

comparative politics: some points for discussion

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Abstract

This article is the result of concern about some developments in comparative politics, and it offers some points for discussion. It seems that three trends unduly confine the domain, scope and quality of research in the field. The subdiscipline (1) hardly deals with the social sources of political phenomena anymore and is disproportionately engaged with institutional analysis, (2) almost exclusively focuses on questions of (cross-national) variation and disregards important issues of similarity, and (3) too easily, and without reflection on the history of the field, produces 'new' theories and concepts in reaction to the charge that its central concepts (particularly the state) have become theoretically obsolete and empirically valueless.

Keywords comparative politics; variation and similarity; institutionalism; political sociology

A first observation is that the field of comparative politics increasingly seems to have been shying away from the attempt to understand the sources of political phenomena (politics as the dependent variable), including social power, and has increasingly focused on outputs and outcomes of political processes and political institutions (politics exclusively as an independent variable). However, some crucial issues seem to call for a rehabilitation of approaches rooted in political sociology.

Second, the subdiscipline has been focusing more on the question why political

phenomena differ between units of analysis rather than on the question of the similarity of political experiences. This is a result of the much applauded professionalization of the subdiscipline, but – important as it may be to phrase research questions in terms of variation – many fascinating and relevant research issues concern questions of similarity. The methodological stress on 'variation' now seems to be such that posing issues of similarity have become illegitimate.

Finally, the field has difficulties defending itself against the reproach that – in the wake of a host of developments

ending with '-ization' – its core concepts (such as 'country', 'state' or 'national political system') have lost their theoretical and empirical usefulness as units of comparative analysis. Prudence is called for in how matters of conceptual improvement and transformation are decided. Too often empirical verification of the '-ization' developments is lacking and too often conceptual 'innovations' are proposed in ignorance of the history of approaches and concepts in the field. There is a danger of reinventing poorer versions of the wheel time and again.

THE DISAPPEARING SOURCES OF POLITICS AND THE INSTITUTIONALIST PARADIGM

Comparative politics seems to have been shying away further and further from the study of the sources of political phenomena and has increasingly focused on the 'outputs, or even simply the outcomes of political processes and political institutions, and hence the attention to politics as an independent rather than a dependent variable' (Mair, 1996: 321).

Moreover, around the mid-1980s the field was captured by the neo-institutionalist revolution. By the mid-1990s the authoritative statement on the discipline by Goodin and Klingemann (1996a) signalled how far the institutionalist colonization had already proceeded. In the handbook they edited, politics was defined as the constrained use of social power, and political science was characterized as the study of the nature and sources of these constraints and the techniques for the use of such power, *de facto* excluding the sources of power as an object of research. Their discussion of power and their embrace of Dahl's 'power over' definition (A has power over B to the extent that A gets B to do something that B would not otherwise

do) reinforced their moving away from typically distributive definitions of power (Lasswell's 'who gets what, when, how' or Easton's 'authoritative allocation of values').

They strongly believed that political science was 'solidly in a period of rapprochement. The single most significant contribution toward that rapprochement was (...) the rise of the "new institutionalism"' (Goodin and Klingemann, 1996b: 11). Such theoretical rapprochement was taken as a sign of maturity of the field. Moreover, debates between political scientists, they argued, were only a matter of different emphases and 'concessions have been made gladly rather than grudgingly. They have been made, not out of a "live and let live" pluralism, still less out of post-modern nihilism. Rather, concessions have been made and compromises struck in full knowledge of what is at stake, what alternatives are on offer and what combinations make sense' (Goodin and Klingemann, 1996b: 12). This could happen, because political scientists were fortunately all institutionalists now.

Katznelson and Milner (2002: 4), in their introduction to the APSA millennium state-of-the-discipline volume, pointed out that the move away from political sociology has been a long-term trend. Political science has defined its own distinctive identity as an academic discipline 'by pushing certain areas into the margins. Demarcating itself from history, political science showed a greater concern for current events. To differentiate itself from sociology, it became relatively disinterested in the social bases of political action and inequality'. In their evaluation of the discipline's history and its current state of affairs, they recognize a clear focus on the state and its institutions and promote the continuation of this focus. Introductory textbooks routinely embrace the institutional approach (e.g., Mahler, 2008: 16–17).

