

What Do Historical Sociologists Do All Day? Analytic Architectures in Historical Sociology¹

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Drawing on an original data set of over 15,000 in-text citations, we use quantitative and qualitative techniques to analyze 37 award-winning publications in historical sociology between 1995 and 2015. We show that historical sociology comprises no fewer than four distinct analytic architectures that rely on different kinds of sources and use evidence and theory in different ways. We find suggestive evidence that the recognition of these different architectures has varied over time, such that award-winning works of historical sociology increasingly use architectures that favor the heavier use of primary sources and/or constructive theoretical syntheses. These findings suggest that analytic architectures are a consequential facet of the practice of social research that may yield important insights into dynamics of scholarly recognition, consecration, and methodological pluralism across the social sciences.

INTRODUCTION

In studies of the social factors that shape knowledge production (e.g., Lazarsfeld 1962; Merton 1979; Latour 1987), scholars have long recognized

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a gap between idealized representations of scientific methods and the bundle of practices that characterize the actual course of research. Though initially focused on the physical and life sciences (e.g., Kuhn [1962] 1996; Latour and Woolgar 1979; Knorr-Cetina 1981; Pickering 1995), this line of work has recently been extended to the social sciences, including sociology (e.g., Leahey 2008; Camic, Gross, and Lamont 2011; Peterson 2015). Yet despite its many insights, this scholarship has largely overlooked how authors link empirical material to existing theoretical debates and how they make those connections recognizable and meaningful to their intended audience. This neglect may reflect lingering assumptions that the translation of findings into recognizable terms is either unproblematic (e.g., Watts 2014, 2017) or simply a matter of rhetorical polish (e.g., Green 1988; Gans 1989; McCloskey 1990). In this article, by contrast, we argue that these practices should also be understood as socially structured and that they centrally shape how knowledge is produced and received (Leahey 2008).

To capture these research practices, we introduce the concept of “analytic architectures,” by which we mean the links that scholars make between theory and evidence for a given community of interlocutors. As sociologists have long noted (e.g., Lieberman 1992; Abend 2006; Gross 2010; Lamont and Swidler 2014), there are many ways to forge these links. We extend this observation to argue that choices about how to forge those links are consequential for communication, argumentation, and persuasion. Within any given scholarly community, architectures act as familiar templates for linking theory and evidence; they are thus vehicles for both formal and informal communication. Put differently, to communicate within and across scholarly traditions, methods, and substantive domains, authors use analytic architectures to make (or fail to make) their work legible to an intended audience of interlocutors. In the process, authors provide a set of orientations that communicate research findings while also sending important signals about scholarly identity and legitimacy.

We develop this argument by examining the case of historical sociology, an ideal place to study analytic architectures because it is substantively and theoretically fragmented (Skocpol 1984; Adams, Clemens, and Orloff 2005) and methodologically pluralist (e.g., Mahoney and Rueschemeyer 2003; Goertz and Mahoney 2012; Lange 2013; Mahoney and Thelen 2015). Historical sociology thus resembles a “fractal” (Abbott 2001) microcosm of the entire discipline, and if analytic architectures are an important aspect of scientific communication, we should expect to find them in historical sociology.

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To analyze the analytic architectures of historical sociology, we constructed an original data set of over 15,000 in-text citations from 37 award-winning publications in historical sociology between 1995 and 2015. We then subjected this data set to inductive quantitative and holistic qualitative techniques. We find that historical sociology includes at least four distinct analytic architectures and that each uses different kinds of sources and interfaces evidence and theory in different ways. Further, we find suggestive evidence that the recognition of these different architectures has varied over time, such that award-winning works of historical sociology increasingly use architectures that favor the heavier use of primary sources and/or constructive theoretical syntheses. We argue that this trend toward forms of presentation that resonate with the more archivally intensive “third wave” of historical sociology (Adams et al. 2005) may be driven as much by organizational dynamics of subdisciplinary reward as it is by shifts in scholarly focus.

This article thus makes three important contributions to sociological knowledge. First, and most important, we build on studies of the production of social knowledge (e.g., Camic et al. 2011) to emphasize a neglected structure—the analytic architecture—and demonstrate its importance to scientific practice. In particular, we show that strategies of organization, argumentation, and persuasion in the research process cannot be reduced to either the transparent communication of findings or pure rhetoric. Instead, they must be understood in light of the relationship between the author and her intended audience.

Second, we contribute to recent debates about methodological pluralism by suggesting that analytic architectures are an important means of managing methodological pluralism, at both subdisciplinary and disciplinary levels. Recent arguments both in favor of (e.g., Goertz and Mahoney 2012; Lamont and Swidler 2014; Porpora 2015; Aviles and Reed 2017) and against (e.g., King, Keohane, and Verba 1994; Jerolmack and Khan 2014) methodological pluralism largely assume that familiar methods are themselves coherent, integrated wholes. By contrast, we suggest that this familiar terrain is crosscut by analytic architectures that may divide some methods while bridging others. Discussions of pluralism in historical sociology and elsewhere thus need to be enlarged to include a focus on how analytic architectures may help make arguments and findings more or less legible across orthodox methodological and disciplinary divides. Further, since analytic architectures provide an important structure to historical sociology’s pluralistic terrain, we suggest that their description and analysis are likely to be useful in other areas as well (such as various forms of quantitative work or ethnography).

Third, the methods we employ to study scholarly writing and research contribute to the sociology of knowledge. Most studies of scholarly writing either intensively analyze a very small number of selected texts (e.g., Bazerman

1988; Abbott 2014) or engage in sometimes breathtakingly large-scale analyses of thousands of individual publications (e.g., Moody 2004; Foster, Rzhetsky, and Evans 2015). By contrast, we subject in-text citations in a relatively large sample of works to individual coding and analyze them at the level of distinct publications.² We thus link the substantive internal structure of scholarly works with their position in a larger population of texts and reveal how they cluster around similar but distinct analytic architectures. This intensive, medium-*n* strategy is potentially useful for other studies of knowledge production that seek to reveal the connections between the content of a work and its larger structural position (cf. Childress, Rawlings, and Moeran 2017).

We proceed as follows. We first develop the concept of analytic architecture, situating it within ongoing debates about the production of scientific and social knowledge. We then introduce the case of historical sociology, and the concerns about the interface of theory and evidence that have long troubled it. After detailing our data set and analytic strategy, we next present a cluster analysis of award-winning works of historical sociology and show that they fall into four different analytic architectures. After that, we examine how architectures have been rewarded over time, showing a recent trend toward rewards accruing to architectures favoring primary-source-based and theoretical-synthetic citation. Our conclusion discusses the implications of our findings and directions for further research.

ANALYTIC ARCHITECTURES

The philosopher of science Karl Popper ([1959] 2002, pp. 7–8) famously distinguished two phases of scientific research: the “context of discovery,” in which theoretical propositions are devised, and the “context of justification,” where one tries to falsify those propositions. Through this distinction, Popper (2002, p. 8) wanted to separate the “irrational element” that inspires discovery from the explicit, logical, methodological work of the scientific method. Ever since, sociologists of knowledge and science have challenged this distinction (e.g., Abbott 2004; Reed 2011; Tavory and Timmermans 2014), arguing that scientific research is a “complicated mixture of procedures” (Feyerabend 1975, p. 149) subject to social influences, regular adjustments, and second-guessing.

Despite the efforts of these scholars, many sociologists still treat everything that happens after the formulation of hypotheses as simply the carrying out of objective methodological formalities. For instance, in a recent

² Although it has precursors, to our knowledge we are the first to combine formal analysis of citation patterns, systematic coding of how they are used in the text, and an analysis of how those patterns relate to one another.

exchange in the pages of this journal, Watts (2014, 2017) criticized *verstehen* as an impediment to predictive social research. When challenged in a commentary (Turco and Zuckerman 2017), Watts conceded that *verstehen* was important in his own research process because the experience of graduate school led him to develop his renowned small worlds study. Nevertheless, he also strongly implied that it was irrelevant to his methodological choices: “The whole modeling process was an extended exercise in *converting* *verstehen* into *mathematics*. Once each model was specified . . . the exercise of exploring its properties *was essentially a mathematical one* . . . there is no *verstehen* there, just a mathematical result” (Watts 2017, pp. 1296–97; emphasis added).

Of course, this stance recapitulates Popper’s discovery/justification distinction. But more important for our purposes, Watts’s argument also implies that the “end stages” (Leahey 2008) of the research process are an unmediated, transparent formality whose working out requires no comment. Put differently, for Watts, once a model has been “specified,” all that remains is to demonstrate its properties. From this perspective, it makes little sense to consider how scholars present evidence and structurally arrange that presentation relative to their own and others’ theories or that those choices might affect the legibility of social-scientific knowledge.

Watts is hardly alone in treating the end stages of social research as unremarkable; even as sociologists of science have made huge strides in showing how social research is itself a social product, most of them have neglected these questions. Ironically, however, one of the strongest cases for studying the structure of published work comes from Popper himself. In *Objective Knowledge*, Popper (1972) depicts published research as a “third world” that bridges the world of “things in themselves” and the world of people’s beliefs about those things. Scholars interact with the third world by contributing scholarship to it, studying its internal structure in the hopes of discovering new problems generated within it, deriving empirical tests from it via critical engagement with its structure, and cyclically contributing to it anew. In Popper’s (1972, p. 121) evocative metaphor, scholars are “workers who are adding to the growth of objective knowledge as masons work on a cathedral.”

Popper thus provides an important warrant for examining the structure of published scholarly work, even if the modern sociology of knowledge and science (e.g., Mannheim [1936] 2015; Merton 1979; Kuhn 1996) has emphatically rejected his explicitly antisociological argument that the third world is an autonomous realm populated only by theories, propositions, and models subject to critical-rational reconstruction (Popper 1972, p. 114). Yet few have followed Bloor’s (1974, p. 70) suggestion to reimagine Popper’s third world as the “social world”—that is, to seek out the social influences on the actual process of composing and communicating research findings. We take up that task in this piece.

Our argument starts from studies of “methods in practice” (e.g., Abbott 2004; Reed 2011; Tavory and Timmermans 2014). This work usually begins with the blunt observation that methodological practices bear little resemblance to how they are eventually described in published research (e.g., Stinchcombe 1978, pp. 3–4; King et al. 1994, pp. 12–13). Instead, published findings are embedded in a much larger network of scholarly practices, including laboratory interactions with people and objects of study (Latour and Woolgar 1979; Pickering 1995; Peterson 2015), conversations with colleagues as they analyze data (Greiffenhagen, Mair, and Sharrock 2011, 2015), conference presentations (Gross and Fleming 2011), and evaluation and peer review (Guetzkow, Lamont, and Mallard 2004; Lamont 2009; Strang and Siler 2015; Teplitskiy 2016). During these interactions, “methods” do more than communicate findings; they also supply credibility for scientists’ claims (Shapin 1994) and signal a deeply ethical commitment to transparent reporting of “the truth” such that another scientist could (ideally) reach the same conclusions (Merton 1979; Daston and Galison 2014). In other words, Popper’s hard separation of logical “methods” from the social practices of scientific research inevitably produces a mismatch between “laboratory life” in its various forms and the procedures reported in eventual publication (Schickore 2008, 2009).

These direct studies of methods-in-practice have been complemented and extended by a second line of work: analyses of the rhetoric of published scientific work.³ Here, scientific practice is understood to include attempts to persuade a skeptical audience of the validity of one’s findings. In probing how this persuasion happens, scholars have recognized that science is written, and hence subject to rhetorical and literary analysis (Bazerman 1988; Locke 1992). Scholars have used these tools to analyze the narrative structures, rhetoric, and stylistic choices of historians (White 1973), anthropologists (Geertz 1988), sociologists (Green 1988; Agger 2007), economists (McCloskey 1990, 1998), and even snake-venom biologists (Schickore 2017).

There are thus two main domains of methods-in-practice: the concrete practices of the research site and the rhetorical structures of published work. But what connects them? Here, social studies of science stress acts of “translation” between the evidence produced during research itself and its presentation, which are reducible to neither pure practices nor rhetoric but rather

³ This work contrasts (sometimes sharply) with arguments that conceptual choices determine the rhetorical structure of a piece. For instance, Ermakoff (2017; but see Ermakoff 2019) argues that since indeterminate concepts “have no definite content” (2017, p. 129), they provide the dubious “gift” of “adapt[ing] and adjust[ing] to the specifics of the claims they are intended to back up” (p. 130). This, however, is a relationship of logical entailment: “The logic of empirical demonstration . . . gets turned on its head. Instead of setting constraints on the scope of the claim, the concept becomes its appendices. It trails the argument” (p. 130).

are constituted by their links to one another (Knorr-Cetina 1981). Scientists thus rely on the “written traces of laboratory work” as they develop publications, subjecting their actual practices first to “decontextualization, relived only in the rationales found in the scientist’s written notes,” and then “recontextualizing” them in “the literary mode of production” (Knorr-Cetina 1981, p. 130). Or as Bazerman (1988, p. 26) usefully summarizes, “an article is an answer to the question, Against the background of accumulated knowledge of the discipline, how can I present an original claim about a phenomenon to the appropriate audience convincingly so that thinking and behavior will be modified accordingly?” (See also Bazerman 1981.)

