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INTRODUCTION

By Benjamin Smith



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Full disclosure: my first academic panel experience was in November 1996, at the Middle East Studies Association annual meeting in Providence. It was a panel on state-society relations in MENA and featured Ellen Lust, among others. Ellen's presentation struck me then, and her work has struck me more since, for its keen synthesis of "old" area studies and "new" social science theory and method. Years before the September 11, 2001 attacks catalyzed massive new funding for scholars of the MENA region, Ellen was pushing into a region at the margins using a set of theoretical tools from microeconomics, that which we of the generation learned as rational choice theory.

The late 1990s featured a broad, lively debate between "area studies" scholars and traditional social scientists, in places as illustrious as the pages of this newsletter. We return to it in this symposium, reflecting on how area and country specialization fit, or do not fit, into contemporary comparative politics. I am taking my prerogative as editor to weigh in briefly on this insightful set of contributions. One of the most salient veins, reading these four essays, is the theory-identification divide. Where, a quarter century ago, it was "theory" meaning formal theory versus contextually focused inductive research, it is now ironically area specialists who often seem more attuned to external validity concerns.

Gans-Morse, Gingerich, and Pepinsky conclude that area studies—defined somewhat more simply as "a sustained focus on one part of the world"—is alive and well. As you will see, that is a contested conclusion. Gallagher, Lust, and Siddiqui all point to problems in conceiving of it that way, largely because it is untethered from the original point of area studies scholarship—to point out important dynamics, visible only from immersive research, that shed light on, elaborate, and

challenge general theoretical frameworks. Design-based, single-site interventions, whether experimental or not, simply do not accomplish this. My own track record of asking questions in panels and other settings about this often brings answers such as, "Well, we think [African city] is pretty similar to other African cities" or "I did my survey experiment in [country X] because it had an election the year I could be in the field." So, how do we bring back both the focus on context-only insights alongside a commitment to broad comparative inquiry?

One current effort to alleviate the generalizability weakness of many experimental design studies involves carrying out the same experiment in multiple settings. While that takes us some of the way toward understanding external validity, it runs the risk of multiplying what Lust points to as a key weakness of these studies: limited contextual foundation of the kind that comes only through extended research in a single setting. However, the original theoretical ambition that drove the first generation of area studies—a universal theory of social science—is probably not much stronger than structural functionalism turned out to be.

Both Lust and Gallagher (and, indirectly, Siddiqui) argue for more interdisciplinary and for more cross-regional research. I would endorse this, and I would point to the kind of team-driven research at the [Peace Research Institute Oslo](https://www.peaceresearchinstitute.org/) as a good example. One simple observation is that scholars who are trained from the start with a sensitivity for context and for the ways in which local dynamics drive politics innately think about "countries as cases" differently than their counterparts trained statistically, with an eye to viewing most cases as a priori comparable. More than an outcome in a single country impugning a theory, area specialty training can be absolutely crucial

for identifying traits of individual or small numbers of cases that are likely to exert undue influence in a larger sample (or even universe of all independent states). In the last several years, I have been working on a set of projects that provides strong evidence that the Middle East is singularly responsible for a set of findings we refer to as the “resource curse.” Put simply, accounting for two historically contingent factors—the Iran-Iraq war of 1980-88 and the 18th-19th century British imperial intervention in the Persian Gulf—we see that oil wealth has no statistically meaningful impact on either autocratic durability or international bellicosity. I mention these conclusions because they illustrate how a research program originated in the Middle East and North Africa (MENA), became global, and then came under challenge as the result of new insights from the region that was its origin story.

The implication is that, to call for more support for area training, we need to understand its contribution to comparative politics not just as immediate knowledge production, but also as the likely but unpre-

dictable source of insights for broader research questions. This mindset is not unlike the one that drove decades of funding for the US space program: entirely unclear how we might benefit, but pretty sure we would benefit. In this specific case, a foundation of regional expertise contained, but at no point ever really aimed to learn, the cross-border peculiarities of Iran and Iraq: how they came to be, the disputes that underlay them, conflicts between respective leaders, cross-border ethnic and sectarian dynamics and, finally, the escalation dynamics of the war between them.

All of this brings us back to the lofty ambition for area studies that inspired its birth—a “universal social science theory”—and the question of how we fit theoretical ambition in comparative politics to the growing priority of causal identification. The contributors to this issue’s symposium have marshaled an impressive array of empirical and theoretical vision that I hope finds its way into new scholarship and, eventually, graduate training in comparative politics.

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If you have submissions for the
dataset review section of the APSA-
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TWO CHEERS FOR THE "NEW" AREA STUDIES

By Jordan Gans-Morse, Daniel Gingerich, and Thomas B. Pepinsky

Debates about the relationship between area studies and comparative politics are as old as the subfield itself. In the late 1990s, however, a particularly vigorous series of exchanges in the *APSA Comparative Politics Newsletter*, *PS: Political Science and Politics*, and other venues renewed and expanded attention to these debates. Scholars at opposite ends of these exchanges seemed to advocate two starkly different approaches to research on politics around the world. One side promoted an area studies approach that studies politics alongside literature, language, history, and culture: its distinguishing features included extensive fieldwork, immersive ethnography, and linguistic and cultural competence. The other advocated a social scientific approach that favors abstraction, theorization, and theory testing: in the place of specific local knowledge, it sought general findings, and therefore viewed politics in one country or region as an instance of politics everywhere.

Although this distinction between area specialists and social scientists was often presented as a "caricature" (e.g., Bates 1997), it struck a nerve among an entire generation of comparativists (see Hall and Tarrow 1998; Katzenstein 2001; Szanton 2002; Graham and Kantor 2007; Mahbubani 2010). And it implied a hierarchy of prestige in which the generalists dominated the specialists, with the latter deemed conservative and outmoded. In Bates's (1996, 1) words, "Rare (...) is the department wherein the area specialists fail to constitute a center of resistance to new trends in the discipline. They tend to lag behind others in terms of their knowledge of statistics, their commitment to theory, and their familiarity with mathematical approaches to the study of politics. They often oppose the appointments of those who have trained in such areas but who may be

deficient in language skills." To progress, comparative politics would require a reconciliation of area studies and social science, but this would be on the social scientists' terms—what Emerson (2008) describes as area specialists' "terms of enlistment" to modern social science. Those who resisted the subordination of area studies to general social science would need to find refuge in area studies programs, where humanistic forms of inquiry were preeminent.

Almost three decades later, certain features of that debate are still with us, even if others—such as the focus on rational choice theory as the axis of conflict—are now outdated. Whereas Laitin (1993) warned that "a specter is haunting comparative politics; it is the specter of pure theory," few comparativists today would share that specific worry. Yet, the tensions between area studies and social science are still central to understanding the field of comparative politics, and they have broad relevance to other disciplines as well.

We are comparativists who began our PhD coursework not long after these debates appeared in these pages. As scholars of three different world regions—the former Soviet Union, Latin America, and Southeast Asia—we know that tensions between our area studies communities and the social sciences continue to exist, but the debates of the 1990s do not resonate with our own experience. Inspired by that gap between those earlier debates and our own careers as comparativists, we set out to understand the evolution of area studies and comparative politics, using the tools of modern political science. First, we collected primary data on attitudes and behaviors of political scientists through an original survey of APSA members conducted in late 2022

Figure 1: Respondents by Year of PhD (N = 1143) By Year of PhD

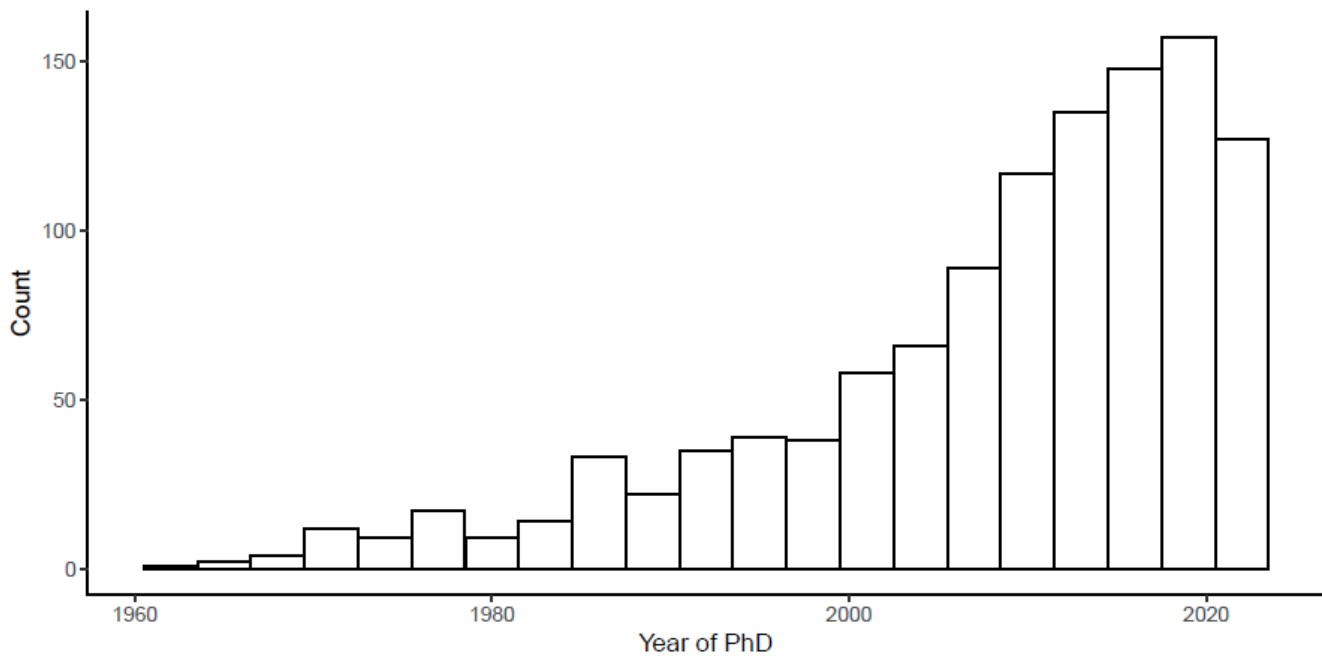
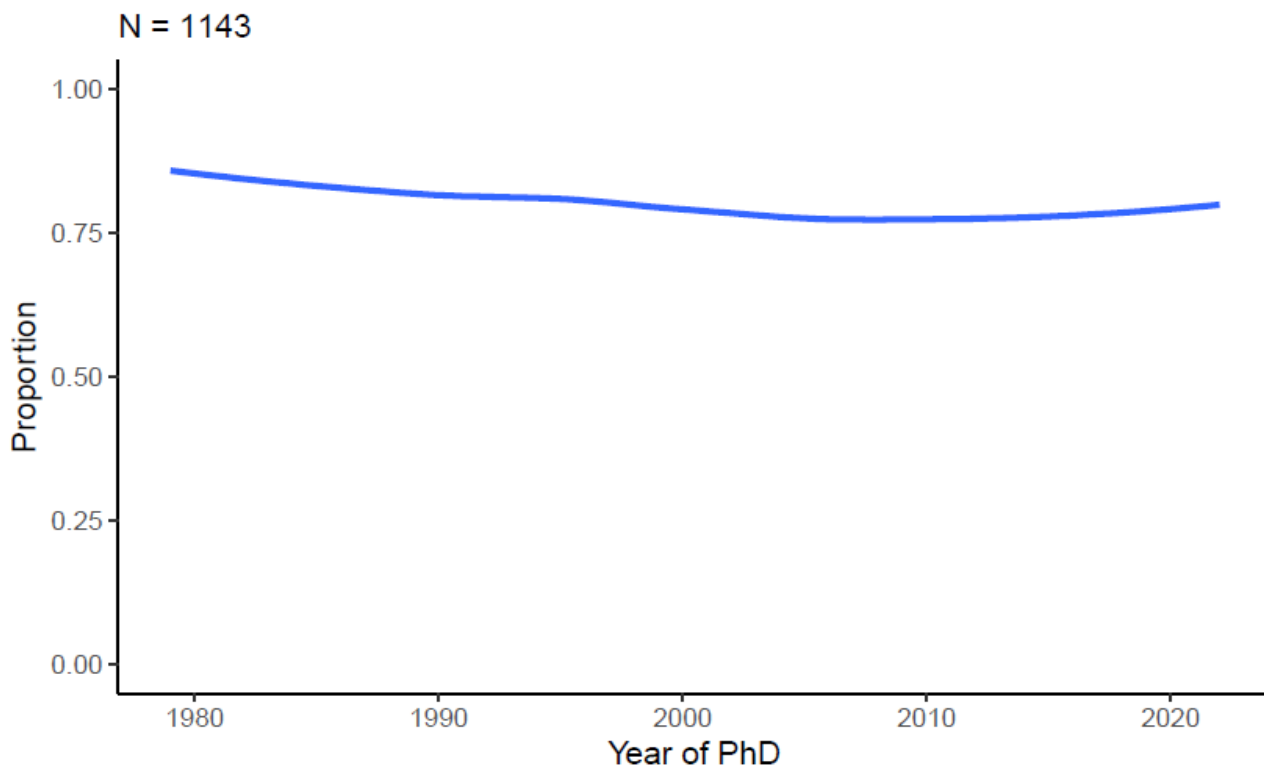
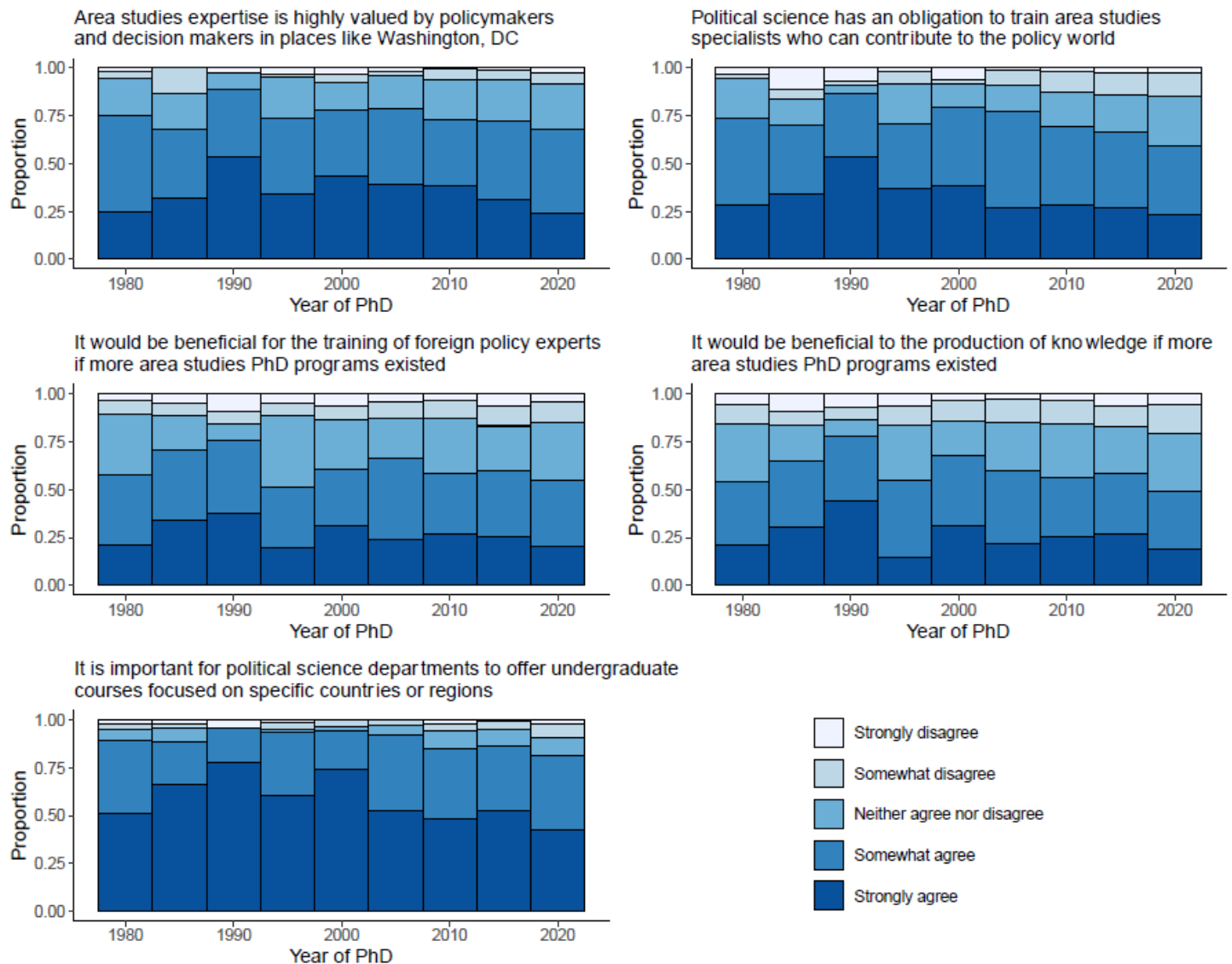


