2. *The sociology of knowledge is a highly diversified field that encompasses a wide range of approaches. What shared intellectual efforts or questions hold the field together? Would you argue that the sociology of knowledge constitutes a cohesive field or are individual factions isolated from one another?*

The sociology of knowledge struggles with the fact that its central focus – *knowledge* – is necessarily implicated in every field of study in the human sciences and, perhaps even more importantly, constitutes the purpose and substance of *study* as such. This means that there are claims pertinent to it being made in every field, grounded in the literature and traditions of that field and not necessarily with any great familiarity with the literatures and traditions of the sociology of knowledge more narrowly construed. It also means that all *reflective* claims (what am I doing, how, why, and how, and in what relations, do I assess my own claims?) are pertinent to it, albeit they may not be well grounded in any literature, but only in commonplaces and pragmatic considerations. With such a well-nigh all-encompassing mandate, it is hardly surprising that the sociology of knowledge is, at best, only very loosely cohesive, despite having a clearly demarcated, historically consistent, and unquestionably important core. It is, in a relevant sense, simply too *big* for itself (which is a sociologically significant claim in itself).

In the following pages, I will first give some attention to the sociology of knowledge’s common core, to make clear that there is one and thereby to demarcate what might be indicated by a sociology of knowledge “narrowly construed”. I will then consider the (lack of) cohesion of the field or fields so indicated, briefly reviewing some of the principal components thereof. Specifying the exact degree of cohesion or isolation between these components would be an entirely feasible, and very interesting, bibliometric project, but goes beyond what is possible for present purposes, though the evidence in Moody and Light (2006) is suggestive. Instead, I will look at evidence on the institutionalization of disciplinary fields and higher-level evidence of continuity between literatures.

**The Common Core: The Social Foundations of Knowledge**

Saying that the common core of the sociology of knowledge is *knowledge* is neither difficult nor terribly informative, as it says so right on the package. So, what *about* knowledge is the sociology of knowledge interested in? Is there a *kind* of knowledge that the sociology of knowledge concerns itself with? Or, put another way, when the sociology of knowledge speaks of “knowledge” what exactly does it have in mind, what does it include or exclude? As will become apparent, it is precisely because “knowledge” in the relevant sense includes phenomena that are commonly discussed in a multiplicity of other terms that it becomes very easy for it to lose cohesion – and hard for it not to.

That said, the particular interest of the sociology of knowledge is in what has traditionally been called the “social foundations” of knowledge. It was already explicit in Comte’s “positive philosophy” ([1839–1853] 2009) – wherein a discipline of “sociology” was first proposed – that different social orders, different regimes, were associated with different kinds of knowledge and that, as such orders succeeded one another in history, so too did knowledges. Comte saw this as evidence for the “Natural Progress of Human Society” and this interpretative strand remains important, particularly as refracted through Durkheim’s work ([1903] 1963; [1912] 1995). But at least as important was the similarly based, but less unambiguously “positive”, vision found in the writings of Marx and Engels to the effect that, “The ideas of the ruling class are in every epoch the ruling ideas” ([1845–1846] 1976:59). Their notion of “ideology” inspired both elaboration and refutation. But, however evaluated, it is what Comte and Marx have in common that gives rise to a sociology of knowledge: the claim that different social orders (and different positions within those orders) are accompanied by different knowledges.

In this connection, I consider the caveat made by Berger and Luckmann in the introduction to their *The Social Construction of Reality*, that the sociology of knowledge concerns itself with “whatever passes for ‘knowledge’ in a society” not with its “ultimate validity or invalidity” (1966:15), to be important. Philosophy has long drawn a distinction between ἐπιστήμη and δόξα, “knowledge” and “opinion”, where “knowledge” was to be reserved only for (as Berger and Luckmann put it) the “ultimately valid”. The sociology of knowledge sets this aside. “Knowledge” is what people understand themselves to “know”. In terms of the classic distinction, it is *doxa*. That different people, on various bases, claim to “know” different things (especially about what they otherwise take to be the same thing) is what makes studying “knowledge” interesting, useful, and important. Whether one or more of those claims should prove to be “ultimately valid” (if that is even meaningful) is not, as such, particularly germane, for all that the question proves inescapable. It indicates only that *epistēmē* is a subset of *doxa*.

If that offers some clarification of what is meant by “knowledge”, it remains to offer some clarification of what is meant by “social foundations”. The key, as just noted, is that *different people* claim they “know” different things. What are the differences between people that are accompanied by differences in what they claim to know? What are the differences in what (and how and why) they claim to know? What are the relations (cause, consequence, or otherwise) between these two sets of differences?

When it comes to differences between people, we need to consider “different social orders”, meaning both different historical periods and different “cultures”, using this notoriously equivocal term in the sense of populations in different places, rather than at different times, particularly where those populations had minimal or no contact with each other. We also need to consider “different positions within those orders”, along all the lines of differentiation and stratification each order features. In the current world order, we commonly gloss these as “race, gender, and class”, though each of these glosses several different differences (“race”, for example, typically being short for race, ethnicity, nationality, culture – in a different sense – including racialized/ethnicized differences of language or religion) and there are still some left out (notably “disability” and age). Importantly, each of these needs to be considered both as *knowers* and as *knowns*.

When it comes to differences in how and why people claim to know, we need to consider at least socialization/acculturation (nonspecialized knowledge transmission), education (specialized knowledge transmission), communication (knowledge circulation), and, most importantly in the history of the field, specialized knowledge production, which goes under many names (originating in its conflictual history) of which the most significant are Art (or “Culture”, in yet another sense), Religion, and Science & Technology. It will be apparent how the project of tracing the relations between all these differences can quickly explode in scale – and this, keep in mind, is sociology of knowledge “narrowly construed”.

