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**Cumulative Social Science and
the Question of Well-formed Research Problems**

Leonidas Tsilipakos¹

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¹ Leonidas Tsilipakos is Lecturer in Sociology at the University of Bristol.

Cumulative Social Science and the Question of Well-formed Research Problems

Leonidas Tsilipakos²

ABSTRACT

In this paper I will discuss some aspects of the issue of whether the social sciences are (and can be) cumulative, with special reference to sociology. The issue is important regarding both the self-conception of the discipline and its relation to public policy. I will argue that the matter depends on the logically prior question whether sociology has succeeded in posing well-formed research problems. I will, in turn, offer some thoughts on a number of arising topics, namely, the accurate description of social life, the aggregability of different (re-)descriptions and findings, the state of fragmentation of sociology into many research schools and the difficulties around concerted work towards a certain direction.

KEYWORDS: accumulation; progress; research problems; description; sociology; social policy

² Leonidas Tsilipakos is Lecturer in Sociology at the University of Bristol.

INTRODUCTION

In this paper³ I propose to say something about the issue of accumulation in social science, namely the idea that sociologists in particular have managed to produce results, findings or theories that build on previous ones and end up in some kind of established corpus, so that later generations need not be going around in circles, constantly reinventing the wheel. This is an extraordinarily complex issue so I can only hope to show the range of its complexity, while including some thoughts too about whether the picture of social science as cumulative is an appropriate one to have in mind.

I have been thinking about the methodology and philosophy of social science for some time now. Yet the topic of this paper is in part sparked by reading about, QUALITY,⁴ a project having to do with taking research results produced by a number of different studies, and *making* them, as it were, cumulate enough to yield an answer to a practical or policy question. One implication of this way of approaching matters is that this policy question may be posed more or less independently from the particular research questions that individual pieces of research are pursuing. To that extent one may dub this an *external* approach to accumulation –perhaps completely external if taken at its extreme, for example, in the case where social scientific work would be considered computationally by being fed in large numbers into machine learning algorithms. An external approach to accumulation would not be concerned specifically to give an account of the conduct or design of research or to directly evaluate the way sociologists pose their problems and produce findings. Instead, it would be geared to using that work to solve a different problem, such as the one we can find laid out in QUALITY: that of replacing reliance on intuition in policy-making by providing an accountable, transparent evidence-based procedure. I will not be concerned with whether evidence-based policy is a good idea or how procedural, and therefore, technical it may become –it might be interesting to note in passing that these themes are highly resonant with sociological thought from Weber, to Habermas or Foucault. I am interested, instead, in an *internal* consideration of the sociological material that may be used in this way, whether by itself it is cumulative, thought to be and, as it were, designed to be so. Still, what I have to say bears a strong relationship to some of the reasons why taking a set of studies and coordinating them in any meaningful way is a particularly difficult task, precisely on account of how studies are produced. In that sense what follows has everything to do with the nature of the beast QUALITY and any such project is up against. And what a beast it is.

There are different questions here and also different kinds of difficulties I will try to bring out. There is, for one, the question of whether the record shows sociology to be cumulative in this way and that is, of course, different from the questions whether it can methodologically, logically, meaningfully, reasonably be cumulative, which itself might be seen to be a matter of evaluating aspirations and ideals against that record or, relatedly, its institutional organisation or, finally, what its subject matter allows for.

A simple way of responding to some of these questions utilises a logical point about the concepts of evidence, findings and results. That the point is conceptual entails that no textual authority is required to back it up. Yet, and without claiming that the point is original with Popper, or wanting to recommend his views, I think that he expressed it eloquently when he said that “you can neither collect observations nor documentary evidence if you do not first have a problem. A ticket collector collects documents, but [s]he rarely collects historical

³ This paper is a version of a talk delivered at CRASSH, University of Cambridge on 23/10/2019. I am grateful to Christopher Clarke, Rosie Worsdale, Jack Wright and other participants for their comments.

⁴ <http://www.crassh.cam.ac.uk/programmes/qualitative-and-quantitative-social-science-unifying-the-logic-of-causal-in>

evidence.” (1979: 186). What holds for a piece of evidence, also holds for a result or a finding and, of course, a solution, which are all such in relation to a research problem. Contradictory findings then require as a matter of logic that we consider them from the same point of view, in relation to the same problem. Thus, it might be said that making evidence, findings, results and the rest cumulate depends on the extent to which social scientists can agree to coordinate their inquiries under particular problems. To the extent that those research problems are well-chosen, well-defined, realistic or resolvable, then there is accumulation and, in this sense, progress. Simple enough, then, one might think. Well, actually, not at all. In fact, sociologists have not managed to follow this formula (and perhaps there are other formulas, although I will argue that the one provided is hardly all that restrictive). Accumulation and progress have been a struggle. I will not try to argue that this is the case; rather, my purpose is to give the reader a sense of the range of difficulties around this simple formula: there is difficulty and disagreement at every turn. What follows comes in five sections, which deal with:

1. The cumulative ideal and its history.
2. Whether ‘problems-talk’ is appropriate.
3. Some consequential features of sociology’s current institutional organisation and whether they allow it to be problem-based.
4. Debates on sociological problems and their relation to social policy.
5. Some observations on the relation of sociology to its subject matter.

