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CHAPTER

Macro-Level Pluralism: Integrating Qualitative and Quantitative Approaches in the Study of Political History

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Abstract

As is true of many social-scientific traditions, political history is populated by scholars who run the methodological gamut, from those who rely on finely textured narrative accounts to those who employ advanced computational tools, statistical analyses, and formal models. But, as this chapter argues, what is distinctive about political history is that its methodological pluralism is a feature of the field in aggregate, rather than a characteristic of its most influential component studies. This *macro-level pluralism* has helped political historians to address some of the fundamental challenges of working with historical data. Perhaps more importantly, it represents a profitable alternative to the way political scientists have tended to think about mixed-methods research.

Keywords: [political history](#), [methodological pluralism](#), [American political development](#), [American political economy](#), [historical political economy](#)

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When faced with unexpected developments in American politics, political scientists often turn to the past. There, the search begins for suitable historical analogs that can help to explain or, at minimum, contextualize events that confound us in the present. Thus, the worldwide spread of Covid-19 prompted scholars to re-examine domestic and international responses to the 1918 “Spanish Flu” pandemic (Palen 2021; Zhukov and Walden 2021). So, too, in the aftermath of Donald Trump’s unexpected victory in the 2016 presidential election, new studies endeavored to locate the antecedents of the forty-fifth president’s populist conservatism, from the antebellum, nativist Know Nothing Party to the inflammatory (if unsuccessful) presidential campaigns of George Wallace (Alphonso 2020; Sides, Tesler and Vavreck 2018; Smith and King 2021).

The richness of political history as practiced in the academy—by political scientists, historians, sociologists, anthropologists, economists, biographers, and lawyers (among others)—aids considerably in these efforts. We use the term here in its broadest sense. Political history is, on our account, a thriving social-scientific tradition devoted to examining past outcomes, behaviors, and power dynamics—whether to inform our present politics, develop new theories and concepts (or test old ones), or simply to enrich our understanding of political institutions and behavior.

While much has been written on the virtues of doing political history, this chapter highlights one in particular: its methodological ecumenicism. As we detail, because it has long been a self-consciously interdisciplinary enterprise, political history is remarkably pluralistic in its analytic perspectives. The field is populated by scholars who run the methodological gamut, from those who rely on finely textured narrative accounts to those who employ advanced computational tools, statistical analyses, and formal models. But while it is perhaps easy to view this pluralism as a historical artifact—a path-dependent feature of how political history has evolved, particularly in political science—we suggest that it can and should be seen as a model for how to do social science well. In particular, we emphasize the fact that political history’s methodological pluralism is a feature of the tradition in aggregate, rather than a characteristic of its most influential component studies. This *macro-level pluralism*, we think, represents one way that scholars can address some of the fundamental challenges of working with historical data. But, we argue, it also represents a potentially profitable alternative to the traditional way political scientists have thought about mixed-methods research.

We begin by delimiting some boundaries. What is political history and how has it evolved across the social sciences? Next, we turn our attention to political history as practiced by political scientists, identifying several discrete streams of scholarship and celebrating the gains in knowledge production that flow from their methodological ecumenicism. We then describe two challenges that political historians, irrespective of discipline or methodology, regularly encounter. We contend that it is political history’s macro-level pluralism that best equips its practitioners to respond to these challenges and excavate the past with precision.

A final note on the chapter’s scope. While work by political historians explores a wide range of countries and subnational jurisdictions, this chapter focuses exclusively on studies of the United States. In the service of depth, we have deliberately chosen to sacrifice breadth.

What is Political History?

As we understand it, political history is not a set of methodological precepts or a discrete substantive area. Rather, it is best thought of as a network of intellectual communities—in history, political science, and sociology (alongside anthropology, economics, and law)—that share an interest in a common set of questions about politics and rely on data (whether qualitative or quantitative) produced in prior eras.¹ Regardless of disciplinary affiliation, those who study political history typically do so out of a desire to better understand how the past informs the present (Orren and Skowronek 2004). What, if anything, can those of us alive today learn from those who came before? For this reason, as Daniel Galvin observes, many political historians seek to “explain important outcomes in American political history or to uncover the origins of contemporary political problems” (2020, 489). But there is also a burgeoning literature that uses data drawn from earlier periods in time to assess the explanatory power of existing theories of political behavior and institutions and develop new ones. This trend is motivated by the recognition that the past offers social scientists a wealth of observations to draw on in thinking systematically about political phenomena that puzzle us today.

