APSA | COMPARATIVE POLITICS

THE ORGANIZED SECTION IN COMPARATIVE POLITICS OF THE AMERICAN POLITICAL SCIENCE ASSOCIATION

NEWSLETTER

VOLUME XXXVIII ISSUE 1 SPRING 2026

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CHAIR'S WELCOME

by Anna Grzymala-Busse

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I'm delighted to welcome you to the first CP newsletter edited by our exciting new editorial team, led by Leo Peisakhin and including Andy Harris, Gabriel Koehler-Derrick, Giuliana Pardelli, and Melina Platas. Our huge thanks go to the outgoing team, expertly led by Ben Smith at the University of Florida, and including Hannah Alarian, Sebastian Elischer, Andrew Janusz, Nicholas Kerr, Juliana Restrepo Sanin, and Treethep Srisanga.

This issue explores how comparative political research deals with the past: as historical legacies, as mechanisms, as a new(ish) subfield of historical political economy, and as a comparative enterprise. It also explores the past and present of individual scholars. And because the past is often a distant country but the present is always with us, we have several pieces dealing with teaching and researching politics in turbulent times. Finally, the issue contains a memorial to Kimuli Kasara, one of our best and brightest.

Thank you for reading, and we very much hope to see you at CP workshops, APSA panels, and of course our fabulous reception in Boston on Friday, September 11.

EDITORS' LETTER

by Leonid Peisakhin, J. Andrew Harris, Gabriel Koehler-Derrick, Giuliana Pardelli and Melina Platas

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We are honored to be entrusted with the CP Section Newsletter over the next few years and are grateful to Ellen Lust, the past section chair, and Anna Grzymala-Busse, the incoming chair, for inviting us to contribute to the conversation. We benefit greatly from high standards set by our predecessors—most recently, colleagues at the University of Florida and Johns Hopkins University. Special thanks to Kate Baldwin and Ellen Lust for helping with the transition.

We are piloting a new format for the newsletter. In addition to the usual symposium on a research topic in comparative politics, we are introducing interviews with prominent scholars in the discipline where we invite interviewees to reflect on the substantive and methodological evolution of our field, share lessons from their careers, and reflect on the relevance of CP research to current affairs. We are also adding an "In the News" section, where we ask scholars to consider how ongoing events outside of the academy impact how we study and teach politics. As in previous editions, we will continue to introduce new datasets of broad interest and to share section-relevant announcements.

We are also changing the dissemination of the newsletter. Bi-annual newsletters will now be shared via email to increase visibility and readership. An archive of all past issues of the newsletter is available at the CP Newsletter's online home.

In the current issue, contributors discuss how we study the historical past and how contemporary current affairs impact how we teach comparative politics:

- In the symposium, we take stock of the burgeoning field of historical political economy and reflect on its future direction.
- The "Interviews with CP Scholars" section features conversations with Marc Beissinger, David Laitin, and Pearl Robinson about their careers and the state of the subfield of comparative politics.
- An interview with Steve Levitsky on our responsibility as teachers and scholars to engage with current political events in the classroom and beyond dovetails with essays by political scientists in South Korea, Indonesia, and Brazil on the challenges of teaching politics in 'turbulent' times.
- The newsletter concludes with reflections on the career of the late Kimuli Kasara, a stellar scholar and generous mentor.

This newsletter serves the CP community. Therefore, we very much welcome your comments about its substance and format. Most importantly, we hope that you will send us suggestions on topics for future research symposia, ideas for scholars to interview, and thoughts on what issues deserve coverage in the section on the intersection of current affairs and the studying & teaching of politics. Please reach out to us at apsacpsectionnewsletter@gmail.com or write directly to the editors.

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INTRODUCTION

by Giuliana Pardelli and Leonid Peisakhin

Historical Political Economy (HPE) has become an exciting meeting point of research agendas and methodological approaches. It invites us to treat "politics" and "economics" as related domains and "history" as the substance of explanation. Yet the very success of HPE brings an organizing problem. As HPE expands methodologically, substantively, and across disciplinary boundaries, what is the field's comparative advantage? Where are its conceptual and methodological weaknesses? And what standards should we use when we claim that "history matters"?

The four contributions in this section address (1) what HPE should prioritize as a research agenda, (2) how HPE relates to older comparative-historical traditions, and (3) how we should interpret and discipline claims of persistence in long-run research designs. Taken together, they push HPE toward a stance that is simultaneously more ambitious and more self-critical: more ambitious in expanding the field's objects of explanation and units of analysis, and more self-critical in clarifying concepts, specifying scope conditions, and tightening inferential claims about historical transmission.

Cortesi and Mazzuca's note begins from the premise that HPE is now vibrant enough to require something like agenda-setting rather than boundary-policing. They propose a 2 x 2 map of political economy research that separates (a) national from subnational scale and (b) institutions from capacities/resources. Their diagnosis is that HPE's comparative advantage lies in populating the underdeveloped quadrant: subnational capacities—historically produced, territorially uneven fiscal, administrative, and human resources that condition development and governance within countries.

This proposal converts a methodological preference in HPE (within-country designs) into a substantive agenda. The authors argue that subnational units are where political economy happens, as decentralization shifts responsibilities for public goods to provinces and municipalities. But the move also raises a demanding conceptual problem, that "capacity" is not a single thing. Fiscal extraction, bureaucratic competence, coercive reach, and informational infrastructure may travel together in some settings and decouple in others. If HPE reorients toward subnational capacities, it will need theories that disaggregate capacity into components, specify which components matter for which mechanisms, and treat territorial unevenness as a political outcome, not merely an explanatory variable.

Soifer's essay cautions that an exclusive focus on subnational units might be detrimental to our understanding of historical persistence, given that studies in the older comparative-historical analysis tradition (CHA), often operating at the level of cross-national comparisons, tended to ask similar questions to ones asked in the sub-national HPE research and provided useful answers. He stresses that multiple institutional logics might operate at different spatial levels simultaneously and that, as researchers, we must be able to explain how subnational dynamics relate to outcomes at the level of the nation. Soifer's point is that the field lacks a sufficiently explicit theoretical toolkit for moving between scales. When do national categories obscure the phenomena we should explain, and when does subnational disaggregation risk generating findings that are hard to interpret at the level of major comparative questions? He is especially sharp in rejecting the easy rhetoric that "smaller is always better." Spatial disaggregation can strengthen identification, but it can also introduce its own inferential problems and conceptual mismatches if the theory has not specified the spatial scope of the mechanisms it posits.

Wittenberg's contribution tightens the field's most common empirical genre—long-run "persistence studies"—by foregrounding a relationship that is often assumed away, the link between an antecedent historical regressor (X_H) and its contemporary counterpart (X_C). He argues that the magnitude of this relationship (δ) is a scaling factor for interpreting long-run effects. When δ is low, it becomes harder to claim that a historical instrument operates through the intended contemporary regressor, and easier to suspect alternative channels that mimic "persistence" while undermining the exclusion restriction.

Wittenberg challenges the conceptual "sameness" assumption behind treating X as one variable measured at two distant points in time. The longer the temporal distance, the less credible it is to presume continuity in meaning, as institutions, practices, and norms can mutate, split, or recombine. The implication is: estimate or bound the degree of persistence where possible, document treatment evolution, and treat "history matters" as an interpretation that must be earned, not a label that automatically follows from a significant coefficient.

Charnysh's contribution complements this by asking what comes after identification, even when persistence is credibly established. She argues that legacy research has devoted enormous effort to original data and research designs, while mechanism evidence is often thin and indirect. Her remedy is a "library of mechanisms" that scholars can deploy and recombine across cases, making it easier to accumulate knowledge and to see when apparently distinct legacies run through similar processes. Because no single HPE paper can typically deliver both clean identification and convincing mechanism evidence, Charnysh proposes that HPE should more systematically borrow from adjacent disciplines that study these mechanisms directly and then use historical variation to establish when and where those mechanisms were likely active.

Read together, the four essays point toward a rebalancing of HPE's priorities. Cortesi and Mazzuca push the field to ask new questions—especially about subnational capacities—where HPE's historical sensibility and within-country leverage can generate distinctive insights. Soifer argues that the payoffs from this agenda will be limited unless scholars theorize scale, periodization, and aggregation rather than treating them as design conveniences. Wittenberg demands that persistence claims be interpreted through the empirical and conceptual relationship between historical regressors and their contemporary counterparts, not through the rhetorical appeal of "long-run effects." And Charnysh offers a way to make explanations portable through deployment of a shared, testable vocabulary of reproduction mechanisms, coupled with greater openness to evidence produced outside HPE's standard toolkit.

The broader implication for the HPE community is that the next advances will likely come less from ever more ingenious identification strategies, and more from integrating three kinds of discipline: conceptual discipline (what counts as "the same" treatment over time), scalar discipline (which level the mechanism inhabits, and how effects aggregate), and mechanistic discipline (whether persistence is durable-cause state dependence or reproduced through identifiable pathways). If the field internalizes those standards, it can preserve what has made HPE successful—its seriousness about temporality and causal inference—while building the explanatory and cumulative architecture required for a mature research program.

Concretely, these essays point toward a set of "best practices" that could make HPE findings more comparable across studies: state the temporal scope and the relevant periodization up front; defend (with historical evidence) whether the treatment is stable, evolving, or bundled; make the spatial unit of the mechanism explicit and discuss plausible aggregation paths; report the empirical link between historical and contemporary measures when the argument hinges on persistence; and locate mechanism claims in a shared typology (or explain why a new mechanism category is warranted). Treating these steps as normal, not exceptional, would help the field keep its identification gains while improving the cumulative interpretability that HPE now needs.

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HPE Symposium

CLARIFYING THE AGENDA OF HISTORICAL POLITICAL ECONOMY: TOWARD A FOCUS ON SUBNATIONAL CAPACITIES

by Sebastián Cortesi and Sebastián Mazzuca

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THE field of Historical Political Economy (HPE) has emerged as a vibrant interdisciplinary endeavor, bridging political science, economics, and history to examine the long-term interactions between political and economic forces. HPE's purpose is to improve on traditional Political Economy (PE) by being more attentive to temporality and path dependence, and more careful about causality and mechanisms. This approach has gained traction in recent years, as evidenced by major publications such as the *Oxford Handbook of Historical Political Economy* (2023), which compiles contributions exploring how historical contexts shape contemporary political-economic outcomes. Yet, as an emerging field, HPE lacks a clearly articulated research agenda. This note proposes a conceptual framework to clarify HPE's priorities, arguing that the field offers a unique opportunity to shift attention from the traditional PE focus on national institutions to the underexplored world of subnational capacities. By classifying existing research along two dimensions—scale (national vs. subnational) and focus (institutions vs. capacities)—we can identify gaps and chart a path forward.

...as an emerging field, HPE lacks a clearly articulated research agenda. This note proposes a conceptual framework to clarify HPE's priorities

To begin, it is essential to differentiate the key concepts. Institutions refer primarily to rules, both formal (e.g., constitutions, laws) and informal (e.g., norms, customs), that structure behavior through incentives and constraints. In contrast, capacities encompass resources—economic (e.g., fiscal revenues), political (e.g., administrative control), and human (e.g., skilled bureaucracy)—that enable actors to implement policies and achieve goals. While institutions provide the framework, capacities supply the means. Traditional PE has primarily focused on national institutions, as exemplified by seminal works like Douglass North's *Institutions, Institutional Change and Economic Performance* (1990) and Hall's and Soskice's *Varieties of Capitalism* (2001), which attribute long-run economic divergence to differences in institutional quality. These studies highlight how

Table 1: Dimensions of Political Economy Research

Dimension	National Scale	Subnational Scale
Institutions	Traditional PE core (e.g., national rules on property rights)	Local governance, federalism (e.g., municipal regulations)
Capacities	State capacity literature (e.g., national fiscal extraction)	Emerging HPE opportunity (e.g., regional administrative resources)

national rules governing property rights, governance, and markets drive prosperity or stagnation.

This institutional focus at the national scale corresponds to one quadrant in the proposed 2x2 classification shown in Table 1. The framework maps research along an X-axis distinguishing national from sub-national scale, and a Y-axis separating institutions from capacities. Traditional PE dominates the "national institutions" cell, with extensive literature on how national rules shape economic trajectories. Analyses of national electoral systems or central bank independence illustrate this emphasis.

the "subnational capacities" quadrant remains sparsely populated, representing both a critical gap and a prime opportunity for HPE

The "national capacities" quadrant has also received attention, particularly in state capacity research. Scholars like Peter Evans in *Embedded Autonomy* (1995) and Timothy Besley and Torsten Persson in *Pillars of Prosperity* (2011) explore how development offices with a sense of national mission or elite bargains under geopolitical pressure underpin the development of fiscal and coercive capacities. Meanwhile, the "subnational institutions" cell includes work on local institutional arrangements, such as Richard Snyder's *Politics after Neoliberalism* and Amartya Sen and Jean Drèze's *India: Development and Participation* (2002), both of which show how variation in subnational institutions shapes economic and social outcomes across Mexico and India, respectively.

In contrast, the "subnational capacities" quadrant remains sparsely populated, representing both a critical gap and a prime opportunity for HPE. Due to renewed historical sensibility and methodological rigor, HPE is ideally suited to fill the void by examining how variation in subnational resources impacts broader political economy outcomes. This shift is not merely additive; it addresses fundamental limitations in PE's traditional national-institutional paradigm.

HPE should prioritize subnational capacities for substantive and methodological reasons. First, subnational units increasingly bear significant responsibilities, especially amid national state failures. In many developing countries, decentralization has devolved authority over public goods like education, health, and infrastructure to provinces and municipalities. As national governments falter due to fiscal crises, corruption, or polarization, subnational entities step in, making their capacities pivotal for citizen welfare. For example, in Mexico, subnational variations in administrative resources have influenced responses to crises like COVID-19, highlighting how local capacities compensate for national shortcomings.

Within-country variation enables HPE scholars to make more credible causal inferences. Whether consisting of cross-sectional or temporal variation, single-country designs provide units that are more likely to satisfy the identifying assumptions of modern observational research designs than cross-national comparisons, which are often plagued by extensive unobserved heterogeneity. In difference-in-differences designs, federations like the US and Mexico offer subnational units that are more likely to satisfy the parallel trends assumption prior to a shock or policy intervention. These subnational units typically share a common national institutional framework, exposure to the same macroeconomic conditions, and similar broad cultural or political environments, making it more plausible that, absent the treatment, their outcome trends would have evolved in parallel. A prominent illustration is the staggered rollout of Medicaid expansions across US states, where pre-treatment trends in health outcomes are far more likely to align than when comparing across countries with fundamentally different health systems, welfare regimes, or economic structures (Courtemanche et al., 2017).

Separately, in geographic or boundary regression discontinuity designs, localities within countries are more likely to satisfy the continuity assumption of potential outcomes. Jurisdictional borders—such as those between states or provinces—serve as the discontinuity point, with treatment (e.g., differing policies, regulations, or fiscal rules) changing sharply across the line and distance to the border as the running variable. Units immediately adjacent on either side are geographically proximate and share similar local confounders (e.g., climate, economic activity, demographics), reducing the risk of unobserved jumps at the border compared to a uniform national threshold, which would compare distant, heterogeneous regions. For instance, adjacent counties or municipalities across US or Mexican state borders experience different minimum wages, health policies, or environmental rules, yet share highly similar local conditions, making continuity more plausible due to subnational proximity.[1] Overall, operating within a shared national context substantially reduces the scope for unobserved differences that commonly threaten inference in cross-national studies, thereby strengthening the credibility of causal claims.

HPE should explore two key causal links at the

1 Dell (2010) identifies the long-run causal effect of the colonial *mita* on contemporary living standards using a regression discontinuity design at the internal *mita* boundary within Peru. Identification hinges on the continuity of potential outcomes at the cutoff: absent the *mita*, expected household consumption should vary smoothly across adjacent districts. This assumption is credible because these districts are geographically close and have shared the same national political and policy environment since independence. Including Bolivian districts would undermine the strategy. Despite geographic proximity to the boundary, Bolivian districts experienced different national policies, state-building paths, and postcolonial labor and welfare regimes. This cross-national institutional heterogeneity would violate continuity by introducing systematic differences in potential outcomes unrelated to mita exposure.

subnational level. First, the causes of subnational capacities warrant deeper investigation. Historical factors such as colonial legacies, international commodity booms, and wartime mobilization are often seen as the source of why some countries accumulate robust administrative resources while others lag. In Europe, subnational capacities in places like Prussia emerged from 19th-century reforms, while they never fully consolidated in southern Italy. Leveraging within-country spatial or temporal variation, HPE scholars can uncover how initial differences lead to durable capacity legacies.

Second, the effects of subnational capacities on local economic development demand attention. Capable local governments facilitate investment, innovation, and service delivery, fostering growth clusters amid national stagnation. Evidence from China shows that provincial capacities drive regional GDP disparities, whereas in India, state-level human capital investments correlate with economic outcomes. HPE is uniquely positioned to examine how subnational capacities shape long-term development paths and create territorial inequalities within countries.

In conclusion, HPE can produce a distinct contribution in relation to traditional PE by pivoting to sub-national capacities. This shift not only leverages HPE's toolkit but also addresses pressing real-world challenges, such as decentralization and inequality. By populating the understudied lower right quadrant, scholars can generate novel insights into state-building and economic trajectories. Future research might integrate quantitative subnational datasets with qualitative historical narratives, fostering collaborations across disciplines. Ultimately, this reorientation promises to make HPE a more comprehensive field, better equipped to explain the persistent puzzles of political-economic change.

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COMPARATIVE HISTORICAL WORK WITHIN AND BEYOND HPE

by Hillel David Soifer

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I am gratified to be asked to contribute to a symposium on historical political economy.[1] As someone who sees himself as a consumer of work in this tradition, along with other research on historical political development, I find myself reflecting on the question of how to integrate the findings of HPE work with those from scholarship that does not fit under the HPE umbrella but which I find equally valuable and informative. In particular, I wonder how we can learn from aggregating HPE work with scholarship in the older, often more qualitative and more macro-oriented tradition of comparative historical analysis (CHA). There has been some effort (Jenkins 2024) to explore this question in the study of American politics in terms of the relationship between APD and HPE scholarship, but to my knowledge those of us working in comparative politics lack a sense of whether and how we might benefit from a conversation across these two research traditions.

I wonder how we can learn from aggregating HPE work with scholarship in the older, often more qualitative and more macro-oriented tradition of comparative historical analysis

Given the limited space to explore these issues, I do so by focusing on two books (Guardado 2025; Mahoney 2010)—one in each tradition—that approach the same broad research question of the colonial roots of development in Latin America, and ask whether and how we can profitably learn about this phenomenon by reading these two books together. What follows is, therefore, not a review or a critical reflection on either book, or on either HPE or CHA as a research tradition, but an attempt to put the books (and implicitly, the traditions) in conversation with one another, and to reflect on the bridges that can be built between CHA and HPE work, and on the obstacles to doing so. I focus not on similarities and differences between these research currents, or on what I might see as their strengths and weaknesses, but instead on how historically minded

1 Thanks to Jenny Guardado, James Mahoney, and Ryan Saylor for comment, and to Sean Gailmard and Melissa Lee for productive conversations about HPE and social science more generally. None of what follows should be read as reflecting the views of these generous colleagues.

scholars might profitably read (and perhaps even work) across them.

Mahoney's *Colonialism and Post-colonial Development*

Like many scholars in the comparative historical analysis tradition, Mahoney begins with an outcome of interest—the contemporary variation across Latin American countries in social and economic development—and turns to a historical cause in the form of colonial rule. Building on what was already a dynamic literature on the effects of colonialism, Mahoney's book makes three key innovations—first, it argues for spatial variation in the intensity of rule across the vast territory of empires and distinguishes between core and periphery, and second that colonialism varies in the institutions it seeks to impose—in the historical context of Spanish rule, this variation distinguishes between mercantilist and liberal institutions. Third, Mahoney argues that mercantilist and liberal colonizers made different choices about where to center their presence, and (crucially, in contrast to an existing literature that focuses on geographic determinants of colonial rule) that these choices were made principally though not entirely with the institutional complexity of indigenous society that colonizers encountered.

The book is focused on describing how these three factors led to different patterns of colonial rule across what would become the fifteen countries of mainland Spanish America. Two periods of colonial rule, each implanted to different extent across space, placed different parts of Spanish America on different trajectories at independence.[2] In so doing, it also emphasizes two main channels by which this variation shaped long-term social and economic development outcomes, highlighting relative development across Spanish America rather than comparing the region's trajectories to those found elsewhere in the post-colonial world. First, Mahoney argues, mercantilist and liberal colonialism empowered different elite economic actors, who then pursued their own interests as they influenced subsequent policies and institutions. Mercantilist elites rejected policies and institutions that could have integrated their territories into the global economy in the 19th century, whereas elites empowered by liberal colonial rule were more likely to do so, and this produced long-term economic development trajectories. Second, mercantilist rule, especially in sites where it centered its exploitation of indigenous society, created an ethnic underclass which remained isolated and exploited, undermining social development in the long term.

