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NEWSLETTER STAFF

Department of Political Science
University of Florida

Editor

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bbsmith@ufl.edu

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CONTACT

Department of Political Science
University of Florida
234 Anderson Hall
P.O. Box 117325
Gainesville, FL
32611 USA
Tel: +1 352 392 0262
Fax: +1 352.392.8127

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Benjamin Smith

is UF Term Professor of Political Science at the University of Florida. His email address is bbsmith@ufl.edu

INTRODUCTION

By Benjamin Smith

This issue is the first for me and the editorial team at the University Florida. I would like to express my thanks to outgoing editor Eugene Finkel of Johns Hopkins University, who graciously gave his time to ensure a smooth transition. My colleagues here at UF on the editorial board and I are privileged to be stewards of this newsletter. Since my graduate school days at the University of Washington, I have looked forward to new issues as the site of enriching dialogues, reviews of new books, and first looks at the increasing number of new data resources for comparative politics research.

In this first issue for the UF editorial team, we engage the growing field of deep historical comparative scholarship, both in the venerable comparative-historical tradition and in the growing field of historical political economy. The former, grounded in foundational scholarship by Tilly, Skocpol, Moore, and others, has focused on key moments (in Diana Kim's essay in this issue, 'ruptures,') and on the causal trajectories that take shape from them. The latter, drawing on newly available or newly digitized quantitative data, generally focuses more on causal identification than on specifying mechanisms but is equally committed to historically lengthy causal inference. This multi-method focus on long-durée causes of state capacity, identity, and development among other big-picture outcomes stands in contrast to the short-range focus of experimental design elsewhere in our subfield and promises to broaden our understanding, and the precision of our study, of historically rooted political outcomes.

Our contributors—Volha Charnysh, Anna Grzmala-Busse, Diana Kim, Pavithra Suryanarayan, and Deborah Yashar—span both approaches and represent a broad range of regional expertise, as brought to bear on the long-range dynamics of identity, state formation, democracy, and development. Keeping with the long-range and macrohistorical affinity in these contributions, we also introduce our first dataset review. Adam

Casey reviews Mark Beissinger's new original dataset of revolutionary episodes dating back to 1900.

As we begin our editorship here, my colleagues and I encourage you to contact us with ideas for symposia, data reviews, and in-depth reviews of important new books in comparative politics. We thank both the executive committees under outgoing section president Scott Mainwaring and new section president Prerna Singh for their support and enthusiasm.

DATASET REVIEW SUBMISSIONS

If you have submissions for the dataset review section of the APSA-CP Newsletter, please email bbsmith@ufl.edu.



A RUPTURE IN COLONIAL TIME

By Diana Kim



Diana Kim

is an Assistant Professor in the Edmund A. Walsh School of Foreign Service and a core faculty member of the Asian Studies Program. Her email address is diana.kim@georgetown.edu

“The times were once again out of joint; it was an age in which fools profited from their folly and the wise suffered for their wisdom.”

- From *Java in a Time of Revolution*¹

What is a meaningful rupture that interrupts a colonial legacy? We are familiar with the lasting imprints of empires and their institutions for colonial rule upon today’s identity categories, economies, and states through the lens of historical persistence—of what continues, of what endures, of what stays resilient over the course of long stretches of time.

This essay reflects on the opposite. It considers what might perturb, disturb, or indeed break the legacy of a colonial past. When do certain events muddy the clarity with which we imagine history to transmit its weight upon the present? Ruptures in historical time abound—wars, crises, revolutions and other major upheavals to social order are but among the most dramatic. Not all ruptures, however, matter for political scientists in analytical pursuit of mechanisms of transmission, invested in tracing processes of institutional reproduction necessary for causally narrating or explaining a colonial legacy. When is a rupture meaningful for colonial time? How can we know? I explore these questions by dwelling on the experience of Southeast Asia during World War Two, when the region—long colonized by multiple Anglo-European empires—was temporarily taken over by the Japanese empire.

The Japanese occupation of Southeast Asia is a

period of around four years during which there was a temporary change of the colonial guard. Beginning in 1941, the Japanese displaced the Americans, the British, the Dutch, the French, and the Portuguese who had long divided the region. When the war ended in 1945, the Anglo-European powers returned.²

It is often easy for non-specialists of Southeast Asian history and politics to bracket the significance of this period, not least because the time span of Japanese rule seems so short compared with the centuries that the Anglo-European empires prevailed, whether from the famed early modern mercantile companies of the British and Dutch or dating back to the territorial colonial states established into the 19th century.³ For quantitative and qualitative studies of colonial legacies among historians and social scientists alike, it is not

Knowing the history and historiography is but a beginning. This particular type of causal description for colonial legacies further asks of a political scientist to be a narrator and to follow the logic of a story in order to discern where and why breaks in a storyline happen.

unusual to sidestep the Japanese occupation by assuming continuity from pre-war institutions and treating the colonial period as a single era from European conquest to independence.

However, there are several reasons for such presumptions to falter. For one, the short-lived Japanese occupation period changed territorial and administrative boundaries. During the wartime 1940s, Thailand grew larger while today’s Malaysia, Burma, and Cambodia became smaller, as the Japanese empire gave Thailand the Malay States of Kedah, Perlis, Kelantan, and Terengganu (previously part of British Malaya), the Shan States of Keng Tung and Mong Pan (previously tied to British

Burma) and control over Battambang, Siem Reap, and parts of northwestern Laos (in the provinces of Sayaboury and Bassac previously in French Indochina). Today's Indonesia was split—the Japanese administered the island of Sumatra as part of Malaya while keeping Java intact. The old Anglo-European borders were reverted back to after the war, a process that was uncontroversial for some sites but would stoke loud claims to irredentism and conflicts for others.⁴

Second, the “who” of colonial rule changed. The Japanese empire was a self-avowed Asian imperial power. The blunt fact of a new foreign ruler, by people of similar skin color and appearance, complicated us-them binaries of Asian versus European that divided colonizer versus colonized; it blurred many identity categories of ethnicity and race upon which pre-war colonial institutions had been built. Japan's wartime propaganda cited a future of pan-Asian nationalism under the idea of a Greater East Asia Co-Prosperity Sphere, which professed dreams of building a world of “Asians for Asians,” invoked theories of shared racial origins between the Japanese and Malayo-Polynesian people, and made promises to liberate Southeast Asia from the yoke of European imperialism.⁵ Pragmatism also prevailed. “No one loved the Japanese,” reminisced the Burmese anti-colonial activist and feminist writer Khin Myo Chit, “[b]ut anti-British feeling had been roused to the pitch of driving us into the arms of whoever was against the British.”⁶

Co-prosperity however, was not equality. Japan's imperialist ideologies also advanced the superiority of the Yamato race of Japan as a master people over other Asians. Thus, during this period, there were countervailing trends of both intra-Asian solidarity building and fractured alliances as racialized hierarchies among Southeast Asians thrived and hardened. The Japanese occupation also ushered in myriad reversals of fortune between majority and minority groups with reasons for conflict and animosity that pre-war European rulers had helped establish through divide-and-rule policies by fostering asymmetric economies of privilege.⁷

Third and relatedly, these four years were animated by varieties of wartime institutions—formal, informal, and illicit—that the Japanese

variably inherited from their Anglo-European predecessors, repurposed, destroyed or built anew. The duration of the Japanese occupation was relatively short but there was a swift tempo to institutional life for extracting resources, coopting elites, mobilizing people's labor and commanding allegiance, repressing dissidents, and performing legitimacy.⁸

For instance, nearly 1,000 stadiums for sporting league events and festivals were built between 1941 and 1945 across Vietnam and Cambodia, new physical infrastructures built under the collaborative wartime regime of French Vichy and the Japanese Imperial Army that at once drew upon and reconfigured pre-war organizations and networks for youth mobilization instituted by the Third Republic and Catholic Church.⁹ In other realms, wartime institutions were directly imported from metropolitan Japan or other parts of the empire in East Asia, such as the tonarigumi system for organizing neighborhood policing in Java and Malaya, and would persist well after the war.¹⁰ Others would prove more ephemeral, such as collective farming settlements for separate ethnic communities (in Endau for the Chinese, Bahau for Eurasians, and Pulau Bintan for Indians). The Endau Settlement (also known as the “New Syonan model farm”) was a sprawling 300,000-acre area to which the Japanese relocated 12,000 Chinese inhabitants of Singapore in August 1943. It swiftly became a site of rice cultivation, with a local school, a bank, a paper factory, a sawmill, and many restaurants. Endau's economic and social life died out, just as quickly, when the war ended and the Japanese left.¹¹

For students of Southeast Asia's colonial legacies, there are high stakes to getting the Japanese occupation “right.” Post-war reversions to pre-war borders may make it seem as if there was more stability to territorial and administrative divides than was actually the case. Given the temporary change in colonial ruler identity, what actors in context meant by “independence” or “liberation” are not necessarily straightforward references to Anglo-European domination. And given the many varieties of wartime institutional interactions, there are risks of conflating or misrecognizing the short-term consequences of war for the long-run effects of colonial

institutions upon contemporary outcomes of interest.