In its contemporary incarnation, political history is relatively more attentive than other areas of scholarly inquiry to the factors that drive—and often inhibit or constrain—institutional change. To what extent do past allocations of power constrain later efforts to change it? How much agency do actors in the present really have

to alter their circumstances? And what is the relationship between elites, often charged with wielding formal governing authority, and mass-level actors, from voters and interest groups to activists and donors?

Political history as a scholarly endeavor long predates the development of the modern research university. Thus, for instance, one might reasonably count Thucydides' account of political life in classical Athens or Edward Gibbons' study of imperial Rome's decline (at least in the western part of the empire) as antecedents of the field. Closer to home, political history can take credit for both Frederick Jackson Turner's celebrated work on the role of the frontier in shaping American political life (1993 [1893])—contending that it was the nation's continuing westward expansion that yielded some of the most distinctive aspects of our political culture—and Charles Beard's Progressive-inspired history of the drafting of the U.S. Constitution (1913), foregrounding the role of the Framers' economic interests rather than their professed political ideals. By the middle of the twentieth century, political historians were no less ambitious in the research agendas they pursued. For instance, landmark studies traced the administrative changes wrought by the New Deal (e.g., Leuchtenberg 1963) and re-examined various critical aspects of American political culture (e.g., Hofstadter 1955; 1960; 1963).

In the ensuing decades, however, political history came under increasing scrutiny (Leuchtenberg 1986). Often criticized for its focus on political elites to the detriment of “ordinary” people, the field was vulnerable to left-oriented calls that historical work should be conducted from the bottom up. Political historians thus moved away in the last decades of twentieth century from their traditional focus on top-level leaders and formal institutions of governance to “research topics that foregrounded symbols, rituals, discourse, and cultural practices” (Bonnell and Hunt 1999, 8). This shift has proved a lasting one. Nevertheless, political historians across the social sciences are gradually returning to the institutions- and leaders-oriented focus that had once characterized the field.

In political science, locating leaders and institutions in historical time was a response to the perceived limitations of the discipline's behavioral revolution. While a focus on rationality as a parsimonious explanation for complex political phenomena made it possible for political scientists to leverage (often quantitative) data in new ways, it left many scholars with a sense that a historically contextualized analysis of governing institutions was not being given its due (Schickler 2020; Whittington 1999). Methodologically, this means that many political scientists engaged in political history tend to rely on more qualitative sources of data, the better to provide the rich level of detail necessary to situate their research in time. Substantively, it means that political scientists doing political history tend to focus on explaining deviations from the outcomes we would expect based on a simple model of rational actors pursuing their interests rationally. Thus, they tend to be especially concerned with “issues of macro structure, the role of temporal ordering or sequence, and a whole host of social processes that play out only over extended periods of time and cannot be reduced to the strategic ‘moves’ of ‘actors’” (Pierson 2004, 9).

In history, attention to macro-level changes in American society—most importantly, the rise of the conservative movement—prompted scholars to think anew about *who* has the power to influence government. Seeking to explain how conservatives reinvigorated right-wing politics in the decades after the Second World War, a vibrant historiography emerged and shows no sign of receding. For example, we have learned much about the role of women in suburban Orange County in propelling what would become the Reagan revolution (Nickerson 2012); the importance of city-dwelling working-class whites in mobilizing against racial integration (Kruse 2005); the rise of evangelical Christianity as a potent political force in American politics (Dochuck 2009; Sutton 2014; Worthen 2014); and the effect of all of these social pressures on Congress (Zelizer 2004). Shaped by other major developments in American politics—the increasing power of organized business groups, long-running wars in Afghanistan and Iraq, to name only two—historians have also produced deeply researched treatments that situate what appear to be one-off political events in the context of broader trends and patterns in our collective past (e.g., Immerwahr 2019; Waterhouse 2014).

