natural sciences nor to have offered precautionary deference to philosophers to avoid the rage of reason. But he did employ what might be considered another protective strategy. He provided a list of what ethnomethodological studies are not relevant to, a list so extensive that one might be tempted to infer that ethnomethodological studies only have relevance as phenomena for further ethnomethodological studies and otherwise leave things as they are (Garfinkel 1967: viii; see also 288). But this seems not only to have failed to deflect the rage of reason but to have stirred up the fury of science as well, to the extent that the field was eventually to suffer some slight damage. It has rightly been said that indifference to others can be more of an affront even than vigorous opposition to them.

[5] Tom Burns, Head of Sociology at Edinburgh at this time, had previously hinted to me that I was missing something important in Schutz, and in this, as in much else, he proved to be right. But if I recall rightly it was Garfinkel who converted intention into action, leading to a return to Schutz and particularly his *Collected Papers; Volume Two* (1964). The links between Schutz and Garfinkel are documented in several sources including Psathas (2004), which also provides some background to the reception of Berger's and Luckmann's own book in the USA, although that is not its primary purpose. It seems clear from Psathas' account that their characterisation of the 'man in the street' was a little too close to the sheep in the flock for the taste of many sociologists in the US.

with whence and how rational individuals obtained that 'information' and made use of it.[6] In brief, the 'Schutzian' account was far and away superior to this individualistic alternative, whether as an account of human activity generally or of 'economic action' in particular and the goings-on in the so-called market with which it was predominantly concerned.[7] The flaws of the latter alternative, arising from its individualism—its recognition only of the subjective and the objective with no attention given to the intersubjective—and from its reduction of knowledge to 'information,' didn't arise if the frame recommended by the former was adopted. And the point of descriptions of 'what everyone knows,' set out with a certain relentlessness in the 'Schutzian' account, was easier to grasp, now that I was more aware of people who were failing to take account of them.

Unfortunately, however, in many ways this re-evaluation of Berger and Luckmann had come too late. By then I had found other work, close to theirs in terms of sociological fundamentals, and rich in case studies that research on scientific knowledge could use as models. I was committed to learning from, and borrowing from, fields that initially included social anthropology and micro-sociology, and a little later, the history of science, and deeply involved in exciting work ongoing in my own department. In terms of my own research my previous failure to make the most of their book was certainly a lost opportunity, but it was beyond remedy and I shall never know how large or small a loss it was.[8] There was also, however, another loss, of more relevance here. Memories from 1967 have, I suppose, a certain rarity value, and mine are of the immediate impact of a new book on an individual, but they are scant and record only the superficial engagement of a then barely competent academic. A decade later, my memories are more extensive and I was very much better equipped intellectually by then and more familiar with the oddities of the academic world. But there is already a plentiful supply of relevant

[6] Individualistic rational choice theory was of course irredeemably impoverished and beset with a number of fatal flaws although these flaws have by no means discouraged its use and it continues today unabated, warts and all. But odd as it may seem some theorists in this tradition did make, and continue to make, major contributions by identifying precisely what these flaws were, and why there was no way or eradicating them. For example, it was shown more than fifty years ago that in actual situations 'complete information' was never attainable, that it was impossible to calculate how much of the endless amount of 'information' potentially available it was rational to gather before an action was decided upon, and that even with 'complete information' rational individuals would not act collectively, as they observably did act, and arguably had to act, in all known societies (Simon 1957, 1978; Olson 1965).

[7] The stark contrast between 'the individual' as described by rational choice theorists and 'homo socius' as described by Berger and Luckmann extends well beyond description and encompasses style as well. In comparison with the former, the latter are indeed refreshingly direct in how they describe human beings: "since human beings are frequently stupid, institutional meanings tend to become simplified in the process of transmission" (Berger and Luckmann 1967: 87).

[8] Looking back now the loss seems a large one: I have the feeling that had I single-mindedly followed the three fundamental tenets of their book and avoided lengthy digressions into other forms of sociological theory, individualistic theory in particular, I could have arrived at the positions I have come to hold, whether in sociology of science or social theory, far more quickly and easily. But this feeling is surely the product of hindsight. Knowing the answer before one starts would indeed be helpful were it not impossible. And it needs also to be borne in mind that, at least in the English speaking world, individualism has gone from strength to strength in the social sciences over the last half century, even if for no good reason, and far from saving effort by ignoring it there has been a duty to follow its development and highlight its inadequacies, even in times when nobody wants to hear about them.

memories and materials from the 1970s onward; and by this time it was no longer possible to attribute ideas and insights to the direct impact of the book, whilst its indirect impact, via the culture wherein knowledge of its contents had disseminated, was no longer identifiable independently of other sources of similar insights. Certainly, I can confirm reports of how influential the book had become in the English speaking world and agree that this represented a good for the social sciences and indeed the academy in general, but this is scarcely news and is hardly worth elaborating upon. So given that this is a book that continues to be widely read, I'll move on and mention some things that continued to puzzle me about it, and indeed still do as I look at it today.