Identifying ruptures in colonial time and deciding whether they are meaningful is a challenging task. It depends on the issue at stake, the level of analysis on which a process is being traced, and the place and timing at which the process unfolds. In other words, it requires rich causal description of a specific site, actors involved, and the layered structures in which they are embedded. Knowing the history and historiography is but a beginning. This particular type of causal description for colonial legacies further asks of a political scientist to be a narrator and to follow the logic of a story in order to discern where and why breaks in a storyline happen. It also calls upon a political scientist to combine positivist and interpretive approaches to historical inquiry, because thinking about temporal ruptures rests upon counterfactual reasoning about the causal significance of events and institutions during a certain *durée*, which are also the alternative futures of an unrealized past. And in the process, a political scientist may come to assume the role of a judge who decides what had once been plausible, imaginable, and therefore reasonable to assume about a historical reality—whether stability over time or a lack thereof.

Notes

¹ Anderson, B. (1972). *Java in a Time of Revolution: Occupation and Resistance, 1944-1946*. Cornell University Press, 15.

² On the Japanese occupation's significance for theorizing colonial legacies that this essay draws from, see Kim, D. *Colonial Legacies and Japan's Wartime Empire*. Cambridge University Press (CUP-Elements Series on Politics and Society in Southeast Asia. Under contract. Within Southeast Asian studies, there have been longstanding debates about whether or not this period radically transformed the region's colonial socioeconomic and political structures. Seminal studies include Benda, H. (1958). *The Crescent and the Rising Sun: Indonesian Islam under the Japanese Occupation, 1942-1945*. The Hague and Bandung: W. van Hoeve; Lebra, J. (1977). *Japanese Trained Armies in Southeast Asia*. Singapore: Institute of Southeast Asian Studies; McCoy, A. (ed.) (1985). *Southeast Asia under Japanese Occupation*. Yale University Southeast Asia Studies; Stoler, A. L. (1985). *Capitalism and Confrontation in Sumatra's Plantation Belt, 1870-1979*. Yale University Press. For a recent historical overview, see Huff, G. (2020). *World War II and Southeast Asia: Economy and Society under Japanese Occupation*. Cambridge University Press.

³ Among scholars of Southeast Asia, there is neither a single nor conventional definition of colonialism, but rather a general understanding that it refers to an agglomeration of transformative processes associated with the region's encounter with Europe since at least the 16th century. This pluralistic conception includes the establishment of colonial settlements and trade monopolies through armed expeditions and commercial treaties. Today's Malacca in Malaysia was the first European-occupied port-town—by the Portuguese in 1511—and East India Companies chartered by the British, Dutch, French crowns as well as Spain's Royal Philippine Company organized Southeast Asia's commerce to the advantage of Western mercantile interests. Colonialism in Southeast Asia also refers to the unequal interactions of already residing people with foreign traders and settlers, Christian missionaries that altered the former's worldviews, norms, legal systems and cultures. Anglo-European colonial rule further includes the imposition of Anglo-European property rights regimes and modes of economic organization that applied differential standards for land and labor than those operative in metropolitan homelands. See Andaya, B. (1998). "From Temporary Wife to Prostitute: Sexuality and Economic Change in Early Modern Southeast Asia." *Journal of Women's History*, 9(4), 11–34; Aung-Thwin, M. (2005). Colonialism - Southeast Asia. In *New Dictionary in the History of Ideas*, 379–381. Thomas Gale.

⁴ Kratoska, P. (1997). *The Japanese Occupation of Malaya: A Social and Economic History*. University of Hawai'i Press; Strate, S. (2015). *The Lost Territories: Thailand's History of National Humiliation*. University of Hawaii Press; on pre-war Thailand, see Loos, T. (2018). "Competitive Colonialisms: Siam and the Malay Muslim South," in *The Ambiguous Allure of the West: Traces of the Colonial in Thailand*, 75-91, (eds.) D. Chakrabarty, R. Harrison & P. Jackson. Cornell University Press.

⁵ Yellen, J. (2019). *The Greater East Asia Co-Prosperity Sphere: When Total Empire Met Total War*. Cornell University Press.

⁶ Cited in Yellen 2019, 137.

⁷ Kratoska, P. (ed.) (2002). *Southeast Asian Minorities in the Wartime Japanese Empire*. Routledge Curzon; Bayly, C. & Harper, T. (2005). *Forgotten Armies: The Fall of British Asia, 1941-1945*. Harvard University Press.

⁸ On analytical distinctions of duration and tempo, see Gryzmala-Busse, A. (2011). "Time Will Tell? Temporality and the Analysis of Causal Mechanisms and Processes." *Comparative Political Studies* 44(9), 1267-1297.

⁹ Raffin, A. (2005). *Youth Mobilization in Vichy Indochina and its Legacies, 1940 to 1970*. Lexington Books, 85

¹⁰ Kurasawa, A. (1988). *Mobilization and Control: A Study of Social Change in Rural Java, 1942-1945*. Cornell University; Jaffrey, S. (2019). "Leveraging the Leviathan: Politics of Impunity and the Rise of Vigilantism in Democratic Indonesia." Ph.D. Dissertation, Department of Political Science, University of Chicago, 120-125.

¹¹ Shinozaki, M. (1982). *Syonan, My Story: The Japanese Occupation of Singapore*. Singapore: Times Books International.



STATE CAPACITY DEVELOPMENT IN HISTORICAL POLITICAL ECONOMY RESEARCH

By Volha Charnysh and Pavithra Suryanarayan



Volha Charnysh

is an Assistant Professor of Political Science at the Massachusetts Institute of Technology. Her email address is charnysh@mit.edu



Pavithra Suryanarayan

is an Assistant Professor at the School of Advanced International Studies at Johns Hopkins University. Her email address is psuryan1@jhu.edu

In Fall 2020, a group of social scientists founded [Broadstreet Blog](#), dedicated to the growing field of historical political economy (HPE). Our goal was to encourage interdisciplinary dialogue and inspire better and more wide-ranging HPE research. The blog embraced an inclusive definition of HPE, and its editorial team includes historians, sociologists, economists, and political scientists. Since then, Broadstreet has published 220 posts, accumulated 2,772 Twitter followers, and hosted 92 guests. Here we reflect on what we have learnt from two years on the editorial team.

First, a note on definitional issues—what is HPE? This question generated a lot of discussion in the early days of the blog. For political scientists, political economy (PE) typically signifies work at the intersection between politics and economics, regardless of method. An alternative definition of PE is the study of political institutions and behaviors using game theory or empirical methods of causal inference. It is this latter definition that is increasingly applied to distinguish HPE work from other historical approaches. HPE as a field partly grew out of the credibility revolution in the social sciences. Whereas earlier historical research typically used “thick” descriptive analysis and process tracing with a focus on building an inductive theory, recent HPE scholarship attempts to test existing and new theories using newly digitized quantitative historical data, formal models, and statistical methods. It benefited from the digitization of historical census data and advances in GIS technology.

Broadstreet, however, has adopted an inclusive approach, promoting both quantitative and qualitative research. Our posts have covered topics relevant to the development of HPE as a field, such as the importance of theory, the measurement of key concepts, and

ethics in archival research, in addition to highlighting recent empirical contributions of scholars working in this tradition. For this APSA-CP symposium, we discuss work featured in Broadstreet on of the most prominent topics in HPE: state capacity.

HPE research on state capacity

State capacity is well suited for interdisciplinary collaboration. State development is, on the one hand, a macro-historical process that unfolds over decades and centuries and is shaped by interstate competition, conflicts, geographic endowments, and colonialism. On the other hand, state building involves micro-level political processes, which require an understanding of actor and group-based incentives and technical topics such as measurement, organizational dynamics, and bureaucratic technology. State capacity is also of great interest to those seeking to understand the drivers and determinants of economic change and development. For this reason, posts on state capacity on Broadstreet have come from historians, political scientists, sociologists, and economists.

The HPE research featured on Broadstreet has enabled not only a dialogue between disciplines, but also a re-examination of some of our long-standing beliefs about the origins and effects of state capacity. We examine some of those discussions below as a way to illustrate the strengths and challenges of Broadstreet as an interdisciplinary endeavor.

The origins of state capacity

Canonical texts on the origins of state capacity have emphasized the existential threat of war as a catalyst for elite investments into state-building, particularly taxation (Tilly 1992, Centeno 2002, Besley and Persson 2009). Second-generation research on this topic by HPE scholars has specified the mechanisms through which

wars can build states more precisely, investigated scope conditions under which war strengthens state capacity, and provided causal evidence for this argument. In a forthcoming book, Didac Queralt (2022) shows that wars result in stronger states only when they are financed through taxes. Using data on war financing in the 19th century, he demonstrates that when rulers have access to cheap credit, they have no incentives to invest in building up tax administration and power-sharing institutions.