And in sociology, history in some sense never left. Indeed, drawing on the lessons of its celebrated founders—from Max Weber to Talcott Parsons, not to mention the influence of Marx and Marxian analysis—the discipline continued to nurture the field of historical sociology throughout the latter half of the twentieth century (e.g., Clemens 1997; Dobbin 1994; Skocpol 1979). Like its cousins in political science and history, political history as practiced by sociologists remains attuned to identifying roads not taken—what Anthony Chen calls the task of explaining “why politics and policies took the particular directions they did” rather than “other directions they plausibly might have” (2009, 230). And so, like other branches of political history, sociologists continue to explore (among other things) how prior choices about organizing politics have shaped subsequent developments in our society (e.g., Prasad 2018).

Situating Political History in Political Science

Within political science, the focus of the remainder of the chapter, political history flows in three scholarly streams: American political development (APD), historical political economy (HPE), and American political economy (APE). We discuss each in turn, comparing and contrasting (albeit in general terms) their respective substantive and methodological orientations.

American Political Development

American political development (APD) is primarily concerned with identifying recurrent patterns in American history—“the sine qua non of the enterprise,” as two of the subfield’s founders have put it (Orren and Skowronek 2004, 7). For this reason, APD scholars tend to compare eras across time, identifying both the core continuities and discontinuities in American politics. To facilitate these comparisons, the subfield boasts a rich conceptual vocabulary. APD research often speaks of “orders” (or sometimes “regimes”) when describing the bundles of institutional and cultural features that link specific periods in American history together (ibid., 14); “intercurrence” when exploring the consequences of conflict between multiple such orders coexisting at the same time (ibid., 17); “layering” when analyzing why it is that institutional change rarely results in a wholesale makeover of previous arrangements (Schickler 2001); and “conversion” when uncovering how political entrepreneurs make use of existing institutions to achieve their goals rather than devising new ones from whole cloth (Thelen 2004). Taken together, these ideas—alongside many others, from “bounded innovation” (Weir 1992) to “electoral capture” (Frymer 1999), “drift” (Hacker 2004), and “frontlash” (Weaver 2007)—have not only enriched our collective understanding of politics, but also demonstrated the virtues of attending to time and temporality in shaping outcomes of consequence.

Though exemplars of their explanatory power abound, consider just two. As Eric Schickler details in his now-classic study of institutional change in Congress (2001), the reality that new institutional arrangements are often layered atop preexisting ones helps us to understand why reforms to the congressional budgeting process produced apparent (and, from a functional perspective, suboptimal) redundancies, with appropriations committees in both houses of Congress surviving the introduction of ostensibly supervisory budgetary ones. In so doing, Schickler’s work helps us to appreciate why the legislative process became even more complex despite lawmakers’ sincere and concerted efforts to streamline it.

Tracing a different developmental process, Sarah Staszak documents how and why arbitration became a “private, nonreviewable, compulsory form for resolving disputes” between employers and employees and between businesses and consumers (2020, 240). Her account chronicles how arbitration was converted from a mechanism designed to resolve conflict between parties with roughly equal bargaining power into one designed to maximize the economic leverage of large businesses. Staszak’s study makes clear that viewing mandatory arbitration as a sudden creation of the contemporary, pro-business Supreme Court not only

misunderstands its origins, but paints a misleading picture about how and when powerful interests are able to exert their influence.

Because those engaged in APD research tend to be interested in “configurations of causal factors that explain outcomes in specific times and places” rather than crafting broader generalizations or covering laws, they have tended to rely more than other scholars of American politics on qualitative methods (Galvin 2016, 210–11). Their work is often presented in narrative form, with detailed case studies laying out carefully crafted “mid-range” theories designed to “illuminate ... only a small number of substantively significant political phenomena” (ibid., 212). Indeed, APD more than any other subfield within American politics is usually concerned with careful process tracing, whether in a sustained effort to correct received wisdom or to showcase the explanatory power of a new concept.

