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Chapter One

Designing Historical Inquiry

Much as the gestures of an orchestra's conductor shape the sound we hear, the research methods we utilize affect how we perceive the world. None of us can comprehend the full complexity of the human condition. No approach can promise full understanding. "Large subjects in history can never be definitively written" (Bailyn 1994, 67).[1] We thus require an array of purpose-specific tools to perform and apprehend the composition of history and social science.

From that perspective, this book advances a particular set of analytical instruments for quantitative analysis. Not, of course, as a comprehensive orientation but as a rigorous complement to a wider array. Our substantive goal, to adapt a statement of Robert Dahl, is to ask how, "while respecting the boundaries between history and the social sciences—or, as many of us would say, between history and the other social sciences—[we can] make it easier for scholars to cross these boundaries" (Dahl 1988, 32).

Convinced that there is a tremendous opportunity waiting to be seized, we advance innovations in methodology to complement what less formal and more qualitative approaches can accomplish. To this end, we identify methods for statistical analysis in harmony with work by historians and historical social scientists, approaches that embed patterns of thought and behavior within specified contexts and that recognize processes as they unfold in distinct progressions over particular dimensions of time (Katznelson and Weingast 2005; Pierson and Skocpol 2002). As specific variables "may have different—even opposite—effects, depending on [their] timing or duration," we wish to search for quantitative methods consonant with what James Mahoney calls thick theories, orientations to social research whose chief elements include "complex arguments about sequence and duration" (Mahoney 2004b, 90).

These objectives are underpinned by a premise, motivated by an observation, directed by an ambition, and guided by an intuition.

The premise: There was a massive shift toward quantification in the late medieval and early modern European world, at the very moment the institution of the modern university was being crafted, when religion in its increasing variety moved from being an enveloping reality to a series of choices; modern states were being formed; a Europe-centered global trading marketplace was being advanced; and often brutal patterns of conquest were revealing the dimensions of human pluralism. Each of these profound developments was accompanied by ever more sophisticated instruments of measurement. Quite steadily, there occurred a "shift from qualitative perception to, or at least toward, quantificational

perception" (Crosby 1998, 49). Quantity joined quality in description, analysis, and evaluation. The quantification of time and space increasingly went hand in hand with mathematical advances and the spread of computational processes, based in practical affairs, as with bookkeeping, in tandem with a deepening scientific curiosity. The pioneer social science communities fashioned in the late nineteenth and early twentieth centuries were premised on these social knowledge transformations. Quantification in reality and the measurement of reality propelled key features of social research.

The observation: Presently, there is a mismatch between social scientific studies with historical substance and appropriate statistical tools. Imagine a conductor with a baseball bat rather than a baton. Searching for more suitable implements, we show that it is possible, without sacrifice of precision and validity, to more fully investigate how fundamental factors configure over different ranges of historical time inside varying circumstances. The longer the time period under examination, "the fewer institutional or structural conditions that can be considered fixed" (Meyer and Conrad 1957, 542), hence the need to give proper pride of place to ways of working that engage the problem of identifying parameters for causal inference. The intervals and settings within which key processes function can vary quite a lot at different periods, and may have distinctive force within dissimilar circumstances. The meaning and intellectual capacity of generalization's hypotheses and mechanisms are vitiated when they do not comprehend and account for specific contexts. Measurement and corroboration lose power, even potentially mislead, when they fail to apprehend demands of historical specificity regarding particularities of place and the punctuated qualities of time.

The intuition: Used thoughtfully, quantitative tools can become means to conjoin historical specificity with generalizing ambitions, and advance work on large, vexing questions, at once analytical, empirical, and normative. Only when selected appropriately can methods—qualitative and quantitative, separately and together—be handmaidens of such inquiry. There is much to be gained when we select tools that promise to discipline quests for exactitude and portability by the desire to incisively probe historical situations and temporal processes. Qualitative scholars often worry that quantification in history sidesteps what Charles Tilly called the big structures and large processes that fundamentally affect the human condition (Tilly 1984). By contrast, we believe that the right selection of statistical tools—resonant with specifications of time and place—can enhance the ability to probe significant puzzles ambitiously.

1.1 CONUNDRUMS

This book seeks to transcend too stark a choice between deep case knowledge and causal precision. Within the framework of identifying cogent reservations about mainstream quantitative scholarship by those who practice historical research

as their primary craft, our primary goal is to demonstrate how the thoughtful utilization of approaches that privilege parameter variation can serve as promising means, though not the only means, to bridge these research communities by systematically capturing key features that are central to historical scholarship.

Historians and historical social scientists rightly worry that scholarship that is quantitatively oriented too often works on historical data without attending sufficiently to context, historical specificity, temporality, and periodicity. We share their view that historical research must not proceed by way of a flattening universalism. At the heart of this book lies the conviction that statistical means that are deployed to empirically comprehend different historical moments and processes must attend to parameter heterogeneity and grapple with specific patterns of complexity inside the very construction of quantitative methods.

We proceed with two primary sets of readers in mind, whose ties we hope to facilitate. First, we wish to help guide social scientists whose work is primarily statistical, especially when they seek to transcend the limits of existing methods. Moving toward alternative ways of working with more appropriate tools in hand, quantitative scholars can achieve more direct engagements with historians and qualitatively-oriented historical social scientists. Second, we hope to entice colleagues who are skeptical about quantitative approaches to history to understand that statistical methods of the kind we advance in fact do respond to their doubts, and thus can be marshaled to contribute to meaningful and substantial advances on significant problems in historical understanding in tandem with archival and literature-based qualitative scholarship. While we do not expect that those working primarily in the qualitative tradition will suddenly become quantitative scholars, we hope to convince such individuals that quantitative scholarship that attends to their concerns in the ways that we advocate is just as valid for making contributions to historical questions.

We are well aware, however, that these aspirations are not uncomplicated or undemanding. Vexed by scholarly partitions, a circumstance identified long ago by Aristotle (whose *Posterior Analytics* (1960) opens by insisting that every distinct subject has its own principles and rules of inquiry), more than a few barriers must be overcome. Subject-level distinctiveness certainly has been the dominant reality in the era of modern social science. Max Weber's posthumously published *Economy and Society* underscored such divisions between social science and history. "Sociology," he memorably wrote, "seeks to formulate type concepts and generalized uniformities of empirical processes. This distinguishes it from history, which is oriented to the causal analysis and explanation of individual actions, structures, and personalities possessing cultural significance" (Weber 1978[1921], 19).

This designation of apartness has become a truism. Addressing the gap between Sociology and History in the late 1960s, Seymour Martin Lipset wrote "the task of the sociologist is to formulate general hypotheses, hopefully set within a larger theoretical framework, and to test them." By contrast, "history must be covered with the analysis of the particular set of events or processes. Where the sociologist looks for concepts which subsume a variety of particular

descriptive categories, the historian must remain close to the actual happenings and avoid statements which, though linking behaviour at one time or place to that elsewhere, lead to a distortion in the description of what occurred in the set of circumstances being analysed" (Lipset 1968, 22–23). There is much to be said for such distinctiveness. It is perfectly reasonable that the structure of good explanations in different disciplines is distinctive. As the philosopher Tim Scanlon has argued, "the truth values of statements about a [scholarly] domain, insofar as they do not conflict with statements of some other domain, are properly settled by the standards of the domain they are about" (Scanlon 2014, 19). But this position does not preclude how bodies of knowledge might interrelate notwithstanding significant differences, and the terms of connections across disciplinary impulses. From this point of view Charles Tilly's wry comment that "History does the transcription, Sociology the analysis" hardly offers a meaningful connection (Tilly 1981, 5).