Some Queries and Points of Disagreement: On Method and Causal Accounting

When Berger and Luckmann identified the primary task of the sociology of knowledge as the analysis of the reality of everyday life they also identified the method best suited to the (philosophical) task of framing that reality for analysis. It was the method of phenomenology, a 'purely descriptive' method[9]—"'empirical' but not 'scientific'," a method that refrained from 'any causal or genetic hypotheses' and made no ontological claims (Berger and Luckmann 1967: 34). But as no attempt to justify exclusive use of this method was made in what was a purely expository account, it remained unclear why 'pure description' had a special methodological standing, and no less so why speculative theorising was again permitted once the authors moved on from philosophy to sociology. Equally unclear was why, when explicit theorising did duly reappear as the book moved on to sociology, explicitly causal theorising did not. Although I could find no rationalisation of it, and may have been wrong to infer it, the authors' did appear in practice to be averse to the use of 'causal hypotheses' even in the social sciences, including social theory, and indeed to have something of an aversion to causal accounting generally.

Causal hypotheses were not just absent from the explicit methodological repertoire of the authors; causal accounting was not easy to find among the human activities they selected for study, even though it is always very much present in the context of everyday life. The social construction of the institution of causal connection was something they largely passed over, ubiquitous and important though that institution is. For all the richness and variety of the material in their book, there are no references to causation in its index. And although there are several appreciative references to the work of Émile Durkheim, they are almost entirely to his questionable injunction to 'treat social facts as things,' and not to his

[9] This is why the philosophical argument unfolded as descriptions of 'what everyone knows', which actually described what the authors' believed that everyone believed. Unfortunately, the authors were not sufficiently numerous for their beliefs to self-validate here, but they could well have been right in most instances.

late work, seminal to the sociology of knowledge and rich in references to causation and causal discourse as topics of sociological interest.[10]

On the face of it, Berger and Luckmann preferred to avoid involvement with causality at this time, whether as resource or as topic,[11] which seems strange given they were social theorists familiar not only with everyday knowledge but with that of various kinds of intellectuals and specialists as well. 'The man in the street' is liable to invoke causation whenever an excuse for something is required. Philosophy has been rich both in causal hypotheses and analyses of causation from Aristotle and before. And religious professionals have long used causal discourse to do theological work, recognising what might anachronistically be called the 'emancipatory possibilities' of the closely related notions of predestination and the predetermination of human action. Even in the contexts that were of most interest to the authors, a very great deal is overlooked if the institution of causal connection is overlooked. And of course in many of the contexts that interested me at this time a failure to pay heed to causal accounting would have rendered their study impossible.

I need to emphasise that none of this counts as direct criticism of Berger and Luckmann. Nobody is in a position to study everything; and a study may contribute to the stock of knowledge whatever manifestation of human activity it focuses on. But an important qualification is necessary in this case. At the micro-level, a study may be learned from whatever its specific focus; and selection bias, if such we can call it, can be beneficial and is harmless at worst. But care is then needed if what is learned is to contribute to understanding at the macro-level; and since there is not a great deal we can know at the macro level other than via micro-level studies, how the shift of levels is made is of great importance.[12] In particular, if there is an interest in 'macro' entities such as the social distribution of knowledge—and this interest is of course very much present here—selection biases at the micro-level may engender through their careless aggregation 'macro' descriptions with serious inadequacies. It is important, for example, that a lack of interest in the institution of causal connection as a focus of specific studies does not lead to a picture of the social order as a whole that misrepresents the role of that institution therein. And unfortunately there are instances where this seems to have happened. Energetic

[10] To me it was astounding that scholars so familiar with and appreciative of Durkheim would choose to cite *The Rules of Sociological Method* (1895) here, rather than *The Elementary Forms of the Religious Life* (1912). Perhaps they were moved by pedagogic considerations to cite an accessible text, like those sociologists in the English speaking world who long gave saturation coverage to Max Weber's *The Protestant Ethic and the Spirit of Capitalism* (1905) and neglected the more penetrating work included in *Economy and Society* (1922). Less plausibly, perhaps they were ranking method above fecundity, not always a good idea in my view. The *Elementary Forms* is indeed a truly appalling book if evaluated purely in terms of methodology, as also is *The Protestant Ethic*. The latter was the first work of Weber's I read, and such were its methodological failings, magnified I later learned by an unsatisfactory first translation, that it was some time before I read any more Weber and realised what riches were there.

[11] It could be that I am wrong about their general aversion to causal hypotheses. I have omitted discussion of social psychology here, in which the author's had a strong interest despite the causal flavour of important parts of that field, as well as their own discussion of socialisation, wherein possible counter-examples to my suggestion exist. With regard to causality as topic, I think the case for my suggestion is stronger.