For some scientific disciplines—such as historical sociology—a crucial aspect of scientific writing is citation. If the “written traces of laboratory work” provide the substance of an argument, citations to other publications provide crucial intellectual context, motivate research questions and even signal academic affiliation (Kaplan 1965; Bazerman 1988; Hyland 1999, 2003; Moody 2004; Rawlings and McFarland 2011; Foster et al. 2015). As Latour (1987, p. 33) vividly puts it, “A paper that does not have references is like a child without an escort walking at night in a big city it does not know: isolated [and] lost, anything may happen to it.” Citations help combat this isolation, Latour continues: “The difference . . . between technical and nontechnical literature is not that one is about fact and the other about fiction, but that the latter gathers only a few resources at hand, and the former a lot of resources, even from far away in time and space” (1987, p. 33). In other words, recontextualizing a piece of research into the specifically scientific mode of literary production requires embedding it in a network of references both to the work done by the researcher herself and to the work of others (Gilbert 1977, p. 116).

Sociology, of course, has not been immune to structural analysis of its written work. Scholars periodically lament that sociologists’ writing is terrible (Mills 1959; Cheek and Rosenhaupt 1968; Selvin and Wilson 1984; Fine 1988) and that writing is seldom taught as a central component of method (Becker 1986; Zerubavel 1999; Mayrl and Westbrook 2009). Yet it is only recently that these laments have produced systematic scrutiny (Abbott 2014), most often of the rhetoric of sociological argumentation. Some work investigates the increasingly uniform structure of articles, variously attributing it to mimicking high-status statistical techniques (Leahey 2005) or a response to reviewers’ demands that authors obey particular conventions (Strang and Siler 2015; Teplitskiy 2016).⁴ Other work dives into the substantive rhetoric of sociological publications. This work shows that linguistic tropes, metaphors, and narratives run alongside formal methodological

⁴ Abbott and Barman (1997), however, question whether sociology publications have become more rhetorically standardized over time.

concerns, supplying a crucial aspect of what makes research persuasive. Thus, for instance, ethnographers use “styles of causal thought” to make their arguments (Abend, Petre, and Sauder 2013), quantitative analysts use models that “[inscribe] stylistic differences in writing the structuring of social relationships, with the styles in turn based on different images of the structuring of the cells of a table of data” (Breiger 2002, p. 97), and sociologists generally employ causal “metaphors” derived from cognitive linguistics to explain outcomes in, variously, “initiating,” “conditioning,” or “probabilistic” ways (Vaidyanathan et al. 2015).

In line with these studies, our concept of analytic architectures attends both to the formal and substantive aspects of published sociology, while also extending it in two crucial respects. First, no work (to our knowledge) has yet explored whether and how the causal heuristics scholars use relate to patterns of constructively, or critically, engaging with theoretical literature or to marshaling specific sorts of evidence instead of others. Second, and more concretely, we conceive of analytic architectures not simply as a facet of rhetoric, but as an important interceding step that must be taken before rhetorical polishing can begin. To adapt Popper’s metaphor, if contributing to pure scientific knowledge resembles collectively building a cathedral, writing a publication or undertaking a research program resembles constructing a house. “Methods,” as traditionally understood, are the first stages, where the various building materials are collected and assessed. “Rhetoric” captures the final stages, when trim is applied, the house is painted, and the landscape is beautified. But in the middle, a builder must outline the final product—this is the analytic architecture of the publication. Without this step, the house—no matter how fine the materials it is made from and no matter how masterfully plastered or painted—cannot stand. For sociologists, theory and evidence are like wood, glass, metal, and concrete; to build a stable house—to say nothing of beautiful and enduring cathedrals—these materials must be carefully collected and sorted and arranged into recognizable and stable architectures.

WRITING AND CITING IN HISTORICAL SOCIOLOGY

Historical sociologists, like producers of any other scientific research, depend on analytic architectures to bridge their research process and published work and to make their arguments legible for their readers. In fact, analytic architectures are likely to be particularly important for historical sociologists because of the discipline’s long-standing theoretical, methodological, and substantive fragmentation. As the field passed from its second to its third wave,⁵ historical sociologists have generated a kaleidoscope of

⁵ Although the numbering of these waves has been challenged for neglecting marginalized currents in the subdiscipline (e.g., Steinmetz 2010), we employ the standard numbering here for simplicity’s sake.

new research areas and interests (Adams et al. 2005) and introduced a growing array of new methodological options (see Goertz and Mahoney 2012 and Lange 2013 for recent reviews).

Existing methodological writing in historical sociology, however, has paid little attention to how to interface theory and evidence compellingly. Instead—as with methodological writing more generally—historical sociologists have tended to prescribe the best logical procedure for arriving at valid propositions,⁶ thus largely sidestepping how actually to construct an argument or what makes it persuasive to an audience. Nevertheless, carefully reading the work of historical sociologists somewhat against the grain reveals two deep, recurrent concerns: (1) how theory and evidence should relate to one another and (2) what kinds of sources count as evidence. We contend that these elements are the building blocks of historical sociology's analytic architectures.

Theory and Evidence, Dealt with Constructively or Critically

In her foundational edited volume, *Vision and Method in Historical Sociology*, Skocpol (1984, pp. 16–17) contrasts historical sociology that is “theory driven” with that which is “problem oriented.” Theory-driven scholarship is conducted “in close, albeit critical, relationship to the dominant macrotheoretical paradigms of contemporary sociology” (pp. 16–17). These works are therefore “permeated by their arguments with” existing theoretical statements (p. 16). Problem-oriented scholarship, meanwhile, is oriented to empirical particulars. Accordingly, Skocpol observes, the primary aim of this scholarship “is not to rework or reveal the inapplicability of an existing theoretical perspective, nor is it to generate an alternative paradigm to displace such a perspective. Rather, the primary aim is to make sense of historical patterns, using in the process whatever theoretical resources seem useful and valid” (p. 17).

⁶ This prescriptive work has three main strands. First, a large literature stresses the interface between theory and conceptualization, and particularly how sociological theory shapes the interpretation of historical data (e.g., Bonnell 1980; Ragin and Zaret 1983; Kiser and Hechter 1991, 1998; Quadagno and Knapp 1992; Calhoun 1998; Somers 1998). A second strand queries the appropriate units of analysis, including case selection (e.g., Ragin and Becker 1992; Rueschemeyer 2003; Steinmetz 2004) and whether and how comparison reveals causal relations (e.g., Skocpol 1979, 1984; Skocpol and Somers 1980; Ragin 1987; Lieberman 1991, 1994; Haydu 1998; Mahoney 2000, 2003; Mahoney and Rueschemeyer 2003; Rueschemeyer 2003; Steinmetz 2004; Clemens 2007; Lange 2013; Pacewicz, forthcoming). Finally, a third strand develops techniques and strategies for inferring causality (e.g., Abbott 1992; Goldstone 2003; Griffin 1993; Stryker 1996; Abbott and Barman 1997; Emigh 1997; Steinmetz 2004, 2014; Ermakoff 2008, 2019; Goertz and Mahoney 2012; Padgett and Powell 2013). As Lange (2013) notes, historical sociologists have paid far more attention to questions of analysis and inference than to questions of data collection (but see Tilly 1981; Skocpol 1984; Mayrl and Wilson 2016, 2019; Møller and Skaaning, forthcoming).

Skocpol's contrast between theory-driven and problem-oriented scholarship suggests two important axes of variation: (1) the relative importance of theoretical versus empirical citations and (2) the mode of engagement with those citations (what we call a citation's *valence*). Scholars pursuing both approaches doubtless engage in careful and penetrating historical analysis, but they are also likely to *draw on theory and evidence in different proportion*. Similarly, we would expect that scholars using one or the other approach to engage with theory and evidence in very different ways: *constructively* deploying strands of theory synthetically in the service of empirical analysis is different from referencing a body of theory to *critically* deconstruct or reconstruct it. While Skocpol merges these axes, in reality they describe different orders of citation: the distinction between theory and evidence addresses *what* is cited; the distinction between constructive and critical modes asks *how* something is being cited. It is thus easy to imagine works drawing on identical proportions of theory and evidence, but engaging with them in very different ways.

These distinctions have echoes in other methodological writings in historical sociology, suggesting that—while not articulated in terms of architectures—they are recurrent, even central concerns. For instance, one longstanding methodological debate addresses whether theory and evidence should be understood as separate or unavoidably intertwined and ponders how theories make evidence meaningful and vice versa (e.g., Mann 1986; Kiser and Hechter 1991, 1998; Somers 1995, 1998; Calhoun 1998; Steinmetz 2005). Similarly, prefatory reflections in landmark works in the field often highlight the project's overall critical or constructive orientation for the reader. Anderson's *Passages from Antiquity to Feudalism*, to take one example, explicitly argues for a constructive empirical synthesis as a warrant for his work (1974*b*, pp. 9, 17), while Skocpol explicitly vows to “take issue with certain conceptions, assumptions, and modes of explanation” in existing works, using these critiques to create an opening for her own argument in *States and Social Revolutions* (1979, p. 14). In deciding how to place theory and evidence in dialogue, then, historical sociologists must decide the mixture of the two and also whether to engage the two constructively or critically.

Kinds of Evidence

Beyond constructive or critical engagement with theory and evidence, historical sociologists may also choose different kinds of evidence—most significantly, primary and secondary sources. Many historical sociologists prize primary sources (i.e., “any source that came into existence contemporaneously with the event or phenomenon under investigation” [Bonnell 1980, p. 158*n*9]), viewing them as directly related to the object of study. Although primary sources can never be taken at face value or treated as

completely unmediated “data” (e.g., Goldthorpe 1991), adequately vetted primary sources are typically treated as the gold standard in historical research.

By contrast, secondary sources (“accounts compiled on the basis of primary sources” [Bonnell 1980, p. 158n9]) have a more fraught position (Møller and Skaaning, forthcoming). Indeed, historical sociologists have developed three distinct approaches to secondary sources. Some rely on them with little or no recourse to primary evidence; many works in the second wave of historical sociology were built on massive syntheses of secondary research (e.g., Moore 1966; Skocpol 1979; Wallerstein [1974] 2011),⁷ and early methodological statements argued that using secondary works was unavoidable if one wished to answer large macrocomparative questions (Skocpol 1984).⁸ Other scholars, meanwhile, advocated a “targeted primary” strategy, wherein historical sociology based mainly on secondary sources can be “strategically supplemented by carefully selected primary investigations or reinvestigations” in order to resolve ambiguities, answer novel questions, or supplement the historical record (Skocpol 1984, p. 383). Here, primary research is cast as a beneficial supplement to synthetic secondary analysis (see, e.g., Amenta 2003).

More recently, these two positions have been challenged by a third that directly questions the use of secondary sources. Because secondary sources have been selectively constructed by historians, sociologists who use them risk relying on “interpretation of interpretations of, perhaps, interpretations” (Goldthorpe 1991, p. 222; but see Stryker [1996] for a more positive formulation). Accordingly, this position encourages scholars to avoid secondary sources in favor of (apparently) less problematic primary sources.⁹ Though this has long been an option for historical sociologists (e.g., Tilly 1964, p. 14), today’s third wave practitioners may now be inclined (if not expected) to “do the kind of high-quality, original, archival, primary source research expected of historians” (Adams et al. 2005, p. 27).

DATA AND METHODS

To understand the analytic architectures of historical sociology, we examine every published book and article that won an award from the American Sociological Association’s Section on Comparative and Historical Sociology (ASA-CHS) between 1995 and 2015—a population that consists of 18 books

⁷ This approach dominated historical sociology in its early days; one study found that 84% of late 1980s macrocomparative sociology relied exclusively on secondary sources (Bollen, Entwisle, and Alderson 1993, p. 344).

⁸ Indeed, many considered unearthing “facts” to be the job of historians, not sociologists (Tilly 1981; McDonald 1996).

