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Taking Stock and Looking Forward

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Scholars in the field of comparative politics have recently offered a number of stocktaking exercises (Lichbach and Zuckerman 1997, 2009, Boix and Stokes 2007, Robinson and Landman 2009).[1] These exercises are useful; indeed, nearly indispensible, given the large amount of literature that is currently being produced on comparative politics. But each individual overview in these edited volumes, as similar overviews published regularly in the *Annual Review of Political Science* and various handbooks, is actually quite limited in scope. That is, though these essays collectively cover all main topics being studied in the field of comparative politics, the standard approach is for each essay to focus separately on a certain actor, institution or policy area rather than address the big picture.[2] And, as a result, comparativists essentially focus on their topic of interest and do not engage in a broader discussion about the direction of substantive research in comparative politics as a whole. Indeed, the broadest and most heated debates have focused on methods, both for theorizing (Green and Shapiro 1994, Munck 2001a) and for empirical analysis (King, Keohane and Verba 1994, Brady and Collier 2004). But even these debates focus on certain aspects of the research process and have rarely been linked closely with an assessment of substantive research. Indeed, with several important exceptions,[3] discussions of comparative politics have largely failed to focus squarely on the substantive goals, and the associated methodological concerns, of the field of comparative politics as a whole.

This paper is driven by the conviction that, even though the primary goal of comparativists is to produce knowledge about politics around the world, to reach this goal it is essential that comparativists think about what they do and engage in a reflection about knowledge production in the field of comparative politics as a whole.[4] The development of such metaknowledge, which should be distinguished from intellectual history, is not always recognized as a key aspect of research. Moreover, a call to focus on "knowledge about knowledge" might meet with some resistance by "practicing" comparativists, all too eager to just get on with their research. After all, particularly for comparativists who see themselves as dealing with "real world problems," a call to discuss the field of comparative politics rather than do comparative politics might seem like a fruitless diversion of energies. Alternatively, comparativists could argue that any

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[1] I conceive of the field of comparative politics as the study of politics around the world through the empirical comparison of multiple units, including especially countries. Thus, I do not define the field of comparative politics in strict disciplinary terms, as a subdiscipline of political science, let alone as excluding the study of United States politics, as is commonly done in US academia. In other words, I include in this assessment the important tradition of studying comparative politics in sociology and history, as well as the more recent work in economics—I refer generically to scholars interested in comparative politics as comparativists—and see the study of American politics as part of the study of comparative politics.

[2] The tendency to break down overviews into topics is also evident in the roughly decadal surveys of comparative politics sponsored by the American Political Science Association (Migdal 1983, Rogowski 1993, Laitin 2002).

[3] Geddes (1991, 2003), Schmitter (1993, 2009a), Mair (1996), Almond (1997), Easton (1997), Eckstein (1998), Laitin (1998, 2004), and Lichbach (2009).

[4] Indeed, this paper is a follow-up on an earlier historical overview of comparative politics (Munck 2007), a set of interviews with leading scholars in the field about their life's work and recent debates about the field (Munck and Snyder 2007a), and an assessment of the state of the field as gleaned through articles published in the field's leading US journals during the 1989-2004 period (Munck and Snyder 2007b).

debate about knowledge production is largely irrelevant, because changes in research patterns come about through exemplars, that is, actual works that show how to carry out a certain kind of research, as opposed to more abstract debates about what should be done. But I hope to show that focusing on metaknowledge helps to bring out into the open and address in explicit terms questions that comparativists carry in their minds but all too often never confront head on, such as: Which, among the alternative research projects I could undertake, would likely yield the larger contribution to the field of comparative politics? Indeed, as Karl Deutsch (1986: viii) wrote, "It is essential that the social sciences, and those who work in them, should gain a clearer awareness of their mission and their powers." And, to that end, it is critical to recognize that meta-reflections are actually fundamental to the successful framing and pursuit of research agendas and should be seen as integral parts of the work of a community of scholars.

To offer a comprehensive and forward-looking assessment of the study of comparative politics, I rely on a general framework for assessing social scientific knowledge. This framework will be elaborated in the body of the paper. But, at the outset I seek to highlight two critical points. First, in assessing a body of research, it is important to acknowledge that there are different kinds of knowledge, which respond to distinct questions, and entail different research processes (and associated methodological issues) and products (see Table 1). More pointedly, an assessment of research must take a broader view than is conveyed by the common understanding that research results or findings consist of the conclusions of tests of explanatory hypotheses. To be sure, explanations are a central goal of the social science, but they certainly are not the only goal. Indeed, even the broader framing of the goals of social science as including both description and explanation (King, Keohane and Verba 1994: 34, 75) falls short, because it fails to recognize the importance of theory, and specifically of concepts and conceptual systems and of theories and theoretical systems, as products of research.

Table 1. A General Framework for Assessing Social Scientific Knowledge I: The Dimensions of Social Scientific Knowledge

Kind of Knowledge	Question	Process	Product
Theory	*What* is y and what is the domain of y (i.e. what units does y refer to)?	Conceptualizing *	Concepts and conceptual systems
	Why and *how* does y occur (i.e. come into being and endure)?	Theorizing	Theories and theoretical systems
Empirics	*How much* y do units 1. 2, 3 … n have?	Measuring	Descriptions
	Why and *how* does y occur (i.e. come into being and endure) in units 1. 2, 3 … n?	Assessing causality	Explanations **

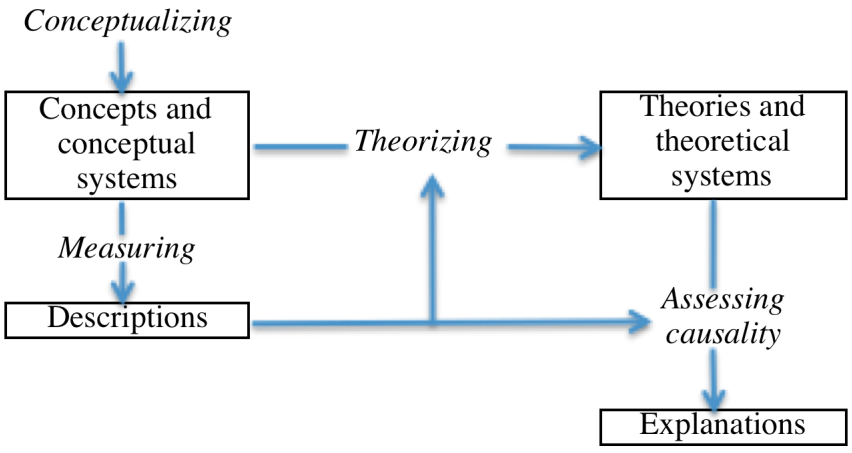
Note: The products of research can be given the genearic label of *substantive* knowledge and hence deserve to be highlighted in an assessment of a field like comparative politics. But it is important to recognize that all four research processes involve the application of *methodological* knowledge and that this methodological knowledge in turn in the product of research. Furthermore, it is important to note that, taking a further step backwards, substantive knowledge draws on knowledge about ontology and epistemology.

(*) Conceptualizing is as much of a theoretical activity as what is usually labeled theorizing. Thus, a more accurate label for conceptualizing would be "conceptual theorizing" or "constitutive theorizing." But in this paper I shall use the conventional terms "conceptualizing" and "theorizing".

(**) Not all explanatory theory is causal, but the main explanations are causal and hence call for exercises in causal assessment. Moreover, though the related but distinct goal of prediction should be acknowledged, explanation is usually considered more central in the social sciences.

Second, in assessing a body of research, it is also critical to recognize how the various products of research are connected and in what sense the products of some research processes may be considered building blocks for other research processes. In this regard, as any practicing researcher well knows, it is clear that the four research processes identified in Table 1 develop interactively. For example, efforts at measurement may lead to new ideas regarding conceptualization and work on causal assessments may spur new theorizing. Nonetheless, logical strictures do impart a structure on knowledge (see Figure 1). Specifically, conceptualizing stands logically prior to theorizing and measuring, and theorizing and measuring stands logically prior to assessing causality. In other words, the results of certain processes are building blocks for other processes—e.g. a poorly specified concept simply cannot be well measured—and hence the importance of each research product is due both to its intrinsic importance as well as its status as input in other processes of the entire research cycle.

Figure 1. A General Framework for Assessing Social Scientific Knowledge II: The Logical Ordering of the Dimensions of Social Scientific Knowledge



Note: Terms in italic denote a research process, while terms of boxes denote research products. For a further explanation, see Table 1. Arrows start at a logically prior product or process and end at a logically posterior producr or process.

This framework serves as the organizing principle of the paper. Starting with concepts and conceptual systems, each of the four main research products is addressed. And with regard to each product, the strengths and weaknesses of current research are highlighted, some points about methodological practices are made, and opportunities and challenges are identified. I seek to show that, over the past 40 years or so, a lot of important work has been done and that current choices about research strategies are likely to be more fruitful inasmuch as comparativists have a clear sense of the strengths and weaknesses of this large body of research, and use the lessons from such an assessment in seeking out promising direction for future research. By way of conclusion, a general suggestion is made regarding synthesis and the research-praxis nexus.