Yet, an array of recent scholarship demonstrates that APD is not an exclusive redoubt for scholars employing qualitative methods. For example, Devin Caughey (2018) uses sophisticated quantitative tools to challenge the traditional view that the combination of de jure restrictions on Black suffrage in the Jim Crow-era South and de facto restrictions on white suffrage severed the connection between the region’s elected representatives and their constituents. Caughey’s deliberate choice to eschew broad generalizations about representation in favor of mid-range claims about how representation operated in a particular region of the country under a particular (and lamentable) set of institutional rules situates the *Unsolid South* comfortably within APD. At the same time, his application of novel data and cutting-edge statistical techniques underscores that APD need not be synonymous with a reliance on purely qualitative data.

Historical Political Economy

Historical political economy (HPE) represents a second, more recent, stream of political science-inflected political history. According to one of the field’s founders, HPE is, like APD, “the study [of] how political and economic actors and institutions have interacted over time.”² Unlike APD, however, HPE is explicitly oriented around contemporary best practices for causal inference. As Sean Gailmard puts it (2021, 70): “all theory-oriented empirical work in HPE answers essentially one question: whether specific causal mechanisms are or are not consistent with observed case evidence.” HPE is therefore political history in the sense that it most often seeks to canvas past events (and perhaps only past events under some set conditions that are no longer in place, thus making them meaningfully historical; see ibid, 73) with one of two purposes in mind: either to develop a novel theoretical explanation that can best make sense of them or to evaluate their consistency with some existing theoretical explanation. Put another way, HPE scholars tend to view the past as a rich set of “raw materials” for the traditional process of hypothesis generation and testing, with the goal of producing ever “more precise theories about institutions” (Dennison 2021, 106).

Although HPE is a relative newcomer in the study of American politics—the inaugural issue of the *Journal of Historical Political Economy* was published in 2021—it has already proven to be a fruitful and productive area of inquiry. To take only two examples. Thomas Gray, Jeffrey Jenkins, and Philip Potter (2022) have recently located one source of the emergence of the modern presidency in an unexpected place: the switch from indirect to direct election of senators. On their account, accountability to the voting public—rather than state legislators—reduced senators’ incentive to surveil the executive’s handling of foreign affairs, thereby conferring greater discretion on presidents to conduct foreign policy. After all, voters care substantially less about what happens far away than about events closer to home. To make the argument, the authors leverage variation in the timing of each state’s adoption of direct primaries, generating several hundred senator-vote observations before and after the rules change.

Turning to a different era in American history, Mario Chacon, Jeffrey Jensen, and Sidak Yntiso (2021) have explored the role of military force in promoting Black representation in state legislatures and state

constitutional conventions during Reconstruction. Using records documenting where federal troops in the former Confederacy were stationed, as well as data on the racial identity of state legislators, they find that Black representation is positively correlated with Union occupation. The authors draw on an array of techniques to rule out unobserved omitted variables, including that railway networks would both facilitate Black political mobilization and dictate the location of military outposts.

Given HPE's commitment to causal inference, its practitioners tend to privilege quantitative and formal models. As Gailmard writes, "one important aspect of HPE's development is that it has occurred alongside the 'credibility revolution' in empirical social science."³ Given these commitments, HPE scholars employ some of the most cutting-edge ideas in research design and quantitative methods, from natural experiments and instrumental variables to data imputation and large-scale textual analysis that leverages advances in the digitization of historical materials (Cirone and Spirling 2021). Unlike APD, HPE tends to eschew narrative, instead favoring "evidence established through statistical correlations that are given causal meaning through precise research designs" (Cirone and Pepinsky 2022).

At the same time, HPE scholars remain engaged in fruitful conversations with political historians of other methodological stripes. Some of these exchanges concern shared substantive interests—in particular, about the rise of states, the nature of institutions, the effects of time and temporality, and the role of historical "persistence," that is, causal mechanisms that unfold over very long periods of time. But HPE has also embraced dialogue in an effort to exploit the comparative advantages of different kinds of political historians. Thus, while HPE scholars rightly see themselves as leading the charge to bring contemporary improvements in quantitative methodology to bear on analyzing the past, they have also expressed openness to collaboration with others—what Tracy Dennison calls "bringing together different forms of expertise to enable researchers to approach a question from more sides than their own tools allow."⁴