⁹ Archives themselves, however, are selectively constructed sites with their own biases (e.g., Blouin and Rosenberg 2011; Skarpelis 2019).

and 19 articles in total (see table 1).¹⁰ There are strengths and weaknesses to this strategy. In examining the ASA-CHS, we do not capture other institutional hubs for comparative-historical sociology with different histories, foci, and leadership—most notably the Social Science History Association (SSHA) (Abbott 2001). But since the SSHA is interdisciplinary, it also rewards the work of historians, political scientists, and others. Focusing on the ASA-CHS is thus a more appropriate means of assessing trends in historical *sociology* specifically. Additionally, using ASA-CHS awards as the basis for our population also centers our study on *American* historical sociology, while significant contributions to the subdiscipline have, of course, been made elsewhere in the world.

Our focus on *award-winning* books and articles is also consequential—they have, after all, been recognized and consecrated by subdisciplinary elites as exemplary. On the one hand, the fact that these works are literal exemplars provides a useful window into the methodological norms that ASA-CHS promotes. But on the other hand, the selection process for awards is inevitably political (Lamont 2009), so our approach examines dominant expressions of historical sociology, perhaps to the neglect of alternatives.¹¹ This strategy does, however, provide one further benefit: it allows us to make observations about the relationship between analytic architectures and consecration processes, which we do below.

Analytic and Synthetic Strategy

Having selected our population, we then pursued a three-step research procedure. In our first step, we followed the discussion above and focused on citations as particularly important connections between research practices

¹⁰ From 1995 to 2004, the ASA-CHS awarded a single prize (the Barrington Moore Prize) to the best book or article in the field. Beginning in 2005, however, the award was split into best book (Barrington Moore) and best article (Charles Tilly) awards. In 1996, no award was presented, and the Moore awards for 1995, 1997, 2009, 2013, and 2014 were shared, as was the Tilly award for 2011. We examined every winner and co-winner, but excluded honorable mentions. Because this article considers only published materials, we do not examine Reinhard Bendix (best student paper) Award or Theda Skocpol (best dissertation) Award winners.

¹¹ We follow previous scholarship that has examined award winners to glean insights into science (e.g., Azoulay, Graff Zivin, and Manso 2011; Leahey and Cain 2013; Foster et al. 2015). While some other studies of published works (Bollen et al. 1993; Abend 2006; Goertz and Mahoney 2012) randomly sample articles in major journals, this approach has its own elite bias (Goertz and Mahoney 2012, p. 227) while posing two further problems. First, historical sociology is published in diverse journals and book series, making the underlying population to sample unclear. Second, an appropriate filtering mechanism is also unclear: even a crude metric, such as the term “historical sociology,” excludes some works like Fishman and Lizardo (2013), which won the ASA-CHS Tilly Prize but which uses that phrase only in the title of an item cited in its bibliography.

TABLE 1
ASA-CHS SECTION AWARD-WINNING BOOKS AND ARTICLES

Year of Award	Author	Publication
2015†	Melissa Wilde and Sabrina Danielsen	“Fewer and Better Children” (2014)
2015*	Kathleen Thelen	<i>Varieties of Liberalization and the New Politics of Social Solidarity</i> (2014)
2014*	Wenkai He	<i>Paths toward the Modern Fiscal State</i> (2013)
2014*	Cybelle Fox	<i>Three Worlds of Welfare Relief</i> (2012)
2014†	Robert Fishman and Omar Lizardo	“How Macro-Historical Change Shapes Cultural Taste” (2013)
2013*	Michael Mann	<i>The Sources of Social Power</i> (2012)
2013*	Monica Prasad	<i>The Land of Too Much</i> (2012)
2013†	Elisabeth Anderson	“Ideas in Action” (2013)
2012*	Yang Su	<i>Collective Killings in Rural China during the Cultural Revolution</i> (2011)
2012†	Nicholas Hoover Wilson	“From Reflection to Refraction” (2011)
2011*	David Garland	<i>Peculiar Institution</i> (2010)
2011†	Danielle Kane	“The Puzzle of Korean Christianity” (2009)
	Jung Mee Park	
2011†	Andreas Wimmer and Yuval Feinstein	“The Rise of the Nation-State across the World, 1816 to 2001” (2010)
2010*	Andrew G. Walder	<i>Fractured Rebellion</i> (2009)
2010†	Dan Slater	“Revolutions, Crackdowns, and Quiescence” (2009)
2009*	Karen Barkey	<i>Empire of Difference</i> (2008)
2009*	Ivan Ermakoff	<i>Ruling Oneself Out</i> (2008)
2009†	Cedric de Leon	“No Bourgeois Mass Party, No Democracy” (2008)
2008*	George Steinmetz	<i>The Devil’s Handwriting</i> (2007)
2008†	John F. Padgett	“Organizational Invention and Elite Transformation” (2006)
	Paul D. McLean	
2007*	Monica Prasad	<i>The Politics of Free Markets</i> (2006)
2007†	Andreas Wimmer and Brian Min	“From Empire to Nation-State” (2006)
2006*	Michael Mann	<i>The Dark Side of Democracy</i> (2005)
2006†	Monica Prasad	“Why Is France So French?” (2005)
2005*	Vivek Chibber	<i>Locked in Place</i> (2003)
2005†	Marc Steinberg	“Capitalist Development, the Labor Process, and the Law” (2003)
2004*	Philip S. Gorski	<i>The Disciplinary Revolution</i> (2003)
2003*	Jack A. Goldstone	“Efflorescences and Economic Growth in World History” (2002)
2002*	James Mahoney	<i>The Legacies of Liberalism</i> (2001)
2001*	Julia Adams	“Culture in Rational-Choice Theories of State Formation” (1999)
2000*	Anthony W. Marx	<i>Making Race and Nation</i> (1998)
1999*	Jeff Goodwin	“The Libidinal Constitution of High Risk Social Movements” (1997)
1998*	Thomas Ertman	<i>Birth of Leviathan</i> (1997)
1997*	Robin Stryker	“Beyond History versus Theory” (1996)
1997*	David Zaret	“Petitions and the ‘Invention’ of Public Opinion in the English Revolution” (1996)
1995*	Julia Adams	“The Familial State” (1994)
1995*	Roger Gould	“Trade Cohesion, Class Unity, and Urban Insurrection” (1993)

* = Barrington Moore Award.

† = Charles Tilly Award.

TABLE 2
SOURCE ROLES IN ARGUMENT CONSTRUCTION

CHARACTER	VALENCE	
	Constructive	Critical
Empirical	1. Evidence in support of the author's argument.	2. Evidence showing that competing arguments about the case at hand are incomplete or incorrect.
Theoretical	3. Use of theories, concepts, or other tools to help construct the author's argument about the case at hand; invocation of conceptual debates, relevant literatures, or broader intellectual content.	4. Criticism of flaws, oversights, or illogical aspects of competing arguments or theoretical traditions.

and an author's attempts to persuade her audience of a study's findings.¹² We began by taking individual in-text citations within each publication as our unit of analysis. We coded every citation in each article, and major sections (the introduction or first chapter, middle chapter, and an additional randomly selected chapter) of books. We noted the content of the citation (title, author, and year) alongside its position in the book or article. We coded how each source was used in the author's argument according to a four-fold scheme (see table 2). We first judged the citation's valence—whether the author was using it constructively or critically—and also its character—whether it was being used empirically or theoretically. Thus any particular cite could be (1) a constructive-theoretical cite—an author building her theoretical model; (2) a constructive-empirical cite—an author presenting supporting evidence for her argument; (3) a critical-theoretical cite—an author criticizing a wrongheaded approach to explaining outcomes; or (4) a critical-empirical cite—an author critiquing a competing explanation for an outcome as factually incorrect.¹³ Third, for materials coded as empirical, we also coded the course as primary or secondary, using Bonnell's (1980) definitions (above) as a guide.¹⁴ This coding was made on the basis of holistic judgments about both the work's bibliographical notation and also the context of its textual

¹² This focus is also consistent with the importance historical sociologists accord to citations in describing their own research practice (Mayrl and Wilson 2016).

¹³ We divided coding equally during the initial phases of research, but as we expanded the population to include more recent winners, Mayrl coded the five most recent works in our population.

¹⁴ A number of authors used quantitative analyses of primary data (e.g., Zaret 1996; Steinberg 2003; Padgett and McLean 2006; Prasad 2006; Wilde and Danielsen 2014). For such works, we examined all tables and appendices for any information about the sources and coded those sources. When multiple tables were derived from the same data set, each source was coded separately as an independent citation for the first table only,

citation.¹⁵ (For further details on our coding procedure, see online app. A; for illustrative examples, see online app. C.)

We coded 15,256 total citations, and used them to calculate the parameters of the population as a whole, which are reported in the “descriptive statistics” portion of tables 3 (articles) and 4 (books). The lengths of the material coded varied widely. Articles ranged in length from a minimum of 15 pages in Goodwin’s (1997) “Libidinal Constitution” to a maximum of 85 for Padgett and MacLean’s (2006) “Organizational Invention,” while the length of coded sections of books ranged from a minimum of 57 for Chibber’s (2003) *Locked in Place* to a maximum of 214 for Steinmetz’s (2007) *The Devils’ Handwriting*. The density of citations per page also varied significantly for both books and articles—from nearly 10.5 citations per page in Zaret’s (1996) “‘Invention’ of Public Opinion” to a minimum of just over two citations per page in Stryker’s (1996) “Beyond History versus Theory.” Books shared a similar pattern (albeit with lower variation)—on average, Marx’s (1998) *Making Race and Nation* cited almost 10 items on every page, while Barkey’s (2008) *Empire of Difference* cites just under three. Accordingly, we grew concerned that a tendency of authors to cite more items as a whole might distort our search for differential patterns of citation, so we used the proportion of citations by each author for our subsequent analysis.

For each of the works in our study population, tables 3 and 4 report the length of the coded sample, both in terms of pages and total citations, and also the proportion of those citations used in each of the four ways described above—that is, as constructive-empirical (CoEm), constructive-theoretical (CoTh), critical empirical (CrEm), or critical-theoretical (CrTh). In addition, we also computed the proportion of a work’s total citations that were empirical—whether critical or constructive—and were primary (P1) or secondary (S2), as well as the ratio of one to the other. Thus, for example, *Empire of Difference* cites very few primary empirical materials—only about 1%—and instead favors secondary materials heavily. Likewise, most of the works Barkey cites are cited constructively, whether empirically (70.4%) or theoretically (21.6%).

The parameters reported in tables 3 and 4 are striking in two initial ways. First, in both books and articles, there is relatively little critical citation overall, whether empirical (about 2% for both books and articles) or theoretical (about 5% for books and 9% for articles). Second, there is a wide range in the

and each subsequent table was cited as a single “aggregate” archival source. The complete composition of these data sets is not always made clear from the text, however—and in some cases (e.g., Mann 2005) no information about the quantitative analyses was provided—so it is likely that these analyses may undercount primary sources. This is an unavoidable limitation of the data set.

¹⁵ We were unable to rely on authors’ characterization of sources as primary or secondary because, in many cases, the authors did not explicitly discuss their sources in these terms.