Some of this new empirical research has problematized our assumptions about the relationship between wars and state building developed from West European cases in the early modern (1500-1700) era. This work shows that bellecist insights are geographically bounded to regions with high political fragmentation (Dincecco and Wang 2018 and this post) and low land to labor ratio (Herbst 2000; Dincecco and Onorato 2017). Research on China has found that war could in fact weaken the bureaucracy by making rulers prioritize loyalty or elite connections over expertise in bureaucratic selection, or strengthen the hand of competitors to the ruler, as the ruler relies on these elites to supply manpower and raise revenues during conflict (Bai, Jia and Yang 2021; Peng Peng 2021). Thus, the context of state building matters a lot and there may be no one process of state building that applies universally. Mid-range theories that are specific to given historical periods or regions may be more applicable. In this way, the use of fine-grained data, large-N analysis, and causal inference techniques has modified the conventional understanding of the origins of strong states.

Other scholars have advanced alternative explanations for state-development by looking deeper in the past and bringing in non-state actors. In a post on state development in Western Europe, Anna Grzymala-Busse showcases the findings of her new book project that challenges bellicist accounts of state-building. She traces the origins of state-building to the early-Modern era, showing that early states emerged out of rivalry with the Church, which served as a source of institutional and conceptual innovations for medieval rulers.

Work on more recent periods, for which subnational census data exist, has drawn attention to the importance of social structure and identity for state-building. In a series of posts on endogenous state capacity, Pavithra Suryanarayan

discussed recent scholarship that takes seriously the idea that political prerogatives shaped the investments that rulers made into the informational or bureaucratic capacities of the state. These studies variously tackle how episodes of franchise extension, occupation, or war shifted political calculations of elites. The posts highlight the role of something state capacity literature rarely considers: ethnic politics. Recent HPE studies have shown that the ethnic identity of governed populations guided strategic state-building. Taking identity seriously reveals that rulers sometimes sacrificed revenue in seeking to build ethnically homogenous populations (Saleh

That said, recent HPE work appears to have pushed the research agenda on state capacity forward precisely because of its focus on specific cases, measures, and attention to causal identification.

and Tirole 2021). Relatedly, rulers faced greater difficulties in collecting information and taxing non-coethnics, which produced considerable variation in state capacity not only at the subnational level but also from one ethnic group to another (Charnysh 2022; Magiya 2022).

In two papers examining colonial India and the post Civil-War United States, Suryanarayan (2022) and Suryanarayan and White (2021) show that elites may deliberately weaken bureaucratic capacity in anticipation of the redistributive effects of democratization. Importantly, elites used the fear of inter-caste or inter-racial integration in these contexts to build cross-class coalitions against taxation. The papers measure group-based inequalities and different forms of capacity—bureaucratic presence, tax collections and information quality.

Relatedly, posts by Yuhua Wang discuss how a country's social structure and elite networks affect its state-building trajectory. In one post, Wang writes on the relationship between kinship

networks and state building using politician-level data from China's state-building reform under Northern Song Dynasty (960–1127). He shows that elites embedded in geographically dispersed networks benefited from a central state and supported reforms that strengthened state capacity (also see Wang 2022).

Overall, a shift away from the Western European state-building experience has led to a growing understanding of the challenges to building capacity in the developing world. State development outside Europe has often occurred under colonialism, under higher levels of ethnic and religious heterogeneity, and in regions with different geographic endowments.

Measurement of state capacity

In another series of posts, Broadstreet editors have examined the operationalization of state capacity and the relationship between its different dimensions. An enduring feature of early state capacity research is the conflation of capacity with a country's level of development. Per capita taxation and GDP per capita are frequently used to measure state capacity at the state level. Beyond taxation, state capacity has also been measured as the ability of the state to enforce property rights, provide public goods, achieve economic growth and to monopolize coercion, as Suryanarayan discusses in this post. All of these measures capture the outcomes of state capacity and conflate its multiple dimensions (see Berwick and Christia 2018).

Recent HPE research addresses these criticisms by distinguishing among distinct dimensions of state capacity above and beyond extractive capacity that is measured by fiscal returns. Charnysh surveys recent quantitative studies on one such dimension: informational capacity. Informational capacity refers to the ability of state officials to collect information about the population and territory. It draws on the concept of legibility proposed by James Scott (1998) and has been operationalized using data on age heaping (Lee and Zhang 2019, Suryanarayan and White 2021) and the coverage of cadastral records (Sánchez-Talanquer 2020, D'Arcy and Nistotskaya 2016). Despite significant demands on data availability, studying age heaping at the subnational level has been enabled by newly-developed technologies such as the OCR that allow scholars to digitize fine-grained information from historical censuses (Charnysh 2022).

Another dimension is bureaucratic or administrative capacity, which builds on Michael Mann's conceptualization of state capacity as the ability of the state to penetrate society. Gafias (2018, 2019) measures the total number of bureaucrats by municipality in post-revolutionary Mexico using occupational census data; Suryanarayan (2022) measures the change in the number of municipal-level bureaucrats between 1920 to 1930 in Indian districts. Suryanarayan and White (2021) show that bureaucratic capacity is correlated with age heaping and tax collection levels in the US South.

Improvement in the operationalization of state capacity has demonstrated that the reach of the state varies within territory and across population groups and that the growth in state capacity was not linear (Garfias and Sellars 2021, Suryanarayan 2022). Improving the operationalization of state capacity has also allowed us to examine the relationship between different dimensions of state capacity and test the relationships between different indicators used to measure them (Hanson and Sigman 2021). For instance, legal and fiscal capacity are hypothesized as growing in tandem historically (Besley and Persson 2009). Nevertheless, newer work suggests that the development of one type of capacity might explicitly weaken others, either through the intention of elites, or through the trade-offs rulers have to make over which type of capacity to prioritize at key moments (Talanquer 2020).

We have used the example of blog posts on state capacity to discuss ways in which editors at Broadstreet have showcased multidisciplinary arguments and findings as well as varying methodological approaches to studying state building. We have found that a majority of the blog's guest posts come from scholars working in the formal or quantitative traditions. For this reason, we hope to highlight two contributors in particular whose historical and qualitative arguments have enlightened the field.

Diana Kim's guest [post](#) showcases how state development can be a "bottom-up" process. Puzzled by why the colonial state rapidly moved away from opium as a source of lucrative tax revenue within a few decades between the late 19th to early 20th centuries, she finds, through her detailed investigative examination of colonial era records in South East Asia, that the crafting of colonial prohibition policy occurred due to decisions made by low and middle-level

bureaucrats. Bureaucrats problematized the colonial state's dependence on opium and eroded its fiscal legitimacy. By tracing communications between bureaucrats and by following the trajectory of specific policies, she charts a new course in state building by placing bureaucrats centrally into the story. Perhaps more importantly, her work highlights the necessity for integration of different types of evidence – interpretive, descriptive and quantitative research.

Social scientists often start with history, but sometimes provoke criticisms of how they treat historical evidence. Tracy Dennison contrasts the way historians and social scientists analyze cases. She notes that the social sciences tendency to analyze institutions as discrete, quantifiable entities contrasts with historians' view that institutions are "interlocking systems" that cannot be studied in isolation (Dennison 2021). Dennison's posts on Broadstreet have repeatedly highlighted these conflicts amongst scholars of different methodological and theoretical orientations.

HPE is sometimes criticized for overemphasis on causal identification, which can lead to asking narrow empirical questions and placing methods and research designs ahead of theory development. Extrapolating from historically-bounded empirical cases can be challenging, as Arturas Rozenas argued in this guest post. In addition, the phenomena that are hard to quantify and map can be left understudied. These are valid criticisms that HPE research of state capacity should take on board. That said, recent HPE work appears to have pushed the research agenda on state capacity forward precisely because of its focus on specific cases, measures, and attention to causal identification. It accomplished this by reexamining earlier theories in a causal framework, developing new measurement strategies for complex theoretical concepts such as state capacity, by highlighting subnational variation in the reach of the state, and by identifying endogenous processes that influence the process of state building.

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Anna Grzymala-Busse

is the Michelle and Kevin Douglas Professor of International Studies in the Department of Political Science, the Director of the Europe Center, and Senior Fellow at the Freeman Spogli Institute. Her email address is

amgbusse@stanford.edu

EVERYTHING EVERYWHERE NOT ALL AT ONCE THE CHALLENGES OF HISTORICAL ANALYSES

By Anna Grzymala-Busse

Since at least Otto Hintze and Max Weber, comparativists have been fascinated with long-run historical puzzles. We have traced the impact of early state centralization in China, colonial rule in Malaysia, and authoritarian legacies in Africa and Europe, and how urbanization, the Crusades, and dynastic unions influenced the politics of their time. We have also explained phenomena ranging from authoritarian durability to contemporary voting patterns to levels of modern corruption with historical legacies that sometimes reach back centuries.

