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Historical Political Economy: What Is It?

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Comments

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Abstract

In this chapter, we define what historical political economy (HPE)
is and is not, classify the major themes in the literature, assess the
relative strengths and weaknesses of the literature, and point to
future directions. We view HPE as social scientific inquiry which
highlights political causes or consequences of historical issues.
HPE is different from conventional political economy in the
emphasis placed on historical processes and context. While we
view HPE in the most inclusive manner reasonable, we define it to
exclude works that are either solely of contemporary importance or
use historical data without any historical context (e.g., long-run
macroeconomic time series data). The future of HPE is bright,
especially as more historical data from around the world become
available via digitization. Consequently, the future frontier of the
field likely falls outside of the US, which is the concern of a
disproportionate amount of the current literature.

Keywords: historical political economy, economics, political
science, economic history, political history

What is Historical Political Economy? What is it Not?

What is Historical Political Economy (HPE)? At its core, HPE is an interdisciplinary endeavor. It combines insights from history, economics, political science, and occasionally other social sciences (such as sociology and anthropology). In short, HPE is the study of how political and economic actors and institutions have interacted in the past or over time. It differs from much of economic history in that it focuses on the causes and consequences of politics. It departs from much of conventional political economy in that its context is strictly historical, even when it has implications for contemporary political economy. It also departs from much of history in its use of social-scientific theory and methods. Thus, while HPE involves elements of the traditional fields of economics, political economy, and history, it is separate from—and integrative of— them.

We view there as being three distinct criteria which a work must satisfy to qualify as HPE. First, all works of HPE attempt to establish a falsifiable argument, that is, one that can be tested and proven false (or logically contradicted). There are multiple methodologies that can be employed to establish a falsifiable argument. Good works in HPE may be qualitative or quantitative, theoretical or empirical, experimental or observational. So long as the work attempts to establish some form of falsifiable argument—implicitly or explicitly—it satisfies this criterion. This includes most theoretic work, which typically aims to provide propositions that are, in theory at least, falsifiable with the right historical data or narrative.[1] This may seem natural to social scientists, whose training leads them to either theorize or empirically test falsifiable, causal arguments. But this is not always the case, especially for those in the humanities, where descriptive studies are more common. Under this criterion, works that are

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[1] Works that are purely theoretical with no possibility of falsification are unlikely to seriously engage with the historical context, which (as we describe below) is another criterion of HPE work.

merely descriptive do not qualify. Historical description can be incredibly valuable, and HPE scholars often draw on descriptive works of historians. For instance, Mary Beard's histories of Rome or Albert Hourani's histories of Islam and the Middle East contain both political and economic insights and beautifully dive into the histories of the polities under study. But neither attempts to formulate testable hypotheses. Although works of this type are often invaluable to HPE scholars, the works themselves are not HPE.

Second, all works of HPE must have some interest in understanding and explaining historical context. While all works by historians satisfy this criterion, a large fraction of the work of economists and political scientists do not. In some cases, it is obvious what is historical: works on classical Greece, Song China, or the Ottoman Empire clearly qualify. But in other cases, it is less obvious. Here, we stress two points. First, while there is no "cut off" date for what makes something historical, the subject of the work cannot be primarily contemporary. We are writing this chapter in the early 2020s; works focusing on the 1980s or 1990s may be historical, depending on whether they are primarily trying to explain some social scientific phenomena from those decades.

More important than some arbitrary cutoff date, a work of HPE must help understand some historical context. For instance, macroeconomics works which use time series data are not HPE—even if those data go back centuries—if the purpose of the data is merely to test some theory in which the historical context does not matter. Those kinds of studies are essentially ahistorical. Note that this criterion does not preclude works on "historical persistence" – studies seeking the historical roots of contemporary phenomena – as being classified as HPE (see Chapter XX, by Acharya, Blackwell, and Sen, as well as Cirone and Pepinsky (2022)). Yet, for such studies there must be some interest in understanding the historical context of the "cause"

that established the persistence in question. On this point, we agree with Dennison (2021, p. 105), who argued that "those who employ historical arguments, especially about the role of institutions and their long-term effects, must engage more actively with the findings of historians. Failing to incorporate historical research leads to models that mischaracterize the constraint structure faced by individuals and groups in the societies they seek to explain."