TABLE 3
DESCRIPTIVE STATISTICS AND CLUSTERING SOLUTIONS FOR ARTICLES

ARTICLES	DESCRIPTIVE STATISTICS										CLUSTERING SOLUTIONS									
	PP	TOTAL CITES	CITES PP	PRIM	SEC	EM	TH	CONSTR	CRIT	COEM	CO TH	CR EM	CR TH	CoEm	CoTh	CrEm	CrTh			
All Articles																				
Max.....	85	549	10.44	.77	1.00	.86	.78	.97	.24	.85	.75	.09	.24	.85	.75	.09	.24			
Min.....	15	65	2.13	.00	.23	.22	.14	.76	.03	.22	.07	.00	.01	.22	.07	.00	.01			
Mean.....	40	255	6.51	.28	.72	.60	.40	.89	.11	.59	.31	.02	.09	.59	.31	.02	.09			
SD.....	15.82	136	2.55	.28		.20		.06		.21	.18	.03	.05	.21	.18	.03	.05			
Cluster 1:																				
Slater 2009.....	51	229	4.49	.00	1.00	.52	.48	.84	.15	.52	.32	.00	.15	.52	.32	.00	.15			
Wilson 2011.....	34	335	9.85	.10	.90	.41	.59	.85	.15	.41	.44	.00	.15	.41	.44	.00	.15			
Wimmer & Min 2006.....	30	124	4.13	.00	1.00	.40	.60	.77	.23	.33	.44	.07	.16	.33	.44	.07	.16			
Wimmer & Feinstein 2010.....	26	65	2.5	.00	1.00	.38	.62	.80	.20	.29	.51	.09	.11	.29	.51	.09	.11			
Adams 1999.....	21	119	5.67	.06	.94	.30	.70	.76	.24	.30	.46	.00	.24	.30	.46	.00	.24			
Average.....	32.4	174.4	5.33	.03	.97	.40	.60	.80	.20	.37	.43	.03	.16	.37	.43	.03	.16			
Cluster 2:																				
Padgett & McLean 2006.....	85	549	6.46	.56	.44	.86	.14	.96	.04	.85	.11	.01	.03	.85	.11	.01	.03			
Prasad 2005.....	44	371	8.43	.64	.36	.82	.18	.93	.07	.82	.11	.00	.07	.82	.11	.00	.07			
Steinberg 2003.....	44	446	10.14	.60	.40	.77	.23	.93	.07	.77	.16	.00	.07	.77	.16	.00	.07			
Zaret 1996.....	49	512	10.45	.77	.23	.85	.15	.89	.11	.83	.07	.02	.08	.83	.07	.02	.08			
Wilde & Danielsen 2014.....	43	305	7.09	.76	.24	.72	.28	.89	.11	.72	.17	.01	.11	.72	.17	.01	.11			
Average.....	53	436.6	8.51	.67	.33	.80	.20	.92	.08	.80	.12	.01	.07	.80	.12	.01	.07			

Cluster 3:												
Kane & Park 2009	35	283	8.09	.03	.97	.67	.33	.94	.06	.67	.27	.06
Goldstone 2002	66	330	6.00	.00	1.00	.80	.20	.90	.10	.73	.17	.08
Adams 1994	35	188	5.37	.09	.91	.79	.21	.97	.03	.79	.18	.00
Gould 1993	33	125	3.79	.18	.82	.79	.21	.94	.06	.71	.22	.04
de Leon 2008	43	214	4.98	.34	.66	.74	.26	.93	.07	.68	.25	.00
Average	42.4	228	5.65	.13	.87	.73	.27	.94	.06	.72	.22	.05
Cluster 4:												
Stryker 1996	48	102	2.13	.00	1.00	.22	.78	.97	.03	.22	.75	.00
Cluster 5:												
Fishman & Lizardo 2013	23	199	8.65	.30	.70	.44	.56	.91	.09	.44	.47	.09
Anderson 2013	34	213	6.26	.39	.61	.56	.44	.89	.11	.55	.34	.01
Goodwin 1997	15	152	10.13	.56	.44	.49	.51	.90	.10	.49	.41	.00
Average	24	188	8.35	.42	.58	.50	.50	.90	.10	.49	.41	.09

TABLE 4
DESCRIPTIVE STATISTICS AND CLUSTERING SOLUTIONS FOR BOOKS

	TOTAL		DESCRIPTIVE STATISTICS										CLUSTERING SOLUTIONS																
	PP.	CITES	CITES PP	PRIM	SEC	EM	TH	CONSTR	CRIT	CO EM	CO TH	CR EM	CR TH	PP.	CITES	CITES PP	PRIM	SEC	EM	TH	CONSTR	CRIT	CO EM	CO TH	CR EM	CR TH			
All Books																													
Proportion of All Citations																													
Max.....	214	1495	9.79	.73	1.00	.89	.31	.99	.18	.89	.25	.03	.15																
Min.....	57	249	2.98	.00	.27	.67	.13	.82	.01	.64	.06	.00	.01																
Mean.....	101.11	577.50	5.70	.27	.72	.81	.19	.94	.06	.80	.14	.02	.05																
SD.....	40.06	305.11	1.77		.23		.07		.04	.07	.06	.01	.03																
Cluster 1:																													
Mann 2005.....	101	312	3.09	.15	.85	.72	.28	.82	.18	.70	.13	.03	.15																
Su 2011.....	91	338	3.71	.37	.63	.67	.43	.89	.11	.64	.25	.03	.08																
Average.....	96	325	3.40	.26	.74	.70	.30	.86	.14	.67	.19	.03	.11																
Cluster 2:																													
Thelen 2014.....	111	696	6.27	.19	.81	.75	.25	.95	.05	.74	.21	.01	.04																
Barkey 2008.....	101	301	2.98	.01	.99	.73	.27	.92	.08	.70	.22	.02	.06																
Garland 2010.....	88	491	5.58	.06	.94	.72	.28	.93	.07	.71	.22	.00	.06																
Average.....	100	496	4.94	.09	.91	.73	.27	.94	.06	.72	.22	.01	.05																
Cluster 3:																													
Mahoney 2001.....	78	485	6.22	.04	.96	.82	.18	.91	.09	.77	.14	.03	.06																
Gorski 2003.....	113	695	6.15	.31	.69	.84	.16	.90	.10	.81	.09	.03	.07																
Prasad 2012.....	69	450	6.52	.20	.80	.86	.14	.93	.07	.83	.10	.03	.05																
Average.....	86.67	543.33	6.30	.18	.82	.83	.17	.91	.09	.81	.11	.03	.06																

Cluster 4:													
Ertman 1997	147	762	5.18	.02	.98	.89	.11	.97	.03	.88	.09	.00	.03
Mann 2012	73	357	4.89	.00	1.00	.89	.11	.99	.01	.89	.10	.00	.01
He 2013	76	458	6.03	.09	.91	.86	.14	.96	.04	.85	.11	.00	.03
Marx 1998	99	969	9.79	.22	.78	.86	.14	.97	.03	.86	.11	.00	.03
Average	98.75	636.5	6.47	.08	.92	.87	.13	.97	.03	.87	.10	.00	.02
Cluster 5:													
Chibber 2003	57	249	4.37	.36	.64	.79	.21	.96	.04	.78	.18	.02	.02
Ermakoff 2008	90	339	3.77	.46	.54	.84	.16	.97	.03	.82	.15	.02	.01
Prasad 2006	179	952	5.32	.60	.40	.84	.16	.94	.06	.82	.12	.01	.04
Stemmetz 2007	214	1,495	6.99	.52	.48	.84	.15	.94	.06	.83	.11	.02	.04
Fox 2012	66	587	8.89	.73	.27	.87	.13	.93	.07	.87	.06	.00	.07
Walder 2009	67	459	6.85	.66	.34	.88	.12	.95	.05	.86	.08	.02	.03
Average	112.17	680.17	6.03	.56	.44	.84	.16	.95	.05	.83	.12	.02	.04

empirical use of primary source materials, from none at all (in multiple cases for books and articles) to well over half (73% for books in Fox [2012] and 77% for articles in Zaret [1996]).

We supplemented this first analytic step—which focused on rates of individual citation within particular works—with a synthetic second step designed to uncover differences and similarities among the different works. To pursue this approach, the inductive methods offered by cluster analysis were the most appropriate route to follow. This was, first, because cluster analysis has the ability to visualize the similarity and differences across multiple dimensions in our data in an intelligible way and, second, because cluster analysis allows us to produce and select appropriate clusters from those differences for further analysis. Following standards in the field (e.g., Everitt et al. 2011, esp. secs. 2.3.2 and 4.2), we first produced multidimensional scaling plots separately for books and articles (see figs. 1 and 2). These plots show the distances among (1) the rate of constructive and critical citation; (2) the rates of theoretical versus evidentiary citation; and (3) the rates of citing evidence which is primary. Consequently, members closer together on the plots are more similar along the five dimensions calculated than those farther apart.

Beyond simply plotting the similarity of the books and articles in our population in a low-dimensional space to make their underlying similarities easier to visualize, we also used a clustering algorithm to divide the members into relevant groups of citation patterns based on this data. There is little consensus on the reasons to prefer one algorithm over another (see, e.g., Rand 1971), but we chose Ward's (1963) agglomerative method. This was because our data fulfill its technical requirements (as we have ratio-rather than ordinal data) and because alternatives—centroid or medioid clustering, for example—tend to favor extreme outliers (in our case, Stryker 1996, Su 2011, and Mann 2005). The second dilemma in our clustering approach was to select an appropriate number of clusters. Again following “largely heuristic but intuitive procedures” (Hothorn and Everitt 2006, p. 247), we chose to minimize the within-cluster k -means sum of squares. For both books and articles, this suggested a five-cluster solution. These clusters are also reported in the Clust column of tables 3 and 4, and we remained sensitive to how well each member fit its cluster as we conducted further qualitative analyses.

Finally, mindful that analytic architectures behave as “generative mechanisms”—that is, that to observe and compare them is to gauge “the vicissitudes of [a] conceptual mechanism across differing concrete contexts, cases, and events” (Steinmetz 2004, p. 392)—we investigated whether common features were expressed across both books and articles. Comparing the characteristics of the individual clusters to one another, we aggregated them into four qualitatively distinct analytic architectures (table 5 and fig. 3). Two of these architectures did indeed find expression across both books

Analytic Architectures in Historical Sociology

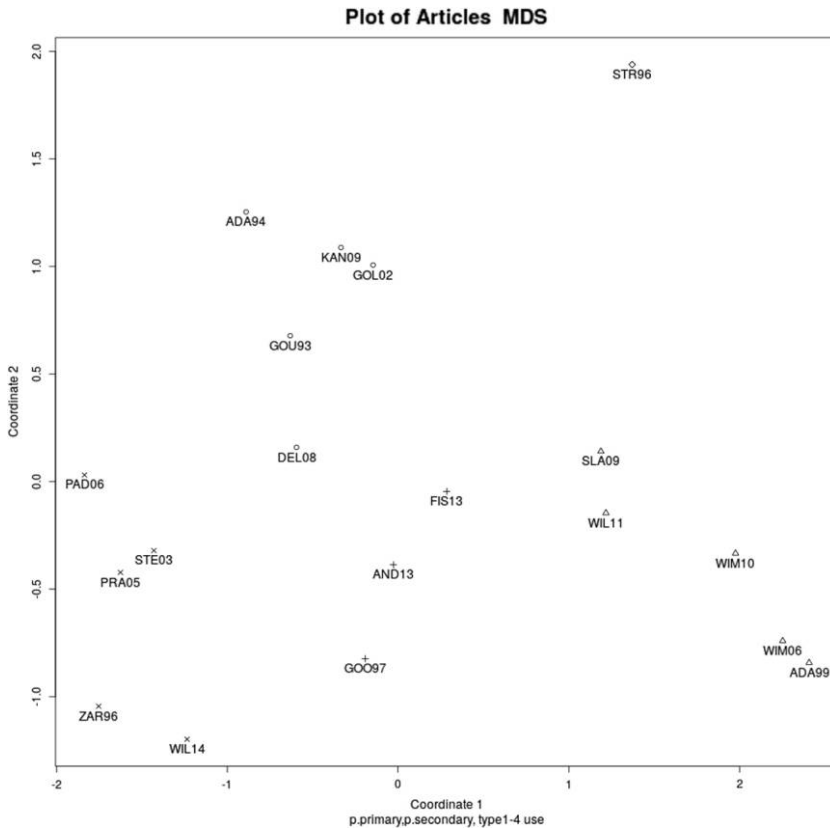


FIG. 1.—Scaled MDS plot of award-winning articles in historical and comparative sociology

and articles, while two others were more intimately tied to the formal demands of their particular genre. To better illustrate the central tendencies of each architecture, therefore, we selected a representative work (one with a low minimum squared distance from the average of all members in its architecture) and subjected it to more intensive qualitative analysis while also casting an eye to significant variations within each architecture.¹⁶

ANALYTIC ARCHITECTURES IN HISTORICAL SOCIOLOGY

Historical sociology has at least four distinct analytic architectures, each of which interfaces theory and evidence in characteristic ways. In this section,

¹⁶ For further details on our clustering and architecture composition procedures, see online app. A.