1.2 HISTORY AND POLITICAL SCIENCE: A CENTURY OF DIVERGENCE

When described in the early 1920s by Weber, this division only recently had emerged. Crisp separate trajectories for History and the social sciences did not exist before the start of the twentieth century. As we think about appropriate statistical methods for historical social science it is worth recalling that era's tradition of mutual constitution, not as an idyllic model but as a challenge and motivation. During the second half of the nineteenth century and the opening of the twentieth, History was entwined with both Economics and Political Science, especially in locations—notably Germany and the United States—where the modern research university dedicated to systematic *Wissenschaft* was being fashioned.

Certainly, this was the prevailing feature at Johns Hopkins and Columbia, the first such American institutions. History was central, indeed a constitutive element, during the founding in 1880 of Columbia University's School of Political Science, a pioneering research and graduate teaching institution that also included Sociology and Economics as sites of emphasis but not yet separate disciplines. Reciprocally, students of politics belonged to the American Historical Association (AHA) that first met in 1884. The AHA was the very first professional association in the United States, composed by a membership quite distinct from that of the American Social Science Association that had been launched at the close of the Civil War in 1865 by non-academic amateurs.

The academy's matrix of specialization was in its infancy when the AHA was fashioned (Higham 1979). History had yet to differentiate intellectually or institutionally from Economics and Political Science. Wright (2015) describes the emergence of Economic History as a distinct subject arising out of the rejection of some scholars in the early twentieth century of the emerging neoclassical

paradigm and deductive methodological approach that would come to dominate the discipline of Economics. Economic History was largely the province of historians, with academics who studied the subject as or more likely to be in departments of History as in departments of Economics.

The frontispiece of the *Johns Hopkins University Studies* series of publications prominently displayed a locution of Edward Augustus Freeman drawn from his 1880 opening address in Birmingham, England, to the Historical Society: "History is past Politics and Politics present History" (Pierson 2015). Echoing this orientation, Columbia's John Burgess proclaimed at the 1896 annual meeting of the AHA that "Political Science must be studied historically and history must be studied politically in order to have a correct comprehension of either. Separate them, and one becomes a cripple, if not a corpse, the other a will o'-the-wisp." But heralding the division that soon would come, one colleague, H. Morse Stephens (1897, 212), a Cornell-based historian who had arrived from Scotland two years prior, specified his "astonishment amounting to somewhat of a disgust, that history [in the United States] was regarded ... rather as a handmaid of Political Science than as a subject of study for its own sake."

At the time, this comment represented a minority view. Dominant was what key practitioners called "historico-political studies," the name that, among others, John Burgess, Herbert Baxter Adams, and Woodrow Wilson used to designate their writing.[2] More than one boundary was still indistinct—not only that dividing History from Political Science, but the frontiers distinguishing Political Science from Sociology, Psychology, and Economics. Pushing too far, perhaps, but not much too far, Robert Adcock has designated the scholarship of this moment as "a single ordered field of knowledge" (Ross 1992; Adcock 2003). As an indicator, we might note that the first modern American journal of political science, *Political Science Quarterly*, founded in 1886 and housed at Columbia, was an independent journal devoted not to a single science of politics but to the various sciences of political studies, notably including history. Certainly most of its early contributors self-identified as historians.

This situated unity did not last. By 1901, the president of the AHA, Charles Francis Adams, was warning his colleagues that the developments they were promoting in History were effectively isolating and marginalizing political scientists. "That politics should find no place at its meetings, I believe," he stated, has become "the unwritten law of this association; and by politics I refer to the discussion of those questions of public conduct and policy for the time uppermost in the mind of the community" (Adams 1902, 203). A report of that year's meeting took note of the dissatisfactions of some political scientists along these lines, but concluded that there is "no danger of disruption of the larger body" (Association 1903, 421).

That complacent estimate proved wide of the mark. As historians in the late nineteenth and early twentieth centuries were developing a characteristic profile, a "model of scientific method," as Peter Novick's history of the discipline noted, that, though "rigidly factual," shunned hypotheses and explicit hypothe-

sis testing, political scientists, as well as many economists and sociologists, had been moving in an opposite direction (Novick 2007[1988], 31, 37, 56, 67). In 1903, a significant departure of political scientists made it possible to found the breakaway American Political Science Association, putting a decisive end to a comprehensive AHA. By 1908, an American Political Science Association committee accurately reported that the distinction between historians and political scientists had grown: "their points of view, aims and their methods are distinct now and getting more so" (Adcock 2003, 507).

Unity had fallen victim to situational, institutional, and intellectual trends. At a moment characterized in the U.S. by charged conflict and division—by industrial development and labor unrest, rapid urban growth, mass immigration, the institutional elaboration of mass political participation, racial violence and the emergence of a Jim Crow South, Native Indian warfare in the West, and the conquest of Puerto Rico and the Philippines—the linear developmental understanding that once had joined the disciplines came to seem simple to the point of naiveté. Institutionally, as the research university model deepened and proliferated—signified by Columbia's move to a capacious campus in Morningside Heights and by the founding of the University of Chicago that quickly adopted a departmental structure—great pressure was put on the prospect of any conjoined organizational site for History and Political Science, together with Economics and Sociology.

As History emerged as a discipline in its own terms, historians in the United States, for the most part, were moving decisively away both from amateurism and Teutonic theories of organic human development. Concurrently, a growing number of political scientists, sociologists, and economists were acquiring a strong sense of themselves as scholars who inhabited zones of thought, teaching, and research that no longer could, or should, be subsumed within the historical craft. Concerned less with the nature of history than with the character of political challenges under ever more complex and vexing conditions, they insisted that emphasizing present dilemmas is not a vice but a value.

By the 1920s, analytic Political Science, including a still primitive statistical propensity, no longer was institutionally or intellectually tethered strongly to History. By today's standard, the early discipline's rules of evidence and procedures for inference were unsophisticated, yet the direction of the period's ambitions was clear. As the Ohio State political scientist Walter James Shepard wrote approvingly and optimistically in 1925, the discipline was making "distinct progress toward a really scientific character" (Shepard 1968, 427). The key, as *The Process of Government* by Arthur Bentley (1908) had underscored, was developing a realistic, empirically-grounded understanding of practices within liberal democracy—arrangements for voting, interest representation, public opinion, and legislative behavior, each of which entailed systematic counting and an advancing mathematical imagination.

A dance of distance on a partitioned landscape followed the "migration out of History" by political scientists and the growing remoteness from social science by historians, despite economists' increased attention to historical questions.

History came to be "defined by its careful use of archival or 'primary' materials, an insistence on meticulously accurate chronology, and its mastery of narrative," and historians designated "where, and especially when, an event occurred [as] an integral part of its meaning, explanation, and impact" (Sewell 2005, 3; Tilly 1981, 14). What historians came most to appreciate is the temporality of human life. "First, and most fundamentally," the historian William Sewell has written, "I think we believe that time is *fateful*." The central qualities of time as irreversible and sequential make outcomes dependent on the temporal fatefulness of chronology. Moreover, he underscored, "as against the implicit assumption of most social scientists, that social change takes place according to smooth, gradual, predictable and linear processes, historians assume that historical temporality is lumpy, uneven, unpredictable, and discontinuous," and, one might add, heterogeneous and multiple (slow and fast, short and long term, punctuated or continuous). Further, and crucially, the rhythms of structures, events, experiences, and behaviors "are brought together in specific ways in specific times and places, in a particular sequence." In short, at the center of such analysis is "historical contextualization ... Historians tend to explain things not by subsuming them under a general 'covering' law, but by relating them to their context" (Sewell 2005, 9, 10).