Figure 2: Regional Expertise by Year of PhD



Note: Figures show the proportion of respondents indicating that their research focuses primarily on a specific geographic region or regions. Trendlines are smoothed.

Figure 3: Perceived Value of Area Studies by Year of PhD



Note: Bins are five-year averages. N = 1125

and early 2023. Our analyses below are based on the 1,143 respondents who had completed their PhDs and reported their primary or secondary subfield as comparative politics.¹ Second, we collected secondary data about the research practices of political scientists by studying publication patterns in the approximately 4,300 comparative politics articles published between 1980 and 2020 in the *American Journal of Political Science*, *American Political Science Review*, *Comparative Politics*, *Comparative Political Studies*, *Journal of Politics*, and *World Politics*. These data allow us to characterize how the relationship between area studies and political science has changed over the years, and they illuminate how our discipline engages with area studies today.

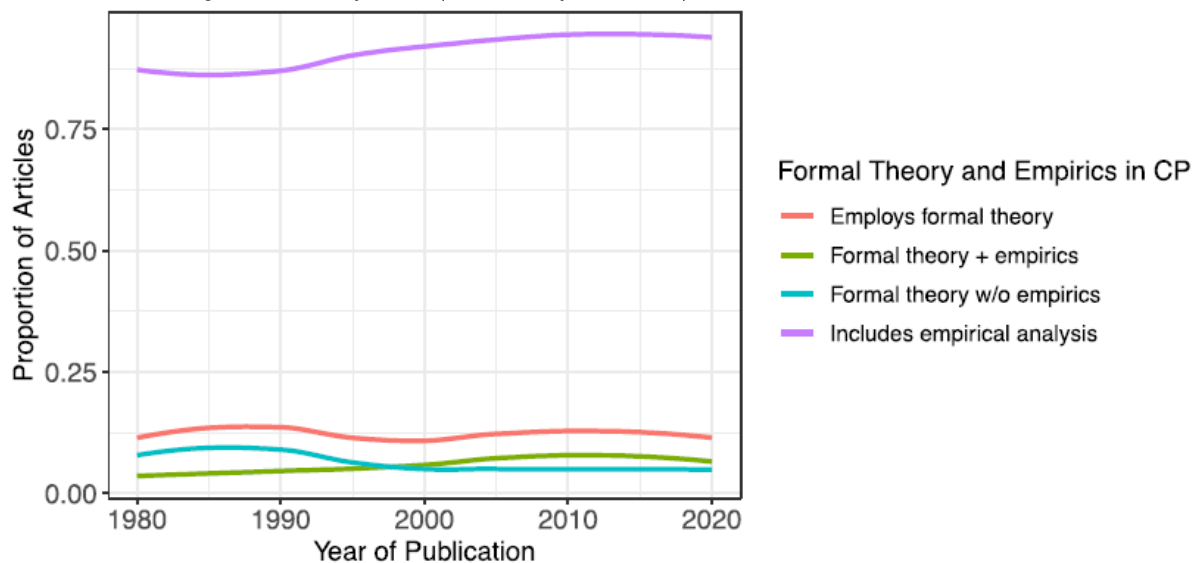
In our analyses of the survey data, we present results organized by year of PhD conferral. Because data are thin for older cohorts, as can be seen in Figure 1, we treat all respondents with PhDs conferred before 1980 as a single cohort. Our data are most informative about

cohorts who have graduated since 2000, but comparisons with scholars from earlier cohorts who remain active APSA members are nevertheless valuable.

Defining area studies is not straightforward, especially when—as we will show below—the nature of area expertise in political science is changing. But the essence of area studies, regardless of which definition one uses, is a sustained focus on one part of the world. Our survey of APSA members shows that a substantial majority of comparativists identify their research as having a regional focus, and that this holds true across all PhD cohorts for the past 40 years (Figure 2).

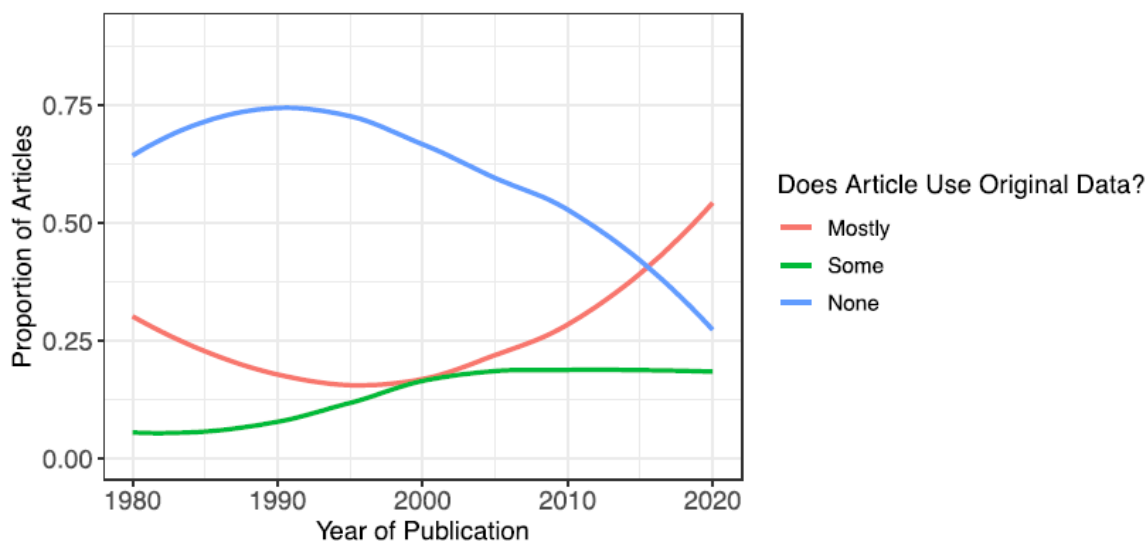
Considering that we also know from existing research that single-country research designs have experienced a resurgence in recent decades (see Pepinsky 2019), there is no evidence that cross-national statistical research has displaced regional specialization in comparative politics. Indeed, our data suggest that many of the fears of schol-

Figure 4: Theory vs. Empirical Analysis in Comparative Politics Articles (1980-



Note: Trendlines are smoothed.

Figure 5: Use of Original Data in Comparative Politics Articles (1980-2020)



Note: Trendlines are smoothed.

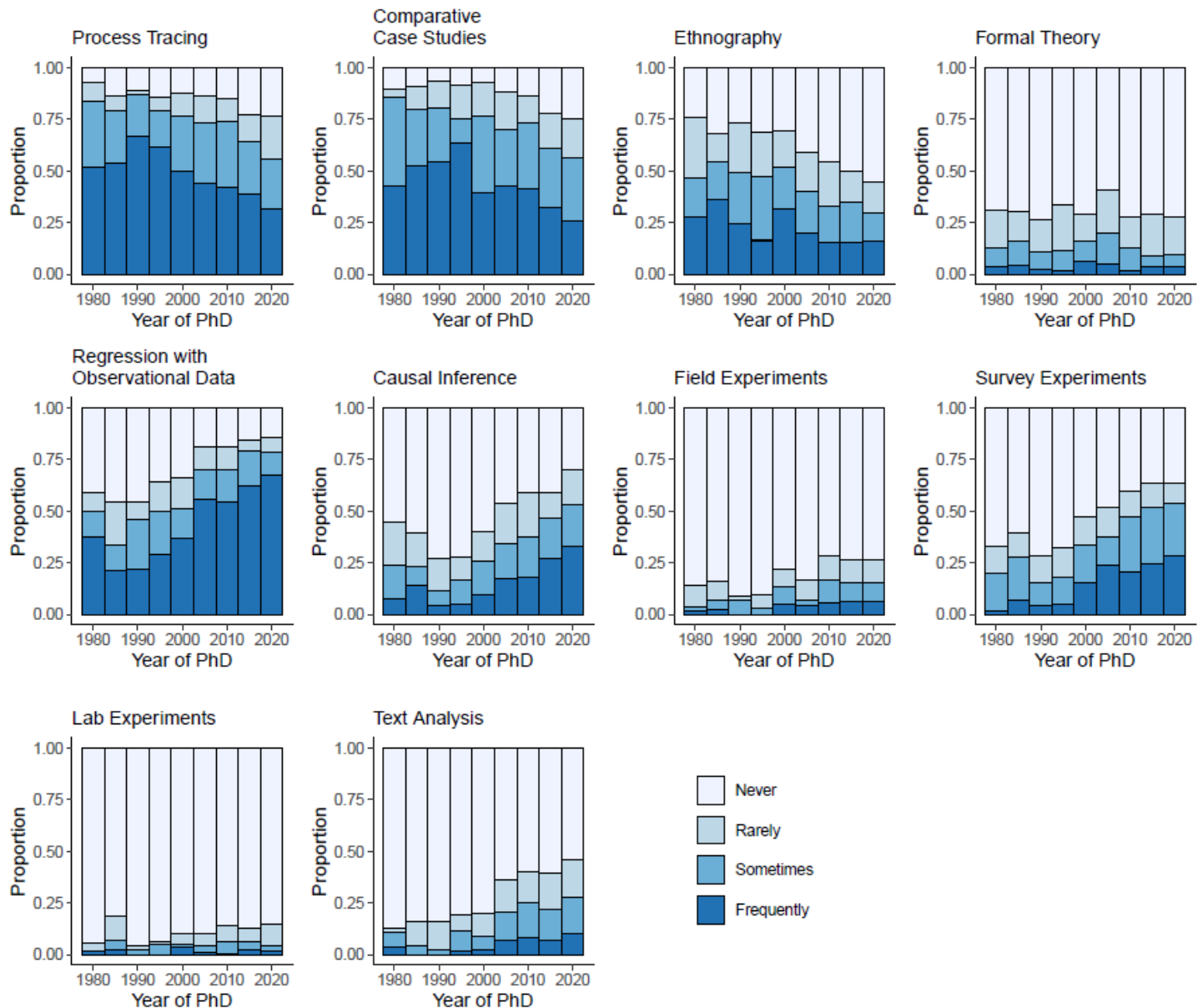
ars in earlier generations about the decline of area studies were unfounded. First, as shown in Figure 3, there is strong agreement among political scientists that area studies knowledge is valuable, and that political science has an important role to play in supporting it. Although the depth of this support has modestly declined among more recent cohorts, most respondents agree that area studies is important for policy, that the knowledge base of political science would be strengthened with more attention to area studies, and that political science has not only an obligation to produce area studies, but also to teach it to undergraduates.