Guardado's *Venal Origins of Development*

Between 1670 and 1750, the Spanish crown faced revenue shortages as it faced multiple wars. To raise funds, it filled some 2,600 colonial offices via sale rather than appointment.[3] *Venal Origins* explores the effect of this policy on long-term development in Latin America by leveraging variation in which offices were sold, when, and under what conditions in the metropole. Like many scholars in the HPE tradition, in other words, Guardado's book begins by identifying a

2 To limit myself to the discussion of colonialism and its legacies, I set aside here the six cases that were on the periphery of the Spanish American empire throughout, and thus had what Mahoney calls contingent development outcomes.

3 To be precise, since some of the sales were for future rights to the office – a spot in the queue of appointees – some purchasers took up their positions long after sales ended in 1751.

source of variation that has a plausibly exogenous component, and seeks to explore its effect. Guardado's argument is that officials who purchased their appointments were less likely to possess traits associated with good governance, that they had greater incentives to profit from their position, and that they were difficult to dismiss because the Crown would have to refund their purchase price. Thus, office selling was associated with venality before independence, as these officials imposed greater exactions on the indigenous population through practices such as the *repartimiento* and labor drafts– Chapter 3 shows that "office selling was a key way to seize economic surplus from the colonies – particularly from [the] indigenous population." (Guardado 2025, 57) Guardado argues that these practices continued after independence, and thus territories where office sales were more common, and where offices were sold for higher prices saw greater propensity for rebellion, more indigenous segregation, and less representative governance, which have redounded to the present.

Crucial for the inferential strategy that follows are three kinds of variation across the Spanish Americas in office selling. First, some offices were never sold—these are places that the Crown saw as too strategic. Second, among the places where offices were sold, some were administered by more purchasers than others. Finally, during the period of office sales, the fiscal needs of the Crown varied (largely due to the wars it faced) and thus the prices for which the same position was sold varied over time. The first would allow exploration of the extensive margins of office sales, though (as I discuss further below) this comparison does not feature in the book because of threats to inference. The last two, which are the focus of the empirical analysis, focus on the intensive margin—the effects of a marginal additional office sale in a setting where other offices are also sold, and the effects of an increase in the price for the sale of the same position.

Guardado shows statistical evidence that greater office sales are associated with greater rebellions, more out-migration by the indigenous population during the Bourbon period, after office sales were brought to an end, and during the 19th century. More venality is also associated with less political inclusion of the indigenous population, greater subnational authoritarianism, and poorer social development outcomes in the present. Thus, Guardado argues that when we look subnationally, the legacy of colonial office sales has had a major impact on variation across Latin America. The book places a lot of emphasis on demonstrating persistence, and thus effectively responds to some critiques of HPE work that fault it for over-claiming about legacies and continuities while only showing correlational evidence.[4]

Commonalities

These summaries reveal several major commonalities across these two books, which suggest that they ought to be in deep and sustained conversation with one another. Both are focused on the same outcome (the variation in economic and social development across contemporary Latin America) and on the same cause—a particular kind of variation in Spanish colonial admin-

4 Indeed it goes quite a bit further in this vein than does Mahoney, who emphasizes colonial origins and places less emphasis on the empirical evidence for the mechanisms underpinning persistence.

istration. The arguments in both books take the form of what Arthur Stinchcombe (1968) called historical causation, in which an outcome persists after the cause is no longer present, and indeed propose similar mechanisms for this persistence, related to the nature of the elites empowered by colonial rule, and the negative impact of intense colonial extraction on indigenous peoples, to account for the persistence of development outcomes.

We have, then, two studies of how variation in colonial rule shaped long-term development outcomes in Spanish America.[5] One could raise issues with the inferential strategy, argument, and evidence of each one and use these to counterpose the CHA and HPE traditions of scholarship; to think through similarities and differences between the books, or stake claims about strengths and weaknesses of the broader approaches. In my view, however, the more fruitful endeavor for scholars interested in historically oriented research questions is to think about how these two studies can be brought into conversation with one another. I therefore turn to that task in what follows below.

A unified reading of colonial legacies

Guardado engages with Mahoney's argument in a particularly sustained and careful way in her concluding chapter. Here, she suggests that we ought to view the core and periphery of the Spanish Empire in terms of office sales—that places where offices were not sold because of strategic considerations ought to be seen as the geopolitical core of imperial rule, while variation in how much offices sold for, which reveals how much extraction was possible, highlights the difference between what she calls an economic core and an economic periphery. There are a few striking things about this re-orientation that provide a jumping off-point for exploring how we might cumulate knowledge across these two books.

On the one hand, the introduction of a distinction between geopolitical and economic motivations for imperial administration marks a sharp difference from Mahoney's core-periphery distinction based on a single dimension of variation. This highlights what Guardado describes as a key contribution of her book – a more complex account of the Crown's motivations in administering its New World empire.

Yet coding subnational provinces where offices were more valuable as the economic core overlaps quite a bit (though not perfectly) with Mahoney's description of the priorities of mercantilist colonialism as emphasizing places with "large indigenous populations or minerals" (Guardado 2025, 214) that in his view led mercantilist officials to prioritize those regions.[6] In other words, it appears that Guardado concedes that Spanish colonial rule followed the same logic argued by Mahoney for mercantilist rule, but with a look at more fine-grained variation. If we take a strong version of this claim, we might explore the question of whether Guardado's argument about the effects of colonial rule can be subsumed within Mahoney's. Recall that Mahoney argued that a crucial channel through

5 I set aside here Mahoney's extension of his argument to the Portuguese and British empires.

6 The correspondence here is not perfect—there are highly priced provinces that lack both minerals and large indigenous populations and vice-versa—but the overall pattern is that the two authors coincide in what they describe as the core of mercantilist colonial rule, though their coding is at different spatial scales.

which colonialism affected development outcomes was the creation of elite economic actors who then used their power to shape institutions to serve their interests. Since opportunities for venality were greatest precisely in the parts of the Spanish Empire where Hapsburg rule was most intense and did the most to create and empower mercantilist elites, we might see Guardado's argument as evidence for this causal channel in Mahoney's argument.

Third, because Guardado focuses on variation in outcomes among the places where offices were sold, she sets aside what she describes here as the geopolitical core and thus limits what we can learn about the varied legacies of this vision of empire. One way to think of this is that Guardado focuses on the intensive, rather than extensive, margin of the effects of office sales.[7] She does so principally for inferential reasons—the exogenous timing of when offices were up for appointment *vis-à-vis* Spanish involvement in wars allow her to credibly identify the effects of venality. But if we are interested in the effects of imperial rule as a whole, we lose quite a bit by excluding the imperial core, which were places where offices were not sold. The upshot here is not just a concern that (as Chapter 3 shows) that these places differ from those where offices were listed for sale in systematic ways. Instead, the challenge for a reader who wishes to evaluate the overall explanatory power of Guardado's account of variation in development in Spanish America is that it only speaks to a subset of the region: those places where offices were sold.[8]

Intercurrence and the nature of colonial rule

Guardado brackets the effects of the broader institutional setting to focus on the effect of the sales of discrete colonial offices. She characterizes that particular policy as being broadly uniform (though varying in its details in terms of who made decisions about sales) precisely across the period where Mahoney identifies a sharp shift from Hapsburg mercantilist rule to Bourbon liberal colonial rule. This has implications for how we think about bringing these two accounts into conversation with one another.

First, if the broader orientation of colonial administration changed over the course of this period as Mahoney argues, we might expect there to be differences in how administrators were empowered, the priorities they were given, and the intensity of imperial presence. And these differences might mean that office sales under mercantilist colonialism might lead to different governance than under liberal colonialism. This is a question about the homogeneity of the treatment effects at the heart of Guardado's argument. Second, and conversely, if Guardado is right that a core element of colonial administration in fact saw continuity rather than change across the disjuncture at the core of Mahoney's argument, this suggests an element of commonality between mercantilist and liberal colonial

7 In Chapters 3 and 5, Guardado incorporates evidence about places that were not sold to uncover the complex motivations of the Spanish Crown, but her conclusions about variation in developmentacross subnational regions in Spanish America, which are at the core of the book's analysis, rely only on the effects of variation in sales price.

8 A related point is that Guardado's analyses of the effects of office sales consistently control for on-the-ground conditions such as indigenous population density or size and features of geography, which she treats as alternative explanations for long-term outcomes. But if we're interested in the broader question of the differential impact of colonial rule, we might in fact be interested precisely in which offices were more lucrative and why.

administration which Mahoney contrasts as radically different in their administrative priorities.

Whereas Mahoney and Guardado each isolate a particular dimension of variation in colonial rule to examine its effects, counterposing the two books suggests that the historical record of colonialism may not map so neatly onto the descriptive categories identified by the two authors. Mahoney's periodization between Hapsburg and Bourbon colonialism suggests that office sales might have different implications within the period that Guardado treats as unified, while Guardado's analysis suggests more continuity than change in a key element of administration than Mahoney's dichotomy implies, which calls into question the validity of how he applies his theory to his cases.[9] The fact that these two books operate at different temporal scales, then, raises some questions about how to describe the variation we observe in administration during the Spanish Empire before we even turn to examining its effects. It reveals an element of what scholars of American political development call 'intercurrence' in the institutions of colonial rule—the "simultaneous operation" of distinct sets of institutions, created at different times and evolving in distinct ways rather than as a coordinated and unified institutional structure. (Orren and Skowronek 2004, 17)

Spatial scale and historical causation

One key difference between the books is the spatial unit of analysis. Whereas Mahoney seeks to explain variation across countries in economic and social development, Guardado's focus is on subnational variation. Thus, Mahoney begins from the strikingly large, and strikingly persistent, variation in social and economic development across countries in contemporary Spanish America, and looks backwards in history to seek its causes.[10] By contrast, Guardado, consistent with HPE's orientation to causal inference, begins with variation in the venality of colonial administrators at the subnational level, and looks forward to find the effects of that variation in communities that fall within the borders of the places those authorities oversaw.[11]

As consumers of scholarship on the colonial determinants of social and economic development, we are left with a puzzle: how are we to bring together findings about variation in economic and social development at two spatial scales so that we can put them into conversation with one another? If we could do so, we would learn whether and how venality interacts with the elements of colonial rule that are at the focus of Mahoney's argument to produce different effects across places ruled more and less intensively, or across mercantilist and liberal colonial models. It would also let us see whether and how the effects of these models and weights of colonial rule are mediated by venality. But having this conversation would require pushing both Mahoney and Guardado beyond their current theoretical frameworks to think about how to put the aspects of colonialism they study in the context of the broader institutional environment of colonialism, and to specify explicitly how their own hypotheses interact with those advanced by the other.

9 A reading of Mahoney's empirical account of Bourbon Spanish imperialism shows little discussion of the decades between 1700 and 1750. This suggests that despite his periodization of the Bourbon period as beginning in 1700, perhaps implicitly he agrees with Guardado that the major shift the Bourbons brought to colonial administration should be dated to the mid-18th century rather than to the change in dynasty.

10 In several places, such as his treatment of Guayaquil's rise in the late colonial period as a contrast to the decline of highland Ecuador, Mahoney diverges from the cross-national emphasis in his book to account for subnational variation.

11 See Maloney & Valencia (2016) for one example of evidence about subnational persistence in development outcomes consistent with Guardado's approach.

Theoretical engagement with more macro-oriented work is, in my view, too often absent in HPE work that focuses on smaller units of analysis because those provide opportunities for better causal identification. Yet it is too easy and glib to justify the micro over the macro on the grounds that coding economic and social development outcomes at the national level misses important subnational variation that is to be explained. Though it takes us beyond my goals in this essay, privileging 'smaller' as always 'better' in aggregate analyses rests on a flawed extension of concerns about ecological inference to aggregate outcomes, and going 'too small' risks inferential problems for a variety of reasons. (Lee, Rogers, and Soifer 2025; Soifer 2019) On the other hand, combining Guardado's empirical findings about the importance of venality at the subnational level with the many references in Mahoney's empirical account of indigenous exploitation to precisely the same causal process suggests that there may be a lurking systematic source of variation in subnational development obscured by Mahoney's focus on the substantively interesting national-level variation.

Learning about historical outcomes will require us not only to integrate historical processes at different temporal scales ... but to take into account the fact that historical processes, both of initial variation and of persistence, may operate at multiple spatial scales

Nor can we simply conclude that the two sets of findings are consistent with one another but asking distinct questions. This seems to be what Guardado implies in Chapters 1 and 8, where she argues that different colonial institutions produce legacies that pattern outcomes at the national and subnational levels. Yet this seems to me a misreading of Mahoney's account of social development, which highlights the same mechanisms of indigenous spatial isolation and exclusion as products of colonial rule, but attributes variation in those to more macro elements of colonial administration, and connects them to social development outcomes mapped at a larger spatial scale. Here we have, in other words, not only an empirical but a theoretical difference between the two books about the spatial scale at which, and the institutions through which, indigenous exploitation and isolation translate into poor social development outcomes, which suggests that we cannot simply agree that both books are correct.

There is also something less than satisfying in theoretical terms about stopping here, since what this does is simply to say that these two studies are asking different questions, and should in some sense be read independent of one another. My sense is that the conversation between more macro-oriented work and more micro work often founders on this challenge and retreats to separate, siloed, discussions. But this failure to engage, by definition, precludes the cumulation of knowledge across these two books, and these two traditions, and prevents scientific progress. Learning about historical outcomes will require us not only to integrate historical processes at different temporal scales into a single account, but to take into account the fact that historical processes, both of initial variation and of persistence, may operate at multiple spatial scales.

Conclusion

In the end, then, there are real challenges to bringing together work in these two traditions of scholarship. One of these—the question of periodization and intercurrence—has been explored in scholarship on American political development, and in the attempts to bring together that body of scholarship with HPE work on the American case, though not in the comparative politics realm. (Jenkins 2024) A second challenge, the question of units of analysis and spatial scale, has in my view been neglected by scholars in both traditions.

Moving forward to integrate these two traditions into a coherent research community that asks about historical processes will require a reorientation of efforts among scholars in both traditions to place greater emphasis on concepts and on theory. Thus the work ahead entails a common endeavor across historical scholars of all stripes, in a way that is too easily obscured by methodological commitments used to justify the validity of one approach to historical social science at the expense of the other. What we need to respond to the first challenge of intercurrence is a theoretical toolkit that will allow us to integrate multiple temporal processes into an analytical framework that also engages in cross-case comparison. Resolving the second challenge of causal processes operating at multiple spatial scales will require greater theoretical and conceptual precision as we identify the spatial units to which our theories apply. Both the CHA and HPE tradition, then, would benefit from complementing a focus on methodological precision with more explicit conceptual and theoretical work about the complex temporal and spatial dynamics they seek to study. I hope that we can avoid reifying methodological differences and instead work to deepen a conversation among all those with shared interests in historical research, so that scholars working in both traditions can learn to build on, rather than criticize or speak past, one anothers' work.

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A NOTE ON PERSISTENCE IN STUDIES OF EFFECTS

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IN recent years there has been a wave of research in the social sciences on the historical roots of subsequent economic, social, and political outcomes. These "persistence studies" attempt to show that subsequent outcomes are a result not just of proximate causal factors, but also distal factors from long ago. These arguments invariably postulate a causal relationship between an antecedent historical explanatory variable X_H and a contemporary outcome Y_C, yielding $X_H \rightarrow Y_C$. In most analyses XH is unobserved and the contemporary measure of the explanatory X_C is instrumented by Z_H to estimate the (persisting) effect of X_H on Y_C.

An example of this is Darden and Grzymała-Busse (2006), which addresses the question of why some communist parties retained power after the collapse of state-socialism while in other cases nationalist non-communist parties emerged victorious in the first post-communist elections. Their answer lies in pre-communist literacy rates (Z_H). Priority to the advent of state-socialism nationalist governments in the region instilled nationalist sentiments (X_H) in the citizenry through the educational system. These sentiments were carried through to the end of the socialist period (X_C), where they then influenced popular preferences for nationalist parties (Y_C) after state-socialism collapsed. Another example is Alesina, Giuliano, and Nunn (2013), which links plough usage (Z_H) to the formation of a gendered division of labor/gender norms (X_H), which then persisted into the present period (X_C) to affect female labor force participation (Y_C). Pretty much every study that relies on the temporal transmission or norms, preferences, practices to explain the effect of an historical regressor on a contemporary outcome can be interpreted in this way.

The purpose of this brief essay is to shed light on a relationship that is rarely explicitly acknowledged but is of fundamental importance for how we interpret the results of persistence regress-

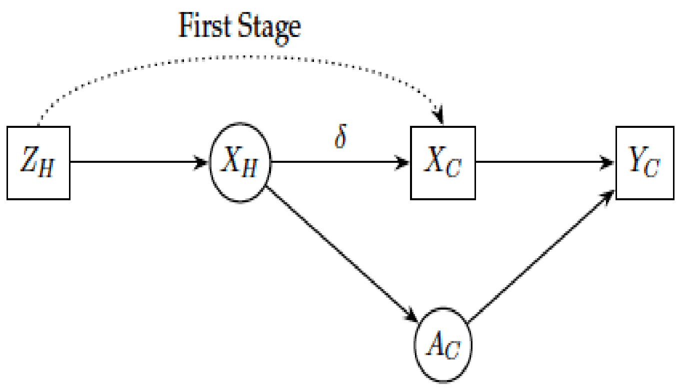

Figure 1: Figure 1 in Casey and Klemp (2021), identifying the first stage in a 2SLS regression and the relationship of primary interest, signified by δ. Rectangular nodes represent observed variables and circular nodes represent (typically) unobserved variables.

ions, that between the antecedent historical regressor X_H and its contemporary counterpart X_C. Two aspects of this relationship are noteworthy. The first concerns the degree of empirical association between X_H and X_C, which Casey and Klemp (2021) argue determines how credibly we can conclude that our outcome Y_C is the result of the persisting effect of the historical regressor X_H. The second aspect is the assumption of "sameness" underlying the practice of treating the regressor X as one variable measured at two points in time rather than different variables at each point in time. I argue that that sameness should be empirically justified rather than assumed ex ante.

Casey and Klemp (2021) offers a general model for interpreting long-run regres-sions with an antecedent historical regressor (X_H), usually unobserved and therefore instrumented by Z_H and a contemporary outcome (Y_C). Figure 1 reproduces their graphical illustration of the model, which includes a potential alternative endogenous regressor A_C that would come into play if the pathway of influence between X_H and Y_C passes not just through X_C, thus satisfying the exclusion restriction, but also through A_C. The implications of not taking the relationship between X_H and X_C into account when interpreting our results hold even when the exclusion restriction is not violated, so I do not consider A_C any further.

The gist of Casey and Klemp's argument is that properly estimating the long- run effect of X_H on Y_C requires knowing the effects of Z_H on both Y_C, which is the usual instrumental variable estimate and the quantity of interest to most researchers, and XC, denoted by δ in Figure 1 and an indicator of the persistence between X_H and X_C. Casey and Klemp demonstrate that this persistence, which is rarely estimated much less reported, acts a scaling factor (δ) moderating the estimated magnitude of the long-run effect (assuming such an effect is actually found). High persistence implies that the conditions created by the historical exogenous shock largely persisted into the present, and therefore the results can be interpreted as the long-run (persistent) effect of X_H on Y_C. Low persistence is strongly suggestive that the estimated effect is likely due a channel that does not involve X_C, in effect a violation of the exclusion restriction.

The practical upshot of all this is that historical persistence researchers should be more circumspect in how they interpret their results even when the standard identification assumptions for these analyses are satisfied. First, the degree of persistence should be reported and the appropriate adjustment made to the estimate of the long-run effect. In the very uncommon case where X_H is observable, Casey and Klemp recommend estimating δ as the slope from a regression of X_C on X_H. When X_H is unobserved they propose a method involving measuring X at a point between X_H and X_C and an assumed law of motion governing change in X.

This is not, however, the end of the story. X_H and X_C represent measures of the same variable X at two different points in time, for example national sentiments in 1910 and national sentiments in 1990 (Darden and Grzymała-Busse) or civic community in the decades before 1920 and in the 1970's across Italian regions (Putnam 1993). When we empirically assess the degree of persistence between X_H and X_C we are assuming that what counts as X during the earlier period is similar enough to what counts as X in the later period to make the comparison sensible. There is no way to interpret the relationship as persistence unless we assume that there is one thing that persists. That persistence assumption is inherent in how we label them, using X in both cases.