1.3 POST-WAR DIVISIONS BETWEEN HISTORY, ECONOMICS, POLITICAL SCIENCE, AND SOCIOLOGY

The dominant epistemology in the social sciences has been different from, at times incompatible with, the way historians think and work.[3] Sewell, who has spent much of his career in a department of political science, notes how "social scientists tend to look for explanations in terms of a relatively limited set of enduring, entrenched, and causally powerful features of the social world," and "tend to single out what they take to be the most causally important features of the world and to elaborate their dynamics systematically" with an insistence on analytical and methodological clarity based on high regard for systematic modes of inquiry, deductive, inductive, and experimental (Sewell 2005, 14).

As they look at each other today, many practitioners of each craft, scholars with dissimilar impulses and orientations, distinguish important differences. For many historians, the fact that "history is a field of study ... in its lack of overall structure or definition," less "tightly bound to canons or bodies of knowledge, to technical methods, or both," thus more eclectic than the various social science disciplines, is a source of strength and vibrancy (Maza 2017, 1, 2). Just this quality is a source of concern for many social scientists who are suspicious, not without reason, about the often casual and seemingly idiosyncratic considerations about causality within many narrative works of history, even when justified by claims of uniqueness based on the obviously correct understanding that particular historical situations are never precise replications of earlier ones.

Reading with a critical eye, social scientists observe an absence of explicit reflection about which elements within complicated narratives are doing the heavy lifting. Instead, as Sewell observes, historians often

> suffer from a kind of narrative overconfidence. When they reach tight spots in their arguments, they tend to try to narrate their way out of trouble, going back to the sources for yet more detail, laying on more and more examples, instances, and anecdotes. This often means that important conceptual questions—about temporal dynamics, about causation, about the relations between events or entities—tend to get lost in a welter of narrative detail rather than being addressed at the appropriate conceptual level (Sewell 2005, 11–12).

Most historians, in turn, who favor the particular rather than the general or the portable as they recognize and organize past events and ascertain their meaning are reciprocally suspicious, not without reason, when they witness the mining and quantitative analysis of data in ways that flatten the richness of the past. Too strong an emphasis on causal inference, they believe, can inappropriately simplify complex processes and sacrifice the quest to comprehend large and significant historical puzzles in favor of methodological rigor. They also view the recent turn to experimental methods, including the search for natural experiments in times past, with suspicion, worried about a narrowing of the questions being asked, and the replacement of historical depth marked by complex causation with a narrowing passion for precise estimations of particular factors. Random assignment of persons regarding the factor being studied makes experimental researchers more confident about inferring causality, but people are for the most part not assigned by chance in the historical world in ways that provide purchase on profound questions. The location of persons and groups in the economic, social, and political order, rather, itself is shaped by complex mechanisms of assignment.

Historians want to know how distinctive situations shape probabilities. Combining evidence with the art of interpretation, they compose chronicles at different scales that privilege context, temporality, and sequence. Because they want to go beyond what and when questions to those that ask why, their explanations are also plainly or obliquely causal, not by finding a single key variable or mechanism but by offering constellations of factors whose combination guides explanations for human experiences and choices by persons placed in specific locations, from the local to the global, over determinate spans of time, from short moments to long epochs.

What many economists, sociologists, and political scientists apprehend, and sometimes deprecate, are historical narratives in which attention to method and the verification of claims takes the dominant form of literary persuasion. By contrast, historians who attend to Economics, Sociology, and Political Science often are bewildered by their kitbag of methods, many of which appear arcane and excessive as they test the explanatory power of particular isolated variables.

They also are put off by uses of history as data to be exploited without humanistic learning or the understanding they prize. They become apprehensive when they witness attempts to transcend the particularities of situations and when they see the instrumental power of a given factor revealed by way of theories, models, and hypotheses applied to a wide array of historical situations without much attention to their diversity or complexity.

As confidence in causal identification among quantitative social scientists grows with more advanced methods, and as historians have doubled down on sources and historiography as dominant disciplinary features, the gaps between History and social science and between qualitative and quantitative orientations have widened.

Seven decades after Max Weber underscored the emerging trend of strong disciplinary patterns of separation, Heinz Eulau decried the "wishful thinking in the assertion that there is 'the need to marry the particularism of the historian with the robust, organizing generalization of the political scientist,'" in a critique of the historian Joel Silbey's effort to do just that (Eulau 1993; Silbey 1989). Twenty years later, Gary Goertz and James Mahoney (2012) portrayed a "two cultures" division between quantitative and qualitative scholarship, underscoring how designs for research in each domain possess distinctive rationales, norms, methods and practices. Logic and set theory, which they designate as core, if often implicit, orientations of qualitative social scientists, vie with inferential statistics, the bread and butter of quantitative scholars. Goertz and Mahoney thus reject the idea that a joint logic of inference can or should drive research, the very argument that Gary King, Robert Keohane, and Sidney Verba famously put forward in (1994), a book advising qualitative scholars to apply the rules of inference found in regression-centered quantitative scholarship.

Like Goertz and Mahoney, we are mindful that certain enduring differences divide fields and research traditions. The qualitative group is especially attentive to scope conditions, sometimes to the point of uniqueness; the quantitative to the development of hypotheses that are as broad and portable as possible across locations and moments, sometimes to the point of timelessness and spacelessness.

This, though, is something of a reified contrast. When the political sociologist Philip Abrams reviewed a joint 1968 effort by the historian Richard Hofstadter and the sociologist Seymour Martin Lipset to build connections between their disciplines, he insisted that ultimately and fundamentally there are no impassable distinctions dividing History and the social sciences. "Reasons of academic convenience apart," he wrote, they "are not separate or even intelligently separable activities," and that "the differences between them are largely fortuitous or *ad hoc*, matters of style or emphasis or technique." From this perhaps overly optimistic perspective, Abrams cautioned against disciplinary fragmentation and argued that variations in method need not connote more than superficial divisions (Lipset and Hofstadter 1968; Abrams 1971, 118).

As much as we would like to strain to agree, and as much as we want to advance collaboration when historical understanding is at stake, the significant

differences in practices and sensibilities that Sewell and others have identified cannot be wished away. The disciplines have reason to be suspicious siblings (Cavezza 2017). Combining disdain with incomprehension, historians rarely have sought to reform the social sciences. Social scientists have been less reserved. As examples, the Social Science Research Council (SSRC) sponsored a series of initiatives culminating in 1946 and 1954 reports of the Committee on Historiography on theory, practice and the role of social science, and a 1963 volume by the Committee on Historical Analysis about generalization in history. Each aimed to deploy social science techniques to improve the historical craft (Social Science Research Council 1946, 1954, 1963). This impulse had been heralded by a 1940 SSRC assessment of Walter Prescott Webb's *The Great Plains* (1931), published in the Council series *Critiques of Research in the Social Sciences*, whose "evaluation committee included such giants of the social sciences of the epoch as Louis Wirth from Sociology, Robert Redfield from Anthropology, and Arthur M. Schlesinger and Roy Nichols from History, ... strong allies of interdisciplinary work in history" (McDonald 1996, 102–103). Drawing on the volume's critical commentary, Wirth took the opportunity to lecture historians about the status of facts, which, he wrote, "are made; they are not just found" and "are always made in the light of some hypotheses." From this perspective he upbraided the historical profession for being "not explicitly aware of the theories upon which they proceed and therefore naively conclude that they have none" (Council 1946, 189, 187).

The subsequent Council volumes on history and the social sciences identified remedies aimed at making historians aware of the strategies, methods, and findings of the social sciences. Notwithstanding their recognition that the historical craft is an art, the SSRC authors called for a more self-conscious selection of theory and explicit hypotheses and for more efforts to systematize and classify historians' findings. These efforts to shift the balance within history from the humanities to the social sciences and emphasize the advantages of social science history and motivate historians to contribute to generalization beyond particular cases proceeded without much openness to learning that might travel in the other direction (Challener and Lee 1956).