Second, our data show that comparative politics remains a primarily empirical subfield. Theory has not come to dominate empirics in

comparative politics research: Empirically-oriented research methodologies are much more common than pure theory in published scholarship, and formal theory is no more common today than it was in the mid-1990s (see Figure 4). Moreover, comparative politics research is not only increasingly empirical but also increasingly characterized by the production and analysis of original data (see Figure 5).

Third, while there is an obvious trend in comparative politics towards quantitative analysis (see Figure 6), comparativists—especially those in younger cohorts—do not perceive contemporary political science’s focus on methods and causal inference as incompatible with area knowledge, nor do they perceive quantitative methods as less compatible than qualitative methods (see Figure 7).

Figure 6: Frequency of Methods Employed in Comparative Politics by Year of PhD



Note: Bins are five-year averages. N = 1122. Sample includes all respondents with CP as primary or secondary field. Causal inference refers to quantitative analyses of observational data that includes an explicit design for estimating causal effects.

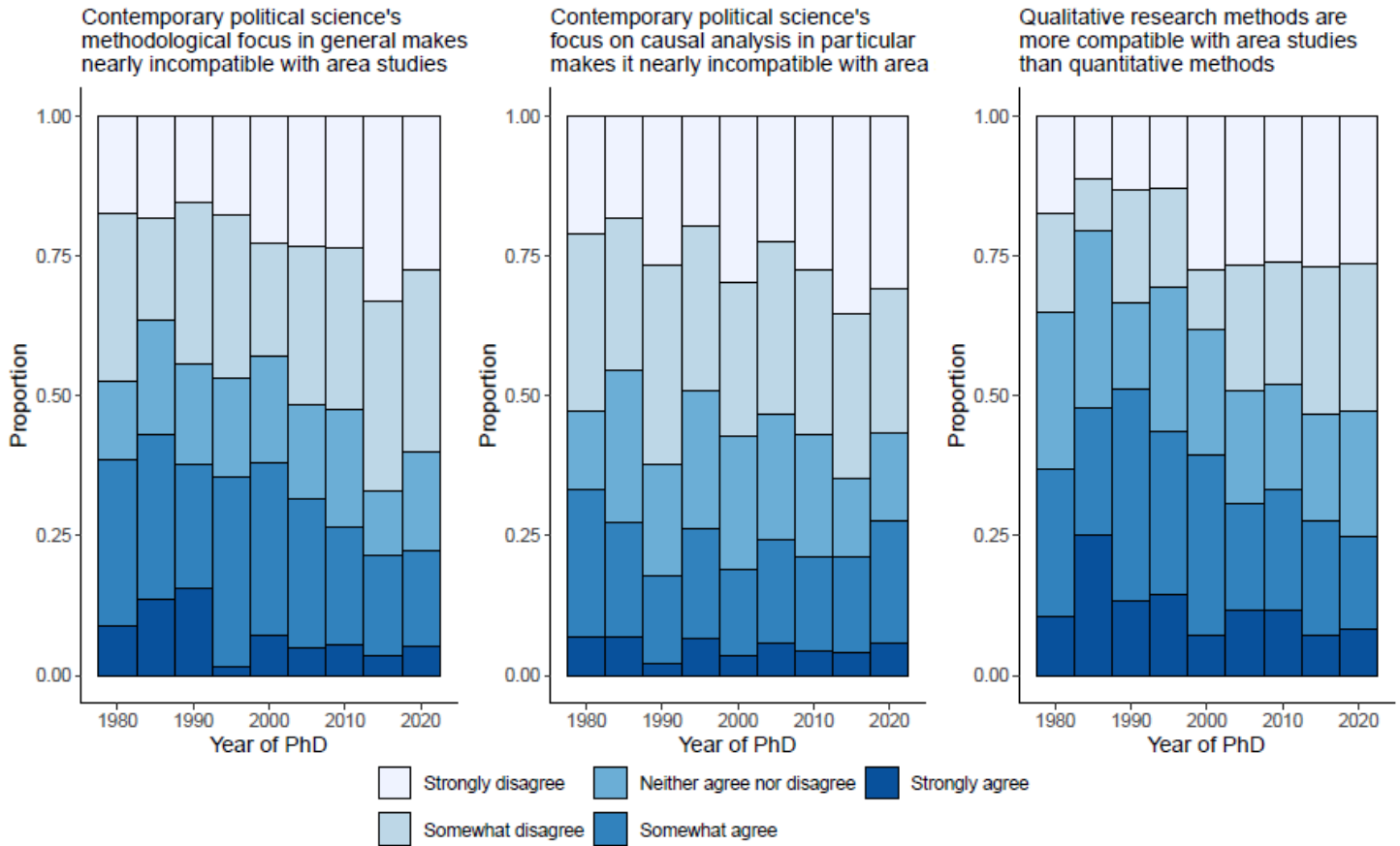
Indeed, the “credibility revolution” in quantitative social science—the shift toward experimental and other causally oriented research designs—arguably has increased the complementarity between area studies and much of the most methodologically sophisticated quantitative political science research currently being conducted. As comparativists have embraced the credibility revolution (see the trends in causal inference and experimental research in Figure 6 above), they have increasingly prioritized internal validity over law-like theories or context-free empirical generalizations. This shift reflects broad appreciation that *only by understanding history and context* can statistical patterns be given causal or theoretical interpretation (Malesky 2008).

As comparativists who are personally and professionally committed to engaging in the regions of the world in which we work, we find

these results to be reassuring. We give two cheers to these trends: one for the near consensus among political scientists in the value of area studies for research, teaching, and policy and another for the compatibility of empirical research, area expertise, and professional success. Yet, our optimism is guarded, because our data also contain evidence of more worrying developments that may eventually undermine current trends.

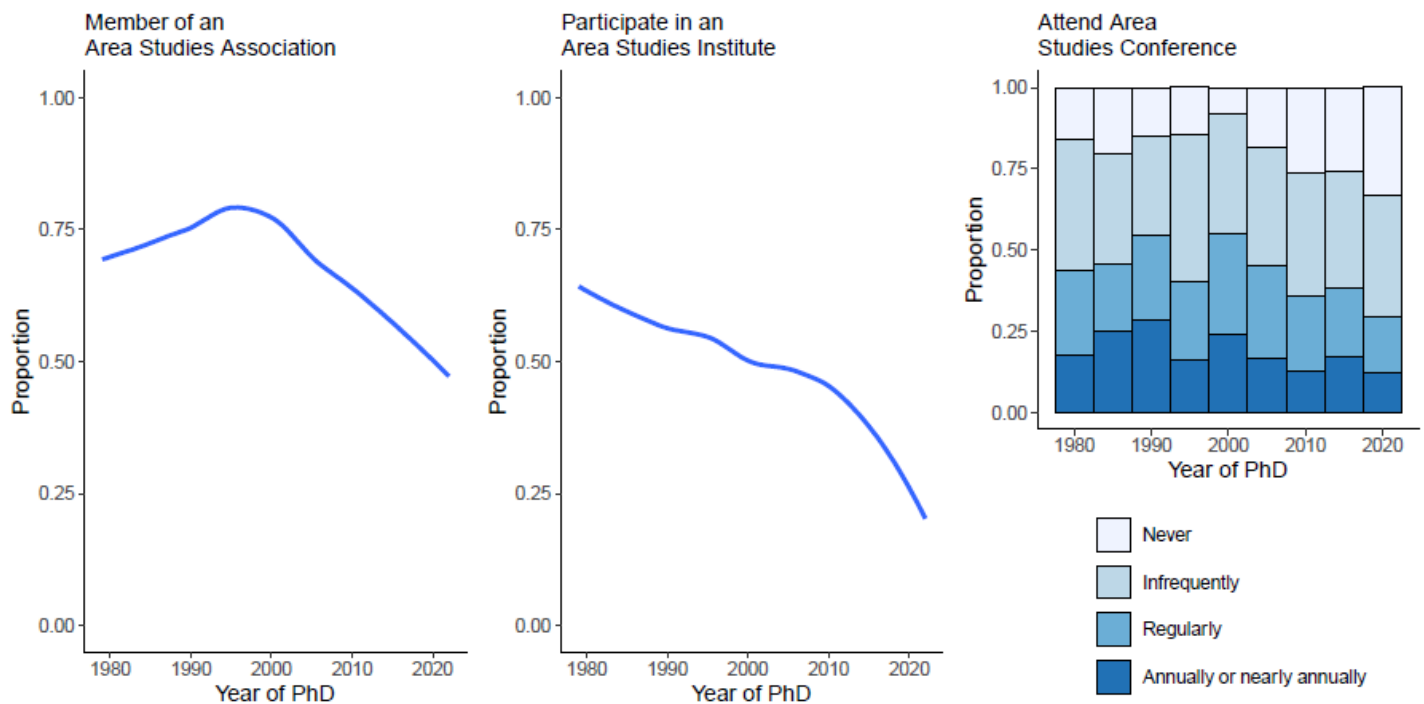
Most worrying is an ongoing change in what constitutes area studies knowledge, and with it, a decline in the willingness of younger cohorts to invest in regionally specific expertise. Our survey reveals a notable decline across cohorts in the length of fieldwork, the depth of area studies training in graduate school, and the intensity of foreign language coursework (figures available upon request). That said,

Figure 7: Perceived Compatibility of Area Knowledge and Social Science Methods by Year of PhD



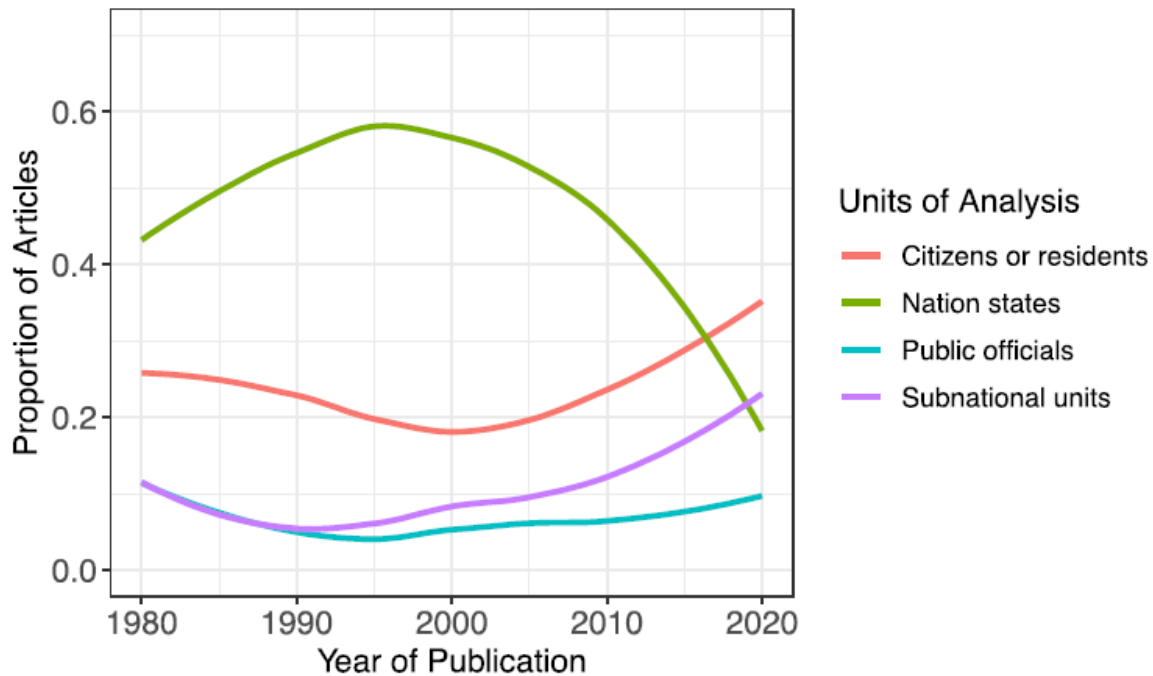
Note: Bins are five-year averages. N = 1134. Sample includes all respondents with CP as primary or secondary

Figure 8: Declining Engagement with Area Studies Communities by Year of PhD



Note: Bins are five-year averages. N = 1135.