When we empirically assess the degree of persistence between X_H and X_C we are assuming that what counts as X during the earlier period is similar enough to what counts as X in the later period to make the comparison sensible

When the temporal distance is short between X_C and X_H one might dismiss this concern about sameness as nitpicking given that any conceptual evolution X underwent would likely be trivial and irrelevant to the outcome of interest. But the greater the temporal distance between the two points of measurement, the more evolution X is likely to have undergone and the less credible the similarity assumption underlying so many persistence studies. As an example, consider Dell (2010)'s argument about the long-term effects on later economic development of a brutally coercive labor practice (the mita) that the Spanish colonizers introduced in the 16th century to exploit silver deposits in Bolivia. Dell argues that places without the mita system evolved systems of property rights and land ownership that ultimately led to greater prosperity centuries later. Places with the mita lacked these institutions, with dire long-term economic consequences.

Abad and Maurer (2021) criticize this analysis. They wonder how long the mita existed in its classical form: "Historians have shown that the *mita* obligation changed over time. . . As a result the *mita* in the 1700s was a very distant cousin to its form in 1578." (Abad and Maurer 2021, 47). If the *mita* did in fact change over time, then the historical treatment Dell assumes does not evolve is actually a bundle that includes two very different *mita* treatments. Two things flow from this. First, the long-run effect will be different depending on which period is under consideration. Second, it is less tenable to attribute the contemporary outcomes to just the 16th century version of the *mita*.

I conclude with two points. First, researchers estimating models as in Figure 1 should report the degree of persistence between X_H and X_C and adjust the instrumen-tal variable estimate accordingly. Where persistence cannot be estimated then bounds on the instrumental variable estimate should be included by employing counterfactual persistence magnitudes.

Second, researchers should be sensitive to both temporal heterogeneity in the historical treatment and the degree of similarity between what counts as that treatment (X) during the historical period and what counts as X in the contemporary period. This means historical due diligence for the antecedent period(s) beyond satisfying the standard identification conditions. The greater the temporal homogeneity and the greater the similitude between the historical treatment and the treatment at the time the outcome is measured, the more credible any claim about the manner in which history matters.

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HPE Symposium

BEYOND IDENTIFICATION: A LIBRARY OF MECHANISMS IN LEGACY RESEARCH

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Historical political economy (HPE) is booming, with increasingly sophisticated methods and ever larger and more granular data. About a quarter of this work explores historical legacies—long-lasting effects of specific past events or structures on outcomes—where the gap between cause and effect may span several centuries (Charnysh, Finkel, and Gehlbach 2023). This literature has generated important insights, but it has also been criticized for paying too little attention to how causal effects are transmitted over time (Cirone and Pepinsky 2022; Simpser, Slater, and Wittenberg 2018). In much legacy work, most of the effort is devoted to causal identification and original data collection, while evidence on mechanisms is relatively thin and indirect.

Developing and refining a "library of mechanisms" that can be deployed and recombined across cases is important for the accumulation of knowledge in HPE

Developing and refining a "library of mechanisms" that can be deployed and recombined across cases is important for the accumulation of knowledge in HPE (Gailmard 2021). Systematizing likely mechanisms disciplines storytelling and makes it easier to see when two ostensibly different legacies are actually reproduced through similar processes. Subsequent studies can explicitly say whether they confirm, refine, or add to existing mechanism types, rather than reinventing labels case by case.

This essay reviews the most prominent explanations for the persistence of historical legacies in recent work. I underscore three points. First, not all legacies require a mechanism of reproduction—durable causes and outcomes may be sufficient. Second, when reproduction mechanisms do matter, they often fall into several basic categories. Finally, because a single HPE paper rarely delivers both clean causal identification and convincing mechanism evidence, HPE should lean more on other fields. Borrowing from non-HPE work can enrich our understanding of well-established reproduction mechanisms and suggest new pathways that link past events to the present-day outcomes.

I begin by clarifying the distinction between persistence driven by durable causes and persistence driven by reproduction mechanisms (Wittenberg 2015). Persistence may result from state dependence, since some historical causes endure and some outcomes change only slowly over time. For example, built and natural environments are both relatively stable and well known to shape attitudes and behavior. Within Poland, the density of the railroad network and patterns of settlement vary across the former partition borders, which were abolished in 1918. Such infrastructural differences may directly shape present-day economic opportunities, migration flows, social capital, and other outcomes. At the same time, infrastructural investments made in the imperial period may have set different localities on divergent developmental trajectories, generating durable economic patterns that cannot be attributed to the continued presence of railways alone. Indeed, some railway lines lost their purpose after state borders changed, but the localities they once connected may have continued to grow due to agglomeration economies. It is only the latter interpretation that requires a reproduction mechanism, and distinguishing empirically between the two accounts is far from straightforward.

Reproduction mechanisms are particularly useful for understanding continuities that outlive the original "treatment" variable and its direct effects. Some of the most common reproduction mechanisms are intergenerational socialization; path dependence resulting from institutional reinforcement or from lock-in, increasing returns, or negative externalities; and geographic sorting. These mechanisms operate at different levels of analysis and can interact and influence one another.

Intergenerational socialization is typically used to explain the reproduction of "cognitions," which include beliefs, attitudes, norms, and preferences (Simpser, Slater, and Wittenberg 2018). Studies that rely on this mechanism build on the theoretical framework developed by Bisin and Verdier (2001), who model direct parental socialization and children's indirect adaptation and imitation of their peers as substitute processes. Family occupies a central place in this model because parents have some control over the environment– they can choose schools and neighborhoods. Intergenerational socialization within families and communities can be reinforced or weakened by state and societal narratives. Churches, schools, and elites also contribute to the reproduction of cognitions over time.

Most papers that invoke intergenerational persistence do not provide direct empirical evidence consistent with its operation. An exception is Lupu and Peisakhin (2017), who survey three generations of Crimean Tatars to show that the legacies of violence against grandparents have shaped their identities and that grandparents' identities, in turn, are correlated with those of their children and grandchildren. To isolate the contribution of socialization within like-minded communities, Charnysh and Peisakhin (2017) study the allocation of Polish families from Galicia to majority- and minority-Galician villages in western Poland, showing that communal influence matters above and beyond family transmission.

Many legacy arguments instead emphasize the role of formal or informal institutions in the reproduction of social and economic outcomes. Institutions structure incentives and constrain choices, reinforcing behavioral patterns and cognitions. Parental socialization efforts may vary with institutional characteristics, since parents may be preparing their children for the future institutional environment rather than merely replicating their own traits and beliefs. Institutions also have distributional consequences, reproducing existing power hierarchies and resource inequalities (Thelen 1999). For example, Hariri (2012) argues that in areas where precolonial state institutions were more durable, colonial powers were more likely to rule indirectly and without a large European presence, leaving existing hierarchies and authority structures intact. As a result, pre-existing autocratic equilibria persisted into the postcolonial period. Research in historical institutionalism and other fields offers a set of further, more micro-level mechanisms to explain the persistence of institutions themselves after the conditions that led to institutional creation have changed (see discussion in Thelen 1999).

However, path dependence does not require institutions; it can also arise from lock-in, switching costs, increasing returns, or negative externalities (Page 2006). The adoption of particular technologies or investment in built infrastructure can set different localities on divergent developmental trajectories that persist long after the initial conditions have changed. For example, Bleakley and Lin (2012) show that cities formed at portage sites continued to grow even after transportation technology changed and their location advantages disappeared. They explain this continuity in the distribution of population and economic activity across space by the interaction of sunk costs and increasing returns to scale.

Geographic sorting is often studied as an alternative explanation for the reproduction of spatial variation in individual "cognitions." However, it can also serve as an important reproduction mechanism in its own right (Acharya, Sen, and Blackwell, 2021). People sort into like-minded communities, preferring to live next to others like themselves, which can reinforce spatial persistence in cognitions, demographic characteristics, and political preferences. For example, members of minority groups often settle in areas with co-ethnics, both for social support and economic opportunity. Over time, this reinforces spatial concentrations of language use, cultural norms, and voting behavior linked to ethnicity.

Different reproduction mechanisms can also work in tandem. Acharya, Blackwell, and Sen (2021) propose the concept of behavioral path dependence, which results from the combination of intergenerational socialization and institutional reinforcement. Lowes et al. (2017) argue that the strength of formal institutions in the Kuba kingdom reduced parental investment in socializing their children to follow the rules and behave honestly. In this setup, institutional constraints influence parental socialization. The persistence of built and natural environments can produce geographic sorting by attracting particular categories of people. For example, Blossey, Haffert, and Stoetzer (2025) show that working-class settlements constructed during late industrialization attract low-income residents today, locking them in and leading to political discontent. In this way, right-wing voting is a legacy of late industrialization, reproduced through the interaction of durable housing infrastructure and demographic sorting.

The difficulty of establishing that a particular mechanism operates is not unique to legacy studies, but historical work arguably faces greater empirical hurdles. Neither causes nor mechanisms studied in HPE can be directly manipulated. While the plausible reproduction mechanisms operate at micro or meso levels and cross administrative boundaries, historical data are often available for highly aggregated units (counties, regions, countries). Aggregation can wash out or confound micro-level mechanisms, and individual-level patterns may not scale up as theorized. Over longer time spans, reproduction mechanisms change and interact with contextual variables. Even in the best-case scenario where the mechanism can be operationalized and the gap between cause and effect is relatively short, the assumption of sequential ignorability necessary for a standard mediation analysis may be violated. The over-emphasis on statistical tests can obscure the messy reality and may lead to privileging mechanisms that are easier to quantify than others.

Because of these issues, few single studies can convincingly demonstrate both causal effects and reproduction mechanisms. Here, it is helpful to recognize that most reproduction mechanisms are not unique to legacy research. Rather than trying to do everything within a single paper, HPE scholars could lean more on work in other fields, despite its contemporary focus, to understand how specific mechanisms operate, and then use historical data to show where and when those mechanisms are likely to have been active.

A more systematic engagement with non-HPE literatures can enrich historical work and suggest new pathways through which historical variables shape present-day outcomes

Looking outside HPE to understand the mechanisms of reproduction can enrich our understanding and produce unexpected revelations. Consider intergenerational transmission, studied rigorously in psychology, neuroscience, sociology, and other disciplines using multi-generation longitudinal designs. Whereas most legacy studies conceptualize transmission as a one-way street, from parents or grandparents to children, research in other fields suggests that intergenerational socialization can also work in reverse, with children influencing parental attitudes through household interactions (Liu et al. 2022). As a result, at least some patterns attributed to the legacies of historical shocks may result from the younger generations reinterpreting their family or communal history in response to contemporary events. Other studies highlight the role of genetic inheritance in the similarity of cognitions within families. Using data on monozygotic and dizygotic twins, Loewen et al. (2013) estimate that 26–43% of the variation in self-reported dishonesty is attributable to genetic heritability. The role of intergenerational socialization in the similarity of cognitions between parents and children may therefore be overstated. A more systematic engagement with non-HPE literatures can enrich historical work and suggest new pathways through which historical variables shape present-day outcomes.

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PATHBREAKING WOMEN IN COMPARATIVE POLITICS: PEARL ROBINSON

Interviewed by Tumi Makgetla, Senior Lecturer in Political Science at the University of Cape Town, and Anna Mwaba, Assistant Professor of Government at Smith College

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Pearl Robinson (PR) *is a pioneering scholar of African politics whose career has exemplified how research and scholarly engagement can both redefine the boundaries of political science and amplify African voices and ideas within it. A professor of political science and international relations at Tufts University, she has also served as the president of the African Studies Association. Her research has shaped our understanding of African political life, democratization, and the politics of knowledge, while also building groundbreaking collaborations with universities and community organizations in Africa.*

This is an abridged version of an interview conducted by **Tumi Makgetla (TM)** *and* **Anna Mwaba (AM)** *in Boston, MA on December 4, 2025. A glossary of names and terms, a write-up of the full interview, and the podcast itself can be found here:* **https://gld.gu.se/en/governance-uncovered-a-podcast-by-gld**

> *I did not think I was challenging the mainstream of political science…I had determined that in order to understand African politics, you have to understand African cultures*

AM: We'd like to begin by hearing a little bit about your early life experiences that encouraged you to eventually pursue political science and the study of African politics, in particular.

PR: Actually, I began as a French major and was planning to be a second-grade teacher. I continued to take French every year. When I got to Bordeaux on exchange in my junior year, I found out I had taken so many French courses that if I took French literature courses in France, some of them wouldn't count towards graduation because there was a maximum number of courses you could take in your major. I thought, I can just take anything that seems to be interesting. I looked at the list of courses and ended up taking international relations, sociology of the book, a course on the politics of the Maghreb, and a two-semester course on Marxist political theory. And I enjoyed them all. These were courses I never would have thought of taking before going to Bordeaux.

But something else was happening. This was '64, '65. There was a convergence of one of my long-standing interests, which was what was happening with race relations in the United States, and a new interest in Africa. It was the first decade of African independence. Martin Luther King won the Nobel Prize while I was in France, Malcolm X was assassinated. And there were, at Bordeaux, a group of African women, foreign service officers, who were being trained to be professional interpreters. They were talking about African international relations and things that I'd never heard about and I found this all fascinating. In my international relations course, there was a Congolese student He had four pictures on his wall, and he would point to them as he called out their names: "Lumumba, Nkrumah, Martin Luther King, Malcolm X, it's all one struggle." And I said, "Yes, it is."

In college, I started reading Negritude Literature, just going to the library on my own to read a few books written in French by Africans. These books weren't taught in my French courses. When I got to Bordeaux, it seemed that everybody I knew had read these books. It was the first time I had a chance to talk about this literature with anybody. And by the end of the year, I still remember, a light clicked on in a hallway, and I thought, "I know what I want to do with the rest of my life. I want to spend my life learning about Africa and sharing what I learn and know with other people." And it would be all about politics. So I decided to declare a second major in political science.

TM: When you went to study abroad, did your parents have any advice for you?

PR: The most stunning advice came from my father. He called me from his office one day and he said, "There's one thing I want you to remember. If you go over there, you're going over there for an education." He said, "When I got my first car, it was a Model T,"—and he's from Arkansas—"Driving down the back roads of rural Arkansas with that car, I was feeling really good. And then I looked and I saw the body of a man who had been lynched was still hanging from a tree. That reminded me that I was getting an education for the advancement of the race. So if you go to Europe for a year, don't call home. Write letters. And remember you're not over there for tourism, you're getting an education for the advancement of the race." I hung up the phone. My father had never said anything like that to me. And I've never forgotten it.

I graduated from UC Davis with a double major in French and Political Science, then did an MA in Political Science with a thesis on federalism in Cameroon. My African Politics Professor encouraged me to do a doctorate. That was a new thought.

When I was in France and started thinking about what I wanted to learn about Africa, I read Frantz Fanon's book *Les Damnés de la Terre* (The Wretched of the Earth) in French. What really drew me into the book was the section at the end, where he talks about the individual experiences of people who were his patients. Fanon was a psychiatrist. Those individual experiences of his patients connected to the book's larger political discussion in a way that enabled me to understand what people were going through in French-dominated Algeria. And I determined at that point that if I was going to study and learn about Africa and African politics, my studies in some ways had to be centered on people and their experiences. And then I also realized that being in France for a year, I had come to know and understand France by becoming fluent in the language. So, I also decided that if I was serious about studying African politics, I needed to learn how to speak an African language.

When I began coursework for my MA I wasn't aiming for a PhD. When I couldn't get a job with an African government, I joined the Peace Corps. I requested a rural development placement and went to Niger as a public health educator. I spent two years and three months in Niger in a rural Hausa town, population about 8,000, doing home visits with mothers who had given birth in the local clinic. Toward the end, I had a case load of about 200 babies and got to know the households of the cross-section of the town. I developed an understanding of polygamy and many less visible aspects of the local culture.

In Niger, I became personally involved at the tail end of the campaign to eliminate smallpox. I also discovered that pockets of slavery still existed in the country and was appalled to see one of these women slapped when she came to the clinic to give birth. For recreation, I bought a horse, learned how to ride and I loved it. I'd ride evenings at sunset.

AM: You started your PhD program at Columbia in 1970. Can you tell us about your most influential mentors there and how they shaped your approach to studying politics?

PR: When I went to Columbia to study Political Science, I knew that I would also take anthropology. I was going through the course catalog and said, "Elliot Skinner teaches here!" Skinner, a Trinidadian by birth, was US Ambassador to Upper Volta (now Burkina Faso) when I was in the Peace Corps. He was the first American Ambassador to an African country who could speak the local African language. He had returned to Columbia University as Franz Boas Professor of Anthropology and Chair of the Anthropology Department.

I made an appointment to talk to Professor Skinner about taking his course. He thought that I was not prepared and advised that I audit his course. I told him, "I can't audit. I am getting two degrees here—a PhD in Political Science and a master's in African Studies. I need course credit for African anthropology—I can't just audit it." He finally allowed me to take the course for credit but cautioned that I might find it a challenge. This story had a happy ending. Elliott Skinner became my mentor and a lifelong friend.

I also had two extraordinary women professors at Columbia. When I learned that Margaret Mead was on the faculty, I enrolled in her course on audio-visual field methods in anthropology. Meade is the best teacher I've ever had. Our small class of 16 was divided into two groups, with each group responsible for producing an audio-visual presentation based on anthropological fieldwork conducted in New York City. She never assigned a book. Instead, during our weekly 3-hour sessions, Meade curated unexpected learning experiences in the classroom. We learned how to "read people," how to document their interactions with each other and the outside environment, and we were encouraged to theorize the consequences of these interactions. I found these methods to be powerful tools for studying African politics up close.

I also took a course in development economics with Barbara Ward. Not a numbers cruncher, she talked about ethical concerns and believed that international development should address issues of poverty and social justice. My exposure in the classroom to these two stellar women professors was truly inspirational.

TM: Your work has helped redefine how we understand political change in Africa, emphasizing culture, legitimacy, and everyday governance rather than just institutions. For example, your dissertation is a groundbreaking study of traditional governance structures in Niger, written in a moment when most political scientists were focused on the institutional design of the state. Looking back to your doctoral studies, how do you think your approach challenged the mainstream paradigms of political science at the time?

PR: I like the way you frame that. I did not think I was challenging the mainstream of political science. I thought I was studying not only what was interesting, but what was important. And whatever the mainstream was doing, it just seemed to me that they were missing the things that I thought were not only interesting but even more significantly important because they were too often absent from conventional political analysis. I had determined that in order to understand African politics, you have to understand African cultures. And that's why I studied chiefs.

I turned to anthropology to study chiefs. I'm certain that Skinner influenced my decision to privilege participant-observation methods. He had done groundbreaking work studying labor migration in Upper Volta/Burkina Faso using participant observation and grassroots interviews to build theory related to strangers and urbanization in Africa. That's the way I wanted to do research. I wanted to be there. And like Franz Fanon, Skinner was talking to ordinary people, writing about their personal experiences, and using the mix to theorize labor migration and urbanization. He wanted to show that anthropology could address contemporary African policy issues.

Quite frankly, I am drawn to chiefs because I like the pomp and circumstance associated with their traditional regalia. I also correctly assumed that studying the chieftaincy as an institution would enable me to better understand the politics of Niger's one-party state.

TM: Can you tell us how you first became connected with the Ford Foundation and how you viewed the significance of the volume you edited with Elliot Skinner, *Transformation and Resilience in Africa: As Seen by African American scholars*?

PR: This goes back to my relationship with Elliott Skinner. Ironically, I think the worst advice he ever gave to me as a PhD graduate student opened the door to a distinctive personal relationship with the Ford Foundation. When it was time for me to do dissertation field work in Africa, I applied to the SSRC and Fulbright for funding. After making the first cut for SSRC, I failed to get a grant. And I went to Skinner despondent and said, "Well, it looks like I'm not doing fieldwork, and I won't be able to become a reputable scholar of Africa. I didn't get the SSRC grant.

Then, Skinner told me that The Ford Foundation was offering a new dissertation research fellowship for African-Americans to do field research in Africa. I think there was funding for eight awards. In its first year, applications were way below target.

Skinner advised that I apply for the Ford grant and was pretty certain that I would be successful. He was right. Subsequently, I learned of my selection for a Fulbright dissertation award. That posed a dilemma. Applications for Ford's African-American fieldwork fellowship remained low, and there was concern that the program might be terminated. Skinner encouraged me to take the Ford fellowship, so I did. That meant declining the Fulbright.

What I didn't know, however, was that the Fulbright included a second year's funding for write-up. With the Ford money, I spent 14 months in Niger and returned to New York with no financial support. I was angry as hell.

Skinner hired me as a research assistant on a UN project related to the Sahelian drought. He had a UN contract to produce a background publication. Paid on an hourly basis, I did library research on topics related to French colonial agriculture and drought in the West African Sahel. I also wrote narrative summaries of the information. The work was tedious and I was neglecting my dissertation. I felt exploited and felt my career slipping away.

Then, one day, I happened to be in my dorm room when the phone rang. It was the personnel office of the Ford Foundation, doing an affirmative action outreach call. I was invited to interview for a position as a Program Officer for Francophone Africa. I went for the interview, but the job weas offered to a woman who was a Professor at UC Berkeley. She ultimately turned it down. A few weeks later, I was again contacted by Ford's Personnel office and told that I was the second choice for the Francophone position. But knowing that I was still working on my doctorate, the Middle East and Africa office, which had funded my dissertation fieldwork Fellowship, decided to create a temporary Training Associate position based in New York and was offering it to me. This new arrangement would enable me to work for Ford and earn a salary while writing my dissertation. A year and a half later, I defended the dissertation with Distinction, was awarded my PhD, and moved to Boston to begin teaching Political Science at Tufts University.