This one-way approach, the London School of Economics sociologist Douglas G. MacRae, a student of ideology and society, observed in a tart review of the 1954 report, *The Social Sciences in Historical Study*, "is a little shocking," in its

> bland attempt, implicit throughout these pages, to teach the historians their business. ... Cooperation between historians and social scientists is desirable and surely possible, but will not be achieved by mounting the sociologist, booted and spurred, on the back of the historian. If this is attempted, the social scientist will inevitably get a nasty and well-deserved toss (MacRae 1954, 375).

Such a toss was not long in coming. Many historians and historical social scientists blanched at what they often rightly believed to be too simple—and simplifying—ways of working on the past (Gaddis 2002; Trachtenberg 2006; Tilly and Goodin 2006). An irony was present. The SSRC's work on historiography upbraided historians for failing to deal appropriately with temporality: "The truly scientific function begins where the descriptive function stops. The scientific function involves not only identifying and describing temporal sequences; it also involves explaining them" (Social Science Research Council 1954, 86). But this was precisely the basis for the dismay of many historians when they read social science considerations of historical evidence, particularly put off by approaches to measurement and analysis that flattened temporality and sequence by treating behavior as equivalent under rather different historical conditions, and that failed to distinguish moments by degrees and dimensions of uncertainty.

The tropes of the post-war SSRC reports found expression in *Designing Social Inquiry* by King, Keohane, and Verba (KKV), who sought to bring the sensibilities and practices of quantitative researchers into the qualitative domain. Directing qualitative researchers to improve the scrupulousness of their scholarship by attending to matters of inference that are conventional in systematic mainstream statistical research, KKV wished "to encourage qualitative researchers to take scientific inference seriously and to incorporate it into their work" (ix). One result of their intervention was a series of clear advances in qualitative work. These include clarity about case selection, concern about selecting cases on the basis of dependent variables, ensuring a meaningful degree of freedom between independent and dependent variables, and "a greater sensitivity to the universe of relevant units and to the limits of generalizing about the external validity of findings" (Schmitter 2016, 400).

Their very subtitle—"scientific inference in qualitative research"—signalled commitment to a united social science based on a definition of causality in which the "*effect is the difference between the systematic component of observations made when the explanatory variable takes one value and the systematic component of comparable observations when the explanatory variable takes on another value*" (King et al. 1994, 81–82; italicized in original).

This landmark work's quest for similar research designs in qualitative and quantitative work includes ideas to enlarge the number of observations and address problems of endogeneity, selection bias, and measurement error. These issues, KKV insisted, are ubiquitous, though in qualitative work they may be less apparent due to the absence of formalized empirical models that reveal their presence. Arguing, contrary to Goertz and Mahoney and much like Abrams, that "the logic of good quantitative and good qualitative research designs does not fundamentally differ," they articulated hopes that a "unified logic of inference ... can be helpful to qualitative researchers" (ix).

Our view does not fall squarely in either camp. Rather than blur boundaries, we engage at the frontier by featuring aspects of methodological craftsmanship that can improve open-eyed engagements. Goertz and Mahoney wanted to promote better understanding of often deep methodological differences, listing as

many as twenty-five dimensions of dissimilarity concerning dispositions in each camp, including the status of individual cases, the construction of models, qualities of population and data, concepts and measurement, and the manner in which they deal with counterfactuals (Goertz and Mahoney 2012, 221–223). Perhaps unintended is the implication that overcoming the qualitative-quantitative divide may be impossible. Otherwise, however, each type of social science misses analytical and empirical opportunities.

By contrast, King, Keohane, and Verba sought to reform qualitative practices to create a single social science committed across approaches to rigorous causal analysis. Their appeals proved successful in raising the standards of qualitative work by raising self-consciousness about method and inference, but not without cost. Their ambitious program imposed something of a straightjacket on qualitative endeavors and effectively built a one-way street, eliding important distinctive concerns and contributions by historians and historically-oriented social scientists.

The alternative is not to insist on distinctiveness or identity, but focus on the orientation to research advanced by Sidney Tarrow (2010, 101) to the effect that KKV "ought to have paid more attention to the *relations* between quantitative and qualitative approaches and what a rigorous use of the latter can offer quantifiers."

That is precisely what we wish to accomplish. In order for benefits in research to flow in both directions, qualitative approaches must inform quantitative methods. Our core emphasis thus underscores how the central orientations of the historical craft should guide and can enrich statistical scholarship.

1.4 POSSIBILITIES

Like KKV, we are advocates and reformers. Like them, we also are committed to the idea that, unlike in fiction, claims made by social scientists and historians can be disproved.[4] The primary aim of this book, however, is to invert their line of emphasis. We keenly wish to persuade quantitative colleagues to incorporate the core orientations of historians and historical social scientists, going beyond research characterized by "mixed methods" that oscillates between two epistemologies, and to aim at fashioning a more synergistic analytical history. That pursuit cannot erase or flatten existing impulses that will continue to differ. Rather, the degree of intellectual integration will vary by question, project, and researcher.

What we should like to do is open the prospect of partial but deep mutual constitution where it most makes sense, without underestimating persisting differences between modes of inquiry. Any intellectual investment to move the realities of scholarship closer to the erasure of disciplinary boundaries will fail unless it takes into account such prevailing patterns of difference. "The problem," as the historian David Potter put the point, "is not simply one of bringing

into conjunction bodies of knowledge which lie at separate areas on the same plane, but is one of establishing relations between bodies of knowledge which lie on different planes" (Potter 1955, 80). With that caveat, our objective complements that of in suggesting how to establish such relations, here with the arrow of influence pointing the other way; that is, to reform quantitative practices in ways informed by qualitative approaches.

Like KKV, we want to advance the self-awareness of researchers and offer tools to better accomplish their distinctive tasks. But the direction of our effort inverts theirs. Unlike their recommendations that treat as normative the mainstream assumptions and ways of working in quantitative social science, our point of departure is the inadequacy precisely of these conventional ways regarding systematic work on historical materials.

KKV critique qualitative work that fails to meet the standards of statistical scholarship. We sally forth by showing how qualitative scholars highlight the limitations of much extant quantitative scholarship for serious historical work. Statistical research, we aim to convince, would produce more reliable and compelling evidence and inference if those who practice it—especially those who practice it from a historical perspective—were to pay attention to the standards and concerns of qualitative researchers. Thus the core challenge of our project is to move beyond preaching to operationalize this view through modeling and estimation techniques that ensure sufficient attention to relevant criteria for historical understanding.

These observations, ambitions, and intuitions are indebted to others who have sought to transcend linear models that are too simple (Büthe 2002; Lieberman 2002). Writing nearly three decades ago, the sociologists Larry Isaac and Larry Griffin (1989, 873) argued that much research featuring univariate time series relies on three flawed premises that "(1) separate theory from history, (2) use ahistorical conceptions of time, and (3) presuppose methodological autonomy and privilege statistical theory over historical process."

The generalizability of theoretical claims and hypothesis testing, they observed, tends to be prioritized over concerns about the special character of historical investigation and data. Theory is thought to be best when it remains abstract in the purest sense, "uncontaminated by history"; nor should history be "mediated by theory" (875). This relationship leaves history not theorized as a central component of social scientific investigation; consequently, "time-conditionedness" and "temporal contingencies" are ignored (875).

Time itself is treated as ahistorical, as "a smooth homogeneous magnitude viewed as external to the events and relationships of history, except as an index of chronology" (876). While quantitative time series analysis requires indexing by time, treating time as little more than continuous chronologically-ordered events leads to a consideration of time as homogenous, and by extension, to a homogenizing of empirical relationships across different historical contexts. Further, researchers assume that design considerations are equivalent across different types of studies. As a result, concern about such matters as sample size and selectivity bias tend to trump concern about the historical character

of the analysis. The desire to obtain statistically significant results, to invoke asymptotic justifications for the validity of results, and to avoid charges of selectivity bias leads researchers to want to maximize the length of their time series and not separate them into subseries that may more accurately represent distinct temporal periods.