Figure 9: Units of Analysis in Comparative Politics Articles (1980-2020)



Note: Trendlines are smoothed.

these trends may be less troubling than they initially seem: Our data indicate that among recent cohorts, more students enter graduate programs already proficient in foreign languages and well-traveled in their region of expertise. Additionally, in increasingly globalized US and European PhD programs, many PhD students are native speakers of languages used in the regions where they conduct research.

We also find that recent cohorts are less likely to participate in area studies institutions (professional associations, institutions, and annual conferences; see Figure 8). We interpret these trends as suggestive evidence that the practice of area studies has changed among political scientists. Professional investment in knowledge about a specific part of the world no longer necessarily implies deep engagement with other scholars of that part of the world from other disciplines.

The type of research that comparativists do is also changing. Even though published research in political science still predominantly focuses on particular countries or world regions, the object of inquiry has shifted. As the practices of younger cohorts have become less theoretical, less qualitative, and more quantitative and causally focused, recent work has become increasingly concerned with the behavior of individuals and sub-national units, and less so with the overarching characteristics of national political systems considered in their entirety (see Figure 9). In a sense, comparativists have responded to Hall's (2003) call to align ontology and methodology but in a manner that appears to be the opposite of what he intended. By focusing on the aspects of comparative politics that are most amenable to contemporary standards for quantitative causal inference, contemporary research often aligns ontology to methodology rather than the other way around.

We suggest that the trends we have described may be indicative of a

new model of area studies in political science. This “new” area studies remains committed to empirical description and causal discovery in specific world regions, eschewing law-like generalizations in most cases and combining formal theory, when it is used, with context-dependent analytical narratives. In that sense, the new area studies has intellectual roots in the traditional model of area studies that came under pressure in the 1990s. Nevertheless, the new area studies is far more quantitative and positivist in orientation than its predecessor, and scholarship is tied to the concerns of contemporary social science rather than to the concerns of history, arts and literature, and other humanistic disciplines.

There are both advantages and disadvantages to the newly emerging model of area studies. The new area studies approaches empirical analysis in a manner that is *narrow and focused*. Aspects of the institutional, cultural, or historical environment of direct relevance to causal claims are scrutinized in exacting detail, resulting in troves of original, regionally specific data and in credible tests of specific hypotheses. At the same time, information deemed less relevant to such exercises is often overlooked, limiting scholars' ability to scale up individual studies to offer broader macro-level insights into political systems. By contrast, the area studies that predominated in the mid- to late-twentieth century was *broad and systemic*. Through intensive immersion in the language, culture, and history of a region, combined with extensive fieldwork, researchers produced seminal works explicating how countries around the world were governed, and offered implications of their findings for comparative theory-building. Yet, a lack of attention to rigorous hypothesis testing meant that specific claims made within these ambitious works were less likely to rest on well-grounded empirical foundations.

We welcome the new area studies' emphasis on internal validity. However, without added investments in some of the key features that characterized the old area studies (extensive fieldwork, broad cultural immersion, attention to macro-political issues), we fear that comparativists may miss opportunities to broaden their contributions to knowledge. As we see it, the challenge facing the new area studies is to organize its internally valid but often unconnected findings into coherent and generalizable theories of political behavior. Thus, we echo Chandra (2015) and Geddes (2015) in concluding that even if the new area studies is often narrow by design, its substantive purchase is still unclear. The irony is that only by engaging more fully with area-focused research in other disciplinary traditions can scholars know if their narrow focus is preventing them from making more expansive contributions to political science.

Needless to say, important questions about this new area studies remain. One important limitation of our research is that our analyses of journal articles does not incorporate work published by political scientists in area studies or other more specialized journals, nor does it incorporate books. To the extent that research relying on qualitative or interpretive methods may disproportionately appear in outlets other than the journals in our sample, this too should be considered. The same holds true for formal theory, which may be more likely to appear in specialized political economy journals. Moreover, it may simply be too soon to discern whether the trends we discuss are truly indicative of a paradigm shift in how comparativists engage with area studies, or whether they instead merely reflect a more temporary blip driven by methodological trends that are only incidentally related to area studies.

Our research is ongoing. Future publications will elaborate on trends identified here in greater detail, as well as dig deeper into questions such as whether these trends vary across scholars with expertise in different world regions. For the time being, our hope is that by introducing evidence that area studies is by no means dead, but rather has assumed new forms, we can generate further research and discussion about these issues.

NOTES

¹ Only those respondents who completed at least 90 percent of the survey questions are included in our sample. Additionally, because survey modules about regional specialization were presented only to respondents who reported comparative politics or international relations as their primary subfield, our sample excludes the small subset of Americanists and political theorists who reported comparative politics as their secondary subfield.

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THE LIMITS OF GENERALIZABILITY WHY CONTEXT STILL MATTERS IN COMPARATIVE POLITICS

By Niloufer A. Siddiqui



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The debate around the place of area studies in the social sciences historically centered on a key distinction: while area specialists tended to “focus on the distinctive and unique,” social scientists searched instead “for broader regularities” (Bates 1996, 2). According to this frame, political scientists led with theory and sought to discover trends and patterns that could explain a phenomenon beyond its specific manifestation in the particular country under study. The goal, then, was to advance general knowledge about political institutions and political behavior that applied to all, or most, countries around the world. In contrast, many political and other social scientists perceived area studies specialists as distinct—and frequently inferior—because, by virtue of their focusing on only one region of the world, they were unable (and sometimes unwilling) to draw these same comparisons and broader lessons.

To a large extent, this debate mapped onto the methodological debate which in parallel characterized American political science more broadly. Proponents of this area studies-political science demarcation implicitly drew battle lines between qualitative methods and area studies, on the one hand, and quantitative methods and political science, on the other. Indeed, immersion into a particular country required language skills and a deep understanding of the region’s history, its various political and social actors, and complex local dynamics which extensive qualitative fieldwork could more effectively provide than could large-N quantitative studies.

The debate has, no doubt, evolved in the nearly 30 years since Bates wrote in these pages about growing “tensions” between political science and

area studies. New methodological techniques have come into vogue, shifting the quantitative-qualitative debate to new—but equally contentious—boundaries (see the credibility revolution, for example; Angrist & Pischke 2010). And as Gans-Morse, Gingerich, and Pepinsky demonstrate in their previous essay in this symposium, there now appears to be a growing sense among political scientists that area studies has a prominent role to play not only in informing policy but also in informing the academic discipline.¹

Yet, even as the disdain for area studies among some political scientists has subsided, we are still far from arriving at a consensus about the place of area studies within political science—and indeed what we gain by thinking of the two as fundamentally distinct. While we are transitioning away from the world Bates wrote about in 1996, this period—not unlike most transition periods—is messy, and relies on some flawed, and sometimes contradictory, assumptions. In this piece, I outline two sets of concerns that I hold about the current place of area studies within comparative politics. I write from the perspective of a scholar who studies mostly Pakistan, and occasionally India, and who began their research career primarily utilizing qualitative methods and who now increasingly employs survey experimental methods as well.

NOT ALL AREAS IN “AREA STUDIES” ARE CREATED EQUAL

First, while political science as a field is becoming increasingly comfortable with the idea of scholars studying just one country, only certain countries are accorded the privilege of being worthy of study unto themselves. That is, the burden of

generalizability—of drawing broader lessons that extend beyond the place under study—falls unequally across countries, and this inequality is reflective of global power dynamics in the international system.

Which countries hold this privileged status depends on their place in the broader international system, or on their place in the regions into which we have—sometimes implicitly—divided area studies. States which hold, or held, an outsized role in international politics are considered more valuable single-country cases. The United States, of course, is the archetypal example of such a country, so significant as to have its own subfield outside of comparative politics. But other countries, too, such as the United Kingdom, China, or India, hold this coveted status. Alternatively, the largest of the countries in the geographic regions into which area studies is divided are often accorded more power. Thus, Indonesia receives more attention within the comparative politics of Southeast Asia than do Thailand or Vietnam. Such countries are either believed to generalize to other contexts by default or are significant enough on their own that it does not matter if they generalize or not.

I suggest that this hierarchy in which comparative politics places countries is problematic not only in the abstract—reinforcing global inequality and a sense that some countries are more deserving of study than are others—but also because it contributes to maladaptive behaviors. For example, comparative scholars are under pressure to choose their country of study strategically. “Larger” countries in each sub-region of interest are given more weight (India, rather than Bangladesh or Nepal, for example, for those interested in South Asia) which leaves many countries around the world understudied in the field. Few incentives are provided to new researchers and PhD students to fill these gaps.

This imbalance relates to another peculiarity of the way in which areas studies manifests today. While many scholars study single countries, they are nonetheless expected by other political scientists to present themselves as “experts” of the entire region of which the country they study is part. India scholars, then, become South Asian experts; Indonesia scholars, experts of Southeast

Asia; Egypt scholars, Middle East experts, and so on. However, here too, we are in a world where not every country is created equal. It is more difficult for a Nepal or Bangladesh scholar to claim to be a South Asia scholar than an India scholar or less, frequently, a Pakistan scholar. This is the case despite the fact that in all likelihood the scholars specializing in the smaller South Asian countries know more about India than India scholars know about the smaller South Asian countries.

It seems, then, that the priorities of knowledge construction differ from the priorities of knowledge transmission, where specialized knowledge is expected and encouraged.

This pattern further contributes to a flawed sense that one is an “expert” of a region—frequently a very large and diverse region—by virtue of studying one country within it. A scholar could spend their entire career studying just Uganda but will be viewed by many in the political science community as an Africa expert. This impetus comes in part from the marriage between comparative politics and area studies, and the increasing norm of studying a single country but being “comparative” in one’s approach. Yet, such scholars may know very little about the rest of the region of which their country case is part. Indeed, while there are some benefits to dividing the world into regional silos, at other times the countries that fall within a region share in common with one another only the accident of their geography, or some other joint artefact of the past. By conceptualizing the world as a set of mutually exclusive bins, rather than taking a more open lens to other possible sociocultural configurations, we are likely missing several important research agendas.

At the individual level, the incentives for each scholar to market themselves as an area studies expert are clear. The political science job market frequently seeks expertise in a region

of the world, as universities and colleges need instructors to teach regional overview courses to eager undergraduate students. The comparative job market, then, is often organized regionally, advertising jobs for scholars of Latin America, or Africa, or South Asia. This regional focus appears in stark contrast to the hierarchy which frequently disregards area studies. It seems, then, that the priorities of knowledge construction differ from the priorities of knowledge transmission, where specialized knowledge is expected and encouraged.

Even here, however, few departments have room for more than one, or perhaps two, specialists of these regions. These experts then tend to reflect the larger countries. Indeed, if a department can only have one South Asia scholar, they would much prefer that scholar be an expert of India than any of the other countries in the region. This fact was made plain to me during a job talk when I was asked to highlight the extension of my research that I intended to conduct in Pakistan’s larger, more important neighbor.

Finally, “seam” countries that don’t fit neatly into these geographic zones are more likely to be ignored. For example, some North African countries are not considered part of “African Studies,” but they do not form a key part of Middle Eastern studies either. Until Afghanistan became an issue of global importance due to US intervention, it was also understudied within political science. Even today, it is unclear whether Afghanistan falls within South or Central Asia, or whether it should be considered part of the Middle East. This affects the types of questions that we ask about Afghanistan in mainstream comparative politics; it remains effectively a case study of US intervention gone wrong (some excellent studies on other aspects notwithstanding).

HOW THE METHODS DEBATE INFORMS THE AREA STUDIES ONE

The second concern I maintain about the current state of the new area studies is the extent to which it—like its predecessor—suffers from methodological divisions. As Gans-Morse, Gingerich, and Pepinsky demonstrate, today’s area studies employs causal, quantitative methods more frequently than

area studies did in the past. This is a promising move in so far as the turn to new quantitative techniques has encouraged sustained focus on single country cases and has also broadened the number and nature of scholars who could be categorized as “area studies.”

Yet, there remains a sizeable gap in dialogue and conversation between quantitative and qualitative scholars, even those studying the same countries. This is not merely due to methodological norms or conventions, but because the types of questions being asked by each type of scholar, and the type of inference they make, differs substantially. While the increase in single-country articles suggests, perhaps, a new acceptance of area studies within comparative politics, it disguises the ways in which quantitative studies are still privileged over qualitative studies in the field more broadly.

This methodological divide manifests, then, in skewed publication patterns, not only in what is published but how it is published. The “regional background” section of academic articles is often one of the first sections to be cut down when trying to meet word limits. Sometimes, the name of the country under study does not appear in the title, nor abstract, of the paper. This serves to make the country site incidental to the research question, rather than integral to it. This is a problem. We cannot merely view countries as blank slates, devoid of politics, in which researchers can intervene. Doing so results in incomplete analysis, and indeed can have more pernicious effects where scholars seek to emphasize the sameness of their places in order to downplay any situational circumstances that might limit the generalizability of their findings. While the old area studies did not shy away from uniqueness and difference, new area studies tries to paper it over.