AM: Can you tell us about how you grappled with questions of belonging, voice, and balance during your early years as a faculty member? Do you have any survival strategies or guiding philosophies that you would share with early-career scholars today?

PR: No, I guess every place is different. My first day teaching at Tufts University, I went to the faculty dining room for lunch. Nobody from the Political Science Department accompanied me, so I went alone. The person at the door refused to let me in – assuming I had mistakenly found my way to the faculty dining room. When I introduced myself as a new member of the faculty, she responded that nobody seemed to know me.

I looked inside and spotted TJ Anderson, Chair of the Music Department and one of Tufts' two tenured Black faculty members. TJ was having lunch and happened to see me at the door. So, he came over and he said, "She teaches here." With his endorsement, I was allowed in.

The next time I went to lunch, I simply walked in, sat down at a table that was all male, and I introduced myself. One of the men asked me, "How does it feel to know you got your job because you're Black?" My brain clicked in, and I heard myself saying, "I guess I must feel like these Kennedy children who never know if they get the things that they get because they are Kennedys." That ended our conversation. But I continued to have lunch in the faculty dining room. And in a few months' time, I was treated as though I belonged.

AM: Can you describe some of the challenges of engaging in scholarship intended to meaningfully shape policymaking?

PR: If you're in the right place at the right time and have done scholarship on an issue that is critical, you may be able to have an impact on the related policy. But people must be convinced that you know what you're talking about.

For example, when Jendayi Frazer joined the faculty of The Kennedy School, she taught a course on Shaping US Africa Policy and faced strong student advocacy to address the death toll of the AIDS in Africa crisis. While supporting the issue, Frasier insisted that the magnitude of the problem far exceeded USAID's health budget, and that the serious money was in the Defense Department budget. The policy challenge was to redefine AIDS from a health issue to an international security threat—which she tackled successfully. "Whither the Future of Blacks in the Republican Party" was my small contribution to this policy shift. When Jendayi moved from Cambridge to Washington to become National Security Advisor for Africa in the George W. Bush Administration, I gave her a copy of my article. It argues that we need policy-competent Black analysts in both major US political parties as activists on behalf of Black peoples' interests. She acted accordingly.

TM: In recent decades, your research has also focused on the representation of women's interests in Muslim-majority countries like Niger, culminating in the documentary "Mama Kiota" on the woman's movement she established. How did you decide to focus on Mama Kiota and why did you decide to pursue this project through a film documentary?

PR: I had never focused on women in my research and I wanted to do that. While I was in Niamey, a Nigerien friend invited me to attend a symposium she had organized on Women and the Law. There were three panelists: The Dean of the Law faculty, a Nigerien woman judge, and this young woman in her 20s who was extremely self-possessed and articulate. She recited the Quran, the Hadith, and was enrolled in a course at the Law School. I thought, "I've never seen any African woman, or any American woman, quite like her."

I went up to her and said, "I'm Professor Pearl Robinson and I'm a political scientist from the U.S. Who are you?" The conversation went as follows:

Zahara: "I'm Zara Sheikh Abubakar Kiota."

Pearl (smiling): "Are you related to Sheikh Abubakar of Kiota?"

Zahara: "Yes, he's my father."

Pearl: "I met your father."

Zahara: "Have you met my mother?"

Pearl: "I've never heard of your mother."

Zahara: "Well, you must meet my mother. I'm inviting you to meet my mother. She is called "Mama Kiota."

My first trip to meet Mama Kiota coincided with a Muslim religious holiday, so her religious duties kept us from spending time together. She told me that her Muslim women's organization, the Jamiyat Nassirat Dine (JND), was having a "Congrès" the next month and invited me to attend. They would be talking about the role of Muslim women in the World Bank's poverty reduction program for Niger. I almost fainted. I told myself to act like this was a normal thing to say. I said, "Yes, that sounds really interesting."

There were more than 1,000 people at the poverty reduction Congrès. Government officials and religious leaders were there, but the event showcased the voices of Muslim women. I watched as a woman walked to a centrally located mike to comment on poverty and cooperation: "We women, we have to work together." Events continued into the night. As I sat watching, I decided, "I'm going to study what they're doing, and call it politics."

I returned to Niger the next four summers to work on my Mama Kiota Project. Research at the Niger National Archives as well as life history interviews with 20 JND leaders revealed Mama to be a shrewd political operative. My visits to Kiota during the first summer were enriched by her personal archives, which include statistics on her schools as well as family photo albums.

During subsequent trips, I visited Kiota's solar-powered rural radio station, the JND's credit union, Mama's schools (kindergarten through lycée), and the library. I was present when Niger had its first local government elections and filmed the long lines of women waiting to vote. Because Kiota is a religious community, Sufi singers and calls for prayer produce a dramatic soundtrack for daily life. I used PowerPoint to organize files of all these sounds, images, and events. At the end of each day, I showed Mama what I had collected, shared what I had learned from interviews, and asked for her comments. One evening, she stopped me and said, "You're learning things about us that we should know. Is there a way that we could put those pictures up on a wall so that people can talk about what you are learning and we can share knowledge? I told her "Yes, that can be done with a projector." When I returned the following summer, Mama Kiota told me, "My son went to Dubai. I now have a projector. What do you have for me?" That was the beginning of my filmmaking career. I had been thinking about journal articles, but Mama Kiota wanted a movie.

I teamed up with the prizewinning Nigerien filmmaker Moustapha Alassane as principal Editor. I wrote the script with considerable help from Rekia Hama, an English teacher and native Hausa speaker from Niger. Moustapha edited the 40-minute movie, which is narrated in Hausa, with people speaking the five languages used daily in Kiota (Hausa, Zama, French, English, and Arabic.) Subtitles are in Hausa. The film, titled Mama Kiota, projects striking images of female empowerment in Niger. It is available as a DVD.

After the film was completed, I did a launch tour in Niger, Ghana, and Nigeria. Screenings were always followed by open mic, and the audience was invited to comment on what they learned. I arranged a screening Iin Maradi, Niger to which 15 formerly enslaved Nigerien women were invited. One of these women picked up the mike and shouted, "I'm tired of being poor! I want to learn how to read! I want Mama Kiota!" I was unable to hold back tears. I still need to do a version with subtitles for English-speaking viewers.

TM: Your current research focuses on Ralph Bunche's intellectual biography. How has this project changed your understanding of the history of African studies?

PR: The working title of my book is *Ambition, Erasure and Resiliency: An Intellectual Biography of Ralph Bunche.* What I've discovered is how much of Bunche's intellectual biography has been erased. The first major erasure was his award-winning Harvard dissertation on French Administration in Togoland and Dahomey—which was never published, despite rave reviews of the dissertation.

Bunche states in the Preface that he went to Africa to study colonialism "from the perspective of the native." His introduction begins with a discussion of governing systems in Togo and Dahomey that existed before colonial rule. Then, turning to his field studies, he details how the French were training Africans for self-government, and compares these activities with what was done before colonial rule, when societies were governing themselves.

Had the dissertation been published, it might have set a precedent for political scientists who study colonialism and its aftermath to pay more attention to the modalities of pre-colonial self-government. Instead, a precedent was set for identifying Africans by the people who colonized them – e.g., anglophone, francophone, and lusophone Africans.

AM: We'd like to turn to your engagement with African universities.

PR: When I arrived in Kampala at Makerere University for a year in 1997-1998, I joined the Political Science Department as a Ford Foundation Visiting Professor. With a teaching staff of 18 professionals, I was one of only 4 PhDs. My office had 3 desks for 5 people. And nobody dared keep their books on the shelves, for fear of theft. Yet as a Political Scientist who studies African politics, Makerere was the most intellectually stimulating university experience of my career.

The University bristled with political activity. Internationally prominent speakers—including Heads of State—were par for the course. And students were eager to learn. Every day, I read 3 local newspapers, watched local TV, listened to the radio, and discussed politics with my Makerere colleagues. We lacked the latest books and dependable internet connectivity. I could manage, but the average student struggled. Returning to Tufts, I wondered if there was a way to have the best of both worlds.

I went to the Ford Foundation with an idea and learned that Ford and 3 other US foundations were planning a ten year, three hundred million-dollar commitment to revitalizing 18 universities in Africa. If I would wait for a year, they could fold my project into it. And that's what happened. I eventually received grants from Ford, Rockefeller, and USAID for projects at Makerere and the University of Dares Salaam.

My Ford grant funded the very first computer lab in the Faculty of Social Sciences at Makerere. Overall, the project included infrastructure, curriculum development, books on regionalism in African International Relations, staff development opportunities for teaching faculty and digital librarians, computer-enhanced learning for students, and pedagogy.

I believe more political scientists who study Africa should address policy issues and focus on crafting trajectories of change that also involve African diaspora communities around the world

TM: In 2003, you wrote "Area Studies in Search of Africa," assessing African studies over time in the American academy. You served as president

of the African Studies Association just three years later.

In this role, what were your major goals in terms of shaping the study of Africa?

PR: I was the last person to chair the SSRC's Joint Committee on African studies. The Cold War was over, and funders had decided that Area Studies were no longer necessary for the US national defense. I never thought of African Studies as a Cold War project. For me, it has always been an intellectual project with political legs. When David Szanton invited me to write the chapter on African Area Studies for his book *The Politics of Knowledge: Area Studies and the Disciplines*, I saw this as an opportunity to write the Historically Black Colleges and Universities back into the origin story of African studies in the United States. As I began to reflect more broadly, I accomplished my initial goal—and much more.

TM: When you served as president of the African Studies Association, what are your proudest achievements?

PR: My first general meeting as an elected officer, I was Vice President, and our President announced that the Association was going bankrupt. He said, "When Pearl gives her presidential address, she will probably be announcing the end of the African Studies Association." My three years as part of the elected leadership were spent working collectively with people who were trying to save the organization financially, and we succeeded! That's what I consider my biggest achievement in terms of the organization as a whole.

But I had an earlier contribution. Before becoming an elected officer, I served three years on the field? I consider Africa to be the entire continent and African peoples spread across the globe.

That year, the Committee spent a lot of time debating which books should be read and whether certain ones should be eliminated. In the end, a book that addressed Africans on the African continent as well as Africans spread across the globe won the prize. Now, when you walk through the book exhibit at the African Studies Association's annual meetings, you will see that most publishers are defining African Studies as Africa and its Diasporas.

TM: Do you have any final pieces of advice that you would give to graduate students and junior scholars in political science who hope to produce the type of impact that has defined your career?

PR: As a graduate student, I focused on completing individual tasks: "I've got to write this paper, I've got to do field work, I've got to write a dissertation, I've got to finish this grant proposal, I also have to go on the job market." I wasn't thinking about larger projects. But afterwards I began to see these discrete initiatives as part of a trajectory. Working at the Ford Foundation enabled me to learn something about failure as well as success, and to think through how things might be improved – one step at a time.

I believe more political scientists who study Africa should address policy issues and focus on crafting trajectories of change that also involve African diaspora communities around the world.

Interviews with CP Scholars

RESEARCH IS *ME* SEARCH: DAVID LAITIN

Interviewed by Melina Platas, Associate Professor of Political Science at New York University - Abu Dhabi

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Melina Platas (MP): I was thinking about how to introduce you for this interview, and was reminded of David Letterman's show—"My Next Guest Needs No Introduction"—that's how I feel about you. But since some of your key contributions have been in the field of identity, I was wondering if—by way of introduction—you could share some of the identities that you think have most shaped your scholarship and the way that you have thought about the world?

David Laitin (DL): As you know, I've pushed for the notion of multiple repertoires, and depending where I am, the way I think of myself and present myself differs based upon context. So for years I portrayed myself as someone from Brooklyn, as this was an important identity for me. It was a fantastic place to grow up. Two and a half, three million people, most of them second generation immigrants, quite diverse—from Catholic, Protestant, Jews, but very few Muslims in that era. And every ticket in New York City elections had to have one Catholic, one Protestant, and one Jew for the three leading posts. So we felt ourselves as a kind of immigrant paradise. Most of my teachers in Brooklyn were Depression graduates who graduated from college in the depression of '29 to '35 and could not get any other job, or certainly couldn't get into graduate school beyond the teacher certificate. And it turned out, in retrospect, that my teachers in Midwood High School were so much smarter than my professors at Swarthmore College who were all blue-bloods, gone through the Harvard system. It took me years to realize how special the Brooklyn identity was.

...most of the stuff being written on Africa when I got to Berkeley, [was] asking whether these countries were going to become pro-communist or pro-democracy... I had an intuition which said—this can't be what people in African villages would think about, this has no relevance

MP: Were you always interested in politics?

DL: I was always interested in politics. When I was interviewed at Swarthmore, brought into Swarthmore by the tennis coach, the dean of the college interviewed me, and he asked me what I wanted the major in. And I had never used these two words together in my whole life—I said "Political Science," not having any idea what it was, except I was politically interested in New York and national politics. And also, I was a great John Kennedy fan. I'll talk about that in a minute because it's another identity. He asked me if I thought political science could be a science. I gave him some coherent answer, saying, "Yes, we have to be systematic about what we are trying to learn, and we have to keep a kind of objectivity away from our own prejudices." I said these things, never having said them before. And I started off at Swarthmore as a political science major. I went to graduate school as a political science person. So yes, it goes back to when I was 18 years old.

MP: Was there something in your childhood that made you interested in politics?

I would say, coming home as a six-year-old with my mother glued to the new television watching the McCarthy hearings, with her generation being Hunter College and CCNY socialists—although never saying that to me—but scared about people she knew who were being fired from the New York City school system, and the fear that she would be named. Her name came up once in the hearings. So politics had this exciting and fearful feeling for me.

MP: I want to ask more about your journey as a social scientist. I feel like some of us have formative places or cases that we end up in almost by accident, and they have an outsized influence on the rest of our lives and the rest of our career. I get the sense that for you, Somalia is somewhat like that. It seems your experiences there had an effect on the rest of your career, and the topics and questions you think about. I don't know if that's an accurate representation, but I'm interested in that part of your life.

DL: It's completely accurate. And it started off as, how should I call it, an intuition. I came to Berkeley for graduate school and I was interested in Africa. I was interested in Africa in large part because in 1960-61, that was the era when Africa's future looked like it was the most exciting thing happening in the world, and I wanted to understand it. All these new ambassadors were coming into New York from countries I've never heard of, but it seemed exciting. John Kennedy certainly picked it up. I often lecture on and I've written on the excitement of John Kennedy supporting the Algerian revolution and the anti-colonial perspective that he brought to bear, and then the notion of the Peace Corps, which had a tremendous influence on my life.

But getting back to your question, almost all the dissertations written in Africa, most of the stuff being written on Africa when I got to Berkeley, were asking whether these countries were going to become pro-communist or pro-democracy, and are they going to be pro-Soviet or pro-American. I had an intuition which said—this can't be what people in African villages would think about, this has no relevance.

I was influenced by the *négritude* movement that Senghor and others were part of, not so much, but it gave me the idea that with this ideology of *négritude* and African personality that the new leaders were recognizing or pushing politically, that one aspect of African culture and African personality was being ignored—and that was continued use of European languages and colonial languages for all aspects of official life. I wondered why this was so.

The doyen of African studies came through Berkeley— Ali Mazrui—and I asked him why, and he provided the typical answer, "Well, Nigeria has 400 different languages, and the only fair way to do it is to choose a language which nobody speaks." He was a humorous guy. And I thought about that, and then realized there was one country in Africa—I had one factoid about it—and that was Somalia, which had only one language, three foreign languages, and the official languages were the three foreign languages and not the one indigenous language they all shared. Something was wrong with Professor Mazrui's—

MP: Hypothesis?

DL: Or certainly joke. I went around to other graduate students that I knew at Berkeley and said, I can't write this dissertation. I don't speak Italian. I don't speak Arabic. And I don't speak Somali. But this will be an interesting contribution about neocolonialism and why colonial languages survive or prosper, even after independence.

And meanwhile, this is another long story, I was subject to the draft of the Vietnam War. Most of my—from my senior year in college to my first year in graduate school—most of my time was spent in trying to figure out how to avoid the draft. And my draft board, for reasons that I don't fully understand, permitted me to go into the Peace Corps. Even though LBJ—President Johnson—was clear that that should not be and cannot be a way to defer the draft. But they allowed it.

It's another long story, but I pursued an application to the Peace Corps. I got a phone call from them, in the midst of my first year of graduate school, in my class of '67—we were given one year of graduate school before we had to be subject to the draft—this was six or seven months into my first year of graduate school at Berkeley. I get a call from the Peace Corps and they said, "There's a new opening for Somalia." And I said, "Will I learn the language?" They said, "There's a four-month immersion program in Somalia that all the people in the program will be required to pursue." I said, and I wonder why I'm not divorced now, I said, "We accept." To make a long story short, I fell in love with a place. Every day I fell in love with the place, with the attention to poetry, with the way people walked, the way they talked. I can give many examples, but that was the first place I understood cultural difference and the implications of it.

MP: So you picked Somalia because it was the place that would allow you to answer this question that you had?

DL: I didn't pick Somalia. The Peace Corps picked Somalia. I had been interested in Africa. I put, and I wanted, my first choice was Tanzania. I was a fan of the first president of Tanzania, Julius Nyerere, I thought this would be an exciting country to learn about.

MP: Maybe just out of my own curiosity, but also because I feel like people have such particular imagery of Somalia when they think of it today—can you describe what it was like living there on a daily basis?

DL: Well, I was living on a normal school campus. So it wasn't in a village. And therefore, I didn't have the experience many Peace Corps volunteers had of experiencing everyday village life. But I did all the marketing, which meant a 25 kilometer jitney trip to Mogadishu, hanging out in Mogadishu for the day and going to the different markets, talking to all the sellers in the markets and hanging out at various places. So I got a feel for Mogadishu life, less of a feel for what it was like to wake up with the camels around me in the nomadic life. So my experience was quite limited, in only eight months there, before we were thrown out because of the military coup. But I would say, the interactions with first generation urbanites were always interesting, always fun.

MP: And out of that experience eventually came two books. Can you tell us a little bit about how those two books came about?

DL: So, I told you about this interesting dissertation. I immediately started reading for this dissertation, told all the Somalis who were our language teachers what this idea was, and almost, I think, two months into Somalia, I wrote a letter to my dissertation advisor of what this book would be, what this dissertation would be. And chapter by chapter, that's what it became, and the first book.

But the basic idea was two questions. First, why were the Somalis unable to agree upon a script that would have allowed them to make Somali official? There was a military coup there, and after we left, it ultimately chose a script for the language -- it couldn't be done through democratic procedures before the coup. I was interested in the question of what it took to make Somali the sole official language of the country.

But the big question was, did it make any difference? I got research clearance to come back to Somalia to answer that question. I went to Somalia to run what we today call experiments, getting people who were bilingual in Somali and English to discuss the same topic or to engage in dialogues on the same topic and to see if there was any systematic difference in the way they approached political issues. That was my big idea. And I got thrown out of Somalia within two weeks because I was a former Peace Corps volunteer. Put on the next plane to Nairobi. And my dissertation was cut.

The first place I went, I told a cab driver, "Take me to a place,"—I only had a few hundred dollars of travelers checks—"Take me to a place which is not too expensive where I can get a room," and he took me to Eastleigh [Nairobi]. And the first place I went to get rice for dinner, all I heard was Somali around me. I said to myself, I think I could fulfill my dissertation idea in North East—what was then called the North Eastern Province [of Kenya]—which was a Somali-speaking province. And after a month or two, I got clearance to go to Wajir and performed those experiments in Wajir. And they confirmed my early hypotheses about what differences I would expect. And that became part two of the dissertation and the book.

MP: I'm curious, do you consider yourself an Africanist, and what do these regional identities mean for us as comparativists, do you think?

DL: Let me just make one small correction. I was heavily influenced by Clifford Geertz and the notion of cultural system. I saw language as a cultural system. And I wanted to ask myself, when I finished that, is there another cultural system that I could study to see more broadly beyond language? And religion was obviously the next one to think through. I started reading the sociology of religion, and I realized I could not go back to Somalia to study the impact of religion on politics, because everyone in Somalia was Muslim. I had to find a place where you would get the exogenous placement of religion on a single population, and then I would be able to look at Christians versus, say Muslims, and say— is there any difference from your grandparents being converted to Islam from your grandparents being converted to Christianity, and everything else about your culture was the same, could you find any difference? That became the project in Nigeria, which was published as *Hegemony and Culture*. That wasn't the question you asked.