Letting these concerns dictate design potentially introduces bias into the analysis as observations that are governed by different data generating processes are shoehorned into homogenous models that frequently are excessively straightforward. Here lies a significant difference between our work and that of KKV. *Designing Social Inquiry* argues that simplification through abstraction is one of the key features of good social scientific work; "social science generalization", they argued, "depends on our ability to simplify reality coherently" (93). Reflecting on how qualitative work privileges complexity whereas quantitative work privileges simplicity and parsimony, they opt for the latter. Simplicity makes it easier to determine whether research meets quantitative standards of causal inference.

To this end, they advocate a focus on units that are "homogeneous," which they define as follows: "Two units are homogeneous when the expected values of the dependent variables from each unit are the same when our explanatory variable takes on a particular value" (91, emphasis in original removed). Or, stated another way, "For a data set with n observations, unit homogeneity is the assumption that all units with the same value of the explanatory variables have the same expected value of the dependent variable" (91). KKV do acknowledge that units in practice will differ in some ways, but implore that units must be the same on average over many hypothetical replications of a given sample. In the absence of unit homogeneity, they contend, causal inference becomes more difficult and perhaps impossible, since "the notion of unit homogeneity ... lies at the base of all scientific research" (93).

The unit homogeneity assumption is in tension with what KKV state is "some of the most useful advice [they] have for qualitative research ... to expand the number of observations by looking for more instances in subunits or by considering instances over time" (221). Increasing the number of observations risks introducing heterogeneity into the sample, a possibility KKV acknowledge. Thus they caution researchers to take care not to compromise the scientific integrity of their work when they move in this direction. They focus more on expansions across space that incorporate additional comparative cases or examine subsets of geographic units than they do on expansions over time, making it is less easy to discern how they suggest negotiating this tension in historical work.

Quantitative historical researchers generally do not press a concern to increase their sample sizes as the scope of history they incorporate into research designs is problem-driven. For example, investigating the impact of institutional changes requires looking at periods before and after those changes. But they frequently treat their models as if the unit homogeneity assumption holds for the entire time span of data because there is no differentiation among units in

terms of the relationship between explanatory and dependent variables. In this way they elide core concerns of historians about the distinctiveness of contexts and situations whether they confine their work to a well-defined historical scope or seek to increase the number of observations by expanding the analysis over time.

Central to this book is our argument that historical quantitative researchers should relax the unit homogeneity assumption, but in ways that are deeply theoretical. KKV acknowledge that while "attaining unit homogeneity is often impossible—understanding the degree of heterogeneity in our units of analysis will help us to estimate the degree of uncertainty or likely biases to be attributed to our inferences" (93–94). Heterogeneity is viewed pejoratively—something that is likely to bias inferences and increase uncertainty and thus limit what we can understand about the world.

Our view, by contrast, is that the degree of heterogeneity in the units of analysis is something to be theorized, modeled explicitly, and exploited. Discounting or rejecting analytical units that might lead to heterogeneity in the data precludes reconciliation of quantitative analysis with traditional historical investigation. While accommodating heterogeneity in our models no doubt increases uncertainty in our parameter estimates, a more fruitful response is to seek improvement in estimation approaches that can accommodate the additional uncertainty that is introduced. Heterogeneity can be treated as a systematic feature of the data and estimation approaches can be adjusted accordingly. Indeed, we would push this point further and argue that unit heterogeneity is a desirable thing and should be made a constitutive part of historical analysis. Instead of perceiving it as limiting an analysis, embracing unit heterogeneity and adjusting models to accommodate it is essential for the empirical component of historicizing theory.

Perhaps part of the reason KKV portray heterogeneity in such a negative light is because their discussion is largely restricted to basic regression theory and techniques. Such a move makes sense given that their target audience members are not characterized by strong quantitative backgrounds, which necessitates keeping mathematical representations of key concepts as simple as possible. Indeed, one of the main thrusts of their argument is that the language and notation of quantitative analysis can communicate more clearly where qualitative analysis falls short of scientific standards. But this creates a dilemma because it obscures ways that basic regression techniques can be augmented to account for complexity and the multifaceted nature of historical processes. KKV argue that "Complexity is likely to make our inferences less certain but should not make them any less scientific" (10). By moving beyond basic regression techniques, we can also deal with additional uncertainty and maintain the scientific integrity of our inferences. The development of the application of such techniques to historical analysis is a key contribution of this book and one that serves KKV's overarching goal of narrowing the divide between qualitative and quantitative research, yet from an angle not anticipated by them.

As we have seen, the project of deepening the instruments employed by historically-oriented political scientists faces a healthy degree of suspicion, especially from historians, area-studies-oriented scholars, researchers in American Political Development, and historical sociologists. Although their various apprehensions—to the effect that when researchers proceed by utilizing models that employ and impose a potentially inaccurate homogeneity on a long time series and thus do not theorize about the historical aspects of the data—are not identical, together they pose significant challenges that require attention and remedy.

As these are our concerns, we thus invite colleagues who work in various quantitative traditions to infuse their work with the orientations and habits most historians take for granted in order to gain command over context and temporality. Like Isaac and Griffin , we support an "historicized quantitative analysis" that involves ascertaining the temporal stability of time series coefficients without positing by theory rather than empirical research the location of structural breaks; building periodization that is both theorized and justified by structural shifts among relationships revealed in the data; and restricting theoretically grounded historical generalizations to "contexts or periods having more or less the same historical structural regularity"—that is, "to those particular contexts with near temporal invariance" (885).

In so doing, it is useful to recall that not just social scientists but historians have conducted quantitative historical analysis at least since the late nineteenth century. These early studies employed descriptive statistics to better understand election outcomes and roll calls. Addressing the AHA in 1897, Orin Libby called for a quantitative approach to the study of votes in the U.S. Congress to determine representatives' views—a shift from the conventional approach of studying speeches for this purpose. Libby contended that a systematic investigation of roll calls would be superior because silent majorities and vocal minorities can distort our understanding of members' positions. He discussed cases where vote outcomes led to inferences about preferences that differ from what speeches could make known, and he particularly noted how mapping votes revealed changing sectional patterns of position regarding the tariff, internal improvements, and the national bank (Libby 1897).

By the mid-twentieth century, some historians had begun to collaborate with political scientists and sociologists to build extensive data archives of election results and congressional roll calls based on a commitment to the value added by systematic quantification to central historical problems, though as late as "1970 there were no more than three or four dozen historians on American faculties who had a grasp of statistics up to multiple correlations and regression analysis" (Drake 1973, 395). Analysis of large data sets proved important to the era's "new political history."

The marginalization of Economic History in the discipline of Economics reversed with the emergence of the "new economic history" and cliometrics in the 1950s and 1960s. This movement, which sought to deploy the modern theoretical and empirical tools of the discipline to study the past, brought a rigor that

had been lacking in the field of Economic History and helped to reshape our understanding of fundamental and expansive questions about economic growth and its impact on social welfare. Through the reevaluation of existing data and the gathering of new treasure troves of quantifiable information, cliometricians often rejected the conclusions that had been reached through more qualitatively-oriented approaches. But this scholarly progress did not come without substantial controversy. Slavery in the U.S. was a primary focus of the new economic historians and their findings—about the enduring profitability, productivity, and viability of slavery, the centrality of slavery to the entire U.S. economy, and the material conditions of enslaved peoples being much less harsh than previously thought—generated substantial backlash (Meyer and Conrad 1957; Fogel and Engerman 1974; Fogel 1989; North 1966). Critics, motivated in part by what they viewed as normatively compromised conclusions, took issue with the research designs and methods used to reach them. As C. Vann Woodward (1974) put it,

> Confronted with equations they cannot read, with techniques they cannot understand, with copious data beyond their comprehension, traditional historians have reacted defensively and belligerently themselves. They see their authority challenged, their humanistic values threatened, their canons of criticism ridiculed, and their cherished classics derided as "soft," impressionistic, and unscientific.