These accounts can be grouped into two categories. The first adjudicates among the multiple causes of an outcome. Sometimes known as Comparative Historical Analysis (CHA), this approach relies on structured comparisons that often use historical process tracing. It shows strong affinities to what historians do: sit comfortably with the compound causes of a particular outcome, attending to context and complexity. The second approach traces the effect of a single causal factor. Known as Historical Political Economy (HPE), it shares epistemological roots with the turn in economics to causal identification, although its substantive focus is far broader. The emphasis here is on cleanly tracing causal impact of a given factor, obtained through experiments or quasi-experimental identification strategies, and features prominently both in the *Broadstreet* blog, and a forthcoming handbook from Oxford University Press.

Simpser, Slater, and Wittenberg 2018 and Cirone and Pepinsky 2022 have both brilliantly reviewed the literature on historical legacies and historical

persistence, respectively. Here, I first identify some commonalities and divergent concerns with causal mechanisms versus causal factors. I then turn to three common challenges faced by both CHA and HPE: historical data, the changing impact of historical legacies, and difference between persistence and reproduction.

Both CHA and HPE attempt to identify the causal forces at work, and they share the same emphases on identifying causes, cognizant of their multiplicity, their complex interactions, their appearance at different points in time, and their different roles at various points in time. However, they focus on distinct aspects of causation.

CHA focuses on causal mechanisms: the ways in which historical causes are reproduced, reified, or undermined over time. Mechanisms are not intervening variables, or a way of increasing the variance explained. Rather, they are “recurrent causal links between specified initial conditions and outcomes” that specify change: how and why we see shifts, trends, and developments (Grzymala-Busse 2011). They are not directly observable, but an extrapolation of causal mechanisms through process tracing and other strategies is often considered necessary (but not sufficient) for CHA analyses.

In contrast, HPE focuses on causal factors: specific variables that exhibit a causal relationship to the outcomes of interest. The potential outcomes framework frequently used in HPE analyses does not require that a causal mechanism is specified: it is the causal effect, rather than the mechanisms,

that is the core focus of these analyses. There is nothing in the toolbox of causal identification strategies that isolates mechanisms, and they are often a distinct afterthought. Precisely because causal identification strategies can establish a causal effect without specifying mechanisms, many analyses rest there.

The two approaches thus rest on distinct logics of historical analysis and causation. They potential complements, but not substitutes. A focus on causal mechanisms and context can enrich the claims of HPE and give greater plausibility to the claims of causation: not just that a cause is present, but that we know how it contributes to the outcome. The emphasis on demonstrating causation through credible research design could make some CHA analyses more plausible: even if an experimental or quasi-experimental design is not possible, scholars working in this tradition can trace and minimize potential confounders that underlie both the purported cause and its outcome.

These different analytical frameworks also inform how CHA and HPE approaches contend with the same messy complexity of historical research.

First, historical data presents its own challenges: data that is missing, and systematically so (thanks to everything from natural disasters to colonial differences to state capacity). CHA has to contend with overdetermined outcomes and multicollinearity: historical factors and legacies often go together and are correlated to each other, so that their joint explanatory power is considerable, but it is not clear what any one individual legacy does, and statistical estimates become unstable (Pop-Eleches 2007, 920.) HPE faces the challenges presented by historical data in causal identification: historical instrumental variables (IV) rarely satisfy the exclusion restriction, for example, since there are multiple ways through which the IV could influence the outcome (see Lal, Lockhart, Xu and Zu 2021), while canonical difference-in-difference designs are not intended for multiple entries into treatment, characteristic of historical processes (see Goodman-Bacon 2021, Callaway and Sant’anna 2021).

Second, historical causes can vary over time, and across contexts. Yet many historical analyses, especially those focusing on historical

legacies, rely on an unspoken assumption of causal consistency or durability, which we can think of as a vector of duration (persistence) and stability (lack of variance). HPE analyses often assume that legacies of the past persist unadulterated, or that causal factors have a constant impact. Yet while some historical factors persist over time, other legacies attenuate, grow stronger, or appear and reappear depending on circumstances. For example, the impact of institutional and cultural legacies of communism on democracy scores actually increased over time (Pop-Eleches 2007.) One possibility here would be to demonstrate this durability directly, and show that the expected relationships hold across time. If medieval antisemitism explains the Nazi vote, for example, we would also expect it to be associated with early modern pogroms, support for exclusionary welfare policies in the nineteenth century, and so on. The impact of historical factors can also be contingent on contemporary political context: voters whose families had seized the assets of Holocaust victims in Poland were more likely to vote for extreme-right parties, but only after Holocaust culpability and reparations became a politically salient issue (Charnysh and Finkel 2017). Context also matters for the impact of causal factors: state antiquity has been found to cause state institutional quality in some studies (Bockstette et al 2002) and to undermine it in others (Hariri 2012).

On a related note, we also still know relatively little about when historical factors and causes cease to matter, and the half-lives of historical processes, factors, and legacies. If it does, how does their impact taper off? Both HPE and CHA analyses locate the origins of historical persistence; but they are less focused on when it ends, and how. These decay rates are rarely specified. Given the complexity of historical arguments, and the different outcomes they may affect, we need new frameworks to investigate how and why some long-term

causal factors and historical legacies can wither away while others remain.

Third, continuity is distinct from reproduction, as Cirone and Pepinsky 2022 point out. Some outcomes may be the result of a simple accumulation, attrition or constant change from a starting point, or of a deterministic sensitivity to early conditions (Page 2006). The analysis of causation here is relatively straightforward in that we can assume a constant causal effect. Other outcomes (or distributions of outcomes) depend not only on previous states but also on their sequencing and non-linear, non-deterministic mechanisms of reproduction such as individual experience, increasing returns to education, negative externalities produced by institutional policies, etc. Here, we cannot assume a constant causal effect and instead

have to examine the potential mechanisms of reproduction of a given effect. Comparative historical analysis can sort out these dynamics in ways that historical political economy does not attend to. This is not to say that all analyses require that we deduce or demonstrate causal mechanisms; rather, we need strong reasons to think that a

given effect is constant over time. Stronger theory can help us to specify whether to expect reproduction or persistence, and thus which kind of evidence we will need to bring to bear.

I have faced, and tried to address, some of these challenges in my own work on the religious roots of the European state (Grzymala-Busse forthcoming). I develop historical narratives about the role of the church in the fragmentation of Europe, institutional diffusion, and the development of human capital, law, and representation, and buttress these with models that mimic difference-in-difference designs. I address potential confounders by attending to the sequencing of historical developments and eliminating insofar as possible factors that could cause both the power of the church and development of the

Given the complexity of historical arguments, and the different outcomes they may affect, we need new frameworks to investigate how and why some long-term causal factors and historical legacies can wither away while others remain.

state, such as the legacies of Roman empire, the rise of self-governing cities, or conflict. The threats to inference are similar to the ones identified by Cirone and Pepinsky: missing data, spatial dependence, and post-treatment bias (including variables that occurred after the “treatment” that could mediate its impact on the outcome). In the end, the findings do not indicate that mine is the only possible account: only that it is likely, and likely to be at least as powerful as others.

Comparative historical research is constrained by deeper epistemological forces: both by our inability to randomly assign people, countries,

and institutions to different treatments, and by our inability to demonstrate causal mechanisms directly. What we lose in pristine identification, however, we gain in new appreciation for the complexity of historical forces, and the shared challenges of historical scholarship.

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COMPARATIVE HISTORICAL ANALYSIS LOOKING BACKWARDS AND FORWARDS¹

By Deborah J. Yashar



Deborah J. Yashar

is Professor of Politics and International Affairs and lead editor of *World Politics*. Her email address is dyashar@princeton.edu

Hegel remarks somewhere that all great world-historic facts and personages occur, as it were, twice. He has forgotten to add: the first time as tragedy, the second as farce....

Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly found, given and transmitted from the past.

- Karl Marx, *The Eighteenth Brumaire of Louis Bonaparte*

In *The Eighteenth Brumaire of Louis Bonaparte*, Marx anticipated the core concerns of comparative historical analysis (CHA). He prompted us to consider if history repeats itself, with the past providing a mirror of the future; if so, such a pattern would lend itself readily to comparing units across time. And yet, Marx argued for greater analytical and political caution since the past does not simply repeat itself; provocatively and memorably, Marx argued that it appears first as tragedy and then as farce. If history indeed shapes (even distorts) what is to follow, social scientists must accordingly understand both the conditions in which history unfolds and the ways in which political actors seek to rise above the conditions they inherit. Yet actors can never fully make history as they wish precisely because history, in all its torturous grandeur and complexity, weighs on the future (oftentimes in unexpected ways).