1. THE CUMULATIVE IDEAL AND ITS HISTORY

It is not clear what cumulative ideal, if any, is relevant or appropriate for the social sciences. So it makes sense to trawl through some of the options while presenting some potentially useful comparisons with history and science. The idea that the cumulative aspiration has been fulfilled very strongly by a set of scientific disciplines is as old as the complaints against those other disciplines that have been perceived as failing to live up to it.

A schoolboy today knows more geometry than Pythagoras: what do the greatest classical scholars of our time know about ancient Rome that was not known to Cicero's servant girl? What have they added to her story? What, then, is the use of all these learned labours? (1974: 332)

This is written by Isaiah Berlin, but it is Descartes who is speaking, attacking history in the 17th century for failing to match, let alone produce improvements on, once available knowledge. One could very well direct equivalent complaints towards sociology: for example, one could complain about the discoveries it has not made! It is telling in many ways that history was the first to face methodological derision before the social sciences did, which means that it had to stake its claim, to think of itself in relation to the cumulative ideal in ways that were perhaps different to the natural sciences. A case in point are thinkers such as, Giambattista Vico and Johann Gottfried Herder, both of whom Berlin identifies in his writings as not only originating a certain distinctive form of history, so-called ‘cultural history’, but also *thereby* originating, nearly 300 years ago (*La Scienza Nuova* was published in 1725), the divorce, as Berlin calls it, between the humanities and sciences. Berlin portrays Vico’s insistence in taking historical periods in their own terms as tantamount to taking modes of experience or inquiry in their own terms, both of which are thought to diverge from the cumulative ideal.

Vico goes far beyond Bodin and Montaigne and Montesquieu: they (and Voltaire) may have believed in different social *esprits*, but not in successive stages of historical evolution, each phase of which has its own modes of vision, forms of expression, whether one calls them art or science or religion. The idea of the cumulative growth of knowledge, a single corpus governed by single, universal

criteria, so that what one generation of scientists has established, another generation need not repeat, does not fit this pattern at all. This marks the great break between the notion of **positive knowledge** and that of **understanding**. (1974: 351-2; my emphasis)

This quotation is quite complicated. One should, first of all, say that it is juxtaposed to Voltaire's insistence to look at past periods through the lens of allegedly universal criteria, namely, those of science, and to evaluate those periods in terms of their contribution to contemporary knowledge. Vico's idea then is that the history of different periods or past societies needs to be governed not by the tenets of enlightenment science but by appropriate cultural forms which allow one to judge appropriately past activities. But the further implication seems to be that precisely because earlier periods are not related to later ones as early attempts at a specific scientific topic to more developed ones, say, what Pythagoras knew and what contemporary mathematicians know about geometry, a different, non-scientific sensibility is required and it is at that point that *historical understanding* and *science* part ways. Naturally, arguing the case in this way bears the potentially unwelcome implication that this is true for all cases except for the case of science, whose history is thought to be unproblematically available and whose historiography is therefore no different to science itself. We are nowadays much more sensitive to the problems with this claim and quick to separate science from its perceived and actual history. For one, we might remember here Kuhn's observation that it is difficult to teach scientists to approach past science in its own terms because they "know the right answers" (SSR: 165n). Kuhn, of course, problematized the account of science as making progress in the way usually advertised. Whatever account of its history and of its historiography we might adopt, however, it remains the case that science is – no pun intended– paradigmatically progressive and cumulative: it moves ahead, it adds to what is known and, thus, it grows. History, on the other hand, cannot grow in this way because, under the reasoning presented above, its object, as it were, does not develop in such a uniform fashion, but is rather heterogeneous and discontinuous.

Vico can be seen, without too much violence, as offering something not too dissimilar to the kind of *a priori* methodological argument offered by philosopher of history Louis Mink in 1968, when in a great review essay he distinguished history from science by reference to the descriptive forms each employs:

...a scientific account of an event determines a standard description of the event, by counting, say, statements of the mass and velocity of a moving body as relevant descriptions, and statements about its color or the states of mind of people watching it as irrelevant. History, on the other hand, reports how descriptions change over time [...] Thus there can be a history of science, that is, of the changes in the kinds of description accepted as standard at different times, but no science of history, that is, a complete description of events which includes or subsumes all possible descriptions. (1968: 690-1)