Third, there must some *political economy* element of a work for it to qualify as HPE. By political economy, we mean it in the way Weingast and Wittman (2006, p. 3-4) defined it in their introductory chapter to the *Oxford Handbook of Political Economy*: "political economy is the methodology of economics applied to the analysis of political behavior and institutions. As such, it is not a single, unified approach, but a family of approaches … this is tied together by a set of methodologies, typically associated with economics, but now part and parcel of political science itself." The same is true of HPE, albeit with the stipulation that the work also be historical in nature. Works of economic history concerned with wages, prices, capital, etc. are not necessarily HPE, unless they are concerned with the political processes driving these phenomena. Works of political history concerned with the fundamentals of legislative decision making, the origins and development of political parties, or the evolution of representative government are not necessarily HPE unless they consider the political economy of these processes. That is, works of HPE not only dive into the history of the phenomenon in question, but also consider the preferences, goals, and incentives of the political actors at the heart of the processes or institutions under study.

These three criteria dovetail nicely with the criteria proposed by Pablo Beramendi in a March 31, 2021 post on Broadstreet, a blog dedicated to HPE. His definition is worth quoting in full:

> What I think identifies historical political economy (*HPE*) as an approach relative to prior efforts is a three-fold commitment. The first one is theoretical: an explicit

effort not to let the description of a process substitute for an argument (often in the form of an endless forest of arrows quickly progressing towards negative degrees of freedom) and, more constructively, to develop an abstract logic about the causal relationships of interest that, subsequently, guides empirical efforts. The second one is methodological: a pledge to both dig deep into historical sources to improve measurement as a theoretically driven exercise and pursue, as far as possible given the data context, compelling research designs. Finally, the third one is a commitment to transgression, transgression of artificial disciplinary boundaries and scholarly prejudices to embrace the diffusion of tools, approaches, models and techniques to maximize access to new data (often in vast amounts) and, critically, to analyze it rigorously.

In short, we can imagine a Venn diagram between the disciplines of history, economics, and political science (see Figure 1). At the intersection of the three disciplines is HPE. The chapters in this handbook cover and analyze hundreds (if not thousands) of works at this intersection. While we are concerned with the intersection for the purposes of this volume, the best works of HPE draw extensively from other parts of the Venn diagram—from economic history, political history, political economy, economics, political science, and history. Each has its own methodologies, asks its own questions, and has its own context. This is also true of HPE. Which is why it is a field in need of a handbook synthesizing its recent advancements.

Figure 1: Venn Diagram of Historical Political Economy
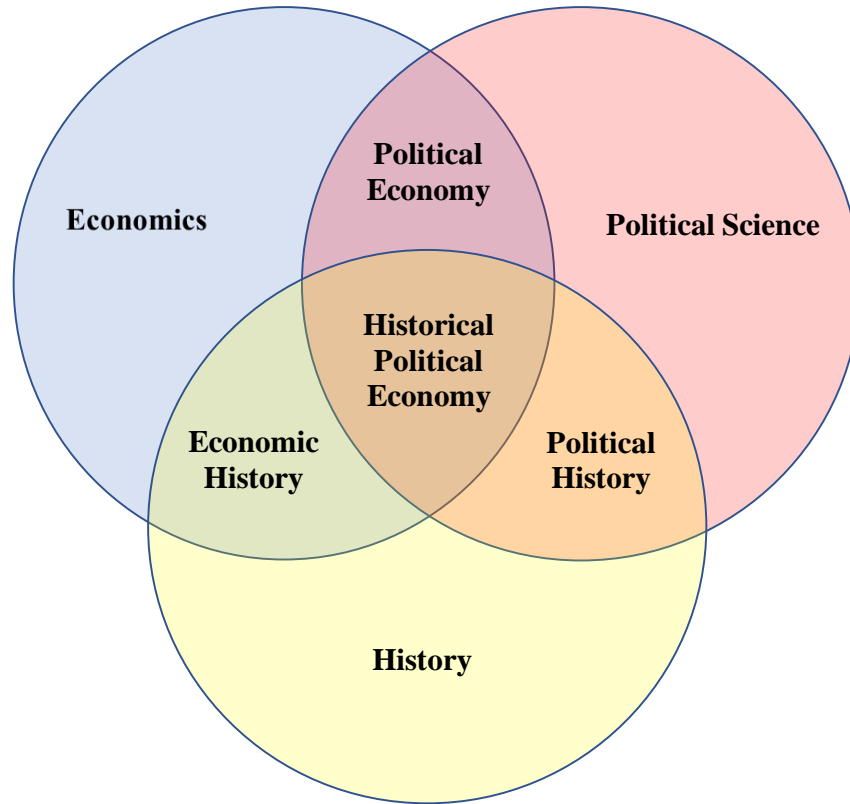
In some respects, HPE is among the oldest of the social sciences. Certainly, the works of Alexis de Tocqueville and Karl Marx qualify as HPE by the criteria laid out above, albeit with a 19th century twist. In the early 20th century, giants like Max Weber, Werner Sombart, and R.H. Tawney produced several important works of HPE. Works of all these scholars engaged with history and attempted causal explanations centered on political economy. Such works of "classic HPE" differ from modern HPE in a few ways. First, the types of data used and analyzed are different. While both classic HPE and contemporary HPE employed archival data, larger data sets with multiple variables were typically beyond the reach of earlier scholars. This is of course no fault of their own; it is mostly due to advances in computing and digitization technologies. Second, and related, the approach to causality has changed. Advances in econometrics and computational power now permit causal inference to be achieved in a manner that was