A long line of scholars have since grappled with the kinds of foundational questions that grow out of such observations, including: What can we learn from history and how to study it? What are the

structural conditions that constrain the choices of the present? What room is there for rupture, agency, and enduring change? This newsletter asks us to tackle deep questions such as these in light of the CHA tradition. My comments will necessarily fly high.

Comparative Historical Analysis and Political Economy²

Before evaluating comparative work that incorporates history as well as political economy, it is essential to provide a bit of conceptual background and historiography.

What kinds of history matter, how, and why? Here is where the classic work in comparative historical analysis turned to political economy. In its earlier incarnation, political economy was not a “method” as much as a theoretical claim that politics and economics shaped one another. If Marxists argued that certain material/economic conditions provided the structural foundations on which all politics played out, Weberians argued for the greater autonomy of the political (institutions, leaders, and ideas) as it shaped outcomes. Those working in a political economy tradition sought to make sense of relationships between these macro-spheres – including work by Barrington Moore (1966); Guillermo O’Donnell (1973); Jeffery Paige (1975); Theda Skocpol (1979); Fernando Henrique Cardoso and Enzo Faletto (1979); Peter Evans (1979); Peter Hall (1986); Gøsta Esping-Andersen (1990); Ruth Berins Collier and David Collier (1991); Dietrich Rueschemeyer, Evelyne Huber Stephens, and John D. Stephens (1992); among many others. To say that the economy mattered was simply the start of questioning which features of the economy explained a given outcome: capitalist markets or

communism; agricultural and/or industrial economies; domestic units and/or the international system, etc. Alternately, to say that politics mattered opened debates about which political actors mattered (classes, the state, political parties, and/or coalitions that defined the range of probable outcomes), what their preferences were, and the conditions under which they could forge or sustain outcomes.

Indeed, the emerging comparative historical analysis moved away from a mechanical reading of economic structure and challenged teleological approaches explaining development and modernization. In turn, it highlighted the divergent pathways that countries followed (and sometimes the multiple pathways that countries followed towards the same endpoint). In this mid-level theorizing, these works shared a commitment to look at the historical conditions (for some economic structures; for others political institutions) that shaped long-term trajectories via different mechanisms, including historically defined preferences, class-based organizing, state autonomy, and political alliances. Overall, most of this work was theoretically driven with a preference for macro and meso-level analysis that took organizations and groups seriously (over the actions of individual/micro-level analysis), and an effort to pinpoint the mechanisms that mattered over time (both generative and reproductive, recalling Stinchcombe 1968). So too, much of this work adopted what we now refer to as a critical juncture approach (with the juncture identified by theoretically informed and empirically identified ruptures) that presumed path dependence. Many have written about critical junctures (including Collier and Collier 1991; Mahoney 2001; Capoccia and Keleman 2007; Slater and Simmons 2010; and most recently Collier and Munck 2022).

The foundational work in CHA (often following Barrington Moore) featured ambitious macropolitical questions and bold claims complemented by efforts to trace historical arcs—some pinpointing the role of commercial agriculture and classes; others focusing on capitalist industrialization and the rising working class; others the role of international capital in a dependent economy; among other features. This work was informed by theoretical

debates about political economy (the relationship between politics and economics); and much of it set out to understand the structural versus contingent nature of this process. What this literature sometimes lacked in parsimony it gained in theoretical innovation, especially relative to the behavioralist and presentist literature of the time.

Those who subsequently took up the CHA mantle set out to ask parallel macro-level questions (regime politics, revolutions, industrialization, economic policy, party systems, etc.) but did so with more analysis of the autonomy of the political and more attention to methods. They sought to do it better—with more attention to methodological rigor, scope conditions, mechanisms, timing, and sequence. In the early 1990s, Collier and Collier (1991) and Rueschemeyer, Stephens and Stephens (1992) were exemplars of this analytical move.

Methods

Today, CHA is methodologically pluralist. It encompasses qualitative and quantitative work – even if its progenitors were largely qualitative (Rueschemeyer, Stephens and Stephens 1992; and Paige 1975, notwithstanding). But let us pause for a moment to consider the virtues of each approach, if done separately. Qualitative work has the virtue of knowing a case(s) well and being able to evaluate and adjudicate among mechanisms that aim to link theoretical claims about cause and effect in specific places and with specific actors. It also has the virtue of paying attention to the timing and sequencing of factors. It can entertain contingency, concatenation, and endogeneity as theoretical arguments rather than methodological boogymen. In doing so, it historicizes the work beyond identifying key variables to understand their relationship and effects. In turn, it might generate unexpected insights (with theoretical implications) that might not have been anticipated prior to the research (especially where there is original fieldwork). That said, well-rehearsed criticisms note that it has problems associated with too many

variables given the number of cases, complexity, external validity, among other concerns.

Many scholars have since turned to quantitative work that incorporates and privileges sophisticated causal identification strategies. While some of this work has been cross-national and macro-comparative (consider the work by Acemoglu, Johnson, and Robinson 2006), much of it has become increasingly attentive to micro-level behavior within a given country (consider Acharya, Blackwell and Sen 2016). Often and increasingly, this work has appropriated the term political economy to mean the formal and quantitative methods deployed to study politics (usually focused on micro-foundations) rather than the theoretical question of how politics and economics constitute one another (as emphasized by the earlier work).

When evaluating CHA, it is key, therefore, to consider what history means and how it used – whether we are talking about historical conditions that shape what ensues or historical preconditions which, when met, increase the likelihood of democracy taking hold.

This is a notable repurposing of a term that was once associated with a theoretical question for a term defined by a methodological orientation (and privileging a micro-level analysis). While this work has importantly

provided an ability to specify statistical significance, probabilities and micro-level foundations, it has often done so with a notable tradeoff. Mechanisms are plausibly asserted more often than empirically substantiated; timing and sequence are often elided; and contingency is often written out of the narrative by virtue of the method that is used. This methodological approach has, moreover, often replaced explaining given outcomes (in particular places) with explaining the effects of possible causes (evaluating statistical significance across cases, even if the magnitude proves to be quite small).

It is therefore not surprising that mixed methods (combining the strengths of qualitative and quantitative work) has proven so productive in recent years. In the field of

comparative politics, the award-winning comparative work by Albertus (2015), Singh (2015), and others stand out for combining quantitative and qualitative work within the traditions of CHA. Lieberman (2001 and 2005), in particular, has provided important insight into how best to pursue mixed-method work in ways that self-consciously explore methods with the question at hand, the state of the theory (theory building versus theory testing), and the accessibility and reliability of data. While qualitative work is often designed to explain outcomes with a small N, quantitative work is designed to explain the probability that certain factors have a statistically significant impact on a large N. These are different modes of answering questions, but they are complementary—especially if informed by a shared set of concerns and theoretical priors. Mixed-method work, after all, is precisely the effort to combine these methods (recognizing that they have different strengths and weaknesses) to adjudicate among different arguments and often levels of analysis. If we are to take Marx’s opening salvo to heart, we have to remember that whether we do qualitative or quantitative work, both have to be mindful of if and how history matters. One cannot presume that all cases are exactly the same; nor that all actors are operating in equally comparable circumstances. History requires us not only to search history for comparable data but to probe its historicity and to consider timing, sequence, and contingency.

CHA and Democracy

A targeted discussion of the regime literature provides an opportunity to identify some issues that CHA has and should privilege in debates about democracy: namely, how history matters; historicizing concepts and theory; and contemplating historical lock-in.

Different Uses of History: History matters but it often does so in different ways across the comparative literature. Some CHA work has specified how prior historical conditions in the economy and/or political institutions shape what is likely to occur (think Moore 1966; Dahl 1971; Rueschemeyer, Stephens, and Stephens 1992; Bermeo 2003; Yashar 1997; Mahoney 2001; Tudor 2013; Riedl 2014; Berman 2019; etc.). Some other work seeks to trace the historical developmental arc that indicates at what stage countries are most likely to

democratize (think Lipset 1959; Przeworski and Limongi 1997; Boix and Stokes 2003, Acemoglu and Robinson 2006). These two general approaches assume different roles for history. One locks in what is possible going forward. The other highlights where along the developmental arc one can say that democracy is most/least possible. Both might pay attention to timing and sequence, both might tend towards structural arguments, and both might consider conflict and tradeoffs. Nevertheless, the former’s emphasis on historical legacies contrasts with the latter’s emphasis on the economic conditions when democracy is more/less possible. When evaluating CHA, it is key, therefore, to consider what history means and how it used – whether we are talking about historical conditions that shape what ensues or historical preconditions which, when met, increase the likelihood of democracy taking hold. In either case, we would do well to “read history forward” (Capoccia and Ziblatt 2010).

