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RESISTING THE AMERICANIZATION OF COMPARATIVE POLITICS

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In what is almost required reading for graduate students in political science, Barbara Geddes (2003) implores scholars in comparative politics to think both big and small in her book, *Paradigms and Sand Castles*. Our questions, guided by our passions, should be ambitious, but the evidence we mount in the service of answering them should be precise. In this way, comparativists can both have their eyes on the prize of understanding long term social and political transformations that have been of major importance to generations of people, and also slice off manageable pieces of these questions for microscopic scrutiny, thereby contributing incrementally to our understanding of important political phenomena.

Seen from the vantage of the early 2000s, this advice was undoubtedly sound. But soon thereafter, several forces – both methodological and economic – were simultaneously set into motion that made small questions, and small answers, appear to be the only safe strategy for junior scholars. These forces are driving what I see as the Americanization of comparative politics, a phenomenon that has three broad attributes. First, the unreflexive importation of hypotheses from the American context into comparative studies without an explicit comparative lens; second, the burgeoning of experimental re-

search designs (both field and survey) that are “behavioral” instead of institutionally focused; and third, the publication arms race that begins ever earlier and which has shifted the intellectual medium in which younger comparative cohorts present their research.

This essay provides a rough sketch (a “practitioner” history in the admittedly incomplete sense in which Vitalis (2016) uses the term) of the intellectual and methodological trajectory of political science. I then argue that the one of the fields with which I am most familiar, gender and politics, is well poised to help us resist the Americanization of our field. I provide an example from my recent book wherein the similarities of franchise extension are revealed when America is explicitly theorized as one case among many. In closing I articulate a political economy understanding of the Americanization of Comparative Politics that is currently afoot.

Inter-American and Comparative Relations

In an essay on the intellectual linkages between American and Comparative Politics Kimberly Morgan (2016, 168) argues that over time the two fields have cross-pollinated, but that the increasing pull of behavioral and electoral re-



search draws scholars of American politics in their own, parochial, direction. Although political scientists have always been concerned to some degree with elections in a broad array of countries, a lack of individual level survey data, and growing understanding of the methodological problems of ecological inference, meant that behavioral research did not truly blossom until Gallup conquered the continents sometime after the Second World War (see Igo 2007 and Achen and Schively 1995, Chapter 1).¹ Looking back, one can surely argue that the 1960s was an extremely fertile time in political science, but EE Schattschneider (1969, 8) warned that the behavioral revolution threatened to produce a “mountain of data surrounding a vacuum” (1969, 8).

From the 1970s through the late 1990s, as computing power increased and local and international institutions made new data series widely available, scholarly ability to analyze large-scale datasets improved. At the same time, the growing prominence of Economics in both the real and academic worlds heralded the arrival of rational choice as a new theoretical apparatus to guide investigation. The fascination with formal theoretical models (and precise microfoundations) was, for some, a welcome change. But for others, formal theoretical models threatened the dominance of “theory driven” research as opposed to research agendas driven by “problems” (Green and Shapiro 1996).

The turn of the century resistance to quantitative methods brought forth two massive changes in the discipline: First, the “Perestroika” movement of the early 2000s, which led both to the founding of *Perspectives on Politics*

and to an increasing push within APSA and top grad programs for qualitative (if not interpretative) methods courses, pushed back against the rising dominance of researching involving mathematics (Yanow and Schwarz-Shea 2010). Second, an extremely productive literature on historical institutionalism was born. The writings of Hall and Taylor, Pierson, Thelen, Steinmo, and Mahoney, to name a few, allowed for a return to big questions and a concern with processes, junctures, and shifts. The institutionalist surge has had lasting effects, demarcating what Capoccia and Ziblatt (2010) called the “historical turn” in comparative politics, trickling down into the study of politics and gender (Krook and Mackay 2010; McBride and Mazur 2010) and to be increasingly influential field of comparative political theory (Simon 2014).

Publication Driven Research

And yet, in spite of all of this counter-hegemonic momentum, from where I sit as one of the discipline’s junior (if no longer young) members, progress seems to have stalled.

Put simply, instead of problems or theories driving our research, it feels as if our research has become publication driven. That is, the questions we ask are guided by our beliefs about whether (and even more cynically *where*) research on that type of question is publishable.

It is in the realm of publication-driven research that the Americanization of the field is taking place. At the crudest level, it involves projects that take a novel method or minor finding developed in the American context and applying it without augmentation to a different country. This type of project, which takes the form “does

1. Let it be noted that one of the first questions that behavioralists were concerned with was understanding the gender vote gap in a period when women had just won suffrage rights, and that one of the first scholars to pioneer ecological methods in political science was a lady social scientist, Inez Goltra (see Ogburn and Goltra 1999; Achen and Shivley 1995, 7).