MP: But it actually follows on the same theme—I had said in the beginning, it struck me that one of the things about you is Somalia, and your early experience there, featuring in how you think about the world and kind of the questions that you ask. But a second thing about you is that you find cases or places that are best suited to answer the question you have rather than the other way around. So going back to my question earlier about being an Africanist or not, or whether that label or identity makes sense—

DL: Yes. So, to link that to your earlier question, there was a group of us in the political science world in comparative politics that created the CP Comparative Politics section of the American Political Science Association. Peter Lange was a member of this. Ronald Rogowski was a member of it. Sidney Tarrow was a member of it. And we all pushed for a comparative politics section, and not an Africanist or a Europeanist or an Asianist. We felt that, if we did that, we would silo ourselves from being able to explain variation across societies and across the world. Eventually, the area studies folk did create sections, but we held it off for a while, and we were the first generation that really saw comparative as question-based rather than region-based.

But let me emphasize that that did not mean that we weren't careful to study the actual places we were working in, that Bates and I, who basically trained most of the Africanists of the next generation, were competent speakers—Bates of Bemba and me of Somali -- whereas our earlier generation, [David] Apter, [Henry] Bienen, and [James] Coleman, they only spoke French and English. So we much more, let's say, studied the places, the cultural foundations of the places we worked, even though we didn't consider ourselves particularly as Africanists.

MP: You wrote "Comparative Politics: The State of the Subdiscipline" in 2000. Reflecting on it now, is there anything that surprised you in the intervening years?

DL: I would say a couple of things here. First, on the dependent variable focus. One of the ways the discipline has changed in the past 25 years, and I'm part of it, is much more focus on the independent variables. That is, looking at what we now call "treatments" and seeing the effect of a treatment on an outcome. I give some space in that article to an independent variable focused approach, and don't shut it down, but prefer the dependent variable [approach]. But more and more that comparativists are asked, "we're interested in turning troops into farmers in Sierra Leone, and we have this democracy treatment, and we want see the effect of the treatment on outcomes." So that's, I think, a big change from trying to find out what lowers the probability of a civil war onset in cross-national data sets.

A second exciting change, linked to that, is that the experimental approach has become a necessary part of many dissertations. In that volume where my essay appears, there's a piece by Gerber and Green that basically foresaw the turning of comparative politics into biology almost, where experimental labs are an essential part of the toolkit. And that's an exciting development.

And maybe a third thing is addressing the issue, as the discipline has become more and more micro—that's another big change, as opposed to seeing macro variables, we're seeing many more micro variables—that one of the big issues that arises when you're dealing with micro studies is external validity. I would say the comparative politics field is moving more and more into multiple-authored attacks on an independent variable—let's say what the Metaketa group has done, is to take something like community policing—that's the independent variable—and study protocols, and then sending leading scholars to different places in the world with the same protocols, and thereby being able to say something about the external validity of any finding of a [lab] experiment or field experiment.

So I would say the approaches of independent variables' effect on outcomes, rather than looking at outcomes and seeing what independent variables explain them, is a change in orientation. It's led to more micro studies, that is, the treatments, and the micro studies raise the issue of external validity, and then comes in Metaketa, and Metaketa and other approaches of looking at the same treatment in different contexts, is a major change from the way I foresaw the the subfield 25 years ago.

MP: There have been huge benefits of many of the changes you're talking about, the credibility revolution, the expectation of causal inference in much of what we do. But taking a long view, do you feel there are questions that we're missing or tend not to try to address precisely because they don't fit this new model very well? One can also get the sense that we're firing many different directions, and we're learning a lot, but it's hard to put it all together as neatly and conceptually as it was when it was more organized around dependent variables. I don't know if you agree with that assessment, but I'm curious about the consequences of these changes for the production of knowledge?

DL: If you look at our immigration policy lab, I think what we see is just a whole range of micro studies. Is there someone who's going to come back with a macro vision and say, this all fits into a view of comparative politics that can be more general, more macro, that we feel is more unified? Right now I think we're in an exciting moment where our small findings, not so small, but we can think of them as narrow or well-specified —I like to call them well-specified—but that we can know things like the size of the population of slaves in a particular district in the American South has implications for racism 150 years later, and we can make this a causal argument, is exciting.

So I don't want to say that we should try to do better than this, but someone will do better and give us a more general view of the field, that always happens. But right now, I think we're in a period of exciting experimentation in field experiments, in lab experiments, and in use of historical data to find patterns that were previously unknown. So I think it's a fairly exciting time.

Let me say something about causal identification. There's a kind of fetishness in economics that we copy in Political Science. As if, if you're not doing causal inference, you're not doing anything. It reminds me of 20 years ago, if you didn't find Nash equilibrium, you're not doing science. And 10 years before that, if you don't have a cross-national dataset, you'll never know what the patterns are.

And you've got to give credit to Josh Angrist and others for bringing Rubin causation into our field—but to say, "if you're not doing that, you're not doing anything," is a gigantic error. From what I see in reviewing *APSR* submissions, I think the editors are aware that causal identification doesn't have to be the key part of every published contribution. I think that we should appreciate the tremendous success of this revolution, but also understand that it's going to be surpassed by some other, "if you're not doing it, you're not doing science," move in that scientific direction.

There's a kind of fetishness in economics that we copy in political science. As if, if you're not doing causal inference, you're not doing anything...but to say, "if you're not doing that, you're not doing anything," is a gigantic error

MP: Okay, I have two more questions for you. First, I think there has been an increasing effort for academics to engage as public intellectuals, or at the very least, share more of our research in a way that's accessible— with the public, with policymakers, etc. How effective do you think this has been, and what reflections do you have on the role of the academic in society, the role of political scientist in a *political* society?

DL: My views on this have evolved over my career. In my early career, I felt that the policy spectrum—that was, legitimate views of, let's say, foreign policy were so narrow that you were called a communist or fascist if you moved in any direction away from this policy consensus. My argument then was we should be like Shakespearean fools. We should be allowed to say things which are considered not serious by the policy community. Tenure allowed us to be able to say things which you couldn't say if you were serious about wanting to be Secretary of State.

After 9/11, my views began to change. No longer did the Cold War delimit political views, but I sensed that the government had no idea how to handle this new threat of Islamic jihadists and the terror they were perpetrating. I joined a Blue Ribbon Commission run out of the CIA, although we were all using open source data, to think of better measures of terrorist acts so we we would be able better to understand global patterns. We couldn't understand global patterns until we agreed what a terrorist act was.

I found myself years later worrying about what's the best policy for citizenship for green card holders? Entering into the center of policy ideas. And that's a big change for me. I think it was the shock of 9/11 where I felt I could be useful to the U.S. government, not in sending people to jail, but in thinking about how best to deal with a new threat.

So one more point here is that now we're living in a world where the President couldn't care less about what we think. And I feel that's a loss for us. Thank God at the state level we're finding many, many officials that are willing to work with us to address fundamental public policy question, and there's some degree of relevance for what we do. So, I'm reporting on a big change in how I think about the role of academics and public policy.

MP: And finally, do you have any hopes for the future of the discipline, or advice for graduate students who are about to start careers as comparativists today?

DL: Something I tell my graduate students, and they've taken that advice very seriously, and that is: don't listen to Laitin. Find what you think is before you in the world that needs explanation. And I'll work with you the best I can to see that you can fulfill your own ambitions in providing an explanation.

MP: Thank you so much. This is a tremendous privilege and I feel privileged myself to have been able to work with you and interact and learn from you for such a long time. So thank you, and thank you from all of our students and colleagues.

DL: Very welcome. Thank you for listening to me.

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OF THE AMERICAN POLITICAL SCIENCE ASSOCIATION

RESEARCH IS *ME* SEARCH: MARC BEISSINGER

Interviewed by Perry Carter, Postdoctoral Research Associate at New York University - Abu Dhabi

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Perry Carter (PC): To start with, it would be great if you could tell me about how you actually got into political science in the first place, maybe even going back to before you went to grad school.

Mark Beissinger (MB): I guess I was always interested in politics, even when I was in high school. I think my decision to major in political science when I went off to college was probably already decided, before I went. And I also already had an interest in Russia, although I really couldn't do too much about it. I don't have any Russian background, I didn't have any Russian language. So when I went off to college, I started to take courses on what was then Soviet politics, and history of communism and socialism, and I just really got hooked on it. I continued on and took Russian classes, where I learned it pretty well in terms of reading. But I couldn't speak very well, because you didn't have too much opportunity for practice in those days. I learned it like Latin rather than as a spoken language. When I went to the Soviet Union for the first time in 1976, no one could understand me, because I wasn't putting the accent on any particular syllable, since no one had taught me about that. So I had to learn Russian all over again. But my interest in political science was really something that was in my family upbringing.

> *I'm a methodological agnostic in the sense that I think questions are more important and that questions should lead you to the methods. Asking the right question and figuring out how to unpack it and selecting the methods from there is the way I've always approached it*

PC: And did you have a clear topic in mind when you started graduate school, or did you just come in with a general interest in the Soviet Union?

MB: You know, I had a topic, but it was not a good topic. The topic was about the politics of consumerism in the Soviet Union, but I never did pursue it. And, really, it's unnecessary when people apply to graduate school [to have a clear topic]. It's more about having an interest and showing the intellectual curiosity and ability than it is about having a dissertation topic in mind.

PC: What made you realize that your topic was kind of a dead end, and what put you onto working on what you eventually wrote your dissertation on?

MB: Oh, I didn't see any traction in it, I didn't see any real conflict or politics in it. It sounded like a topic rather than a question. And so eventually —actually pretty quickly— that was abandoned. My dissertation topic came to me through my course work. Thane Gustafson, who was then teaching Soviet politics [at Harvard], had a graduate seminar on Soviet politics where we learned how to read a Pravda editorial. The trick was that you go all the way down to the word "however" and you start there, skipping everything else before that. He was interested in particular in how bureaucracy and technology were altering Soviet administrative practice.

All of that eventually got me to my dissertation topic, which was around the theme of convergence. It focused on Soviet efforts to borrow Western management ideas. When I went to the Soviet Union to do my dissertation research, there were 40 Americans who were allowed to go to the Soviet Union every year on an official exchange. There were maybe 5 or 6 political scientists there. I went and I trained with Soviet managers for 3 months in a management training program. Of course, as an outsider in Soviet society—and my Russian was good, but it wasn't great—I can't say I was fully integrated into that program, but I did observe them dealing with the [Western] theories about administration. And then when I wrote that up as a book, I seriously revised it, made it a bit more historical. Though it drew from theories of bureaucracy, it wasn't really so oriented towards the discipline of political science, I have to say. Historians still use the book, but I think political scientists never really paid it much attention, and perhaps rightfully so. My real foray into political science happened later, as I started to work on Soviet nationalities issues.

PC: And how did you come to that from first researching Soviet management?

MB: Completely by accident! I had a friend who wanted to organize a lecture series on nationalities issues in the Soviet Union. This was before they had become fashionable. In fact, at that time, focusing on nationalities issues was considered the kiss of death in political science, because everything was Russian, everything was focused on Moscow. There was no inkling that there was a society [out there] beyond that. So I agreed to help him out. We brought in pretty much all of the leading lights at the time on Soviet nationalities issues in that lecture series. That was my education on these issues. And then, as protests began in the Soviet Union around early 1988, I decided I wanted to study them. I knew nothing at the time about social movement theory or nationalism and had very little statistical background. I ended up going to ICPSR in the summer when I was an assistant professor, living with the football players in the dorm there while learning statistics.

PC: Did you feel that knowing those methods was expected in the same way it is now? Or was that more your decision, something you felt would be useful to you for what you were trying to study?

MB: It was more my decision. I just felt these methods were useful for me in what I was looking at, because I wanted to look systematically at patterns of protest. It eventually took me to more sophisticated methods. People who worked in these areas told me, you know, you really can't do an OLS regression on this stuff. My original foray was just wrong. So I self-corrected and got there in the end. But it was because I felt like there was a need for the method, and that's kind of been my approach to methods in general. I am a multi-method person, I've worked with students ranging from ethnography to very sophisticated quantitative analysis, experiments, and so on. I'm a methodological agnostic in the sense that I think questions are more important and that questions should lead you to the methods. Asking the right question and figuring out how to unpack it and selecting the methods from there is the way I've always approached it.

PC: On that note, one of the challenges of being a multi-methods researcher is that, whereas you can go and read a textbook or take a class [to learn statistics], it's much less clear how you become a good interviewer or a good archival researcher. Did anyone teach you that, or was that just learning on the job?

MB: It was mainly learning on the job. I actually don't think a lot of that can be taught. You can take a class in it and you can learn some tips from other people, but you basically learn by what works and what doesn't. The best ethnographers are able to empathize with their subjects but still remain somewhat independent of them. Any kind of research always requires stepping back a little bit from the empirics of the work and thinking about what it means and sometimes even reframing it. So, the whole idea, for instance, that you go in with your theory and hypotheses all prepared and that you're going to confirm or deny those hypotheses: yeah, that's a great start. But the real creative theorizing often happens once you've gone out into the field and thought about what you've seen, what it means, and how it relates to a bigger question.

PC: Relatedly, could you reflect on the current research climate in terms of expectations of pre-analysis plans and an increasing movement towards more rigidly following a scientific approach where we do very explicitly commit to having certain hypotheses and methods before we ever hit the data?

MB: Yeah, Perry, as I think you know, I'm not a huge fan of that. Once you start putting these kinds of stringent requirements on things, the kinds of questions that you can ask are relatively narrow. Now, I think experiments definitely have their place in political science. They help us adjudicate between different possible mechanisms once we know what we're looking at. But to some extent, it's gone overboard. Like most things in social science, there are fads that come in, and then they become reified in ways that maybe aren't the most productive. So, experiments are fine, they just need to be put in their place. I would just warn about not asking the big questions and thinking about the big things. Some questions, you just can't do experiments on. I mention this in my book on revolutions: you can't subject populations randomly to the treatment of a revolution. I mean, it's just not something that you can do at the societal or the individual level. If we require everything to be so methodologically pure, we would never really get any creative thought.

PC: If you had a time machine and the ability to go back and do your research and write "Nationalist Mobilization" again with the benefit of everything you've learned and all of the advances in in the field, do you have regrets you would correct? Are there things that you wish that you could have done, or that you would do differently?

MB: Well, you know very well that I ask that question of every dissertator when they come up for their final dissertation defense. So, turning it back is fair play! Any project is a journey. You end up in a place where you didn't begin, and you discover things and adjust along the way ... "Nationalist Mobilization" just began as a project about protest in the Soviet Union as it was beginning. It mushroomed and got so large and the ideas of the project evolved. All of the protests analyzed in the book were coded by hand. So if I had a time machine, and I could bring the textual analysis methods that we have now, that would have been an enormous help.

What really affected me theoretically in that project was reading social theory, which had a huge effect on my thinking, especially the structure/agency problem, reading Giddens, and books surrounding him. I used that to frame the book and the ways in which we think about big historical events. I wanted to loosen the strictures of structural determinism in how we think about historical development, to think about ... the relationship between agency and structure in a way that was a little different than how others had, in an eventful way. If anything, there's too much social theory at the beginning of the book that maybe inhibits people from diving into it!

PC: But there's no experiment you wish you could have run?

MB: No. Things move so fast in a period of tectonic change like that, so experiments would be very difficult to mount. I'm not even sure that a survey experiment would have told me what I was interested in, and I doubt I could have done a lab-in-the-field experiment. Things were changing so fast. The ground underneath us was shifting.

PC: So that book, of course, in many ways has defined your career, but it also coincided with the actual collapse of the Soviet Union, and one of the most difficult things any researcher can go through: the disappearance of the thing that you studied. I wonder if you could speak about what that felt like and how you went about finding a new anchor.

MB: Yeah, well, my dissertation, as you noted, is not what made my career. It was the second book that I wrote. In many ways, there's a lot of serendipity in a career. I finished my dissertation and wrote it up and published it around the time that momentous changes were beginning in the Soviet Union, and therefore had the luxury of defining a new topic at the right moment. I saw many people in the field fall by the wayside because they couldn't make the adjustment to the changes that happened in the Soviet Union and the disappearance of the object they were studying.

If I had only written about Soviet bureaucracy, I know my career wouldn't have been the same as it was. When things change like that, it has a huge impact on a field, and you can see it in other areas of the world as well -- the Middle East, for instance. What typically happens then is it draws in a whole new group of people into the area, which is incredibly helpful. Some of them have no area expertise whatsoever but bring a social science sensitivity to the area. That was true with the collapse of the Soviet Union. People like David Laitin or Rogers Brubaker enriched the field in many ways by enhancing the orientation towards social science theorization and posing questions that the area scholars didn't pose.

PC: When I asked about how you got into political science, your story was very much that of an area studies person. Although obviously that's something that's endured throughout your career, you're less so now than you were before. Was there a point when you came to realize that you were a political scientist first and an area studies person, second?

MB: I think I didn't become a real social scientist until I left Harvard and went to Wisconsin, where I started interacting with Crawford Young and a number of other people and started thinking a bit more globally. Crawford had his focus on Africa, but also had an encyclopedic knowledge of much of the world. We became close friends and colleagues. We both had graduate students who, after the collapse of the Soviet Union, saw that there were some similar problems associated with the breakdown of the state in both regions of the world. The students brought us together to focus on some of the significant similarities and differences in the 1990s in Africa and the Soviet Union and we ended up producing an edited volume. So that was part of my evolution towards being more of a comparativist. My book on the Soviet collapse also brought me into contact with a wider swathe of people working on ethnicity and nationalism in different parts of the world, as well as scholars of protest and social movements like Chuck Tilly and Sid Tarrow. They also exercised a huge influence on me, pushing me in a more comparative direction.

PC: On the other hand, one of the first things that you spoke about was your experience learning Russian. Of course, it's now relatively easy for people to work in contexts where they have zero language skills through the use of automated translation. How important has that kind of linguistic and contextual knowledge been to you, and how important do you think it is for people now moving forward, despite all those technological aids?

MB: I think it's important to be grounded in a place, even as you think more broadly as a comparativist, and even if you're doing global work. My knowledge of having lived in the former Soviet Union and visited places among the post-Soviet states informs almost everything that I do in terms of comparison. It's not like I'm always looking through the lens of that at everything else, but it provides an anchor point. I think it's critical to get some prolonged exposure, to have a sense of what [a place] is about. And if you go in as an outsider, I think you really don't understand what it is you're looking at. In the case of Russia, for instance, there's so much informality in Russian society and politics that, as an outsider, you wouldn't necessarily understand what's going on.

PC: We've mostly spoken about your research career so far. I wonder if you could sum up the most important things that you feel that you've learned about the rest of the job, especially service and administrative roles.

MB: Well, I did a lot of service throughout my career. I think about half of my career was in different service positions. The most challenging of the positions was department chair, because then you're dealing with all the personnel issues in the department. I don't want to advise people to avoid that position. Somebody has to do it. But, you know, I think... 3 years of duty is plenty.

Overall, though, I found service enriched me. It brought me into contact with people across the university that I wouldn't otherwise have made contact with. Most people run away from service as much as possible. And there's some rationality to that. But, I think it can enrich you; you can do service and have a rich research career as well.

PC: We've already touched on this a little bit, but I wonder if you could reflect on what are the best and the worst developments that you've seen over the course of your career?

MB: The worst development for the discipline... I think there's a lot of bureaucratization in the discipline, a lot of excessive formalization of things. And, that's, of course often in response to particular issues that are there. But it also has its downsides. We do have to watch out for the impingement of external goals on the academic enterprise, but also from the federal government as well. Especially today. Those sorts of things can have deleterious effects.

The most positive thing? The whole discipline has altered. When I was in graduate school, I don't think theory was emphasized as much. That has transformed the discipline in a big way. Even someone like Barrington Moore, who was in sociology, but also in political science, was kind of on the fringes of his disciplines, even though he's considered now an icon of social science. I don't think you'd find Barrington Moore on the fringes of political science if he were alive and writing today. So we've become more ambitious, theoretically, which is great. Methodologically, we've also become much more sophisticated. That is perhaps the biggest change. And much more rigorous. But maybe a little too so. And this tension between theorization and methodological rigor is driving a lot within the discipline today.

PC: You mentioned fads, and I'm reminded of the Russian saying that everything new is just something old that's been well forgotten. Are there any things that you wish people would remember from the past that don't get talked about anymore?

MB: I think this is a common complaint: our graduate education tends to focus on works that were produced within the last 10 years. Or 5 years! And we tend to forget anything that was published before that. Often, I find myself saying, "hey, you know, so-and-so wrote about that earlier", which is maybe not so good for the accumulation of knowledge.

The other thing is that I think graduate education these days tends to be much narrower and too focused on method. People need to read more widely. They need to read Marx, and Weber, and Durkheim and lots of other things besides. Those are just the basics. I don't think students read widely enough and graduate education narrows them excessively on method. Often, the question gets defined in terms of method. It would be good to think more about the question first and methods second.

PC: If you had to pick the key areas in comparative politics where either you're excited about new developments, or you think there are kind of holes in our knowledge that need to be filled, what would those be?

