

# Debating the State of Comparative Politics

## Views From Qualitative Research

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Gerardo L. Munck and Richard Snyder's analysis is an important effort to systematically characterize the state of comparative politics. The article quite appropriately insists that our portrayals of research practices in the subfield should be grounded in real data. Munck and Snyder's own characterizations accordingly are based on a large-*N* sample of published articles in three leading subfield journals. This data-driven approach to debating the subfield is not often used, and Munck and Snyder are to be applauded for employing it here. The approach helps us move beyond some stale debates that are grounded in false conceptions of research.

In this short reply, I discuss Munck and Snyder's essay from the standpoint of qualitative research and qualitative methodology (my own research orientation). From this perspective, at least two themes from the article are especially noteworthy and merit close consideration. The first concerns the extent to which qualitative research is practiced in the subfield. Munck and Snyder show that 63.3% of articles in the leading subfield journals are mainly qualitative, whereas only 36.7% are mainly quantitative. Furthermore, they find that far less than half of all articles use any kind of deductive methods and only a tiny percentage (4.4%) use formal deductive methods. As Munck and Snyder suggest, however, the situation may be quite different outside of the subfield journals. Hence, we need to look at other journals to develop a more comprehensive understanding of the kind of research that is published on comparative politics.

A second theme concerns Munck and Snyder's discussion of the methodological problems that hinder the generation of knowledge in the subfield. Some of these problems are uncontroversial for qualitative researchers; in

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fact, Munck and Snyder find that certain issues are handled especially well in qualitative research (e.g., linking data to concepts). But others are controversial, and I focus on them here. In doing so, my intention is not to suggest that qualitative research is flawless (or that quantitative research is especially flawed). Rather, I want to use this discussion to try to orient the debate over qualitative research in the most productive directions.

### **A Different Universe: Findings From the General Disciplinary Journals**

Munck and Snyder note that their findings from the three subfield journals may not be generalizable to other kinds of journals. In particular, they note that research on comparative politics published in general disciplinary journals “could diverge systematically from the kind of material published in the three journals on which this article focuses” (p. 7). Is this, in fact, the case?

To explore the issue, a research assistant (Larkin Terrie) and I examined work published in *The American Political Science Review* (APSR), *The American Journal of Political Science* (AJPS), and *The Journal of Politics* (JOP). We looked at all articles ( $N = 169$ ) that focused on comparative politics in these journals for the same years analyzed by Munck and Snyder (i.e., 1989, 1992, 1995, 1998, 2001, and 2004). Given our interest in issues pertaining to qualitative research, we coded empirical articles on comparative politics according to whether they are mainly qualitative or mainly quantitative (see Table 1). We also coded comparative politics articles (empirical and non-empirical) according to whether they engage in formal deductive modeling, informal deductive modeling, or no deductive modeling (see Table 2).

The data in Tables 1 and 2 suggest a dramatic contrast with the results obtained by Munck and Snyder (see also Bennett, Barth, & Rutherford, 2003). Most strikingly, qualitative research on comparative politics is rarely published in the disciplinary journals (i.e., about 8% of all publications). Of the three journals, APSR publishes the most qualitative research, but its articles are still nearly 90% quantitative. Furthermore, a majority of all publications (53%) engage in deductive modeling, and one fourth (26%) use formal modeling. Thus, with these journals, deductive theorizing in the study of comparative politics is as common as not, and formal modeling is not at all rare.

It is important to be clear what these new findings suggest. They do not call into question Munck and Snyder’s argument that their data represent much of the best work on comparative politics. Munck and Snyder are doubtless

**Table 1**  
**Methods of Empirical Analysis by Journal**

	AJPS		APSR		JOP		Total	
	<i>n</i>	%	<i>n</i>	%	<i>n</i>	%	<i>n</i>	%
Qualitative	3	(6.6)	6	(11.8)	2	(4.7)	11	(7.9)
Quantitative	42	(93.3)	45	(88.2)	41	(95.3)	128	(92.1)
<i>n</i>	45		51		43		139	

Note: AJPS = *American Journal of Political Science*; APSR = *American Political Science Review*; JOP = *Journal of Politics*.

