

Appendix A: Further Methodological Details for Main Analyses for “What Do Historical Sociologists Do All Day?”

Prepared for *The American Journal of Sociology*

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This appendix supplements the description of our research procedure reported in “What Do Historical Sociologists Do All Day?” Our goal was to balance the analytic precision of quantitative techniques and the depth and texture of qualitative analysis. When applied to the appropriate units of analysis, quantitative methods can provide sterling insight into the internal structure of and relations among published academic scholarship. Yet when applied to entities like “meaning” (or, as in our case, constructs such as analytic architectures), when used mechanically or in isolation they can prove misleading in a variety of ways (see, e.g., Biernacki 2014). As outlined below, we therefore combined a variety of coding and clustering techniques, but also employed them with an eye towards satisfying our own expert judgment when it seemed necessary.

Our procedure consists of four main elements: (1) coding individual citations in a sample of award-winning books and articles; (2) composing those books and articles into distinct clusters; (3) combining those clusters into distinct methodological architectures; and (4) describing typical cases in each analytic architecture. This appendix provides more detail on each of these elements.

Coding Procedure for Citations

The fundamental unit of analysis in our coding was the individual in-text citation. In our original research design, we assigned each work in a given citation an exclusive category in an eight-fold scheme (whether it was critical/constructive, empirical/theoretical, and whether it was particular to the direct argument being made in the work or a more general reference to the intellectual milieu informing the research). However, the particular/general dimension proved difficult to assess reliably in our pretesting, and several categories, such as theoretically general critical citations (only 44 total, or 0.29% of the sample) and empirically general critical citations (only 77 total, or 0.5% of the sample), were vanishingly rare. Accordingly, we collapsed the general/particular dimension and continued coding with the four-fold scheme described in the manuscript. The frequencies of our original eight-fold coding scheme are reported in Table A1.

In assigning an exclusive valence to each work cited in the text, we were faced with two options: we could either pursue some version of a literal reading of the text (for example, seeking out positive or negative evaluative words and quantifying them relative to their proximity to a citation, as is the basis for large-scale text analysis and computational-linguistic approaches); or we could base our assignments on more contextual and holistic close readings of the citation's deployment over the course of the argument (as in, e.g., Hyland 1998). Given that both of us hold Ph.D.s in Sociology and are specialists in historical and comparative work, the latter of these two strategies seemed most profitable, since it would turn this expertise to our interpretive advantage, even as it introduced some inevitable bias towards "over-reading."

Our expert-coding approach came with some cost, however. Both because we collaborated in the development of our coding schema, and because we collaboratively test-coded that schema on a small set of initial texts, we were not independent coders, and hence could not be judged in terms of standard Inter-Coder Reliability tests, such as Cohen's kappa (Hallgren 2012).¹ Instead, we randomly assigned the original population of 32 books and articles to each coder.² Assuming that the underlying distribution of codes was even, we could therefore judge intercoder reliability by comparing the proportion of codes assigned by each coder. Table A2 reports these proportions, which, as might be expected, were almost identical for the most frequent citations in the sample (Uses 1 and 2) but varied somewhat among less frequent ones (Uses 3 and 4).

Beyond issues of intercoder reliability, assigning each citation to an exclusive category, while a central tenet of orthodox content analysis and coding approaches (Krippendorff 2002:132, 151), also introduces the problem of ambiguity or polysemous meaning in citations. Ambiguity is possible **at the level of the individual citation**; that is, it could be unclear whether a particular reference (Author 1900) is theoretical or empirical, or constructive or critical. In these instances, we assigned the **predominant** character or valence of a citation that was apparent to us. The overwhelming majority of citations were easily categorizable—Table A3 provides some examples—but to resolve any cases of difficult interpretation of assignment, we first reread the citing sentence carefully, then the paragraph in which the citation appeared, and then the section, in

¹ More precisely, the appropriate measure would have been a weighted Cohen's kappa, since there were far more 1 and 2 codes in our data than in 3 or 4 codes, which would have yielded an artificially high estimate of our inter-coder reliability had we used the unweighted calculation (Hallgren 2012:29).

² During revisions, we expanded our population to include awards given in 2014 and 2015. Mayrl coded these five works. In total, then, Mayrl coded 21 works, and Wilson coded 16 works.

each case to reinforce our close, holistic reading of the argument (hence employing a variety of “thematic coding” of the citation’s intended use; see Krippendorff 2002:107-110). In cases where the assignment was still unclear, we consulted with one another and debated the assignment until we were in consensus. (Specific examples of such ambiguous citations and our rationale are provided in Appendix C, examples 4a and 4b.)³

In our original research design, we also expected there to be considerable ambiguity at the level of the work being cited. In other words, we expected that the author of one of our works might cite Author 1900 in a variety of different ways over the course of the book or article. This might, for instance, reflect deep engagement with a “classic” work that spans both its theoretical and empirical arguments (that is, empirical-theoretical hybrid uses), or a nuanced reading that builds from some parts of an argument while criticizing or rejecting others (constructive-critical hybridity).

Table A4 reports the frequency that a given work had more than one code within a given piece of scholarship. As it shows, only 11.7% of the 6,994 unique items cited across our population were used in more than one way, with nearly half of those items (5% of all works) falling into only one of the six possible code combinations: works that were cited as both empirically and theoretically constructive. (By contrast, just over 6% of works cited were either theoretical or empirical constructive-critical hybrids.) Examples of hybrid citation patterns may be seen in Appendix C, examples 2a, 2b, and 2c. While this kind of multiple citation is an important phenomenon, and is something we will explore further in subsequent publications, it is also worth noting that nearly 9 of 10 works cited in our population were cited consistently.

Finally, we adopted straightforward rules for coding items into different types. The most important for the analysis was the distinction we drew between primary and secondary materials. Here, as noted in the main text, we adopted Bonnell’s (1980:158n.9) definition of a primary source as “any source that came into existence contemporaneously with the event or phenomenon under investigation” but added posterior memoirs and accounts by actors who participated in the event. To determine the meaning of “contemporaneous,” after reading each work we assigned a temporal period to its argument, and coded empirical citations to works within it as “primary.”⁴ We also included “published primary” materials—source books, collections of documents,

³ Another strategy for handling ambiguity in citations would be to allow coders to select more than one use per citation—thus giving them room to accommodate particularly polysemic references. This, however, would worsen the risk of “over-reading,” insofar as it might invite coders to undertake idiosyncratic interpretations and infer meanings that other readers might not see.

and so forth—as primary sources, but coded “primary quoted in secondary” materials—where an author cites the quotation of a primary source in a secondary work—as secondary. An example of this coding scheme at work can be found in Appendix C, examples 3a and 3b.⁵

Clustering Procedure and Goodness-of-Fit

One of the challenges of conducting cluster analyses is that there is little consensus on (1) the appropriate clustering algorithm to use, and (2) the best number of clusters to use in the analysis. For the first issue, we opted for a traditional “agglomerative” technique, Ward’s (1963) method, since it is recognized as a conservative starting point and there was no evident issue precluding its use. For the second issue, a standard technique is to examine the within-group-sum-of-squares (WGSS) for a number of different clusters, then select the number of clusters beyond which there is little decrease with the WGSS. We computed this for both books and articles (Figures A1 and A2) and selected five clusters for each.