The new economic history made a tremendous impact on the discipline of Economics, as cliometricians became highly sought after for positions in departments of Economics. Despite earlier interdisciplinary efforts, few historians embraced the cliometric approach despite favorable conditions and concerted, systematic efforts to build an academic infrastructure for training students to be "new economic historians." Books like *An Introduction to Quantitative Methods for Historians* (Floud 1973), *Historian's Guide to Statistics* (Dollar and Jensen 1971), and *The Historian and The Computer: A Practical Guide* (Shorter 1971), all published by top presses in the early 1970s, sought to reorient the mindset of historians with respect to methods. Access to mainframe computers and the development of statistical and mapping software dramatically enhanced the ability to collect, store, and analyze large data sets. Quantitative historians sought to institutionalize these efforts by introducing more comprehensive quantitative training in their departments. At the University of Chicago, economic historian Robert Fogel led an effort to develop a systematic and comprehensive training program in mathematical methods for History PhD candidates. By the mid-1970s, Fogel (1975, 332) characterized the "progress of the mathematical approach in the mainstream of history" as "substantial," while acknowledging that most historians remained dubious that the most significant historical puzzles could be probed by quantitative means. As evidence that change was coming, notwithstanding, to the mindset of historians, Fogel cited the awarding

of the Bancroft Prize in 1974 to Thernstrom's *The Other Bostonians* (1973), which centrally featured quantitative methods.

But the change that Fogel predicted did not come to pass. Efforts to recruit and train a new generation of quantitative historians largely stalled by the 1980s. Early in that decade, Charles Tilly, who sought keenly to build bridges between History and the social sciences, nonetheless took note of emerging conundrums. While celebrating how an increasing number of social scientists had come to work earnestly with historical data and how, in turn, a growing number of historians had come to use social science methods in their research and writing, he detected significant pitfalls, above all the fact that the meeting ground of the disciplines was not substantive or theoretical but methodological, largely in the form of borrowing, much uncritical, from technical innovations and statistical techniques to history from within the social sciences. He warned, cogently, that quantification as such is not the essence of the issue, for quantification can only advance historical understanding when it is part "of a much larger analytical apparatus—an apparatus of deliberate conceptualization, explicit modelling, painstaking measurement, and self-conscious comparison." Further, he observed how then "available quantitative models and statistical techniques are inadequate to deal with many of the more-or-less statements, which do, indeed, abound in historical argument" (Tilly 1981, 27–29, 34).

As it turned out, quantitative historical analysis failed to deliver on many of the promises advanced by its early proponents. Subsequent research uncovered technical errors that cast doubt on important findings from that era. But what appears to be poor or overly simple execution of quantitative inquiry was cast as a fundamental and fatal flaw for this type of research. While replication with new and better methods, possibly leading to different results and inferences, is essential to progress in any discipline, critics of quantitative methods seized on the problems with early quantitative work by historians as a way to condemn the entire enterprise. More importantly, these disappointments, we believe, primarily were generated by a credulous tendency to transfer approaches from ahistorical social science to historical questions without necessary adjustments. Many of the most assertive quantitative historians came up short precisely because they did not adapt or develop quantitative methods more sensitive to the craft of history (Benson 1984).

The slowing of the new economic history movement after the initial novelty wore off led to existential angst and some of its practitioners to question by the 1980s "What went wrong with the cliometric revolution" (Rutten 1980). Douglass North, arguably the most important cliometrician, came to the conclusion that the field had ultimately failed to add a new dimension to Economics (Lyons et al. 2008, 211). Solow (1985, 331) bleakly stated that "Economics has nothing to learn from Economic History but the bad habits it has taught to Economic History."

Quantitative history then found itself in a difficult position. Its initial contributions were flawed in part for the methods employed, but that moment's state of statistical and econometric theory, statistical software, and computing power

made it nearly impossible to engage in a quest for methods that could have allayed the concerns of those who viewed quantitative work as ignoring essential complexity. Even as some historians like Gavin Wright stressed how quantitative approaches could be married to a view of the economic world in which historical time plays a fundamental role, the new history movement withered in the absence of the creation of methods distinctively suited to the study of history (Monkkonen 1984). In Economics, the study of history went mainstream, with an erosion in the distinctions between economists working on historical subjects and other economists in terms of types of questions pursued, research designs, methods employed, and publication outlets (Margo 2018).

Quantitative work by historians, moreover, had to deal with fierce resistance by a great many colleagues. Writing in 1962, during his stint in the Kennedy White House, Arthur Schlesinger, Jr., averred in the pages of the American Sociological Review that he was "strongly prejudiced in favor of empirical social research," and conceded "how dismally written history lacks in rigor, how impressionistic the historian's analysis so often is, how imprecise his generation, how loose his language, how literary his whole style of attack.," Nonetheless, he strongly raised doubt about what he called "the formidable effort to make [history] surrender to quantitative solutions." He particularly lambasted those who would dismiss "a whole range of historical issues which happen not to be susceptible to quantification," warning of a "false precision" and leading him to insist that "almost all important questions are important precisely *because* they are not susceptible to quantitative answers" (Schlesinger 1962, 768, 770; emphasis in original). Likewise, writing a few years later, the English historian G. Kitson (Clark 1967, 21) expressed doubt about the growing prevalence "of historical and sociological thought ... with scientific pretensions, which are fortified by the use of an elaborate, heavily latinized, technical vernacular in which statements can be dogmatically and impressively made which are commonplace, or unprovable, or frankly meaningless."

The history profession soon witnessed a drumbeat of comparable misgiving. Passions ran deep. A presidential address at the AHA by Bernard Bailyn cautioned against "the mind-absorbing, soul-entrapping" qualities of technical quantification, warning how they "can destroy the foundations of historical understanding by limiting questions to available numerical answers, by endowing with a spurious rigor claims that have no basis in fact, and by diverting attention from the central themes of an evolving inquiry" (Bailyn 1982, 9). A comparable screed against historical quantification by Lawrence Stone lamented how cliometricians "are defined by a methodology rather than by any particular subject-matter or interpretation of the nature of historical change." Proclaiming "scientific history to be a myth," he criticized "historians who build paradigmatic models, sometimes counter-factual ones about worlds which never existed in real life, and who test the validity of the models by the most sophisticated mathematical and algebraic formulae applied to very large quantities of electronically processed data ... expressed in so mathematically recondite a form that they are unintelligible to the majority of the historical profession." Stone's

complaint was not with exacting descriptive data ("historians can no longer get away with saying 'more,' 'less,' 'growing,' 'declining,' all of which logically imply numerical comparisons, without ever stating explicitly the statistical basis for their assertions") but with what he believed to be the hubris of quantifiers seeking to displace historical narration by "scientific" forms of explanation (Stone 1979, 6, 10, 13).

Our book risks being characterized, perhaps dismissed, in this way. Its goal, though, is not to replace the best qualities of the historical craft but to resonate with their character and priorities. The reservations expressed by Schlesinger were grounded in the suspicion that "the social sciences deal with short statistical runs" and are characterized by the quest for trans-historical determination that downplays both particularity and larger historical processes. If his impressionistic account were entirely correct, we, too, would share these deeply-held reservations; indeed, quantitative work too often has fed just such doubt. But Schlesinger's critique and others like it elide how quantitative work properly deployed can illuminate some of the biggest historical questions when they face up to challenges of context and periodicity, temporality and specificity.