Mink later augmented the claim that what historians seek to record is a changing stock of incompatible descriptions with the further idea that historians produce their own stock of incompatible descriptions in constructing different narratives. Fast forward to the present and a recently published book by philosopher of history and social science Paul Roth (2020), who takes Mink's position as central. Roth argues that historians produce narratives which, considered together, are non-cumulative, or 'non-aggregative' as he calls them. The rationale for this depends on the logical point about the problem-relativity of evidence and findings as argued above. But Roth substitutes a particular form of description/ narrative under which is subordinated what I have spoken as 'the research problem'. The argument is that each historical

explanandum is stated via a description which is specific to and defined in terms of that particular narrative. Not only is there no standardization across different narratives, but there cannot be any. It is suggested that history could only be cumulative if such *explananda* were separable from the narratives they are embedded in and insertable into other narratives or, what amounts to more or less the same, if historians could come up with a *uniform manner of description*. Given how narratives are constructed and how history tracks changes in descriptions this would not make sense. Perhaps, however, one might argue that it makes much more sense for sociology, which, under a certain conception we will encounter in SECTION 4, is interested in offering such forms in its theories, generalizations, typologies etc. Let it suffice at this stage to note the proximity to those of accumulation of questions as to description, standardization, and sociology's relation to the stock of descriptions in the culture.

Having posed some of the issues in relation to history we might raise the question of the social sciences and sociology. It seems that by asking whether sociology can be cumulative, one is asking all over again a question that has been asked *ad nauseam*: namely, whether it can be a science. Besides the tedium, which is to be expected, there is a surprising measure of irony in the fact that not only is sociology not seen as making scientific progress, but that complaints against this fact, as exhibited in reactions to the embarrassing relation it retains to 'the classics', have not made progress either.

Robert Merton: the attractive but fatal confusion of current sociological theory with the history of sociological ideas ignores their decisively different functions (1968: 2)

Jeffrey Alexander: To answer the questions about the relation between social science and the classics, then, one must think about just exactly what empirical social science is and how it relates to the science of nature. One must also think about what it means to analyze the classics and about the relation this kind of presumptively historical activity might have to the pursuit of contemporary scientific knowledge (1987: 11)

Monica Krause: There have been challenges to the theoretical canon in terms of who exactly should be part of it ... there has been much less sustained debate about the implications of having a canon of people (and texts) in a discipline supposedly focused on objects of another kind (2016: 26)

Perhaps the conception of schoolchildren knowing more than past geniuses (also noted in Merton, 1968) exhausts neither being cumulative nor being a science. Kuhn notes how painting (of all things) was once taken to be the cumulative mode of inquiry *par excellence* in the sense that it could be seen to make progress through various techniques leading to a more and more detailed and accurate representation of reality, perhaps until that aim was displaced by photography and eventually gave way to yet others. And he adds:

If we doubt, as many do, that non-scientific fields make progress, **that cannot be because individual schools make none. Rather, it must be because there are always competing schools**, each of which constantly questions the very foundations of the others. The man who argues that philosophy, for example, has made no progress, emphasizes that there are still Aristotelians, not that Aristotelianism has failed to progress. (SSR: 162-3; my emphasis)

The description of many competing schools is appropriate to the state of sociology too. In fact, I think undoubtedly so. The upshot is that one should look within, for example, analytical sociology, critical realism, Bourdieusian sociology, Foucauldian studies, various feminist sociologies, the sociology of race, etc for progress. It seems then that if we remove the

comparison with science, and the way in which science is cumulative, it might be easier to perceive the different, perhaps local, forms of accumulation and progress. This is certainly a good idea, one I will come back to, although I am not sure that it guarantees closer coordination under the research-problem / finding relationship. It raises many further questions regarding what kind of progress is thought to have been made even within particular schools of practice. For example, is progress thought of in terms of the improvement of technique, the expansion of an agreed upon knowledge corpus or the degree of refinement of the key theoretical ideas so that they replace their initial formulation? Difficult questions remain.

I will gradually move to connect some of these concerns to observations on the various practices that make up academic sociology. First, though, I would like to reflect a little on the appropriateness of ‘problems-talk’.

2. IS ‘PROBLEMS-TALK’ APPROPRIATE?

Is speaking of problems really appropriate in describing the business of sociology? Whence this fascination with problems, one might ask, and how much traction does it really afford us? Does it perhaps skew our perception towards disciplines in which problem-solving is the everyday order of business? Now, I see no reason why speaking of problems commits us to thinking of what goes on as Kuhnian problem solving in so-called normal science, that is, in the sense of puzzles that get solved under the auspices of a paradigm. There is sufficient latitude afforded by the term ‘problem’ in the way we ordinarily speak and we can, of course, also alternate between ‘the problem’, ‘the task’ we set ourselves, ‘the questions’ we attempt to answer’ and even sometimes ‘the animating worry’ (‘the problem’) without doing violence to what sociology is about.