When it comes to relating these differences, a final comment should be made about speaking of the common core of the sociology of knowledge as studying the social “foundations” of knowledge. This is the traditional locution, but it can be misleading. “Foundations” implies a relation of causality, where whatever is being discussed as “the social” *causes* whatever is being discussed as “knowledge”. This is a common claim in the sociology of knowledge, and an important one, as it is the claim that got the field off the ground. But it is by no means the only claim made and it is clear that matters are nowhere near so simple. Indeed, amongst the significant questions in the field is how certain “social” differences – as knowns – are caused by other “social” differentia, including how they are known, and what being marked by those differences does to such people as knowers. Straightforward, one-way causality is not presumed.

**Everywhere but Nowhere: A Fragmented Field**

If the sociology of knowledge clearly has a core, its cohesion as a field is anything but clear. Consider: The Canadian, American, and International Sociological Associations each have a research cluster/section with “knowledge” in the name (the British and Australian don’t). The CSA’s is simply “Sociology of Knowledge”. The ASA’s is “Science, Knowledge, and Technology”. The ISA’s is “Sociology of Communication, Knowledge and Culture”. The “Sociology of Science and Technology” is a separate cluster of the ISA (which, communication and culture notwithstanding, also has clusters for “Language and Society” and “Sociology of Arts”), while “Communication, Information Technologies, and Media Sociology” and “Sociology of Culture” are distinct sections of the ASA. The CSA also has separate clusters for “Internet, Technology and Digital Sociology” and the “Sociology of Culture” – but none that specifically name “Science”. We could say that there is something of a Venn diagram here, with the sociology of knowledge either the intersection or the union of the sociologies of communication/media, culture, science, and technology. But even saying that implies that contemporary sociology of knowledge is submerged by, or incorporated within, these “other” sociological fields. Their intersection may not be empty, but it is the other content of their union that predominates – and the culture/science cleavage seems the key line of fracture.

In so far as Scheler, inaugurating the field *as* a field 100 years ago and clearly following Comte, construed the “material problems” of the sociology of knowledge to include the sociologies of religion, metaphysics, positive science (including technology), cultures of knowledge, and the development of knowledge and politics ([1924] 2012), it is also notable that the “Sociology of Religion” is a separate grouping at the ISA, ASA, British and Australian SAs, cleanly distinguished from this Venn diagram.

In a similar way, it is worth considering the evidence of figures 5 through 8 in Moody and Light (2006), showing four time slices of the topic network of sociological articles between 1970 and 2000. In each of these, science and technology form distinct peaks, but ones close to each other. The sociology of sociology and, where they appear, theory and postmodernism, are typically fairly close to science and technology, though less so in the early 1980s. Culture starts off clearly distinct from all of these, though it seems to migrate in their direction over time (it isn’t shown in the late 1990s figure). Neither education nor (where it appears) language ever seem particularly close to these other topics, while of the various lines of stratification, only class (early 1990s) and general inequality (late 1990s) approach them – specifically drawing near the sociology of technology. While the number of clusters identified by the authors clearly exceeds those they use in annotating their figures, it is notable that religion doesn’t appear in any of them – and neither does “knowledge” as such.

That said, there seem to be at least three major wings of the sociology of knowledge today, groupable under the headings “sociology of science”, “sociology of culture”, and “critical theory”. There is interaction between them, but each is following its own path, without any necessary reference to the others, and, crucially, each has its own identity as a field, all of which are better institutionalized than the sociology of knowledge. Importantly, there is also an alternative umbrella for all three wings collectively, which is vastly better institutionalized, namely, “theory”.

Probably the largest of the three wings is ***Critical Theory***, which takes up the question of the relation of knowledges to different positions within the social order. In this wing, I am including Marxist (e.g., Lukács [1922] 1971; Gramsci [1929–1935] 1971; Horkheimer and Adorno [1947] 2002; Althusser 1971), feminist (e.g., Smith 1990; Hartsock [1983] 2003; Harding 1986; Collins 1990; Haraway 1991), postcolonial (e.g., Du Bois [1903] 2015; Fanon [1952] 2008; Collins 1990; Connell [2007] 2021; de Sousa Santos 2015; Go 2016; Mbembe 2019), Indigenous (e.g., Tuhiwai Smith 1999; Kovach 2009), Queer (e.g., Butler [1990] 2006; Sedgwick [1990] 2008) and recent “Crip” (e.g., McRuer 2006; Oliver and Barnes 2012) theory. Institutionally, some of these have research clusters/sections of their own (e.g., the CSA and ASA both have “Marxist Sociology” clusters) *as well as* having clusters dedicated to the difference with which they are most associated (e.g., “class”, “race”, “gender and sexuality”). It is this wing which focuses most on how knowledge is implicated in constructing knowers (as knowns) and what that demands and permits those knowers when they then seek knowledge. What carries this wing so far outside the sociology of knowledge “narrowly construed” is thatit speaks to the very possibility and *right* of knowers from stigmatized social positions to *be* knowers and to assert *different* knowledge claims than those from privileged social positions. It is a *practical* demand that such knowers have faced over and over, from the earliest days of institutionalized sociology (cf., Du Bois [1903] 2015; Smith 1990; Tuhiwai Smith 1999): the Master’s stigmatizing knowledge is “objective” and “neutral”, while your destigmatizing knowledge is “subjective” and “political”. To which they can reply: if objectivity means the object is authoritative about itself, well, we’re the object here – a position Harding (1992) labelled “strong objectivity”. The ramifications of this touch every study that considers human differences, which is to say, every part of the human sciences.