Finally, it is worth thinking about how new area studies could benefit from adopting more qualitative methods, at the very least as the precursor to their quantitative interventions. Having language skills, understanding local context, recognizing actor-specific peculiarities allow for better, more informed surveys and field interventions. I am continually surprised at how difficult it is for certain survey experimental methods common in compara-

tive politics to be adopted to the Pakistani context—conjoint and list experiments, for example—or how certain, simple words are difficult to translate and meanings tricky to convey (ethnic identity, to give one example from my site of study). Understanding even simple norms can help eradicate a lot of confusion that may arise in analyzing data. To provide another quotidian example, those who have spent time in South Asia will recognize that collecting respondents’ age is not always straightforward; respondents often round up or down to the nearest five or ten (35 instead of 32; 70 instead of 67). Again, having the area-specific knowledge to anticipate and address these issues is important not just for normative reasons but because it means we end up with better data and stronger findings.

WHERE DO WE GO FROM HERE?

There is much to gain from analyzing and situating singular countries within larger, comparative frameworks. So too are there numerous advantages to the causal inference revolution that have allowed us to answer questions in a rigorous, falsifiable manner. Both of these characteristics of today’s comparative politics should be nurtured and encouraged.

At the same time, we must balance these advantages with the recognition that the messy politics and peculiarities of a country cannot merely be ignored. We must also balance the need for generalizability with the recognition that sometimes countries are worth studying because we don’t know enough about them, because they provide new sites to continually refine our theories, and because studying them can give rise to new agenda of inquiry. Leaving the study of these contexts to history, anthropology, or other humanities means that we are not using the tools, or asking the questions, that characterize political science, which is a loss much beyond our discipline.

NOTES

¹I follow Gans-Morse, Gingerich, and Pepinsky in defining area studies as studies “with a sustained focus on one part of the world.”

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THREE CHEERS FOR BUILDING ON POST-WAR AREA STUDIES

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In “Two Cheers for the ‘New’ Area Studies,” Gans-Morse, Gingerich, and Pepinsky celebrate two current trends in comparative politics: that political scientists express a “near consensus” in the value of area studies for research, teaching, and policy, and that there is “compatibility of empirical research, area expertise, and professional success” (p. 8). They draw these conclusions from a careful study of comparative politics, including a survey of political scientists and an analysis of publications in highly ranked journals. However, they base the study on a definition of area studies as research with “a sustained focus on one part of the world” (p. 3). Equating area studies with single-country or region-based research omits key aspects of area studies as historically defined – as *interdisciplinary research that aims to further a social, cultural, and political understanding of specific areas and to develop a universal social science theory*.¹ Considered from the perspective of area studies’ early proponents, Gans-Morse, Gingerich, and Pepinsky instead present strong evidence that area studies research in comparative politics is on life-support, if not yet dead. The question is whether it should be resuscitated or is better left alone.

THE POST-WAR VIEW OF AREA STUDIES

Area studies in the US gained prominence in the post-war period. Still spinning from two world wars in the first half of the century, the academy’s inability to meet military demands to better understand foreign lands and peoples, combined with questions over whether “wiser scholarship [could] have brought about a better understanding of world relationships and prevented war altogether,”² many saw area studies as the key to an effective study of international relations and lasting peace. Universities, foundations, and the

US government sought to strengthen area studies by establishing centers, founding professional associations, and funding projects. Scholars joined suit, and by the early 1950s, an APSA survey of 75 political science departments in US universities found that 308 of their 797 research projects focused on international relations and, of those projects, 198 (or 64%) were “area studies” research.³

In general, the goal was for area studies not only to provide a comprehensive understanding of countries and regions, but also to advance a universal theory of social science. Julian Steward, founder of the University of Michigan’s anthropology department, saw “the development of a universal social science” as a goal of area research.⁴ Jean Duroselle, Dean of the Faculty of Arts at the University of the Saar, argued area studies aimed, in part, at “working out a theory of international relations as a whole (...), in finding the fundamental data which explain these relations in general, independently of geographical areas.”⁵ Hans Morgenthau, one of the most influential international relations scholars of the 20th century, noted that “area studies are expected to contribute to the development of a universal social science which will arise from the isolation, analysis, and comparison of similar phenomena in different cultures.”⁶

Proponents viewed area studies research as inherently interdisciplinary. Robert Hall, a geographer who served as founding director of the University of Michigan’s Center for Japanese Studies and Chair of the Committee on World Area Research, wrote in 1947 that area studies required the participation of the humanities, natural sciences, and social sciences.⁷ The introduction to the 1952 UNESCO International Social Science Bulletin stated that a noteworthy feature of area

studies was the “opportunity for fruitful collaboration between specialists in different branches of study,” including “historians, geographers, sociologists, anthropologists, psychologists and linguists...”⁸

The challenge, as many noted, was not simply to bring together scholars in multidisciplinary collaboration, but to undertake truly interdisciplinary research.⁹ Morgenthau likened this to “representatives of different disciplines stand[ing], as it were, on the same hill looking in the same direction at the same object and try[ing] to discern the same thing about this object, be its nature, its movements, its influence upon other objects, or the influence to which it is subjected by other objects, and the like.” Using different theoretical and methodological instruments, these different scholars “are bound to see different qualities in that object. By communicating to each other what they have seen, the minds of all concerned correlate the results of the researches of the others with their own.”¹⁰

Research, thus, required a collaborative approach. Projects were to be team-based, led by well-placed specialists (presumed, at least by some, to be scholars of international relations).¹¹ In this regard, scholars undertook large collaborative projects and founded area studies centers. Duroselle, based in France, lauded the American approach. “The originality of the American scientists,” he wrote, “probably lies in the fact that they have devoted themselves to this form of study in a wider and more systematic manner and gone further in discarding the single author method in favour of large-scale teamwork.”¹²

Efforts to promote area studies met challenges, of course. Proponents debated questions such as what theoretical approaches best fit different countries, how to advance interdisciplinarity, and how to define which areas and countries to privilege (a problem Sid-diqui notes still haunts us today). Many voiced concerns that area studies scholars would be isolated, either geographically or functionally, from other regions and disciplines.¹³ In 1955, Marshall Powers noted that the University of Florida had anticipated this problem, requiring faculty to be “an area specialist and an active worker in his [sic] disciplinary specialization at the same time.”¹⁴

Proponents of area studies also faced detractors. This may have been, at least partly, because area studies presented “a new attack on the hard and fast conceptions of university teaching and research, a breach in the wall, which the established, well-defined, ‘recognized’ branches of study continually put up against new branches.”¹⁵ Some voiced concerns that area studies would not have a “theoretical core.” Reporting on a study of the status of area studies at 24 universities, conducted just after WWII, Hall described one administrative officer’s reaction: “I can imagine a man trained in area [studies] as being a most charming gentleman and interesting conversationalist,” he reported the officer saying, “but not as being a scholar.”¹⁶ Such comments resonate with more recent critiques of area studies in comparative politics.

Without understanding the intricacies of contexts (...), it is easy to base research designs on false assumptions and misinterpret results.

THE “NEW” AREA STUDIES MAY BE NEW, BUT IS IT AREA STUDIES?

Early supporters of area studies may recognize current critiques, but they would be unlikely to view the scholarship that Gans-Morse, Gingerich, and Pepinsky uncovered as a new and improved area studies. Their goal was to create a practice in which scholars with various disciplinary and methodological tools come together to conduct deeply grounded research examining critical problems, use findings from different contexts to effectively define and recognize scope conditions, revise theories in their respective disciplines, and then engage with scholars across disciplines and regional specialties to develop a universal theory of social science. This vision of area studies stands in stark contrast to the picture painted in “Two Cheers for the ‘New’ Area Studies.”

“‘New’ area studies” may be new, but it is not

area studies. Gans-Morse, Gingerich, and Pepinsky’s study reports a “notable decline across cohorts in the length of fieldwork, the depth of area studies training in graduate school, and the intensity of foreign language coursework.”¹⁷ They argue graduate students may have better language skills and more overseas experience than previous cohorts, which might partially mitigate this problem. However, there is reason to believe these initial skills do not make up for the loss of area studies practice.

Scholars of comparative politics today generally do not prioritize gaining deep, contextual knowledge of their research sites. As the study reports, “scholarship is tied to the concerns of contemporary social science rather than to the concerns of history, arts and literature, and other humanistic disciplines.” Most newly-minted PhDs also do not engage in regional studies associations and area study centers, which likely reflects both their own choices and their perceptions of the profession’s values. Similarly, collaboration has become more frequent, particularly as projects have become more complex and technical, but there is little emphasis on interdisciplinary teams. In short, Gans-Morse, Gingerich, and Pepinsky identify the prevalence of single-country- and region-based research, not a ‘new’ area studies in comparative politics.

DOES COMPARATIVE POLITICS NEED AREA STUDIES?

The turn away from area studies is detrimental to the discipline of comparative politics. The move away from interdisciplinary research, grounded in a deep understanding of context, limits the ability to design and develop research programs, establish scope conditions, and advance theories of comparative politics.

Studies in comparative politics require a nuanced, contextual understanding to design research and understand results. Princeton philosopher David Bowers noted in 1944 that, to understand a “civilization,” one must “view its institutions, mores, and ideals, not as isolated facts but as parts of a larger context, in which each confers and has conferred upon it, a significance that would otherwise be lacking. To see less than this is to see nothing at all.”¹⁸ The language is a bit dated, but the essence remains. Without understanding the

intricacies of contexts – the roles actors play, the obligations that bind them, and the power structures underlying them, it is easy to base research designs on false assumptions and misinterpret results. Interdisciplinary research, in which scholars with different tools and theoretical frameworks view the same object, is best designed to reveal these nuances.

This approach is also the foundation for determining scope conditions and advancing theory. Current research emphasizes causal identification at a specific place and time, but it often fails to sufficiently identify the contextual conditions to establish scope conditions. Moreover, much of the research is conceived, implemented, and interpreted without the interdisciplinary area studies engagement that fosters theoretical and methodological advancement.¹⁹ This limits theoretical advancement in comparative politics.

A PATH FORWARD OR BACK TO THE PAST?

The challenge is to restore the advantages of area studies approaches, without losing the empirical rigor and progress made in the past decades. I end with three recommendations that may allow us to do so, facilitating progress.

First, we need to recognize the value of area studies and promote engagement with the community. For administrators, this means making available the necessary time and resources for researchers to engage with area studies communities and, where necessary, undertake longer-term fieldwork. For established faculty, this means attending area studies conferences and university programs as available, encouraging graduate students to develop relevant area studies skills, and rewarding this knowledge in hiring, journal reviews, and other professional decisions. For graduate students, it means investing time in engaging with area experts outside the discipline.

Second, we need to promote specialization and collaboration. A 'return' to area studies need not come at the expense of disciplinary progress and methodological sophistication. Early area studies proponents valued interdisciplinary collaboration—something which may open opportunities for both theoretical and methodological cross-fertilization and advancement. The advantages of specializa-

tion and collaboration can be found in other areas as well. Research teams can benefit from the collaboration of researchers specialized in different tools, theoretical approaches, and area expertise. Graduate programs, too, may consider allowing students to specialize. All should be literate in quantitative and qualitative methodologies, but some may benefit more from shifting time from methods training to the development of relevant theoretical or area studies to the benefit of all.

Finally, there should be efforts to promote cross-regional dialogues over similar research interests. Establishing forums for sharing deeply grounded, area-based findings across regions will promote the study of comparative politics, as well as the areas. Integrating interdisciplinary, area-based research into the discipline will have a greater impact, promoting new findings and key theoretical revisions.

NOTES

¹ See, Steward, Julian H. (1950) *Area Research: Theory and Practice*. New York, NY: Social Science Research Council, Bulletin 63. p. 2. Available at this [link](#). Similarly, Jean B. Duroselle said "The best handy definition of an area study, till we have had time to work out something more accurate, is the scientific study of a region presenting a certain politico-social unity with a view to understanding and explaining its place and its role in international society." (1952) "Area Studies: Problems of Method," *UNESCO International Social Science Bulletin*, 4(4), p. 636. Available at this [link](#).

² Powers, Marshall K. (1955) "Area Studies," *The Journal of Higher Education*, 26(2), pp. 82. Available at this [link](#). See also Hall, Robert B. (1947) *Area Studies: With Special Reference to Their Implications for Research in the Social Sciences*. New York, NY: Social Science Research Council, Pamphlet 3, p. 21. Available at this [link](#).

³ No author. (1952) "Introduction," *UNESCO International Social Science Bulletin*, 4(4), p. 633.

⁴ Steward, 1950, p. 2.

⁵ Duroselle, 1952, p. 639.

⁶ Morgenthau, Hans. (1952) "Area Studies and the Study of International Relations," *UNESCO International Social Science Bulletin*, 4(4), p. 651. Available at this [link](#).

⁷ Hall, 1947, p. 50.

⁸ "Introduction," 1952, p. 634.

⁹ "Introduction," 1952, p. 635; Simey, Thomas S. (1952) "The Contribution of Sociology and Psychology to Area Studies," *UNESCO International Social Science Bulletin*, 4(4), p. 675-682. Available at this [link](#).

¹⁰ Morgenthau, 1952, p. 651.

¹¹ Hall, 1947, p. 49.

¹² Duroselle, 1952, p. 637.

¹³ Hall, 1947, p. 72.