American Political Economy

A final stream of political history practiced by political scientists is another recently constituted subfield: American political economy (APE). Much like their APD counterparts, APE scholars are primarily interested in how "institutional configurations shape coalitional politics to produce long-term developmental processes" (Hacker et al. 2021, 7). While APE is less explicitly oriented than APD around the state as a web of institutional relationships to be examined, the two research communities share the view that politics is a fundamentally developmental phenomenon. Accordingly, APE recognizes that understanding how politics shapes markets (and vice versa) requires viewing politics as a set of "outcomes, institutions, and power relations ... forged through long-term conflicts within and across multiple institutional venues" (ibid., 2).

While APE is in its infancy as a formally constituted research community, political historians have long been interested in the relationship between politics and the marketplace. Such works are far too numerous to list here, but Richard Valelly's (1989) study of state-level economic radicalism is illustrative. As Valelly chronicles, American states in the early twentieth century were sites of considerable agitation on the part of citizens to alter the traditional role of government in the economy. But while the financial pressures of this period were felt nationwide, Valelly shows that it was a particular configuration (or overlap) of institutional arrangements—one-party dominance in the absence of thick party loyalty, coupled with the presence of political entrepreneurs experienced in conducting third-party, usually Socialist, campaigns—that explains why that unrest translated into successful political movements in only some areas of the country.

Building on this rich research tradition, many more recent works of APE seek to better understand the lasting economic impact of America's troubled racial politics. For instance, Jessica Trounstone shows that a municipality's preference for neighborhood-level segregation in 1970 can help to explain the stringency of land-use regulation in the early part of this century (2020, 447). Trounstone's invocation of policy lock-in—a

core theme within both APD and APE—to explain variation in how the market for land is structured in the United States situates her work comfortably in political history’s heartland.

Notably, while scholars of APE share a common substantive focus, they do not insist on privileging one methodological approach over others. Some APE research, like Valelly’s *Radicalism in the States*, relies on detailed case knowledge and an array of historical sources to craft a complex, mostly qualitative, narrative account. Other studies, like Trounstine’s, draw on statistical modeling to make their case. As we explore in greater depth in the next section, it is precisely this macro-level pluralism that has enabled political historians to pursue such varied and consequential research.

Common Challenges, Collaborative Solutions

As a scholarly endeavor, political history requires accurately characterizing the past. Whether in search of causal explanations for past events or a clearer understanding of how those events might shape the present, political historians must have confidence that their data will not lead them to misunderstand what really happened. In this section, we first highlight two ways that the process of accumulating and using historical data can bias how we represent the past, training our focus on both the data *loss* inherent in preserving information about the past and the data *overflow* increasingly characteristic of contemporary political processes. Against that backdrop, we then revisit the macro-level pluralism of political history as it has developed within political science. We suggest that, given these twin limitations, this methodological pluralism is no luxury. Rather, it is an appropriate, and perhaps necessary, response to the challenges of working with historical data, and one that may prove a workable—and, we think, preferable—alternative to the increasingly common ideal (at least within political science) of mixed-methods research.

Data Loss

Because our records of the past are necessarily incomplete, political historians must constantly wrestle with a fundamental question: why do we have the data we have? And what happened to records we might have had but don’t, either because they were never produced or because they got lost along the way? Sometimes, the answers have to do with forces beyond politics, indeed, beyond human agency. Natural disasters—not to mention fires, pests, mold, and water damage—have destroyed an immeasurable treasure trove of records (Cirone and Spirling 2021). In many such cases, it is impossible to know what we no longer have access to. And so, while political historians are always aware that they are dealing with incomplete information, there is little they can do about it.