While splitting books and articles apart facilitated our first round of cluster analysis, our concept of analytic architectures implies that they should generally be expressed *across* books and articles as well. That is, in spite of the formal differences between the citation patterns in books and articles,⁶ we expected that relative differences within books and articles might correlate to analytic architectures spanning both forms of publication. To capture this, and to assemble the analytic architectures as discussed in the main text, we therefore pooled the book and article clusters into a single figure along their main axes of variation (the mean proportion of theory cites for each book or article cluster, and the mean proportion of evidentiary cites which were primary; see Figure A3). Having closely read all the works in our population between us over the course of coding, we partitioned the graph by emphasizing three main qualitative distinctions: between those clusters with very high proportions of evidentiary cites which were primary and all other clusters (creating the Sociologist as Historian architecture); those books and articles that cited relatively high rates of theory (creating the

⁴ Thus, in Steinmetz’s *Devil’s Handwriting*, for instance, (at that time) scholarly ethnographies that Steinmetz uses as evidence of modern colonial principles of vision and division in the imperial ethnographic field were counted as “primary” sources if they fell within the period of his empirical argument and were deployed as evidence.

⁵ Note that example 3b also includes a citation to a single work that was coded as both primary and secondary according to how it was contextually mobilized.

⁶ Articles, as a whole, cite far lower proportions of evidence relative to theory than do books. As a result, cluster analyses run on the whole population generally saw this as the most important difference and accordingly partitioned books and articles in separate clusters.

Theoretical Frontier); and those books that cited relatively little primary-evidentiary or theoretical work (constituting the Macro-Causal Analysis architecture. That left the third article cluster, which, upon discussion, we decided to separate into a unique Analytic Architecture (that of Data-Driven Theorizing.) At first blush, 3A may seem to belong to the Theoretical Frontier, but we separated it because, in our judgment, the formal differences between books and articles trumped 3A's similarities to its neighbors 2B and 1B. 3A, after all, is the only article cluster with both few theoretical and primary-evidentiary citations, but it also has substantially more primary-evidentiary citations than the other two article clusters in the Theoretical Frontier. (And 2B and 1B both, in turn, have substantially more theoretical citations than their Macro-Causal Neighbors in 4B and 3B—the most of any book clusters, which is why we included them in the Theoretical Frontier.)

Selection of Representatives for Qualitative Analysis

Cluster analysis has a feature that is at once a benefit and drawback: clusters are composed of elements that share given characteristics *to varying degrees*. When transitioning from quantitative analysis to judgment of how those features manifest in concrete cases, two issues are thus especially important—the selection of “typical” cases to analyze in depth and how best to handle cases that may not wholly or neatly fit in clusters.

To pick appropriate representatives for each analytic architecture, we first computed the mean values for each architecture used to compute the individual clusters prior to composition into architectures (described in the previous section). Next, we summed the absolute distance of each measure for each constituent element in the analytic architecture. To select our representatives, we then sought the minimum distance of particular works from the mean for that architecture (these relationships are visually represented in Figure A4's stripchart.)

While two representatives—Steinberg's “Capitalist Development, the Labor Process, and the Law” (Sociologist as Historian) and Slater's “Revolutions, Crackdowns, and Quiescence” (Theoretical Frontier)—have the minimum distance from the means of their architectures, the other two—Prasad's *The Land of Too Much* (Macro-Causal Analysis) and Adams' “The Familial State” (Data-Driven Theorizing)—are the second-closest to their means. This is an artifact of the expansion of the population considered in the article, which we updated to include new awardees at the time of submitting the first revision of the manuscript. As a result of that expansion, in turn, while the

analytic architectures and their membership were robust, the addition of new members changed the internal composition of the architectures, thus making some exemplars still quite central to the architecture, but no longer the strict minimum summed distance from the mean.

In our qualitative analysis, we decided to tolerate this change, and leave our exemplars as they originally were, for two reasons. First, although there are some subtle differences, He's *Modern Fiscal State* and Gould's "Trade Cohesion and Class Unity" bear such striking qualitative resemblances at the level of their expression of their respective analytic architectures to the works already analyzed that substituting them seemed unnecessary. Second, the Data-Driven Theorizing and Macro-Causal Analysis architectures were also the most internally cohesive. This can be seen in Table A5, which presents the mean and standard deviation for the summed absolute distance from the architecture's mean for each constituent of a cluster. Read in conjunction with Figure A4, this clearly shows two outliers across the Theoretical Frontier and Sociologist-as-Historian architectures: Stryker's "Beyond History versus Theory" and Fishman and Lizardo's "How Macro-Historical Change Shapes Cultural Taste." On the one hand, we take the fact that these works are outliers to be a validation of the underlying integrity of the clusters, as each departs significantly from their companions in the architecture or the population as a whole. (Fishman and Lizardo, are among the few to rely on interviews for their primary data, and mix theory, evidence, and constructive and critical valences in unusual proportions; Stryker cites no primary evidence, is mainly theoretical in her citations, and cites in an almost-completely constructive valence.) On the other hand, these outliers also serve as a note of caution—as the summed distance from the mean of an architecture increases, so too does the likelihood that any given constituent represents a blend of different architectures, or perhaps even a *sui generis* effort to forge a new one!⁷

⁷ This drawback to using cluster analysis has another facet: composing clusters with over-time data fails to adequately represent the emergence, institutionalization, and even dissolution of architectures. We attempted to deal with this challenge in various ways in the paper, but the outliers for different architectures present a fascinating avenue for further research. Are they harbingers of the mutation of different architectures, or will they be misinterpreted by audiences because they don't adequately map on to any one architecture?

References

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Tables and Figures

Table A1. Frequencies of Citation in Original Eight-Fold Coding Scheme

		Valence				Total
		Constructive		Critical		
		Empirical	Theoretical	Empirical	Theoretical	
Analytic Level	Particular	10504 (.90; .69)	1441 (.58; .09)	197 (.72; .01)	781 (.95; .05)	12923
	General	1160 (.10; .08)	1052 (.42; .07)	77 (.18; .005)	44 (.05; .003)	2333
Total		11664	2493	274	825	15256

Note: First rounded proportion in each cell is for column total, the second is for the overall citation sample.