unavailable even a decade ago, let alone a century. Third, the integration of formal theory into contemporary HPE has contributed a level of mathematization that was not present in classic HPE studies. Yet, the best works of classic HPE used methods still used by many current HPE scholars, particularly in books: they formulate (in words) some causal, falsifiable theory and substantiate it with substantial historical evidence (though not necessarily statistical evidence).

Even though "classic HPE" has been asking questions of importance to HPE scholars for over a century, HPE is also a relatively "new" field, at least in its current guise. Building on the work of scholars like Douglass North, William Riker, and many other economic and political historians, HPE of the 21st century has taken advantage of new empirical methodologies, digitized historical data, and (to a lesser extent) a deeper theoretical toolkit than what was available to their predecessors. The chapters in this handbook by and large overview this "new HPE".

The very best HPE work is squarely in the mainstream of economics and political science (though not in history; for a discussion on how to bridge that gap, see Chapter 2 by Dennison and Gehlbach). This can be seen from a simple scan of the references cited throughout the handbook; they regularly include the *American Economic Review*, *American Political Science Review*, *Quarterly Journal of Economics*, *American Journal of Political Science*, and most of the other top journals in the two disciplines. These include some of the most highly cited works in economics and political science of the 21st century. One need look no further than the works of Daron Acemoglu and James Robinson—whose joint work is clearly HPE—as evidence of this. Less frequently, HPE scholars publish in top history field journals within their discipline, such as *Journal of Economic History*, *Explorations in Economic History*, and *Studies in American Political Development*.

But a field is not really a "field" without a journal of its own. Until 2021, HPE had no such journal, and thus scholars working on interdisciplinary projects did not have an obvious outlet for their work. This changed in 2021 with the introduction of the *Journal of Historical Political Economy* (*JHPE*), founded and edited by one of us (Jenkins). *JHPE* serves a latent but increasingly active and growing community in political science and economics. In recent years, more and more political scientists have been doing quantitative historical work that involves political economy. During that same time, economic historians—and applied economists more generally—have increasingly focused on political economy topics and taken seriously the "politics" in that research. Given the boundaries that typically exist across academic disciplines, these two groups of scholars rarely talk to one another or read (or cite) each other's work. *JHPE*'s goal—as well as the goal of this handbook—is to get these two groups in dialogue.

There are still hurdles the field must overcome to be viewed as established within economics and political science. First, there are no large annual conferences dedicated to HPE. This almost certainly dampens network formation—so key to most academic disciplines—that often occurs at such events. This is even more the case because HPE is *interdisciplinary*, meaning that scholars working in different fields are unlikely to run into each other regularly. Second, and perhaps more important, is that academia has become increasingly siloed. Economists, political scientists, and historians are less likely to be rewarded (i.e., tenure and promotion) by their departments for interdisciplinary work, especially when that work is not published in a known outlet within the discipline. The inherent interdisciplinarity of HPE also means that one must keep up with multiple literatures. This is time consuming, and is typically not rewarded by the profession. This of course is a bigger problem—not just for HPE—but it is one that fields like HPE can help solve. We hope that the synthesis of high-quality work provided in this

handbook—and indeed, the very fact that there is an Oxford handbook on the topic—will be a step along this journey. We could think of no better outcome for the field than for this paragraph to be outdated in a decade.