There are some who wish to restrict the use of ‘problems-talk’. Robert Nisbet’s book *The Sociological Tradition*, dealing with the origins of the discipline in counter-enlightenment/conservative thought exemplifies the view that sees problem-solving as something pertaining to the sciences, while issuing the warning that sociologists never ceased to operate as artists and, therefore, beyond the confines of “problem defining, problem solving thought” (1993: 19). Nisbet continues this line of thinking with the claim that classical sociologists like Weber or Durkheim did not derive their ideas through problem-solving or ‘logico-empirical analysis’ and continues by noting that “each was, with deep intuition, with profound imaginative grasp, reacting to the world around him, even as does the artist, and, also like the artist, objectifying internal and only partly conscious, states of mind” (idem: 19). Nisbet is perhaps confusing contexts of discovery and justification, in that the genetic account of how classical sociological ideas were formed does not need to work under the logic of problem-posing and is in part unnecessary in the specification of a problem as long as the latter can be arrived at or demanded retrospectively. But, more importantly, he underplays the ways in which what these thinkers were doing can receive such a description and the fact that one can hold this view while also accepting that the ways those thinkers worked did not resemble normal science.

It is worth juxtaposing Nisbet's remarks to Ernst Gombrich's practice in his international bestseller on the history of art, where he makes extensive use of ways of describing artistic tasks as responding to problems that artists were endeavouring to solve. One instance is 15th century Florentine Antonio Pollaiuolo who is described as someone who "tried to solve this new problem of making a picture both accurate in draughtsmanship and harmonious in composition. It is one of the first attempts of its kind to solve this question, not by tact and instinct alone, but by the application of definite rules" (1998: 262). One outcome of these attempts is the depicted⁵ *The Martyrdom of Saint Sebastian*, which can be found at The National Gallery, London. In turn, Pollaiuolo's efforts, we might add, produced a new problem for later artists to tackle: how to come up with a harmonious and symmetrical synthesis but without making it look artificial. To add one more example, Gombrich portrays Cezanne's problem as being "to achieve a sense of depth without sacrificing the brightness of colours ... [but] one thing he was prepared to sacrifice if need be: the conventional 'correctness' of outline" (1998: 544). One could object here that Gombrich's use of 'artistic problems talk' is a presentational device useful to the history of ideas



so as to draw attention to discontinuities between various historical contexts rather than an account of the explicit goals artists set themselves and pursued, while also arguing that the case of 'definite rules' is rare in the history of art. There is certainly some truth in this, yet the point of talking about problems allows us to get a handle on what those artists could intelligibly be seen to be doing and, I think, even saw themselves as doing. It is a question which rather than force any falsification of artistic endeavour adds not only to our understanding but to the possibilities for justifying chosen ways of proceeding. On the condition, that is, that one does not exaggerate the singular understanding of the problem and is careful not to obscure the degree of individuality modern art has come to express, as centred on the figure of the artist while moving away from past forms of collective organisation through academies or standardized religious vocabularies or commissions by noble and wealthy patrons. One would have to discuss features of the contemporary art world here.

On the other hand, sociological work is mostly conducted as part of academic institutions. There is an explicitness of purpose, of task and set objectives, features which may be absent from art. It is also collectively orientated, arguably featuring a rather different relationship between the discipline and the individual scholar, though a lot of what goes on in the teaching of sociology, from seminar discussions to the writing of dissertations, would lead one to believe differently, centred around the individual student and their interests as it is.

In sum, we can employ the idea of problems and even notions as to the well-defined nature of such problems in a simple sense, having to do with the clarity, appropriateness, self-understanding, coherence and consistency of the aims, objectives, tasks, even questions (as we will see below) under which one is labouring. It is at this basic level that sociology is struggling, and it is possible to see that this is so without entering into debate about the range of strict application of 'problem' and 'problem-solving'.

⁵ Photographed by the author.

3. SOME OBSERVATIONS ON SOCIOLOGICAL PRACTICES

I now turn to offer some observations on sociological practices. Writing textbooks, teaching classes, guiding students in research, writing monographs and research articles, among others, are all aspects of the discipline at work, though I will only cover some of them.

In a small-scale pilot study entitled *Sociological Islands* for the journal *Ethnographic Studies* I looked at one issue from the UK sociology journal with the highest impact factor. I focused on connection-making practices in the reporting of research, and, perhaps unsurprisingly, found them particularly insular in the sense that ties and contrasts to other pieces of work or to public debates, were tokenistic rather than well-thought through. In other words, they were utilized as a convenient springboard rather than as a solid basis for construction. Counter arguments frequently suffered from an “absence of well-defined connections to a controversial issue and to logically appropriate support” (2017: 71) and ended up also being tokenistic.