The ***Sociology of Science***, like the sociology of religion, could be said to begin with Comte, but, as a distinct tradition, is commonly discussed as beginning with Fleck ([1935] 1979) and Bernal (1939). Both were working natural scientists who reflected on the recent history of, and social processes at work in, their own sciences during the “crisis” that the natural sciences experienced after the development of quantum mechanics. That scientists were part of social processes was no finding – de Solla Price ([1963] 2019) took his notion of “invisible colleges” from a label attached to a group of natural philosophers in Britain who met together before the founding of the Royal Society in 1660. That those social processes, and the “thought collectives” that embodied them (Fleck [1935] 1979), mattered to the science produced – that was a challenge. The work of Robert Merton (1973) offered valuable insight into the social processes of scientists, but it was Thomas Kuhn ([1962] 2012) who, following Fleck into the science itself, gave the sociology of science its contemporary impetus. Kuhn argued that science advanced by way of “revolutions” in which the “paradigms” (exemplary findings and their interpretations) guiding “normal science” were overthrown in favour of new ones that were – his most contested claim – “incommensurable” with their predecessors. Following up Kuhn’s insights with detailed studies of particular scientific worksites and products has formed the main work of the sociology of science since, with ethnographic investigations of scientific labs (starting with Latour and Woolgar [1979] 2013; Knorr Cetina 1981, [1999] 2009) having a special pride of place. Institutionally, this wing is not just often distinct from the sociology of knowledge, it can even exceed the bounds of sociology itself, as “Science and Technology Studies” with its own learned societies[[1]](#footnote-1).

The ***Sociology of Culture*** is itself a highly variegated field, as the various different senses of “culture” noted above might suggest. What perhaps gives it its greatest unity is the common debt acknowledged by many of its main protagonists to the work of Pierre Bourdieu (especially to Bourdieu [1979] 1984). Bourdieu’s earliest focus was on education and the role played by “cultural capital” in reproducing the social order, both independently of “economic capital” and amplifying it (Bourdieu and Passeron [1964] 1979, [1970] 1990; Bourdieu [1984] 1988, [1989] 2022)[[2]](#footnote-2). Two of his key contributions, from the point of view of the sociology of knowledge, are his notions of “classification struggles” and “*habitus*”. Classification has been a major area of work in the sociology of culture, particularly around matters like food and music and their relation to social identities (e.g., Lamont and Molnár 2002; Lamont 2012), and is a key point of contact with the sociology of science (notably Bowker and Star 2000). *Habitus* has provided a new framework in which to study socialization, implicating the body and senses in cognition (Ignatow 2007; Lizardo 2004, 2017; Cerulo 2018; Cerulo, Leschziner, and Shepherd 2021)[[3]](#footnote-3), and grounding the rich notion of “cultural schemas” (Hunzaker and Valentino 2019; Taylor and Stoltz 2020; Boutyline and Soter 2021). Cultural sociologists have also provided new approaches to the study of intellectuals (Lamont 1987; Bar-El 2021), which is also the key area of cross-pollination with self-identified “theorists” (Camic and Gross 2004; Frickel and Gross 2005).

These overviews are brief and schematic, but they do demonstrate how these literatures have their own dynamics and their own institutional forms, for all that they do interact. Do they add up to a sociology of knowledge? Substantively, perhaps, but only weakly in any institutional respect. To the extent that they do come together and form a common conversation, one that is *received* as a common conversation outside sociology, it seems more plausible to say that conversation is known as ***Social Theory***.

3*. An important claim from the sociology of knowledge is that to understand what counts as legitimate knowledge one must delve into the organizational processes underlying its production. Write an essay applying this perspective to the notion of the canon in sociology and evaluate its role in disciplinary knowledge production. In what ways has the canon been useful or a hindrance to knowledge production?*

There is an underlying, eminently practical, reason for canon formation: you can’t read everything. You especially can’t read (and discuss) everything in a 13-week semester with 39 hours of lectures, most likely happening concurrently with four other series of such lectures. Perhaps four article/chapter-length reading selections are feasible for each week, with able readers and where only weak engagement with the text is expected, implying 52 for the semester. There you have your upper bound. Out of however many thousands of texts on one’s focal subject, you can pick 52. How do you choose?

The usual claim is that one should pick “the best” such work, the “most influential”, “most insightful”, “most consequential” works. But which are those? Does not “most influential” simply mean “conformity”, picking the same things as others pick? While we might like to think that the “most insightful” work is the “most influential”, do we know that is true? A case like Ludwik Fleck, whose work from the 1930s only became influential after it was cited as an inspiration by Thomas Kuhn in the 1960s, whose own work was enormously influential, suggests that the connection is anything but assured.

Which raises a further issue: time. When we speak of text selection as “canon formation”, we commonly have in mind a *historical* selection of texts. It is the works of at least one generation prior, and preferably two, three, or even more, that can be deemed canonized. Current works may be highly cited – and thus *candidates* for canonization – but there are simply too many of them for any to have become “canon”. This contrasts sharply with the notion of the “state of the art” or the “research frontier”. In such a text selection process, it is precisely the *most recent* works which are prioritized, older works having been superseded: their strengths incorporated into the new work, their flaws identified and improved upon. A canon may be of interest to a historian of such a subject, but not to an active researcher.