Unlike colleagues who have largely been dismissive, many prominent historians have sought an engagement with systematic social science, including its quantitative dimensions, in full awareness that such connections raise complex challenges about the general and particular, analysis and narration. More than a century ago, in 1912, the historian James Harvey Robinson promoted "a new history" at the juncture of history and the social sciences. Just over four decades later, not long before Schlesinger addressed sociologists with his concerns, another leading American historian, Richard Hofstadter struck an inviting tone. Writing about history and the social sciences at a time when the application of statistical models to historical analysis yet was in its infancy, he aimed to develop forms of analytical history that could either complement or invigorate traditional historical narration (Hofstadter 1956).

Some historians, of course, think that quantitative social scientists spend too little time in archives or with secondary sources written by historians. But these are relatively superficial matters. After all, there is no reason scholars cannot exercise more due diligence with historical evidence or broaden their libraries (Kreuzer 2010). More important are lines of criticism and fundamental challenge posed by scholars who take the social sciences seriously, and who, like Hofstadter and the pioneering French historian Fernand Braudel, wish to develop analytical history as a craft.

To that end, Hofstadter's essay on "History and the Social Sciences," urged his colleagues in History faculties to proceed by "combining traditional history and the social sciences" in a manner that "will differ from the narrative history of the past in that its primary purpose will be analytical," and "will be informed by the insights of the social sciences and at some point will make use of methods they have originated." Such histories, he also advised, should "differ from the typical historical monograph of the past" in order to "focus on types of problems that the monograph has all too often failed to raise" (Hofstadter 1956, 363).

Expounding this perspective in an essay carrying an identical title, Braudel precisely reversed Schlesinger's lament to insist that quantitative social science promised to enlarge, not shrink, the temporal range. "All historical work," he wrote, "is concerned with breaking down time past, choosing among its chronological realities according to more or less conscious preferences and conclusions." It was "traditional history" that truncated time, "with its concern for the short time span, for the individual and the event" in its "headlong, dramatic, breathless rush of its narrative." Noting pioneering work in Economic History, including empirical scholarship in the Kontratiev tradition, Braudel observed how work on price curves, demography, wage series, and variations to interest rates, the money supply, and productivity demanded fresh approaches to mechanisms and cycles of duration and temporal progression. Taking the example of the statistically-based overturning of long-established propositions about such matters as the profitability of slavery under different conditions, Braudel observed how key traits of this body of scholarship within social science were creatively differentiating among moments of more or less fairly fixed structural patterns of organization and relationships. This writing also underscored constraints and opportunities inherited from times past, and was sensitive to geographic scope conditions. In all, it directed scholars to "the different kinds of historical time." If "the historian can never get away from the question of time in history," he wrote, here lay instances in which deeply quantitative scholarship was advancing "richer conceptualizations of time and its mechanisms" (Braudel 1980, 27, 29, 31, 33, 47).

Braudel and Hofstadter were well aware, of course, that history and the social sciences possess distinctive qualities, irrespective of any overlap and ultimate conjoint purposes. They understood that, within the scholarly division of labor, economists, political scientists, and sociologists are less focused on period-specific narratives of "what happened" than attuned to theoretical and empirical models that are fashioned to discern causes and mechanisms. They also recognized that sometimes the discrepancies between the practices of the different crafts are too extensive to permit reconciliation. When social scientists seek generalizations based on work with large populations of instances, their purposes grate against the sensibilities of colleagues attuned to distinct cases. But what is most of interest to us are the means by which self-conscious efforts can be made by a wide array of social scientists to grasp historical subjects and materials in a manner that respects the historical craft, thus bridging disciplinary differences creatively and productively.

We take heart from Kitson Clark's emphasis, even when "account has been taken of all these reservations," on systematic inference based on interpretation and analysis without abjuring abstraction and generalization, and his appreciation of how the results of detailed research and of quantification began [in the late nineteenth and early twentieth centuries] to bite deep into the prevailing myths of history and in some cases to support a new general picture more firmly based on methodical research." Observing that the importance of "quantification can hardly be exaggerated" as a means to transcend distortion, approxi-

mate truth, and search for causation, he advised that historians gain at least rudimentary statistical training, simultaneously cautioning that quantification, however elegant, "is no adequate substitute for the historical imagination and the historian's insight" (Clark 1967, 177, 180, 193). We fully agree. Likewise, we observe Bailyn's concession a decade after his fervent address conceding, if grudgingly, that "I don't see why quantification must ignore time and context" (Bailyn 1994, 35). Neither do we.

Yet even within communities of historians comparatively well disposed to social science, those least put off by the inherent dissimilarities of the disciplines, and most sympathetic to this book's project, we find cool reserve and bracing warnings. An instructive example is John Lewis Gaddis's *Landscapes of History* (2002), a book that chastises social scientists when their work insufficiently attends to specificity, context, and time. Regarding specificity, he offers the image of mountains. An endless variety of shapes and types does not exist. Every mountain is constrained by the logic of geometry and the limits of geology. Notwithstanding, every actual mountain possesses distinct aspects. None is exactly like any other. Similarly, history is restricted to a limited range of possibilities by an array of structural constraints. But as with mountains, specificity matters. "Causes," he admonishes, "always have contexts, and to know the former we must understand the latter" (Gaddis 2002, 83, 97).

Taken seriously, this maxim implies that particular factors or variables gain potency only within a given situation, not independently of it (Weingast 1995). The past cannot be foreshortened as an older version of the present, or simply as its antecedent. This cautionary lesson does not dispose of more general claims about the logic of things like the geometry of mountains, but it does insist that causal relations alter as parameters change. Gaddis properly worries that social science, including historical social science, often overlooks shifting conditions.

When seeking generalization and portability, and when adhering to norms and practices that especially value linear models, social science strategies of measurement frequently discount shifts in context and ride roughshod over distinctive terrain. As parameters alter, variables do not always retain comparable meaning. Their values and capacities can alter when emplaced inside different circumstances. Not just their causal power, but even their very content and meaning can alter with shifts to context.

Time, Gaddis further observes, is also causative. He insists that we have to be sensitive to the heterogeneity of factors in time, noting the "distinctions that have to be made ... between the immediate, the intermediate, and distant." Nor is each type or each moment simply equivalent. Units of time, of course, are standardized in order to be counted and measured, but the character and meaning of time itself—including its scales and rhythms—vary by context and situation. Understood this way, each unit of time is not just like any other (Gaddis 2002, 95). We periodize as a means to understand social complexity, distinguish critical moments when more open possibilities exist and vectors of causation alter in their degree of influence. These aspects of temporality are especially vulnerable to being neglected when scholarship is embedded in ways

of thinking about the world that are shaped by linear models that flatten the various dimensions of time (Abbott 2001, 37–63).

Our moment, we believe, is ripe for productive engagement. History and the social sciences presently share an unprecedented degree of ontological and epistemological pluralism. Their center has not held. Intense subgroups within each discipline distinctively combine substance and method, often with mutual incomprehension. Such deep variety opens prospects to fashion an overlapping consensus on behalf of analytical history across disciplinary lines that does not require historians and other social scientists to perfectly align, but to only discern a location for collaborative endeavors.

We thus begin with the view that despite grounds for suspicion across the history-quantitative social science divide as well as the great variety of approaches within disciplines, practitioners in each share key traits that can bind a common intellectual community and undergird the kind of quest we are pursuing in this book. For we all are modelers. We all are concerned with causes. We all grapple with situations.