MB: There's a lot of interesting stuff taking place in political psychology. I often rely on that when I think about the micro-dynamics of things, and often go to other disciplines as well. I don't confine myself necessarily to political science. I think it's a good idea to look outside the discipline and think more broadly about social science.

There's already been so much written on de-democratization and democratic backsliding, and by now it's already -- I don't want to say passe, because we're living through it. But, in terms of scholarship, I think we need to focus now on how to reestablish democracy once you've gone down that path. That is going to be the critical question confronting us, both in the United States and many other societies.

Right now, historical political economy is a big area. It tends to be very empirical and focused. I really like the looking back on history and thinking about the present through the eyes of history. There's a lot of rich stuff that's coming out of there.

By contrast, almost everything in political science today has to be a survey. A lot of that is to get down to micro-mechanisms and what's happening at the individual level. And that's great, but I think it's really important to try to think beyond that, because surveys can only capture so much. We need methodological diversification.

I think political scientists confine themselves too much to political science...Look across [other] disciplines, at people who are addressing similar questions that you're investigating... And you'll learn a lot from it

The discipline has a bit of a survey fetish, much as historians have a bit of an archive fetish. That is, something doesn't exist for historians unless it's in the archive. Political scientists have started to become a little bit like that with surveys. I think it's important that the discipline pull back and think a bit more broadly.

PC: Finally, what advice would you give to junior scholars starting out now?

MB: Well, first of all, being a comparativist is the hardest task within political science. No other field is as demanding. No other field requires as much knowledge of area, methods, and theory. One can aspire to trying to integrate all of them, but you have to make choices. One thing I would say is what you do in the early part of your career is not necessarily what you'll do later on. There are certain things you have to do. Places you have to publish, types of things that people are expecting you to address. Once you have tenure, you have freedom to think more broadly, to become more ambitious. And so I think it's a good idea to think about your career in two parts. At least, that's the way it worked for me. There is the early part of your career where you are satisfying others. And the middle and latter part of your career, where you're satisfying yourself intellectually. Where you pick the questions that you want to research, and the ways in which you want to do it, with the full creativity that you can bring to it.

Also, you don't need to be bound by the discipline. I think political scientists confine themselves too much to political science. They don't look to sociology or history or economics. Some do. But look broadly. Look across all those disciplines, at people who are addressing similar questions that you're investigating, at what they say and how they've attacked them. And you'll learn a lot from it. Once you've gotten tenure, you will have gotten a great experiential education from all the things that you've done and learned. But it's only the beginning of your career. You can redefine yourself, or define yourself in a more ambitious direction once you have a sense of stability.

PC: Lastly, I'd like to give you a chance to end on a high note. What are you most proud of that you've achieved in your career?

MB: Probably the books that I've written that have won awards. I'm also very proud of the students that I've worked with over the course of my career, and the what I've contributed to them. But I think that I'm probably most proud of my research and writing that others find useful and interesting.

PC: Thank you so much, Mark. It's been great talking to you.

CURRENT AFFAIRS: STEVEN LEVITSKY

Interviewed by Gabriel Koehler-Derrick, Assistant Professor of Political Science at New York University - Abu Dhabi

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Gabriel Koehler-Derrick (GKD): I wanted to start by asking you whether you thought that professors had a particular obligation to speak up during periods of political turmoil.

Steven Levitsky (SL): I'm not sure I have a clear answer on that. My instinct is to be really cautious. I think that citizens have an obligation to speak up. One of the sources of the great crisis of democracy we're facing in the United States right now is that far too few citizens, particularly in the elite, have spoken up when they when they might have. So I do think it's important as citizens to speak up. But I worry a lot about professors putting on a Superman outfit and politicizing their teaching. It's very easy to say yes to your question, but I think there are real costs. So yes, those of us who study democracy, I think should be part of the public discussion about the state of our democracy, both as citizens and as experts. I feel an obligation to speak out publicly. But I'm much more cautious about how I bring that to the classroom.

... the principal cleavage in American politics, as in much of the advanced democratic world, is what I would call cosmopolitan, ethno-nationalist... For the first time in any of our lifetimes, universities are on one side of that cleavage

GKD: Did your experience with these distinct approaches from your mentors shape how you think about a professor's obligation to either their students in the classroom or to graduate students in a doctoral program?

SL: Absolutely. When I was a college student, my undergraduate advisor was my role model. I idolized her, and I really wanted to be, this isn't a fair term because she's a super serious scholar, but kind of an activist scholar. That's when I was twenty years old. But my perspective evolved a lot in grad school. I learned a tremendous amount from my graduate advisors about the need to maintain credibility, the need to be taken seriously by a wide range of members of the academic community and particularly what we owe our students in terms of not imposing our ideas on them, letting them grow and develop their own ideas free from from our position. So I learned and was inspired by both advisors, and ended up in a distinct place entirely, but heavily influenced by both.

GKD: This is a nice sort of transition because my recollection is somewhere around 2017 you pivoted in a big way towards a kind of public intellectual role. My recollection, although I could be wrong here, is that that was, at least in part inspired by an interaction with a student and perhaps even in your introductory class to Comparative Politics. Could you talk about that experience?

SL: Let me take a couple of steps back. It's not entirely true that I made a conscious decision to jump into a more public role in 2017. After I got tenure about a decade prior, I went through a kind of a medium sized—I don't know if it was a midlife crisis—but a sort of "what do I do now? What am I here on this earth for?" period. And decided that I that there there were important contributions that I wanted to make that were not just publishing books that five hundred people read.

So I wrote a newspaper column in Peru for five years, from 2011 to 2016, which was a pretty interesting experience: a big time sink, but a very interesting experience that flowed out of wanting to to do more than remain in the Ivory Tower. The leap that I made in terms of discussions of American politics after Trump's election was pretty accidental. It was not a self-conscious decision at all. But the first step was in fact, a student in my Foundations of Comparative Politics class, pretty big class, a couple hundred students. They, as most Ivy League students, were blown away, really shocked and dismayed after Donald Trump's election. Trump was elected on a Tuesday and I lecture on Tuesday and Thursday. So on Thursday I lectured I don't even remember well the logic behind it, but I decided not to say anything. I think I either made a one sentence reference to the election, or maybe said nothing at all. I may have felt like I just hadn't sorted out my thoughts and didn't have anything to say. I try to be careful not to vomit my own political views in the classroom, but I've never been super strict about that either. I don't think I felt inhibited, but for some reason, I decided not to to say much of anything.

Afterwards, a student came up to me, a Venezuelan-American student, and tore into me for not having addressed, in this big introductory politics course, the biggest issue of the day and the biggest political event of probably their lifetimes. And, wow, that hurt. That kind of stunned me. So I went back and I wrote a twenty minute mini lecture that I gave the following week, and that turned out to be the kernel of an op-ed in the New York Times that Daniel Ziblatt and I wrote, which in turn was the start of How Democracies Die.

So it was only being pushed by that undergrad to say something intelligible and provide some insight to my students that I got dragged into this much more public role. Even after the New York Times op-ed, Daniel and I didn't think about a book at all. It didn't even cross our minds until a book agent read the New York Times piece and wrote Daniel and said, you guys have to turn this into a book. And so this agent, who was brilliant, is the one who really invented the book.

I had wanted to take on a more public role. I didn't really know how. I didn't because I'm a Latin Americanist. I never really thought I would do much on the United States, but after after a little pushing and shoving, I realized, wow, this is an opportunity to to speak and maybe be heard more than I usually am.

GKD: Did it feel like an opportunity or also a bit of an obligation?

SL: Obligation is maybe a little strong, but during that period, Daniel and I were obviously really worried about the fate of US democracy. I think we were a little more worried more quickly than many of our peers. And I'll speak for myself, I didn't know what to do with that concern. So writing that book and beginning to give interviews and speak publicly was my way of doing something. It was my way of exercising citizenship in defense of democracy. Sounds a little bit too high minded to call it an obligation, but it seemed like a useful thing that I could do.

GKD: After a decade plus in this different role, are things that you've learned that were particularly valuable or instructive, or that you wish you'd known before jumping in with both feet?

SL: Things I've learned along the way? Well, one one kind of small thing is, boy, did I not know how to do media interviews, particularly television, when I started. That's actually a set of skills that one doesn't just roll out of bed and employ. I wish I knew more about how to do an interview when I did my first one hundred and fifty interviews. Dan and I, we had the same publisher for the last two books that we wrote, and as soon as we signed the contract for the second book, the publisher insisted that we get media training, which says something about our media performance after the first book! So I would have liked to know how to do an interview.

Time has gone by so quickly and I'm still learning how to have an impact. I'm still learning how to measure the broader impact of what I say and write. When I teach students in a classroom there are lots of ways to evaluate whether what I'm saying is getting through. We obviously live in a very politically segregated environment here, at least in the United States. And I do a lot of public speaking, and I sometimes am able to speak to pretty big audiences, but they are strikingly homogeneous in their their partisanship and broadly, their political views. It's a lot of work for me to get myself in an audience of more skeptical citizens, people who come from a very different social, political background. I think going back I would have spent more time thinking about how to engage with and reach those audiences. That's a realization I came to kind of embarrassingly late.

GKD: As a result of this experience, do you have suggestions or guidance that you would give to colleagues or peers who are similarly interested in how they can bring relevant work into the public discourse, especially from comparative politics?

SL: Many of my colleagues are now doing this in various ways. Trump's election has created many more venues for scholars of comparative politics to be heard and listened to in the United States. Americanists realized that they had some catching up to do in thinking about political regimes, and populism.

Also, the communication landscape has changed. It's so much easier for scholars to reach broader public audiences. Earlier in the century, you saw the emergence of journalists and news websites who positioned themselves as translators of academic work into the public realm. I think of Ezra Klein and Vox and sites like that. Now with Substack and with YouTube, academics are able to directly reach the public.

The hard thing, which I think a lot of us don't appreciate enough, is that it takes a lot of translating work to take the ideas that come out of our research and communicate them to non-academics in a way that's meaningful and interesting. On one level, that's an obvious statement. There's obviously data that need to be presented in more comprehensible ways. Obviously, there's certain jargon that we that we drop when we're not with our Comparative Politics peers. But if you really want to reach people you got to put in a lot of work, a lot of into thinking about how to communicate.

I should also say we got extraordinary editing help from our publisher, and they really retaught us how to write for that book. But that experience reinforced this notion for me that it takes a lot of work to bring sophisticated ideas from political science and reach people. I would not say that I've figured it out, but I've come to appreciate the amount of time and work that takes. And that's an investment that takes us away from other things. So I look around and I read Substacks and listen to podcasts by academics that are really very good. And I'm struck by the change over the last decade in terms of the number of top academics that are doing high quality communication to the public. That was not true when I started in this business.

GKD: Changing direction, we're experiencing pretty unprecedented times in American academia. Do you think there are periods from the past that are helpful or instructive for understanding the current political moment?

SL: That's a great question. This is something I'm trying to learn about. I'm part of a study group at APSA that is tackling this very question. We sometimes forget that, at least in the United States, the kind of civil liberties that many of us care about now and want to defend and worry are being lost, that kind of academic freedom is all pretty recent. It's really post-World War II. Go back to any other era in in US history, and I suspect most of the democratic world, and professors didn't have the kind of space that we expect to have now.

So obviously there are lessons from the McCarthy period, but what is relatively new, and what is certainly different from any period post-World War two in the United States, certainly any period of my lifetime, is that the principal cleavage in American politics, as in much of the advanced democratic world, is what I would call cosmopolitan, ethno-nationalist. You have an urban, predominantly secular, college educated, broadly liberal group on the one hand, and you have a largely more rural, small town, more religious, generally whiter and less college educated camp.

That cleavage is not the same as left and right. We sometimes call it left and right, but it's a dramatic change in our political competition. For the first time in any of our lifetimes, universities are on one side of that cleavage. Universities in most places, in most of modern history have leaned left / liberal. They tend to house dissent, but always in the United States, universities have been able to keep a foot in both major parties. There may have been fewer conservatives than liberals, but there were always Reagan Republicans at places like Harvard and Yale and Princeton.

This new cleavage, call it cosmopolitan / populist, or cosmopolitan / ethnonationalist means that universities are virtually single party. There been a little bit of a resurgence of Trumpism here on on Harvard's campus, but because the other side, politically is increasingly anti-science, universities are never going to house a lot of folks who are anti-science and that makes universities politically vulnerable in a way they never were before.

Harvard has always produced Democratic and Republican elites. It has always produced liberal and conservative elites. Harvard has always been embedded in and protected by the liberal and conservative establishments. That's no longer true. Now we are really of one party. There's still lots of people who are right of center at Harvard, but the vast majority of them are Never Trumpers, and that's true of other elite universities. So there is a vulnerability when we have no protection, no support, no constituencies among one of two major political parties. That leaves us extremely vulnerable.

Now, Republicans aren't always going to behave as aggressively and in as authoritarian a way as Trump has. But the way the political cleavage is structured today in the United States, we are on the other side of the political divide from from one of the major parties. That's a source of extreme political vulnerability. You see similar things happening in different kinds of party systems. So a little bit different, but similar dynamics in Western Europe as well. It's worrisome. And that that does seem to be pretty new. I don't know enough to call it unprecedented. I'm sure it's not unprecedented, but it's pretty new in the modern era.

GKD: As someone who has studied Latin America for such a long time and who has done so much to advance our understanding of political institutions outside of the US, can you think of particular moments or challenges to academia, either in Latin America or in other settings that you've studied or think are relevant to the challenges facing American academic institutions today?

SL: In Latin America, obviously there have been some very repressive regimes, either military dictatorships, or revolutionary communist regimes, like in Cuba, that have assaulted universities. And to some extent Pinochet or Videla, in South America, either completely subordinated the universities or silenced them. But two things make Latin America different. First of all, there is a tradition of university autonomy that is quite strong, even in fairly authoritarian regimes. Fujimori in Peru didn't really violate the autonomy of the universities. This is also true of other authoritarian regime. For many years in Mexico, there was a strange underutilization of power by even some authoritarian regimes.

But most importantly, higher education plays a much smaller role, socially, culturally, and politically in Latin America than in the United States. In the United States, higher education is a huge part of our economy. It helped make the United States a superpower. It's the source of a tremendous amount of revenue and it helps to produce a pretty broad elite. Latin America universities, until very recently, educated a much smaller stratum of society and they didn't do the kind of cutting edge research that drove national greatness or the economy. And so the stakes were just much lower in going in and storming a university or or removing the president of university. In the US you're talking about a major jewel and a centerpiece of the whole post-World War II American project. And you just can't say that about about any country in Latin America.

I don't know much about higher education in Germany, but I think the better comparison is probably what the Nazis did to higher education in Germany, where you had a similarly important set of higher education institutions that was destroyed by an authoritarian regime—obviously much more far reaching than the United States —in a way that undermined national power amd really weakened the country in important ways.

GKD: Lastly, could you talk about how you think universities in general have responded to increased pressure from the Trump administration? Do you have thoughts about why universities seem to have adopted such different policy responses to similar pressures?

SL: Well universities are just like the government of Canada and the government of Denmark and the government of Mexico and businesses across the country, and major media and law firms, all of which have been operating under this reality in which the the US federal government has extraordinary levers of power, that it never uses, because it's in everybody's best interest that the United States not bully Canada and not bully Mexico and not bully universities and not bully the private sector. In some cases there are legal protections against the these attacks. In other cases, it's just more tradition and enlightened self-interest. But universities, under the old rules of the game, obviously felt safe. But if any government decides to abandon these self-imposed constraints they can do a lot of damage.

It's probably too much to ask the president of Harvard to think about democracy when he's making his decisions. But...there is a collective cost that we are now paying for not thinking about democracy in the long term in our decisions

Point is, we were not expecting it. Universities did not expect the government to use every tool in the toolbox, some legal, some illegal, to bring universities to heel, to threaten, to punish, to weaken universities. It just it seemed like an act of national suicide: why would any government do that? We were taken by surprise. I was in meetings with Harvard University leadership after Trump got elected, but before he took office, when they were planning for best and worst case scenarios. And the worst case scenario was Congress passing was raising the endowment tax. That was the worst case scenario.

None of the things that happened to us—threatening to take away Harvard's right to host international students, freezing two and a half billion dollars worth of funding—not on the table, not discussed, not expected. So we were all taken by surprise. Which is why there was a there was a real deer-in-the-headlights response, I think by most university leaders, in general.

I'm writing a book about this, and I have been disappointed by the the way that university leaders have responded to the bullying of the Trump administration. But it's important to say that universities are no different from CEOs. They're no different from law firms. They're no different from media companies or foundations. Our elite, really across the board has responded without much courage. They've responded very pragmatically, and in my view, in a very short sighted way, which is to appease, to make concessions in the face of often illegal and authoritarian bullying in the hope that'll get us out of the crosshairs and the problem will go away.

That is not how you defend a democracy, in my opinion, but it's also a problem of collective action. It's very hard to ask the leadership of Columbia University, or the leadership of Brown University, or the leadership of Harvard to stand up and and take a hit for the good of democracy. We don't have very strong associations in the United States: like strong business associations, there are no associations that bring together university leadership, or law firms, or media companies. So individual organizations, be they private sector firms, or unions, or universities or law firms, had to respond as individuals: they were not able to think collectively about the long term. It's probably too much to ask the president of Harvard to think about democracy when he's making his decisions. But there is a cost. There is a collective cost that we are now paying for not thinking about democracy in the long term in our decisions. So I think we would have been much better off had law firms, CEOs, churches, unions, media companies, foundations, and universities stood up and tried, hard as it is, to forge collective responses to to the Trump administration's bullying.

INTRODUCTION

by J. Andrew Harris and Gabriel Koehler-Derrick

American Academia is facing a number of pressing challenges: declining public trust in universities, highly politicized debates about the role and extent of campus protests, and a series of reversals by the Trump administration of support for research across a wide range of scientific domains. These challenges play out against a global shift towards increasingly authoritarian tendencies. How should professors of comparative politics respond to these developments in the classroom? While there is no 'one size fits all' answer to this question, we solicited contributions from colleagues at institutions across a range of different countries willing to share their recent experiences about teaching during politically turbulent times. Our hope is that their insights help facilitate ongoing discussions across our subfield: how much of what is occurring in the US today is 'unprecedented'? How might academics productively and collectively respond to rising autocracy and similar threats to democracy? Do attacks on higher education in other countries provide insights relevant to the US today? How can we support colleagues whose research agenda is under attack for political motives?

TEACHING DEMOCRACY IN REAL TIME: LESSONS FROM SOUTH KOREA'S SELF-COUP CRISIS

by Nam Kyu Kim , Seongjo Kim , and Jaemook Lee

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On the night of December 3, 2024, President Yoon Suk Yeol appeared on national television and declared emergency martial law across South Korea. For a country long celebrated as one of the most successful cases of democratic consolidation in the third wave of democratization, the announcement was almost impossible to believe. Nearly four decades had passed since military rule, and in that time South Korea had become a successful democracy characterized by peaceful transfers of power, robust civil society, and strong civilian control over the armed forces. Martial law had become a relic of history, something students encountered only in textbooks about a distant authoritarian past.

> *[W]hen troops were deployed and the president invoked emergency powers against his political opponents, students struggled to make sense of what they were witnessing...The shock delivered a powerful message: democracy is not a given*

In the weeks before December 3rd, rumors of martial law had circulated, but few took them seriously. The notion seemed too absurd, too anachronistic to warrant genuine concern. Yet when those whispers hardened into reality and soldiers were deployed against political rivals and opposition voices, the familiar suddenly became strange. For a generation that had only known a secure democracy, this was their first visceral encounter with democratic fragility, transforming the crisis into an unexpected yet powerful occasion for learning about democratic resilience and the necessity of accountability. This essay explores how this crisis, despite its gravity, became an unexpected yet powerful occasion for learning about democratic resilience and the necessity of accountability.

When the Unthinkable Became Real

The images that flooded social media and news broadcasts that night possessed an almost surreal quality. Military helicopters landing at the athletic field behind the National Assembly building. Soldiers smashing windows of the National Assembly building to gain entry. Lawmakers scaling walls and climbing through barriers to reach the chamber in time to vote. Each image carried pedagogical weight, transforming abstract concepts into concrete, undeniable reality.

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For those of us who teach Korean politics and democratic institutions, these visuals were deeply unsettling. Our students had grown up in a society where elections, the rule of law, and the peaceful contestation of power were simply the normal way politics worked. They illustrate precisely what Juan Linz and Alfred Stepan meant when they described democratic consolidation as a condition in which democracy becomes "the only game in town"—not merely the formal rules, but the unquestioned assumptions about how political competition must unfold. Behaviorally, no major actor seeks to overturn democracy by force. Attitudinally, the public expects democratic procedures to resolve conflict. Constitutionally, all players operate within the law and accept its constraints. Our students had been raised under all three conditions.