Table A2. Proportions of Codes Assigned by Each Coder

		Code Assigned			
		1	2	3	4
Coder	Author 1	0.762	0.164	0.010	0.064
	Author 2	0.769	0.162	0.019	0.047

Table A3. Exemplary Codes

<u>Code</u>	<u>Example</u>
(1) Constructive- Empirical	“Ongoing updating and the need for broader and more theoretical skill profiles resulted in an increase in the average length of training from under thirty-four months in the mid-1980s to close to thirty-seven months since 1990” (Thelen 2014:87)
(2) Constructive- Theoretical	“[T]he analysis that follows shows the usefulness of recent attention in historical sociology to the importance of specific <i>transformative events</i> in structural change: social change is nonlinear because a specific event can quickly change opinions, reveal or highlight elements of a phenomenon, spread information or misinformation, and transform how characters conceive of their own goals” (Prasad 2006:37)
(3) Critical- Empirical	“It is sometimes claimed that the Pietist movement injected a new ethos into the Prussian state, and, more concretely, that Lutheran Pietists were heavily represented within the royal service. I have not found any evidence to support this view” (Gorski 2003:112)
(4) Critical- Theoretical	“[N]either Gould’s nor Poggi’s otherwise excellent books recognizes that these bodies were shaped and honeycombed by family cliques, and that the ‘traditional’ rights and values on which they were based were patriarchal and familial” (Adams 1994:527n4)

Table A4. Hybrid Citations

		Code		
		Cr-Em	Co-Th	Cr-Th
Code	Co-Em	352 (0.050)	84 (0.012)	147 (0.021)
	Cr-Em	X	43 (0.006)	153 (0.022)
	Co-Th	X	X	41 (0.006)
Note: Cells report frequencies of citations across all works that are coded at least once by both codes within a single work. Parentheses report the proportion for the 6,994 unique citations in the population.				

Table A5. Mean and Standard Deviations for Analytic Architecture Constituents to Mean of Architectures

Analytic Architecture	Mean*	Standard Deviation
Theoretical Frontier	.32	.12
Sociologist as Historian	.25	.14
Macro-Causal Analysis	.15	.06
Data-Driven Theorizing	.17	.08
* Of group for summed absolute value of distance on measures of each element to architecture's mean		