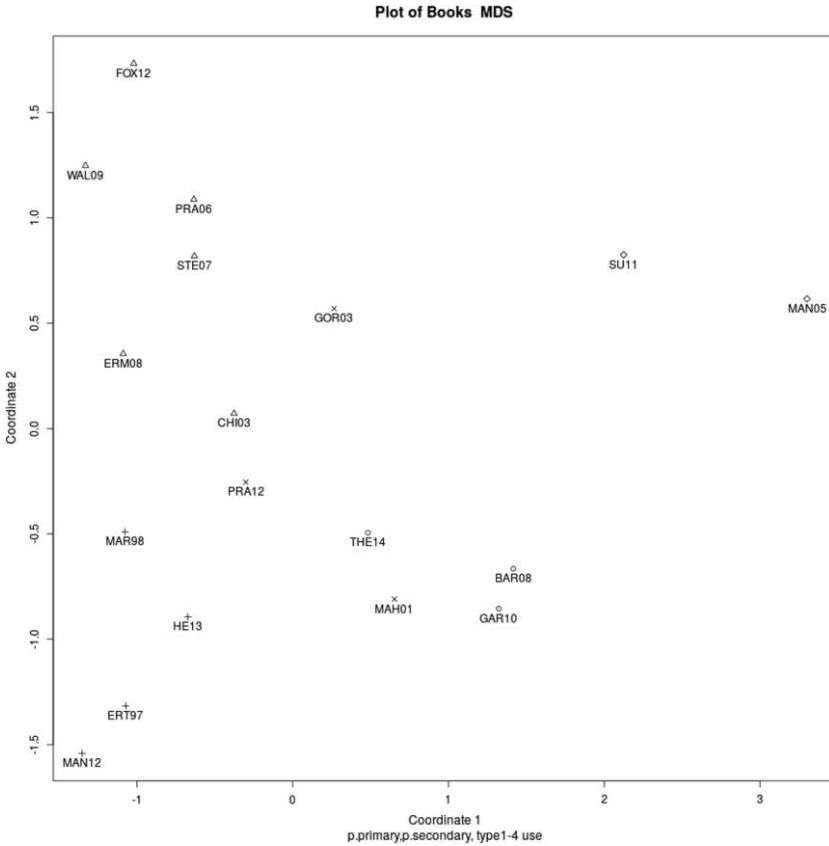


FIG. 2.—Scaled MDS plot of award-winning books in historical and comparative sociology

we draw upon both our quantitative analyses and a qualitative reading of the books and articles to describe how each architecture draws together its empirical and theoretical materials into a legible form.

Architecture 1: The Theoretical Frontier

A first analytic architecture, typical of four clusters (book 1 and 2 and article 1 and 4), is characterized by many theoretical citations. Over a quarter of all citations in book cluster 1 (29%) and book cluster 2 (27%) are theoretical, far higher than the rate of the next-closest book cluster; and the majority of citations in both article clusters (4, 78%; 1, 59%) are theoretical in character—rates that, again, far outpace the other article clusters. Much of this theoretical work is constructive; all of the articles included in the article clusters

Analytic Architectures in Historical Sociology

TABLE 5
RELATIVE EMPHASES OF BOOK AND ARTICLE CLUSTERS

RELATIVE CITATION OF PRIMARY MATERIALS	RELATIVE EMPHASIS IN CITATION USE	
	Empirical	Theoretical
High	Article cluster 2 (.667, .197)	Article cluster 5 (.416, .500)
High	Book cluster 5 (.555, .157)	. . .
High	Book cluster 1 (.258, .304)
Low	Article cluster 3 (.127, .267)	Article cluster 1 (.032, .595)
Low	Book cluster 3 (.182, .165)	Book cluster 2 (.088, .269)
Low	Book cluster 4 (.084, .126)	Article cluster 4 (.000, .784)

NOTE.—Of the paired numbers in each parenthesis, the first = the proportion of evidence cited that is primary; the second = the proportion of all citations that is theoretical.

and all of the books in the book clusters use above-average numbers of constructive-theoretical citations. At the same time, many of these clusters also use a large number of critical-theoretical citations—with book cluster 1 (11% of all citations) and book cluster 2 (5%) outpacing the other book clusters in terms of critical-theoretical citations, and article cluster 1 (15%) having the highest usage of such citations of any cluster. The exception is article cluster 4, where Stryker (1996) ends up in her own cluster largely because of her extremely low rates of critical citation (3%), despite her similarly theoretical and secondary-source-based structure. Finally, these clusters—especially the article clusters—tend to cite primary sources at relatively low rates (though Su [2011] is an important exception) and all but one of the works that uses no primary sources whatsoever falls in these three clusters. Taken together, these citation patterns suggest that these works are more focused than the other types on evaluating existing theories, and/or developing their own theoretical framework in constructive dialogue with existing concepts and theories.¹⁷ In recognition of this relative emphasis on theory, we term this analytic architecture the *theoretical frontier*.

In part because of their common theoretical emphasis, the works in the theoretical frontier architecture are distinctive in stylistic and methodological terms, and—to an extent—in the structure of the studies they conduct. Many of the works in the article 1 cluster (Adams 1999; Slater 2009; Wilson 2011), for example, have particularly high rates of critical-theoretical citations and very low rates of critical-empirical citations. They draw overwhelmingly on secondary sources, in an almost exclusively constructive fashion, to make their case. These articles focus on breaking down existing

¹⁷ While nearly all works of historical sociology are engaged in theory building and/or concept formation to some extent, these books and articles are particularly devoted to grappling with the theoretical and conceptual bases of existing sources.

Relative Emphases in Book and Article Clusters			
		Relative Emphasis in Citation Use	
		Empirical	Theoretical
Relative Citation of Primary Materials	High	Sociologist as Historian (Article 2 & 5; Book 5) <i>[Primary Citations Push Boundary Between History and Sociology]</i>	
	Low	Data-Driven Theorizing (Article 3) <i>[Empirical Citations Build Theories]</i>	Theoretical Frontier (Book 1 & 2; Article 1 & 4) <i>[Theory Citations Evaluate Existing Perspectives]</i>
		Macro-Causal Analysis (Book 3 & 4) <i>[Empirical & Constructive Citations Support Large-Scale Comparison]</i>	
"p." = proportion of evidence cited that is primary "t." = proportion of all citations that is theoretical			

FIG. 3.—Relative emphases of book and article clusters and associated analytic architectures.

theories in detail in order to build up new ones, drawing on concepts and theoretical insights from adjacent literatures and empirical material drawn primarily from existing secondary sources.

The theoretical frontier architecture also includes those articles with a distinctive methodological emphasis. These include both mainstream quantitative works (Wimmer and Min 2006; Wimmer and Feinstein 2010) as well as Stryker's (1996) meditation on narrative methods in historical sociology. None of these works uses any primary sources; their empirical material is drawn entirely from the existing secondary literature. The need to theorize methods and construct novel quantitative categories leads to particularly high rates of constructive-theoretical citations. Stryker (1996) is particularly unusual in her extremely high rate of constructive-theoretical citations (75% of all citations). Yet the quantitative methods pieces similarly devote extensive space to constructive-theoretical citations, while further

subjecting evidence to searching critical engagement (indeed, no works have proportionally more critical-empirical citations than these two articles).

Finally, the works in book cluster 1 and book cluster 2 (Barkey 2008; Garland 2010; Mann 2005; Su 2011; Thelen 2014) all represent attempts to theorize relatively overlooked, or insufficiently theorized, social facts—empire and diversity, capital punishment, genocide, collective killings, trajectories of liberalization. The goal of each of these works is to introduce a sociological perspective to these familiar and important topics, largely in dialogue with nonsociologists. To do this, the authors truck heavily in theoretical reconstruction, critically engaging with the theories and concepts of these alternative disciplines (or, in Garland’s case, “conventional wisdom”) and drawing from sociological theories and concepts to “sociologize” the phenomenon of interest. The majority of their theoretical citations are therefore constructive, indicating theory building. The source material used varies widely: while Su, Thelen, and Mann use a substantial number of primary sources in building their cases, Barkey and Garland rely overwhelmingly on existing secondary sources. As is also the case with the article clusters in this architecture, there is no overarching concern with exhaustively returning to the primary sources to nail down an empirical story here—indeed, compared to other award-winning books, the use of constructive-empirical citations in general is somewhat low in these clusters. This pragmatic approach to evidence differs sharply from other architectures, as we shall soon discover.

Although the works in this architecture share its central features with varying degrees of intensity, Dan Slater’s (2009) “Revolutions, Crackdowns, and Quiescence” illustrates its central tendencies. Through a comparative analysis of seven Southeast Asian countries, Slater argues that variations in counterauthoritarian mobilization (and movement success) are significantly influenced by the presence of autonomous communal elites who provide material resources for democratic mobilization as well as legitimacy for the movement’s emotive appeals. To motivate and substantiate this argument, Slater relies on extensive theoretical engagement and two layers of evidence. Nearly half (about 47%) of the article’s citations are theoretical, used primarily to justify focusing on the power of communal elites and communal identity in democratic mobilization. Unlike works employing the *data-driven theorizing* architecture (discussed below), Slater engages in extensive, and relatively systematic, engagement with the existing theoretical literature on democratic mobilization, which culminates in a virtually point-for-point constructive engagement with the literature on culture in politics (Slater 2009, pp. 214–24). The article’s evidence, meanwhile, is exclusively drawn from secondary sources. Slater mobilizes these sources first to illustrate his theory’s applicability to a broader selection of seven cases and then, “because of the difficulties of doing justice to so much political history in seven diverse cases in a

single article,” to conduct in-depth analyses of three “especially informative cases” through an exhaustive engagement with the secondary literature on each case (p. 228).¹⁸

Finally, the organization of the article shows the ongoing engagement with an existing body of theory characteristic of works in the theoretical frontier architecture. The theoretical discussion in the article is front-loaded and explicitly formulated as an extension and engagement with theories of democratic mobilization (hence narrowing the theoretical focus from grand theories of democratic transition to the specification of relevant mechanisms). Theory is then used to justify a focus on Southeast Asia as a place with sufficient variation to examine all relevant outcomes of democratic mobilization and to specifically justify the study of collective identity (and especially religion) among communal elites as a specific counter to the “shortcomings . . . in the democratization literature” (p. 214). Slater concludes by explicitly setting his model in dialogue with older theories of democratic mobilization (pp. 214–24). While he makes extensive references to empirical cases throughout the article and to the overall substantive finding (that autonomous communal elites make successful democratic mobilization more likely), it is only after Slater motivates the substantive theoretical focus on communal elites by a discussion of the shortcomings of existing literature that he presents a systematic empirical evaluation of its utility.

Architecture 2: The Sociologist as Historian

A second analytic architecture of historical-sociological scholarship, found in our analyses as article clusters 2 and 5 and book cluster 5, is characterized by extensive, detailed, primary research. This architecture is fundamentally distinguished by its source material, drawing heavily on original archival and other primary research. Sixty-seven percent of all empirical citations in article cluster 2, 42% of all empirical citations in article cluster 5, and 55% of all empirical citations in book cluster 2, are linked to primary materials. Indeed, the works in this cluster appear to come closest to answering the third wave call for more extensive engagement with primary sources in ways that blur the boundary between history and sociology. Accordingly, we refer to this analytic architecture as the *sociologist as historian*.

Within the sociologist as historian architecture, there is discernible variation in the format of books versus articles. Articles using this architecture tend to engage in detailed studies of a single case¹⁹—Florentine partnership

¹⁸ In the 16 pages containing Slater’s concrete discussion of his three cases (pp. 229–45), there are 77 secondary empirical citations.

¹⁹ An important exception is Fishman and Lizardo (2013), which is unique in that much of its primary evidence base consists of interviews. For the purposes of our coding,

networks, the English civil war, child-labor legislation in Germany, the Huk Rebellion in the Philippines, the Victorian pottery industry. These case studies typically rely upon exhaustive engagement with a relatively delimited set of traditional sources gathered through archival research—for instance, internal political documents and legislative drafts (Anderson 2013), petitions and manuscripts (Zaret 1996), denominational periodicals (Wilde and Danielsen 2014), or captured rebel documents (Goodwin 1997). Some further subject these primary sources to quantitative analysis (e.g., Steinberg 2003; Padgett and McLean 2006). This intensive, detailed historical depth largely precludes comparative studies in article format.

Books, on the other hand, tend to feature comparisons—French and German abdications, Indian and South Korean postwar development, or neoliberalism or colonial policy in multiple distinct settings—though some engage instead in deep synchronic accounts of specific phenomena, like the rise of the Chinese Red Guard or the American welfare state. Many books adopt a more omnivorous approach to primary materials, drawing upon a diverse mix of archival sources, memoirs, newspapers, interviews, and published primary documents whose persuasiveness derives as much from their breadth as their depth (e.g., Fox 2012; Prasad 2006; Steinmetz 2007). The longer format of books likewise permits the juxtaposition of several detailed case studies in a broader comparative analysis. In both cases, however, a richly detailed narrative sourced to primary documents dominates the ultimate presentation.