1. Concepts and Conceptual Systems

Concepts or, more precisely theoretical concepts, understood as abstract ideas that refer to an empirical domain, are the elementary units of thought. Indeed, since all aspects of the research process rely on concepts, starting with the framing of research questions, concepts are the quintessential building blocks of social science research. Insightful conceptual innovations have fruitful implications for all the other research processes. And poor conceptualization inevitably has negative consequences for efforts at theorizing, measuring and assessing causality. It is hard to exaggerate the importance of concepts and hence of the need to evaluate the concepts and conceptual systems elaborated and used by comparativists.

1.1. Baseline Practices and Knowledge

Considering routine practices and knowledge that frequently is taken as a given by researchers working on other aspects of the research process, what might be called baseline practices and knowledge, there is much that is positive concerning work on conceptualization in comparative politics. A rudimentary but certainly non-trivial matter is the considerable degree of consensus that in the study of comparative politics certain concepts—political power, the state, political order, political regime, democracy, citizenship, the welfare state, constitutionalism, the rule of law, and human rights—are basic, in the sense of serving as anchors for quite disparate research efforts. Fleshing out this shared view regarding which are the core concepts of comparative politics, a considerable amount of work on concept formation has helped to clarify core concepts of comparative politics. A prime example is the career-long work of Robert Dahl, who from *A Preface to Democratic Theor*y (Dahl 1956) to *Democracy and its Critics* (Dahl 1989) gradually refined a theoretically derived, and normatively justified, definition of democracy. Indeed, probably no concept in the study of politics has been theorized as much as democracy (Sartori 1987a, 1987b, Schmitter and Karl 1991, O'Donnell 2010, Przeworski 2010). But innovative work has been done on a variety of concepts that have been central to comparative politics, including authoritarianism (Linz 1964), corporatism (Schmitter 1974) and the unit of analysis of politics (Schmitter 2007). And all these efforts contribute to shaping the language used to frame comparative research.

The lack of consensus among researchers regarding the definition of core concepts, as well as the confusing and disorderly use of concepts, is undoubtedly a key problem. But somewhat of a counteracting trend has been provided by conceptual analyses. Such analyses have focused most commonly on a single concept (Chandra 2006, Collier and Gerring 2009: Part II). [5] But conceptual analyses have taken other forms as well. Some analyses address a set of concepts (Donnelly 1999, Munck 2009: 120-27). Others explicitly link matters of conceptualization to explanatory theorizing (Motyl 1992, Kalyvas 2001, Johnson 2003, Mazzuca 2010a). And a new genre of publications on conceptualization and measurement has sought to link conceptual analysis with the task of measurement, simultaneously addressing questions such as what is democracy or the rule of law and questions such as how is democracy or the rule of law to be measured (Bollen 1990, Munck and Verkuilen 2002, Sambanis 2004a, Abdelal et

[5] For further examples, see the Working Paper Series of the Committee on Concepts and Methods on Political Concepts, available at: http://www.concepts-methods.org/

al. 2009, Skaaning 2010). Though conceptual problems continue to hamper comparative research—the need to focus on concept formation and improve concepts should be considered a key challenge and opportunity for comparativists—this research on concepts has served to clarify how concepts are used and, in some cases, offered a basis for making choices among alternative conceptualizations.

Table 2. Assessing Comparative Politics I: Concepts and Conceptual Systems *

Current Practices and State of Knowledge		Challenges and
Strengths	**Weaknesses**	**Opportunities**
• Consensus about which are the basic concepts • Some concepts (e.g. democracy) formed with a basis in normative theory	• Lack of consensus regarding the definition of basic concepts	• Greater focus on concept formation and improvement of concepts
• Some "mid-range" conceptual frameworks • Some attempt at conceptual synthesis	• Lack of a comprehensive conceptual system, i.e., a "general theory of politics"	• Greater emphasis on conceptual synthesis, explicitly rooted in normative theory
• Conceptual analyses, of many concepts • Conceptual analyses, linked to explanatory theorizing • Conceptual analyses, linked to measurement	• Conceptual analyses focus mainly on a single concept at a time	• Conceptual analyses that jointly address multiple basic concepts, i.e., that address conceptual systems
• Work on conceptual analysis methodology	• Lack a well-developed and formalized conceptual analysis methodology	• Development of a conceptual analysis methodology

Note: (*) Though the focus is on the products of research, the process of research and, more precisely, the methodologies used in the process of research are addressed below the dashed line.

1.2. Conceptual Systems

An assessment of conceptualization in comparative politics must also address the status of conceptual systems. The development of conceptual systems, that is, sets of interrelated concepts, plays a key role in research. This is so because one of the tasks of concept formation is to delimit a certain concept from other concepts. Thus, at the very least, the formation of a concept should addresses what a concept is not. But conceptual systems are also important for another reason. As is widely recognized, there are important payoffs associated with an analytical approach to the study politics, that is, an approach that calls for the disassembling of big or broad concepts into smaller and more manageable parts. Indeed, because the payoffs of an analytical approach are considerable for both the processes of measuring and theorizing,[6] comparativists routinely

[6] On the payoffs of an analytical approach for theorizing, see Geddes (2003: Ch. 2).

disaggregate or decompose the basic concepts in comparative politics into multiple component parts. Yet, the benefits of an analytical approach are only forthcoming if comparativists formulate, as a complement to the principle of analysis used to identify parts of a whole, a principle of synthesis, that is, if they address and answer the question of how the parts are interrelated and how the parts relate to the whole. Otherwise, whatever knowledge is produced will have a disjointed, ad hoc quality.

In this context, it is worth recognizing that there is something to the charge that comparativists have given up on the ambition, shared by many in the 1950s, 1960s and 1970s, of developing a "general theoretical framework" or a "general theory of politics" (Almond 1997, Easton 1997: 16-18, 25-27, Eckstein 1998: 512-13, 524-32). Indeed, the shift is notable. (To avoid confusion, rational choice theory purports to be a general theory of action but not of politics.) During the post-World War II years, leading scholars worked on general conceptual frameworks for the study of politics and published books such as Harold Lasswell and Abraham Kaplan's (1950) *Power and Society: A Framework for Political Inquiry*; David Easton's (1965) *A Framework for Political Analysis*; Gabriel Almond and Bingham Powell's (1966) *Comparative Politics: A Developmental Approach*; and Harry Eckstein and Ted Gurr's (1975) *Patterns of Authority: A Structural Basis for Political Inquiry*. But these days the goal of developing a general conceptual framework suitable for comparative politics has largely been abandoned.[7]

Still, a claim that in this regard the situation in comparative politics was better in the 1960s and 1970s than in the first decade of the twenty-first century, and a call to resurrect the general theoretical frameworks from the "golden age" of comparative politics, is unwarranted. First, the various proposals of a general theory of politics—such as Almond's structural-functionalism, Easton's system analysis, and Eckstein and Gurr's authority patterns—have been dropped for good reasons, including the highly abstract nature of their concepts, their exclusive focus on statics to the detriment of dynamics, their lack of attention to political institutions, and the failure to squarely address the growth and distribution of political power.[8] Second, it is not the case that comparativists currently do without conceptual frameworks. For example, more or less elaborate frameworks have been proposed and used to study topics such as the partial regimes of democracy (Schmitter 1992: 426-30, 2009b: 29-33), the judiciary (Dyevre 2010), and decision-making and accountability in democracies (Cox and McCubbins 2001, Tsebelis 2002, Przeworski 2003: Chs. 4-8). And, interestingly, many of these unifying frameworks draw less on ideas from sociology, as was the case in the past, than on ideas from

[7] To be sure, some of these frameworks have been further elaborated and refined by their authors well into the 1980s (Almond and Powell 1978, Easton 1990) and a ninth edition of a textbook that used Almond and Powell's framework has recently been published (Almond, Powell, Dalton and Strom 2010). Moreover, it may well be the case that elements of these frameworks have surreptitiously been incorporated into current thinking. Indeed, Linz's suggestion that structural functionalism in particular continues to influence how researchers think (Linz in Munck and Snyder 2007a: 159; see also Almond in Munck and Snyder 2007a: 78) raises the possibility of what Merton (1968: 23) called "obliteration by incorporation," a common occurrence in the history of ideas. However, a comparison of current frameworks and those developed in the 1960s and 1970s reveals many divergences, in terms of goals as well as content.

[8] For critiques of Almond's and Easton's frameworks, see Holt and Richardson (1970: 29-45), Holt and Turner (1975), Chilcote (1994: Ch. 5) and Mair (1996: 314-17); for a critique of Eckstein's framework, see Laitin (1998: 424-31). More broadly, see Schmitter (1993: 172-73; 2009a: 39) and Ruttan (2003: Ch. 4).

economics, such as spatial modeling, game theory, and principle-agent theory.