In considering how “the canon” has been useful or a hindrance to knowledge production in sociology, it is this contrast with a “state of the art” that is at issue – both the strengths and the weaknesses of the current frontier. For, evidently, *some* sociologists found the canonical works to be useful for their own knowledge production. That’s how the works got canonized in the first place. But is that now only of historical interest? Is the canon superseded? And, if not, in what manner does it remain generative? I am, by disposition, a historian of thought. I am deeply interested in how particular claims came to be made in particular places at particular times, and in the path-dependencies that led to and followed from them. I am not always convinced the seeming state of the art does supersede it predecessors, rather than recapitulate them in new words – though the new words may be important. But, perhaps that is only a problem with identifying the state of the art, for I am also dubious about how truly generative the sociological canon remains, at least if one restricts that canon to the classic triptych of Durkheim, Marx, and Weber[[4]](#footnote-4).

Russell Collins’ *The Sociology of Philosophies* (1998) offers us our most directly applicable route into this problem. Collins begins by offering a substantial theory of intellectual life as a special case of “interaction ritual chains”, a particular kind of social market, building on Goffman ([1967] 2017). Intellectual products are “sacred objects”, whose sacredness arises from face-to-face encounters, “interaction rituals”, wherein they are made the mutual focus of attention of the participants, the mutual focus gives rise to a sense of membership in a group and “emotional energy” proportionate to the intensity of the interaction – the accumulated store of such “charged symbols” Collins calls (following Bourdieu) “cultural capital”. The sequence of such interaction rituals over a person’s life forms an “interaction ritual chain”, the steps of which (and the emotional energy deriving from them) are determined by the person’s position in the relevant network, conditional on what the whole network looks like. Intellectual networks are stratified competitions for attention and “ritual centrality”. Collins derives numerous behavioural conclusions from this, but two seem key: first, “the law of small numbers”, according to which an intellectual field can sustain only three to six rival positions; second, the need for forming lineages, master-pupil chains. Drawing on data from de Solla Price ([1963] 2019), Collins outlines the degree of stratification in the intellectual field as a whole, from the audience and would-be recruits, through transients, an outer and inner core, to the scientific stars, “leaders within the core”, who become the “heroes one reads about in histories of science.” (Collins 1998:44). The bulk of his 1000-page *opus* is then dedicated to those “heroes” and to the stratifications amongst *them* – the stratifications we call “canonization”.

Collins is quite unapologetic about equating “creativity” with “reputation”, arguing that, “Ideas are creative because they hold the interest of other people … the ‘creativity’ of a particular philosopher is not established until several generations have passed [!! – *ed.*] … Intellectual greatness is precisely one’s effect on the course of intellectual history, influencing generations downstream from one’s own.” (Collins 1998:58–59). Collins counts generations of 33 1/3 years – meaning the 2600 year coverage of his text amounts to 78 generations. By this standard, sociology is but 5.6 generations old, counting from 1838, or only 3.9, counting from 1895[[5]](#footnote-5). The “creativity” of sociologists, *any* sociologists, cannot yet be deemed firmly established, though Marx must presumably top the list of candidates. Collins makes one other important comment in this respect, “Canons do change, but only among those figures who have entered into the long-term chain of reputation in the first place.” (Collins 1998:59) Fleck’s elevation by Kuhn a generation later, or the rise of DuBois and Fanon over the last generation, are not, from this perspective, counter-examples to canonization, but evidence for it – and evidence of the prominence demanded to even be in the game (it is notable how the Marxists J.D. Bernal, a contemporary of Fleck’s, and C.L.R. James, a contemporary of Fanon’s, have *not* seen similar elevations).

What, then, are the processes by which this transmission takes place? Collins focuses on the “chains of personal relationships” between the leading figures (minor, secondary, and major), arguing that the most prominent figures are those with the most connections to other prominent figures. He has in mind particularly the relationships between teachers and students, but also those between contemporaries, recognized as “circles”. He also draws attention to the importance of *actual* “schools” – organizations with property where teaching and disputation take place – in sustaining these relations. These personal connections allow for (1) the passing of cultural capital, (2) the transfer of emotional energy, and (3) a structural sense of intellectual possibilities, “especially rivalrous ones” (Collins 1998:71). Because it is the *intensity* of emotion in interaction rituals that is most generative, conflictual contacts can be as productive as allegiant ones, and often more so.

That is the personal side. But Collins also draws attention to what he calls the “structural crunch”, his law of small numbers. Prominence demands not just recognizing and seizing possibilities made available by developments in the network, but also doing so *first* (that is, thinking here of Scheler and Mannheim, first with respect to the kinds of arguments that the network demands). This directs our attention to developments in the intellectual network/attention space and the possibilities in permits. In economic terms, the personal factors could be said to be the supply-side factors. But the network factors are the demand-side factors, and those are what shape the market. This leads to what seems Collins’ most audacious (and Hegelian-Heideggerian) argument, “It is not individuals … that produce ideas, but the flow of networks through individuals.” (Collins 1998:77). As far as a canonical history of thought is concerned, it is the *network* which determines the story that is told.