So when troops were deployed and the president invoked emergency powers against his political opponents, students struggled to make sense of what they were witnessing. This was not how the game was supposed to be played. Their disbelief was not naïveté but rather a measure of how deeply democratic norms had taken root in a generation that assumed Korea's democratic achievements were secure and irreversible. The shock delivered a powerful message: democracy is not a given. In the classrooms that followed, initial reactions of disbelief gave way to anger, then fear, and finally to serious, probing questions: Why did this happen? What safeguards might prevent it from happening again.

What We Did: Inside and Outside the Classroom

In the days following December 3rd, political scientists had formed online coordination channels that quickly swelled to more than five hundred scholars. What began as frantic messages evolved into deliberation, drafting, and collective action. Within days, we produced a joint statement declaring the emergency decree unconstitutional, calling on the National Assembly to immediately introduce articles of impeachment against President Yoon, urging lawmakers across party lines to support them, and demanding a swift return to democratic normalcy. A total of 573 scholars signed the declaration, released through national media on December 8th. Similar statements followed in the days ahead until martial law was fully lifted. The boundary between studying democracy and defending it had entirely dissolved.

Beyond the academic community, universities around the country issued their own declarations. Faculty councils, student councils, and campus organizations spoke in unison against the suspension of democratic rule. At the same time, scholars organized public-facing seminars—first at Yonsei University, then Seoul National University, Korea University, and other campuses—exploring the political meaning of martial law, the nature of democratic crisis, and the paths to institutional recovery. These events were streamed online, allowing students from multiple universities to participate in real-world efforts to defend democracy. In short, confronted with an illegal and anachronistic assault on constitutional rule, Korea's political scientists mobilized every tool available to them, as teachers, researchers, and citizens, to defend both the practice and the principles of democracy.

Inside the classroom, we worked to provide students with historical context for understanding current events. We discussed theoretical frameworks for analyzing the illegal martial law declaration, including concepts like executive aggrandizement, democratic backsliding, and the distinction between coups and self-coups. Question-and-answer sessions addressed why this happened and what institutional reforms might prevent recurrence. Students debated proposals for constitutional change and measures to strengthen democratic resilience. What had been abstract theoretical categories suddenly had immediate, pressing relevance.

Outside the classroom, professors modeled civic engagement while teaching about it. The experiences of attending protests, signing declarations, and participating in public deliberation were then brought back into seminars for further discussion. Students witnessed their instructors not merely analyzing democracy from a scholarly distance but actively participating in its defense. This bridging of theory and practice may have been the most valuable lesson of all.

Democratic Resistance, Accountability, and Critical Reflections

The speed and decisiveness of democratic resistance was striking. Within six hours of the martial law declaration, 190 lawmakers had broken through the National Assembly blockade to pass a resolution demanding its lifting. Citizens rushed to the National Assembly where special forces had entered the building in an attempt to detain opposition lawmakers. They stood outside barricaded walls, facing police lines, chanting against martial law. Some military commanders refused to execute orders they recognized as illegal and unconstitutional. A broad consensus emerged almost immediately among citizens and elites that the president's actions violated the constitutional order.

In the weeks and months that followed, accountability proceeded with similar determination. Impeachment proceedings moved through the National Assembly. Those involved in the insurrection plot were investigated and detained. The Constitutional Court deliberated on the president's removal. Unlike the aftermath of the January 6th attack on the U.S. Capitol and France's February 6, 1934 crisis, where the failure to hold participants accountable contributed to democratic erosion in the years that followed, Korea demonstrated that democratic survival requires consequences for those who attempt to subvert the constitutional order.

In the aftermath of the crisis, students no longer treated democracy as a self-enforcing system guaranteed merely by elections and constitutional provisions. What emerged from their reflections was a recognition that democracy depends on the active commitment of political actors, not just constitutional text. Citizens, lawmakers, judges, and even soldiers who refused illegal orders had played essential roles. The constitutional order was preserved not simply by laws on paper but by the active will to enforce them.

This realization carried several implications. First, democracy does not operate automatically; it requires active defense. Second, without accountability, democracy weakens. Historical comparisons revealed the long-term consequences when violations go unpunished. Third, democratic citizenship demands more than periodic voting; it requires vigilance, engagement, and the willingness to mobilize when democracy is threatened, as emphasized by Weingast (1997). For several students, this experience marked a departure from political cynicism toward a more engaged civic sensibility, one in which democracy was encountered less as an inherited ideal than as a practical problem of maintenance and struggle.

The timing of these lessons was sharpened by broader cultural context. Only months earlier, novelist Han Kang had been awarded the Nobel Prize in Literature. Her work centered on democratic struggle and state violence in South Korea's authoritarian past. Students suddenly understood that the history her novels explored was not safely confined to the past but remained latent, capable of returning under the right conditions.

Yet even after the crisis was formally overcome, unresolved challenges remained visible. On one side, significant segments of the public continued to justify martial law or to circulate election-fraud narratives that had no basis in evidence. In the weeks that followed, on January 19, 2025, hundreds of the president's supporters stormed a court building after a judge extended his detention, overwhelming riot police, smashing windows, and destroying office equipment and furniture inside. These reactions revealed deep fractures in democratic consensus and in the epistemic foundations of legitimacy itself. On the other side, the new government established a task force to investigate civil servants suspected of involvement in the martial law declaration. Critics argued this measure violated constitutional warrant requirements by pressuring officials to submit their phones for examination. Meanwhile, the ruling Democratic Party pursued an ambitious judicial reform agenda, including expanding the Supreme Court from 14 to 26 justices (potentially allowing President Lee Jae Myung to appoint 22 of them), restructuring the judicial nomination process to increase political influence, and introducing a "trial appeal" system that would enable the Constitutional Court to overturn final court decisions—measures critics warned would undermine judicial independence and require constitutional amendment.

Students grappled with this tension directly. How far can a democracy go in defending itself without reproducing the logic of exclusion it seeks to resist? This is the central dilemma of what scholars call militant democracy or defensive democracy, the question of whether, and under what conditions, non-democratic means can be justified to protect democratic order. There are no easy answers. If accountability is too weak, those who attack democracy face no consequences and may try again. If accountability becomes punitive overreach, it risks undermining the very principles it claims to defend.

Conclusion: A Crisis Well-Managed as a Teaching Moment

What transforms a democratic crisis from mere catastrophe into a teaching moment? Several conditions allowed this particular episode to become an educational opportunity rather than simply a collective trauma. First, accountability proceeded swiftly and through constitutional channels, demonstrating that legal institutions could function even under extreme stress. Second, citizens mobilized actively and in large numbers, showing that democratic defense requires more than institutional machinery. Third, a critical mass of elites across political divides reached consensus that the president's actions were illegitimate, preventing the crisis from being normalized through partisan framing.

The lessons Korean students learned from this experience carry universal relevance for democracies worldwide. The contingent choices made that night—by citizens, lawmakers, and soldiers—underscored that democratic survival is not an automatic outcome of constitutional design but a fragile product of human agency and commitment. Democracy requires eternal vigilance; it cannot be taken for granted simply because it has endured for decades or because formal institutions appear robust. Violations of democratic order must have consequences. And democratic citizenship demands the willingness to show up when it matters, to speak out against illegality, and to defend constitutional principles when they come under attack.

Fortunately, the crisis was contained quickly, the constitutional order was preserved, and accountability has proceeded through proper legal channels. But fortune alone did not determine the outcome. Citizens chose to mobilize in the middle of the night. Lawmakers chose to resist by physically breaking through barricades. Soldiers chose to disobey orders they recognized as illegal. Scholars chose to speak collectively and organize public deliberation. Each of these choices mattered; each was contingent; each could have gone otherwise.

This is not to suggest that Korean democracy is perfect or that all challenges have been resolved. Deep polarization persists, conspiracy theories about electoral frauds continue to circulate, and the task of democratic maintenance continues with no clear endpoint in sight. But this particular moment of crisis was well-managed, and the experience of managing it has left students with a more demanding and more realistic understanding of what democracy requires. That understanding, carried forward into their lives as citizens, scholars, and professionals, may prove to be the most durable lesson of all.

TEACHING POLITICAL SCIENCE DURING POLITICALLY TURBULENT TIMES IN INDONESIA

by Amalinda Savirani

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In late August 2025, a wave of street demonstrations, largely led by young citizens, swept across Indonesia, including the capital, Jakarta. The protests began in mid-August in Pati Regency, where thousands of residents marched for days to oppose a drastic land-tax increase of up to 250 percent. Initially local in scope, these grievances quickly resonated nationwide as they intersected with broader economic anxieties, rising living costs, and widespread perceptions of political elite insensitivity.

In Jakarta, protests culminated in the death of a motorcycle taxi driver, Afan Kurniawan, who was struck by a police vehicle. His death further escalated public anger, including in Yogyakarta. On 31 August, university students and young people gathered in front of the local police headquarters to protest the incident. As tensions rose, Sultan Hamengkubuwono X, who serves both as King of Yogyakarta and regional governor, visited the protest site in an attempt to contain the crowd and de-escalate the situation. Shortly after his departure, however, confrontations intensified, culminating in the burning and partial destruction of the police headquarters. Police officers subsequently chased demonstrators and deployed tear gas, dispersing the protest. In the aftermath, security forces conducted sweeps in neighbourhoods with large student populations, reasserting control through surveillance and intimidation.

> *I never imagined that, almost three decades after the fall of Suharto, history would begin to repeat itself...I could no longer point to Indonesia...as a convincing example of democratic practice. This shift generated not only pedagogical discomfort, but also a deeper sense of democratic exhaustion and hopelessness*

On the morning of 3 September, I taught an "Introduction to Political Science" class, online through Zoom. Many of my students had participated in the demonstrations over the previous weekend. That week, Universitas Gadjah Mada, along with several other universities in Yogyakarta, shifted all classes online in response to security concerns for students. Before beginning the lecture, I invited students to share how they were feeling, using the chat function on Zoom. Most responses conveyed fear, anxiety, and confusion. Students living near the police headquarters reported hearing tear gas canisters throughout the night, which left many feeling frightened and unsafe. Several students explained that they usually display faculty stickers on their motorcycles to access campus parking, but fearing that such markers could identify them as university students, they had removed the stickers. I felt sorry for their fear.

The recent protests, together with the return of authoritarianism during the second term of Joko Widodo, or Jokowi (2019–2024), have compelled a profound pedagogical reflection. How does one teach democratic participation under conditions of increasing autocracy and pervasive undemocratic practices? How should democracy be taught when participation carries the risk of physical harm, and when the state itself has become a source of fear rather than protection? Faced with these questions, I found myself increasingly uncertain about the effectiveness and the ethical limits of conventional democratic pedagogy.

In my "Introduction to Political Science" class, students are introduced to democracy as a system grounded in equality, fair competition, and equal opportunity. Yet in Indonesia's current political context, this principle increasingly appears as a hollow normative claim rather than an empirical reality. This contradiction is exemplified by the political rise of Jokowi eldest son, Gibran Rakabuming Raka, despite his failure to meet the constitutional minimum age requirement of forty, in 2024 presidential election. He is the 'nepo baby'. The Constitutional Court reinterpreted eligibility rules to allow candidates under forty with prior elected office, to run for president or vice president. Gibran was then Mayor of Surakarta (2021–2024). The ruling, issued under the leadership of Chief Justice Anwar Usman, Jokowi's brother-in-law, underscores how democratic equality is undermined by elite intervention and conflicts of interest.

These developments, reinforced by the leadership style of Prabowo Subianto, which closely echoes that of his former father-in-law Suharto, transported me back to my undergraduate years under the New Order regime (1966-1998). At that time, studying Indonesian politics and democracy felt both futile, dispiriting, made you angry; and wanted to change it. Liberal democracy was a distant aspiration, civic space was virtually absent, and what prevailed was Pancasila Democracy: a hollow, formalistic ideal rather than a lived political reality. Elections were held routinely every five years, yet only three parties were permitted and outcomes were largely predetermined. Political participation was ritualistic rather than meaningful; politics was managed, not genuinely contested.

Our analytical tools then came largely from scholars such as William Liddle, Dwight King, and Harold Crouch, who documented how power was concentrated around Suharto through personal rule and the mobilisation of the bureaucracy and military as political machines. Karl D. Jackson's concept of the "bureaucratic polity" captured this configuration succinctly. Indonesian scholars such as Afan Gaffar, who analysed Javanese voting behaviour, and Mohtar Mas'oed, who examined the political economy of the New Order—particularly the oil boom—further illuminated how authoritarian rule was materially and culturally sustained. The near-total absence of space for civil society was not incidental, but a structural outcome of this system.

I never imagined that, almost three decades after the fall of Suharto, history would begin to repeat itself, or that a younger generation would experience the return of authoritarianism. Teaching political science over the past few years has become increasingly difficult for me. I could no longer point to Indonesia, the world's largest Muslim-majority country, as a convincing example of democratic practice. Instead, it was democratic backsliding that increasingly defined the classroom discussion. This shift generated not only pedagogical discomfort, but also a deeper sense of democratic exhaustion and hopelessness. I did not anticipate the re-entry of military power into civilian politics through legislation reviving the armed forces' dual function, nor did I expect that both former and current presidents would echo Suharto's authoritarian style by promoting family members into positions of political power, and by jeopardizing liberal democratic principles. Yet this is precisely what has unfolded, paradoxically within the formal framework of a democratic era.

There are, of course, important differences between contemporary authoritarianism and the New Order. Today's authoritarian turn unfolds gradually and often imperceptibly: democratic institutions formally persist, yet their substantive functions are steadily hollowed out. This what scholars identify as the features of autocracy (Lührmann and Lindberg 2019) and autocracy did not happen in one night, like the fall of the Berlin Wall in the late 1980s. In Indonesia, autocracy is coupled by populist style of leadership. Jokowi delivered what many citizens demand such as expansive social assistance (*bansos*), and large-scale infrastructure. This sustains high approval ratings until the end of his tenure. Marcus Mietzner (2025) characterises this governing strategy as "majoritarianism," in which popular support marginalises critical and minority voices rather than constraining executive power. This is in line with Aspinall's et al (2020) finding that for majority of Indonesian, democracy means 'economic democracy', rather than protection of civil rights. As a result, minority group (activists and other critical group) who demand on democracy as equality before the law. Finally, unlike during the New Order, digital media now plays a central role. Authoritarian consolidation is pursued not only through control of political institutions, but also through the management of political narratives, with artists, celebrities, and influencers mobilised as instruments of state-aligned propaganda.

Here, teaching using critical thinking method is more important than ever. Data from Kompas Research dan Development shows that generation Z (17-26 years old) in a percentage of 32.7% voted for Prabowo-Gibran in the 2024 presidential election (BBC News 2023). Their main reason, according to the survey, is because the candidate "accommodate[d] more young people". Here, voters seem to be unaware that their decision will affect the next five years before the 2029 election.

Yet this preference appeared largely detached from an awareness of how electoral choices shape governance over the full five-year term leading up to 2029. This raises an uncomfortable question: if democratic regression is enabled through popular consent, what collective failures—of political education, public discourse, and civic imagination—have brought us to this point?

One form of critical thinking I emphasize in my teaching is positioning students as ordinary citizens whose electoral choices carry real consequences. This approach brings political science out of abstraction and into students' everyday lives. I think this is important and relevant in the current political climate. In each of the class, I typically begin the first session by posing questions drawn from students' daily experiences. In my "Labour Politics" class, for example, I ask what kinds of jobs they aspire to after graduation, and then contextualise these aspirations through discussions of insider networks and patronage (*orang dalam* or *ordal*) in accessing employment opportunities. I emphasises how unequal access—whether to jobs or political power—is structurally embedded.

I adopt a similar approach in my "Civil Society in Indonesia" course, where I assign comparative cases from other countries to examine strategies for defending civic space under authoritarian pressure. Through these methods, I hope students will recognise why democracy matters, and how economic and political inequalities shape their own life chances. In all classes, I emphasize that as citizens we have rights to respond/protest/get organized collectively on the structural condition wherever we are and whatever sector we are in.

I do not know whether these teaching methods will be sufficient for my students or for Indonesia's future. But in politically turbulent times like these, continuing to teach, to ask critical questions, to encourage reflection, or to get organized is very important. If nothing else, trying, patiently and collectively, remains a way of holding on to the possibility of democracy.

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A PERSONAL REFLECTION ON THE 'CULTURE WAR' IN BRAZILIAN ACADEMIA

by Christian Lynch

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My professional experience under the far-right government of Jair Bolsonaro began in January 2020, when I was working as a researcher at the Casa de Rui Barbosa Foundation, a federal research institution in the humanities based in Rio de Janeiro. Until then, since the impeachment of former President Dilma Rousseff and the inauguration of the moderately conservative government of Michel Temer, I had observed no significant alteration in the regular functioning of the federal public administration. Institutional routines, administrative criteria, and the relationship between politics and bureaucracy remained within recognizable parameters of normality. Even in 2019, I could not have imagined that an elected government would openly disregard statutory law, constitutional guarantees, and the consolidated jurisprudence of the higher courts, built over more than two decades.

The arrival of Jair Bolsonaro to power... represented a deliberate and explicitly assumed rupture...what occurred was... the disappearance of administrative normality itself. Regular procedures were replaced by the public spectacle of social media

My appointment to the Casa de Rui Barbosa Foundation was not the result of political nomination, but rather the outcome of a standard professional trajectory within Brazilian public academia. In Brazil, federal foundations devoted to research in the humanities coexist with universities and operate as permanent research institutions, largely staffed by career civil servants holding doctoral degrees. Prior to my formal admission to the foundation through a nationwide public competitive examination in 2015, I had worked there for several years as a research fellow, while simultaneously pursuing an academic career as a political scientist and legal scholar. I held a tenured professorship at a federal university, published extensively in political theory and intellectual history, and conducted research at foreign institutions. My successful admission through the civil service examination granted me full tenure under Brazilian constitutional law, ensuring job stability and protection against arbitrary dismissal. My move to the foundation thus reflected a common pattern of institutional circulation between universities and public research bodies, rather than any form of partisan or administrative alignment.

The arrival of Jair Bolsonaro to power on January 1st represented a deliberate and explicitly assumed rupture with established administrative and constitutional norms, rather than a mere change in policy orientation. His government openly adopted, as official policy, the promotion of a so-called "culture war" within the State, directed above all against areas regarded by far-right ideologues as contaminated by the "virus of leftism" or "communism": culture, education, science and technology, human rights, racial equality, and the environment. The declared inspiration for this orientation was the first Trump administration in the United States.

This rupture was not merely rhetorical. It entailed a systematic dismantling of long-standing routines that had traditionally separated partisan politics from the stable professional corps of the federal administration. Under Bolsonaro, public administration was explicitly politicized and partisanized: administrative precedents were ignored, legal constraints were treated as obstacles rather than binding norms, and settled judicial interpretations were openly questioned and deliberately strained.

A clear illustration of this attack on process was the Bolsonaro administration's nominee to the presidency of the Casa de Rui Barbosa Foundation. A federal law enacted in 1966 stipulates that the institution must be headed by an individual of recognized scholarly distinction and demonstrated knowledge of the life and work of Rui Barbosa, one of Brazil's foremost jurists and diplomats. This requirement did not prevent the nomination of a president who possessed neither recognized scholarly credentials nor even basic familiarity with Rui Barbosa's intellectual legacy: a television scriptwriter specializing in children's and religious programming, with no prior administrative experience and no familiarity whatsoever with the research agenda or institutional mission of the foundation she was appointed to lead. Such appointments were not isolated anomalies, but rather part of a broader strategy aimed at the deliberate demoralization of scientific and cultural institutions and the partisan capture of the federal administrative apparatus.

Until 1986, cultural policy in Brazil had been administered as a secretariat within the Ministry of Education. From that year onward, culture existed as a cabinet-level ministry, reflecting its recognition as a distinct domain of public policy. In 2019, Bolsonaro reversed this institutional arrangement by downgrading the Ministry of Culture back to a secretariat—this time subordinated to the Ministry of Tourism. Unlike the earlier subordination to the Ministry of Education, this decision had no administrative or policy rationale and was never publicly justified. The attachment of cultural policy to tourism was widely interpreted as a symbolic act of institutional humiliation, intended to ridicule the very idea of a dedicated cultural administration and to signal contempt for culture as a legitimate field of State action.

Because the new government lacked qualified technical personnel to occupy civilian posts, it relied extensively on military appointments. This reliance was neither incidental nor marginal. During Bolsonaro's presidency, thousands of active-duty and retired military officers were appointed to civilian positions throughout the federal administration—an unprecedented development since the end of Brazil's military dictatorship in 1985. This occurred partly because Bolsonaro's political group lacked a structured party organization capable of supplying trusted civilian cadres, having never developed a conventional party apparatus. It also reflected a deliberate strategy aimed at the political capture of the Armed Forces themselves. Throughout his mandate, Bolsonaro repeatedly referred to the military as if it were personally loyal to him, speaking of it as an extension of his own political authority and suggesting a supposed symbiosis between his government and the Armed Forces as an institution. It was therefore not accidental that, following his electoral defeat, Bolsonaro encouraged and legitimized initiatives aimed at a coup d'état, seeking to secure his continued stay in power by mobilizing precisely this imagined identification between his leadership and the military.