**Table 2**  
**Methods of Theorizing by Journal**

	AJPS		APSR		JOP		Total	
	<i>n</i>	%	<i>n</i>	%	<i>n</i>	%	<i>n</i>	%
Formal modeling	9	(17.0)	32	(44.4)	3	(6.8)	44	(26.0)
Informal modeling	25	(47.2)	15	(20.8)	6	(13.6)	46	(27.2)
No modeling	19	(35.8)	25	(34.7)	35	(79.5)	79	(46.7)
<i>n</i>	53		72		44		169	

Note: AJPS = *American Journal of Political Science*; APSR = *American Political Science Review*; JOP = *Journal of Politics*.

correct that *World Politics*, *Comparative Political Studies* (CPS), and *Comparative Politics* publish much of the best work in the field. Instead, the new data here show that the leading disciplinary journals publish an unrepresentative sample of this work. Scholars who employ mainly qualitative methods are virtually shut out from publication in the most prestigious discipline-wide journals.

More generally, I would caution that readers not jump to the conclusion that qualitative research dominates even the subfield journals, especially when we take into consideration issues of prestige among these journals. CPS, now the flagship of the subfield, increasingly publishes quantitative research (Bennett et al., 2003). By sampling the full universe of 204 substantive CPS articles published during the 2001 to 2005 period, for example, we found that 60% of its articles are now quantitative. Of the three journals analyzed by Munck and Snyder, *World Politics* is often regarded as the most prestigious outlet. Yet its publications on comparative politics are not overwhelming qualitative; they appear to be fairly evenly split between quantitative and qualitative

research. This leaves *Comparative Politics* as the one journal that primarily publishes qualitative research.

In short, with respect to the publication of qualitative and quantitative work in comparative politics, I would characterize the current situation as follows: (a) The most prestigious disciplinary journals publish almost entirely quantitative work. (b) *World Politics* and *CPS* are pluralistic, though the latter is becoming increasingly quantitative. (c) *Comparative Politics* publishes mostly qualitative articles.

As for formal modeling, Munck and Snyder show that it is not commonly used in the subfield journals. However, the situation is again very different with the disciplinary journals (as Munck and Snyder hypothesize it might be). Formal methods are commonly used in comparative articles in *APSR* and not uncommon in *AJPS*. As a consequence, scholars who use these methods—or even informal varieties of rational-choice theory—may command disproportionate influence in the field. In addition, *CPS* publishes an increasingly large percentage of articles that use informal or formal deductive methods (Bennett et al., 2003). These facts may explain the great attention that rational-choice theory in comparative politics has received, an occurrence that otherwise would seem puzzling in light of its relative absence in certain subfield journals.

## Appraising Qualitative Research Practices

Munck and Snyder are right to call on scholars to take methods seriously and to put methodological issues on center stage in future debates about comparative politics. Nevertheless, I argue that some of their specific criticisms of qualitative research methods are based on partial or problematic indicators. I suggest that better measurement itself requires a more qualitative (or at least “case-oriented”) approach.

Munck and Snyder argue that qualitative researchers fixate on the country as a unit of observation and thus fail to employ within-case analysis in their research. Yet their measurement of within-case analysis as the percentage of articles that employ country-time periods or subnational regions seems problematic. As a large methodological literature suggests, the fundamental basis of within-case analysis is the identification of “causal-process observations” (to use the language of Collier, Brady, & Seawright, 2004). With causal-process observations, one needs neither to move away from the country level of analysis nor to substantially increase the *N* to achieve powerful leverage for causal inference. In fact, a small number of causal-process observations at the country level may lend decisive support for or against a given theory (George & Bennett, 2005; McKeown, 1999).