Inside the network, this takes two forms: strong positions subdividing as they grow larger (and their inner conflicting parties become able to sustain themselves), weak positions amalgamating and, if they can’t recover, disappearing. But the strength or weakness of positions is determined by their *external* support, the material bases that permit intellectual work. Changes in those external conditions are what demand “structural realignments” of the whole network. This is most obvious in the conditions of existence of organized, property-owning, schools. In effect, intellectuals can rely on bureaucratic organizations or on charismatic followings, as Weber defines these, with bureaucracy being by far the more reliable. Prominence in the network goes to both the subdividers of strengthening positions (“the creativity of fractionation”) and to the amalgamators of weakening ones (“the creativity of synthesis”), with their transmission depending on the degree of their routinization and bureaucratization.

Having considered Collins’ argument in some detail, I should take a moment to acknowledge that his points are not his alone. Frickel and Gross’ (2005) account of “scientific/intellectual movements”, while much less detailed and empirically supported, makes very similar points, where they conceive of such movements as resulting from grievances (especially of high-status actors) and depending on opportunity structures (to access resources), micromobilization contexts (to recruit followers), and collective action framing (to position the movement in a field or fields). Indeed, as they cite Collins, their argument can be seen as simply a restatement of his argument in terms of social movement theory, rather than on the basis of symbolic interactionism and social network analysis. Knorr Cetina’s discussion of “epistemic cultures” ([1999] 2009) offers much greater detail and texture to Collins’ “interaction rituals”, with her account of “becoming a laboratory leader” demanding a reorientation to the social being particularly salient. De Solla Price’s ([1963] 2019) account of “invisible colleges” hosting “scientific commuters” offers a useful image of the contemporary infrastructure of Collins’ “schools”, *across* (rather than *identical with*) universities. And, of course, Kuhn’s ([1962] 2012) account of anomalies and crises of paradigms demanding resolution through revolutions offers a useful set of alternative metaphors for the structural demands of Collins’ intellectual network, despite Kuhn’s own stress on the (relative) absence of competing “schools” in the natural sciences. Taking a prevailing reading of a text as a “paradigm” and dramatically new ones (dividing or amalgamating previous ones) as responses to crises in that reading certainly makes sense, and the proponents of such new readings are evident contenders for future canonization.

All that said, it remains to consider how this applies to the sociological canon. Trajectories of descent from Marx, Weber, and Durkheim do, of course, remain. Marxists retain a distinct identity as a self-conscious school, with their own institutions (parties, publishers, publications, scholarly associations) and even some degree of autonomy from the university. That said, they also remain relatively weak, especially compared with their standing during the late 1960s and early 1970s. This is perhaps clearest in that Marxism is generally treated as simply a current within critical theory, with feminism, critical race theory, postcolonialism, decolonialism/Indigenous methodology, and queer theory all being seen as more dynamic currents. At the same time, all of these acknowledge debts to Marx, which implies they are evidence of extensive fractionation due to the *strength* of Marx’s lineage overall. These currents are undoubtedly the most active in proposing new candidates for canonization, drawing on alternative accounts of sociology’s history. Marxism has also experienced important apostasies (e.g., Habermas, never that orthodox, is now viewed as a liberal not that distinguishable from Rawls, Laclau and Mouffe declared themselves “post-Marxist”) and some unexpected syntheses (e.g., Žižek’s synthesis of Marx with Lacan and post-structuralism, Badiou’s synthesis with a Platonism). Of specifically Marxist and political economic projects, it would seem that the most dynamic presently is world-systems analysis (e.g., Abu-Lughod 1989; Arrighi 1994; Chase-Dunn and Grimes 1995; Frank 1998; Wallerstein [1974] 2011)[[6]](#footnote-6).

Weber and Durkheim, however, never formed schools in the independent and self-conscious manner of Marxism. While some sociologists still speak of being “Weberians” or “Durkheimians”, the exact contents of such attributions aren’t especially clear. Weber unquestionably remains an icon and a paradigm of comparative-historical sociology – acknowledged as such by more recent icons like Michael Mann and Theda Skocpol – but Marx can be appealed to on similar grounds (hence Wallerstein [1974] 2011), and comparative-historical sociology seems (unfortunately) to be relatively niche in contemporary sociology, outside of some parts of political sociology. Durkheim does *not* remain a paradigm of statistical analysis in sociology. Indeed, while Durkheim does seem to be enjoying something of a renaissance at the moment (e.g., a “Canadian Network for Durkheimian Sociology” was founded in 2013, in dialogue with similar recently founded networks in other countries), what interest in him is not simply antiquarian seems to be largely due to Bourdieu being claimed as a “neo-Durkheimian”[[7]](#footnote-7). There is a joint mélange of Weber and Durkheim, a certain reading of Weber on “objectivity” ([1949] 2017a) and “ethical neutrality” ([1949] 2017b), a certain reading of Durkheim on “social facts” ([1895] 1982), that has been mobilized in opposition to critical theory and postmodernism, in defense of a self-declared “positivism” and “scientific” or “analytical” project for sociology. While such a synthesis is a sign of weakness, this project does have a certain dynamism, in its commitment to the exploration of new quantitative methodologies in social research. But, crucially, it is entirely possible to explore those same methods with no reference to the analytic project and its tendentious claims. If anything, it seems much easier and more natural to adopt an interpretive lens on these methods that positivism would decry, given the reliance of these methods on simulation, Bayesianism, and auto-encoding neural networks.

That discussion of Marxism’s weakness led to a discussion of newer fields of theory and research connected to newer political movements, while discussion of the weakness of Weber and Durkheim led to consideration of methods, is significant. The most substantive divide in sociology over the last generation or two has been that between “qualitative” researchers (for whom we might look to Glaser and Strauss [1967] 2017 as a paradigm) and “quantitative” ones (who lack any single paradigmatic text, unless you count something like Moore, McCabe, and Craig [1989] 2009). And, while that divide remains very much alive, it is also one that is widely viewed as an uncalled-for and out-of-date hindrance that should be abandoned. To the extent that Weber and Durkheim have been mobilized for one side and Marx and critical theory for the other, as preposterous as it should seem, that is clearly detrimental to the discipline.