X cause Y” does not require any country-specific knowledge nor any real discussion about why a causal process found in one place may, or may not, be activated in a different institutional or social context.

Instead of letting American politics colonize our field, I think we should push comparativists to be more comparative.

Another slightly more self-aware variant on this theme comes with statements like the following: “I want to do the American Politics of X country.” What doing the American Politics of X country means is to apply the set of questions that the scholars of congress, the courts, or elections have studied in the United States to a different country setting, but without any explicit comparison to the U.S. The desire to do American politics somewhere else is surprisingly common among entering graduate students, who sometimes express confusion about why, then, they should be labeled as comparativists.²

To be fair, the state of our knowledge of typical subject areas in American Politics – legislatures, courts, of the pathways to political office, and even of public opinion – is less developed outside the global north, even today. But my concern is that the lack of an explicit comparative frame (with other countries in the same region, or even with the U.S.) portends Schattschneider’s vision of tons of data in a theoretical vacuum.

America as part of the conversation

Instead of letting American politics colonize our field, I think we should push comparativists to be more comparative, and, even better, to insist America deserves a place as one case among many. Luckily, the explicit comparison of other countries with the U.S. is already present in several enclaves within comparative politics, including in my home turf in gender and politics.³

Much of the earliest work on gender and politics was related to women’s ascension to positions of power in the United States. The work of Virginia Sapiro, Wilma Rule, and others, was foundational for thinking about how status and gender ideology impacted when and where women could become political actors. For a time, comparative scholars of gender were less interested in legislative attainment than they were in understanding the role of women’s movements in promoting women’s rights and in the processes of democratization, and the way that different regimes could produce different legacies of gender equality (e.g. the work of Lisa Baldez, Georgina Waylen, Mala Htun, Lee Ann Banaszak, Laurel Weldon, to name a few).

As both survey research and parliamentary data collection became easier, and as gender quotas blossomed onto the international scene, comparativists turned their attention to trying to understand the institutional differences that

2. Many Americanists might argue that they do compare, but just across states in the US. I’m in favor of sub-national comparisons (and agree with many of Richard Snyder and Isabela Mares’s separate writings on the subject) but think they are particularly apt when embedded in a larger, explicitly comparative framework. See too Kuo (in press).

3. America has always been an important case in the welfare state literature, but America-as-a-case is also thriving in historical political economy. Several recent books delve into the specific historical and institutional differences that have driven variation in electoral systems, representation, clientelism, and women’s rights. These include a book by Ahmed (2013) on the origins of electoral institutions, by Jusko (2017) on electoral geography and representation of the poor, Bateman (2018) on how disenfranchisement was critical to democratic projects, Kuo (2018) on when business interests work against state institutions and when do they work for them, a recent dissertation by Perera (2018) on public unions and mental health care, and Teele (2018) on the interaction between social movements and electoral politics in the quest for women’s suffrage. Newer research on political development primarily compares the U.S. to Europe, but older studies on long-term developmental consequences of different imperial experiences (such as Acemoglu and Robinson, and Engerman and Sokoloff, and Hartz) compared the U.S. with Africa, Asia, and Latin America.



promoted women in leadership positions, citizens' tastes or distastes for supporting female candidates, and the efficacy of quotas for getting more women into politics.⁴ More recently, there has been an extremely rich literature detailing the strategic bases of quota expansion (see Bush 2011; Weeks 2018; O'Brien and Rickne 2016; Clayton and Zetterberg 2018).

Many comparative scholars of gender have found it difficult *not* to engage with the U.S. scholarship, even though it appears that the U.S. literature is oblivious to the fact that there are other countries in the world where women have made considerably more progress in politics. This pressure to engage with the U.S. literature has, however, made the institutional insights from gender and comparative politics richer (e.g. Piscopo 2019; Barnes and Beaulieu 2019). And many important works show that attention to gender enhances our understanding of comparative politics writ large. An example from my book is instructive.

Forging the Franchise: The Political Origins of the Women's Vote, which compares women's enfranchisement in the U.S. with the trajectories in France and the UK, gives an implicit demonstration of how a rich literature dedicated to the United States missed the theoretical forest for the trees. For many decades, the U.S. literature has focused on the singularity of racial politics for creating chasms within the suffrage movement and for halting the progress in the various states. While the racial cleavage was instructive for U.S. suffrage politics, I argue that at a higher level of relief it operated in a very similar way to the religious cleavage in France, and to views about the "Irish Question" in the U.K. In

other words, placing the U.S. as one case among many revealed that the mechanics of franchise reform were similar across countries.

In each country, suffragists considering whether to build a large coalition or a small one thought about the size of the already extant male franchise, and determined which type of reform to fight for (whether limited to the wealthy or white, or unlimited universal rights) depending on their ideas about how the rest of women, who may not share their political beliefs or economic status, would vote. Politicians voting on suffrage bills were informed by similar calculations, weighing the potential benefits available to their party against the risk involved in expanding the franchise to such a diverse group of people.