It is particularly striking that there is no coherent research interest anymore, say, in the study of the social bases of politics (the subtitle of Lipset's (1983 [1959]) classic *Political Man*) that would come anywhere near the attention devoted to institutions and institutional output and outcomes. This holds for research on democratization (e.g., Przeworski *et al*, 2000), and also on models of democracy, where once upon a time the sources of social conflict played an important role in the explanation of why and how democratic institutional devices for conflict resolution evolved, but where these now seem to have lost all attention (e.g., in Lijphart, 1999). In the Munck and Snyder (2007a) volume on the heroes of comparative politics, Lijphart is even presented as the leading democratic political theorist who 'reintroduced the study of political institutions into comparative politics in the wake of the behavioral revolution, which had de-emphasized institutional factors in favor of attitudinal and sociological ones' (Lijphart, 2007: 234).

A final, and telling, example is Adam Przeworski's (2007) contribution to *The Oxford Handbook of Comparative Politics* (Boix and Stokes, 2007). It is worth quoting him at length:

Many research questions in comparative politics concern the impact of some institution, policy, or even event on some outcome, result, or performance. I will generically refer to the former as 'the (potential) cause' and to the latter as 'the effect'. Examples include: (1) The impact of political institutions on economic development. (2) The impact of political regimes on the initiation of wars. (3) The impact of electoral systems on the number of parties. (4) The impact of trade strategies on economic performance. (5) The impact of signing particular international treaties, say of signing the

'there is no coherent research interest anymore, say, in the study of the social bases of politics'

Kyoto protocol on carbon emissions. (6) The impact of revolutions on subsequent social change. (7) The impact of peace-keeping missions on peace. The list is endless: I just want to emphasize that the causes may include institutions, policies, and events. (Przeworski, 2007: 147–8)

Of course, research on political parties has continued to consider parties as actors that respond to changes in the social, economic and cultural environment. But it seems that this is currently identified as a *weakness* of this literature. So, Siavelis (2006: 368), in a handbook on political parties, argues: 'While analyses of political parties as dependent variables is certainly a valid enterprise, parties are also independent agents that frame issues and elaborate party platforms, affecting how cleavages translate into values, beliefs, and political behavior' (but see Mair, 2006).

It seems that the institutionalist revolution has been expelling political sociology to the margins of the mainstream research programmes or to other disciplines such as history and sociology. With it has gone the concern with the social sources of political power, distributive struggles and conflict. There are, of course, major exceptions or rather 'pockets of resistance', such as Ronald Inglehart's theory of value and belief change and how these affect politics and institutions (e.g., Norris and Inglehart (2004); see also Linder and Bächtiger (2005) and below). Interestingly or perhaps tellingly, however, Inglehart has only one

single entry in the index to the Katznelson and Milner (2002) volume. His work appears in the chapter by Laitin on comparative politics in a section where the impact of political culture on democracy is rather quickly dismissed as having the causal arrows going in the wrong direction (Laitin, 2002: 635; Muller and Seligson, 1994; but see Mishler and Rose, 2005).

Another example is the research on social capital: 'trust' (generalized reciprocity) arises in horizontal social networks and such trust is conducive to democratic stability and the functioning of democratic institutions. As with Inglehart's work, the authorities gathered in the APSA state-of-the-discipline are not impressed by this strand of research and Shapiro (2002: 260), in his review of the state of democratic theory, sets aside social capital research as follows: 'At present, (...) it is difficult to see a compelling case, conceptual or empirical, that low levels of civic trust are subversive of democracy' (but see Stolle and Hooghe, 2004).

Inglehart's work with Welzel provides theoretical arguments 'why it is more plausible that the dominant causal direction in the relation between human values and democratic institutions runs from values to institutions rather than the reverse' (Inglehart and Welzel, 2005: 159). 'Even the best-designed institutions need a compatible mass culture ... (O)ne cannot assume that making democracy work is simply a matter of having the right constitutional arrangements' (Inglehart and Welzel, 2005: 159–60). One strong argument against institutionalism is that there is 'no logical reason why – and no mechanism *how* – the sheer presence of democratic institutions could instil self-expression values in people' (Inglehart and Welzel, 2005: 171). Empirically, their findings 'support a cultural explanation of democracy and disconfirm an institutional explanation

of political culture' (Inglehart and Welzel, 2005: 209).