Figures

Figure A1. Within-Group Sum of Squared Variance for Books

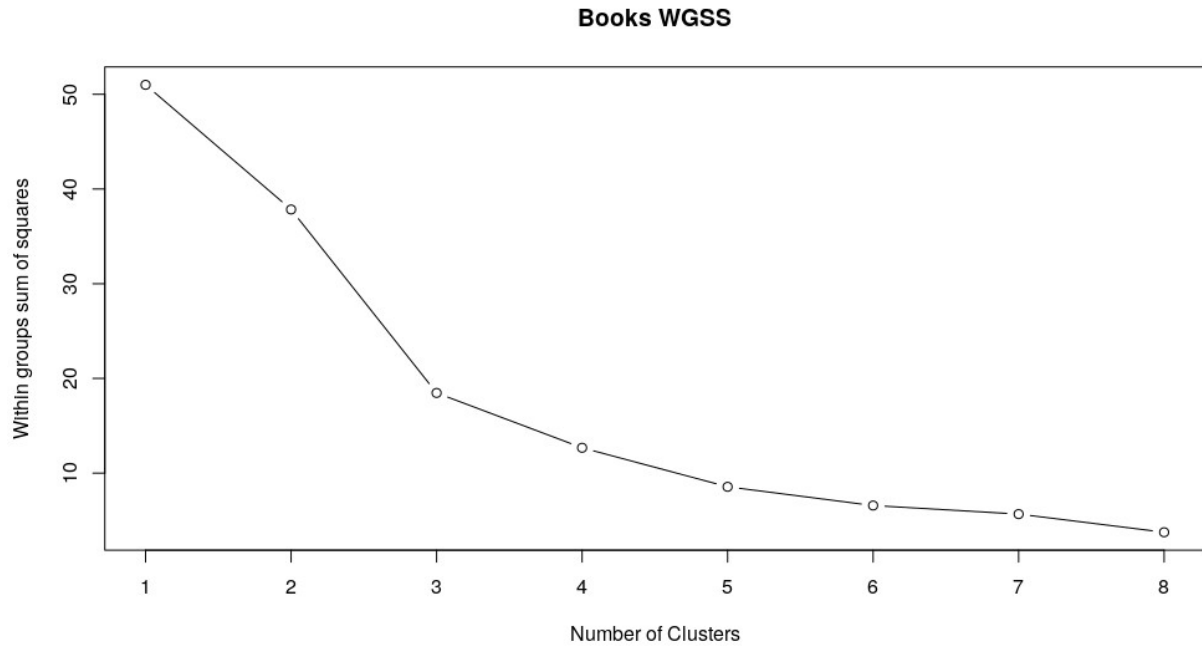


Figure A2. Within-Group Sum of Squared Variance for Articles

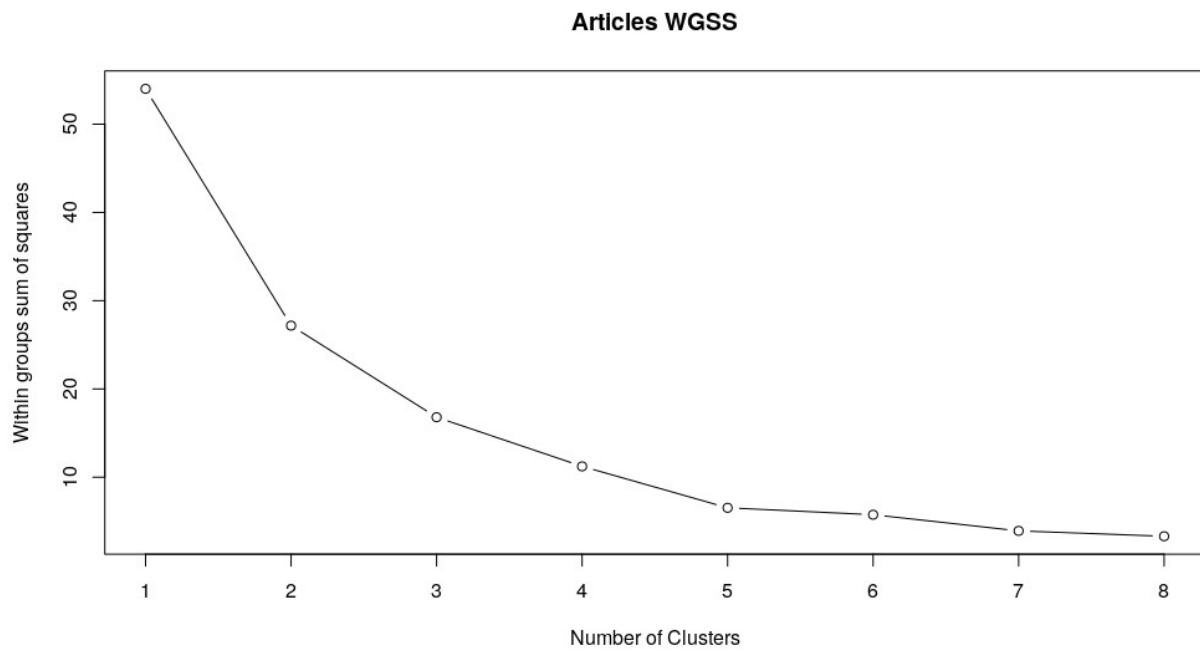


Figure A3. Cluster Plot

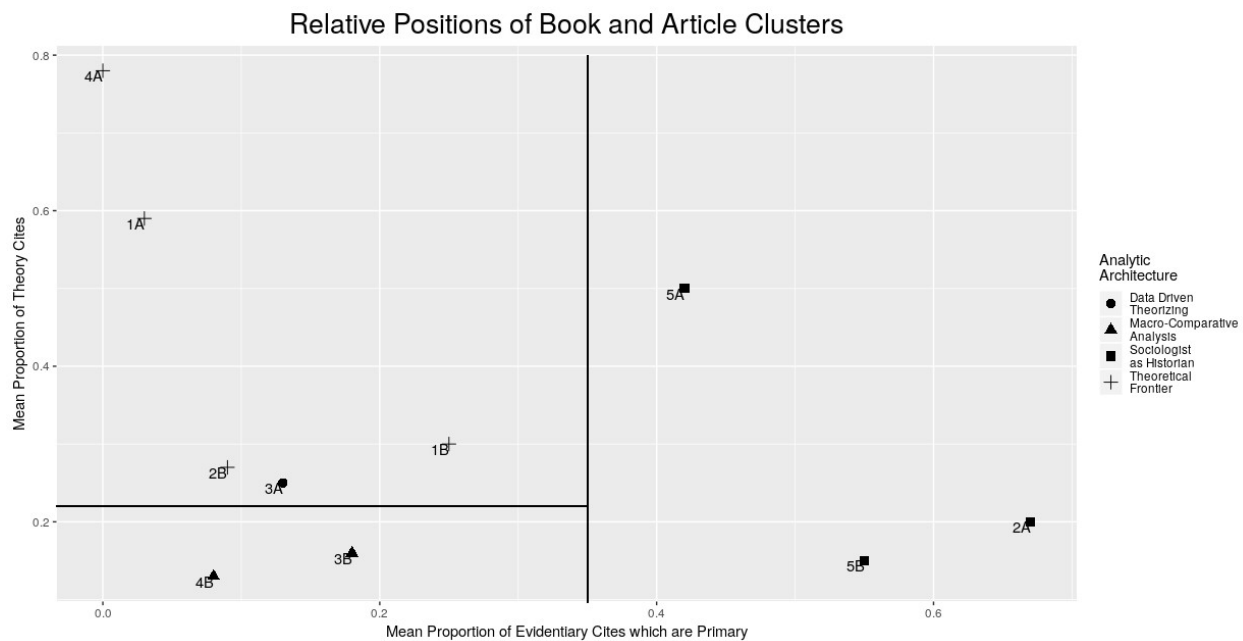
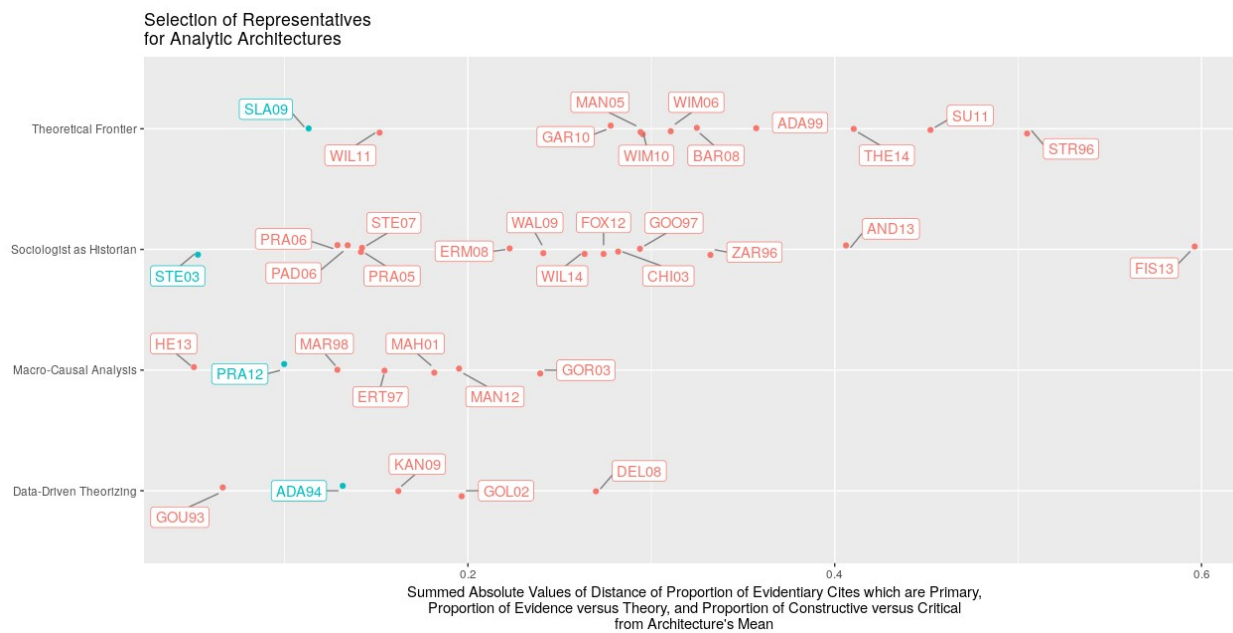


Figure A4. Relative Positions of Representatives for Each Architecture



Appendix B: Supplemental Analyses of Composition of ASA-CHS Award Committees

One potential explanation for why tastes in architectures might fluctuate over time is that the winners of an award are often asked to serve on the same committee the following year. Thus, cycles of architectural dominance might be a result of scholars who favor a particular architecture winning the award, and then imposing their same principles of “vision and division” on the subsequent set of submissions in a way that leads an architecture to repeat.