¹⁴ Somewhat ironically, Powers continued, "Admittedly, however, this arrangement would be difficult to maintain with uniform effectiveness in every institution." 1955, pp. 85-86.

¹⁵ "Introduction," 1952, p. 634.

¹⁶ Hall, 1947, p. 29.

¹⁷ Gans-Morse, Jordan; Gingerich, Daniel; and Pepinsky, Tom. "Two Cheers for the 'New' Area Studies," p. 9.

¹⁸ Bowers, David F. (1944) *The Princeton Conference in American Civilization: A Description and an Appraisal*. Princeton, NJ: Princeton University Press, p. 5, cited in Hall, 1947, p. 26.

¹⁹ Karl Popper argued that even if various problems 'belong' primarily to distinct disciplines, their solution requires an interdisciplinary perspective. As he wrote, "We are not students of some subject matter but students of problems. And problems may cut right across the border of any subject matter or discipline." (See Popper, Karl R. (1962). *Conjectures and Refutations: The Growth of Scientific Knowledge* (New York: Basic Books), p. 67). For more recent evidence that interdisciplinarity may help advance theories, across disciplines, see Okamura, K. (2019). "Interdisciplinarity revisited: evidence for research impact and dynamism." *Palgrave Communications* 5, p. 141., and Greaves, J., & Grant, W. (2010). "Crossing the Interdisciplinary Divide: Political Science and Biological Science." *Political Studies*, 58(2), 320-339.



AREA STUDIES 2.0

EMBRACING AND EXPLAINING HETEROGENEITY TO HELP SOLVE REAL-WORLD PROBLEMS

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"Perhaps it's worth restating an obvious point. Empirical evidence on any given causal effect is always local, derived from a particular time, place, and research design."¹

This statement comes not from an area studies scholar's lament about the disadvantages of the rise of causal inference in the social sciences. Its authors are Angrist and Pischke in their 2010 piece lauding the credibility revolution and its salutary effects on economics research. While it is a bleak reminder of the limitations of social science research, it can also be read as a recognition of the importance of area studies. While social science tends to look for generalities and reward research that claims it, our methods direct us to recognize the importance of context, place, and time.

In this brief reflection on the Gans-Morse, Gingerich, and Pepinsky piece, with which I largely agree, I offer some cautionary analysis on the synergy between area studies and this revolution. As Pepinsky argued in an earlier piece, the return of the one country study is partially driven by the centering of causal inference in this revolution and the associated need to do narrow and focused studies in a single place.² The connection and symbiotic relationship between this methodological shift and traditional area studies is obvious and should be celebrated and noted. However, despite this potential synergy between causal inference, single country studies, and traditional area studies, there continues to be a large gap in the aspirations and expectations of area study specialists and many in the quantitative social sciences. This is no longer a gap between different methodological approaches or even a debate about theory versus empirics. It is

rather a difference in ambition and scope that is centered on the value of variation versus the value of generalization. Normal social science continues to prioritize generalizability and the reach of a finding to as many contexts and time periods as possible, despite the daunting nature of that task. Area study specialists are, by definition, more interested and *find more value* in the findings of a particular place at a particular time.

These may be simple differences of taste and intellectual orientation and should be able to co-exist under a large tent of intellectual inquiry. Some prefer to go deep and others to go broad in their research. However, as long as normal science prioritizes generalization, this tension between area studies and studies of causal inference is hard to rectify without downgrading area studies while continuing to elevate general theories or findings. However, the intellectual prioritization of a general finding should be interrogated, especially when problems of external validity are so severe. Moreover, if the main goal of social science research is to help understand and solve real world problems, general findings are not enough. Implementation always requires engagement with a particular place. One must understand the context and history of a place to make a general theory useful.

This is not a zero-sum competition between quantitative social science and traditional area studies. There are ways to combine the two approaches in such a way that science is advanced, policy prescriptions are more accurate, and the human "subjects" of our research benefit as much as possible. This essay explores these goals and makes a few recommendations to edge closer to them. I invoke the relationship be-

tween clinical practice and random controlled trials (RCTs) in medical science as a metaphor for how comparativists might think about area studies and experimental work. In both cases, researchers are presented with research subjects (patients or countries) with vast differences between them and many unknowable factors that can confound treatments. This necessitates the triangulation of methods, especially when trying to understand scope conditions of an experimental finding and ascertain external validity through comparison across different units. Given our focus on research of particular places, I also interrogate the prioritization of generalization over a focus on variation and unit heterogeneity, especially in applied social science where policy impact should be centered as the goal. Finally, alongside the use of meta-analysis across studies to explain variation, I advocate for the reinvigoration of comparative case studies and comparative historical approaches that prioritize explaining variation.

AREA STUDIES AND THE CREDIBILITY REVOLUTION: SAME BED, DIFFERENT DREAMS

The credibility revolution has brought back social science research that is narrowly focused on a causal relationship in a single setting and often at one point in time. To leverage the power of an experimental approach that models itself after the medical sciences with treatment and control groups, many comparativists have returned to single country studies that prioritize establishing causality between variables. Cross-national statistical studies that use observational data across many different national settings to establish correlative relationships have fallen out of favor, despite their ability to study a question across many different settings.

For many reasons, this shift has been good for area studies in the sense that single country studies are now part of normal social science. The accumulation of knowledge in the social sciences is now more built on the idea of incremental findings across many contexts, because these incremental findings are more robust than the previous approach of cross-national statistical analysis of observational data. This has had a beneficial impact on the professional opportunities of area experts. Studies of a single country are now routinely

published in the top journals without needing to justify focus on said country. If the study is well executed and the question is interesting, there is no longer any intellectual justification to state that X country is unimportant or that what happens in Y country should only be published in regional journals. These criticisms have long been a common frustration for area study experts, in particular when a country's political situation appeared unique or *sui generis*.

As with the iterative approach in the medical sciences, researchers should practice experimental research in the field with constant attention to the country in question in order to understand the context of the experiment, the variability of each place or context where the experiment is implemented, and the need for interdisciplinary collaboration with other country experts, practitioners, and research subjects.

However, there are also important differences between the goals of the credibility revolution and those of traditional area studies, as the Lust essay in this issue deftly covers. The concerning survey findings discussed in the Gans-Morse piece is a product of this tension. For example, the return of the single-country study and professional acceptance of single-country articles should be good for area studies by centering the need for requisite skills and knowledge of specific places during graduate school. This includes intensive language training, area study courses, which encourages interdisciplinary perspectives, and extended time for fieldwork. As their survey findings attest and as Lust argues, so far, the return of single country studies in the social sciences has not done much to revive the attention to area studies skills in graduate school training. This has long term

impacts on how scholars early in their career define themselves and socialize professionally. As the survey demonstrates, political scientists do not tend to engage much with regional studies associations. This reduces opportunities for connections to scholars in other disciplines. Thus, few research projects are built with interdisciplinary teams.

These tensions and gaps between area studies and the social sciences is still largely due to the elevation of generalizability as a key goal while area studies prioritize deep understanding of single cases and contexts. But general findings are not enough to engage in research that is relevant to the policymakers and communities that we study. How can single-country experts engage with traditional area studies such that their research is more than just "one data point" in the long journey toward accumulation of general knowledge.

BEYOND THE RCT:

WHAT ELSE CAN SOCIAL SCIENCE LEARN FROM THE MEDICAL SCIENCES?

Social science's interest in experimental research, especially the use of randomized controlled trials, as a "gold standard" in research methods stems from its original use in the medical sciences and its contribution to drug discovery, the connection between disease and lifestyle, and environmental causes of disease. However, even in the medical sciences, RCTs are part of a broader continuum of research that includes clinical practice, case studies, qualitative research, and observational studies. In medical fields, RCTs are critiqued for similar reasons as in the social sciences, including issues related to external validity and fit for individual patients.³ As a general practice, medical doctors engaged in RCTs for research and publication success also are clinicians, seeing patients and engaging in patient care often in relation to the drugs or other treatments in their experimental work. Their work is deeply iterative, going between experimental, highly controlled and constrained trials with patient care that is individualized and interdisciplinary.⁴

This dynamic between experimental research and patient care could be seen as a metaphor for a vision for "normal" social science

and area studies that is deeply connected and iterative. As Lust discusses in her essay, a single-country study that is not deeply knowledgeable about the context of the country and is unaware of interdisciplinary research in related fields IS NOT area studies. It is thin social science and ultimately probably unable to explain research results and likely heterogeneity between the country of study and other countries. As with the iterative approach in the medical sciences, researchers should practice experimental research in the field with constant attention to the country in question in order to understand the context of the experiment, the variability of each place or context where the experiment is implemented, and the need for interdisciplinary collaboration with other country experts, practitioners, and research subjects. This is also a more ethical and humane approach to social science research.⁵

It is important to note that this dynamic between experimental work and contextual area studies research is not necessarily a methodological division between quantitative and qualitative work. Contextual research could still be quantitative. However, the attention to interdisciplinary knowledge does encourage mixed methods, collaboration between qualitative and quantitative approaches, and triangulation between multiple research methods and disciplinary approaches.

PUTTING THE COMPARATIVE BACK INTO COMPARATIVE POLITICS

This iterative approach between general social science and area studies research fosters an inherent interest in comparison and heterogeneity across different national or regional settings, which is the founding justification for the subfield of comparative politics. The rise of single-country studies, focused on causal inference, has shifted us away from comparative research, research that aspires to explain cross-national differences, not only macro general trends or microlevel differences between individuals.⁶

New initiatives such as the Metaketa Initiative seek to widen the scope of the credibility revolution in political science through collaborative research and coordinated field experiments across multiple national settings. This initiative is intended to improve the reliability

of experimental studies by aggregating the results of multiple studies, done in a coordinated manner, on the same question. In this way, the Metaketa Initiative is explicitly comparative, as it seeks to use coordinated multiple studies across different national settings. However, as the authors note, the Metaketa Initiative is geared towards the goal of generalizability, not toward comparison:

One sensible solution to the challenge of generalizability is to combine the results of multiple studies on the same topic. Such “meta-analysis” is commonplace in some physical sciences, and it appears from time to time in the social sciences, too. Aggregating evidence from multiple studies may give us greater purchase on whether – and even why – results differ across distinct contexts, or whether, instead, findings point us in the same direction across settings.⁷

This first Metaketa project of information and political accountability deployed similar experimental studies across six countries. The researchers found little evidence that providing voters with reliable information on candidate quality significantly changed voting behavior. The authors conclude that the generalizability of the finding is more reliable, given the wide agreement in results across multiple contexts. “Nevertheless, we feel substantially more confident in the generalizability of conclusions drawn from multiple studies implemented in different settings than we would from any single study carried out in one setting.”⁸

Metaketa holds significant promise as an initiative to encourage quantitative researchers to engage deeply with their single country cases while also using the collaborative framework to engage in comparative analysis that explores heterogeneity between countries. However, given the consistent findings across the six countries in this initial study, generalizability can still be achieved, and comparison avoided or unnecessary. However, it is still unclear how comparative meta-analysis will explain heterogeneity. A narrow focus on differences in study design might unnecessarily detract away from institutional or cultural differences between the countries in question. Therefore, meta-analysis cannot be the only solution to explaining heteroge-

neity. The field of comparative politics needs to do more to integrate the rise of field experiments with other traditional methods of the subfield, including comparative case studies and comparative historical analysis.

This synergy between traditional area studies, field experiments, and comparative analysis can be realized through co-authored work, collaborative initiatives like Metaketa, or mixed methods research by individual scholars. This requires methodological diversity and humility. It requires a clear acknowledgement that none of our methods alone serve as a gold standard for research rigor or sophistication. Moreover, generalizability should be downgraded as a goal more superior than explaining results in a single place or explaining heterogeneity between places.

Gans-Morse, Gingerich, and Pepinsky are right to cheer for the potential return of area studies to the discipline. Lust’s caution that we aren’t there quite yet acknowledges the persistent gap between the interdisciplinary approach of area studies and the current credibility revolution. Initiatives like Metaketa emphasize the need to compare across units and to grapple with heterogeneity and the intractable problem of external validity in the credibility revolution. Comparative politics, at its core, offers a solution, which is the goal of comparison not generalization.

NOTES

¹ Angrist and Pischke, “The Credibility Revolution in Empirical Economics.”

² Pepinsky, “The Return of the Single-Country Study.”

³ Rothwell, “External Validity of Randomised Controlled Trials”; French, “Can Evidence-Based Guidelines and Clinical Trials Tell Us How to Treat Patients?”

⁴ Liberati, “Conclusions. 1.”

⁵ Phillips, “Ethics of Field Experiments.”

⁶ “More than Good Intentions.”

⁷ Dunning et al., “The Metaketa Initiative,” 17.

⁸ Dunning et al., 9.

⁹ Dunning et al., 413.

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HONORABLE MENTION OF GREGORY LUEBBERT PRIZE FOR THE BEST BOOK IN COMPARATIVE POLITICS

THE FOUNDING OF MODERN STATES

(New York: Cambridge University Press, 2022)

An interview with the author, Richard Bensel

WHAT MOTIVATED THIS PROJECT?