Third, and probably most importantly, starting in the 1970s there has been a shift in the preferred mode of theorizing, that has key implications for the way the building of conceptual frameworks is approached. Specifically, even though the proposals of comparativists during the 1960s were not as ambitious as some other contemporary synthetic works in the social sciences (Bell 1982: 8), there has been a move away from grand theorizing aimed at the development of overarching frameworks, in the mould of Talcott Parsons, toward what Robert Merton (1968: Ch. 2) called middle-range theorizing and the elaboration of middle-range conceptual frameworks.[9] What is characteristic of middle-range theorizing, as Merton (1968: 41, 51, 68) explains, is that they are not "logically *derived* … from a master conceptual scheme," focusing instead on a limited conceptual range and set of assumptions, formulating ideas in dialogue with observations, and logically deriving hypotheses that are amenable to empirical testing.[10] And grasping the significance of the trend toward the elaboration of middle-range conceptual frameworks is essential to an identification of the conceptual challenges and opportunities currently faced by comparativists.

In the immediate post-World War II years, the development of a conceptual approach that would guide the new research on all four corners of the world, until then ignored by comparativists, was a key imperative. Thus, it is not surprising that an influential framework, such as Almond's structural-functional framework, was actually formulated initially (Almond 1960) without any direct contact with Third World societies and before much information on the politics of these societies was available (Munck and Snyder 2007a: 76-78). But it is also important to recognize that it is a mistake to assume "that systems of thought can be effectively developed before a great mass of basic observations has been accumulated" (Merton 1968: 46). Indeed, though the aspiration of developing a "general theory of politics" remains laudable, the way forward is not likely to include a reconsideration of the frameworks of the 1960s and 1970s.[11] Rather, as Merton (1968: 51) suggests, it is more likely that "the road to effective general schemes" will be constructed by efforts to scale up middle-range conceptual frameworks, that is, "by evolving, not suddenly revealing, a progressively more general conceptual scheme that is adequate to consolidate groups of special [i.e. middle-range] theories."

This new conceptual challenge and opportunity has not gone unnoticed by comparativists. Juan Linz (1975) offered an early conceptual synthesis that connected authoritarianism to totalitarianism, democracy, sultanism and a range of other political

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[9] This shift in comparative politics has been part of a broader shift in the social sciences, including sociology (Rueschemeyer 2009: 10-11).

[10] To avoid confusion, a shift to middle-range theorizing does not imply a narrowing of the empirical scope of research, as in commonly assumed in discussions of comparative politics (see Mair (1996: 316-17). To take one of Merton's (1968: 40) favorite examples of middle-range theorizing, the theory of reference groups, there is no indication that such a theory would necessarily refer to some social groups and not others. Thus, middle-range theorizing is a matter of conceptual scope and the role of observation, and is not inherently associated with a narrowing of the empirical scope of comparison. Also, to avoid another source of confusion, though the terms "theory" and "hypothesis" are usually understood to apply to the context of explanation, they apply equally to description. After all, theoretical concepts are measured indirectly, through observable indicators. And the relationship between a theoretical concept and its indicators should be treated as a hypothesis, which must be theoretically justified and empirically tested.

[11] Indeed, efforts to rescue structural functionalism (as attempted by Lane 1994) seem doomed to failure.

regimes. Michael Mann (1986, 1993) has been engaged in probably *the* most ambitious of syntheses through his analysis of the structure of societies in terms of four sources of social power: ideological, economic, military, and political power. Charles Tilly was a recurrent conceptual synthesizer, linking the study of strikes, wars, revolutions, social movements, and other forms of political struggle by bringing them under the umbrella concept of contentious politics (McAdam, Tarrow and Tilly 2001); and then linking contentious politics to democracy and state capacity (Tilly 2004, 2006, 2007). David Samuels and Matthew Shugart (2010) have developed a framework that brings together the study of political parties in different systems, specifically parliamentary, presidential and semi-presidential democracies. And various other attempts have been made to integrate the study of topics routinely studied separately by bringing them under some unifying conceptual framework (Kalyvas, Shapiro and Masoud 2008, McAdam and Tarrow 2010).

These attempts at conceptual synthesis of areas of research where a considerable body of middle-range theorizing has already been produced are important and play a key role in the development of knowledge about comparative politics. Thus, their promise of integrating "islands of theory" (Guetzkow 1950) should be fully recognized. However, to further systematize this kind of research, some steps should be taken. Specifically, as part and parcel of the effort to use conceptual analyses to clarify and improve concepts, conceptual analyses should simultaneously address multiple basic concepts, that is, conceptual systems. But, in addition, to prevent an obfuscation of the stakes of research, the basis in normative theory of conceptual systems should be retained as a critical consideration. Indeed, to make the point more directly, though comparativists should draw on various resources, including some new ideas familiar to economists, they should not forget a key lessons they owe to Dahl: that an essential anchor for research is provided by concepts, such as democracy, that have been laboriously formed and justified in terms of normative theory. Indeed, this same principle applies to conceptual systems.[12]

1.3. Conceptual Analysis Methodology

Finally, an assessment of conceptualization in comparative politics must address the status of the methodology of conceptual analysis. This is a complex issue, given that the role of theoretical concepts in research in the social sciences is quite distinct. As Mario Bunge (1996: 49) puts it, "Concepts cannot be … tested, because they neither assert nor deny anything. Hence there are no true or false concepts" (Bunge 1996: 49). This key point about the distinctiveness status of concepts notwithstanding, there is a place for a methodology of conceptual analysis. After all, there has been an ongoing discussion about conceptual analysis in philosophy and the social sciences.[13] Moreover, the efforts by Giovanni Sartori in the 1970s and 1980s (1970, 1984a), and by David

[12] My view is that the key conceptual challenge facing comparativists is to build on the consensus around a minimalist definition of democracy and to broaden their conceptual scope beyond a minimalist democracy. To this end, though some of the more interesting conceptual debates are occurring among legal scholars, I know of no better guide than Norberto Bobbio, who placed great emphasis on the analysis of fundamental concepts (Bobbio 2003: 111-12), and whose most encompassing collection of essays, rightly titled *General Theory of Politics* (Bobbio 2003), offers a nuanced synthesis of the classics of political thought.

[13] For an overview of approaches to concept formation and conceptual analysis, see Bryant (1995: Ch. 2) and Brons (2005: Ch. 2).

Collier since the 1990s (Collier and Mahon 1993, Collier and Levitsky 1997),[14] have brought issues of conceptual analysis to the attention of comparativists. Nonetheless, conceptual analysis is not an established and readily recognizable methodology.

Part of the problem is the lack of interest in the development of a conceptual analysis methodology. A discussion of such a methodology routinely bumps up against the views of two camps that otherwise have little in common: those who implicitly or explicitly subscribe to operationism and do not recognize the key role of concepts in research (e.g. King, Keohane and Verba 1994), on the one hand; and those who critique methodological discussions yet show no commitment to developing a methodology for doing empirical research (e.g. Bevir and Kedar 2008), on the other hand. Indeed, the fact that Philippe Schmitter (2009c), a key conceptual innovator in comparative politics, was forced to coin a term, "conceptualist," to label what he did throughout his career, is indicative of the lack of a recognition of work on concepts as a central part of research on comparative politics.

But the problem is deeper. First, by and large, the methodological guidelines that have been proposed in the context of comparative politics have apparently not had much effect on how comparativists go about forming their concepts or even on exercises of conceptual analysis. Indeed, when Schmitter (2009c) discusses his efforts at conceptualization, his insightful ideas about concept formation are offered as "retrospective thoughts" and make no mention of the guidelines offered by any methodologists. And even the conceptual analyses presented in Sartori's (1984b) edited volume *Social Science Concepts* do not actually follow Sartori's (1984a) guidelines in a step-by-step manner. At the very least, the gap between the "logic-in-use" and the "reconstructed logic" (Kaplan (1964: 3-11) is quite stark.

Second, the proposed methodological guidelines are partially deficient. Some of these guidelines are questionable. For example, methodologists frequently fail to adequately distinguish between things and their conceptual properties, between conceptualization and measurement, and between theoretical and empirical concepts; and, as a result, some of their central messages, such as how to avoid conceptual stretching, are debatable (Munck 2008). Furthermore, these guidelines are silent on key issues. Most critically, they never tackle head on the issue of the scientific "validity" of concepts and thus do not offer criteria to justify conceptual choices; alternative conceptualizations of the same concept are not portrayed as better or worse, not just different. This is the sort of guidance that would be of great assistance to conceptualists as well as to users of concepts. And it does not seem farfetched to require such advice from methodologists.[15] Thus, the development of a usable methodology of conceptual analysis remains a key challenge.

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[14] See also Collier, Hidalgo, and Maciuceanu (2006) and Collier and Gerring (2009).