As I have noted, sociology remains young by philosophy’s (and Collins’) standard. The stature of even its most eminent exemplars remains to be firmly established. There can be little question that large parts of the disciplinary network are calling for a broadening or revision of the canon, based partly on the increasing numerical predominance in the discipline and its audience of voices who are not straight white males, based partly on the declining relative geopolitical-economic weight of the U.S., U.K., Germany, and France, and the concomitant growth of academia elsewhere. There is, however, little to suggest any wholesale realignment. Marx’s greatest generativity is his relevance for precisely this broadening of the canon, which has the effect of diminishing his own singularity. Weber’s and Durkheim’s greatest generativity may be attention to methods. But that is a discussion in which they play little continuing part. Compared to, say, Foucault and Bourdieu, Latour, Butler, Hill Collins, Haraway and Braidotti – or to texts like Wasserman and Faust (1994) or Mohr, *et al.*, (2020) – the continuing importance of the canon seems slim.

3*. Discuss the major theoretical approaches to the causes/origins and outcomes of revolutions. In your view, has there been any major improvement in our understanding of the causes and consequences of revolutions since the 1950s and ‘60s?*

The prevailing narrative about the sociological study of revolutions has modernization and Marxist theories contending from the 1950s to ’70s, then being superseded by “state-centred”, “social-structural” theories, of which the canonical case is Theda Skocpol’s (1979) *States and Social Revolutions*. As this narrative is professed by protagonists of this latter approach, it is seen as a major advance, though it leaves many interesting problems for future research (Goodwin 2005) or is itself in need of supersession (Goldstone 2001, 2003). But, I find myself in the camp of Levi Martin and Judd (2020) and underwhelmed by these purported advances. The very research frontiers outlined by Goodwin and Goldstone suggest that the field remains fundamentally unclear about what it is actually studying. And, where the focal variable and its levels aren’t well-specified, no claim about its causes or consequences can inspire great confidence, since there’s every reason to think that very different phenomena are being confounded. Given this criticism, I will reverse the usual order and *first* summarize the contending theories as presented in the prevailing narrative, *then* turn to the definition of “revolution” and what the retailed developments in the field imply about it.

***Modernization Theory***

Modernization theory is a version of the classical idea of Progress that has been part of sociological theory since Comte’s founding of the discipline in the 1830s, updated for the conditions of the post-World War II world, and is perhaps most closely associated with Rostow (1960). “Modernization” is understood as a fundamental process of the contemporary world and a secular trend in all societies since the Scientific and Industrial Revolutions revealed the conditions for economic growth. There is a staged path of economic development from “traditional” agrarian subsistence to “modern” industrial economies, which is accompanied by similarly staged paths of social and political development – urbanization, secularization, and bureaucratization on the one hand, ultimate democratization[[8]](#footnote-8) on the other. But, if the direction of travel is clear and inescapable, the road along the way is not devoid of hazards and conflict, and each state must travel it on its own. Which is where the question of revolutions come in.

The key exemplars of modernization theory in the study of revolutions are taken to be Johnson (1966), Huntington (1968), and Gurr (1970). Johnson (1966) is presented as the exponent of revolutions due to *social* (and specifically *value*) changes during the transition from “traditional” to “modern” societies. Increased mobility, trade, and communications undermine the “traditional values” of a static, subsistence life, while the organization of social and political life remains tied to those values. Huntington (1968) is more focused on specifically *political* changes. States support and encourage the participation of their populations in the market economy, not least through expanding education, but refuse to similarly support people’s political participation. Gurr (1970) is framed as more “psychological”, but it may be more appropriate to say that he is a proponent of specifically *economic* changes. A rising standard of living (especially if *rapidly* rising) creates a “revolution of rising expectations” that can outpace actual economic performance, especially if growth slows or there is an actual downturn, and the consequent “relative deprivation” produces revolutionary impulses[[9]](#footnote-9).

The commonality between them is that there is a disjunction in the developmental sequence that needs to be rectified – certain aspects of social life are getting too far out ahead of other aspects and effort is demanded to get the laggard elements to catch up[[10]](#footnote-10). The key to whether such efforts become revolutionary is then the reaction of political authorities. If they provide ways to accommodate or facilitate the necessary changes, revolution becomes unlikely. If they actively resist and repress the necessary changes, a revolutionary situation emerges. “Ascending or aspiring groups, and rigid or inflexible institutions are the stuff of which revolutions are made” (Huntington 1968:275).

***Marxist Theory***

Revolution is, of course, a core concept of Marxist theory. But this elides an important distinction, which seems poorly recognized in the prevailing narrative (if also amongst Marxists). The important revolution is the revolution in the *mode of production*, the transition from wage-labour without ownership of the means of production under capitalism to collective ownership of the means of production under socialism. This is seen as following on a *political* revolution, in which the working class seizes control of the state. The seizure of state power is intended to *facilitate* the revolution in the mode of production, not least by preventing use of its armed forces against the working class, but the political revolution is not *itself* “the Revolution” which Marxism seeks. It is not clear that the idea of “social revolution” (even as defined by Skocpol (1979:4) – “rapid, basic transformations of a society's state and class structures” – much less in the later narratives) is sufficiently precise here, which matters in terms of the exemplars of “socialist” revolutions considered and the challenges they pose. But perhaps I get ahead of myself.

