What is new in the study of policy diffusion?

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Abstract

This paper surveys what is new in the study of the international diffusion of policies. We critically review the most recent contributions on the topic, trying to identify the substantive and methodological innovations in this literature. On the theoretical side we argue that, whereas it is acknowledged that the various diffusion mechanisms overlap, hardly any attention is devoted to the way in which they interact (either with other mechanisms of diffusion or with domestic factors). More generally, mechanisms are studied separately and are not integrated into a coherent model of diffusion. On empirics, we argue that existing studies have concentrated on policies that seem to have diffused in an explosive way even though both theory and methods are appropriate to study *any* type of diffusion. More attention should also be paid to modeling diffusion processes to account for the fact that causal patterns may be highly heterogeneous in time and space. We show that the latest wave of studies on diffusion may have had the unintended consequence of opening up new questions. Some of these questions are about the mechanisms that initiate rather than accelerate the diffusion of policies and institutions, why policies diffuse in clusters rather than globally, why some policies diffuse faster than others and why some policies do not diffuse at all. Finally, we present and discuss the contributions in this issue, which tackle many of the points posed above.

1. Introduction

In this introduction, we discuss what is new in the study of policy diffusion and discuss the contributions to the present issue.

The internationalization of policies and politics is catching the attention of a growing number of scholars in the field of international political economy and comparative public policy (Simmons, Dobbin and Garrett, 2006; Levi-Faur and Jordana, 2005; Knill, 2005; Weyland, 2004, 2005). The general motivation of this work is that existing explanations of policy choices do not pay sufficient attention to their international determinants. True, comparative political economy has been prolific in the study of domestic responses to international shocks, and successful at showing how those responses vary depending on local institutions and political and economic conditions. Hence, such studies emphasize divergent policy choices in countries confronted to common conditions or, in other words, different ways of implementing policies in those cases in which policy choices were the same.

However, the recent wave of democratization, economic liberalization, deregulation and re-regulation, together with the sense that policy choices are growing more alike, call into question the specification of those models. For example, existing studies do not adequately explain why the degree of variance in the world with respect to capital account openness, political regime type and the revenues derived from privatization of state-owned enterprises (SOEs) more than halved in the 1980s and 1990s (Simmons, Dobbin and Garrett, 2006). We describe this process of growing *global* similarity in policies as "*convergence."* It could be the case that the convergence in policy choices was the outcome of the independent discovery of the best practice by independent units. But it could also be the case that choices in one country affected the choices of other countries in the same direction. This is what we define as

"diffusion." Note this important distinction: policy choices may converge for reasons other than interdependent decision making. For example, countries may choose the same policies in response to the same exogenous shock but *independently from each other*.[1] As well, recent studies show that policies may diffuse without causing policy convergence at a global level. This may happen if policies diffuse around different policy equilibriums or, in other words, if there is not a single policy stance to which countries converge. In that case, we would observe clusters of policies and institutions along some dimension. Thus, policies may diffuse in clusters, but this may not imply that policy choices converge globally (see Plümper and Schneider, this issue).

Alternatively, as we explain below, policy choices may converge globally for reasons *other* than diffusion. Thus, according to our definition, diffusion is only one possible cause of policy convergence, and convergence is only one possible outcome of policy diffusion. We believe that the distinction between *diffusion* and *convergence* is crucial. We also contend that this analytical distinction is important in arguing against the selection bias that characterizes current research: we do not need to concentrate only on policies that have spread globally and explosively to learn about the diffusion of policies and institutions.

Research on the internationalization of policies seeks to improve the specification of current models of policy choice so as to take into account the possibility of diffusion. The general strategy consists of adding an operationalization of diffusion to the right-hand side of the equation to test the null hypothesis that *only* domestic socioeconomic and political variables explain a particular policy choice. As we discuss later, this null hypothesis is rejected in studies covering a wide range of policy choices: capital and current account liberalization, privatization, regulatory policies, trade liberalization, and independent central

banks among others are proved to have diffused across space and, in a few cases, very fast. Hence, it appears that previous models of policy choice missed an important part of the story.[2]

But this may not be -- or, better, should not be -- the only contribution of the internationalization research agenda. We are particularly interested in discussing whether the mechanisms and methods used in these studies can help to illuminate diffusion processes in general, not only the most recent wave of liberal economics. Indeed, a good many of the contributions we review acknowledge that the wave of political and economic liberalization followed a wave of authoritarianism and economic closure of a similar nature even if less deep (Guisinguer, 2005; Kogut and McPherson, 2007, Simmons, Dobbin and Garrett, 2006; Elkins, Guzman and Simmons, 2006; Quinn and Toyoda, forthcoming, 2007). Thus, if diffusion is not a new social phenomenon, what is new in the study of diffusion?