In 2007, I presented a paper, "Emerging from the State of Nature: The Construction of Rules, Rulers, and Membership in Legislative Assemblies," to the Congress and History Conference held at Princeton University. At the Princeton conference, I contended that there was no way in which a legislative assembly or constitutional convention could be democratically organized through the processing of individual preferences. One of the implications of this argument was that rational choice theories concerning legislative decision-making were always going to be incomplete because they could not encompass the creation of a legislative assembly. This contention caused some controversy at the conference and, after some years of thinking over the problem, I formalized the argument and published it as "The Opening Dilemma: Why Democracies Cannot Found Themselves" in *Studies in American Political Development* 36:2 (October 2022): 104-119.

One of the key elements in this analysis is that, despite the "opening dilemma" that all legislative assemblies must encounter, they, in fact, did successfully organize themselves. The question was how they managed to organize and, at the same time, retain their claim to be representative democratic institutions. On a parallel track, I realized that the analysis could be applied to the founding of national states because they, too, managed to found themselves by resolving, through arbitrary processes that relied on simple fiat, the same opening dilemma. That led me to investigate the historical processes through which modern states have been founded: three of them (the English Constitution, the 1787 American Constitutional Convention, and the National Assembly during the French Revolution) are conventionally considered "democratic" foundings while three others (the Soviet Union, Nazi Germany, and the Iranian Revolution) are usually thought to be "non-democratic." In all six cases, there was an arbitrary decision in the founding of these states that enabled the founders to both claim that they were acting as the "will of the people" and, at the same time, resolve the "opening dilemma." This resolution, in each instance, melded a "transcendent social purpose" for the new regime, the erection of central state authority, and the "will of the people" in a way that had important and durable implications for their subsequent development.

WHAT SURPRISED YOU MOST

AS YOU WERE DOING THE RESEARCH FOR IT?

When I began this comparative study, I had only very general knowledge of five of the six cases (I knew the American founding fairly well). Although the amount of research varied a bit, I spent at least a year of intensive research in each of the six (about ten years in total from the beginning of the writing until publication). The most surprising element in this research was the discovery that the founders in each instance both fervently believed in the ideological frame which guided them through the process and that they were acting in accord with the "will" of their respective peoples.

WHAT DO YOU HOPE WILL BE THE MOST LASTING

CONCLUSION FROM THIS BOOK IN 10 OR 20 YEARS?

I hope that this book will encourage scholars: (1) to revisit these historical episodes as they attempt to reconcile the logical consistency of political processes with the ideological beliefs of political actors; (2) place greater emphasis on the way in which cultural norms allow people to overcome logical contradictions; and (3) realize that a belief in mythological concepts such as the "will of the people" are a necessary element in the political stability of modern states.

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HONORABLE MENTION OF GREGORY LUEBBERT PRIZE FOR THE BEST BOOK IN COMPARATIVE POLITICS

THE ORIGINS OF SECULAR INSTITUTIONS: IDEAS, TIMING, AND ORGANIZATION

(New York: Oxford University Press, 2022)

An interview with the author, H. Zeynep Bulutgil

WHAT MOTIVATED THIS PROJECT?

In Political Science, we have a vibrant literature on the emergence of bureaucratically sophisticated states, nationalism, and regime type. What I found striking is the relative absence of works that focus on the emergence of secular states, i.e. states that minimize the control of religious rules and organizations over the public and private lives of individuals. Given the far-reaching implications of institutional secularization on individuals' most basic rights and intimate decisions, my view is that the topic deserves as much social scientific attention as the other dimensions of state formation.

There is also the empirical puzzle. Relevant theories, which include Tocquevillian arguments on throne and altar relations or modernization arguments on economic development, do not easily account for why secular institutions emerge. Contexts with an experience of close throne-altar relations vary remarkably in their trajectory of institutional secularization. They include early secularizers such as France in the 18th century as well as Saudi Arabia today. Economic modernization argument also raises empirical puzzles: for example, Pakistan and Morocco, neither of which have adopted secular institutions, have levels of urbanization and literacy higher than cases such as Turkey or Albania in 1920s when those countries adopted secular institutions.

WHAT SURPRISED YOU MOST AS YOU WERE DOING THE RESEARCH FOR IT?

For me there were two surprising parts. First, secular states were by no means a foregone conclusion even in Western Europe. If you study debates about state reform in 17th and 18th century Europe, the idea of separating religious and state institutions is both intellectually rare and subject to censorship. Most reformers want to harness the organizational capacity of religions rather than push them out. What allows for the unlikely emergence of secular movements is the spread of Enlightenment ideas through printing press in contexts where the censorship mechanism is weaker. This argument, for example, explains why France was an early-mover and Spain was a late-comer. In France, the censorship mechanism simply did not work as most printers and thinkers could move across the border to Switzerland or the Dutch Republic and smuggle back their ideas in print. In Spain, this type of easy escape was not possible.

Second, I was also surprised to find that the timing and sequence of secular and religious parties is critical to understanding the geography of institutional secularization today. In contexts in which secular parties emerged early, they could build a grassroots organization and compete effectively with religious parties. In contexts in which secular parties emerged around the same time or after religious parties, they were not competitive. Among other things, this argument sheds new light on comparative political history of Europe and the Middle East. In Europe, the Vatican curbed liberal, pro-suffrage Catholic movements in early 19th century and unwittingly allowed seculars decades during which they could establish grassroots networks before they faced Christian Democrats. In much of the Middle East, secular parties did not have this decades long breathing space to build organizations before they faced religious rivals.

WHAT DO YOU HOPE WILL BE THE MOST LASTING CONCLUSION FROM THIS BOOK IN 10 OR 20 YEARS?

I hope the book's first lasting conclusion is its theory and empirical findings, which underscore the diffusion of ideas and organizational timing of competing political movements as key mechanisms in secular state formation. I would also be really delighted if the book sparks a lively literature not only on the emergence of secular state formation but also on the emergence of secular political parties, which, unlike religious or left-wing parties, have been understudied. Ultimately, I hope the book will make the study of secular institutions as integral a part of the literature on modern state formation as it is for studies that focus on bureaucratic capacity and national identity formation.

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HONORABLE MENTION OF GREGORY LUEBBERT PRIZE FOR THE BEST BOOK IN COMPARATIVE POLITICS

MAKING GENDER SALIENT: FROM GENDER QUOTA LAWS TO POLICY

(New York: Cambridge University Press, 2022)

An interview with the author, Ana Catalano Weeks

WHAT MOTIVATED THIS PROJECT?

My book focuses on the policy impacts of electoral gender quota laws in Europe. The use of quota laws has increased rapidly over recent decades. Today, more than 60 countries around the world have adopted them. These laws have given women new access to political power, but does it translate into meaningful policy change in the direction of their political preferences? This is the practical question that motivates the book: not only if, but when and how do quotas lead to policy change for women?

To investigate the policy impacts of quota laws, I visited four countries – two which had quota laws and two similar countries which did not. This book weaves together statistical analysis of party agendas and government policies with the qualitative interviews I gathered in my fieldwork.

WHAT SURPRISED YOU MOST AS YOU WERE DOING THE RESEARCH FOR IT?

One surprising finding is that much of the policy change I document occurs within political parties on the right of the political spectrum. Left-wing parties often already had many women in their ranks, not least because many had internal party quotas already. So quotas lead to bigger change within Christian Democratic, conservative, and far right parties, and these women were able to shift their parties' priorities and positions on some issues. I focus on work-family policies, because this is an issue that women across parties tend to prioritize more than men. I find that women on both the left and right moved work-family policies in a more gender-egalitarian direction – that is, more well-paid, shared parental and paternity leave (which encourages gender equality in both the household and labor market) and less maternity-only leave and cash benefits (which does not). Women achieved this at times by working within parliamentary committees and at times through ministerial appointments, depending on the institutional context.

Another surprise was just how normalized people became to gender quotas. I interviewed several politicians who strongly opposed quotas in the past, but they had changed their minds. In the US, "quota" often seems to be a word people use in a pejorative sense, so I found that fascinating (and encouraging).

WHAT DO YOU HOPE WILL BE THE MOST LASTING CONCLUSION FROM THIS BOOK IN 10 OR 20 YEARS?

I argue that in order to understand when quotas, or women's descriptive representation, matter to policy outcomes, we need to consider not only which issues divide men and women but how these issues fit into the main lines of party competition in a country. For example, there is a large gender gap over social spending, but because this is so well-crystallized in the context I study – parties are clearly organized in opposing positions around it – I make the case that quotas are unlikely to lead to policy change on it. It is those "uncrystallized" issues (in the words of Jenny Mansbridge) which are not well-aligned with the main left-right political dimension that offer the most potential for policy change. Absent the presence of women in office pressing for change, parties have strategic reasons to ignore these issues because they could detract from core issues and divide men and women within the party. I show that work-family conciliation is one of these issues. Women are significantly more progressive than men when asked questions about whether it is okay for mothers of young children to work, and egalitarian change in family policy has come from (especially women politicians in) parties across the left and right. However, I think there are many other issues likely to be characterized by large, cross-cutting gender gaps – e.g., violence against women, "pink" taxes, or indeed quotas themselves. These are worth further exploration.

Empirically, my book adds to a long line of studies that show descriptive representation matters. Quotas are effective not only at boosting numbers of women in office but also at translating at least some of women's political preferences into policy outcomes. In Europe, quotas lead to more egalitarian work-family policies. And these changes don't only benefit women, but all working parents.

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HONORABLE MENTION OF GREGORY LUEBBERT PRIZE FOR THE BEST BOOK IN COMPARATIVE POLITICS

UNITY THROUGH DIVISION: POLITICAL ISLAM, REPRESENTATION AND DEMOCRACY IN INDONESIA

(New York: Cambridge University Press, 2022)

An interview with the author, Diego Fossati

WHAT MOTIVATED THIS PROJECT?

This project was motivated by my desire to contribute both to the study of democracy and to our understanding of Indonesian politics. In the field of democratic backsliding, researchers often assess the state of a democracy based on its protection of liberal rights. While this is undeniably important, I believe that other dimensions of democracy, such as representation and participation, are equally worthy of study. This book aims to underline the significance of these aspects for democratic legitimacy, and to challenge the prevailing yet overly simplistic equation of liberalism with democracy.

As for Indonesia, its democratic achievements are noteworthy when appraised in comparative perspective. Yet, most area studies scholars have offered a more somber perspective, portraying its politics as being dominated by predatory elites, devoid of substantive debates, and driven by clientelism. This book offers an alternative viewpoint by highlighting the underappreciated role of ideological and cultural debates in giving meaning to political participation and representation. I thus wanted to shift the analytical focus to a strength of Indonesia's democracy, rather than discussing one of its perceived weaknesses.

WHAT SURPRISED YOU MOST AS YOU WERE DOING THE RESEARCH FOR IT?

Perhaps the most surprising element I encountered was the puzzle that I discuss at the beginning of the book. This puzzle is derived from public opinion data which show that Indonesian citizens reported greater satisfaction with democracy at the same time as scholars were arguing that the quality of democracy in the country was deteriorating.

This finding was intriguing for two key reasons. Firstly, it revealed a disconnect between ordinary citizens and academic elites in their conceptualization and evaluation of democracy. This discrepancy is not a result of the public's ignorance about democracy, but rather reflects the narrower liberal perspective typically adopted in academic analysis. Secondly, this pattern was not easily explainable using existing approaches to the study of Indonesian politics, presenting an opportunity to develop an original argument.

WHAT DO YOU HOPE WILL BE THE MOST LASTING CONCLUSION FROM THIS BOOK IN 10 OR 20 YEARS?

In 10 or 20 years, I hope this book will be recognized as a compelling example of scholarship that approaches the study of democratic development from an original, less commonly adopted perspective. I hope that it will serve as a reminder of the potential for less conventional analytical strategies to yield new insights.

More broadly, I hope that the book will contribute to consolidating the idea that democracy is multi-dimensional, and that academic research on democracy should account for this complexity. Democratic development often involves difficult choices and trade-offs between equally desirable goals, rather than simple dichotomies and straightforward trajectories of democratic advancement and regression. I hope this perspective will inspire future research to adopt a similarly nuanced and multi-faceted approach to studying democracy.

I also hope that this book will familiarize scholars of comparative politics with the Indonesian case, enriching their analyses and perspectives. Ideally, this book will be remembered as offering an informed, original and needed account of a critical juncture in Indonesia's journey towards democracy.

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HONORABLE MENTION OF GREGORY LUEBBERT PRIZE FOR THE BEST BOOK IN COMPARATIVE POLITICS

OUTSOURCING REPRESSION: EVERYDAY STATE POWER IN CONTEMPORARY CHINA

(New York: Oxford University Press, 2022)

An interview with the author, Lynette H. Ong

WHAT MOTIVATED THIS PROJECT?

Economic growth necessitates the redistribution of resources—including land, labor, and capital—from one group to another. Autocracies may be more effective than democracies in forcibly redistributing resources by means of coercion and subsequently [withstanding the pressure](#) exerted by the losers. Yet, [persistent coercion may invite backlash](#), provoke resistance, and delegitimize the regime. If a regime bent on growth enacts a policy that redistributes resources and subsequently causes dissent among the losers, how does the regime balance coerced compliance with minimized backlash?