Historicizing democracy (and its theoretical implications): Much of our most recent literature identifies and explains democracy across time and space—often as if it were the same outcome. Yet, the meaning of democracy has not stood still; it has changed over time. Democracy initially began with a highly restricted group of voters and elected officials. Moreover, expectations of what democracy would deliver was more limited. So too, its institutions and suffrage extension unfolded more slowly and in a quite different political economy than what transpired later in the 20th century. These observations raise important conceptual (and related theoretical) questions for our historical theories of democracy, especially if we seek to compare outcomes across time and space. First, should we identify cases as democratic (assuming they meet a historically defined institutional minimum) even if they excluded most of the adult population save white (propertied) men? I think there is ample reason to come up with other terms for competitive regimes that give voice to segmented populations while excluding others along gender, racial, class, and other lines. As Dahl (1971) famously argued, regimes can vary in terms of inclusion and contestation, and we might be better off labeling the more restrictive regimes with other terms, such as polyarchy.³ Second, once we recognize both that the thresholds for contemporary democracy are

considerably higher than in 19th century and that transitions happen more rapidly now than before, we might further interrogate if theoretical arguments need to be further tailored to explain different waves of democracy (as argued by Bermeo and Yashar 2016). Much as Gerschenkron (1962) argued that later industrializers might require different factors to jumpstart industrialization, so too we might reasonably question if the same theories can explain both 19th and 21st century democratization, given the latter’s more demanding conceptual and institutional threshold and more compressed timeframe for regime transition. Surely a very high level of abstraction might be able to theorize over time. But if we take CHA seriously, then we would be well advised to theorize how historically different moments and conditions differentially shape older and newer pathways to democracy.

Contemplating the Limits of Historical Lock-In? Many classic theories assumed a lock-in effect in advanced industrial economies in the post-WWII period (on increasing returns, see Pierson 2000). Redistributive conflict theories seem to boast such assumptions (consider Przeworski and Limongi 1997; Boix 2003; Acemoglu & Robinson 2006). Some statistical work highlights that the longer democracy exists, the more likely it will continue to persist (Gerring et al 2005). This set of theoretical claims coincided with basic modernization theory assumptions that more advanced democracies had passed a threshold beyond which democracy would endure. Yet, we have seen two important challenges to this argument, both of which highlight how actors maneuver within structural constraints (sometimes to reform, sometimes to rupture, sometimes purposefully, or sometimes by mistake). First, an important line of work has highlighted the limits of structurally deterministic forms of historical institutionalism, arguing that we must also pay attention to actors who maneuver within existing macro institutions to effect micro-level institutional change (on this general point, see Thelen 2004; and Mahoney and Thelen 2015). An older and newer line of argumentation has focused on the actors who try to effect regime change—whether it be leaders who make poor decisions that facilitate democratic breakdown (Linz 1978; Stepan 1978), actors who strategically maneuver to effect transitions from authoritarian rule (O’Donnell and

Schmitter 1986), or autocrats who make mistakes that precipitate democratization (Treisman 2020); in these examples, actors make a difference—raising the question of when and why political actors can transcend structural constraints and effect changes not predicted by historically defined institutions. Second, historical lock-in arguments are also challenged by contemporary events that include democratic backsliding and democratic breakdown in longstanding democracies (see Bermeo 2016, Waldner and Lust 2018, Levitsky and Ziblatt 2018). If backsliding and breakdown have taught us anything, it is that we should not be so sanguine as to take democratic institutions for granted. Sometimes actors erode democracy and the question is when, how, and why they are successful in doing so (whether the structural conditions have changed, leaders have gone rogue and effectively destroyed institutions, organizations have realigned in ways that no longer support democratic practices, and/or societal preferences have shifted). Whether considering theory or empirics, the two broad challenges just highlighted emphasize the need to consider if and why historical legacies endure and if the mechanisms for doing so have changed over time. CHA provides the opportunity to evaluate historically deterministic structural conditions relative to the impact of leadership (mis) calculation and unintended outcomes.

Stepping back, if CHA has profoundly shaped theoretical debates about democracy and

authoritarianism, so too much work still remains to be done to precise how history matters across theories, cases, and time. The next generation would do well to ask not only how history matters but also to further probe the mechanisms of reproduction, erosion, and rupture.

In Conclusion

Comparative historical analysis encompasses a rich and capacious research agenda, with varying ways to analyze political economy and deploy methods. Political economy as theory has its place, as does multimethod work that can speak to different sets of questions: critical junctures, timing, sequence, contingency, complexity, enduring legacies, among other issues. I say: let a hundred flowers bloom. But let's not forget that history is not just a data source to be mined; it is also a record of meaning whose complexity needs to be unpacked and whose mechanisms need to be understood if we are to move beyond tragedy and farce to creatively probe the deep and enduring political questions of our time.

Notes

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² A significant literature assessing comparative historical analysis and comparative political economy includes Evans and Stephens (1988), Mahoney and Rueschemeyer (2003), and Mahoney and Thelen (2015). On historical institutionalism, in particular, see Steinmo, Thelen, and Longstreth (1992) and Fioretos, Falleti and Sheingate (2016).

³ In this essay, I adopt Dahl's (1971) definition of democracy, as amended by Schmitter and Karl (1991).

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MEASURING REVOLUTIONS

By Adam E. Casey



Adam E. Casey

is a Postdoctoral Fellow at the Weiser Center for Emerging Democracies at the University of Michigan. His email address is aecasey@umich.edu

In his new book, *The Revolutionary City*, Mark R. Beissinger uses original data on revolutionary episodes to build a compelling argument about how urbanization has transformed modern revolution.¹ Beissinger's book weaves high-quality empirical data with an elegant argument tested at multiple levels of analysis.

At its core, the book relies on a data set which covers 345 'revolutionary episodes' from 1900-2014.² Beissinger defines a revolution as "a mass siege of an established government by its own population with the goals of bringing about regime-change and effecting substantive political or social change."³ Beissinger understands revolutions to be relatively broad yet still restrictive: military coups without any mass mobilization are not revolutions, neither are electoral turnovers or political reforms by existing governments (including 'revolutions from above'). For Beissinger, all revolutions seek to "achieve power and to bring about substantive change."⁴

In addition to providing the empirical support for his argument, Beissinger's data set is a considerable asset to the field of comparative politics. There are three principal advantages to the way Beissinger has gathered and structured his data. First, the data are contextually rich. Entries in the data set are given as revolutionary episodes (n=345, 1900-2014) with narrative descriptions. Episodes include information on the location and timing of revolutionary contention (down to the month and day); the goals of the rebellion; forms of contention; features of incumbent regimes before the contention; peak participation size estimates; rebellion dynamics; violence and death; relationship to other revolutionary episodes; and outcomes of contention. The data set also includes information on 131 episodes that came close to meeting the threshold for inclusion but nevertheless fell short.

Revolutions are also disaggregated by type, allowing scholars to distinguish social from political revolutions as well as the urban or rural nature of the contention.

Second, the data set includes both successful and failed revolutions. This allows scholars to examine the determinants of revolutionary success as well as the consequences of successful revolution for other political and economic outcomes. This will allow scholars to assess systematically why some countries and time periods saw 1) no major revolutionary challengers emerge; 2) mass revolutionary challengers emerge but fail; and 3) revolutionary challengers emerged and succeeded in seizing power. By merging Beissinger's data with other data sets, scholars can examine the relationship between successful and failed revolutions and repression, democratization, international and domestic conflict, economic growth, and authoritarian durability.

Third, the data set meets rigorous standards of data transparency. Coding decisions are given in narrative form with citations from the source materials used to make the assessment. This is the gold standard for cross-national comparative historical data collection. Unlike popular data sets which rely on opaque expert surveys where contemporary experts are asked to retrospectively assess how they would code historical cases on a variety of indicators, the Beissinger data follows other high-quality data in comparative politics like the Geddes, Wright, and Frantz (2018) autocracy data set⁵ and the Colpus coup data set⁶, in providing transparent sourcing materials. This enables other scholars to see on the basis of which information inferences are made and challenge the individual coding decisions of the data set.

[Comparison with Other Data on Revolutions](#)

Beissinger's conceptualization of revolution is less restrictive than the definition used by Lachapelle, Levitsky, Way, and Casey (LLWC) in their recent and forthcoming work on revolutions.⁷ These authors restrict their focus to "social revolutions" which are defined as "the violent overthrow of an existing regime from below accompanied by mass mobilization and state collapse, which triggers a rapid transformation of the state and the existing social order."⁸ Beissinger also acknowledges a difference between social and political revolutions, both in his manuscript and in his data set.⁹

Beissinger and LLWC agree that revolutions are distinguished from other modes of irregular regime change by the nature of the core actors presiding over the removal of the incumbent regime. Revolutions emerge 'from below' in that they are led by outsiders and not current incumbents or members of the existing state. They are also events that feature mass participation. The two data sets differ in the emphasis placed on the transformation of the state, the necessity of violence, and the nature of revolutionary goals.