If I had taken my cues only from the U.S. literature, I would probably have shied away from any explicitly comparative frame. There are so many interesting state level twists and turns that one could easily focus on just that. Moreover, had I realized how difficult it is for a non-Americanist to try to publish on the United States, becoming an area specialist on the U.S. might have seemed like the safest strategy. But a theory of suffrage politics that emerges from an understanding only of the American case would have gotten bogged down by thinking primarily about how minor differences in procedural rules (such as what type of majority was required for electoral reform, or how many bills could be put to referendum per year) impacted suffragists' chances. Instead, working first on the U.K. case, I returned to the U.S. with more macro level ideas in mind: people cared about whether Ireland should be free, and they had ideas about how women would vote on the Irish question. Divisive politi-

4. The work of Mona Krook, Susan Franceschet, Sarah Childs, Rosie Campbell, Leslie Schwindt-Bayer, Aili Tripp, Michelle Taylor-Robinson, and Alice Kang can get you started.

cal issues like this informed not only what legislators did, but also how suffragists formed their own coalitions. Similar logics appeared in the U.S., where smaller movements with narrower demands were the purview of the South, worried as the Southern women were that white supremacy would be threatened should the black women vote.

Studying a major historical moment, and one of the best organized social movements of all time, required going deep in the weeds. Once I began to grasp the complexity of the cases, it was clear that any hope of testing an abstract theory of politics could only be folly. Although there is a cool small-N natural experiment in the UK chapter, and a clever proxy for religious entrenchment in the chapter on France, nothing in my book is particularly well identified. Thus, many aspects of the project felt risky, like permanently swimming against the current.

Yet there were some rewards for the risk. I gathered enough original data on things like political machines in the U.S., and the spread of the Women's Christian Temperance Union, to be able to contribute to others' research on comparative political development. In addition, my book provides a modular theoretical apparatus that should apply to many other movements for and instances of franchise reform.

Finally, and in the spirit of more comparison, the book contends that taking the case of women seriously adds to our understanding of comparative political development in substantive ways. In the conclusion, I argue that by focusing only on a handful of cases of male franchise reform, the democratization literature has failed to understand that groups can get credit for their own emancipation even if they do not take up arms against the state. Abandoning this (highly gendered) notion of agency opens up new ways

of thinking about how overlapping inequalities and deep-seated social cleavages impact the formation of social movements and the opportunities for equality around the world.

A Political Economy Account of Americanization

There are two forces that, I believe, have undergirded the Americanization of our field, one methodological and one economic. On the methodological side the so-called causal inference "revolution" emerged, taking first development economics and then American politics by storm. In the most brazen (and my favorite) statement on the subject, Green and Gerber (2014) pronounce that learning from observational research is illusory: without setting into motion the phenomenon that we seek to study, we have no hopes of understanding causal relationships. Sweeping a century of research under the rug, the "randomistas" also hoped to get rid of the last vestiges of area studies, and in so doing to remake comparative politics in the image of the increasingly technical American Politics field.

The causal inference juggernaut fed off of the economic lull of the second half of the aughts. In 2008, the onslaught of the financial crisis and the beginnings of the Great Recession reverberated throughout academic institutions. Universities tightened their ladder lines and moved towards increasing casualization (i.e. adjunctification) of their teaching portfolios (Thelen 2019), leaving the number of good jobs to appear vanishingly small. (One even heard horrifying tales of offers rescinded due to budgetary changes.)

The pressure of a contracting labor market (made more acute by the growing reserve army of political scientists stationed in multi-year



postdocs) brings with it the temptation to seek out quick fixes and to follow prophets. It has also led to the relentless professionalization of PhD candidates and an increasing emphasis on publication during graduate school. Since job market candidates now need to demonstrate not only the *potential* for academic excellence, but also *evidence* of “productivity”, the result is a near arms race among graduate students and junior faculty who, in their individually rational attempts to diversify their own portfolios, have now collectively lost the prisoner’s dilemma.

The pressure to perform productivity has led to a sense in many corners that we are asking small-

er and smaller questions. As someone once said to me in conversation: We are teaching students to count, but have they learned to think? Instead of letting comparative politics be Americanized, and to avoid the trivialization of our profession (Falletti 2016), we should push for comparative frames to be brought to America. This requires thinking through the ways that specific institutional contexts shape and bind political behavior, affect electoral and legislative outcomes, and constrain the possibilities for equality, both gendered and otherwise. Ultimately, this means a blurring of the subfield lines. The answers may not all be small, but messiness is a price I’m willing to pay for asking bigger questions. ●

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