The characteristics of this architecture are well illustrated by Marc Steinberg's (2003) "Capitalist Development, the Labor Process, and the Law." Steinberg's goal is to demonstrate that the law, far from being simply a mystifying ideological support for capitalism, was actually an essential coercive tool used by capitalists to subordinate labor. To do so, he undertakes a detailed case study of the pottery industry in Victorian England that relies primarily upon primary sources: approximately 60% (207/343) of Steinberg's evidentiary citations are to primary sources. These sources are quite varied, spanning parliamentary papers, memoirs, newspapers, judicial reports, and a variety of sources gathered in British archives, including hiring agreements, arbitration reports, court registers, and organizational minute books. Newspapers, however, form the backbone of his empirical evidence, with over a quarter (123, or 23.6%) of all citations in the article (35.9% of all empirical citations) going to newspaper sources. These primary sources are mobilized to provide a vivid picture of the role of legal prosecution in shaping labor conditions in 19th-century Britain under the Masters and Servants Law.

however—and consistent with the treatment of oral histories by historians (Brown 2018)—these interviews were coded as "primary" evidence, thus placing Fishman and Lizardo's work firmly in the sociologist-as-historian cluster.

Further (and, notably, unlike most of the other works in this group), Steinberg combines several these sources into a database of labor prosecutions, which he then deploys throughout the article to provide a statistical portrait of the role of law in shaping the labor process in the pottery industry.

By contrast, secondary sources (largely books and articles by historians) are primarily used to provide context—on the history of the Masters and Servants Law, the operation of the British legal system, the organization of the pottery industry, and the nature of employment in 19th-century Britain, among others. Primary sources are woven throughout these more secondary-source-based discussions as well, most often to leaven them with qualitative detail. Yet for the most part, major empirical claims are not sourced to secondary works; instead, primary sources do the heavy lifting, both conceptually and descriptively, throughout the piece. Nearly every major theoretical and empirical claim—about the operation of the law, the volume and character of labor prosecutions, and so on—is sourced back to original primary documents wherever possible.

Finally, the structure of the text is also distinctive. Its body is devoted to the building of a positive empirical case, and, accordingly, the dominant mode of citation is constructive-empirical. Yet this detailed history is juxtaposed with a strong (and strongly differentiated) theoretical frame. Although these citations coexist with a reasonably high (ca. 25%) proportion of theoretical citations, there is a clear separation between theoretical and empirical citations sections of the article: the first 11 pages are devoted nearly entirely to theoretical engagement (both constructive and critical), as are the last six; while the bulk of the article (28 pages) is devoted to empirical citations. Its empirical portion, in particular, is nearly exclusively so: only one citation (of 334) in that part of the article is theoretical.²⁰ This structure bears a strong resemblance to work in history journals.

Architecture 3: Macrocausal Analysis

A third architecture, particular to books, and represented in book clusters 3 and 4, consist of works devoted to *macrocausal analysis*. These publications address large-scale questions in a comparative manner. What distinguishes them from works in the other architectures are their very high rates of empirical and constructive citations. Eighty-eight percent of all citations in book cluster 4, and 84% in book cluster 3, are empirical in character (with book cluster 4 having the highest percentage of empirical citations of any

²⁰ In the theoretical part of Steinberg's (2003) article, a similarly low 11 (of 112) citations are empirical—most of which serve to provide empirical backing for a theoretical point.

cluster, book or article); and 97% and 91%, respectively, of all citations are constructive in character—with book cluster 4, in particular, again having the highest percentage of constructive citations of any cluster. These books also tend to be very densely sourced—four of the six works have above-average per page citation rates for books. In method and narrative, then, these works prioritize the construction of a detailed and thorough evidentiary case upon which to base their conclusions.

This cluster incorporates two different styles of historical analysis. Some (Ertman 1997; Marx 1998; Mann 2012) are essentially classic macro-comparative syntheses, which draw on existing empirical research to build synthetic accounts of major social processes, whether state formation, racial formation, or social power. These syntheses spend little time critically engaging their sources, instead using sources as building blocks for new synthetic arguments leveraged through comparison. Others, by contrast (Mahoney 2001; Gorski 2003; Prasad 2012), might be better thought of as critical revisitations. These works undertake large-scale investigations of classic sociological questions—American exceptionalism, state formation, liberalism—with a critical eye to both the evidence and theory used in classic accounts. Critical citations, both empirical and theoretical, thus appear at elevated rates in these works, yet the bulk of each work is devoted to building an airtight empirical case for an alternative theory.

The source materials deployed in the course of analysis also vary substantially within this cluster. Some works (Ertman 1997; Mahoney 2001; Mann 2012) are based almost entirely upon secondary sources, while others (Marx 1998; Gorski 2003; Prasad 2012; He 2013) make substantial use of primary sources. When primary sources are used, however, it occurs in accordance with Skocpol's (1984) classic "targeted primary" strategy. The basic evidentiary framework on which these books rest is an extensive network of secondary citations, in which primary citations are embedded in a largely supporting role. Consequently, the overall per page rates and proportional use of primary citations for works in this architecture are lower than those of any work in the sociologist a historian architecture.

The features of this architecture are illustrated nicely by Monica Prasad's (2012) *The Land of Too Much*. Like the other books in this cluster, Prasad asks a broad question in macrocomparative terms: Why does the United States, compared with other advanced countries, have such a weak welfare state and such high poverty rates? Prasad argues that these features paradoxically spring from America's abundance and prosperity. The problems produced by wealth and abundance spurred an agrarian movement that advanced particular forms of taxation and government intervention. These policies in turn inhibited the development of a European-style welfare state and encouraged neoliberal policymaking and a dangerous overreliance on credit and financialization in the late 20th and early 21st century. In posing

and answering a large, classic sociological question with an equally sweeping argument, Prasad mirrors the other books in this cluster—Gorski's (2003) invocation of confessional conflict as a means of explaining variation in state formation, for example, or Marx's (1998) explanation of contrasting orders of racial domination as the outcome of elite conflicts among whites. Similarly, like the other works in this architecture, Prasad makes her case through periodic comparisons—in this case, with Europe and Canada.

Unlike some of the other works in this cluster (although like Gorski [2003] and Mahoney [2001]), the work engages in considerable criticism. *The Land of Too Much* is, in part, an answer to the classic sociological question about the roots of American exceptionalism. Prasad accordingly critically surveys earlier arguments in the second chapter of the book and periodically criticizes the empirical bases of those arguments in the course of making her own—as she does in critiquing one source for failing to account for important taxation legislation in the run-up to World War II (Prasad 2012, pp. 156–57). Yet these critical citations play a relatively minor role in the book as a whole. Instead, Prasad is primarily engaged in building a solid and detailed empirical case for her alternative argument. The book repeatedly delves into marvelous detail, the chapters peppered with illuminating quotes and apposite illustrations of crucial theoretical points, each lavishly sourced to empirical material. About five in six citations in the chapters we surveyed are constructive-empirical, about average for this architecture and considerably greater than the rate of constructive-empirical citations in other works. In fact, Prasad's constructive attention to detail in some chapters far exceeds this; in chapter 5, for instance, fully 98% of all citations are constructive-empirical in nature. The net result is that the reader is immersed in mountains of evidence as a means of constructing a convincing case for her argument.

Importantly, the material drawn upon as evidence is often from secondary sources. Prasad uses the “targeted-primary” approach advocated by Skocpol (1984), and therefore draws upon a wide variety of primary sources, including archival materials, congressional documents, statistical databases, and oral histories, but her main primary sources are newspaper articles. These primary sources tend to be concentrated in particular chapters. Thus, chapters on failed efforts at a national sales tax, and on the successful push for a national income tax, draw extensively upon national newspapers and the writings and speeches of Huey Long; meanwhile, a chapter on the democratization of credit draws heavily on archival materials. Yet these chapters are interspersed with others that build their case with hardly any recourse to primary materials (e.g., chapters on comparative welfare state development and adversarial regulation). Even the chapters that draw heavily upon primary materials embed them in a larger fabric of empirical citations to secondary sources—her chapter on Huey Long and the progressive income

tax, one of the most heavily primary-sourced chapters in the book, nevertheless relies on more secondary than primary sources for its empirical basis. In other words, primary sources are used prominently to provide rich detail and to nail down key theoretical points, but much of the narrative and constructive empirical work of the book is left to secondary materials. In these respects, Prasad's work is similar to other works in this cluster. Gorski (2003), for instance, builds his richly detailed argument through the skillful meshing of published primary and archival material with secondary sources. And Marx (1998) rests his argument primarily upon secondary sources, punctuated with primary data drawn from interviews, court decisions, and archival sources.

Architecture 4: Data-Driven Theorizing

A final architecture, represented by article cluster 3, might be called *data-driven theorizing*. In its emphasis on empirical citations, article cluster 3 resembles article cluster 2 in the sociologist as historian architecture. However, whereas article cluster 2 overwhelmingly cites primary citations (67%), article cluster 3 emphasizes secondary sources (12% overall) and is also somewhat more critical of its evidence than article cluster 2 (4% critical-empirical citations vs. 1%).²¹ The two clusters also differ in their approach to theory: works in article cluster 3 devote more time to constructive theory-building than those in article cluster 2. Twenty-one percent of citations in article cluster 3 are constructive and theoretical, compared with only 12% of citations in article cluster 2. At the same time, article cluster 2 engages critically with theory at nearly twice the rate (7% vs. 4%) as article cluster 3. Taken together, these clusters thus differ in both the raw materials emphasized and in how they treat them: article cluster 3 is deeply empirical (including a propensity for critical engagement with its sources) without being archival.

In other respects, article cluster 3 resembles the macrocausal analysis architecture's book cluster 3, in that both cite a good deal of empirical material constructively (72% vs. 80%) and use relatively little primary material compared to other clusters (12% and 18%, respectively). Yet the two clusters differ dramatically in their engagement with theory—although book cluster 3 and article cluster 3 criticize theories at almost exactly the same rate (4% and 5% respectively), article cluster 3 cites nearly twice as many constructive theoretical materials (21%) as book cluster 3 (11%). Although this difference may simply reflect the fact that longer books need fewer theoretical

²¹ The deployment of primary materials varies significantly within the data-driven theorizing architecture: de Leon (2008) uses comparatively extensive primary citations (fully one-third of all citations) while Goldstone (2002) cites no primary materials and Kane and Park (2009) very few (about 3% of all citations).

citations once they have done the work of establishing their architecture, the two clusters also seem to differ in the structure of their arguments. Whereas book cluster 3 is populated by macro comparative studies that theorize from multiple different cases, those in article cluster 3 tend to focus on a single case (often with express or implied “shadow” comparisons). Thus, for example, Adams (1994) marshals evidence from the early modern Netherlands to make broader arguments about the nature of state formation as a whole, while Kane and Park (2009) use the rise of Christianity in Korea (and its relative failure in China and Japan) to link nationalist rituals and religious diffusion. In short, these works devote more energy not only to constructing an empirical story, but also to engaging constructively with theory.

One reason for these differences seems to be that the theoretical interventions made by the members of article cluster 3 take place at two different levels of abstraction. For example, both Goldstone (2002 p. 334) and Gould (1993 p. 722) share a similar structure in their theoretical interventions. Each motivates their research by relatively quickly criticizing a particular received wisdom about their subject—for Goldstone, economic historians’ dichotomies of “‘Europe’ vs. ‘Asia’ and ‘Modern Economic Growth’ vs. ‘Stability’”; for Gould, historians’ “thesis of artisanal activism” during the 19th-century French commune. But both of these theoretical puzzles run alongside explicit engagement with more abstract and constructive theoretical interventions: for Goldstone, how best to envision change in economic history (as periodic yet contingent “efflorescences” catalyzing into self-sustaining growth in Europe); for Gould, the roots of social solidarity in political mobilization. In each case, therefore—and in contrast to the relatively more explicit theoretical evaluation and comparison of alternative explanations typical of article cluster 1—the theoretical interventions in architecture 4 (both specific and abstract) emerge out of engagement with the puzzles and issues presented by a single or limited number of cases.

Julia Adams’s (1994) article, “The Familial State,” exemplifies this data-driven theorizing architecture. Adams’s goal is to synthesize links among early modern state formation and patrimonialism through an intensive analysis of the Netherlands during the 17th and 18th centuries. Adams argues that a neo-Weberian theory of patrimonialism helps explain not only the particular puzzle of how elite Dutch families monopolized local political power, but also the larger theoretical question of how familial ties contributed to the rise and decline of the Dutch republic—a finding that poses a challenge to orthodox state formation theory.

Like others using this architecture, Adams relies on a set of evidence drawn mostly from secondary materials (about 91% of all citations) and relatively few primary materials (about 9% of all citations). In other words, the work of illustration is mostly done by a variety of secondary sources (both general and specific) in both English and Dutch. However, these works are

supplemented at key moments in the argument by primary materials. Thus, to demonstrate that the “politics of marriage” for elite families was a crucial aspect of political power in the Netherlands, Adams (1994, p. 509) not only quotes from Marshall’s (1987) *The Dutch Gentry, 1500–1650*, but also a variety of Dutch secondary sources and documents from the Amsterdam Gemeente Archief.