All this is rather consequential for the question of accumulation. Mere likeness, loose resemblance, (dis)analogy are not relations that allow one study to add itself on top of the other nor to replace or correct another, or to point in a sharp way towards a finding. Nor are such connections sufficient to settle issues, if by accumulation we are to mean a corpus of accepted findings and ideas. Of course, this pilot study, even if of one of the best UK journals, is no secure basis for generalization, even more so after only having looked at one issue. Besides, it will be said that this is not indicative of anything other than operative constraints regarding what it takes to publish a journal article and that all this is something editors, authors and audiences understand and budget for. Accordingly, there are a series of other contexts where sociologists actually take the time to furnish those connections. Perhaps. Some further realities of sociological practice may suggest otherwise.

If we take accumulation as a matter of the relationship between studies mediated by a set of theoretical ideas, which function as a way of focusing inquiry, checking against previous findings, augmenting the corpus, etc. then it is important to consider the role of sociological theory. The issue of theory is a perennially problematic one, as previous remarks on the relationship to the classics may have indicated. I offered some of the following thoughts to the BSA theory study group in September 2019.

One might as well begin with the most direct question one can ask: Does sociological work really depend on theory? Suppose for the sake of argument that it ought to, in other words that there are no, let us say, meta-theoretical reasons for preferring an *a*theoretical sociology. One would have to observe, nevertheless, that most sociologists do not find those reasons, whatever they are, particularly convincing and behave as if they do not need or care for theory very much –in pretty much any of the senses of ‘theory’ (Abend, 2008) we might distinguish here. Naturally, this is not the case when we are trying to publish a paper in a journal and are in the process of making sure that it is ‘theoretically informed’ which in some cases can be reduced to appending some kind of theoretical terms to an empirical paper that could very well stand intellectually without the appendage. Nor, for that matter, when we are writing research proposals where again connections to theory might bestow on the proposal a disciplinary stamp necessary to obtain research funding. Yet, in terms of day to day activities I think it is true to say that most sociologists do not see theory as highly relevant to their work. Sociologists certainly do not spend their time theorizing or studying theoretical texts, their knowledge of theory is many times limited to summary versions of positions and, accordingly, they would much rather teach their specialist units on particular topics than classical or contemporary theory, and overall, would prefer to leave theory to the few of their colleagues that they

consider, perhaps with an element of bafflement or perhaps jealousy or probably derision, as theorists.

The distance between what empirical sociologists and theorists do is a reality which perpetuates superficial relationships that benefit no-one. They do not benefit empirical researchers because an institutionally or officially mandated doff of the hat to theory does not, practically speaking, enable or facilitate what they do, nor do they benefit theorists who may find it more difficult than it already is to relate their concerns to empirical work. Finally, and most gravely, this casts doubt on the aforementioned meta-theoretical reasons for keeping sociology theoretical: if researchers do not feel they need theory much, if at all, to address their concerns in ways that satisfy them then who, and on what authority, is to say any different? To my mind, someone making the opposite argument would need to show, *contra* converse problems of underdetermination of theory choice by data, the necessity of a theoretical concept to a particular argument and to the description of a particular concrete study. One could, of course, claim that some theory is necessary, not that *a specific* theory is necessary –in that case the question is whether it can be acknowledged without such resources.

The schism between theorists and empirical researchers implies not only that no agreement is forthcoming as to what constitutes progress, which would enable theoretical advances to be designated as such, but also that it will remain difficult to see, logically speaking, what impact these advances would have on the empirical aspects of the discipline and how those might actually serve as checks against any claims to progress that may be false. Furthermore, the rejection of theory by practitioners, given the coordinating function a theory may serve, leads to a lack of accumulation.

The extent of the ‘dysfunctional’ character of the sociological division of labour goes beyond the distinction into theorists and empirical sociologists if we consider what has been happening in sociology in the past decades in terms both of the variety of institutional contexts sociologists find themselves in, for instance in Management or Business schools, in Criminology or Social Policy departments (Benzecry et al., 2017) and the variety of research agendas they pursue, both theoretical and empirical. One might simply look at the line-up of papers in any sociology conference. There is not only significant variety, which is a good thing, but, one might say, returning to Kuhn’s remark on different schools, heterogeneity and fragmentation, both of which can be problematic: In other words, there are numerous areas and modes of inquiry that draw on different traditions, sets of books or methods, which do not speak to each other and are not coordinated in any meaningful way. They are largely incompatible and driven by a host of rather different concerns, among which one might mention, besides the traditional sociological approaches, varieties of postcolonialist, feminist, poststructuralist and other traditions.