To evaluate the extent to which the popularity of an architecture might be a function of the composition of the award committees, we gathered data on the composition of both the Moore and Tilly award committees. Our first source of data were section newsletters, posted on the [ASA-CHS section website](#). In many cases, the newsletters contained calls for nominations which included the award committee members’ names; in others, the committee members’ names were provided in announcements of the awards themselves. We supplemented this information with information gleaned from our personal archives of letters from the section chair, which often included calls for award nominations; this allowed us to get complete information for the year 1995 and the years 2004-2014 inclusive for both awards, with the exception of 2008.

We next approached the chairs and secretary/treasurers for the section for the years in which we were missing data. The 2014-15 section chair was able to provide us with the composition of the awards committees in 2015; and the section secretary from 2003-05 was able to provide us information about the composition of the awards committees in 2003. Unfortunately, the section officers prior to 2003 did not have records or recollections of the membership of the awards committees during this time; and the section’s business archive, we learned, was literally lost in the mail in its transition from one secretary to another in 2011. We were thus unable to identify the composition of the section award committees from 1997-2002.

This left us with 2008. Unfortunately, the 2007-08 section chair could not recall or retrieve any information about the committees during that year, so we turned to the previous year’s award-winners (Monica Prasad, Andreas Wimmer, and Brian Min) to see whether they had served in 2008. Prasad was able to unearth an email with the complete composition of the Moore Award committee in 2008, while Wimmer and Min confirmed that they were not part of the 2008 Tilly Award committee without providing the actual committee members. Nevertheless, this allowed us to get a complete picture

of which previous award-winners served on the following year's committees between 2003 and 2015.

As can be seen in Tables B1 and B2, there is no immediate relationship between the previous year's winner serving on a committee and the same architecture winning. Whereas the previous year's winner served on the committee 78% of the time (18/23 years), the same architecture "repeated" only 43% of the time (10/23 times). However, when architectures did repeat, the previous year's winner was usually on the committee (true in 9/10 instances). Similarly, years when there was no previous winner on the award committee often coincided with a shift to a new architectural "run," as with articles in 2013, and with books in 2011 and 2013. Again, however, there were years when the previous winner did not serve and an architecture repeated anyway (as with books in 2012).

Together, these analyses suggest that—while not determinative—the composition of awards committees plays an important role in mediating shifts in which architectures are rewarded.

Table B1. Award Committee Information for Barrington Moore Award

Year	Winning Architecture	Previous Year's Winning Architecture	Same Architecture as Previous Year?	Previous Year's Winner on Award Committee?
2003	DDT (Goldstone 2002)	MCA	No	Yes
2004	MCA (Gorski 2003)	DDT	No	Yes
2005	SAH (Chibber 2003)	MCA	No	Yes
2006	TF (Mann 2005)	SAH	No	Yes
2007	SAH (Prasad 2006)	TF	No	Yes
2008	SAH (Steinmetz 2007)	SAH	Yes	Yes
2009	SAH (Ermakoff 2008), TF (Barkey 2008)	SAH	Yes	Yes
2010	SAH (Walder 2009)	SAH, TF	Yes	Yes (Both)
2011	TF (Garland 2010)	SAH	No	No
2012	TF (Su 2011)	TF	Yes	No
2013	MCA (Mann 2012; Prasad 2012)	TF	No	No
2014	MCA (He 2013), SAH (Fox 2012)	MCA	Yes	Yes (Prasad)
2015	TF (Thelen 2014)	MCA	No	Yes (Both)
TOTAL			5/13	10/13

KEY: DDT = Data-Driven Theorizing; MCA = Macro-Causal Analysis; SAH = Sociologist-as-Historian; TF = Theoretical Frontier

Table B2. Award Committee Information for Charles Tilly Award

Year	Winning Architecture	Previous Year's Winning Architecture	Same Architecture as Previous Year?	Previous Year's Winner on Award Committee?
2005	SAH (Steinberg 2003)	N/A ¹	N/A ¹	N/A ¹
2006	SAH (Prasad 2005)	SAH	Yes	Yes
2007	TF (Wimmer/Min 2006)	SAH	No	Yes
2008	SAH (Padgett/McLean 2006)	TF	No	No ²
2009	DDT (de Leon 2008)	SAH	No	Yes
2010	TF (Slater 2009)	DDT	No	Yes
2011	DDT (Kane/Park 2009), TF (Wimmer/Feinstein 2010)	TF	Yes	Yes
2012	TF (Wilson 2011)	DDT, TF	Yes	Yes (Kane) ³
2013	SAH (Anderson 2013)	TF	No	No
2014	SAH (Fishman/Lizardo 2013)	SAH	Yes	Yes
2015	SAH(Wilde/Danielsen 2014)	SAH	Yes	Yes
TOTAL			5/10	8/10

KEY: DDT = Data-Driven Theorizing; MCA = Macro-Causal Analysis; SAH = Sociologist-as-Historian; TF = Theoretical Frontier

NOTES: (1) The Tilly Award was introduced in 2005, so there were no previous winners to cite. (The previous year's winner of the Moore Award was Gorski 2003, a macro-causal analysis book.); (2) We were unable to determine the composition of the entire committee for 2008, but neither of the previous year's winners reported serving on it; (3) The repeat architecture (TF) was not the same architecture as the one the committee member won for the previous year (DDT).

Appendix C: Coding Examples

EMPIRICAL VS. THEORETICAL CITATIONS

Example 1a: Passage is from Philip S. Gorski's *The Disciplinary Revolution* (2003:40):

"...The standard theory of state-formation (that is, the bellicist model) implies that the Dutch state should have been weak. But in terms of the usual indicators (namely, external power and internal order)—the very indicators the bellicists prefer—it seems to have been quite strong. How was this possible?

One potential answer is wealth. This is the answer advanced by world systems theory and also by some historical institutionalists, and it is not without merit. [fn3: Here I am thinking of Charles Tilly, *Coercion, Capital, and European States*; Brian Downing, *The Military Revolution and Political Change*; and Bruce Carruthers, *City of Capital*, chap. 4.] After all, most economic historians would probably agree that the Northern Netherlands were the most developed country in Europe during the seventeenth century, and it seems that they also remained the most prosperous during the next century, even after losing their hegemonic status in the world economy to Great Britain. [fn4: See, especially, Fernand Braudel, *Civilization and Capitalism*, chap. 3; Jonathan Israel, *Dutch Primacy in World Trade*; and Jan de Vries and Ad van der Woude, *The First Modern Economy*.] And most historical sociologists would agree that material resources are one of the key ingredients of state power. If war is the ultimate test of a state's strength, and money is the sinews of war, then state capacity will be at least partly a function of monetary resources. Still, this answer is far from complete. In particular, it does not explain why the Dutch state was able to maintain such a high degree of social order—or even why it was able to extract such high levels of material resources. A strong economy may be one potential source of strength, but it is by no means the only possible one."