[12] Despite the self-denying ordinance issued earlier, it is impossible to forego mention of the invaluable work of ethnomethodologists here.

disregard of the calculative, the causal, the technical, the 'mechanical,' has on occasion generated 'macro' accounts of the everyday 'lifeworld'[13] with a distinctly rustic quality. And selective attention to these same things outwith the 'lifeworld' has facilitated the construction of thoroughly depressing alternative realities. One illustration of where this path may eventually lead may be found in the dualist thought of Jurgen Habermas, who in his widely read and highly influential *The Theory of Communicative Action* (1987) constructed contrasting 'macro' accounts of 'system' and 'lifeworld' in modern societies rather of this sort, and then diagnosed a threat of 'colonisation' posed by the former to the latter—something that only deserved to be taken seriously if the macro picture he had constructed was adequate empirically at that level.[14]

Not even this proviso, however, important though it is, justifies complaints against Berger and Luckmann citing selective attention and what they failed to study. And I would be among the last people to make such complaints, since I believe that attention focused on particular exemplary instances is essential to the acquisition of knowledge. Social anthropologists in Britain used to begin their careers with prolonged, in-place studies of a single tribe. A soaking in the tribal culture inevitably coloured their perception of humans generally and informed their understanding of other tribes, and then by interaction with peers soaked with experience both similar and different from one to another, the knowledge of the field as a whole would be enlarged and enriched. Something similar can also happen even in the realms of macro social theory and philosophy, where the importance of experience deriving from specific situations is easy to underestimate. Max Weber, writing of the emotional links among members, and between members and the collective as a whole, identified war and men fighting together as the context wherein those links attained their maximum intensity. Alfred Schutz, writing of the communicative links between members, spoke of what is involved in the making of music, thereby drawing insight from a specific realm of activity of which he had detailed knowledge. Berger and Luckmann, writing about 'the reality of everyday life' drew upon quite specific aspects of their own remembered lives; and whilst the standing of these memories may be queried, they clearly had a key part to play in the development of their general social theory. As to myself, having gained some considerable experience of the natural sciences in one way or another, I now see them as remarkable exemplars of how human 'sociality' can engender highly

[13] Berger and Luckmann preferred not to make use of the term 'lifeworld' in their book, but they quote Schutz employing the term, or rather the near-untranslatable term 'Lebenswelt,' and of course 'lifeworld' is used routinely by Luckmann later (1973).

[14] The initial reception of Habermas (1987) included criticism on both empirical and theoretical grounds. The former cited the ubiquitous existence of forms of action other than the instrumental and strategic forms within what Habermas identifies as 'system,' or else the ubiquitous existence therein of interactively constituted worker sub-cultures resistant to external 'media-steered' control. The latter argued that entities recognisable as persisting systems simply could not be constituted entirely from instrumental and strategic action and accordingly would never be encountered. Both forms of criticism implied faulty construction of macro entities on Habermas' part (Baxter 1987). Of course, other forms of criticism were advanced at the same time of no relevance here, including rejections of an ethical stance which found it perfectly acceptable for human beings to spend most of their working lives in a miserable condition constituting the media-steered sub-systems of society as Habermas imagined them.

coordinated knowledgeable activity of quite extraordinary potency and finesse. And just as the study of other specific forms of human activity can enrich our understanding of human activity everywhere, no less can the study of these exemplars, or so I believe.

Concluding Remarks

On this last, it will be obvious that I have long disagreed with Berger and Luckmann—believing that the sciences were good to study, as it were, when they believed that they were not and that there were more important things to be doing. But this is a mere tactical disagreement, on something open to revision as society changes. Perhaps they were right about what was good to study 50 years ago, even if what they wrote then is more difficult to justify today. The education system, the occupational structure, and the extent and nature of our technological resources have all changed radically over that time. There are now more female truck drivers and truck drivers with degrees than ever before. There are now more scientific professionals than truck drivers in some societies. Sadly, there are now fewer alpine paths free of the eyesore of ski lifts. And much else has changed that could not have been anticipated on the path to our present day Plutocratic societies.

It goes without saying that other points of disagreement at this level exist. But what is more important to emphasise by way of conclusion is that I have no quarrel with Berger's and Luckmann's general conception of the sociology of knowledge. It should indeed, as they say, study whatever passes for knowledge in a society, regardless of its ultimate validity or invalidity by whatever criteria. It should feel free to study everyday knowledge, scientific knowledge, the knowledge of philosophers and epistemologists, its own knowledge, and any other extant knowledge of whatever sort. My only difference with them at this level is that I am an epistemological relativist and they were not; and a small matter such as this is no impediment to recognising their achievement. Indeed this is a good year in which to be recalling that achievement, since what they have written may be tested and challenged in new ways in the interesting times that lie before us, and serve as a reminder, especially in the English-speaking world, of methods for making sense of them that remain neglected still, in those contexts where, as we tend to say, economic and political power is wielded.[15]

[15] It is of course by no means impossible that ongoing changes will present challenges even to the fundamental claims of the book before very long. The rise of the internet raises the question of what face to face interaction actually consists in. The 'social media' that utilise it are already raising fundamental questions about social relationships. And the electronic trading of what are euphemistically referred to as 'shares' at higher and higher speeds may also encourage reflection on human 'sociality' and whether it can take different forms or exist at qualitatively different levels of intensity. At the same time, migrations of unprecedented magnitude and rapidity, as well as raising questions about how far the reality of everyday life is the 'paramount reality,' may prompt renewed interest in the precise relationship between knowledge and interaction, and ensure that reflection on this fundamental issue is well-informed empirically once it begins.

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