Other times, however, the choice of what to record for future generations is shaped by political considerations. Historical data tend to tell us more about elites than “ordinary” citizens precisely because of differences in political power (ibid.). Elites are likely to have more time and resources to preserve their contemporaneous thoughts and later reminiscences; so, too, authorities with the capacity to save records are more likely to aid them in these efforts. Members of marginalized groups are less likely to leave a historical footprint than those in a more privileged position. More prosaic political concerns are often at play as well. For instance, the desire to cast supporters in a positive light may lead to the destruction of records that would facilitate criticism by later generations. By the same token, the desire to cast opponents in a negative light may lead to calculated preservation efforts, the better to ensure that enemies do not find favor in the eyes of future scholars. The bottom line, as Francis Blouin and William Rosenberg write, is that political history requires “discern[ing] how and why ... sources have been assembled, what they evidence indirectly as well as directly, what sociocultural and sociopolitical conventions lie behind their formation, and what their silences may mean” (2011, 118).

Making matters worse, keeping, maintaining, and organizing materials for subsequent generations of scholars to examine is itself an unusual act (O'Toole and Cox 2006). Not everyone will think to, or want to, hold onto their own papers, particularly when doing so is not required by law. For example, Justice Owen Roberts's votes to uphold key aspects of the New Deal proved pivotal to the revolution in constitutional interpretation occasioned by the Great Depression. But Roberts chose to destroy his personal papers, depriving later generations of an insider's perspective on key developments during this critical period in American politics. At least on the Supreme Court, Roberts is no outlier. Long-serving justices James McReynolds (26 years), George Sutherland (16 years), and Byron White (31 years) similarly opted not to make all or part of their personal papers available for study, as have many others (Watts 2018). One of White's clerks remarked that they could not believe "how much history" was being consumed by the justice's shredder (*ibid.*).

The reverse is also true. Political historians' accounts are necessarily shaped by the prejudices (conscious or not) of those individuals whose materials they do have access to. Justice Harry Blackmun's papers, including his notes on votes taken during the justices' private conference, have offered scholars a detailed look behind the scenes at how the Supreme Court works (Black and Owens 2009). But Blackmun was no neutral party in the court's decision-making. It is thus imperative for scholars to evaluate the justice's own considerations in deciding to hold onto his notes and ultimately make them accessible (see *ibid.*).

Even where they are preserved and donated, materials of great interest to political historians may come with important limitations. For example, the bulk of Senator Joseph McCarthy's personal papers remained inaccessible to researchers for more than a quarter century after McCarthy's death, when Marquette University concluded it was not bound by the terms his widow had sought to impose on the collection. When those materials were released, however, the bulk turned out to be duplicative of what was already in the public domain, consisting mostly of congressional publications, newspaper clippings, and recordings of speeches. Those documents scholars hoped would reveal the senator's private thinking on matters central to his rise and fall either never existed or had not been preserved.⁵

There is also the matter of how later generations will organize the materials that have been made available. Political biographer Robert Caro has recounted his search for proof that then-Representative Lyndon Johnson used his control over political contributions from the Texas oil industry to elevate his stature in the House (2019). As Caro tells the story, these materials were, presumably deliberately, removed from boxes comprising LBJ's "House Papers" and placed into a subgroup of documents organized by correspondent name. Only by intuiting that relevant documents were likely to be found in file folders corresponding to Texas's largest oilmen did Caro discover what he was looking for.

Data Overflow

While many political historians struggle to make do with too little data, those who study more contemporary eras are increasingly bowing under the weight of the vastness of what has been (or can be) retained. Indeed, "even the most historically minded archivists have been overwhelmed in recent decades by the exponential rise in the amount of records produced," from emails and tweets to Facebook posts and perhaps even TikTok videos (Blouin and Rosenberg 2011, 9). This means that, going forward, political historians of all methodological stripes must wrestle with a different set of concerns. If they cannot look at everything, how should researchers select the data they will analyze?

Happily, an array of increasingly sophisticated computational tools can be of assistance. Data in digital form can be scraped and analyzed using R or Python (Munzert et al. 2015). Computational text analysis has become a mature, independent area of cross-disciplinary scholarship, making it possible for researchers to break down documents in a variety of ways. These tools allow political scientists to examine the frequency of terms and their coincidence to make assessments about elite behavior (e.g., Grimmer 2013); craft "topic models" to study

changes in the political agenda (e.g., Quinn et al. 2010); and evaluate the sophistication of political communications (e.g., Benoit, Munger, and Spirling 2019) and the complexity of institutional structures (e.g., Shaffer 2022). The more information that is available to political historians, the more these tools become critical parts of our collective scholarly infrastructure.