There are six citations in this paragraph (underlined to make them easier to see). The first three are to established works of state theory espousing the "bellicist" model Gorski criticizes in this passage as "far from complete." The last three are to histories demonstrating the prosperity of the Netherlands during the seventeenth century. We coded the first three citations as critical-theoretical and the latter three as constructive-empirical, since the former are being engaged with for their theoretical claims, while the latter are being engaged with for their empirical findings.

Example 1b: Passage is from David Zaret's "Petitions and the 'Invention' of Public Opinion in the English Revolution" (1996:1501-02)

"This critique of earlier work requires two caveats. First, criticism of speculative, inconsistent references to capitalism and Protestantism as causes of the public sphere does not imply their utter irrelevance. I suggest that their causal relevance be explored at the level of communicative practice—printing is a preeminent instance of early capitalist enterprise (Eisenstein 1980). Second, sociologists have not completely ignored communicative issues. Habermas (1989, pp. 16, 24) notes the importance of printing for 'the new domain of a public sphere whose decisive mark was the published word.' Bendix devotes more attention to this point in remarks on 'intellectual mobilization' (1978, pp. 256-58, 261-67; see also Calhoun 1988). Mayhew (1984, pp. 1285-87) and Wuthnow (1989, pp. 201-11) refer to the new vocation of publicist created by pamphleteering. Yet these remarks treat communicative developments in the printing revolution too narrowly, as a factor that facilitates change by disseminating new ideas more rapidly and to a broader audience. As Eisenstein has shown in her analysis of the impact of early modern printing on learned culture (1980, pp. 691-92; see also Chartier 1987; Darnton 1979), novelty in the mode of communication can have intimate links with novel ideas. The cultural impact of printing goes beyond issues of access and distribution. The printing of petitions as propaganda not only increased the scope of communication but also created novel practices that simultaneously constitute and invoke the authority of public opinion."

This is the concluding paragraph for a subsection of the paper's introduction criticizing existing theories tying the rise of the public sphere to the emergence of capitalism and/or the rise of Protestantism. In it, Zaret engages with aspects of existing scholarship that have incompletely addressed its sources, or overlooked important theoretical aspects. We thus coded all of the citations in this paragraph as theoretical. The citations to Eisenstein 1980, Chartier 1987, and Darnton 1979, which advance a broader approach to the relationship between technology and culture that Zaret builds on, are coded constructive-theoretical; the citations to the other scholars are coded critical-theoretical—even though they "have not completely ignored communicative issues," they still "treat communicative developments in the printing revolution too narrowly"—a theoretical critique that is primary in the context of this section of the paper.

Example 1c: Passage is from Padgett and McLean's "Organizational Invention and Elite Transformation" (2006:1481):

"SOURCE.—(1) 1348-99 annual censuses of cambio banking partnerships: ASF, *Arte del Cambio* 14. (2) 1348-58 companies with whom Alberti *nuovi* did business: Goldthwaite, Settesoldi, and Spallanzani (1995). (3) 1369 companies that shipped goods through the port of Pisa: Silva (1908), Peruzzi (1868, pp. 219-22). (4) 1385-99 companies with whom Datini did business: Melis (1962, tables 27, 28, 31, 32, 35, 36, 39, 40). (Through double-checking the partnerships reported in these tables with dates of the business letters cited by Melis in his extensive footnotes, we eliminated the listed companies that did not operate in the 1385-99 period.) (5) 1353 census of active wool manufacturers: ASF, *Arte della Lana* 20. (6) 1382 census of wool-manufacturing companies: ASF, *Arte della Lana* 46. (7) Catasto: ASF, *catasto* 64-85."

This passage represents notes on sources for the construction of Table 1, which shows the "Number of Partnerships or Industries in Which Florentine Businessmen Participated." Because all of these citations are to empirical evidence used to build the overall argument, each of these eight sources is cited independently as a constructive-empirical citation. This table is subsequently listed as a source for Tables 5 (p. 1503), 6 (p. 1504), 7 (pp. 1511-12), 8 (pp. 1513-14), and 9 (pp. 1515-16). In each of those instances, the citation back to Table 1 is coded once as a constructive-empirical citation, per our general guidelines for handling multiple tables drawing on the same dataset.

CONSTRUCTIVE VS. CRITICAL CITATIONS

Example 2a: Passage is from Monica Prasad's *The Politics of Free Markets* (2006:20-21):

“One of the most sophisticated recent exponents of the argument that ideas are important in politics is Mark Blyth (2002), who argues that the rise and fall of Keynesianism in both the United States and Sweden is best explained through an examination of economic ideas. But Blyth’s own evidence does not fit his thesis. The U.S. case, with its adoption of Keynesianism in the 1920s and 1930s, and its rejection in the 1970s, provides evidence that the particular ideas won because they provided political benefits to politicians who adopted them. For example, Blyth notes that Hoover called a conference of economists to suggest policies to deal with the Depression, and that the conference produced *laissez-faire* ideas. But as Blyth writes, ‘[G]iven that the depression showed no signs of curing itself, and the idea of the depression as a therapeutic smacked of being patronizing, the deployment of such *laissez-faire* economic ideas confined American academic economists to the margins of debate and influence for most of the 1920s and 1930s. Given the *practical inadequacy* of such ideas, Hoover *sought a rationale for a more active policy*’ (52; emphasis added). And, as chapter 1 shows in greater detail, the *ideas* of supply-side economics never convinced more than a handful of people in the 1970s and early 1980s in the United States; the real question is why this small number of people acquired such disproportionate power. This is an argument closer to the structural argument of Skocpol and Weir’s (1985) analysis, which found that particular structures were more or less conducive to experimenting with Keynesian policies during the Great Depression—that is, that structures determine whether ideas will be successful. Blyth writes, ‘[R]emove the ideas of business from the practices of the Conservatives in the 1990s, and the policy responses...make no sense’ (261). But this is too easy; what we actually need to show, when arguing for the importance of ideas, is that if we could ‘remove’ the ideas, they would not be regenerated from the structural settings (as seems to have happened in the Laffer curve episode under Reagan as well as the monetarism episode under Thatcher, discussed below). The materialist argument, after all, is not that people act *without* ideas—as if they were robots—but that certain material structures prove advantageous to the spread of certain ideas.”

There are four citations in this paragraph, three to Blyth 2002, and one to Skocpol and Weir 1985. We coded the citations to Blyth as critical, since Prasad is clearly criticizing

Blyth's argument; but the citation to Skocpol and Weir as constructive-theoretical, as Prasad is citing that chapter to strengthen her own alternative theoretical approach. The first two citations to Blyth are coded critical-empirical, since Prasad is critiquing the evidentiary basis of Blyth's argument (in part by citing his own evidence!), while the last citation is coded critical-theoretical, as it is a criticism of the theoretical argument he presents. As such, Blyth represents a hybrid citation, cited for both empirical and theoretical purposes.