In relation to this state of affairs American sociologist Irvin Louis Horowitz, characteristically lamented, speaking of ‘The decomposition of sociology’ (1992) in the U.S., that sociology has served as a “repository of discontent, a gathering of individuals who have special agendas”, the implication being that various activist, politically partisan, interests have found a home, perhaps instrumentally so and therefore have taken sociology further away from its disciplinary commitments and academic modes of inquiry. One does not need to subscribe to Horowitz’s aversion to such agendas. I, for one, do not have a quarrel with the politics being advocated. Neither do I have a problem with doing politics per se as long as those who do politics operate under a clear conception that this is what they are doing and accept all that comes with the territory. It could very well be said that this is precisely what ended up enriching sociology, making it more relevant and thereby breathing new life into it. Perhaps this is a popular view. But it is a view that is, in truth, inconsistent with continuing to think, teach and

research sociology in the way it has been taught and researched and, I think, it is deeply incompatible not only with many academic values but with the ideas those who cling to such views also espouse, namely that they are doing something which policy makers and the public ought to respect and pay attention to given that it is produced from the academy, and which generations of undergraduates ought to subscribe to.

Once again, we can see that heterogeneity of this kind implies very strongly a lack of common criteria that would enable accumulation. Now, as per previous remarks, addressing the problem in a way that respects the multiplicity of ways of theorizing and inquiring into society might imply conceiving of the question of accumulation as split into many sub-questions, each attaching to a particularly well-defined area of theory and research that can provide for its own appropriate criteria. Given the only local coherence of those criteria, it might not be possible to integrate the different questions attaching to specific fields in any straightforward way but at least one would be able to judge the sub-questions based on narrow criteria of what is an improvement and what is not. Indeed, this approach would go a long way towards addressing the issue; but it would not go all the way.

The problems with and left unresolved by this way of thinking are many and I think not unimportant. One central problem, perhaps apparent only when we stick with this tack long enough, is that it assumes the adequacy of local criteria and that the latter can alone decide the question of improvement when it is patent that in some cases at least what needs to be established is that one is not about to re-invent the wheel. In other words, one of the additional reasons fragmentation is bad is that it precludes knowledge of how other sub-fields have dealt with potentially similar questions: where they have gone into dead-ends and what lessons they may have learnt in the process. This ends up in a kind of parochialism that cannot be broken out of through the insistence on locally limited criteria.

The dangers in parochialism (and the absence of an agreed upon stock-taking and assessment mechanism) imply that there needs to be, besides what might be locally agreed upon, some kind of external check, external not only in relation to the sub-field in question but to the discipline as well. For example, before commencing on an investigation, there needs to be some measure of what understanding is already available. Otherwise, one is bound to surrender to the convenient yet erroneous presumption that many authors make when they approach a particular topic from a sociological perspective, namely, that there is no available understanding at all, and that, therefore, it is up to sociologists to understand it *de novo*. Equally, there needs to be some external check in terms of what is thought to be puzzling or lie beyond comprehension and thus to require explanation given our knowledge of society. *Whose* comprehension we have in mind is also important given that many times we speak to people who are practical experts in their area of activity.

It is exactly considerations of this sort that I want to pursue a little in the next issue I wish to discuss, namely the formulation of sociological problems, eventually returning to the issue Mink and Roth raised about description and its (lack of) standardization.

4. DEBATES ON ‘SOCIOLOGICAL PROBLEMS’

The late Michael Banton, a historic figure in British sociology, Professor of Sociology in 1965 at the University of Bristol and first editor of the journal *Sociology*. In his recently published (2016) reflection on sociology and social policy he aspired to separate what he thought *would be* the technical, culture-free concepts of sociology from the ordinary language of social policy addressed to the public. He distinguished the ease of finding problems in social policy given that “the mass media highlight such problems every day. Necessarily, they describe them in ordinary language.” (idem: 995) On the other hand, sociology, he thought, is concerned with

technical, “uni-vocal’ language suited to the accumulation of knowledge which necessitates a ‘restriction of vision’” (ibid.: 999), and presumably is not able to name its problems with such ease since they are not ready-made. Banton recommended a Popperian critical rationalist approach, according to which explanations vie with each other for survival. In his view,

...knowledge grows more rapidly if the sociologist first identifies a puzzling observation and then asks the best explanation. This entails a differentiation of the *explanandum*, the thing to be explained, and the *explanans*, that which explains the *explanandum*. To start with a discussion of the concept is to stay within ordinary language and its *explanans*, instead of starting from the *explanandum* and assessing the value of alternative explanations. (idem: 999)

What Banton has presumably in mind in recommending the break between ordinary and technical concepts is that somehow ordinary ways of discussing social life are conceptually self-explanatory, they provide their own common sense *explanans* or perhaps fail to strike us as puzzling, whereas formulating an *explanandum* in technical language, allows us to begin by identifying something striking and then to further assess various equally technical explanations. The point about formulating an *explanandum* in technical terms for Banton is that it constitutes a restricting of meaning and of vision. The idea seems to be that this is desirable in the face of uncontrollable open-significations of many ordinary concepts that fail to pick out a well-defined phenomenon.