Major Themes, Strengths, and Weaknesses of HPE

There are several themes at the center of much of the HPE literature. We have organized this handbook around what we view as five of the most general HPE themes. These themes are:

1) *Historical Political Economy: An Overview*. This set of chapters presents a broad theoretical and methodological overview of HPE as a field. Because HPE is such a new scholarly endeavor, and one that is interdisciplinary, the first section includes elements that one might find in a basic research design course.

2) *How States are Organized*. Chapters in this section include those which overview regime types, dynasties, and aspects of the states themselves.

3) *Components of the State*. These chapters are built around distinctly political components of the state, focusing on key political institutions, the nature of the political system itself, and functions of the state vis-à-vis its political institutions.

4) *Long-Run Legacies*. Chapters in this section focus on broad economic aspects and long-run political and economic legacies. These include macro-level phenomena as well as economic conditions or relationships in HPE that typically occur within states.

5) *The State and Society*. These chapters consider how particular aspects of a state's demography—or human population—and political economy have developed over time.

The handbook's coverage is extensive, but not in any way comprehensive. HPE is a widely encompassing field and spans important substantive areas in multiple disciplines. No single

volume could ever fully capture the breadth of HPE. That said, the chapters in this volume provide a useful overview of many important subjects in HPE. They highlight the strengths of the field, while also laying out its weaknesses.

In our view, many of the best works in HPE are at the research frontier of economics and political science. The best studies contextualize history in new ways by using new tools available to social scientists such as network theory (see Slez 202X). There are two types of studies for which HPE is at the forefront: those employing the latest techniques of causal inference, and those studying the persistence of cultural, institutional, or other societal features. The best empirical HPE studies are also able to take advantage of advances in digitization to access and analyze data in a manner that would have been impossible a mere decade ago.

In their chapter "Causal Inference and Knowledge Accumulation in Historical Political Economy," Callis, Dunning, and Tuñón (202X) argue that HPE has been greatly influence by the so-called "credibility revolution." This should not be surprising. There are many HPE questions for which rich data exist and long-run outcomes are available. Researchers can also leverage historical "natural experiments", discontinuities (e.g., political border changes), historical instruments to identify causal effects, and "shocks" or reforms that permit a difference-in-difference approach. With publication increasingly requiring rigorous identification in economics and political science, HPE seems like a natural setting to employ such techniques.

There are drawbacks, of course, to a narrow focus on causal identification. As Callis, Dunning, and Tuñón (202X) argue, external validity is not always apparent in HPE work, theory is often lacking, and the treatments that really matter for outcomes may be not random. In other words, in the search of causal identification, researchers may miss out on the big picture. As Dippel and Leonard (2021, p. 1) recently noted: "After the credibility revolution, much of

cliometrics shifted toward 'natural experiments,' especially in papers published in general-interest journals ... this shift comes with certain trade-offs between statistical and contextual evidence, and … the refereeing process currently makes these trade-offs steeper in historical settings than in other observational-data settings."

Yet, Callis, Dunning, and Tuñón suggest that an emphasis on identification need not be detrimental to HPE. One reason is that while such studies themselves may lack external validity, they can also help generate cumulative learning. Certain types of historical processes build on each other. If we can gain insight into one of these steps in one study, it can help inform other studies that may seek to understand a different part of the story. Second, although mechanisms that connect a causal chain may be context specific, they help shape expectations about what one might find in a different context. This is a key insight. As we argued above, one of the key features of HPE studies is that *historical context matters*. Indeed, we would refrain from considering a work as HPE if it used historical phenomena solely to gain identification without any regard for the historical context. Yet, that does not mean that historical context is the only thing that matters. As long as we recognize that when A causes B in context X it will not necessarily do so in context Y, there is much to be learned from studies that carefully identify important historical connections.

A second, related set of works for which HPE is at front and center of the research frontier are those on historical persistence. Since the publication of Acemoglu, Johnson, and Robinson's (2001) "Colonial Origins" article, a large proportion of empirical HPE studies have had some "persistence" component to them. Acharya, Blackwell, and Sen (202X) note several reasons why historical phenomena may persist, including path dependence, critical junctures, and "reproduction mechanisms." These works typically employ causal inference techniques

described above to link some historical phenomena to later events (often to the present day). The literature on historical persistence is way too large to summarize here; the interested reader should consult Acharya, Blackwell, and Sen's chapter in this handbook or comprehensive review articles by Nunn (2014), Voth (2021), and Cirone and Pepinsky (2022).