The important feature of the Marxist theory is class struggle. The classes that are party to that struggle vary across modes of production, but in capitalism the crucial actors are conceived as the capitalist “(haut) bourgeoisie” and “proletarian” industrial wage-workers. Industrial labour in the factory is already visibly and practically “social”, which serves as the basis for the proletariat to organize itself to realize the social character of production *de jure* and not just *de facto*: the revolution. As the strength of the working class will be greatest where industrialism is most advanced, it is presumed that the leading capitalist states will be ripest for revolution.

But, of course, the countries where professedly “socialist” revolutions took place were not the leading capitalist states, with the highest rates of industrialization. Rather, they occurred in predominantly agrarian states and, if anything, the degree of industrialization preceding such revolutions declined over time. Moreover, the agent of those revolutions was not so much industrial workers as agrarian ones: peasants. Goodwin (2005) reviews some of the Marxist dispute about *which* peasants were most revolutionary (Wolf 1969; Paige 1975), but that need not concern us here beyond its demonstration of one key point about Marxist analysis: the centrality of *detailed class analysis* to its comparative-historical investigations. The theory can be stated in very abstract and logical terms (cf. Shaikh 2016). But historical study demands getting into the details.

A last point which must also be made is that the revolutionary advance in the mode of production is presumed to be permanent, barring some catastrophic disaster that sets back total human productive capacity. More advanced modes of production are *more productive* of social wealth, so reverting would make no sense. Yet, almost all allegedly “socialist” countries reverted to capitalism in the last quarter of the 20th century, in most cases reversing their political revolutions as well. This is a matter of interest for the larger narrative and not just for Marxists.

***State-centred, social-structural theories***

Which brings us to the alleged great advance, the state-centred theories of Skocpol (1979) and those following her. Skocpol’s work grew out of a critical engagement with Marxism and the key differentia of her work – a focus on structural determinants of revolution, rather than voluntarist ones; a focus on the world-system, not just intrastate processes (if without endorsing Wallerstein’s own approach); and, most importantly, a focus on the not just “relative” but potentially *actual* autonomy of the state from the dominant class – reflect choices between Marxisms, including (in the last case) to push beyond the Marxist state of the art. Each of these prove important to her subsequent conclusion that the outbreak of social revolutions require direct pressure from more advanced competitor states, elite defection from the state, and organized peasants, while their outcome depends on the degree of dispersion of productive assets. Subsequent work seems less engaged with these issues, such that Goodwin can summarily assert that the approach explains revolutions “in terms of fluctuations in the nature and extent of state power” (2005:409).

Goodwin (2005:410–13) provides a nice three-dimensional schematization of this claim, in terms of the type of state organization (from patrimonial/clientelistic to bureaucratic/rational), the type of political regime (exclusive/repressive to inclusive/liberal), and its degree of infrastructural power, in Mann’s ([1986] 2012) sense. Weak repressive states promote the emergence of revolutionary movements. Weak repressive patrimonial/clientelistic states are vulnerable to being overthrown.

As nice as this schema is, it seems rather a step back from Skocpol’s argument. The dimension of international competition once again disappears, as does any class analysis, whether of elite conflict with the state, non-elite organization, or the distribution of capitals. Perhaps this reflects that Goodwin is speaking more generally of “revolutions and revolutionary movements”, while Skocpol was focused on the conditions for social revolution. Goldstone’s account of his own earlier work (2003:68–69) diverges less sharply from Skocpol’s, but his further argument (2001, 2003) also seems a step back, as he clearly is focused on reintroducing voluntarist factors and otherwise proliferating causes and cases. Which brings me to my critique.

***Critique***

In opening, I asserted that the field seems fundamentally unclear about what it is actually studying. This is not for lack of definitions. Goldstone, with his historical emphasis, provides an overview of several (2003:52–55), culminating in one of his own, “an effort to transform the political institutions and the justifications for political authority in a society, accompanied by ... mass mobilization and noninstitutionalized actions that undermine existing authorities” (2001:142). But he is quite explicit that he prefers to work from cases than from definitions, dismissing the alternative as “a large-*N* statistical approach”. And that has consequences, in so far as it is not clear that all the cases he cites, particularly the ones he sees as most challenging (2003:73–76), actually fit that definition and, if they do, that all the cases under that definitional umbrella are actually events of the same type.

Laudably, he recognizes this and offers a laundry list of “types of revolution” (2001:142–44) including great, political, social, elite (or “from above”), failed or abortive, peripheral, central, mixed, liberal/constitutional, communist, and Islamic revolutions, as well as rebellions, protests, and a general collapse of the government, to which one can append the anticolonial, nationalist, and antidictatorial revolutions he cites as challenging (*op cit.*). One notes the absence from this list of “counter-” revolutions, which seem particularly important for the East European and Soviet cases[[11]](#footnote-11). Goodwin similarly defines political, social, and failed revolutions, as well as “revolutionary movements” (2005:404–5). With such a mélange of alleged “cases”, is it any wonder that identifying clear causes and consequences proves elusive?

Then there are the research frontiers Goldstone and Goodwin identify. Goodwin explicitly frames his topic as “revolutionary movements” as well as “revolutions”, but his questions reveal just how broad this becomes – the election of Allende? the cultural contexts conducive to collective action? tactical repertoires[[12]](#footnote-12)? (2005:421–22) All of these are perfectly fine questions, but the study of revolutions seems to end up coinciding with the study of contentious politics – which is to say, simply *politics[[13]](#footnote-13)* – as such. Goldstone is explicit that he sees convergence happening between these literatures (2003:72–73). And, in arguing that a “fourth generation” of revolutionary theory should turn to the study of *stability*, we seem to be back to Parsons (1951) rather than Marx.