And yet, while these tools make it possible for political historians to manage data overflow, they cannot resolve the challenges it creates. For one, data overflow conditions what is available to political historians. Archivists at even well-resourced sites are struggling to keep up with the volume of material generated by contemporary political institutions. Archivists estimate, for example, that the Reagan Presidential Library contains more than 60 million pages of documents, 1.6 million photographs, 500,000 feet of motion picture film, tens of thousands of audio and video tapes, and over 40,000 artifacts.⁶ Given this stunning quantity of materials, many records from Reagan's two terms in office—now over thirty years old—remain unprocessed, subject to review for privilege and classification and therefore unavailable for scholarship, unless through a specific request made under the Freedom of Information Act (FOIA). Indeed, things are so bad that one scholar recently suggested that he had “better luck in Moscow” getting access to Soviet government files (De Groot 2022). And while records made available through a FOIA request are often made public for later scholars to examine, they may themselves be a source of selection bias, as they are surely not a random sample of what remains unprocessed. Separate from these concerns, a growing quantity of audio-visual records, from the Nixon tapes to more workaday recordings of presidential communications, often require other kinds of intervention, from touching up to transcription. These, too, require the time and attention of often short-staffed archivists.

Aside from these very practical limitations, there are also conceptual issues to consider. The awesome quantity of material for political historians of the recent past to wade through means they must make difficult choices about research design up front. As is true for political scientists more generally, researchers must decide what it is they are interested in and how the data will permit them to measure it (e.g., Adcock and Collier 2001). In using the powerful computational tools available to them, they must take care to contextualize their searches and to avoid historical anachronism. As Alexandra Cirone and Arthur Spirling (2021) explain, for example, though the *New York Times* has been around since the immediate post-Civil War era, researchers should not simply assume that it has always been the nation's newspaper of record. So, too, words like “gay” that have a specific, contemporary meaning may not have had the same meaning in the past. To accurately characterize the past, political historians must exercise care and consideration at every decision point.

The Importance of Macro-Level Pluralism

Given these challenges, political history's macro-level pluralism is not only a strength of the field, but increasingly a necessity. To begin, the laws of comparative advantage apply in political history as anywhere else. No one scholar is likely to be equally adept at executing the various research approaches that comprise the field. But the good news is that they do not need to. Political history's ecumenicism allows individual political historians to benefit from doing what they do best, secure in the knowledge that they can take advantage of the work of those who have other kinds of expertise.

At a minimum, this means that scholars are likely to work with types of data they know best. Familiarity with particular data sources—specific archival sites, for instance, or data-generating institutions like the U.S. Census Bureau—means scholars are likely to be better acquainted with the limits of the data they have chosen to work with. But political history's macro-level pluralism also means they can further compensate for those limitations by relying on others doing the same. For example, scholars who seek to deploy large-scale text analysis can turn to the work of researchers situated within other streams of political history for necessary context. By the same token, the identification of a novel causal relationship can and should prompt others to undertake a more focused inquiry into the meso-level, localized mechanisms undergirding it. This kind of

search may well lead political historians familiar with the organizing logic of, say, a specific person's papers to discover new materials in a previously hidden or otherwise untapped place.

Implicit collaboration on specific research questions offers one possible model for what this might look like and what its relative advantages might be. Consider some recent political-historical scholarship on the American South. Although each study approaches the subject with a quite different methodological toolkit, they coexist in a rich ecosystem. Robert Mickey's (2015) study of the contemporary democratization of the former Confederacy relies largely on detailed, state-specific case studies. David Bateman, Ira Katznelson, and John Lapinski's (2018) examination of the priorities of Southern legislators between Reconstruction and the Civil Rights era and their role in shaping national policymaking is classically mixed-methods scholarship, relying on new quantitative measures and data, together with a more traditional "immersion in congressional debates and newspaper coverage and various primary sources" (ibid., 22). And Avidit Acharya, Matthew Blackwell, and Maya Sen's (2018) work on slavery's long-term influence on southern politics is powered by their identification of a powerful quantitative relationship between "the share of the population that was enslaved in 1860" and an array of "important attitudes expressed by Southern whites in contemporary times" (ibid., 61).