[15] For example, given that at the very least we would expect a clear distinction between theoretical and empirical questions, it seems reasonable to say that, other things being equal, a concept of say democracy that distinguishes clearly between conceptual issue and empirical questions is better than one that does not. Along similar lines, a concept that is backed up by a theory and is derived in a logically coherent manner from that theory is better than one that does not do this. Furthermore, it seems reasonable to say a concept that is part of a conceptual system is preferable to one that is free floating concept. In other words, it does not seem farfetched to argue that it is possible to posit certain criteria for the evaluation of concepts. Finally, a concept that is formally and fully defined, that is, that includes a specification of its conceptual attributes and the relationship among these attributes, is preferable to one that is not so defined.

2. Theories and Theoretical Systems

Theorizing, as is frequently observed, is intimately linked with conceptualizing. Indeed, Abraham Kaplan (1964: 53-54) refers to the "paradox of conceptualization," whereby "the proper concepts are needed to formulate a good theory, but we need a good theory to arrive at the proper concepts," and suggests that this "paradox is resolved by a process of approximation: the better our concepts, the better the theory we can formulate with them, and in turn, the better the concepts available for the next, improved theory." Thus, there is an interactive relationship between conceptualizing and theorizing. Moreover, theorizing is sometimes developed in close links with efforts to describe, a point addressed ion the next section. Yet theorizing, conceptualizing and describing, as conventionally understood, are distinct activities that address different questions (see Table 1). And the work on theorizing by comparativists can be assessed in its own terms.

2.1. Baseline Practices and Knowledge

No comparativist is brazen enough these days to claim to attempt to explain politics *tout court*. Rather, comparativists have sought to develop explanatory theories of state formation, revolutions, civil war, nationalism, ethnic conflict, social movements, democratization, electoral systems, parties and party systems, voting, interest groups, political culture, government formation, executive-legislative relations. policy making, the courts, bureaucracy, policy implementation, the welfare state, and so on (Munck and Snyder 2007b: 8-10). That is, in line with the goal of elaborating middle-range conceptual frameworks, theorizing has focused on aspects of politics and aimed at producing middle range theories.

This standard mode of theorizing has been productive, in the sense of generating many ideas about explanatory factors and how these factors account for outcomes. More specifically, theorizing on the different outcomes of concern to comparativists has addressed a range of explanatory factors, such as the economic and social factors that have been heavily emphasized by modernization theorists; the formal rules highlighted by institutionalists; and the interaction among, and choices of, actors, analyzed by strategic choice theorists. Some of this theorizing also exemplifies the "paradox of conceptualization," in that scholars have simultaneously refined concepts and built theory. Moreover, a relatively novel but by now standard aspect of theorizing by comparativists has been the effort to hypothesize about the mechanisms that generate outcomes by postulating variables at a lowel level than the outcome itself.[16] This is a noteworthy trend, because comparativists mainly seek to explain macro-outcomes and the search for mechanisms, especially when they involve macro-micro-macro links, has added depth to the explanatory theories of comparativists.[17]

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[16] In other words, as Elster (1998: 69) puts it, "a plea for mechanisms" is "a plea for disaggregation." See also Stinchcombe (2005: 17, 177).

[17] Comparativists have gone about theorizing mechanisms in various ways. Some have proceeded inductively, either with only a sense of an outcome to be explained, as suggested by Goldthorpe (2000: 152-55), or a sense of a correlation between an explanans and an explanandum, as suggested by Lieberman (2005: 444). Others have proceeded deductively, also either with only a sense of an outcome to be explained, as is standard in formal theorizing, or with a sense of a correlation between an explanans and an explanandum, as suggested by Laitin (2002: 630-31). An alternative to these standard approaches has been proposed by Elster (1998); in line with his contrast of mechanisms to laws, he has suggested starting the

These positive features of theorizing in comparative politics notwithstanding, a key problem deserves mention. Theories, as Dietrich Rueschemeyer (2009: 7) reminds us, consist of "several logically interrelated theoretical propositions." Yet much of what passes for theories in comparative politics are actually "one line theories" (Bunge 2009: 12), such as "economic development leads to democracy." And this understanding of theory has several negative implications. It has been associated with theorizing that leads to an unwieldy proliferation of possible explanations of outcomes,[18] the positing of explanatory variables that are frequently seen as vaguely mattering for some outcome, and the positing of explanatory variables whose status as competing or complementary factors is not always clear. And, by producing weak theory, which is hard to test, this mode of theorizing reduced the possibility of a feedback from empirics to theory. In short, given baseline practices regarding theorizing, an obvious opportunity and challenge could be framed somewhat generically as the need for strong theory, that is, for theories that explain more with less. But the problem with baseline practices cannot be dissociated from a consideration of theoretical systems and questions of methods (see Table 3).

Table 3. Assessing Comparative Politics II: Theories and Theoretical Systems *

Current Practices and State of Knowledge		Challenges and Opportunities
Strengths	**Weaknesses**	
• Considerable "mid-range" theories, that is, theories on various aspects of politics • Theorizing of a comprehensive range of explanatory factors • Some simultaneous concept formation and theory building • Emphasis on mechanisms	• Weak theory: Proposals of "one line theories" and the proliferation of explanatory variables	• Greater emphasis on strong theory
• Some partial attempt at theoretical synthesis through the consolidation of middle-range theories	• Lack of a comprehensive theoretical system	• Greater emphasis on theoretical synthesis • Explicit theorizing on macro-micro linkages
• Fertility of informal theorizing by qualitative researchers • Deductive rigor of formal theorizing	• Verbal and formal theorizing frequently results in vague and/or inadequate statements about hypotheses	• Greater emphasis on the proper formalization of results of theorizing • Greater emphasis on the development of hypotheses about change and about mechanisms

Note: (*) Though the focus is on the products of research, the process of research and, more precisely, the methodologies used in the process of research are addressed below the dashed line.

analysis with a focus on mechanisms and moving from an understanding of mechanisms back to the conditions that trigger certain mechanisms and forward to the outcomes of mechanisms.

[18] For example, the theory of democracy presented by Diamond and Linz (1999) posits eleven broad explanatory factors, and an even larger number of subsidiary explanatory factors.

2.2. Theoretical Systems

The problem of weak theory serves to underline the value of theoretical systems that could bring order to theorizing by introducing some principle to unify otherwise disjointed exercises in theorizing. And many comparativists have recognized the importance of theoretical systems. But, much as is the case concerning the development of conceptual systems, two somewhat different approaches have been advocated and practiced.

One approach hinges on the suggestion that what comparative politics needs is a paradigm, which has been usually understood as somewhat loosely as referring to a set of common theoretical principles. Thus approach has been linked with claims about the potential of rational choice theory (Geddes 1991, 2003: Chs. 1 and 5, Wallerstein 2001), or, at least, a competition among paradigms (Lichbach 1997, 2003, 2009). And the surge of this approach in the 1990s has served to counter some of the problems with standard theorizing. In particular, the application of rational choice theory—by far the most elaborate of the contending theoretical perspectives—to the study of comparative politics triggered a debate that has served to clarify some of the broad underpinnings of theorizing in comparative politics that got lost in the more common discussion in the language of variables and thus helped to sharpen the idea of distinct theoretical perspectives. But the actual substantive theorizing of rational choice theorists, in contrast to their programmatic statements, yields other conclusions.

Consider the example of research on democratization using game theory, an extension of rational choice theory to interdependent decision-making.[19] Pure applications of game theory, that is, unconcerned about the domains of action to which rational choice theory is applicable, rely on the same basic building blocks: actors, with certain preferences, confronting certain choices, and making choices according to the precepts of expected utility. Thus, game theory imposes a considerable degree of discipline on this theorizing. But, in part because the theory in game theory is so minimal, the actual work is done by factors external to the theory—such as who are the actors, what preferences the actors have, what choices do the actors face—that are addressed in the actual modeling of the game. And, since these key factors have been specified in a range of different ways in different models, actual applications of game theory to democratization differ, sometimes considerably.

In the end, even if all the proposed models are built with the same basic building blocks, and rely on the same theory of action, the models of democratization vary considerably and not necessarily in readily apparent ways. And things get even more complicated because some applications of game theory to the study of democratization have been more pragmatic,[20] in the sense of recognizing that not all action can be explained in terms of rational choice theory, and have sought to integrate elements extraneous to rational choice theory, such as institutions and culture. Thus, a new layer of differentiation is introduced. Not only do the more pragmatic models differ among each

[19] See, among others, the models in Przeworski (1991: 61-64), Bates, de Figueiredo and Weingast (1998), Wintrobe (1998), Boix (2003), Geddes (2003: 50-62), and Acemoglu and Robinson (2006).
[20] On the distinction between purist and pragmatist applications of game theory, see Munck (2001a).

other. In addition, all these models differ from the models that are pure applications of game theory.