There are certainly drawbacks to the historical persistence literature. In attempting to rigorously identify historical events with future outcomes, too many studies in this literature pay short shrift to historical context or are blasé regarding mechanisms of persistence. This is a common complaint regarding the persistence literature—one both authors of this chapter have heard many times in casual conversation and one we both agree with. Abad and Mauer (2021) likewise argue that there may have been an "overcorrection" for past oversights on historical persistence. The pitfalls they cite include (p. 31): "the failure to recognize institutional change ('anti-persistence'), vague mechanisms, the insufficient use (or misuse) of historical sources and narratives, the compression of history, and a failure to account for the effects of geography."

Yet, Callis, Dunning, and Tuñón's insights regarding causal inference apply here as well. Taken as a whole, we know much more than we used to about when and why historical phenomena persist. This is evident in Acharya, Blackwell, and Sen's chapter. Taking a broad view of the literature, they are able to address such issues as "what persists?", "what explains persistence?", "when is persistence broken?", and "proximate vs. distal effects". Importantly, their insights come from our *cumulative knowledge*, attained mostly in the 21st century, and not on just one or two studies. In short, the very best persistence studies have greatly enhanced our knowledge of what might persist, why, and under what contexts, while paying careful attention to historical context.

A third margin on which HPE is at the cutting edge is in its creative use of gathering data. As Cirone (202X) documents, advances in digitization and investment in online repositories have opened up a new world to researchers. More data is available now than ever before, and we have every reason to believe that this will only become more the case over time. Cirone (p. XX) classifies HPE data into seven broad categories: "i) sociodemographic and population data, ii) government or institutional records, iii) geographic and spatial data, iv) political data, v) economic data, vi) ethnographic data, and vii) civil society data." Newly-acquired access to such data places HPE on the research frontier for two reasons. First, we are now able to gain insight into *historical contexts* that were unavailable just a decade or so ago. While historical context requires more than just what is available with quantitative data, such data help shape the context as we understand it and (sometimes) as those who lived through the period in question understood it. This type of context is not always possible with qualitative evidence or individual case studies. Second, there are new questions that can now be answered that were unanswerable with any type of precision until recently. As Alexandra Cirone notes (in personal correspondence), HPE scholars are "better at systematically considering and addressing challenges that come with historical data, such as missing data, archival silences, and sample selection." As more data become available and accessible, questions that previous generations would not have considered tackling—indeed, may simply not have considered at all—are becoming common fodder for Ph.D. dissertations.

Overall, the best works in HPE are much closer to the research frontier than ever before. Yet, there are still some aspects of the field which could use improvement in our opinion. An obvious one, discussed above, is that too many studies—including many of those published in top journals—eschew historical context in favor of focusing on causal identification. This is

certainly not true of the best HPE work. And, as discussed above, this does not mean that such

studies add nothing to our cumulative knowledge. Yet, one of our goals in editing the present

volume and the creation of the *Journal of Historical Political Economy* is to encourage scholars

to take historical context as central to any HPE analysis, even if its primary purpose is to show

"persistence."

Second, we agree with Gailmard (202X) that HPE is now undertheorized. To be clear, this is

not just an issue with HPE; the pendulum has swung in large parts of economics and political

science towards data-driven empirical analysis at the expense of theory. Of course, many of the

best works do both, but this is no longer required for publication in top journals. As Gailmard (p.

XX) eloquently notes "Credibly identified treatment effects of some X on some Y do not explain

why an effect occurs. They simply rule out all explanations in which X is unrelated to Y … or in

which the relationship is driven by a confounder W … We use models because we want to know

more than this." Gailmard (2021) went further in a post on *Broadstreet*: "while credible

identification has obvious and important benefits for empirical HPE, making it the centerpiece of

any research program in HPE is going to distort the kinds of historical and theoretical

understanding we create. In particular, centering the demands of causal identification will slant

case selection without any corresponding benefits for generalized historical knowledge."[2]

This is an important point that may help square Callis, Dunning, and Tuñón's more

optimistic take that *cumulatively* the field has learned much from individual studies, even if

individually those studies do not speak to general mechanisms. The missing connection is theory.