The answer is quite paradoxical. We argue that, *substantively* speaking, there is nothing new in the study of diffusion: diffusion processes have always existed and have long been acknowledged under the Galton's problem rubric (see, for example, Ross and Homer, 1976). *Theoretically* speaking, there are few innovations; as we shall see, all diffusion mechanisms identified in the literature have long traditions in international relations, public policy and sociology. *Empirically* speaking, the use of modern quantitative techniques to explore policy diffusion provides us with an impressive amount of data and original results. Yet we hold, and the contributions in this volume demonstrate, that these techniques are still insufficiently developed. On the positive side, we argue that the true novelty of recent diffusion studies is the questions that these studies pose and that, surprisingly, qualify the sweeping character attributed to the last wave of political and economic liberalism.

A critical review of recent works on the topic and the presentation of the special issue follows. In section 2, we discuss a few conceptual issues regarding the mechanisms spurring diffusion. In section 3, we delve into the empirical results of these works, with special emphasis on the historical periods and samples surveyed, the nature of the policy choices being analyzed, and the analytical techniques used. In section 4, we describe and discuss the contributions to this special issue, which in our view constitute a major step forward both conceptually and empirically in the contemporary study of policy diffusion, convergence, and globalization. Section 5 concludes.

2. On mechanisms of diffusion

Policy and institutional diffusion is not a new topic. It dates back to at least 1889 (see Ross and Homer, 1976: 1-2). The fact that observations in comparative politics are most likely not independent is discussed in virtually all methodology texts under the Galton's problem rubric (though typically just in a few lines). In many comparative studies, the possible existence of diffusion is briefly acknowledged and often soon forgotten.[3] An early exception is the study by Collier and Messick (1975), who explicitly analyzed the adoption of social security programs in 59 countries as an interdependent choice. On the other hand, a well-established literature concentrates on the diffusion of policies in the US states (for example, see Berry and Berry, 1990; 1999; Berry, Fording and Hanson, 2003; Mintrom, 1997; Mintrom and Vergari, 1998; Grossback, Nicholson-Crotty and Peterson, 2004; Shipan and Volden, 2006; Volden, 2002; 2006). These studies aim to separate the role of interdependencies from that of state-level variables on policy adoptions, but rely on a quite basic conceptualization of diffusion.[4]

A main focus of the recent diffusion literature is on the mechanisms by which policy choices in one country may influence policy choices in others. It is standard to distinguish among (1) diffusion promoted by dominant actors, which falls into a realist account, (2) diffusion due to social emulation, which falls into a constructivist view, (3) diffusion resulting from economic competition, and finally (4) diffusion caused by learning from others. Cooperative, as opposed to competitive, explanations may lead policies to diffuse through the creation of network externalities (Milner, 2006). To be brief, *coercion* is the imposition of policies on national governments by powerful international organizations or powerful countries. *Emulation* is a process whereby policies spread because they are socially valued independently of the functions they perform. *Competition* is a process whereby governments that compete for the same resources adopt the policy stance of their competitors for fear of an economic loss in case they deviate. Finally, *learning* is a process whereby the experience of others supplies relevant information on the outcomes of a given policy.[5] Note that this list of mechanisms partly overlaps with the "channels of diffusion" developed in the sociological literature. These channels include competition, prestige, spatial proximity, cultural reference groups, and learning (see Strang and Soule, 1998).