To avoid misunderstanding: I am not arguing that the institutionalist turn in political science was wrong or has been unproductive. That would be ridiculous and entirely unfair. However, it seems unnecessary to push the study of the socioeconomic and cultural sources of political phenomena entirely to the margins or to think too little of contemporary work in this tradition. Moreover, there is a compelling methodological-substantive reason for re-assessing a political sociology approach in the light of the institutionalist paradigm and its shortcomings.

There is the well-known, but insufficiently acknowledged or openly admitted, challenge to comparative research, namely the problem of endogeneity. This is, in Franzese's (2007: 61) words, the difficulty that 'almost everything causes almost everything else'. Recognition of this problem should already make any claim for the *a priori* primary importance of institutional explanations sound suspect, or as Przeworski (2004a: 168) puts it: 'Everything, and thus nothing, is "primary". The only motor of history is endogeneity'. This generic problem is particularly pressing for institutionalist researchers, whether of the rational choice or historical persuasion, because 'the institutions seen as constraints on politicians are themselves routinely changed by politicians' (Munck, 2007: 56), so that their explanatory claim is doubtful. The point is that institutions may or may not have one-directional explanatory power, and the difficult task is to argue and show which causality is at work in which context, in the light of a potential opposite direction of causality in the same or other context, in other words, to disentangle analytically and empirically multidirectional causality (see Przeworski, 2004a, b).

Although Przeworski (2007; see above) stresses the 'politics as independent variable' designs as typical of comparative politics research, he also points out that – related to the problem of endogeneity – many (if not all) questions of the proposed causal effect of political institutions can only be answered if we know where the causes (the institutions) come from: 'Hence, we need to study *causes of effects* as well as *effects of causes*' (Przeworski 2007: 148).

Let me illustrate these points with the puzzling case of the continuing – albeit variable – relevance of religion for politics, as in the case of Christian democracy. The political sociology approach *prima facie* does not seem helpful here because – generally – its theses on the decreasing importance of social structural characteristics for party affiliation and choice cannot cope very well with the variable survival of class- and religion-based mass political parties. Historical institutionalism may provide a better answer, particularly because it seems capable of identifying the self-reinforcing mechanisms that play a role in the survival of cleavage-based political movements, even in the absence of the relevant original social or cultural cleavage (see for the general theoretical point: Pierson and Skocpol, 2002). Christian democracy is part of the party system *status quo*, and the in-built institutional mechanisms reinforce the tendency to reproduce itself.

Secularization, a social structural phenomenon, is obviously affecting Christian democracy, but why and how is not well understood. Analyses tend to rely on the standard 'secularization paradigm' and neglect recent advancements and debates in the sociology of religion. Secularization as a social condition is manifest in: '(a) the declining importance of religion for the operation of non-religious roles and institutions such as those of the state and the economy;

'institutions may or may not have one-directional explanatory power and the difficult task is to argue and show which causality is at work'

(b) a decline in the social standing of religious roles and institutions; and (c) a decline in the extent to which people engage in religious practices, display beliefs of a religious kind, and conduct other aspects of their lives in a manner informed by such beliefs' (Bruce, 2002: 3).

In this paradigmatic perspective, one can expect the 'natural' clientele of a religious party to wane, when church membership and church attendance declines and when the political significance of religion among loyal church-goers weakens. However, secularization can also be taken to represent the translation of a religious morality into a secular ethics and culture. As such it comprises a transformation of religious contents into worldly substance, as a result of which the open religious influence on politics tends to vanish and is supplanted by a 'diffuse moralism throughout society' (Mead, 1983: 52–3). Therefore, the 'natural clientele' of Christian democracy is likely to be bigger than assumed, on the condition that the movement adjusts to secularization and starts to appeal to this diffuse moralism, stripping off the explicitly Christian coating of its political message, thereby itself becoming a force of secularization. Because secularization has a double effect – on the one hand eliminating the direct impact of religion on political attitudes, but on the other hand diffusing the (in origin) Christian

ethics throughout society – there is no linear, one-directional causality between secularization and the support for Christian democracy.

The point I am making is that such types of political sociology-inspired reasoning might help improve (historically lacking) institutional explanations of the variable fate of cleavage-based politics. When one addresses the wider question of the *re-emergence* (rather than the continuing relevance) of faith and religion for politics, one crucial observation is that this occurs in widely varying institutional contexts. Here, clearly the institutional approach faces difficulties and the political sociology questions gain relevance: why, how and under what conditions does religion (again) become a source of social and political identity and of political opposition and conflict? (see for a nuanced review of the literature Wald *et al* (2005); on fundamentalism, see Fox and Sandler (2004); on the resurgence of political Islam, see Sutton and Vertigans (2005)). For diverging puzzles such as ethnic cleansing, and right-wing and xenophobic populism a political sociology approach is also helpful, if only because such puzzles are so explicitly linked to the social and cultural sources of politics.