In *Outsourcing Repression*, I use China's [ambitious urbanization scheme](#) as a window of observation for state policies that entail immense resource reallocation to study the practice of state power to gain compliance and mute dissent. [Rapid urbanization](#), by its very nature, involves vast population relocation and resource redistribution among groups. In China, property owners have been coerced into compliance, their voices muffled as the state tries to fast track the urbanization process. To be sure, popular resistance has arisen against land taking and housing demolition, yet no country has ever pulled off the spatial transformation and relocation of people on the scale achieved by China while simultaneously maintaining social stability.

WHAT SURPRISED YOU MOST AS YOU WERE DOING THE RESEARCH FOR IT?

When I began my field research for this project in 2012, I was inundated with tales of thuggish harassment and threats during my field interviews with the Chinese citizens who had undergone land grabs and housing demolition. It was compelling and sufficiently novel in China that I decided to investigate the prevalent, yet understudied, roles of hired violent actors (thugs-for-hire) in the implementation of government policies. In the book, I contrast the violent actors with non-violent actors, namely community volunteers and enthusiasts, who are mobilized by the state to persuade citizens to comply with eviction orders.

WHAT DO YOU HOPE WILL BE THE MOST LASTING CONCLUSION FROM THIS BOOK IN 10 OR 20 YEARS?

If state has the capacity to outsource repression, the book thus invites

readers to reimagine the contours of state power.

The book advances two major arguments about outsourcing repression which lies at the core of *everyday state power* over society. First, the state implements unpopular policies effectively and swiftly by using the "everyday repression" of outsourced violence. Carried out by "thugs-for-hire" (TFH), it is an expedient state strategy to impose its will on society. Second, the state extracts citizen compliance and resolves state-society conflicts by mobilizing the masses, which involves marshalling a small segment of society—the brokers—to gain acquiescence from the larger society. Because mass mobilization is conducted through social networks, brokers with social capital legitimate state repression, in turn minimizing resistance and backlash.

Together, these nonstate actors allow the state to deeply penetrate society and mobilize societal members within it. Their complicity in state's actions – by threatening citizens to part with their land or leave their homes, or "persuading" recalcitrant households to sign consent papers using relationship traps – serve to augment state power in implementing challenging everyday policies.

Theoretically, everyday engagement of these nonstate agents by the state to coerce pushes the boundary on what scholars currently understand as state repression. Because the repressive capacity of the state is a central element of its strength, I also invite readers to reimagine what constitutes the notion of state power.

China serves as a case study in the book to exemplify a state that practices vigorous *everyday state power* over society to extract compliance and stifle dissent. The authoritarian nature of China's political system serves to strengthen this power; but it is not a necessary condition. I draw on comparative cases—notably, pre-, and post-democratized South Korea, and India, as the most similar and the most dissimilar case studies to demonstrate cross-country external validity of the arguments.

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HONORABLE MENTION OF GREGORY LUEBBERT PRIZE FOR THE BEST BOOK IN COMPARATIVE POLITICS

RESISTING BACKSLIDING: OPPOSITION STRATEGIES AGAINST THE EROSION OF DEMOCRACY

(New York: Cambridge University Press, 2022)

An interview with the author, Laura Gamboa

WHAT MOTIVATED THIS PROJECT?

I came of age in Colombia shortly before Álvaro Uribe came to office and just a few years after Chávez attained power in Venezuela. Despite the polarizing narratives about these presidents, they always seemed very similar (and equally dangerous) to me. For most of my life, Venezuela was the successful neighbor, the country Colombians migrated to. The fact that Chávez was able to coopt state institutions and stay in power while Uribe was not seemed very puzzling to me.

I started thinking more systematically about that puzzle in a course paper during the first years of my PhD. The existing literature (which at the time focused almost solely on what drove populist leaders to power) did not quite address it. It blamed the erosion of democracy (an ill-defined concept then) on state crises, weak economies, weak parties, and popular presidents. But all of those were factors that applied to Uribe as well.

It was at that point that Scott Mainwaring suggested I transformed this paper into my dissertation. It was a huge puzzle and a risky and challenging endeavor. But I was lucky to be at an institution and under the mentoring of scholars who care and are excited about big normatively important questions regardless of size or methodology. Not only did they encourage me to chase this puzzle, but they pushed me to think out of the box and bring forward a bold argument to answer it.

WHAT SURPRISED YOU MOST AS YOU WERE DOING THE RESEARCH FOR IT?

The importance of congressional politics in Colombia. For two reasons. First, when we talk about Álvaro Uribe and the survival of Colombia's democracy, the Constitutional Court is usually front and center. At the end of the day, it was the one that stopped the reelection and other damaging legislation. Nobody talks about congress. Yet, as I show in the book, the role that minority legislators played in curbing Uribe's anti-democratic reforms was absolutely crucial for these rulings.

Second, I never expected congressional politics to be this interesting. I had always thought of legislative politics as one of the least interesting subjects in Political Science. It was all about the types of laws pro-

duced, who produced them, how they were related to the executive, or how many or what kind of veto players were required to pass legislation. It always seemed very formal, very static. Seeing how minority coalitions used (and still use) legislative procedure to influence Court decisions was something fascinating that I never expected and has opened a whole new research agenda for me.

WHAT DO YOU HOPE WILL BE THE MOST LASTING CONCLUSION FROM THIS BOOK IN 10 OR 20 YEARS?

I hope the lasting conclusion of my book is that weak oppositions can defeat strong authoritarian leaders and prevent the erosion of democracy. Tempering the conditions that bring potential autocrats to power in the short term or expecting them to change their behavior, I think, is almost impossible. Our best shot to protect democracy from this kind of democratic reversal is to figure out how to respond to them. My book offers some ideas, but it is not the be-all end-all of it. My hope is that it sparks all sorts of research on how to counter executives with hegemonic aspirations who come to power democratically. In the near future, I hope to read books and articles expanding on topics I barely touch (like the international community), refining the theory I put forward, and/or finding new strategic pathways that may help opposition hinder the erosion of democracy.

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HONORABLE MENTION OF GREGORY LUEBBERT PRIZE FOR THE BEST BOOK IN COMPARATIVE POLITICS

WOMEN AND THE ISLAMIC REPUBLIC: HOW GENDERED CITIZENSHIP CONDITIONS THE IRANIAN STATE

(New York: Cambridge University Press, 2022)

An interview with the author, Shirin Saeidi

WHAT MOTIVATED THIS PROJECT?

My main motivation for writing this book was to challenge the ways in which war, state formation, and citizenship were conceptualized in the scholarship in political science, international studies, and international relations. In the conventional literature in political science, citizenship is mostly associated with democracies as a state construction tool that prevents revolutionary politics and stabilizes centralization of state power. However, when it comes to non-Western states, citizenship is not examined for its substance or long-term impacts. My book demonstrates that the Islamic Republic has a sophisticated governing structure where the most notable form of citizenship is an activist one.

My book destabilizes binary thinking around state construction, and as such offers a novel look into post-1979 Iranian politics. What's interesting is that most reviewers of the book, which have been generous, nevertheless miss this important dimension and instead focus on a narrow area studies lens (i.e., looking at what the book contributes to Iranian Studies). In some instances, it is the gender component of the book that captures reviewers' attention. In some cases, there is even an exaggerated focus on my politics and access to field sites in Iran. These approaches to reviewing the book are endemic of a larger problem in the social sciences. Non-Western case studies, but especially Iranian Studies, which is a highly securitized area studies in both the West and Iran, continue to be viewed as places to understand for policy purposes and not as sites of political novelty.

WHAT SURPRISED YOU MOST AS YOU WERE DOING THE RESEARCH FOR IT?

I was surprised by how unprepared I was for the overwhelming love, support, and care that people in Iran offered me as I wrote this book. Students, archivists, librarians, professors, activists, and at times even government officials placed their own safety and credibility at risk to help me to gather important archival material.

WHAT DO YOU HOPE WILL BE THE MOST LASTING CONCLUSION FROM THIS BOOK IN 10 OR 20 YEARS?

It is my hope that in ten or twenty years, scholars and students will view my book within a broader category of scholarship in political sci-

ence that refused to accept the foundational myths of international relations. I wish to be read alongside other serious decolonial thinkers who actually did the work that gives platform to and highlights the knowledge, care, and insight that people in non-Western contexts have utilized to craft a more just world, despite all the odds against them. In short, I hope to be read in relation and in community with my colleagues writing about current transformations in the international system.

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HONORABLE MENTION OF GREGORY LUEBBERT PRIZE FOR THE BEST BOOK IN COMPARATIVE POLITICS

CITIZENSHIP IN HARD TIMES: HOW ORDINARY PEOPLE RESPOND TO DEMOCRATIC THREATS

(New York: Cambridge University Press, 2022)

An interview with the author, Sara Wallace Goodman

WHAT MOTIVATED THIS PROJECT?

I had spent several years researching how immigrants become citizens, tracing this new policy trend (civic integration) that made sure immigrants knew liberal democratic values and behaved in ways that demonstrated a genuine commitment to them. I kept thinking to myself: "Wow – policymakers seem to have a very clear understanding of what it takes for immigrants to become 'good' citizens. I wonder how much people who are already citizens believe these things." So, I turned the lens around. What does it mean to be a good citizen? The fact that I started to pursue an answer to this question as the institutional guardrails of democracy began to erode in the US and Europe was coincidental but made me expand the project to think about whether norms shift in response to democratic hard times, and if norms of citizenship—which theoretically rises above and precede partisanship—become partisan themselves.

WHAT SURPRISED YOU MOST AS YOU WERE DOING THE RESEARCH FOR IT?

Most people still agree that there are core obligations of good citizens, and these include liberal democratic values like toleration, vigilance, and forbearance. I probably forgot that because I live in an information silo like most everyone else. I was less surprised that individuals updated their understandings of what good citizens are obligated to do, be, or believe if it was good for their partisan "team". Sadly, I was prepared to see that outcome.

WHAT DO YOU HOPE WILL BE THE MOST LASTING CONCLUSION FROM THIS BOOK IN 10 OR 20 YEARS?

Consensus around liberal democratic values is fragile. My book shows that, in democratic hard times, citizens mobilize to protect their "side" even if it conflicts with democracy. Put another way, we see what happens when partisan goals supersede civic goals. Diverse institutions that promote zero-sum outcomes, like the US and the UK, only exacerbate the problem. This book is the first to insert the citizen's perspective and agency into the backsliding discussion. It complements work on elites and institutions while reminding us that citizens are the source of democratic legitimacy. National unity around group goals is also a guardrail, one whose erosion threatens the very foundation of democracy.

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INTRODUCTION TO THE 2024 APSA ANNUAL MEETING COMPARATIVE POLITICS SECTION

Graeme Robertson Susan H. Whiting
University of North Carolina University of Washington
APSA Comparative Politics Division Chairs

This year's APSA is taking place in an extraordinary year when billions of people in disparate countries are participating in elections—some free and fair and some less so—that will legitimize leaders in those countries. The world, it seems, is at a potential turning point, when the forces of nationalism, the mobilization of identities, and increasing global flows of refugees and migrants seem set to end definitively the 30-year, post-Cold-War era and replace it with a new world (dis)order.

It seems especially timely then, that our conference theme this year should be *Democracy: Retrenchment, Renovation, Re-imagination*. These issues, of both threat and, crucially, of possibility, dominated the submissions to the Comparative Politics Division this year and are well-represented in the panels selected and created for the conference.

CP co-sponsored theme panels will take place in the context of several mini-conferences; we highlight four panels here. The mini-conference on Learning from Global Democratic Challenges and Innovations (I) includes two CP theme panels on "Countering Illiberalism in Liberal Democracies"; the first focuses on "Constitutional Perspectives", while the second examines "Political and Civil Society." The Mini-Conference on Political Parties and American Democracy also features two CP theme panels: "New Approaches to Political Participation in Comparative Perspective" and "Left Parties in Advanced Capitalism."

Perhaps understandably, political scientists seem currently more focused on identifying the threat to democracy, whether it be from backsliding, populism, far right parties, or polarization. Each of these topics will be represented in a range of panels supported by the section. This is also a time of extreme environmental threat in a context where the natural sciences have done their job of defining the problem and proposing answers, while social scientists still struggle to come up with effective political solutions that will facilitate progress. The challenge of climate change is definitely growing as a subject of research by political scientists, and this concern is reflected in the program.

The section also reflects the rising interest in the field in the politics of policing and state capacity (both repressive and productive) at the local level, while autocracy and how it works remain among the hottest topics in the field, with multiple panels exploring authoritarian government and governance across the globe. Protest too, a perennial topic in political science, plays a substantial role in the program, with panels examining the causes and effects of mass protest and the effects on both authoritarian regimes and democratic development.

All told, the more than 60 panels, lightning rounds and round tables supported by the Comparative Politics Division demonstrate clearly the discipline's continued commitment to addressing real-world problems with the highest degree of theoretical and empirical rigor. As our panels show, however, there is no one understanding of what rigor means. Instead, we have an epistemologically, methodologically, and geographically diverse group of presentations from senior and emerging scholars.

The Comparative Politics Section reception will take place Thursday September 5, beginning at 7.30pm at [Milkboy](#). The address is 1100 Chestnut St, and it is located 2 blocks away from the Marriott.



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