We argue that some of these mechanisms are misplaced and actually do not accord with the standard definition of diffusion. In addition, the conceptualization of these mechanisms has not led to a theoretical model of diffusion (Braun and Gilardi, 2006). The implicit model is one where each mechanism is a sufficient condition for increasing the probability of policy adoption, and in which the effect of each mechanism adds to that of the others. It is also a model that does not account for the existence of feedbacks among the proposed mechanisms of diffusion. This view largely corresponds to the "general linear

reality" denounced by Abbott (1988), and it is not a satisfactory approach. Moreover, each mechanism has a specific theoretical anchorage, which only complicates things: some of these mechanisms entail diffusion via a change in governments' incentives (competition and cooperation) whereas other mechanisms promote policy diffusion via a change in information about policy outcomes (learning).[6] Note also that there are mechanisms that presuppose fully rational actors engaged in complex learning processes or strategically anticipating and reacting to the actions of competitors. But we also have mechanisms such as emulation that assume that the importance of any function that policies may perform is dwarfed by their symbolic dimension. This variation is problematic because it precludes the development of a coherent model of policy diffusion (Braun and Gilardi, 2006). An immediate consequence of this lack of theoretical coherence is that the specification of empirical models is not driven by theory, but only by the attempt to include as many mechanisms on the right hand side as the data allow in a simple, additive fashion. We return to this point later.

One of the main difficulties in adjudicating among mechanisms of policy diffusion both conceptually and empirically is to distinguish between what is realist and what is constructivist. Actually, there are numerous overlaps among the different mechanisms of diffusion proposed in the literature (Simmons, Dobbin and Garrett, 2006). To start clarifying things, we consider it necessary to distinguish between *coercion* and diffusion. In particular, convergence on policies that may result from external pressure by supranational or international entities, such as formal conditionality, has nothing to do with diffusion as defined in this article and others, that is, countries influencing other countries, which in turn adopt the same policies without the mediation of any pressure.[7] Coercion may be a cause of worldwide policy convergence, but, if one is to be conceptually coherent, it should not be

considered as a mechanism of policy and institutional diffusion. Moreover, even if one considers international institutions as determinants of policy convergence via coercion, distinguishing when these organizations are imposing – thus, removing all agency from one of the parties -- from when they are persuading and being carriers of norms and ideas is far from easy. [8] Certainly, the give and take of political negotiations among countries can be seen as exercises of power; but there are also elements of persuasion and coincidence resulting from professionalization and socialization within networks. Thus, the realist and the constructivist accounts overlap when it comes to interpreting coercion. Besides, this is not a new topic in international political economy (Haas, 1992).

As for the *economic competition* mechanism, it is subject to both strategic and coercive interpretations. The basic idea is that the adoption of a particular policy in one country creates a comparative advantage in that country's favor that "forces" competitors to make the same move under the threat of suffering an economic loss. This mechanism has been used by Simmons and Elkins (2004) and Elkins, Guzman and Simmons (2006) to explain why countries that compete for international capital liberalized their capital accounts and why countries entered into bilateral investment agreements (BITs), moving away from multilateral arrangements. Note that one may interpret economic competition as coercion of a decentralized type. In this case, it is the market that proffers the sanctions. Thus, there is another overlap between the realist and the competitive explanations. When a country adopts the same policy stance as another, more powerful, country, not only is power involved but also market power playing a forceful role in causing convergence around a salient policy.

Learning is a likely mechanism of policy diffusion. Countries' policy choices are natural experiments from which others may learn. Given that governments are interested in

faring well and that policymaking is a process plagued with uncertainties, the experience of others provides a free source of information about what works and what does not work. In its rational version (Meseguer, 2004; 2005; 2006), governments scan all available information and converge in their beliefs about the expected outcomes of policies and, consequentially, in their choices. In the bounded version of learning, limited analytical capabilities preclude governments from considering all available information. Rather, politicians use cognitive shortcuts to analyze others' experience (McDermott, 2001; Weyland, 2004, 2005; Elkins and Simmons, 2005; Elkins this issue). Under bounded learning, governments pay more attention to nearby experience and to outstanding performance, overestimating initial success. Note that this version of learning also overlaps with some elements of the constructivist view: when sociologists point to follow-the-leader and symbolic imitation as a mechanism of diffusion, one could interpret this as the result of a biased search for information from successful peers. Alternatively, when sociologists refer to emulation of countries with similar objective characteristics or culturally similar nations, it is clear that the search for informational cues is part of that imitation.