Regarding the scope of generalization, Munck and Snyder conclude that “Most comparativists do not attempt to provide generalizations of even a moderate scope” (p. 22). They draw this conclusion from evidence that shows that most researchers analyze only a small number of cases. However, they do not code the domain of cases to which comparativists believe their arguments are applicable. It is quite possible that scholars who analyze single cases or small *Ns* occasionally or even frequently attempt to provide broader generalizations than the cases under analysis. In fact, some methodologists go so far as to say that case studies by definition attempt to provide these kinds of generalizations (e.g., Gerring, 2004). One may be skeptical of the validity of these generalizations by case-study and small-*N* researchers, but that concern raises a separate set of methodological issues about how best to generalize (as opposed to the scope of intended generalization, which is what is under discussion).<sup>1</sup>

I would argue that adequately coding both within-case analysis and the scope of generalization requires a more case-oriented approach to measurement. Many previous efforts at analyzing within-case analysis and causal-process observations have taken place though the in-depth analysis of particular pieces of research. The basic process of measurement has involved a close reading of particular works in a way that is akin to qualitative data analysis. Likewise, to the extent that methodologists have assessed the scope of generalization in case-study and small-*N* research, they have done so by trying to carefully appraise whether inferences in particular exemplary works are intended to apply more generally or only to the cases at hand. In the future, it would certainly be desirable to accumulate a large number of these close inspections. At present, however, firm conclusions about the extent of within-case analysis and the scope of generalization in comparative research seem premature.

At the end of their article, Munck and Snyder assert that the most important challenge facing comparative politics involves overcoming certain methodological impediments. One can hardly disagree with them that the field would benefit if data were better linked to concepts, hypotheses were more explicitly and clearly formulated, and variable scores were more systematically reported (numerically or qualitatively). To the extent that qualitative research falls short in the last two of these areas, the remedy probably involves better training in qualitative methods at the graduate level (there are too few qualitative methods courses in our discipline; see Bennett et al., 2003). Two other challenges linked to qualitative research are more controversial: the infamous small-*N* problem (too few cases relative to variables) and the infamous data-mining problem (using the same data to both generate and test

theories). The small-*N* problem has been extensively debated elsewhere, with opinions differing widely. My own view is that Munck and Snyder's data do not directly speak to the issues that animate this debate. For example, scholars who defend the use of small-*N* comparisons often emphasize causal-process observations, hypotheses about necessary and/or sufficient causation, and the data requirements for testing complex (or "thick") theories. These issues are not picked up in the Munck–Snyder database.

Concerning data mining, I would argue that this is especially a problem in quantitative research. To be sure, data snooping is ubiquitous in both qualitative and quantitative research. However, the practice can be better defended if one's goal is—as it often is in qualitative research—identifying the causes of specific outcomes as opposed to estimating the average effects of independent variables (see Mahoney & Goertz, 2006; McKeown, 1999). Modifying one's theory in the course of making multiple passes through the data may be condemnable given the goal of estimating average effects; it is not necessarily so given the goal of explaining outcomes in particular cases.

### **Conclusion: Setting an Agenda**

In addition to teaching us much about contemporary comparative politics, Munck and Snyder's work should be taken as a call for future scholars to better ground their characterizations of the subfield in systematic data. I have argued here that these future efforts could benefit by examining a larger range of journals (and books too), by developing some alternative measures, and by measuring certain practices in light of a very close reading of individual studies. In addition, I believe that future efforts could benefit by measuring a series of other research practices. For example, in statistical research, one might wish to study questions such as What percentage of articles employ interaction effects? What is the average number of independent variables used in statistical models? What specific statistical techniques are used to test hypotheses? With qualitative research, one might wish to ask, How commonly do these researchers develop deterministic arguments? How often do they formulate path-dependent arguments? How frequently do they emphasize leadership and choice as key explanatory factors? These questions are merely suggestive of some of the directions that one might wish to take this kind of analysis in the future. Nevertheless, they do illustrate the better promise for characterizing the subfield by following the fine example of Munck and Snyder.