The evolution of substantive theorizing using rational choice theory, thus, is quite indicative. This research actually ends up looking very much like the rest of the literature—yielding a proliferation of models rather than explanatory variables—and does not seem to deliver on the promise of theoretical unification.[21] But, probably more importantly, it reveals something about adepts of rational choice theory in comparative politics. True practitioners of "economic imperialism" (Lazear 2000), such as economist Gary Becker, have sought to exploit the potential of rational choice theory to the hilt by applying it to all sorts of social problems and seeing how far it could be applied. In contrast, rational choice comparativists, much like the rest of the comparativists, have taken a view of paradigms that is closer to another economist, Albert Hirschman (1970), and displayed a willingness to drop or water down their paradigms when they have been shown to be a hindrance to their effort to explain politics. Indeed, comparativists as a whole have been less committed to a certain theoretical perspective than to answering questions about politics or, to put this in different terms, they have been less interested in exploring the "effects of given causes" than uncovering the "causes of a given effect" and have largely favored and conducted y-centered rather than x-centered research.[22]

This commitment to y-centered research has several implications. It has led comparativists to tilt toward the view that a battle of paradigms is "a largely sterile enterprise" (Eckstein 1998: 514) rather than "a source of great vitality and creativity" (Lichbach 2003: 141).[23] It has led comparativists to be quite accepting of theoretical eclecticism, and the messiness inherent in eclecticism,[24] and hence to a sense that, forty years after an assessment of the field concluded that "comparative politics is … in a preparadigmatic stage" (Holt and Richardson 1970: 27), things have not changed all that much. But it would be a mistake to suggest that the failure of work within a paradigm to unify theorization has meant that comparativists have abandoned the quest for theoretical systems and remained content to think in terms of one variable at a time. Indeed, a second approach to bringing order to theorizing, shorn of the language of paradigms, has been adopted by many comparativists.

This alternative approach to the development of theoretical systems is rooted in the core fact that the formation of research communities in comparative politics has taken substantive questions about politics, more than theoretical perspectives, as the key point

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[21] The issue of the potential benefits of formal theorizing, a different matter, is addressed below.

[22] The terminology about effects of causes and causes of effects is drawn from Mill (1874: 276).

[23] To be sure, some comparativists still seek to foster the sense that there are meta-theoretical traditions and metatheoretical debates across traditions. Indeed, the currently fashionable labels of "rational choice institutionalism" and "historical institutionalism" are used to that effect. But attempts to articulate the defining features of these alternative traditions (Katznelson 1997, Pierson and Skocpol 2002, Weingast 2002), as well as the attempts to find ways to integrate these traditions (Katznelson and Weingast. 2005), are strikingly contrived in nature. Likewise, the reviews of the literature produced by authors from this or that tradition—exercises that are carried out more than anything to publicize the advances that are owed to a certain tradition—appear forced because so many of the authors whose works are claimed by a certain tradition so clearly are not committed to any sort of a paradigm.

[24] This stance was formally articulated by many of the contributors to the 1995 *World Politics* symposium on the role of theory in comparative politic, and was nicely captured in Peter Evans' strong defense of what he calls the "eclectic, messy center" in comparative politics (Kohli et al. 1995).

of reference. Indeed, as David Laitin (2004a: 11-12) has argued, what unites comparativists and political scientists and gives intellectual coherence to their research is the shared interest in answering questions about dependent variables. Moreover, the key debates in comparative politics have centered on these substantive questions.[25] And, in the context of these grounded debates, some scholars have sought to build on middle-range theorizing and develop more comprehensive theoretical systems in response to problems that emerge in the literature. Indeed, attempts at this kind of synthesis have been quite common.

The field of democracy studies, once again, offers an example of how scholars have sought to build on middle-range theorizing and move toward stronger theories. Some of these efforts have focused on explanatory factors of democratization, Along these lines, noteworthy efforts include those by Ruth Collier (1999) to integrate the largely separate literatures focused on social classes and on elite strategies, and by Sebastián Mazzuca (2010b) to link the literatures on capitalist development and state formation.[26] Other scholars have focused on explanatory factors of democratic stability, as is the case of Barry Weingast's (1997) proposal to integrate the literatures on citizen values, divided societies, and elite pacts, and of Alfred Stepan's (2001: Chs. 9 and 15) effort to connect the literatures on federalism and nationalism. And yet other scholars, such as Carles Boix (2003: 2-3, Ch. 1) and Daron Acemoglu and James Robinson (2006: xiii, Chs. 6 and 7), have sought to break down the barriers that have traditionally divided research by seeking to explain democratization and democratic stability simultaneously.

As Daniel Little (1989: 27) has asserted, "there is no reason to expect that any single theoretical framework can explain a complex social and historical process." But the value of these efforts at theoretical synthesis should be recognized. Indeed, much like the trend in conceptualization to develop unifying frameworks by building on middle-range conceptual frameworks as opposed to overly abstract general frameworks, these efforts to produce general explanations by building on and consolidating middle-range theories are, as Merton (1968: 51) counseled, a more promissory path than efforts to build general theories from scratch and hence should be further encouraged. Thus, a greater emphasis on theoretical syntheses, which necessarily must address the bases for such syntheses and which should increasingly tackle micro-macro links, is needed to counteract the one-variable-at-a-time approach and develop stronger theory.

2.3. Methodologies of Theorizing

Finally, an assessment of theorizing in comparative politics must address the methodologies of theorizing. In this regard, the two modes of theorizing favored by comparativists, informal and formal theorizing, have some well-known strengths.[27] Informal or verbal theorizing, the more common mode of theorizing, has been

[25] For an example of how actual research agendas have actually evolved, see Munck (forthcoming) on the research on democracy in the context of Latin America

[26] Mazzuca's (2010b) work is interesting, furthermore, because it also exemplifies the "paradox of conceptualization." Specifically, Mazzuca's effort to improve on existing explanatory theories of democratization by integrating two literatures is linked with, and premised on, a reconceptualization of the explanatory outcome, democratization.

[27] One method that has not been discussed much in comparative politics is computer simulation. On computer simulations, and agent-based modeling in particular, see Lustick and Miodownik (2009).

remarkably fertile. In particular, the qualitative researchers who have largely relied on verbal theorizing have been particularly effective in using their substantive knowledge to theorize about causal processes. Indeed, it is only fair to recognize that many of the ideas that have driven theorizing in comparative politics have been initially broached by comparativists who have developed their ideas fully in verbal form. In turn, though some informal theorizing has relied on deductive thinking, the value of using a formal methodology of theorizing is the logically rigorous manner in which conclusions are deduced from assumptions. But the standard modes of theorizing in comparative politics have also been characterized by some basic methodological weaknesses that have implications for the empirical testing of hypotheses.

A key problem with informal theorizing is that the hypotheses derived from theorizing frequently are either stated in a vague form or in a manner that does not capture theoretical ideas correctly. For example, many times informal theorizing posits interesting arguments about interactions among variables, causal chains that distinguish between proximate and distal causes, feedback effects, time lags, threshold effects and so on (Pierson 2004). Yet, when it comes to stating hypotheses in formal terms—an essential step in preparation for testing—many comparativists do no more than identifying a certain factor as an explanatory variable or as a mechanism, or incorrectly summarize their efforts at theorizing by falling back on default additive and linear causal models (for examples, see Munck 2001b: 135-39).

Formal theorizing has its distinct problems. A lot of formal models seek to account for change (e.g. democratization) and, moreover, to specify the mechanisms of change. Yet the typical approach to derive testable hypotheses from formal models has been to generate comparative statics by analyzing how changes in the parameters of a model affect the model's solution. For example, a comparative static that Acemoglu and Robinson (2006: 80, 189-91) derive from their game theoretical model of democratization is that there is an "inverted U-shape relationship between inequality and democratization." But the advantage of comparative statics, the relative ease of statistically testing this kind of hypothesis with widely available data, is offset by a more important shortcoming. Specifically, comparative statics are not suited to questions of change: a comparison of two equilibria is not the same as a change from one equilibrium to another. Moreover, comparative statics shift the focus from process variables—such as the actors and choices that are featured prominently in game theory models—to macro variables, and thus restrict any reference to mechanisms to the realm of speculation.[28]

In sum, just as happens in verbal theorizing, the possibility of things getting lost in the translation that is required to connect theory to empirics is present in theorizing that relies on formal methods. Indeed, though the strengths of formal models should be recognized, it is equally important to understand that hypotheses are not derived automatically from models and that the sharp distinction between formal and verbal theorizing that formal modelers usually draw should be questioned (Johnson 2010: 287). Finally, and more importantly, comparativists should go to greater lengths to ensure that they capitalize on their theoretical work, placing emphasis on the proper formalization of the results of theorizing and on the development of hypotheses about mechanisms, and thus establishing a better link between theorizing and empirical testing.

[28] On the difference between comparative statics and comparative dynamics, and the limitations of comparative statics, see Harsanyi (1960).