VARIATION AND SIMILARITY

The second observation is that comparative politics as a professional discipline has been focusing more and more on the question of why political phenomena *differ* between units of analysis. In the last decade or so this tendency seems to have become even stronger. Mair (1996) more than ten years ago still defined the goal of comparative politics as 'the systematic comparison between countries, with the intention of identifying, and eventually explaining, the *differences and similarities* between them' (Mair,

'Most research puzzles or problems concern differences rather than similarities'

1996: 310; italics added). However, Apter (1996: 372; italics added) in the same handbook argued that its goal was: 'to determine *what difference differences* make between the ways power can be deployed (...)'. Laitin (2002: 630; italics added) argued that substantively 'research in comparative politics seeks to account for the *variation* in outcomes among political units on consequential questions that have been posed in political theory'.

Most research puzzles or problems concern differences rather than similarities, variation rather than resemblance, change rather than stability. This is not a new phenomenon. Peter Flora (1999: 12) makes the following observation on the work of Stein Rokkan: 'Rokkan was less concerned with why the territorial state succeeded *everywhere* in replacing the other types of political system, or why cultural features became more important *everywhere* as defining characteristics of political membership, or why institutions and organisations of mass political participation emerged *everywhere*; what intrigued him was why these general processes of state formation, nation-building, and democratisation took such different forms'.

By way of illustrating the preoccupation with variation, I took the issue of the *European Journal of Political Research* (EJPR) (2005, 44, 6) available to me when preparing this section. The puzzle of the opening article by Daugbjerg *et al* is the variation in farmers' perceptions of agriculture's dependence on support. Their explanation: 'Farmers receiving direct aid payments, which are a highly visible means of agricultural support, were more likely to state that agriculture

was dependent on support than were farmers receiving price support, which is indirect and thus a less visible support measure' (Daugbjerg *et al*, 2005: 763). The next article by Jacobsen (2005) is concerned with the variation in public sector reforms, and tests whether it is the implementation by the bureaucracy that explains this. The finding is that it does not. Sakamoto (2005) tests the well-known hypothesis in comparative political economy that the variation in economic performance is an effect of type of government, in which weaker governments (e.g., multi-party or minority) are expected to have a poorer record than strong and stable ones. The finding is that this all depends on whether the Central Bank is independent or not. The third contribution by Veugelers and Magnan (2005) aims to explain the cross-national variation in support for new radical right parties between 1982 and 1995. It is due to the variation in the restructuring of the space of party competition. Then Linder and Bächtiger (2005) challenge the standard economic explanation of the variation in democratization and introduce culture (especially the negative role of extended family and kinship ties) as the answer to the puzzle of differences in democratization in Asia and Africa. Lawrence Ezrow's study (2005) shows that the puzzle of the variable electoral fate of parties (vote share) is partly solved: parties gain votes when they move to the median voter position. Finally, Adams and Merrill (2005) follow up on this and find that parties and candidates are punished moderately when they move away from the centre, but that such penalties can be overcome by non-policy-related advantages arising from economic voting. In sum, *every single article* poses a puzzle of variation and dissimilarity among political units, *none* one of similarity.

The same exercise with the other leading European Political Science journal,

West European Politics (WEP), and with other issues of both journals yielded similar results, with one notable exception, namely Caramani's argument that the 'left-right dimension that imposed itself everywhere in Europe during processes of nationalisation and that caused a fundamental similarity between European electorates *today permeates the EU party system (...)*', so that there is '(...) an overlap between European and national constellations indicating that the main national alignments also structure the European party system' (Caramani, 2006: 20; italics in original; see Caramani, this issue). Of course, this is admittedly somewhat an impressionistic form of providing evidence. And unfortunately, the data that Munck and Snyder (2007a) have gathered do not report on the type of research problem. So let me then conclude that my impression is that there is a fixation on variation.