One feels rather like Freud in the face of women’s sexuality, “What does revolutionary theory want?”

3. *Many scholars refer to our time as the “age of populism.” Drawing on literature from the reading list, write an essay evaluating what theoretical perspectives you find most compelling for explaining the current populist wave. Are there theoretical perspectives from the list that confounded by these developments?*

I have been rather allergic to the very idea of “populism” since I first encountered William H. Riker’s *Liberalism against populism* (1982) at least three decades ago. Riker invokes “populism” to make a no-holds-barred attack on the very idea of democracy in favour of an unrestrained capitalism. Since then, “populism” has ever-increasingly been invoked, first to describe grassroots far-right opposition to Bill Clinton, then, more vociferously, to Barack Obama, then, to describe the leadership campaigns of Bernie Sanders and Jeremy Corbyn, the election of Donald Trump and the result of the Brexit referendum, Boris Johnson and Nigel Farage, the growing abundance of European far-right parties, from the Sweden Democrats and True Finns to Vox and Turkey’s AKP, with Orbán’s Fidesz, Le Pen’s National Rally, the German AfD, and all the members of the Great Italian Moving Right Show, from Berlusconi to Salvini to Meloni, not to mention odder characters like Beppe Grillo and his Five Star Movement, the Pirate parties, and troublesome leaders elsewhere from Ahmedinejad’s presidency of Iran to Chavez and Maduro in Venezuela, Morales in Bolivia, Correa in Ecuador, Duterte in the Philippines, and Modi in India.

1. There are important further things to be said about the relation of the sociology of technology to the sociology of science, since they are by no means identical, and about the special case of medical sociology, which has its own institutional embodiments, but these are dropped for reasons of space. [↑](#footnote-ref-1)
2. In this regard, Bourdieu is a rare point of contact between the sociology of knowledge and contemporary sociology of education, though both draw – in seemingly independent ways – from critical theory. [↑](#footnote-ref-2)
3. And sparking a wave of newly institutionalizing fields, the ISA, for example, having a cluster on “The Body in the Social Sciences”, a working group on “Society and Emotions”, and a thematic group on “Senses and Society”. The whole area of study has also made efforts to become a field as “cognitive sociology”. [↑](#footnote-ref-3)
4. Counting from Comte in the 1840s, sociology is only seven generations old. Durkheim and Weber, from the third generation, were canonized by Parsons in the fourth, while Marx, from what I’ve read, was only canonized in American sociology by the fifth generation in the 1960s. Numerous other third generation figures are at least quasi-canonized (e.g., Simmel, Tönnies, DuBois, Gilman, Veblen, Pareto) as are several from the fourth and fifth generations, but all of their reputations remain in flux. As we shall see, these are relatively short historical distances, which matters. But a sense of canoncity can be obtained from looking at those questioning the canon, see, *i.a.*, Alatas (2021), de Laat and Stokes (2022), Go (2023). [↑](#footnote-ref-4)
5. “Sociology” first appeared in Comte’s *Cours* in 1838, while the first department of Sociology (at Chicago) and the first chair of Sociology (Durkheim’s, at Bordeaux) were launched in 1895. Note that, in my previous note, I counted generations of 30 years, hence the larger number of them. [↑](#footnote-ref-5)
6. Though I would be remiss not to acknowledge Shaikh (2016) as the highest development of Marxian economics. [↑](#footnote-ref-6)
7. I will stress that this is an impression and not something I can properly document. [↑](#footnote-ref-7)
8. Not clearly distinguished from “liberalization” in this context. [↑](#footnote-ref-8)
9. Gurr is actually quite an interesting case here. While one can see how his argument fits the requisite pattern, his main concern is more to refute the longstanding idea that revolt is due to immiseration, *absolute* deprivation. And his main point holds up quite well, though it should be noted that relative deprivation is not just a question of slower growth than expected. Actual decline from a relatively high level is more mobilizing than a consistently low level or a slow path towards a high level. The criticism that grievance alone is insufficient and requires organizing is also fair, but it is worth clarifying how and where grievance originates. [↑](#footnote-ref-9)
10. There doesn’t seem to be consideration of the opposite possibility, reactionary, (counter-)revolutionary effort to rein in the *leading* elements through popular mobilization. The direction of movement is one way. [↑](#footnote-ref-10)
11. Though Mann (2012:264–66) argues these can be classed as revolutions “from above”. [↑](#footnote-ref-11)
12. Goodwin’s befuddlement by the idea of revolution without seizing state power suggests a shocking unfamiliarity with the long tradition of anarchism (see, pointedly, Holloway 2002), but also a larger failure to understand how the seizure of state power may not actually *be* the relevant revolution, as I note in my remarks above about Marxism. For those of us whose interest in revolution is the conditions for a change in the mode of production, that matters. [↑](#footnote-ref-12)
13. If, as Tarrow and Tilly (2007) maintain, contentious politics includes elections, then the distinction between it and what Levi Martin and Judd (2020) call “routine politics” seems empty. As the study of revolutions converges with it, any lingering necessity of the “irregular”, “extraconstitutional”, or even “noninstitutionalized” (forget violence!) presumably must end as well. [↑](#footnote-ref-13)