Theoretical citations, meanwhile, are overwhelmingly (18% of all citations) constructive; there are very few critical theoretical citations (only 6 of 188 total citations, or 3%). This near absence of theoretical criticism in favor of a focused development of a constructive theoretical and empirical base, operating on two levels simultaneously, is reflected in the structure of the article’s argument. It begins by motivating its analysis with a direct appeal to commonsense understandings of early modernity in Europe. It then shifts to justifying its substantive focus on patrimonial power and only then reembeds the theoretical example into a typology of forms of state power (Adams 1994, p. 514, fig. 1). It closes, finally, by circling back to its original question—how patrimonialism sheds light on both the rise and fall of the Netherlands as a world power—with occasional comparisons to other times and places (e.g., Britain). In contrast to works in the sociologist as historian architecture, Adams engages more extensively with theory, but rather than substantively engaging with a competing theory (as works in the theoretical frontier architecture tend to do) her engagement is more in the order of ground clearing. Thus, for example, Adams engages with Bourdieu’s “card-game-theoretic analogy” (Adams 1994, pp. 510–11) in order to show—via the archive of an elite Dutch family—that there is an “affectual, nonrational component” to family practices. The substantive content of large-scale “theoretical analyses of patrimonial systems” (p. 512), which would likely be central to an analysis conducted according to the theoretical frontier architecture, meanwhile, here is extremely subdued, consisting of a single reference to Perry Anderson’s *Lineages of the Absolutist State* (1974a) as a “striking example” of a “canonical theoretical text of early modern European state formation” (Adams 1994, p. 532n43). In sum, Adams’s analysis focuses on a single case and extended constructive theoretical and empirical engagement with a wide range of sources to synthesize a new theoretical perspective.

A TASTE FOR THE ARCHIVES? RECOGNITION OF ANALYTIC ARCHITECTURES OVER TIME

If contemporary historical sociology comprises four distinct analytic architectures, have the prominence and recognition accorded to each architecture varied over time? To examine this question, we looked at which architectures were recognized with awards in each year between 1995 and 2015. Because our sample size is small ($n = 37$), our findings are only suggestive,

but within this 20-year period, the “taste” for various architectures of historical sociology has indeed varied. Specifically, works adopting the sociologist as historian and the theoretical frontier architectures have been more likely to receive awards recently than they were prior to 2005.

Figure 4 shows the relative prominence of the architectures for all award-winning works of historical sociology broken into five-year periods.²² There is a clear surge in the number of awards given to works in the sociologist as historian category after 2005. Between 2005 and 2010, over 60% of all awards went to works in that architecture, and the increased recognition of works relying heavily on original primary research has continued (though to a lesser degree) since 2010. Similarly, the popularity of the theoretical frontier architecture has been growing over time, rising from 18% to 30% and then to 38% of all award winners across the periods. The gains in these two architectures have come at the expense of the macrocausal analysis and data-driven theorizing architectures, which together received over 75% of awards before 2005.

Figures 5 and 6 examine this trend among books and articles, respectively. Figure 5 shows that, whereas macrocausal analysis books took all of the awards prior to 2005, they took no awards at all between 2005 and 2010. Instead, over 70% of all award-winning books during that period adopted the sociologist as historian architecture. Although the macrocausal analysis has returned to fashion since 2011, it now shares space with the historian and theoretical frontier architectures, which together have won more book awards than the classic macrocausal architecture since that time. Figure 6 shows a similar story for articles. Since 2005, half of all award-winning articles have adopted the historian architecture, roughly double that of the pre-2005 era. Consequently, the data-driven theorizing architecture has seen a decline in prominence, from about 40% of all award winners before 2005 to around 18% since. Interestingly, however, the turn to the historian architecture appears to have endured better among articles; unlike with books, there has been little drop-off in the proportion of awards given to the historian architecture since 2010.

If the sociologist as historian architecture has reaped an increasing number of awards over the past decade, does that indicate a broader turn in practice toward primary sources? As shown in figure 7, the proportion of all empirical citations sourced to primary sources has been increasing for both books and articles over the past 20 years, but more strongly for books

²² Because book and article awards were not separated until 2003, we collapse the first two periods into a single block from 1995–2004. Each bar thus represents approximately equal numbers of books and articles (1995–2004, $n = 11$; 2005–10, $n = 13$; 2011–15, $n = 13$).

Analytic Architectures in Historical Sociology

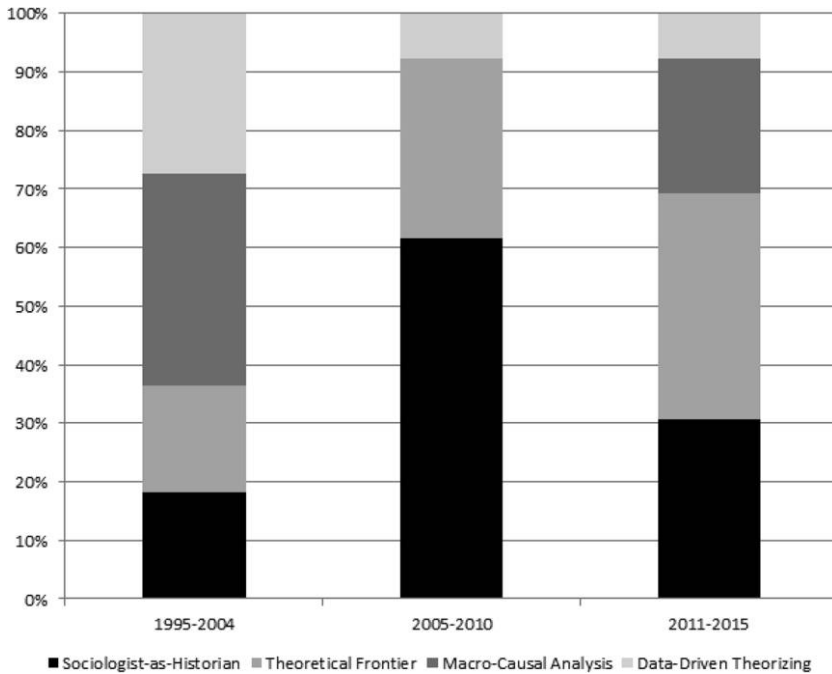


FIG. 4.—Prominence of architectures, books and articles combined, 1995–2015

than for articles.²³ Articles in the late 1990s cited primary sources approximately 25% of the time, and by the early 2010s that proportion had only increased to around 30%. Books, by contrast, saw their proportion of primary source material nearly double, from around one in six to one in three. By the late 2010s, books on average were citing more primary sources than articles. In practice, therefore, the evidentiary base of historical sociology appears to have shifted toward greater use of primary and archival sources, especially among award-winning books.²⁴

²³ As discussed above, we only coded “primary” and “secondary” percentages for empirical citations, and not for theoretical ones. Accordingly, all figures in this section refer to percentages and counts of empirical citations—i.e., those sources which are deployed as empirical evidence for an author’s case, or against a competing account.

²⁴ We suggest caution in interpreting these trend lines, however, as the flatness of the rates of primary citation for articles is being driven by two outliers: Zaret’s (1996) “Petitions and the ‘Invention’ of Public Opinion,” and Wilde and Danielsen’s (2014) “Fewer and Better Children”—which occur just after the beginning and just before the end of our period of analysis. Both belong to the sociologist as historian architecture, and both cite at least 10% more primary sources than any other article in the data set. If they are removed, the rate of change over time for articles becomes nearly identical to that of books (slopes of .010 and .008, respectively, as opposed to .002 for articles as shown). In the interest of a complete description of our population of analysis, however, we have included both outliers in our analysis.

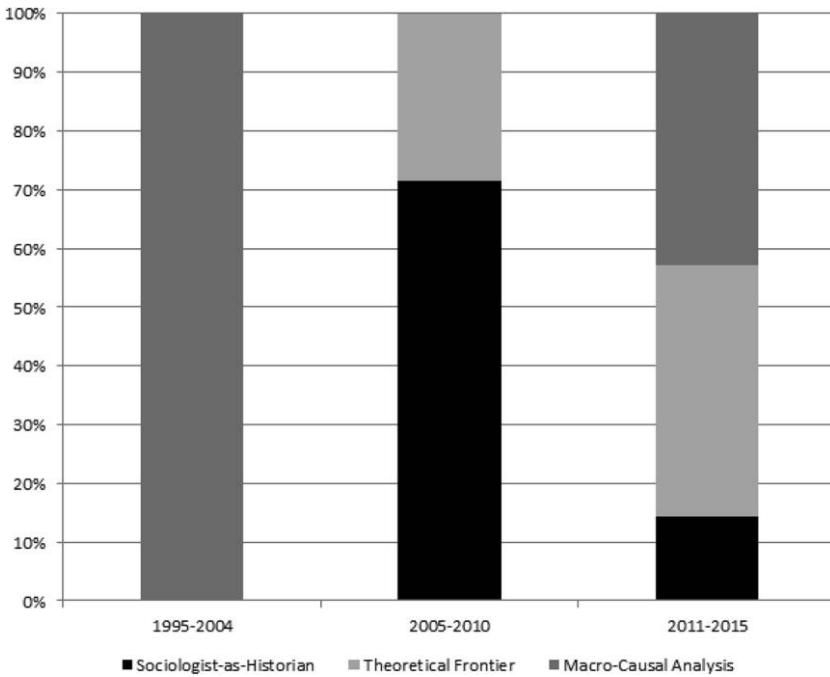


FIG. 5.—Prominence of architectures, books only, 1995–2015

Together, this evidence suggests a turn, since 2005, toward the recognition of works of historical sociology whose evidentiary base is more heavily grounded in primary empirical sources. This turn roughly coincides with the publication of the third wave landmark *Remaking Modernity* (Adams et al. 2005), which argued that historical sociologists were (or would be) increasingly turning to primary sources and archival methods. Our evidence suggests that this argument was influential, and that awards committees recognized works adopting the historian architecture disproportionately for several years. While this tendency has tempered somewhat over time, both the architecture and the stronger reliance on primary sources that lies at its core appear to have gained a strong foothold within the subdiscipline.

However, while these dynamics may reflect a broader shift in the subdisciplinary zeitgeist in favor of the historian architecture, they may also be an artifact of the composition of the awards committees. This is particularly so since, in the ASA-CHS section, the previous year’s award winner is often asked to serve on the next award committee—and thus might reproduce the recognition of a particular architecture over several years. To gain insight into such organizational dynamics, we examined the composition of

Analytic Architectures in Historical Sociology

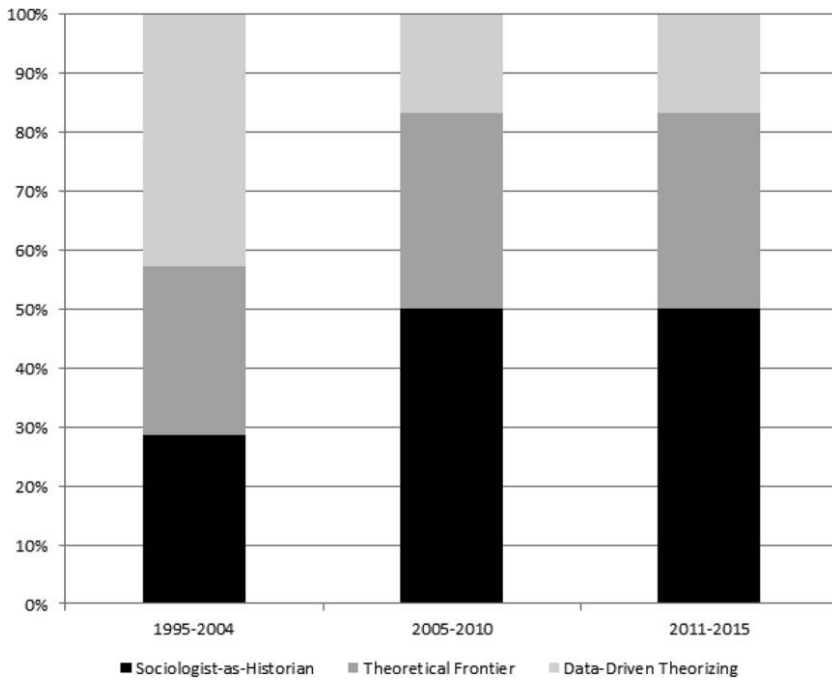


FIG. 6.—Prominence of architectures, articles only, 1995–2015

the ASA-CHS award committees from 2003 to 2015 (see online app. B).²⁵ The previous year’s award winner was asked to serve on the subsequent award committee 78% of the time. However, the same architecture only won the following year about 43% of the time. At the same time, years in which no member of the award committee had won the award the previous year occasionally coincided with “breaks” in the recognition of particular architectures. In 2011, a “fresh” Moore Book Award Committee broke a long run of sociologist as historian book winners, while in 2013 a “fresh” Tilly Article Award Committee inaugurated a new run of sociologist as historian article winners.