It is by reading these works alongside one another that we can most fully understand the legacies of the region's repressive racial regime on our democratic institutions. But beyond a specific substantive concern about how sectionalism has shaped American political development, this particular concatenation of scholarship points to a broader truth about political history as practiced by political scientists. Macro-level methodological pluralism is not only a way to deal with the limits of historical data, but also an important—and we think, more sustainable—alternative to traditional mixed-methods research. Rather than demand that individual scholars tackle a research question from a variety of methodological angles with equally high levels of sophistication, healthy macro-level diversity frees researchers to choose the approach that best suits their research interests and talents. And it is by supporting their choice to do so that political scientists can best ensure that political history remains a vibrant and innovative field of study.

Conclusion: Where Do We Go from Here?

Political history within and outside of political science has much to be proud of. Though we have focused on its macro-level methodological pluralism, our discussion has also hinted at its substantive triumphs. The overarching point is that political history shows the potential of methodological "triangulation," where tolerance of methodological difference means that the field's overall gains in knowledge production exceed the sum of its parts.

But the macro-level pluralism we have described can be brought to bear on a wider range of topics than has been true of political history to date. Indeed, within political science, political history has tended to cluster around a specific set of research topics—most prominently, the development of the American state and its relative strength in comparative perspective. But as the rise of both HPE and APE as vibrant streams of political history makes clear, this is a path-dependent and corrigible feature of political history rather than a fixed state of the world. Given the broader scholarly and public reckoning with the legacy of the Great Recession, political historians could do more to study both public and private financial-regulatory institutions, from the Federal Reserve and the Securities and Exchange Commission to the Financial Industry Regulatory Authority and the New York Stock Exchange (e.g., Binder and Spindel 2017). So, too, while the influence of race and racism on the development of American political institutions has been a persistent—and increasingly urgent—theme of political history, more sustained study of those institutions' engagement with Native Americans and other Indigenous peoples is appropriate (e.g., Frymer 2017; Orr 2017; Sparrow 2006). Finally, political historians would do well to examine in greater depth the role of the United States as a colonial empire and postwar global

superpower. Although this is a growing area of study, particularly for those working at the intersection of American politics and international relations (e.g., Carson 2018; Moore 2017; Saunders 2014), there remains a real gap between America's prominence in world affairs and the amount of attention that the institutions, interest groups, and activists dedicated to the pursuit of foreign policy have received.

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Notes

- 1 As is often the case, delineating the boundaries of a research community (or communities) is difficult and political history is especially porous. Consider that legal scholarship on the founding of the American republic, the development of separation of powers doctrine, and the rise (and perhaps fall) of the administrative state is recognizable and arguably classifiable as political history, however its practitioners might identify (e.g., Barron and Lederman 2008; Grisinger 2012; Klarman 2017; Metzger 2017). Here, in the service of tracing the development of political history as an area of inquiry within political science, we have chosen to highlight the changes in adjacent disciplines that have most shaped it.
- 2 Pers. comm. by email from Jeffrey Jenkins to authors, June 1, 2021.
- 3 Sean Gilmard, “What are we doing in Historical Political Economy?” <https://broadstreet.blog/2021/01/08/what-are-we-doing-in-historical-political-economy/> (Jan. 8, 2021).
- 4 Tracy Dennison, “On the Challenges of Interdisciplinary Collaboration,” <https://broadstreet.blog/2022/02/28/on-the-challenges-of-interdisciplinary-collaboration/> (Feb. 8, 2022).
- 5 See Marquette University, Special Collections and University Archives, *Joseph R. McCarthy Papers*, *Access Note*, <https://www.marquette.edu/library/archives/Mss/JRM/JRM-SC.php>.
- 6 Jessie Kratz, *The Ronald Reagan Presidential Library: Managing the Materials of a Modern Eight-Year Presidency*, <https://prologue.blogs.archives.gov/2015/10/30/the-ronald-reagan-presidential-library-managing-the-materials-of-a-modern-eight-year-presidency/>.