Banton’s contribution is not only typical of long-standing explanatory aspirations but also instantiates, perhaps in a convoluted way, the various issues at stake: what is a sociological problem and how is it different to a social problem (or a policy problem)? are sociological problems selected out of a list of pre-existing social problems or specifically constructed by being technically formulated? how narrowly should terms be defined to pick out phenomena? what kind of restrictions ought we impose on ourselves; in what ways can we re-describe ordinary concepts?

It is instructive to propel the discussion forward by juxtaposing Banton to BU sociologist Ashley Mears (2017) and her contribution to a recent symposium published in *Sociological Theory*. In ‘Puzzling in Sociology: On Doing and Undoing Theoretical Puzzles’ Mears is not focused on sociological problems in the sense in which those are definitive of the discipline, the long-standing sociological problems we might say. For one, it would potentially be too complex to really identify what those are and, besides, she is perhaps not convinced as to their necessity. Rather, she focuses on how lower level postulations feature in published research – Banton’s technical problems, we might say. Her argument is that theoretical puzzles in research reporting are contrivances which mainly function in order to produce surprise, make a case as to the relevance of an investigation, create a sense of importance and, ultimately, act as a shibboleth of disciplinary membership.

She has a number of further complaints. The first one I judge as the more minor one, namely, that puzzle-driven research reporting does not reflect the chronology of the research process. Mears complains that putting the puzzle up front when in actual fact it antedates the data paints a false picture of research as ‘deductive’. Her second and, to my mind, more important complaint is that unlike what she takes to be the case in normal science, in sociology the puzzles actually need to be constructed. We need not take her to be saying that biologists find their concepts and problems ready-made but rather that the nature of puzzlement about something pertaining to life antedates biology’s rendering of that something tractable. True, puzzling over how to interpret infinities in QED equations does not predate the machinery of QED, but the puzzlement as to how the world works at that scale does indeed and it is precisely to that kind of puzzlement that many lower-level problems in science can be connected. In

contrast, she sees sociology as engaged in artificial production of puzzlement for puzzlement's sake and in order to fulfil the above-mentioned functions.

Even further, Mears thinks that the puzzle-posing is external to dealing with data and works against it. She notes that being faithful to empirics and solving a theoretical puzzle are incompatible with each other and when scholars try to do both, both suffer. Mears wonders as to the real need to set up a 'jigsaw' and as to whether we might not stick with 'thick description' and compelling stories, inductive presentation and let 'the empirics' demonstrate the 'so what'. What is of concern to us is that, by implication then, Mears seems to be denying that there can be much sense given to specialised sociological problems under which cumulative advances might take place. Rather, the point or the 'so what' of an investigation is best given by the criteria that anyone concerned about the world they live in might bring to bear so as to find a story compelling. Once a story is seen as compelling, then, the burden of answering the 'so what' has been discharged.

This looks like a conception of sociology that sees its ends as subordinate to the pre-existing interests we have in the social world, very diverse though these might be (not only cognitive, hermeneutic, emancipatory?) but idle curiosity, lack of alternatives, practical engagement, economic gain. The extent of success of sociology would then be in telling compelling stories that answer to these interests, though how exactly they 'answer' and what 'being compelling' would come down to would be very varied: illuminating, just-so, good for passing the time, effective, persuasive, highly priced stories are evidently stories of rather different kinds! Lack of detail does not help this conception which not only specifies itself in divergent ways, being too capacious to tightly fit the bulk of sociological activity, but also does away, to a certain extent, with sociological autonomy in terms of the questions the discipline might set itself.

Yet, I submit, there is something sound in Mears's reaction to problem-posing practices: puzzlement needs to be checked against a common measure of what is known or understood and there needs to be a clear sense of the point of engaging in investigations (if not initially then eventually). That this is a sound response can be seen in the fact that even scholars who take a less extreme position than she does, see problem-posing strategies as problematic and signal towards external checks.

In *Theory and Progress in Social Science* (1997) James Rule retains the ideal of sociology as a cumulative knowledge discipline, yet he argues for its answerability to a pre-existing or public sense of what is important and worthy of study in social life. Specifically, although he acknowledges the plurality of forms within social science, he wants to argue for its invariable accountability—as a condition of its making progress—to what he calls *first-order questions*: "Questions arising from endemic tensions in social life ... the kinds of questions that draw people to study social life in the first place, and that are constantly raised anew in the minds of nonspecialists seeking reasoned bases for action in the face of endemic social tensions" (1997: 45-6). Rule's conception is wide in terms of who such actors are and does not distinguish, for example, between the different forms of understanding that might be appropriate to social scientists, the government, lay folk or people whose job is to deal with specialised activities. Nor does he specify any mechanism for checking against misconceived questions, flights of fancy, hyperbolic ambitions, etc. Rule's purpose is to demote what he calls 'second-order questions' which are "scarcely direct outgrowths of social experience. Rather, their interest arises strictly out of analytical structures created by social scientists." (idem: 46) For Rule, social science is to furnish the understanding that helps actors *cope* with the social world rather than fulfil some kind of function *expressive of* experience with mainly aesthetic features which would render social science closer to artistic forms. That would only enable

sociological theories to make what he calls, ‘formal progress’, whereas what is sought after, he thinks, is ‘substantive progress’. To achieve the latter the most stringent test that a theory has to answer is, thus, to outsiders, who upon acknowledging that it matters to them, that they cannot afford not to take the knowledge offered into account, accord it the highest praise it could receive.