Why does the problem of similarity tend to disappear? Perhaps part of the answer lies in the maturing of political science as a *scientific* discipline since the behavioural revolution, which, according to David Ricci (1984), has been the unfolding of a tragedy. The tragic aspect lies in the trade-off between, on the one hand, the social scientific principles (modelled on either positivism or the Popperian hypothetico-deductive method) that are increasingly demanded in political science, and, on the other hand, the evasive features of politics that cannot be redefined such that they accord to those principles. Exaggerated as Ricci's claim may have been then or still is, even since 'the second scientific revolution' of rational choice (especially in the US; see Munck (2007); Munck and Snyder (2007a)), it may be the case that many of the questions of similarity are indeed typically difficult to formulate so as to fit the dominant prescription of maximizing variation on the dependent variable. By their nature, puzzles of variation are

more easily set in the language of dependent and independent *variables*, and made ready for technically robust quantitative or qualitative analysis. Puzzles of similarity are suspect because of the risk of biases (see below).

Another part of the answer may be found in the observation that political science has abandoned, or perhaps never really believed in or simply failed to implement, the project of formulating a general empirical theory of politics (see Easton, 1965: 14, 15; Easton, 1997: 26–7; Munck, 2007: 33). The trend away from general theory and from the similarity questions has been broad and profound. Mair (1996: 316–7) made the observation that comparative politics has experienced a shrinking of the *scope* of comparison, consequently toning down the ambitions of producing a general theory of politics and therefore descending from a very high level of conceptual abstraction. In the course, the discipline has turned to middle-range theories. The rediscovery of institutions and ‘the state’ in the mid-1980s and the ensuing colonization of the discipline by the various institutionalisms were therefore not so much a shift in paradigm, but rather a shift in scope or level of abstraction. Corresponding to this shift, the discipline turned away from questions of stability and similarity to focus on issues of variation and difference instead.

This is not to argue that problems of variation and differences are unimportant or that the prevailing principles of the subdiscipline need to be adjusted radically. However, there is no reason to dictate to the discipline the necessity to ask *only* questions of variation. I am generally sympathetic to the cause of King *et al* (1994; but see Brady and Collier, 2004; Johnson, 2006), but I really see no point in inculcating in students’ heads the command that ‘all social science requires comparison, which entails judgements of which phenomena

‘there is no reason to dictate to the discipline the necessity to ask only questions of variation’.

are “more” or “less” alike in degree (i.e., quantitative differences) or in kind (i.e., qualitative differences)’, unless the more or less can be taken to refer to issues of similarity too.

I think there is a case to be made that some of the big questions from a comparative perspective are questions of similarity and that these provide us with some of the most intriguing puzzles. This has little to do with the problem of selection bias and whether one should always maximize variation on the dependent variable to avoid this. There is a difference between (un)consciously selecting cases on a single outcome on the dependent variable and observing that the variable tends to have only one outcome. Of course, in the latter case, it is still possible to design the study such that variation is maximized; for instance, by introducing a longer-time perspective, when a snap shot, cross-sectional sample of the variable yields only a single outcome. But my point rather deals with the apparent obligation to construct puzzles in terms of variation, while the really interesting puzzles are rather problems of similarity where, theoretically, variation is expected. What is the bigger puzzle: that levels of political disengagement vary somewhat between advanced democracies or that – despite widely varying institutional arrangements, political histories, cleavage structures, cultural traditions, socioeconomic conditions, etc., and despite the worldwide popularity of democracy as a political regime – all well-established democracies are confronted with the same problem of political

disaffection? Surely, the manifestations vary, but is it not more puzzling that the decline of engagement is a feature of democratic systems *everywhere* and more or less *at the same time* (Van Kersbergen, 2010)? If the pressure to deal with problems of variation only is such that such puzzles of similarity can no longer be raised legitimately, then this *is* a tragedy for comparative politics.

CORE CONCEPTS

The third, less essential observation is that comparative politics is permanently attacked from within and from without for its theoretical obsolescence, because its core concepts of 'country', 'state' or 'national political system' are said to have lost their usefulness as units of analysis. The increasing interdependence of national states and the decline of the autonomous power of public decision-making are argued to imply that cross-national research is meaningless. The subdiscipline is reprimanded for not having the conceptual tools to explain the growing dissociation between authoritative allocations, territorial constituencies and functional competencies (Schmitter, 1996).