Unlike LLWC, Beissinger does not consider the use of violence or the transformation of the state as necessary for a mass-led regime change to constitute a revolution.¹⁰ He also allows for a broader conception of the transformative goals of revolutionaries. For LLWC, revolutionaries seek "radical change" that attacks "the core interest of powerful domestic and international actors or large societal groups."¹¹ For Beissinger, revolutionaries need only seek to "bring about substantive change."¹² Beissinger considers social revolutions to be those espousing leftist goals, or as he puts it, goals "aimed at the transformation of the class structure of society." LLWC are broader in their conceptualization of goals aimed at radical social transformation, allowing for any actions which include attacks on powerful domestic and international actors or large societal groups. This includes coercive land redistribution, campaigns to destroy preexisting cultures, religions, or ethnic orders, efforts to impose new rules governing social behavior,

and foreign policy initiatives "aimed at spreading revolution and transforming the regional or international order."¹³

Unsurprisingly, these conceptual differences lead to different codings of social revolutions. In general, there is broad agreement between Beissinger and LLWC on successful social revolutions. However, there are some notable disagreements. Beissinger includes seven successful social revolutions not included by LLWC: Afghanistan (1978), Congo (1963), Namibia (1990), Portugal (1974), South Africa (1994), South Yemen (1967), and Zimbabwe (1980). LLWC exclude these cases as non-revolutionary for emerging within the state (Afghanistan 1978, Congo 1963, Portugal 1974),

Beissinger and LLWC data for an assessment of the emergence of mass social revolutionary movements, their success and failure, the pursuit of post-revolutionary social transformation, and the durability of postrevolutionary political order.

***Unlike LLWC, Beissinger does not consider the use of violence or the transformation of the state as necessary for a mass-led regime change to constitute a revolution. He also allows for a broader conception of the transformative goals of revolutionaries (...)
For Beissinger, revolutionaries need only seek to "bring about substantive change."***

refraining from fundamentally transforming the state after coming into power (South Yemen 1967, Zimbabwe 1980), refraining from attempting radical social transformation by the end of the first year in power (Namibia 1990), or for ushering in democracies which do not engage in attempts at radical social transformation or fundamentally change state structures (South Africa 1994).¹⁴

LLWC include Finland 1918 (coded as a failed revolution by Beissinger), Afghanistan 1996 (coded as an Islamist rather than social revolution by Beissinger), Rwanda 1994 (coded as an ethnic rather than social revolution by Beissinger), and the Albanian 1944 and Yugoslav 1945 revolutions, which are not included in Beissinger's data set.¹⁵ The remaining revolutions coded by LLWC also appear in Beissinger as social revolutions.

Given the broad agreement on case codings and the conceptual alignment for the category of social revolutions, it is possible to merge the

Notes

- ¹ Beissinger, Mark R. 2022. *The Revolutionary City: Urbanization and the Global Transformation of Rebellion*. Princeton: Princeton University Press.
- ² Beissinger 2022, 48.
- ³ Beissinger 2022, 25.
- ⁴ Beissinger 2022, 24-25.
- ⁵ Geddes, Barbara, Joseph Wright, and Erica Frantz. 2018. *How Dictatorships Work: Power, Personalization, and Collapse*. New York: Cambridge University Press.
- ⁶ Chin, John J., David B. Carter, and Joseph G. Wright. 2021. "The Varieties of Coups D'état: Introducing the Colpus Dataset." *International Studies Quarterly* 65, no. 4: 1040-1051.
- ⁷ Lachapelle, Jean, Steven Levitsky, Lucan A. Way, and Adam E. Casey. 2020. "Social Revolution and Authoritarian Durability." *World Politics* 72, no. 4: 557-600; Levitsky, Steven and Lucan A. Way. 2022. *Revolution and Dictatorship: The Violent Origins of Durable Authoritarianism*. Princeton: Princeton University Press.
- ⁸ Lachapelle et al. 2020, 559.
- ⁹ Beissinger 2022, 3-4, 10, 66, 69-70, 442-59.
- ¹⁰ Beissinger 2022, 3, 25.
- ¹¹ Lachapelle et al. 2020, 560.
- ¹² Beissinger 2022, 3, 25.
- ¹³ Lachapelle et al. 2020, 560.
- ¹⁴ Casey, Adam E., Jean Lachapelle, Steven Levitsky, and Lucan A. Way. 2020. "Revolutionary Autocracies, 1900-2015." *Codebook*, 31, 33, 48, 52-53.
- ¹⁵ Beissinger 2022, 443.

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RESPONSE TO ADAM CASEY

By Adam E. Casey



Mark R. Beissinger

is Henry W. Putnam Professor in the Department of Politics at Princeton University. His email address is mbeissin@princeton.edu

I thank Adam Casey for his careful review and analysis of my “Revolutionary Episodes Dataset,” which is currently available for download by researchers at my website (<https://scholar.princeton.edu/mbeissinger/software/revolutionary-episodes-dataset>). Let me add a few further observations.

The most contentious issue in the study of revolutions is the definition of revolution itself. In *The Revolutionary City*,¹ I defined a revolutionary episode as a mass siege of an established government by its own population with the goals of bringing about regime-change and effecting substantive political or social change.² I understood revolution as a distinct mode of regime-change and chose a broad definition precisely because I was interested in examining how revolution as a political project of regime change from

below has evolved over the past century. As I detail in the book, since its invention in the seventeenth and eighteenth centuries, modern revolution has been used for a wide diversity of purposes: transforming the class structures of society; converting monarchies into republics; attaining civil liberties, establishing a democracy, or containing the abuses of a despotic regime; liberation from colonial rule or independence from a multinational state; inverting a racial or ethnic order; substituting a religiously based political order in place of a secular one; and other aims. Over the last hundred years, revolutions have altered in their purposes

and locations, the forms they have assumed and the processes that they involve, the social forces that they mobilize, and the consequences that they bear for politics and society. My definition sought to provide a sampling frame for capturing these variations.

I considered social revolution to be one type of political revolution that is distinguished from others by its focus on the transformation of the class structures of society. This accorded with Skocpol’s definition of social revolutions as “rapid, basic transformations of a society’s state and class structures that are accompanied and in part carried

through by mass based revolts from below.”³ In contrast to Lachapelle, Levitsky, Way,⁴ and Casey and Levitsky and Way,⁵ I put class at the center of my definition of social revolution, as class relations and animosities have played a principal role in most major

I put class at the center of my definition of social revolution, as class relations and animosities have played a principal role in most major theories of social revolution. A too broad definition of social revolution complicates any attempt to provide an explanatory theory of these revolutions.

theories of social revolution. A too broad definition of social revolution complicates any attempt to provide an explanatory theory of these revolutions.⁶

I also extracted the degree of violence associated with revolution and the actual changes achieved after revolution from the definition of revolution, instead turning these issues into empirical questions meriting their own investigation.⁷ Too often these questions have been overlooked in the study of revolutions through a definitional sleight of hand, with the study of civil wars separated from the study of revolutions,⁸ and the consequences of

revolutions (as well as of failed revolutions) largely ignored. These questions need to be placed centrally in our research agendas. I used the revolutionary episode as my basic unit of analysis, including both successful and failed attempts at revolution, in order to probe why revolutionary opposition succeeds or fails in gaining power and the factors associated with this. A full conceptualization of revolutionary episodes and how they relate to other political phenomena and modes of regime-change can be found in the data description accompanying the dataset.

The data description accompanying the dataset also compares the coverage of the “Revolutionary Episodes Dataset” with that of two other somewhat analogous datasets: Djuve, Knutsen, and Wig’s “Historical Regime Data” on instances of regime-change from 1789 to 2016;⁹ and the NAVCO 1.3 data on so-called maximalist campaigns from 1900 to 2019.¹⁰ The former does not include failed revolutions in its purview, while the latter includes numerous episodes that do not qualify as revolutionary by my definition.

Hybridity is an inherent element of revolutions in that they can simultaneously involve multiple purposes (for instance, liberal demands and independence from a multinational state, or class transformation and liberation from colonial rule), use multiple tactics (e.g., riots, strikes, armed insurrection, and demonstrations), or occur predominantly in the city, the countryside, or both. Some revolutions precipitate military coups. Others end in power-sharing agreements. The coding scheme used in the “Revolutionary Episodes Dataset” remained sensitive to these issues by allowing episodes to be classified in multiple categories when appropriate, avoiding some of the pitfalls of exclusive, dichotomous categorizations. Relatedly, I did not classify revolutions as “violent” or “nonviolent,” since all revolutions involve some degree of violence or threatened violence, and “nonviolent” revolutions can evolve into significantly violent rebellions. Rather, I preferred to code revolutions as armed or unarmed and to record the number of people who died in revolutionary contention to measure the degree of violence involved.