## Note

1. There is also potential slippage between indicator and concept in the measurement of the commonality of “big questions” in comparative politics. Munck and Snyder conclude that rational-choice theorists address big questions because these analysts study many of the general topics listed in their Table 1. However, the listing of topics in that table does not specify the actual research question that is addressed in these analyses. For example, one can study the broad topic of revolution using rational-choice theory, but it may be necessary to ask narrow questions about revolution when doing so (see Skocpol, 1994, pp. 321-326).

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# No Method to the Comparative Politics Madness

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Gerardo L. Munck and Richard Snyder have done two splendid services: First, they forcefully remind us that debates about the direction of comparative politics should be rooted in the data about the field; and second, they have done exactly that—gone out and collected a lot of data about research in comparative politics. Putting aside questions about the representativeness of their sample of journals, had we comparativists been following their example during the past decade, we might have avoided a couple of fruitless debates pitting false choices between area studies and cross-national research, between qualitative and quantitative methods, between deductive and inductive modes of analysis, and between microeconomic and other styles of theorizing. The authors interpret the data as suggestive of a diverse field lacking in deep paradigmatic divisions, albeit one that plays too fast and loose with methodology to be confident in its causal claims. They are a bit divided in their diagnosis of the field's overall health—at times they applaud the largely descriptive aim of most research in comparative politics but then go on to reprimand the field's failure to follow some basic methodological rules of inference. Below, I agree with their point on the pluralism of comparative politics, extend a bit on their excellent methodological discussion, provide an unabashedly negative diagnosis of the field as described in their data, and point to some substantively important divisions within comparative politics that the authors seem to underplay. I conclude by suggesting that the current peaceful state of affairs in comparative politics is not acceptable.

Munck and Snyder's analysis appears at a relatively serene time in comparative politics. The events of September 11, 2001, seem to have underscored for many the value of country- and region-specific knowledge. The more militant calls for the end of area studies have thankfully disappeared. Likewise, just as the difficulty of fully articulating cultural and constructivist theories in comparative politics has become clear, the claims of many rational-choice proponents have mellowed in the face of mounting evidence in cognitive psychology and elsewhere that people systematically diverge from the ideal *homo*

*economicus*. At the same time, there has been a considerable methodological convergence. Many quantitative and qualitative researchers are more closely in tune with the substantial challenge of testing causal arguments about the social world. The change has been most marked among quantitative researchers as concern with endogeneity has induced a caution about the robustness of statistical findings (see, for instance, the recent methodological debate between Przworski, and Acemoglu and Robinson about the relationship between institutions and development).<sup>1</sup> Just as social scientists have long been concerned with the generalizability of qualitative findings, they are becoming more keenly aware of the difficulty of making valid causal inferences on the basis of statistical research. The resulting prudence on all sides has both raised the methodological bar and contributed to the pluralistic paradise comparativists seem to cohabitate at the moment.

Certainly, it is this diversity with regard to scope, methods, and analytical approaches in comparative politics that is the most encouraging point to emerge from Munck and Snyder's analysis. Given the lack of a unifying theoretical framework in comparative politics, the difficulty of collecting data across regions, populations, countries, and so on, the accompanying linguistic and cultural hurdles involved in researching across the world, the aforementioned methodological challenges to testing causal accounts, and the involved task of keeping up with new methodological developments, the overall diversity in the field strikes me as healthy. Far from political science becoming more like economics by narrowing its intellectual range, economics is becoming more like political science as the recent explosion of work on political economy and behavioral economics has thoroughly expanded the scope of inquiry in that field. Indeed, what hits the reader right between the eyes is that Munck and Snyder's data stand in stark contrast to many of the intellectual (as opposed to organizational) claims made by the perestroika movement on the basis of articles in *The American Political Science Review*.<sup>2</sup> Rather than a subfield that values analytical rigor and mathematical precision over substance and relevance, a subfield dead set on the end of area studies and preaching the expendability of country-specific knowledge, we see almost exactly the opposite—a subfield preoccupied with the descriptive, empirical analysis (see Table 2 of Munck & Snyder) of fundamental issues of governance (Table 1) using predominantly qualitative, small-*N* methodologies (Table 3). Not only did the perestroika movement not have anything to complain about with regards to the best comparative journals, but its call for pluralism continues to win the day.