Example 2b: Passage is from Jack Goldstone's "Efflorescences and Economic Growth in World History" (2002:367n.15):

"For some reason, both Cardwell (1969) and Pacey (1992) give precedence to the German Otto von Guericke in demonstrating that a piston in an evacuated cylinder could raise substantial weights. However, Guericke's experiment was carried out in 1661 (Pacey, 1992, 86), while Boyle had already published the accounts of his experiment with the evacuated cylinder raising weights in 1660 (Jenkins 2000)."

This footnote is used to justify Goldstone's focus on the work of Robert Boyle in his discussion of the rise of "engine science." It contains four citations, one to Cardwell, two to Pacey, and one to Jenkins. We coded the first two as empirical-critical, since Goldstone is criticizing their claim that von Guericke deserves credit for discovering the vacuum-powered piston. The third and fourth citations we coded as empirical-constructive, as it draws upon sources that establish a historical timeline giving Boyle precedence. As with the Blyth citation in example 2a, here Pacey (1992) is coded differently in the two citations: the first time critically (for his overall claim that von Guericke deserves precedence) and then constructively (for establishing the timing of von Guericke's experiment in 1661).

Example 2c: Passage is from Danielle Kane and Jung Mee Park's "The Puzzle of Korean Christianity" (2009:370):

"In this article we build on earlier attempts to incorporate culture into network theory (e.g., Bearman 1993; Emirbayer and Goodwin 1994; Gould 1995; Erickson 1996; Fuchs 2001; McAdam 2003; Mische 2003; Passy 2003; Kane 2004; Cardon and Granjon 2005; Smilde 2007). However, while social network research has

privileged a conception of culture that views it as an outcome or correlate of network structure (e.g., Bearman 1993; Gould 1995; Erickson 1996; McAdam 2003), our work follows in a Weberian tradition that views culture and structure as mutually reinforcing (Weber [1930] 1992). In this case we see an interweaving of networks and culture: the geopolitical network structure produced cultural texts that reinforced that structure.”

This passage contains many citations. The first collection of citations (ending with Smilde 2007) are all coded constructive-theoretical, as the authors are citing them to provide generally supportive context for their positive argument (in this case, citing a collection of works which together constitute a tradition they aim to contribute to). The second collection of citations (ending with McAdam 2003’s second appearance) are all coded critical-theoretical, as the authors are citing them as examples of a conceptual approach to culture that they disagree with. The final citation, to Weber 1930/1992, is coded constructive-theoretical, as the authors are citing Weber in support of their own “mutually reinforcing” approach to culture. Note, again, that this means that Bearman 1993, Gould 1995, Erickson 1996, and McAdam 2003 are coded differently in the two citations: constructively-theoretically in reference to their overall efforts to bring culture into network theory, and then critically-theoretically in reference to their approach to culture more specifically.

PRIMARY AND SECONDARY CITATIONS

Example 3a: Passage is from Ivan Ermakoff's *Ruling Oneself Out* (2008:227):

“Deputies arrive little by little, seating as they wish; their seats are not assigned in advance. There are no longer deputies from the Right, from the Center or from the Left. This is the first expression of the dislocation of the former political parties.’ – *La Dépêche de Toulouse*, 10 July

In July 1940 parliamentary groups broke down. ‘There are no longer parties’ (Jean Froget, Radical deputy, letter to his daughter dated 10 July 1940). [fn33: Quoted by Calef (1988, 432).] As they gathered in Vichy, parliamentarians realized that their affiliation groups had lost their significance. The far-right deputy Henri Becquart described how on 9 July ‘deputies [sat] in the boxes of the orchestra as they [were] entering the room. There [is] no longer the Right, the Left, “reactionary,” Radical or Socialist rows. The room [is] like a palette, the colors of which would have been muddled.’ [fn34: Becquart, *Au temps du silence*, 185. Becquart also observes: ‘political groups were dead’ (‘Le groupes politiques étaient morts’). Becquart, *Au temps du silence*, 155.] The Socialist (SFIO) deputy of Puy-de-Dôme Albert Paulin described political parties as ‘dislocated.’ [fn35: Archives nationales, Paris, Dossiers du jury d’honneur: dossier Albert Paulin, *Memorandum for the honor jury*, n.d., AL5325.] The conservative senator Jacques Bardoux sat on the extreme Left side of the room on 10 July ‘to show that the old parties are dead.’ [fn36: Bardoux, *Journal d’un témoin de la Troisième*, 495. In July 1940 Bardoux is affiliated with the Groupe de l’Union Démocratique et Radicale.]”

In this passage, Ermakoff draws on a wide array of sources—newspapers, secondary accounts, memoirs, archival documents—to demonstrate the breakdown of groups in the Vichy parliament. We coded all of these citations as constructive-empirical citations, and we coded all of them—except for the Calef (1988) citation—as primary, since they are contemporary documents or accounts produced by eyewitnesses. This coding reflects some of the rules we used throughout: (1) memoirs and other eyewitness accounts/testimonials are coded as primary sources; and (2) quotes “quoted in” a secondary source are coded as secondary sources (that is, the citation is to the source used by the author rather than to its ultimate source—even when, as here, the author also provides relatively complete information about the ultimate source).

Example 3b: Passage is from Anthony Marx's *Making Race and Nation* (1998:219-20)

“The leadership mantle was taken up by Booker T. Washington, who stressed the assimilationist aspect of Douglass’s message rather than any popular assertion of black identity. Washington pursued a dual agenda of local black self-reliance in the South while garnering federal patronage and white financial support from Northern liberals still eager to help. To the extent his strategy was consistent with racial separatism, Washington’s Africanism remained covert and conservative, at least by most measures. [fn12: Raymond L. Hall, ed., *Black Separatism and Social Reality* (New York: Pergamon, 1977), p. 5.] Seeing no opportunity for their political inclusion in the South where most blacks remained, [Booker T.] Washington focused on separate economic advancement as a practical necessity, largely ignoring the link between such advancement and political rights. [fn13: Charles V. Hamilton, *The Black Experience in American Politics* (New York: Putnam, 1973), p. 28.] While Washington may have provided an example for racial pride, he did nothing to channel that pride into mass mobilization. Commentators and later activists would rebuke him—perhaps unfairly—for capitulation to racial domination. [fn14: Myrdal, *American Dilemma*, p. 65. Washington’s accommodationism remained unshaken, even when he was beaten by hoodlums immediately after his most famously conciliatory speech at the 1895 Atlanta Exposition. See Bart Landry, *The New Black Middle Class* (Berkeley: University of California Press, 1987), p. 18.] Committed to building his own institutional base at the Tuskegee Institute for black education, Washington abandoned political assertiveness and demands for equal citizenship. He accepted segregation and remained silent on the 1890 Force Bill, which, had it passed, would have required black enfranchisement. [fn15: See Myrdal, *American Dilemma*, p. 739; Shklar, *American Citizenship*, p. 20; C. Vann Woodward, *Origins of the New South* (Baton Rouge: Louisiana State University Press, 1951), pp. 337, 360; Fredrickson, *Black Liberation*, p. 36.] In return, he expected continued financial support, black economic advance, and reduced repression.”