3. Empirics I: Descriptions

Turning to empirics, the most basic aim of empirically oriented comparativist is to describe politics around the world. Yet to describe one must measure and generate data. And when the concepts being measured are abstract and multidimensional, as is the case of many of the core concepts in comparative politics, the challenge of measurement is a thorny one—it is essential to stress that measurement involves theorizing and testing—and the value of a good description considerable. Thus, it is a notable achievement that during the past twenty years the ability of comparativists to describe politics around the world has grown at a rapid pace, bringing about one of the greatest changes in field of comparative politics. Indeed, a very important change has taken place in comparative politics as comparativists increasingly recognized that, in addition to the use of descriptions as an anchor for theorizing and a stepping stone in assessments of explanations—if you do not have data to describe the world, you surely can't assess explanations—description is important in itself and good descriptions are only possible inasmuch as the distinctive challenge of measurement is confronted.

3.1. Baseline Practices and Knowledge

Research aimed squarely at producing political data got off to a start in the early 1960s. Important landmarks were the publication of the comparative survey data in Almond and Sidney Verba's (1963) *The Civic Culture,* and the efforts to develop large-N cross-national data sets on institutional and macro variables through initiatives such as the Yale Political Data Program set up by Karl Deutsch (1966). This line of research continued in the 1970s and 1980s. Indeed, during these decades some of the well-known survey data projects—such as the EuroBarometer and the World Values Survey—and macro data projects—such as the Freedom House indices and Polity—got their start. But it was really after 1989 that this work took off.[29]

The sheer amount of data that has been produced over the past 20 years is notable. As a result of the efforts of numerous scholars, data are now available on a range of key concepts related to elections and voting (by citizens and representatives), parties, citizen attitudes, corruption, political institutions, regimes, ethnicity and language, minorities, civil wars, state capacity, rule of law, and human rights. These data are collected for a large number of countries, and in many cases are global in scope, and are increasingly disaggregated at various subnational levels. Moreover, data production is not cut off from other aspects of research. That is, even though many researchers rely on off-the-shelf datasets, as Andreas Schedler and Cas Mudde (2010: 428) report, "in almost a third of the articles [in the field of comparative politics published in leading academic journals between 1989 and 2007] authors process self-made data, hunted and gathered by the sweat of their brow (32.8 percent)." Data production has become an integral part of research.

The critique that some data collection work in the 1960s amounted to "unadulterated Baconianism" (Holt and Richardson 1970: 58-65), that is, a search for facts without theory, probably still applies to some current data collection efforts. But it is

[29] For overviews of survey data projects, see Ashley and Appleton (2009) and Norris (2009); for an overview of non- survey data projects, see "The MacroDataGuide," at: http://www.nsd.uib.no/macrodataguide/

worth emphasizing how much work on measurement has a very clear sense of purpose. The centrality of normative concerns, registered in treaties and conventions that express a broad international consensus about certain values, is obviously behind the collection of data on human rights. Likewise, the central role of conceptualization and theorizing is apparent in many data projects. For example, the conceptual framework elaborated by Eckstein and Gurr (1975) was the point of departure of the Polity dataset, as were the conceptualization by Joseph Schumpeter (1942) and Dahl (1971) for many efforts to measure democracy (Munck and Verkuilen 2002). Moreover, in some recent work, theorizing about causal mechanisms is guiding work on measurement (McAdam, Tarrow and Tilly 2008, Stoll 2008: 1146-49). Indeed, the production of political data is largely guided by conceptual and theoretical agendas, and the payoffs of this work are evident.

In his 1963 book on *The Future of Political Science,* Lasswell (1963: 43) highlighted the need to collect "the vast body of data required to describe the changes in the distribution of power and in the structure and function of political institutions throughout the arena of world politics." Indeed, he elaborated in some detail how such a goal could be tackled (Lasswell 1963: Chs. 3 and 4). Today comparativists seeking to describe politics still face many challenges, to which I turn next. But the changes since the early 1960s are striking. In the 1960s and practically through the late 1980s, depictions of the social world relied mainly on standard demographic and socio-economic statistics. Now, in contrast, we have systematic descriptions based of a wide range of political statistics. Data on political indicators have also been used to develop typologies, such as Lijphart's (1999) well-known typology of democracies, and indices on all sorts of issues, such as state failure, governance, political regimes, presidential powers, ethnic fractionalization, party fractionalization, and electoral volatility. Lasswell would surely consider that much of what he was seeking has been accomplished.

3.2. Challenges and Methodological Questions

The main weakness regarding political data is the lack of measures of some key concepts and the problems of validity of existing data; hence, the key challenge regarding measurement is aptly summarized in the call for more and better data (see Table 4). The most important weakness is the lack of measures for some of the central concepts in political analysis. For example, we do not have comparative data to answer the question Dahl (1961) posed in the context of New Haven, that is, "Who governs?" And we have very few measures to describe the political process once a law has been enacted, that it, the entire process of implementation of laws. But the validity of much of the existing political data is also a matter of concern.

Important steps have been taken to improve the quality of data, a particularly critical challenge for survey data on poor countries (Seligson 2005), and many researchers have conducted assessments of multiple data sets on key concepts in comparative politics and provided valuable lessons about which data sets are more valid and reliable (Munck and Verkuilen 2002, Skaaning 2010). Yet the growing scrutiny of the quality of data, a positive development, has raised concerns about the validity of cross-national comparisons of survey data (Heath, Martin and Spreckelsen 2009, Medina, Smith and Long 2009) and data generated by expert raters (Hawken and Munck 2010). Indeed, many well-known and frequently used data sets suffer from problems as basic as

including within the measure of a certain concept indicators that are extraneous to the concept, including possible causes and consequences of what is being measured.[30]

Table 4. Assessing Comparative Politics III: Descriptions *

Current Practices and State of Knowledge		Challenges and Opportunities
Strengths	**Weaknesses**	
• Production of data bases of broad scope (even global in some cases) on a range of key concepts	• Lack of measurement of some key concepts	• The generation of more and better data, in light of past experiences and drawing on the complementary skills of qualitative and qualitative
• Growing scrutiny of the quality of data and assessments of multiple data sets on key concepts that offer guidance to data users	• Questions about the validity of data on many concepts	• The treatment of data validation as an integral part of data generation
• Work on measurement methodology	• Lack of a well-developed and formalized measurement methodology • A focus on reliability rather than validity	• Development of a measurement methodology

Note: (*) Though the focus is on the products of research, the process of research and, more precisely, the methodologies used in the process of research are addressed below the dashed line.

These problems have several implications. Most directly, doubts are raised about the use of data for the most basic of purpose of describing politics. More indirectly, it raises questions about research focused on explanation. As various authors have noted (Merton 1987: 2-6, Goldthorpe 2001: 10-11, Caramani 2010: 43-44), good descriptions serve to establish what needs to be explained and hence to anchor the development of explanatory hypotheses. Good descriptions can play a role in triggering ideas about possible explanations and in ascertaining the realism of assumptions made in explanatory theories. And, as will be discussed further below, good descriptions are essential to the testing of explanatory hypotheses. In other words, since the ability to describe is central to many aspects of the research process, the challenge regarding measurement facing comparativists is to treat data production as a central goal of research and data validation as an integral part of the process of data generation.

There are no quick fixes to the challenge of producing more and good data. Measurement is a process that entails theorizing and empirical testing—after all, the relationship between a theoretical concept and its indicators should be treated as a

[30] For example, measures of corruption routinely include indicators on purported causes of the level of corruption, such as anti-corruption legislation and agencies, and purported consequences of corruption, such as the costs to the economy. Likewise, measures of civil rights sometimes include indicators on purported causes of the level of respect for civil rights, such as the independence of the judiciary and a system of checks and balances.

hypothesis, which must be justified theoretically and empirically tested. Moreover, an attempt to generate more and better data should draw as much as possible from the lessons of prior attempts at measurement, and draw on the complementary skills qualitative and qualitative researchers bring to the distinct research challenges involved in measurement (Lieberman 2010). But even this is not all.

Part of the problem in tackling these challenges is methodological. Some work on measurement methodology has been done in recent years (Adcock and Collier 2001, Jackman 2008, Munck 2009). But there is a striking imbalance between the considerable attention given to methodological issues relevant to causal assessment and the relatively little attention given to methodological issues relevant to measurement.[31] Moreover, there is a tendency in the literature on measurement methodology to place much more emphasis on issues of reliability than those of validity, even though validity is *the* core issue and should not be confused with reliability. Thus, the elaboration of a suitable measurement methodology, which focuses squarely on the link between the concept being measures and the measures being proposed, remains an intricate part of the challenge of developing more and better data. As Andrew Abbott (1998: 194) counsels, "we should not assume that science must be about causality" and we should get "serious about description."

4. Empirics II: Explanations

The final step in the research process, the testing of explanatory hypotheses, plays a distinctive role in the assessments of theories, in that these tests yield answers that show that a hypothesis is right or wrong. And even though such tests never test theories as a whole but only some of the implications of theories, these tests have an important role in the development of knowledge. Thus, it is a sign of the health of comparative politics that comparativists are heavily oriented toward empirical research, that much of the empirical research conducted by comparativists aims at testing explanatory hypotheses, and that many key hypotheses about central questions in comparative politics have been repeatedly tested.