Because this discussion accounts for neither the architectural tastes of the other members of the committee nor the composition of the submissions, we should be cautious in drawing firm conclusions. But on balance, this evidence suggests that awards committees played at least a marginal role, either in elevating the prominence of the historian architecture, or in mediating a shift in the subdisciplinary zeitgeist. These findings would be consistent with

²⁵ We were unable to obtain information on the composition of award committees between 1995 and 2002.

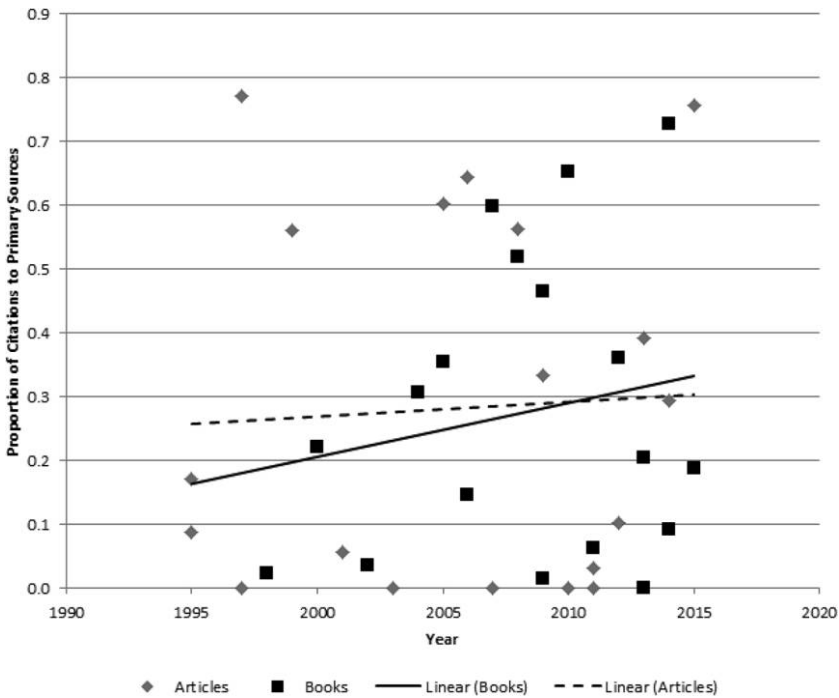


FIG. 7.—Trends in the relative use of primary and secondary evidence, 1995–2015

existing scholarship on consecration, which shows a tendency for prize committees to reproduce their own tastes (Bourdieu 1993; Lamont 2009; Rossman and Schilke 2014), and shows that changing patterns of consecration often depend upon the rise of an audience more receptive to a different style of work (Cattani, Ferriani, and Allison 2014). In either case, these findings suggest analytic architectures may be one important factor shaping the expression of elite tastes—and thereby a subtle influence on academic consecration.

In sum, both the kinds of architectures likely to be recognized and the sources used in historical sociology have changed over time. The impact of the third wave’s call for greater engagement with primary sources appears to have found purchase among both authors and—perhaps more strikingly—award committees. These findings suggest that examining analytic architectures can provide important insights into intellectual trends.

DISCUSSION AND CONCLUSION

In this article, we have argued that the selection of an *analytic architecture*—the links forged between theory and evidence in a way recognizable to an audience—is a consequential facet of scientific research. We used cluster

analysis and deep qualitative reading of texts to uncover at least four analytic architectures sheltering under the umbrella of “historical sociology.” Each architecture reflects choices about what kinds of evidence to present (and in what amounts), how and where (and how extensively) to engage with theories of various kinds, and how to enroll theory and evidence into a coherent presentation. We further suggested that perceptions of different architectures change over time. Among award-winning works of historical sociology, rewards have increasingly flowed toward more primary-sourced and theoretically synthetic scholarship since the early 2000s, consistent with the rising prestige of the sociologist as historian architecture at the expense of the data-driven theorizing architecture.

These findings resonate beyond historical sociology. At a basic level, they underscore the need to train a sociological lens on social knowledge production. While much recent scholarly energy has focused on ethnographic or contemporaneous accounts of “knowledge in the making” (Camic et al. 2011; cf. Pickering 1995; Peterson 2015), our findings demonstrate that published texts also reveal the production of social knowledge—particularly its “end stages” (Leahey 2008). Indeed, we show that these “end stages” are more complex than they first appear and that scholars would benefit from more closely examining how evidence is mobilized in written products (cf. Lieberson 1992; Leahey 2008) and placed in dialogue with theory (e.g., Abend 2006; Gross 2010; Lamont and Swidler 2014).

Analytic architectures also call attention to how scientific findings are transmitted and made legible to their audiences. Sociologists have neglected this question, either dismissing it as unproblematic or irrelevant (e.g., Watts 2014), or reducing it to a stylistic concern that can be addressed via linguistic strategies or rhetoric (e.g., Gans 1989; Green 1988; McCloskey 1990). By contrast, we argue that research findings neither “speak for themselves,” nor are purely a function of persuasive artifice. Instead, published work shows “dual institutionalization” (Abbott 2001, p. 126) in the world of scientific practices *and* in the world of rhetoric.

Because analytic architectures span the production and consumption of scholarship, questions of audience are essential to understanding them. As a cultural product, sociological knowledge is both produced and received (Griswold 1987), and dynamics of reception matter for how that knowledge is absorbed (or not) (e.g., Kuhn [1962] 1996; Green 1988; Fine 1994; Cattani et al. 2014; Lamont 2009). Because choices made in the end stages of social research are always made in (real or imagined) interaction, analytic architectures attune us to important roles that audiences can play in interpreting and evaluating social knowledge and the effects that the anticipation of those roles may have on the overall production of social knowledge.

For example, analytic architectures can reveal dynamics of scholarly identity. Architectures have some characteristics in common with cultural genres,

and thus may act as a signaling device for particular subdisciplinary identities (Clemens et al. 1995). Different analytic architectures give off different signals, thereby positioning a work in a community of scholars and subtly demonstrating affinities by evoking cognate works or paying tribute to inspirational studies. In historical sociology, for example, the choice of a macro-causal analysis architecture may evoke classic second wave titans like Skocpol (1979) or Moore (1966) who used that architecture to great effect. Similarly, where an analytic architecture echoes other disciplinary conventions (as with history in the sociologist as historian architecture²⁶), the choice of that architecture may help to signal the author's sympathies with an interdisciplinary approach or another discipline's standards of judgment (cf. Koppman and Leahey 2019). And scholars wishing to signal a more "scientific" approach to research may choose an architecture that gives pride of place to empirical evidence or that sharply separates empirical evidence from theoretical discussion—thereby allowing them to emphasize the depth of data collection or to act out the classic distinction between contexts of discovery and justification (Popper 2002).

As markers of scholarly identity, moreover, our over-time findings suggest that analytic architectures are also signals affecting how a work is received and judged by its intended audience. Indeed, analytic architectures are likely to be one potential (usually implicit) criterion of evaluation in a larger "heterarchical" evaluation process within most awards committees (Lamont 2012). If the content of a submission affects its likelihood of being consecrated (Rossman and Schilke 2014; Askin and Mauskapf 2017; Childress et al. 2017), analytic architectures may be one factor shaping the reception of academic texts by award committees. Evaluators tend to favor works by people within their professional networks and/or who share their "schools of thought" (Teplitzkiy et al. 2018), but this tendency is tempered by academics' investment in their identity as disinterested experts (Lamont 2009). Analytic architectures may allow evaluators to harmonize these two goals, since analytic architectures allow authors to present their findings in easily legible and legitimate terms that broadly demonstrate research skills while signaling their particular intellectual orientations.

Our findings also have implications for ongoing debates about methodological pluralism (e.g., King et al. 1994; Goertz and Mahoney 2012; Jerolmack and Khan 2014; Lamont and Swidler 2014; Porpora 2015; Aviles and Reed 2017). Though often treated as a single "method," historical sociology is deeply methodologically plural, with new methodological innovations being introduced on a regular basis (Goertz and Mahoney 2012; Lange 2013; Mahoney and Thelen 2015). Yet these innovations have been absorbed

²⁶ For more on the complex role of history as simultaneous ideal and foil in historical sociology, see Mayrl and Wilson (2016).

relatively smoothly. We suggest that this is because analytic architectures help manage this process of incorporation and hence sustain pluralism. Within historical sociology, the relationship between methodological choices and the architectures authors select is neither direct nor invariable; scholars using the comparative method, for instance, have published using all four architectures, while quantitative methods similarly can be found in both theoretical frontier and sociologist as historian architectures. By providing a set of familiar templates for interfacing theory and evidence, we suggest that methodological innovations can be presented in familiar forms, which facilitates their recognition as “historical sociology” even if their techniques are novel. Architectures thus allow scholars to speak across methodological difference to an audience capable of interpreting those forms (e.g., Lange 2013).

At the same time, to the extent that architectures play an important role in mediating difference, they suggest the terms of debates about pluralism may need to be reconfigured. Nearly all recent discussions of pluralism, pro and con, tend to cast the case for pluralism in terms of familiar, and unitary, methods camps—interviews, ethnographies, quantitative analyses, historical analyses, and so forth. We suggest that these familiar methods are overlaid with analytic architectures that may span some camps while dividing others. Going forward, therefore, both those who, like us, favor methodological pluralism and those who find it unnerving ought to look not just at diversity between major methods camps, but within them as well.

Finally, our method of using a small-*n*, substantive analysis of citation patterns as a window into the dynamics of social knowledge may prove beneficial to scholars interested in examining the relationship between cultural content and structural position in cultural fields. In contrast to existing large-scale studies, which tend to envision scientific practices as something like a “pure market” of possible ties (e.g., Moody 2004; Foster et al. 2015), our approach allows us to interrogate both organizational effects and the substance of scholarship as they relate to a work’s relative structural position. Other studies that seek to connect cultural content, structural inequalities, and organizational dynamics, across a variety of cultural fields may find these architectures useful.

Directions for Future Research

The concept of analytic architectures affords a variety of new avenues for research. One immediate extension is to other methods and disciplines.²⁷ Because of their essential role bridging scholar and audience, analytic architectures are likely a common sociological practice. Qualitative approaches such as

²⁷ We caution, however, that our focus on citations may not be appropriate for other sub-disciplines. Ethnographies, e.g., may make use of direct assertions such as “I observed . . .” while quantitative work may rely on explicating mathematics to provide evidence.

ethnography or interview methods, for instance, also need to navigate and integrate various kinds of evidence (observational, interview, content, etc.) with theory. Many qualitative studies begin with a “vignette,” while also citing a wide range of other evidence (some of it, increasingly, historical). What kinds of architectures make qualitative research like this legible to scholars? Similarly, quantitative methods are presented using techniques that exceed their explicitly stated statistical models (Breiger 2002; Leahey 2008), so they too have analytic architectures; indeed, quantitative analyses are well-known for their stark formal structure and ritualized methodological architectural procedures (e.g., the “standard research article” format). Even here, however, there may be alternatives, and there is certainly room to learn, through historical analyses, how the standard format came to be so institutionalized (Leahey 2005).

Additionally, ethnographic and interview-based research could examine how analytic architectures are selected and how presentational strategies are worked out in practice. Are architectures selected consciously and strategically, or are they learned and applied only subconsciously through trial, error, and the review process? Similarly, what kind of relationship obtains between the selection of an architecture and the use of particular methods during the data collection and analysis phases of research? Do certain architectures lend themselves to scholarship that uses certain procedures, or is it possible for an architecture to affect which procedures are used, and how?

Finally, historical analysis might trace the emergence of architectures. Where do they come from? Why does their prominence change? And do their meanings also shift over time? And if there are other architectures that never gained purchase, why did they fail? The results of such studies might provide insights into how pluralism is sustained in the social sciences more broadly. Our study clearly shows that the coexistence of multiple “excellent” architectures is a reality, and further research might examine the historical processes whereby this peaceful and productive coexistence came to be, and what sustains it.

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