If that qualifies as the good news, more distressing is that many comparativists seem woefully ignorant of the robust quantitative and qualitative methodology literatures. Munck and Snyder show that many researchers do

not explicitly state hypotheses and transparently code variables, tend to test hypotheses from the data used to generate the hypotheses in the first place, and frequently fail to maximize analytic leverage by multiplying cases across space and time. Given their findings, one can only agree with Munck and Snyder when they argue that these methodological shortcomings represent a serious obstacle to the accumulation of knowledge in comparative politics. That said, two points are in order. First, the authors come down particularly hard on qualitative research. Such research, they show, is systematically less rigorous in several important ways that limit its capacity to make valid causal claims (see Tables 6 and 8). The authors do not, however, present data that would really allow us to assess whether quantitative researchers are doing a better job given the particular challenges they face. We do not, for instance, know what share of quantitative research addresses concerns with endogeneity, the reliability of instruments, the appropriate way to deal with unit effects and autoregressive processes, whether conditional or interactive hypotheses are correctly modeled and interpreted—all issues that have been at the heart of debates in statistical methods and are central to our capacity to draw valid causal inferences from quantitative data. In short, although Munck and Snyder's data do show that much qualitative research suffers from several shortcomings, it is not clear that quantitative work suffers from different but equally troubling problems. If my experience as a reviewer of approximately 25 manuscripts a year for various journals is any indication, the problem for the field as a whole is probably even worse than Munck and Snyder suggest once we take into account the shortcomings in quantitative work.

Second, the relationship between theory and empirics looks pretty dismal for the field as a whole. We can think of the goal of a given work as ranging from the least ambitious desire to “merely” describe a case or set of cases (we can think of this as data collection), to making descriptive inferences (the use of data on one case or set of cases to learn about other cases), to the most ambitious attempts to draw causal inferences.<sup>3</sup> Consistent with mainstream thinking in the discipline, I am of the opinion that social science should be aimed at making inferences. Descriptive inference requires transparency in the values assigned to data, be it qualitative or quantitative. Combining the data in Tables 2 and 8, it seems that only about one fourth of articles *might* provide the foundation for valid descriptive inferences. Causal inferences, on the other hand, require theoretically informed hypotheses. By that bar, there is little evidence in Munck and Snyder's data that comparative politics is terribly social scientific. Half (49.8%) of all work is predominantly empirical (see Table 2) and would seem to lack much of a theoretical point. Absent any theoretical framework, such work cannot contribute to making causal inferences. Of the

45.8% of the work that is aimed at both empirical analysis and theory generation, fully 71.9% (see Table 10) does not clearly articulate a hypothesis—a fundamental precursor to making causal inferences. Put differently, 82.7% of comparative politics articles are either not aimed at or unable to make causal inferences about the world. When we add the 4.4% of articles that are purely theoretical and have no empirical referent (Table 3), it seems that only about 10.0% of articles in the best comparative politics journals qualify as exercises in causal inference. In a curious disjuncture, Munck and Snyder approve of the tendency of comparative researchers to focus on descriptive research “given the demanding nature of the challenge of empirically supporting causal arguments” (p. 11) but then go on to bemoan the lack of attention to key methodological issues associated with making valid inferences. It seems we should either criticize the predominantly descriptive nature of comparative politics or admit that the methodological demands of inference are irrelevant for a field that does not seem terribly concerned with making inferences. I fall squarely in the former camp. Indeed, the problem seems less that qualitative research is “merely descriptive” than that much of the field of comparative politics is merely descriptive. One can only be dubious about any claims of significant knowledge accumulation in comparative politics. To be sure, given these findings I am wonderfully reassured that comparative politics articles are not aimed at providing policy advice (see page 12). It is hard to imagine we can provide an awful lot of reliable policy guidance given the woeful state of social science in comparative politics.