4.1. Baseline Practices and Knowledge

Comparativists rely on a variety of methods to test explanatory hypotheses. However, there has been an important shift in the methods comparativists rely on to conduct such tests. In the early 1980s, proponents of statistical methods had to go so some length to defend the use of statistics from criticisms by qualitative scholars (Jackman 1985) and there was a "widespread perception that large-scale aggregate data analysis [launched in the 1960s] ha[d] played itself out" (Sigelman and Gadbois 1983: 300). But during the past 20 years the tables were turned. Quantitative research has increased substantially since 1990, now accounting for slightly over a third of all empirically oriented articles in top comparative politics journals and virtually all

[31] Evidence of this imbalance is apparent in the allotment of space in the *Oxford Handbook of Political Methodology* (Box-Steffensmeier, Brady, and Collier 2008). Of the *Handbook's* 36 chapters only five (or 116 pages out of a total 808 pages) focus on "concepts and measurement."

empirical comparative articles in the most prestigious general political science journals (Munck and Snyder 2007b: 13, Schedler and Mudde 2010: 419-20).

This trend, made possible largely by the increased research on measurement and the growing availability of large-N data sets, is largely salutary. After all, the potential use of qualitative research to test hypotheses faces fundamental inherent limitations, including the obvious inability to assess the generalizability of claims and the less obvious but equally handicapping inability to draw inferences about explanatory factors due to the "many variables, small N" problem highlighted forty years ago by Arend Lijphart (1971: 686). Moreover, qualitative researchers routinely fail to specify in clear terms the hypotheses they seek to test, to distinguish what is general and what is idiosyncratic, to take steps to clearly distinguish the information they use to generate and test hypotheses (Munck and Snyder 2007b: 18-19, 21). Indeed, much of what passes as hypotheses testing in qualitative research is actually posthoc analysis and, in the best of cases, is so open ended that it cannot yield any strong conclusions. Thus, the use of statistical methods in testing hypotheses opened the possibility of overcoming the various limitations of qualitative methods.

Unfortunately, however, the problems with the standard forms of regression analysis used in comparative politics are also profound. Most basically, the focus on the dynamics of politics, that is, how things change over time, and on causal mechanisms, that is, the processes through which causes generate effects—two of the issues that qualitative research, for all of its limitations, highlights—is practically lost in statistical tests. Indeed, statistical tests in comparative politics have largely focuses on static relationships (even when time-series or panel data, rather than cross-sectional data, are analyzed) among macro-variables and thus offered tests that are linked with much theorizing in a tenuous manner. Moreover, though in principle statistical analysis is suited to draw inferences about explanatory factors and assess the generalizability of explanatory hypotheses, the actual results of standard regression analysis are routinely contested. Standard critiques focus on the questionable nature of assumptions concerning model specification (the functional form, omitted variables, reverse causality), the sample, unit homogeneity, measurement error, and estimation technique. But critiques also target a series of questionable practices, including data mining and the use of statistical significance tests.[32]

Given this state of affairs, it is not surprising that statistical research in comparative politics has failed to yield robust findings, let alone any substantially significant relationship. Indeed, it is not hard to find important research agendas where this is the case. A prominent example is provided by one of the most studied issues in comparative politics, the link between economic development and democracy and, specifically, the Lipset (1959) theses that economic development increases the prospects of democratization and the stability of democracy. Statistical research on the Lipset thesis was reignited by Adam Przeworski and Fernando Limongi (1997: 167), who argued that though "democracies are likely to die in poor countries and certain to survive in wealthy ones …"[there] are no grounds to believe that economic development breeds democracies" But this view was quickly challenged. Boix and Susan Stokes (2003)

[32] For a discussion of these problems in the social sciences broadly but that are applicable to comparative politics, see Sørensen (1998), Gill (1999), Achen (2002), Starbuck (2006: Chs. 2 and 3, 156-4), Taagepera (2008), and Freedman (2010).

countered that Lipset and modernization theory were right after all, that is, that economic development accounted for both transitions to democracy and the stability of democracy (see also Epstein et al. 2006). And soon enough Acemoglu et al (2008: 813) presented a statistical analysis that showed that there was "no evidence that income has a causal effect on the transitions either to or from democracy" and argued that neither Lipset nor Przeworski and Limongi were right. Thus, a lot of statistical research has so far failed to produce a robust finding that convinces a community of researchers.

The lack of robust findings is not limited to explanations of democracy. Indeed, when sensitivity analyses have been conducted, a healthy practice, they have not yielded encouraging conclusions. The results of statistical research on civil war are not robust to changes in the specification of the model (Hegre and Sambanis 2006) and the source of data (Ross 2004, Plümper and Neumayer 2010). The lack of the robustness of findings to changes in the dataset used in the analysis has been shown to affect research on institutions (Aron 2000), democracy (Casper and Tufis 2002), the number of political parties (Stoll 2008), and corruption (Hawken and Munck 2010). Even research on the impact of political regimes and institutions, despite being the focus of so much recent attention and the subject of statistical research by both political scientists and economists, does not escape this problem. Indeed, Przeworski (2004: 528) observes "how little robust, reliable knowledge we have about the impact of institutions" and Daniel Treisman (2007: 5) reports that "almost no robust empirical findings have been reported about the consequences of decentralization."[33] In short, a few notable exceptions notwithstanding,[34] the failure to produce robust findings affects many mayor fields of research in comparative politics in which statistical tools have figured prominently. If theorizing by comparativists has overcome the problem associated with general as opposed to mid-range theorizing—a propensity to produce "many 'approaches' but few arrivals" (Merton 1968: 52), the statistical research in comparative politics has run into a problem at the level of empirics—a propensity to yield many results but few conclusions.

4.2. Challenges and Methodological Questions

The response of comparativists to the problems faced in assessing explanations through qualitative and quantitative methods has given rise to some extreme solutions. Many advocates of qualitative methods just ignore the limitations of qualitative research and, furthermore, simply assert that the problems associated with statistical research does not affect qualitative research because qualitative researchers are not directly constrained by considerations of degrees of freedom and hence are able to solve the problems of inference faced in statistical analysis with less data, Indeed, they argue that qualitative research on comparative politics, unlike statistical research, has led to a considerable accumulation of knowledge and has even been able to explain outcomes in particular cases. The response offered by many qualitative methodologists, then, is to assert that the

[33] Even more troubling is that the results of some lines of research on the impact of political institutions have yielded different conclusions in patterned ways that give rise to suspicion (Przeworski and Limongi 1993: 60-61, Aron 2000).

[34] An exception is offered by research on the effects of electoral systems. As Shugart (2005: 50) states, "we have largely settled some of the core questions of the field—notably the relation between various electoral system variables and the number of parties and proportionality." On the distinctiveness of this domain of voting, elections and parties, see Grofman (2007: 145-47) and Taagepera (2008: Ch. 17).

rules of inference of qualitative research are distinct and thus that the problems faced by quantitative comparativists are overcome when a researcher opts for a qualitative rather than a quantitative culture.[35]

In turn, some advocates of experimental methods advance a methodologically defensible but also radical solution. They emphasize the old and well-known critique that all research that uses observational data, whether quantitative or qualitative, is problematic, and adopt a purist methodological position that holds that only causal inferences drawn from experimental data, that is, from randomized controlled experiments, are valid (Gerber, Green and Kaplan 2004). The problem, however, is that experiments are useful to answer a certain kind of question, about the effects of causes rather than the causes of effects which, as noted above, is what comparativists are largely concerned about. Thus, the solution to the problems of inference faced in statistical analysis proposed by some experimentalists amounts to saying that comparativists should select their questions in light of methodological considerations rather than pick their methods in light of their questions or, to use a common metaphor, that comparativists should look for their keys under the lamppost even though they know that they dropped their keys somewhere else.

Beyond the two extreme positions of those who pin their hopes exclusively either on qualitative or experimental methods to assess explanatory hypotheses, however, the healthy ongoing debate about the uses and merits of various methods has shown that other more fruitful options are open to comparativists (see Table 5). One promising option, which has been embraced, has been to recognize the limitations of any single method and combine multiple methods.[36] This option has been explored fruitfully in discussions about possible ways of combining quantitative and qualitative research, especially by using qualitative research as a check on the results of quantitative analysis. Indeed, there is a lot to the standard argument that qualitative research carried out in conjunction with quantitative research and focused on processes can serve to check many assumptions of statistical analysis, such as the assumptions pertaining to model specification, causal direction, unit homogeneity, and measurement error.[37] And the growing number of applications of mixed methods research illustrates well the potential of this approach to methods (for examples, see Sambanis 2004b and Lieberman 2009). However, two points about the current framing of mixed methods research in comparative politics deserve stressing.