The claim that we are all modelers is not that all social scientists conduct modeling as "a deductive exercise, which begins with assumptions and concludes with predictions," which is how Jon Elster identifies this kind of work (Elster 2007, 460). Rather it is to aver more broadly that social science analysis must, arguably always, construct partial shadow worlds, models of a reality that is too byzantine, too filled with structures, experiences, dispositions, and behaviors to apprehend fully, all at once. Many narratives, many consequential interactions are occurring simultaneously. As we are not gods, we necessarily choose and simplify to reduce social complexity. Combining imagination and science to identify which, and how, elements move together, where mutual influence lies, and how narrative lines operate, social science models that range from the highly condensed and conceptual to the more concrete via willful empirical selection, are abstractions from reality. In that, they resemble Cubist portraits like those Picasso painted of his art dealers in 1910 that portrayed limited numbers of elements as shapes that could be recombined and assembled to depict actual persons in a manner more analytical and more "true" than standard figurative representations. Modeling brings together features and factors that allow us to see human reality in ways otherwise not seen or understood with comparable clarity. As such, they are not "real," but propositions about what is real and which features of the world possess greater or lesser influence.

Even when historians and social scientists are more oriented to understanding than explanation, they cannot escape the ambition to discover and announce claims about causality. "In making sense of the past," wrote the quite traditional English historian G. R. Elton in a chapter on explanation and cause in a book appraising the principles and practice of political history, "the historian not only tries to discover what happened—and all that those two brief words involve—but also how things that happened were linked one with another: their meaningful relationship." For historians, he underscored, "sequence in time must mean more than mere sequence," for "he necessarily seeks in the past not a narratively

strung agglomeration of particles but an explanatory story of cause and effect" (Elton 1970, 112, 113).

These ambitions typically take one of two forms: either an observation of variation together with a search for factors that best account for this range, or a search for the combination of elements that has fashioned a specific historical outcome situated in time and space, either in the less demanding sense of making it possible or in the more commanding sense of strong determination. The first of these tends to orient work across cases in a search for portability and generalization; the second, by contrast, operates to understand causes within individual cases.

Some of the deepest debates, not just among historians and social scientists, but also among philosophers of history, concern the standing of Carl Hempel's 1942 claim that historical events are best comprehended as subsumed within laws covering analogous events that are alike, even when widely apart in time and place. There is a recurring tension between explanation showing "that what was done was the thing to have done for the reasons given" and the orientation claiming the given outcome was done "in accordance with certain laws ... on such occasions" (Dray 1957, 124).

Irrespective, social scientists cannot escape the particularities of situations. Social ties and relationships, individual choices and patterns of behavior are always embedded in specific locations and moments even when mechanisms are not unique to this or that time and place. It is difficult to comprehend whether, when, and how such contexts shape outcomes. This challenge is often, too often, dealt with by way of a commitment, frequently left implicit, to human regularity. Society is considered as if it were like nature, with central elements sufficiently fixed to make generalizations portable. Sometimes, matters of situation and context are elided entirely by honing in on a specific puzzle without asking how the pattern that is uncovered by research and analysis might obtain, or not obtain, inside other circumstances.

Curiously, this tendency sometimes appears even when social scientists work on historical case materials. Here lies a key motivation for this book. We wish to advance significant analytical historical work in the social sciences that pays due attention not only to situation and context, but also to concerns regarding time and sequence that are central to the craft of historians. Qualitative social scientists have been taking such efforts forward in full awareness that history unfolds with an unstable combination of regularities, mechanisms, change, and randomness that requires moving up and down a ladder of abstraction from the conceptual to proper-named people, places, and events (Abbott and Forrest 1986). Such work is acutely aware that the meaning of a specific place and what happens there can vary, often dramatically, over time.

These transformations challenge historians and social scientists in ways that also have vexed philosophers of history. Can a place be identified meaningfully by an attribution of fixed properties such as its geographic place or is it a location made mutable by changes to processes, alterations to activities, and shifts in meaning? Are there moments when these alterations are especially

potent, in effect creating new realities and new meanings? Are there moments of transformation like those designated in science by Thomas Kuhn that constitute not simple linear development but junctures of transformation? And how can inquiry take such questions on board without lapsing into one or another form of relativism? Are developments across time truly incommensurable?

How, in short, can systematic inquiry navigate this field of tension? When, under what conditions, does history punctuate and pass through moments of critical junctures marked by exceptional change and new directions rather than continue on familiar, even contiguous, paths? How distinctive, perhaps even incommensurable, are the content and operation of key factors before, during, and after such times? When are such transformations tightly circumscribed to particular institutions or issues or populations, and when are they more inclusive, even global? With what implications for analysis? And when such moments exist, are they shaped by exogenous shocks or immanent and evolutionary developments?

Such issues are front and center for many historical institutionalists and other qualitative scholars. Quantitative scholarship has been lagging in its ability to consider these matters, all too often tethered to methods that flatten time, override historical particularity, and give equivalent status to each moment of observation and measurement. We wish to remedy this disparity.

1.5 WAYS AHEAD

We begin this work in Chapter 2 by sketching a roadmap of pathways and orientations. These guideposts underscore our central commitments and point toward a range of quantitative approaches and techniques that can ascertain and analyze historical parameters shifts.

These we identify and evaluate in Chapter 3, which considers ideas about conditional probability associated with Bayesian statistics. The chapter demonstrates approaches to quantitative historical work that handle tests of temporality, periodicity, specificity, and context better than more familiar tools (Western and Jackman 1994; Western 2001; Western and Kleykamp 2004; Humphreys and Jacobs 2015).

For scholars of any skill set to be convinced of the value of methods that privilege parameter variation, it is essential to demonstrate, beyond abstract argument and appeals to common sense not only why standard methods are flawed but how other tools improve on them. By utilizing the toolkit of semiparametric methods and change point models in replications of extant studies, the following chapters illustrate advantages and insights made possible by these fresh approaches to modeling in time. We first subject our own prior work in Chapter 4 to these demonstrations of past limitations and future possibilities, showing gains to analytical power by deploying nonlinear methods. We then turn to excellent and influential, well-executed studies by other scholars, con-

sciously setting a very high bar, to demonstrate how designing inquiry with statistical means that play more by historians' rules can improve on already well-regarded scholarship.

Chapter 5 addresses estimation approaches for path-dependent processes. This approach to historical change, distinguishing critical junctures from times of more ordinary pathways, has become quite central to historical work by social scientists, and thus deserves special attention. Curiously, the rich development of this orientation by qualitative scholars has yet to be matched by more than a tiny number of attempts to model path dependence quantitatively. Despite advances in relevant theorizing and the growing and significant body of work that studies history using quantitative approaches, very little work has been focused on the distinctive challenges that attend empirical modeling of path-dependent processes. To date, there have been only a handful of attempts to develop quantitative modeling tools that researchers can apply to make quality inferences about such processes. Fortunately, as we show, there is a compelling congruence between our arguments concerning the centrality of parameter variation for historical analysis and theoretical formalizations of path dependence.

Chapter 6 takes up issues of causal inference. Qualitative researchers, like other social scientists, are confronting the robust credibility revolution currently underway. We discuss how the very scope of historical analysis can create opportunities to take advantage of natural experiments and exogenous inputs that alter the course of events to produce more convincing causal analyses. We also underscore how historical investigation presents distinct challenges to methodological approaches when causal identification is prioritized. Understanding and confronting these challenges is essential to taking advantage of inferential opportunities presented by historical data, as a consideration of scholarship on economic development seeks to demonstrate.

Concluding the book, Chapter 7 suggests additional ways to move this research program forward by improving communication among traditional qualitative scholars and applied quantitative social scientists who focus on history, as well as those who are at work to develop new methods and estimators.

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