Finally, I disagree somewhat with Munck and Snyder’s characterization of the field as one lacking in important divisions. It certainly seems to be the case that some of the most divisive debates of the last decade have been misguided (theoretical paradigms are not really clashing, theory and area studies are not antithetical, etc.), but there are some striking divisions in their data that have important implications for the collective comparative politics enterprise. The most obvious and important division apparent in the data is that between the small minority that is following basic methodological rules of social science and the large majority that is not. It would seem we comparativists have not yet decided if we are interested in engaging in social science. That is a debate that should be out in the open. Certainly there is a curious disjuncture between the rising methodological demands we make of our graduate students and the underlying descriptive nature of much of comparative politics. Second, in unreported findings, the authors discover a striking imbalance in the geographic coverage of articles in the top journals. Western Europe, North America, and Oceania represent about

12% of the world's population (a measure of admittedly uncertain importance) but are the subject of study in more than 66% of articles. Latin America is nearly as overstudied. Asia, on the other hand, holds more than 56% of the world's population and is covered in only one third of articles. Anyone who has surveyed the literature on South or East Asia and compared it with that on Latin America or Western Europe will note a reasonably sharp methodological and empirical divide. Again, this division is probably symptomatic of a serious division about what it is that comparative politics should be about. Indeed, this geographic divide points to a number of more traditional divisions that do seem pervasive. Areas studies research is systematically more descriptive and less interested in causation (Table 4), and qualitative research looks to be significantly less scientific (Table 8, though note my concern with quantitative work above). In short, even if comparative politics is quite pluralistic, there are a number of substantively important divisions in the field that belie the current era of intrafield peace. Given Munck and Snyder's data, my bet is that we are not done fighting, or at least we shouldn't be.

Indeed, I come away from the article convinced that more informed, explicit fighting is in order. Until reflecting on this piece, I had always been struck by the bizarre amount of self-analysis that goes on in comparative politics.<sup>4</sup> Even if one buys Socrates's claim that the unexamined life is not worth living, it is probably also the case that the overexamined life is indicative of some sort of psychosis. Munck and Snyder have helped clarify the nature of the illness. Comparative politics is a field that makes regular claims of being social scientific but practices something quite different. And so, important debates loom: Do we aim to be a social science? If so, why are most articles in the top comparative journals so theoretically and methodologically uninformed, and why are we reviewers permitting such work in our best journals? Or, as Przeworski (in press) recently implies, is a science of comparative politics all but impossible? And if that is the case, what are the standards by which we should evaluate work when reviewing for our top journals? Social scientists that they are, Munck and Snyder answer these questions with a simple prescription to do a better job of keeping basic methodological imperatives in mind. To that I would add a need to do so in the context of theoretically driven research. I personally would much prefer focusing my energies on the real world at the expense of debating the field of comparative politics. But given the state of the field Munck and Snyder describe, I can only hope that this moment of peace in comparative politics will pass quickly.

## Notes

1. See Przeworski (2004) and Acemoglu, Johnson, and Robinson (2005).
2. For a thorough overview of the history and claims of the movement, see Monroe (2005).
3. I will leave the goal of interpretation to other, more qualified voices.
4. In addition to periodic empirical pieces assessing the field such as this one, we have the *Annual Review of Political Science*, the *Handbook of Political Science*, *The State of the Discipline*, a slew of forthcoming handbooks from Oxford on everything from comparative politics to political institutions, regular reviews of where comparative politics has been and is going in the pages of the *CP Newsletter*, cyclical papers reviewing the state of the discipline on the decadal anniversaries of the major journals, and regular flares of additional attention in response to the particularly inflammatory claims from one bigwig or another.

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