The question is whether the pendulum will swing back towards HPE being "appropriately"

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[2] Sellars (2021), in a discussion of political borders in HPE, endorses this idea: "Political and institutional boundaries are multifaceted, and it is not clear how any locally estimated causal effects will generalize across even similar cases."

theorized. Fouka (2022, italics ours) provides a reason to be skeptical this will happen, at least in the short run:

> HPE researchers, in their majority empiricists, do not put enough effort in engaging with existing abstract theory in designing their studies and interpreting their findings. What currently tends to happen in a lot of HPE work is either total absence of theory or the proliferation of ad-hoc "theories" generated by researchers to explain the empirical findings in the context that they study. These theories are usually narrow hypotheses with little generalizability. Often, *disciplinary incentives in some fields in which HPE scholars operate push them to generate overarching "big" theories out of very context-dependent cases, because new theorizing is valued more than empirical testing of existing theories*.

The disciplinary incentivizes highlighted by Fouka are difficult, though not impossible, to overcome. Fortunately, several chapters in this volume suggest that some topics in HPE have received significant theoretical treatment. These include, among others, theories of authoritarianism (Grzymala-Busse and Finkel 202X), state building (Garfias and Sellars 202X), political legitimacy (Greif and Rubin 202X), assimilation (Fouka 202X), and identity (Suryanarayan and White 202X). This is also true of topics in HPE with respect to particular areas of the world, like theories of parties (Jenkins and Kam 202X) and legislatures (Cox 202X) in the United States and Western Europe, but less so outside of those areas. Some topics in HPE have received very little theoretical treatment, like bureaucracy, thus making Vogler's (202X) chapter of particular interest. As empirical methods become even more developed and data becomes even more widely available, we believe that the relative dearth of theory will become even more obvious in HPE (and, more generally, economics and political science).

The Future of HPE

For all the reasons given above—and for reasons repeated again and again throughout this volume—the future of HPE is extraordinarily bright. It is at the same time one of the oldest and

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one of the newest fields in the social sciences. Its best work is published in the best economics and political science journals. Its practitioners are relatively junior (as evidenced by the authors of the chapters in this handbook, who are among the best in the field). These are all signs of a growing field whose influence will only expand over time.

But where is the future of the field headed? What are the topics and parts of the world for which there is still "low hanging fruit" for the HPE scholar to pick? First and foremost, the geographical coverage of HPE studies leaves much to be desired. The US and Western Europe receive a lion's share of attention in the field. There are good reasons for this: these regions have the best data and, after all, they are where the modern economy and liberal governance first emerged. There is still much to learn about the HPE of the US and Western Europe, and we fully expect some of the best work in the field to be on these regions for decades to come. Yet, given the paucity of research on regions outside the US and Western Europe, there is much catching up to do regarding the HPE of other parts of the world that have unique historical and cultural contexts. Sub-Saharan Africa, the Middle East, Latin America, South Asia, and Southeast Asia have all received relatively little attention in the literature, despite their large populations and historical (and contemporary) importance. Even though HPE interest in China has grown in the last decade, it too is understudied. Given its relative importance in world history (and in the present), we expect to see much more work on the HPE of China in years to come.

The biggest stumbling block to unearthing the HPE of the "non-Western" world is data constraints. But on this margin, there is much reason for hope. Heroic efforts to uncover, clean, and analyze new data are being taken for areas around the globe. Many of the authors of chapters in this handbook are contributing to this effort: in Africa (Leonard Wantchekon and Sara Lowes), Latin America (Emily Sellars, Francisco Garfias, Jenny Guardado, and Agustina

Paglayan), Imperial Russia and the Soviet Union (Tracy Dennison, Evgeny Finkel, and Scott Gehlbach), South Asia (Saumitra Jha and Pavithra Suryanarayan), and East Asia (Yuhua Wang, Chiaki Moriguchi, Tuan-Hwee Sng). Of course, there are also many more HPE scholars whose work does not appear in this handbook that are helping to lead the effort.

As we noted above, another margin on which we hope to see more HPE work in the near future is formal theory. The relative paucity of theory is a deeper issue in economics and political science, where the "credibility revolution" has pushed young scholars into more empirical work. There is much good that has come of this, and the best works using modern causal identification techniques have provided new insights into key HPE topics. But there is also the distinct possibility that these fields are now undertheorized. As Sean Gailmard's (202X) chapter in this handbook notes, theory helps us place reasonable constraints on what is possible and why it is possible. In our opinion, too many papers in HPE (and economics and political science more generally) rigorously establish causal connections without applying that same level of rigor to the *mechanisms* driving those connections. This is where we need formal theory the most: it is a complement, not a substitute, of rigorous causal analysis.