The majority of the citations in this passage are coded as secondary sources, as Marx is citing them for their arguments (which in turn are based on other sources). The exception is the first citation to Myrdal (in footnote 14), which is cited as evidence for the claim that “Commentators...would rebuke him.” In this case, Myrdal is one of the commentators who so rebuked Washington, making him a primary source for the statement. This passage highlights how we coded primary and secondary sources, not purely on the basis of the type of source alone, but depending on how that material was

mobilized. Accordingly, as in this instance, the same work could be treated as a primary source at some points and a secondary source at others.

AMBIGUOUS CITATIONS

Example 4a: Passage is from Melissa Wilde and Sabrina Danielsen's "Fewer and Better Children?" (2014:1713n.5):

"Studies of the social gospel movement (Latta 1936; Carter 1956; White 1990; Luker 1991; Marty 1992; Phillips 1996; Smith 2000) and American religious history more generally tend to ignore this wave of birth control reform (Szasz 1982; Ahlstrom 2004; Marsden 2006). Even Martin Marty, who acknowledges the Anglo-Saxon focus of American Protestantism at this time and quotes a contemporary observer noting that American Protestants were crusading for the 'Americanization of immigrants, and even the gospel of eugenics and birth control' (Marty 1986 p. 215-16; see also p. 63) does not investigate these connections further. The sole exception is Tobin (2001). While her argument is consistent with ours, her book focuses on the early liberalizers and the critics, and largely misses the groups that avoided making public pronouncements."

This footnote defends Wilde and Danielsen's claim that "most studies of American religion...say nothing about the wave of birth control advocacy that took place among American religious groups in the early 1930s." The first two batches of citations (from Latta 1936 to Marsden 2006) are coded critical-theoretical, since the authors are primarily criticizing them for overlooking an important aspect of the history (the critique is not that their evidence is wrong, but that it is ignored). The penultimate citation, to Marty 1986, is coded constructive-empirical, since the authors are primarily citing him to quote a contemporary observer stating the importance of this overlooked aspect of the period. The final citation, to Tobin 2001, is coded critical-theoretical, since the authors are primarily citing her work as a foil to their own argument.

The final two citations here are somewhat ambiguous. For the Marty citation, we could have coded it as critical-theoretical as well, since the general structure of the sentence reads "Even Martin Marty...does not investigate these connections further," thereby tying it to the broader clusters of critical-theoretical citations that precede it. However, the fact that the authors single Marty out from the collection of other historians suggests some distancing by the authors; and, further, that they chose to actively quote and cite to specific page numbers, thereby suggesting that the citation is primarily to that component (which is being used constructively to bolster the authors' overall case that religion matters to the story of birth control politics), led us to code the citation as primarily constructive-empirical. Likewise, we could have coded the Tobin citation as

constructive-theoretical, since the authors are making a sharp distinction from most historians (“the sole exception”) and saying “her argument is consistent with ours.” Nevertheless, the primary discussion of Tobin’s work in this passage (i.e., the final sentence of the footnote) emphasizes what is missing and overlooked in her account—which, in the context of this footnote, led us to code it as critical-theoretical.

Example 4b: Passage is from George Steinmetz's *The Devil's Handwriting* (2007:249):

“...Commerson avoided the word *savage*, which for him suggested the abject and ignoble, but he did use the adjective ‘noble’ and referred directly to Rousseau. Commerson suggested the name Utopia for the island, but also mentioned the local name, Taïti, for the first time in print. The Tahitians were ‘men without vices, without prejudice, without needs, without dissention.’ [fn22: Commerson [1769] 1915, pp. 461-62, 466.] They were ‘governed by family fathers rather than kings’—a crucial distinction for enthusiasts of noble savagery, who did not necessarily oppose political domination but usually preferred the *patriarchal* form of rule, which they understood as softer and more flexible, to the more authoritarian and centralized monarchical forms. [fn23: Ibid., p. 462. Liebersohn (1999) identifies a nineteenth-century discourse of ‘savage nobility’ arising in the context of rejections of the French Revolution, one in which centralized political systems like the nineteenth-century Hawai’i ‘kingship’ were valorized. This is compatible with my argument that formations of ethnographic discourse are generally multivocal, but this was a minor strand in representations of Polynesia. Like Cannadine (2001), Liebersohn suggests that Europeans were looking for a *mirror* of European monarchy after the Napoleonic Wars. But this was clearly not the dominant approach, since a centralized and authoritarian model of politics often made European colonial entrée into the Pacific more difficult. As we will see below, Europeans dropped the demand for a Samoan kingship as soon as they began planning for a colonial takeover. It is also important to attend to the social class position of precolonial European observers. Some of the most influential European commentators on Polynesia, including both of the Forsters and Adalbert von Chamisso, and nearly all of the LMS missionaries in Samoa, were not monarchists but preferred more egalitarian and decentralized forms of government. They were decidedly cool toward Oceanic cultures that reminded them of European feudalism or monarchy.]...”

In this passage and accompanying footnote, Steinmetz demonstrates how the concept of the “noble savage” was applied in Tahiti. The first two citations, to Commerson, are coded constructive-empirical. The citations to Liebersohn 1999 and Cannadine 2001 are somewhat ambiguous. The extended discussion of whether European colonists in Oceania favored monarchy or not contains some empirical observations that contradict the thrust of those authors’ arguments (especially the difficulties monarchies posed to “European colonial entrée,” and the preference of missionaries and commentators for egalitarian forms of government) meaning these citations could plausibly be construed as

critical-empirical in character. However, the footnote also concedes that discourse is “generally multivocal” and argues that pro-monarchic voices were “a minor strand” and “clearly not the dominant approach.” These phrasings suggest that the thrust of the criticism is not whether the cited works are empirically incorrect, but rather whether they have given their weighted their evidence in an appropriate way—a more theoretical critique. In the context of this passage, where the commentary is footnoted to a sentence distinguishing monarchical and patriarchal forms of domination as being at the core of the “noble savage” idea, we concluded that the Liebersohn and Cannadine citations were being criticized predominantly for their theoretical claims, and coded them critical-theoretical accordingly.