Finally, as Adam Casey noted, the dataset aimed at a high degree of transparency. For each episode, a short narrative was composed and is hyperlinked into the dataset; it provides a quick reference for researchers on the events of the episode and notes on classification. The dataset also includes hyperlinks to the sources consulted for each episode and a bibliography of further sources, so that researchers can follow up with these sources as need be.

Notes

¹ Beissinger 2022.

² For similar definitions, see Trotsky 1932, Tilly 1978, Tilly 1993, Goldstone 2001, 2013.

³ Skocpol 1994, 5.

⁴ Lachapelle, Levitsky, Way, and Casey 2020.

⁵ Levitsky and Way 2022.

⁶ As I show in *The Revolutionary City*, a universal theory of the causes of revolutions more generally remains elusive, largely because of the variety of purposes and social forces involved in revolutions.

⁷ Among other topics, the book explores the changing relationship of violence to revolution as well as the evolving character of post-revolutionary regimes and the substantive changes that they introduce.

⁸ For a critique of the artificial division between the literatures on revolution and civil war, see McAdam, Tarrow, and Tilly 2001.

⁹ Djuve, Knutsen, and Wig 2020.

¹⁰ Chenoweth and Shay 2020.

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ASPA 2022 ANNUAL MEETING

The theme for the ASPA 2022 Annual Meeting explores the ways in which our discipline has had to Rethink, Restructure, and Reconnect in a Post-pandemic Era. The conference organizers were, perhaps, a bit too optimistic in their choice of a theme. The COVID-19 pandemic is still very much with us. In spite of the ongoing challenges, the papers and panels that represent the Comparative Politics division highlight the remarkable resilience and innovation of our field. Our division brings together a diverse set of emerging and established scholars with expertise from multiple regions to debate some of the most pressing issues in political science. Here is what to look forward to in Montreal.

A mini-conference on *Historical Approaches to Comparative Politics* explores how archival materials—and history more generally—inform contemporary politics. The mini-conference includes papers on the historical foundations of inequality in the global south, comparative state-building in China and Western Europe, the impact of colonial legacies, and the strengths and weaknesses of qualitative and quantitative methods of historical political economy.

Our division also showcases panels at the frontiers of conceptual research in comparative politics. This includes a roundtable discussion, *Reconceptualizing Regions in a Multipolar World*, that debates not only what defines a region and the critical junctures at which they are redefined, but also the ordering principles, boundaries, and diffusion of ideas across and between regions. Another panel tackles analytical and measurement problems associated with the study of

regime type and the implications for understanding democratic backsliding, *Frontiers in the Study of Political Regimes*. A panel on *Evolving Concepts in Comparative Politics* reconsiders how the field should think about coups, crises, and democracy.

There are also panels on global extremism, populism in comparative perspective, citizen support for democracy, identity politics, digital authoritarianism, propaganda and persuasion, emerging markets, corruption and scandal, climate mitigation, religion and violence, and the short- and long-term effects of COVID-19 around the world. The Comparative Politics division also sponsors a number of panels with a regional focus, including panels organized around governance in the Middle East and North Africa, European political history, corruption in Latin America, patronage and political machines in Southeast Asia, and the political economy of East Asia. Altogether, the Comparative Politics division is supporting over 50 panels at the 2022 conference, including several virtual panels for those unable to attend in-person. In other words, there is something for everyone.

Most importantly, the **Comparative Politics Business Meeting** is scheduled for Friday, September 16, 2022 from 6:30-7:30 p.m. (location: TBA). Safe travels to Montreal.



LETTER FROM PRESIDENT OF COMPARATIVE POLITICS SECTION

By Prerna Singh



Prerna Singh

is Mahatma Gandhi Associate Professor of Political Science and International Studies at Brown University. Her email address is prerna_singh@brown.edu

Mass shootings; wars, genocides, refugee crises; the shrinking of civil rights and liberties; democratic backsliding; the rise of exclusionary nationalisms; discrimination and violence against ethnic minorities; steep and rising inequalities; worsening socio-economic indicators including for women, children, and families; heat waves, droughts, storms, fires, floods and other disasters associated with our changing climate; the ebbs and flows of a global pandemic that has so far claimed at least 6 million lives. At a personal level it can feel difficult to comprehend the many sites and scale of human suffering today. And yet despite or perhaps precisely because of this, it is as scholars what we seek to do. In attempting to “make sense” and imagine ways beyond the seemingly “senseless” (and endless) darkness of our times, I sometimes see scholarship as also our way of ‘coping’ with it.

But might we perhaps now, cautiously, look forward to more? Even with the certainty of an uncertain public health future, the pandemic seems to be settling, at least for now, for the vaccinated, into somewhat less alarming grooves. Semesters and quarters have wound down and summers begun. These much-awaited months of reprieve for many from at least some professional obligations will unfold into the annual meeting in Montreal in early Fall. In as much as this promises to be the largest in-person annual meeting since the beginning of the pandemic, it might be time to cautiously shift thinking away from coping to celebrating, at least in the sense of coming together to mark an occasion.

Comparative Politics will have an important presence at the meeting. Established in 1988, we are, at over 1200 members, presently the largest of all APSA’s organized sections. Our division co-chairs Karie J Koessel and David Steinberg read through hundreds of excellent submissions to

select and put together over 50 panels that tackle the central questions of our time around democratic backsliding, extremism, populism, nationalism, war, race, ethnic politics, climate change, public health and contentious politics. These panels are richly representative of different parts of the world, while a theme roundtable critically interrogates the very idea of ‘region’ itself. They also exemplify the various methods deployed within our subfield. A mini-conference will devote itself entirely to exploring historical approaches to Comparative Politics. While most of our section’s panels are in-person, we also have a significant virtual component. These virtual panels are specially important in light of the the continued challenges posed by the pandemic, and discriminatory visa regimes, that make it difficult for many of our colleagues to travel to Canada.

In addition to the panels, our section looks forward to hosting its first in-person business meeting since 2019. One of the highlights of this meeting will be the opportunity to recognize exceptional scholarship within our discipline. It is my great pleasure to both announce here the prizes that we will be conferring at our section meeting as well as gratefully recognize the contribution of our many colleagues who gave of their served on these prize committees. Our newest prize, the Theda Skocpol Emerging Scholars award is now entering its third year. Further, as you will notice, this year we have experimented with a new two-tier system for the selection of the Luebbert best book prize, which involves the creation of an initial shortlist from which the winner(s) are selected.

I would also like to take this opportunity to thank Seth Jolly who is completing his term as our trusty treasurer and welcome to the role, Gustavo Flores-Macías. In addition on the executive committee, we bid a grateful farewell to Mariela S. Darby and

Erik Kuhonta. They will be replaced by Yuko Kasuya and Daniel Corstange who join Philip Roessler and Wenfang Tang, both serving the second year of their two year term. I am also very pleased to welcome our vice-president and Chair elect, Ellen Lust. Finally, it is wonderful to have this newsletter be in the able hands of Ben Smith and his editorial team at the University of Florida.

I hope you will all feel free to reach out to us with any feedback or suggestions. I look forward to being able to connect with many of you in Montreal and specially to being able to raise a toast with you at our

section reception. Please save the date!

**Gregory Luebbert Prize for Best Book in
Comparative Politics**

Yanilda Maria Gonzales (Harvard), *Authoritarian Police
in Democracy*

Honorable Mention:
Elizabeth Nugent, *After Repression*

**Lijphart/Przeworski/Verba Prize for Best
Dataset**

Amanda Clayton (Vanderbilt), Melanie M. Hughes (Pittsburgh), Pamela Paxton (UT-Austin), and Par Zetterberg (Uppsala), for the "Quota Adoption and Reform Over Time (QAROT)" dataset

Richard Gunther and Paul A. Beck (Ohio State University), for the "Comparative National Elections Project (CNEP)" dataset

Sage Paper Prize

Mathias Poertner (LSE), "Building the Party on the Ground: The Role of Access to Public Office for Party Growth."

Honorable Mention:
Michael Albertus (University of Chicago) and Noah Schouela (University of Chicago), "When Redistribution Backfires: Theory and Evidence from Land Reform in Portugal."

Theda Skocpol Prize for Emerging Scholars

Alisha Holland (Harvard)

Bingham Powell Prize for Best Mentoring

Ellen Lust (University of Gothenburg)

Gregory Luebbert Prize for Best Article

Donghyun Danny Choi (Brown), Mathias Poertner (LSE), Nicholas Sambanis (Penn)



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The Organized Section in Comparative Politics is the largest organized section in the American Political Science Association (APSA). The Section organizes panels for the APSA's annual meetings; awards annual prizes for best paper, best article, best book, and best data set; and oversees and helps finance the publication of this newsletter, APSA-CP.

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American Political Science Association
1527 New Hampshire Ave, NW Washington, DC 20036-1206



(202) 483-2512



(202) 483-2657



apsa@apsanet.org