There are many reasons why the future of HPE is very bright. First and foremost, we have just begun to scratch the surface of the available HPE data. Thanks to the heroic efforts of historians working in archives, new digitization techniques, OCR, and GIS, we are able to say more about the past than ever before. The digital humanities are opening up new lines of research inquiry that were unthinkable as recently as two decades ago. Scholars can now use historical text and images as data themselves. Advances in GIS allow for insights into spatial components of HPE that were difficult, if not impossible, to address quantitatively in the past. There is every reason to believe progress on this front will grow exponentially. Data that used to

take a lifetime to uncover can now be downloaded in a matter of minutes. This frees up scholars to ask—and answer—new types of questions that would have been beyond the traditional purview of HPE.

But challenges also exist. The immediate challenge, as Cirone and Spirling (2021) note in their *JHPE* article on working with historical data, is to establish best research practices. In particular, they underscore the need to be "transparent about the selection of qualitative evidence, how the evidence is analyzed, and how the ultimate results are framed." Another challenge is to overcome language issues in the collection of country-specific historical data. More HPE scholars from different parts of the world who speak different languages will be necessary for archival work. Alternatively, HPE scholars may find it conducive to collaborate with foreign-language scholars in the future as a way to leverage different skill sets.

In the end, we believe the future of HPE is very bright. While it is still a very young field, as evidenced by the many junior and mid-career scholars who have contributed chapters to this handbook, its presence has grown considerably in recently years. Indeed, the very fact that there is an Oxford Handbook for the field is prima facie evidence of its growing importance. In their recent survey of HPE in political science, Charnysh, Finkel, and Gelbach (2022) found that the proportion of HPE articles in the discipline's top journals more than doubled over the last decade. And while HPE's ascension has included some prominent senior scholars, it has been the emergence of young scholars who have fueled the tremendous growth in the field. These young scholars are very well trained in techniques of causal inference, but they also care deeply about history and getting the story "right." They are also at the forefront of "spreading the gospel" by teaching HPE courses at their universities.

The hope is that these talented young scholars will mold the future of the field, train PhD students, teach HPE courses at the undergraduate and graduate levels, and increase HPE's prominence in top journals and academic presses. There are still hurdles to be overcome. The field could certainly use an annual conference, and it is important for the field that journals specializing in HPE, like *Journal of Historical Political Economy*, continue to grow in visibility and citation counts. Yet, the field is well on its way to clearing these hurdles. In the end, we feel confident that HPE will continue its march toward even greater influence in political science, economics, and allied fields.

Jeffery A. Jenkins is a political scientist interested in American national institutions, with an emphasis on Congress and parties, and American political development. Two of his recent books include: *[Republican Party Politics and the American South, 1865-1968](#)* (Cambridge University Press, 2020) with Boris Heersink — which won the 2021 V. O. Key Award (from the Southern Political Science Association) and the 2021 J. David Greenstone Prize (from the American Political Science Association) — and *[Congress and the First Civil Rights Era, 1861-1918](#)* (University of Chicago Press, 2021) with Justin Peck. He is also co-author of *Fighting for the Speakership: The House and the Rise of Party Government* (Princeton University Press, 2013) with Charles Stewart III. He was Editor in Chief of *The Journal of Politics* for six years (2015-2020) and recently started two new journals: the *Journal of Political Institutions and Political Economy* (2020) and the *Journal of Historical Political Economy* (2021).

Jared Rubin is an economic historian interested in the political and religious economies of the Middle East and Western Europe. His research focuses on historical relationships between political and religious institutions and their role in economic development. His book *How the World Became Rich: The Historical Origins of Economic Growth* (with Mark Koyama, Polity Press, 2022) explores the many theories of why modern economic growth happened when and where it did. His book, *Rulers, Religion, and Riches: Why the West Got Rich and the Middle East Did Not* (Cambridge University Press, 2017) studies the role that Islam and Christianity played in the long-run "reversal of fortunes" between the economies of the Middle East and Western Europe. It received best book awards from the Economic History Association and the Society of Institutional and Organizational Economics. Rubin is the Co-Director of Chapman University's Institute for the Study of Religion, Economics and Society (IRES), President of the Association for the Study of Religion, Economics, and Culture (ASREC), and serves on numerous editorial boards. He graduated with a Ph.D. in economics from Stanford University and a B.A. from the University of Virginia.

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