Theoretically and empirically, the sweeping 'end-of-the-state-and-therefore-of-the-subdiscipline' arguments have lacked theoretical coherence and empirical verification and were for a large part characterized by conceptual sloppiness and confusion (see Van Kersbergen, 1999). Although since, say, the late 1990s (but see Poggi, 1990) work on the state has moved away from overly speculative accounts (but see Walby, 2003) and tends to underline the continuing and crucial importance (and resilience) of the state (see Paul *et al* (2003) and the contributions on the section on the state in Katznelson and Milner (2002)), the unreasoned and

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unsubstantiated preponderance of 'end-of-the-state' arguments are still around (e.g., in the governance literature, see Van Kersbergen and Van Waarden, 2004).

It is difficult for the subdiscipline to defend itself against the reproach that its core concepts have lost their theoretical and empirical usefulness as units of analysis. Still, my plea is (1) for prudence in how we decide on matters of conceptual innovation and transformation, particularly by encouraging empirical substantiation of the sometimes overly speculative claims about the obsolescence of theories and concepts before we accept them; and (2) for promoting a raised consciousness about the history of the concepts and approaches of the subdiscipline, in order to avoid reinventing the wheel. Whether we, to quote Schmitter (2006: 29), should be 'starting all over and creating a whole new language for talking about politics and analyzing politics', seems to me a question, not an answer, and prudence as well as an eye on the history of comparative politics would guide us in evaluating properly the 'newness' and 'innovativeness' of alternative concepts and theories.

Take the fashionable term 'governance' (see Van Kersbergen and Van Waarden, 2004), which according to some is brand new. For instance, an otherwise well-informed book (Tiihonen, 2004: 9) starts as follows: 'We can say without exaggeration that governance with its different specifications is a term of the day. It is a new term, which does not have a long history. Even the history of this book is

longer than the record of the term'. However, the concept 'governance' is not new at all, but has been around for a very long time and has always been applied to the type of problems (such as overlapping political and legal jurisdictions, complexity, governing without the state) for which it is currently used. In fact, the oldest reference I have come across is to a famous English text by Sir John William Fortescue *The Governance of England* (otherwise called *The difference between an absolute and a limited monarchy*), written and published somewhere between 1471 and 1476, in which, incidentally, the (at the time!) reluctance of the French to rebel is explained as follows: 'Poverty is not the cause that the common people of France do not rise against their sovereign lord. (...) but it is cowardice and lack of heart and courage, which no Frenchman possesses like an Englishman' (141–2).

Surely, comparative politics is facing many difficulties. The internationalization of the economy and European integration do, of course, affect the politics (including the policy space and policy capacity) of national states. The consequences for a national state of such challenges, however, still depend, to a large extent, on its evolving domestic policy capacity and political adaptability and its political capacity in the international arena. Such capacities and adaptability concern issues of political power mobilization and coalition formation, certainly also at the domestic level. What has been widely interpreted as the enfeebling of the national state is in fact very often a transformation of the *sources of power* upon which actors at various levels (can) draw and of how the relationship between state power and civil society is structured. External requirements and constraints have an impact upon the internal logic of political conflict and cooperation. Seen from this perspective, it is not obvious that we need

an entirely new set of conceptual tools in order to be able to deal with the increasingly complex nature of domestic and international dependence, diffusion, and multi-level governance. Integrating the bodies of knowledge that we already have (from comparative politics and international relations) seems a more prudent strategy.

CONCLUSION

First, mainstream comparative politics has lost interest in the sources of political phenomena, and focuses on the outputs and outcomes of political processes and institutions. This is mirrored in the institutionalist colonization of the field. We need to re-assess this because of major weaknesses in the institutional approach, but also because we need political sociology for explaining political phenomena for which institutional approaches seem ill-equipped. Second, in the subdiscipline puzzles of variation rather than similarity are dominant and permitted. Although 'variation' is important, this should not imply that intriguing issues of similarity must fade away completely. My proposal is to rehabilitate questions of similarity as they touch upon some of the most intriguing problems we face, even if it implies 'violating' some cherished scientific principles. Third, there is a tendency to declare comparative politics obsolete because its conceptual apparatus cannot cope with phenomena that indicate the evaporation of the cherished unit of analysis: the state. My plea is to be prudent with respect to any proposed conceptual revolution of this sort, to make sure that one knows the conceptual and theoretical history of the field and to examine critically and empirically any claim that the traditional tools of comparative political studies are superseded.

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