It took roughly one year before the practical effects of this ideological orientation were felt directly within the foundation where I worked. After several months, abruptly and ostentatiously, the new president dismissed the director and all heads of the research center's divisions—career civil servants—with the explicit purpose of publicly staging the culture war within the institution. The search for replacements followed immediately, but quickly ran into a shortage of available and qualified personnel. The situation dragged on for weeks, until the president began inviting researchers from the research center itself to occupy the vacant posts, including myself.

The invitation was based on the assumption, shared by the president and her advisers, that I would be ideologically aligned with the new government, since my academic curriculum included several articles devoted to the study of conservatism. I accepted the invitation to head my own division, dedicated to the history of political and social ideas and composed of only four researchers besides myself. My motivation was strictly pragmatic: to prevent the division from being placed under the control of someone completely alien to academic work, particularly a military officer with no training in the field.

The president then adopted a practice that would soon become widespread throughout the federal administration: the immediate publication of all her acts on Twitter, as a means of signaling loyalty to the far-right base and, above all, to the presidential family—since her appointment was widely attributed to one of the President's sons. Less than two hours after the public announcement of my prospective appointment, the Secretary of Culture became aware of it, subjected my name to a crude form of ideological "compliance," and publicly declared, also via Twitter, that he would not appoint me.

The Secretary retrieved texts I had written criticizing the Bolsonaro government, describing it as anomalous in comparison with its predecessors, both in political conduct and administrative practice. He publicly declared that I deserved to be dismissed from public service because of my "execrable opinions," explicitly tagging the President's official account to demonstrate his prosecutorial zeal. He added, however, that he could not dismiss me due to civil service tenure, even as he sought to block my appointment through ideological screening.

It is important to clarify that my appointment was never formally made, annulled, or subjected to legal review. No administrative procedure was initiated, no written decision was issued, and no act was published in the Diário Oficial, as required under ordinary rules of Brazilian public administration. Everything unfolded entirely on Twitter. The president of the foundation publicly announced my prospective appointment through her personal account, and a few hours later the Secretary of Culture replied—also on Twitter—that he would not appoint me.

In this sense, what occurred was not a legal veto, but rather the disappearance of administrative normality itself. Regular procedures were replaced by the public spectacle of social media, and institutional decision-making gave way to performative gestures aimed at signaling ideological loyalty. What mattered was no longer legality or routine administrative functioning, but the exceptional visibility produced by the spectacle of the so-called culture war, and above all the eagerness of newly appointed officials to flatter the President's family by transforming ordinary and legally trivial administrative matters into public acts of political theater.

Two days later, the Secretary of Culture himself was dismissed, following pressure from the Israeli ambassador, after a live broadcast in which he presented guidelines for the government's cultural policy by explicitly paraphrasing the discourse and aesthetics of Joseph Goebbels. He even used the prelude to the first act of Lohengrin by Richard Wagner—the same musical piece employed by Charlie Chaplin in The Great Dictator.

From that episode onward, I gained increasing visibility in the media—both in traditional outlets and on social media—and became a frequent public critic of the Bolsonaro government. I criticized it systematically for the erosion of democracy, administrative incompetence, corruption scandals, and acts of institutional and social violence. These criticisms were expressed through press interviews, opinion columns in newspapers, and public statements on social media. At all times, it was made explicit—both formally and substantively—that I was speaking strictly in my private capacity as a citizen and scholar, and not on behalf of the public institution where I worked or in the exercise of any administrative function. This distinction was fully consistent with the long-standing jurisprudence of the Brazilian Supreme Federal Court regarding freedom of expression for civil servants.

Approximately one year later, during the COVID-19 pandemic, when the foundation had shifted to remote work, the persecution resumed—this time initiated by the president of the same Casa de Rui Barbosa Foundation where I had worked for years, and by the very individual who, months earlier, had invited me to head my research division. This shift marked a clear reversal in her stance. Appointed through the political influence of an evangelical pastor serving as a federal deputy, she had become increasingly weakened as the new Secretary of Culture sought her removal. Acting on the advice of one of her directors, she chose to initiate administrative proceedings against me in response to my public criticism of the government, apparently believing that such persecution would signal loyalty to the presidential family and secure her political survival.

By that point, the Bolsonaro government had already transformed legal intimidation into a systematic instrument of governance. Professors, researchers, physicians, and other professionals who criticized the government were subjected to exemplary proceedings. This strategy depended on the active collaboration of public attorneys and members of the Public Prosecutor's Office willing to abandon basic principles of administrative and constitutional law, discarding professional standards in order to serve the purposes of a government operating outside the law—whether out of ideological conviction or sheer opportunism. These were, in general, profoundly mediocre figures, lacking both technical competence and moral authority, who would never have advanced within the administrative hierarchy under a normal constitutional government.

In my case, the president convinced the foundation's attorney-general, likewise aligned with the government, to open four simultaneous administrative proceedings against me, all based on arbitrary and grossly distorted interpretations of the law. One example illustrates the level of abuse involved: a legal provision obliging civil servants to report acts of corruption within their own agencies was invoked to argue that, by commenting on social media about corruption scandals widely reported in the press—particularly within the Ministry of Health during the pandemic—I was legally required to formally identify the corrupt individuals and indicate their whereabouts in Brasília, despite living in Rio de Janeiro and having no professional connection to those agencies.

At the same time, I was compelled to hire private attorneys to defend myself in these proceedings, at significant personal and financial cost, and to mobilize influential professional and institutional contacts simply to secure my departure from the foundation and preserve my physical and psychological integrity. I was not an isolated case. Dozens of other civil servants were subjected, during the same period, to similar forms of harassment and pressure, designed to force them to leave their posts, request early retirement, or remain silent. The strategy was unmistakable: to hollow out institutions through fear and exhaustion, purging critical voices without the need for formal dismissals.

These were the worst months of my life and of my family's life. Daily insecurity, the constant fear of arbitrary retaliation, and the realization that the law had ceased to function as an effective constraint on State action produced profound and lasting harm. For the first time in my life, I sought psychiatric treatment and began taking antidepressant medication. My family life was deeply affected, marked by prolonged anguish, apprehension, and instability.

I deliberately refused to publicly denounce the persecution, as that was precisely what the foundation's president sought: to turn me into an instrument of her own political self-preservation. I limited myself to formally requesting a transfer to other public bodies, including the municipal administration or a return to the federal university from which I had originally come, where I would be less exposed to direct political interference. The president initially refused to authorize the transfer, alleging that I would be subordinated to a politician who was suing her for failing to meet the minimum legal requirements to hold her office.

Only when the Secretary of Culture succeeded in dismissing the adviser who provided her with political backing did she authorize my transfer, and even then only to the federal university from which I had come. Not even on that occasion did she refrain from attempting to persuade me to redirect my public criticism against the Secretary of Culture himself—a man who carried a gun and displayed unstable and obsessive behavior—in her defense, despite the fact that she had initiated multiple administrative proceedings against me.

Fortunately, the Brazilian public bureaucracy resisted as best it could. The Office of the Comptroller General of the Union rejected all proceedings in their entirety, unequivocally recognizing that there had been no administrative wrongdoing whatsoever in criticizing the government. This outcome was possible only because Brazilian civil servants enjoy job stability guaranteed by the courts, which prevents arbitrary dismissals, summary removals, and the wholesale replacement of technical staff by political loyalists. Had it not been for these guarantees, and for the resistance of the Supreme Federal Court itself, the protections of the public bureaucracy would have collapsed. Arbitrary power would have spread throughout the administration, contaminating its routines and hierarchies and rendering any form of resistance in the name of the rule of law effectively impossible.

A VOCATION OF RIGOR, CURIOSITY, AND CARE

by Pavitha Suryanaryan, Associate Professor at the London School of Economics

Kimuli Kasara *was Associate Professor of Political Science at Columbia University.*

I met Kimuli as a newly admitted PhD student on a cold April day in 2009 at Columbia University. Like me, Kimuli had lived across contexts, including Kenya, India, the UK, and Canada. As I told her about my own journey from Chennai to Singapore, Hong Kong, and the US, we fell into an easy conversation about Indian politics. She had spent her schooling years in India, which of course meant she had strong opinions on *daal khichdi* and the inner workings of the Indian bureaucratic state. I left her office that afternoon certain of one thing: Columbia would be the most exciting place to learn about being a political scientist because it had people like her in the department, working on questions that nobody had thought to ask.

Kimuli's papers often poked at deep seated assumptions about political behaviour in the "developing world." For example, as a graduate student at Stanford, she wrote a remarkable paper on how our view of ethnic politics in Africa were shaped by ethnicity being a basis for clientelism and patronage. Yet, she showed that ethnic leaders could also cajole and coerce their own group members to pay into a state, creating a durable basis for taxation. This simple intuition upended our beliefs about ethnic politics as merely a means to give to our kin, and instead showed it as a basis for state-building. She published this paper, "Tax Me If You Can," in the *American Political Science Review* (APSR) by the time she finished graduate school.

In a class on democratic institutions that she taught at Columbia, we were discussing papers on voting inequality, and everyone in the room just accepted the assumption that poor and more marginal voters would vote at lower rates than the rich. Our eyes met across the room, with raised eyebrows, and I ran to her office after class to discuss why this simply did not ring true for the cases we studied in South Asia and Africa. We agreed that the field had overlooked the role of state capacity in shaping the voting behavior of the wealthy. Over the next five years, we published two papers on the links between state capacity, turnout inequality, and class voting.

That collaboration taught me many things. The first was how finicky Kimuli was about research. She would rewrite a paragraph dozens of times before it was perfect. She would make me check our code and data analysis and then check it again, because honest, diligent, and replicable work was the most important part of research. She would not hit "send" to a journal until every word, figure, and table could persuade the reader. A relentless taskmaster, she taught me that we controlled one thing, our work, and that would prevail ultimately.

Under her tutelage, I started a project that sat at the intersection of caste, taxation, and state capacity. When professors would push me to explain why I was arguing that status needed to be developed as its own identity category, Kimuli would encourage me to draw on what caste had taught me about identity and redistributive politics, but she also pushed me to challenge my own assumptions. "Lay it out like you are writing a formal model," she would say; "what do these high status groups want if it is not material resources?" Her words continue to give me clarity on my work. Studying under her was also terrifying because she read everything so closely. It was a gift she gave her graduate students. She parsed and evaluated arguments, turning them inside out. It was always humbling to see entire sections of my writing crossed out, because she wanted us to be more direct and fearless about using our own voice.

Kimuli's door was always open to students and colleagues at Columbia. As I sat in her office for many hours huddled over our paper, I would see a steady stream of graduate students and faculty seeking her out for her perspective and ideas. It is no wonder that she was much in demand as a collaborator, both as a graduate student at Stanford and then as faculty at Columbia. Working with her was fun, combative, and thrilling.

She cared deeply about the well-being of female and minority graduate students. She would push us to add skills to our CVs that we didn't think we could aspire to—taking advanced game theory or signing up to meet an external speaker—and to learn how to take rejection constructively, because she believed that would be the single defining feature of who would push ahead in the field and who would not.

Kimuli played many roles for me as a teacher, coauthor, and friend, but the one I know she took most seriously was her role as my mentor. She was always available for that phone call or WhatsApp text as an ocean grew between us, where I would ask her for advice on how to navigate the discipline. When I last saw her in October, a month before her passing, she remembered to ask how my book manuscript was going, how I had managed to get away to see her in the midst of the teaching term, and if my kids would be okay without me. Ever the mentor, even in the midst of her own illness.

Our field will remember Kimuli Kasara for her lasting contributions to ethnic, distributive and African politics. I will remember Kimuli as the person who sat in the Lindsay Rogers room at Columbia, quizzing a presenter, with the paper already printed, read, and underlined in her hands, speaking with her eyes closed, a smiling face, and a deceptively simple remark that went right to the heart of the argument. I will remember the person who constantly reminded me that what we do is a vocation and not a job, who loved her students, and who, most of all, cared about the ideas.

RESTING PAPERS

by Macartan Humphreys, Director of the Institutions and Political Inequality group at the WZB Berlin Social Science Center

Kimuli Kasara *was Associate Professor of Political Science at Columbia University.*

Kimuli's webpage is spare. It reflects her horror at having to write anything remotely resembling self-promotion and an instinctual aversion to overexplaining. It contains a section for her political song competition where she had students scout for YouTube videos of politicians wooing electorates with song and dance, extolling heroic leaders, lambasting liberals, getting out the vote. Kimuli countering the awfulness of Covid, and the sterility of textbooks, by getting her "kids" to connect with the madness and energy of real politics.

In a quieter part of the page she links to her major pieces. A precious collection. She leads with what's probably her best-known piece, "Tax me if you can." "Tax me" is a polite piece, if you know Kimuli. She's gently suggesting how interesting it is that if you look carefully you'll notice that politicians extract from co-ethnics. But really it's berating us, incredulous, as Kimuli often was, at the naïve takes we have on ethnic groups. How can we be thinking of these like coalitions or clubs, looking out for each other, when they are so obviously messy networks of dependencies and information flows that can be used as much for internal exploitation as for support?

Later come the brilliant pieces with Pavi Suryanarayan and Isabella Mares and two lovely pieces looking at residential segregation, including one using a clever strategy for figuring out causal effects using Kenya's land settlements history.

At the bottom though, after the working papers (one of which, Kimuli, I will try to finish up soon!), she has a section called "resting papers." Four fully developed papers that aren't in general circulation— two have dead links and two none at all. These are papers that meant a lot to Kimuli and were with her for a long time. Now resting.

I want to talk about these four resting papers, and draw attention especially to the last three.

The first is "Electoral Geography and Conflict." This one should have been published an age ago. Lots of work, including by Kimuli, looks at how geography shapes violence. "Electoral Geography" goes the opposite direction and asks how violence shapes demography; does it produce a kind of gerrymandering? When control of an area is important enough, does politics work through demographic engineering? The paper is classic Kimuli: big question, rounded theorizing, new measures, in this case of the political importance of geographic units and satellite imagery to capture violence, and neat strategies to trace the origins of IDPs and figure out the marginal effects of expulsions on demographic compositions. Characteristically, a rich discussion of underlying logics.

The other three resting papers are quite distinct though and show sides of Kimuli not so well represented in her published work.

"Explaining Ethnic Mobilization in Rwanda and Burundi in the 1950s" (aka "Colonial legacies and ethnic mobilization…") takes a deep dive into colonial history trying to figure out the importance of clan membership for ethnic conflict. A more nuanced take on identity and affiliations sheds light on why Hutus mobilized in some parts of Rwanda but not others as well as differences between Burundi and Rwanda. Re-enforcing clan and ethnic cleavages predict the deposition of chiefs and reduced Tutsi dominance. Awkward findings, these, for a range of existing explanations, including those focused on overly simplistic accounts of ethnic group formation by colonial powers. Still, reading it, you feel a tension. The social scientist's task of extracting a simple narrative hitting up against Kimuli's insistence on remaining faithful to the complexity she saw in the historical and anthropological data, and the many different types of outcomes in different types of places that test the logic. There is here a type of fidelity to a case and tolerance for complexity we don't see nearly enough of.

Resting paper three is "Ethnic Beachheads and Vote Buying: The Creation of New Administrative Districts in Kenya, 1992–2001" (though the copy I have has 1963–2001). Kimuli tries to make sense of the political logics behind district formation, figuring out how changing boundaries change ethnic identification, and the role of ethnicity in political calculus. District formation was used as a payoff for political allegiance but also as part of a forward-looking calculus: a type of divide and rule logic that leaves traces in the splitting up of opposition areas. I always loved the title of the piece, focusing on the creation of "pro-government ethnic beachheads in opposition majority areas." I think Kimuli chose the term to capture the kind of long game strategizing that politicians were engaged in. This strategizing is a big part of the story. But her account colorfully shows the flash decision making and the contradictions—pronouncements by politicians that divisions were not politically motivated, all the while credit claiming aplenty during political rallies. Again, Kimuli builds up an overarching insight, but she also sees many things going on at once that refuse to make for a simple story. She pulls out strands for us. She leaves other threads in place. Others she demolishes. You see her efficient dispatching of alternative simplifying, self-serving, logics—the simple story that politicians try to impose, that these new districts were meant simply to "bring development closer to the people." (That quote is unattributed but she has an interlocutor in mind, who she is roasting as they try to pass off an innocent explanation for the curious pattern of hasty divisions. Oh yeah, Kimuli says, if you were after efficiency it wouldn't be like this you'd be doing it.)

The last of the resting papers, "Selecting an Imperial President: Opposition Coalitions and the Kenyan Presidency, 1991–2002" (aka "A Prize too Large to Share"), tries to make sense of opposition failure in Kenya. The key insight is that when presidents are strong, politics is more likely to take on a winner-takes-all characteristic and credibility problems for opposition coalition members start looming large. As she puts it: opposition politicians appear to prefer to remain out of power than to form an alternative winning coalition (unspoken: with sharks). Conversely, at a key moment, prospects of a weakened president made coordination to claim the presidency easier. This paper brings yet another method into play. The empirical heart of the work draws on interviews with political leaders and readings of their MOUs and internal agreements. The piece reads almost like a formal model. A qualitative analytic narrative.

I wanted here for these pieces that were dear to Kimuli to get a bit more exposure. They all show her uncompromising mind at work, in a way that can be less obvious in published pieces. I can imagine how Kimuli might react to me drawing attention to these papers and my trying to guess at her thinking in writing them. She'd give me that evil eye. She'd open her eyes wide and say yes yes, you got a part of it alright. But I'd know and know I missed layers of it.

In losing Kimuli we have lost so much. For so many of us, an intellectual inspiration of course. A complex, layered thinker. But also an energy. Someone to keep us honest. A co-conspirator, an ally, a laugh, a smile.

CALL FOR CP SECTION PRIZES 2026

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The Luebbert Best Book Prize is awarded annually for the best book published in the field of comparative politics. Books published in the last two years (2024 and 2025) are eligible. Self-nominations are welcomed.

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To apply, email committee members:

- Rafaela Dancygier (Chair) rdancygi@princeton.edu
- Jan Pierskalla pierskalla.4@osu.edu
- Joan Ricart-Huguet jricart-huguet@loyola.edu
- Rachel Beatty Riedl rbeattyriedl@cornell.edu
- Anne Meng ameng@virginia.edu

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To apply, email committee members:

- Adam Auerbach (Chair) adam.auerbach@yale.edu
- Anirvan Chowdhury anirvan.chowdhury@louisville.edu
- Aala Abdelgadir aaa436@pitt.edu

Lijphart/Przeworksi/Verba Dataset Award

The Lijphart/Przeworski/Verba Best Dataset Prize is awarded annually to a publicly available dataset in the field of comparative politics. Self-nominations are welcomed.

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To apply, email committee members:

- Dan Treisman treisman@polisci.ucla.edu
- Nick Carnes nicholas.carnes@duke.edu
- Jessica Gottlieb jagottlieb@uh.edu

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The Theda Skocpol Emerging Scholar Award is given to a scholar up to ten years post-PhD whose work has made impactful empirical, theoretical, and/or methodological contributions to the study of comparative politics. Self-nominations are welcome.

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To apply, email committee members:

- David Samuels dsamuels@umn.edu
- Vicky Murillo mm2140@columbia.edu
- Zach Elkins zelkins@austin.utexas.edu

Bingham Powell Mentoring Award

This prize, introduced in 2012, is awarded to a political scientist who, throughout their career, has demonstrated a particularly outstanding commitment to the mentoring of graduate students in the field of comparative politics. The prize was named in honor of G. Bingham Powell, and initiated by his students. Nomination letters for the G. Bingham Powell graduate mentoring award should include, but go beyond, the academic achievements of the nominee, and provide information about the role the nominee has played in advancing the intellectual development and research careers of their mentees. Testimonials from a few former mentees can be included in the nomination letter, which should not exceed five pages.

Deadline for nominations: March 15, 2026

To apply, email committee members:

- Amanda Robinson (Chair) robinson.1012@osu.edu
- Evan Lieberman evanlieb@mit.edu
- Erik Bleich ebleich@middlebury.edu

Barbara Geddes Lifetime Achievement in Research, Teaching, and Graduate Mentoring Award

The biennial Geddes Lifetime Achievement in Research, Teaching, and Graduate Mentoring Award recognizes senior scholars (30+ years post-PhD) who have undertaken innovative research while also showing dedication to teaching and/or mentoring. To receive this award, a scholar must stand out in research and teaching/mentoring.

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Deadline for nominations: March 15, 2026

To apply, email committee members:

- Miriam Golden (Chair) golden@ucla.edu
- Eric Dickson eric.dickson@nyu.edu
- Natalia Garbiras-Diaz ngarbirasdiaz@hbs.edu

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ABOUT

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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