The most compelling ends served by theoretical social science, I maintain, are not those defined only by our theories themselves. Instead, they are ends that we social scientists hold in common with the broad and diffuse public of thoughtful participants in social life. (1997: 238)

Like Mears’ conception of ‘compelling’, Rule’s notion of ‘coping’ is equally diffuse. Most importantly, it is unsuited to be of service to the vision of cumulative inquiry he subscribes to, being unsuited to many parts of mathematics and pure science. Moreover, ‘coping’ can also be a matter of various skills or forms of understanding that are taught by the arts or philosophy – seen as non-progressive forms.

Yet, instructively, and despite being willing to subordinate sociology’s problem posing to external constraints, Rule is careful to argue against sociological activity taking ‘social problems’ as its problems. He is particularly insightful in showing that the logic of problem-solving appropriate to inquiry cannot be applied to the case of ‘social problems’. One reason, and one can think of many more, is that:

Poverty, racial tension, environmental disarray, unemployment all are, strictly speaking, conflicts rather than problems in the sense of conditions equally deplored by all. Such conditions would not persist unless their continuation were gratifying to some parties or interests. And often those who benefit from those conditions are prepared to go to great lengths to perpetuate them. Thus, measures that constitute ‘solutions’ from the standpoint of one group or interest may represent ‘problems’ for others, and vice versa ... (1997: 239)

5. CONCLUDING THOUGHTS

This takes us back full circle to the question of sociology and social policy that Banton (and QUALITY) raised, and to the chosen forms of description and their cumulative potential. I have taken us through some of the heterogeneous aspects of these issues in order to appreciate the architecture of the difficulties, as it were. It is of such a kind that precludes coming down hard on the matter without thereby excluding a whole host of considerations. Nevertheless, I have left it to the very end to say something that ties together all these aspects and I will attempt to do so. The observations I have to make are not terribly sophisticated, nor particularly original. But their implications are profound. They are not usually appreciated, being swept away by programmatics, various other kinds of aspirations and the will to professional respectability. These explain Banton’s motivation for a ‘uni-vocal technical language’ used in ‘formulating *explananda*’ which is designed to emulate the, allegedly at least, strictly-defined concepts of science where, as Toulmin (1958) nicely observes, at least ‘grammatical subjects’ on which predicates are ascribed are typically abstruse. One need only think of dark matter, anisotropic materials and macromolecules. These are concepts which are, to use Mink and Roth’s language, standardized by scientific theories and for which no alternatives exist in the culture.

But this idea paints a poor picture of the social sciences which seek to give accounts of precisely a culture and, thus, are in a second-order position to that culture and its forms of description that can be used to identify social phenomena. Sociologists, to take some key topics, set out to understand populist politics, racism, neoliberalism, social movements, popular music,

the modern self, the body, science, religion, climate change, among many others. All these are phenomena that depend on the changing cultural practices and the stocks of description that constitute them. At the same time it pays to remember that sociology is not the only game in town (nor is it the only second-order game in town; think of journalism, literature, documentary film). That is to say, it is not the only source of understanding. In fact, it depends on cultural membership and the understanding that affords. It follows, as I have argued elsewhere (Tsilipakos, 2016), that it is a mistake to aspire to set up a restricted set of conceptual resources when in fact, removing such restrictions allows us to speak intelligibly and intelligently about social life by drawing on the stock of concepts at our disposal and the typically fine distinctions they enable, and we can follow and focus in on such distinctions as they are contextually made. At this point there is a nice parallel with those early thinkers mentioned in the beginning, Vico and Herder, who in their own, no doubt confused and potentially dangerous ways, nevertheless struck upon a truth: that it is a perverse form of self-denial and self-laceration (1974: 343, 165n), as Berlin nicely puts it, to pretend that we have no useful grip on social life unless a social science is to provide us with it.

What then of accumulation and well-formed problems? I am not sure what it would take to agree on what is a sociological problem and how it is related to social problems, which kind of accumulation is appropriate to aspire to, or whether it is possible to organise research and teaching practices more tightly. As I have suggested, an answer to those questions has to respect these two ideas: That sociology is one among many activities that aim at social understanding and that, being second order, it depends on our existing cultural and linguistic skills. Are these ideas sufficient, will they lead to accumulation? Maybe not. But they are necessary for a logically transparent, measured, and perhaps illuminating sociology.

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