First, though the use of qualitative research alongside statistical analysis helps to make up for some of the weaknesses of the latter, it does not produce a revised quantitative estimate. Indeed, while this version of mixed methods is valuable in that it does not discard a lot of information that bears on a certain question that has been addressed statistically, it does integrate the qualitative and quantitative components of the

[35] For this kind of argument about qualitative methods, see Mahoney and Goertz (2006) and Ragin and Rubinson (2009). For critiques of these views about the uses of qualitative research, see Kitschelt (2003: 51-56), Seawright (2005), Sekhon (2008: 281-90) and Coppedge (forthcoming: Ch. 5).

[36] Hand in hand with this effort to reduce the misapplications of statistical methods as well as misinterpretations of its results, some scholars have worked to develop statistical techniques to test the kind of hypotheses posited by qualitative researchers. See, for example, the work of Braumoeller (2003).

[37] For useful discussions about how quantitative and qualitative research might be combined, and the question of how cases should be selected, see Lieberman (2005), Fearon and Laitin (2008), Rohlfing (2008), and Seawright and Gerring (2008).

study in a formal manner, as is done for example in Bayesian statistics. Thus, if mixed quantitative-qualitative methods is going to constitute an alternative to standard statistical methods, a methodological challenge that needs to be tackled concerns the formalization of mixed quantitative-qualitative methods.

Table 5. Assessing Comparative Politics IV: Explanations *

Current Practices and State of Knowledge		Challenges and
Strengths	**Weaknesses**	**Opportunities**
• Frequent testing of many key hypotheses, by both qualitative and quantitative researchers	• A tendency to post hoc analysis in qualitative research •A lack of tests of many key hypotheses and of causal mechanisms • Regression analyses routinely rely on questionable assumptions and practices	• A greater reliance on qualitative research as a supplement to statistical analysis • The testing of hypotheses about mechanisms
• Sensitivity analyses, on various questions, to test the robustness of results to changes in the model specification and the source of data	• A lack of robust findings	• Incorporation of experimental and quasi-experimental designs as part of a broad toolkit • A return to basic (theorizing and measuring) and the conduct of stronger tests • More explicit distinction between theorizing and testing (out of sample testing), and stronger tests (not merely directional hypotheses)
• An ongoing debate about the uses and merits of various methods	• A lack of recognition of the limits of certain methods	• A formalization of mixed (quantitative-qualitative) methods • Clarification of the status of mechanisms in explanations and the methods for testing mechanisms

Note: (*) Though the focus is on the products of research, the process of research and, more precisely, the methodologies used in the process of research are addressed below the dashed line.

Second, though one of the core selling points of qualitative research in this context is its use to study causal mechanisms, the status of claims about mechanisms remains quite unclear. In particular, the tendency to contrast mechanisms to variables is

associated with the view that some of the standard concerns that apply to assessments of arguments cast in terms of variables and standard methods based on covariational analysis do not apply to arguments about mechanisms. Nonetheless, if arguments about mechanisms are going to have a status of more than interesting add-ons, as Laitin (2004b: 414-15) notes, "mechanisms eventually need to be formalized such that observable implications can be drawn and tested." Indeed, going beyond the current focus on identifying mechanisms, it is critical that hypotheses about mechanisms be formulated and alternative hypotheses about mechanisms be considered. Moreover, going beyond the current view that the study of mechanisms more or less necessarily calls for case studies, it is key to acknowledge that other methods to study mechanisms are available, including statistical methods (Opp 1998, Goldthorpe 2000: Ch. 5) and experimental methods (Stinchcombe 2005: 8-9, 166-70, Gould and Maggio 2007).[38] Thus, along with a clarification of the status of mechanisms in explanations, more attention should be given to the methods for testing mechanisms and the possibility of incorporating experimental research as part of a broad toolkit used by comparativists.

Another promising option, sometimes linked with the previous one, has been to emphasize research design. After all, stronger research designs make the data analysis easier and the conclusions of the data analysis more credible. This is the message coming from statisticians (Rubin 2008, Rosenbaum 2009). This is the direction in which many economists are moving, as they embrace quasi-experiments designs—such as regression-discontinuity, instrumental variable, and difference-in-differences designs—as a way to restore credibility to their empirical analyses (Angrist and Pischke 2010). And this is the option that several comparativists have endorsed and begun to explore through their analysis of quasi-experiments designs and natural experiments (Seawright 2007, Dunning 2008, Humphreys and Weinstein 2009).

And yet another option could be framed as a return to basics. That is, the failure to generate robust findings can be taken as an indication that there could be a problem with the concepts, theories and measures that a researcher has been using, as opposed to a problem with the methods of causal inference. Arthur Stinchcombe (2005: 20) puts it nicely: after testing a hypothesis, a researcher may "cycle between improving the theory, and then improving concepts and measures (with data) in the light of the new theories, revising mechanisms (so they are compatible with data on lower level units, for example), and deriving hypotheses to test again." Yet, to counter the danger of fitting the theory to the data and to add some bite to this advice, it is important that this stepping back from the task of causal inference should be directed to developing a stronger test, by providing not just a vague directional hypothesis but one that explicitly addresses the functional form and size of the posited causal effect, and to increasing the prospects of eliminating or at least clearly diminishing the credibility of some factors, something rarely done in comparative politics (Taagepera 2008). Thus, it is important that the ongoing debate about the uses and merits of various methods for causal inference be framed in terms of the broader set of activities that are involved in research.[39]

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[38] For methodological discussions about the study of mechanisms through experiments, see Imai, Tingley and Yamamoto (2010) and Green, Ha, and Bullock (2010).

[39] Another, related consideration regards the kind of knowledge that comparativists can realistically aspire to. On this issues, see the reflection from different angles by Dahl (2004), Elster (2007) and Przeworski (2007).

5. Conclusions

This paper takes stock of the field of comparative politics. It focuses in particular on developments since 1989, a period that might be characterized, along the lines of the behavioral revolution of the mid-1940s to mid-1960s, as a second scientific revolution (Munck 2007). To give a complete assessment, it addresses four distinct products of research: concepts and conceptual systems, theories and theoretical system, descriptions, and explanations. And, in each case, it draws attention to the strengths and weaknesses of current practices and the state of knowledge, as well as opportunities and challenges for future research. Thus, it presents a comprehensive evaluation of research on comparative politics, identifying areas where progress has been made as well as the limits of current knowledge, and offering various pointers to orient future research.

This conclusion will not offer a summary of the key points of the paper. The tables in each section present the main points in an accessible manner and there is no need to repeat them. An elaboration of how comparativists might effectively tackle the opportunities and challenges I outline, while critical, goes beyond what can be done in a brief space. However, by way of conclusion, I would like to make two general suggestions about the future evolution of comparative politics.

First, it is important that the role of synthesis is not slighted. Though some efforts at synthesis have been made, as identified in the text, comparativist by and large place a greater emphasis on analysis. Normative theory is distinguished from positive theory, theory from empirics, description from explanation, statics from dynamics, micro from macro, and so on. All these distinctions are based on important criteria. And the specialization of research, focused on these different aspects of knowledge, has allowed for important advances in knowledge. But it has also led to a growing problem: the lack of attention to relationships among all these aspects of knowledge and the creation of lots of bits and pieces of knowledge that never quite add up. Thus, comparativists should balance the urge to separate with a deliberately consideration of how distinct parts relate to each other and can be integrated. In short, it is critical that comparativists recognize that the study of politics calls for both analysis and synthesis.

Second, a related suggestion is that it is critical that comparativists not disregard the research-praxis nexus. Since 1989, comparativists have made great gains in terms of professionalism and knowledge about methods. These are hugely positive features that should be fully acknowledged and rewarded. But these gains have been somewhat one sided. Indeed, with only a few exceptions, the level of professionalism of comparativists is rarely matched by their level of passion for learning, and the level of methodological knowledge of comparativists is rarely matched by their substantive knowledge. Thus, comparativists should balance their inward look toward the profession with an outward look toward society. Asking ourselves if the results of our research is of any importance outside of the walls of academia and consciously seeking to break out of the walls of academia can have a very salutary effect. Not only can it bring focus to ones research by clarifying the distinction between the important and the trivial and can be a determining factor in keeping one's passion for knowledge alive. In addition, considering ones research from the perspective of a potential end user outside the walls of academic is probably the quicker, surest way to break down the walls that specialists and camps set

up within academic and restore substantive knowledge to its rightful place. Indeed, comparativists would do well to give greater attention than they have to the research-praxis nexus.

The field of comparative politics has a long history. And, when seen from this long-term perspective, the changes undergone by the field in the past 20 years are quite striking. Important strides have been made thanks to the efforts of a large and diverse community of scholars. But the past 20 years have also revealed the enduring value of earlier works and the profound nature of the obstacles comparativists face in their pursuit of knowledge. Comparativists ignore these insights at their peril. The only open question, then, is what sort of a blend of the old and the new comparativists will fashion as they seek to shape the future of the field.

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