All in all, there is a strong degree of overlap among the different mechanisms of policy diffusion considered in the literature. This makes the specification and the interpretation of empirical tests a fairly arduous exercise: there is a version of the learning channel, that of bounded learning, that overlaps with the constructivist or social emulation approach. The social emulation or constructivist approach also overlaps with the realist approach or power account. Both approaches recognize that international institutions may cause policies to converge through imposition.[9] Realists and constructivists also overlap when it comes to discussing hegemonic ideas. Lastly, the realist approach overlaps with the

economic competition channel if one conceives of the market as a decentralized mechanism that proffers sanctions and hence is coercive.[10]

When these mechanisms are analyzed in the light of the opening question that motivates this paper (namely, what is new in the study of policy diffusion), they score badly. In our view, *realist theories* have a hard time. First, because overt coercion, which is a central element of the realist view, has nothing to do with the standard definition of diffusion.[11] But, even if one agrees that unequal power relations are engines of policy convergence, one is left with the prior question as to where the view of the powerful comes and why these views shift and evolve.

Competitive mechanisms also face a hard time due to their limited scope. Competition may explain why choices in *particular* policy areas converged but, as Simmons, Dobbin and Garrett (2006) acknowledge, a move in the same direction may not be the best response to a change in the policy of a competitor. Competition may cause policies to converge or to diverge, depending on the issue area. Moreover, as Guisinguer (2005) shows in the case of trade liberalization, competition may result in zones of diffusion around different tariff levels, that is, in clusters.[12] Hence, economic competition resulting in policy diffusion is just a particular, not a general, outcome.

We are left with the *informational* or learning mechanism and emulation. There is hardly anything novel in pointing to one or the other as an engine of change, and yet they are potentially the most powerful mechanisms to explain policy change and policy diffusion. This is especially true when these channels are not seen as operating in isolation but as informing each other. Being sketchy, a policy that performs badly will sooner or later be replaced depending on how bounded the learning is and on the availability or otherwise of an

alternative policy that can successfully replace the existing one. Once an alternative policy is in place, its performance will provide information about its plausibility, validating or invalidating it. But a political economy of diffusion would be incomplete without a word about electoral politics. It is striking that the political fortunes of those that followed liberal economic policies and chose liberal political institutions do not enter into the picture as an explanation of their diffusion. However, it is perfectly sensible to hypothesize that policies spread because their proponents fared well at the polls. For instance, in the early 1980s Margaret Thatcher's experiment with privatization is considered to have been a political watershed of mixed economic results. Hence, the informational feedback that characterizes learning is probably not only from policy outcomes to policy choices but also from electoral fates to policy choices. Learning entails that actors look at the experience of others to inform their choices; but it is not specific about what aspects of the experience of others are relevant. It may be the impact of the new policy on economic performance, electoral success, or indeed any other outcome politicians care about. However, this issue is crucial at the empirical level, where researchers are forced to identify the relevant effects of policies that politicians are assumed to look at.

Overall, there is nothing *theoretically* new in invoking coercion, epistemic communities, norms, economic competition, or imitation to explain policy diffusion. There is actually nothing *substantively* new in the last wave of democratization, marketization, and deregulation. True, its scope is unprecedented. But, in our view, focusing on questions of degree may distract attention from more interesting issues. If one agrees on this, there is no reason to study the diffusion of liberal policies and institutions as an isolated and new

phenomenon. In fact, making diffusion equal to its outcome, namely, the scope of the last wave of political and economic liberalization, is misleading.

3. On methods

Little thinking has been devoted to modeling diffusion processes. Most studies assume that each of the diffusion mechanisms depicted above is a sufficient condition for policy adoption. This assumption may be fairly realistic for some mechanisms, such as competition, but it is not plausible for others, such as learning. While competition accounts for both the motivation to change policy (competitive pressures) and the kind of policy selected (that of competitors), learning implicitly assumes that actors are constantly in a process of policy change, and constantly scan the world in the search for alternatives. It would be more plausible, we believe, to assume that learning is not a sufficient cause of policy change and that it becomes relevant as a diffusion mechanism only if, for some other reasons, actors are led to consider policy change in the first place. Thus, whereas current arguments implicitly assume that the effect of diffusion mechanisms is unconditional, the effect of some of them is expected to be conditional on some other factors. It is obvious that this fact should have implications for the specification of the empirical models of diffusion (Volden, this issue, follows this approach).

Studies of the internationalization of liberal policies and institutions provide us with an impressive amount of new data and results on the diffusion of a wide array of policy choices (central bank independence, trade liberalization, BITs, public sector employment, international agreements, tax policy, privatization, financial liberalization, the internet, welfare policies, regulatory policies, and regulatory agencies). Less research has been done on

the diffusion of political institutions (see the work on democratization by Gledistch and Ward, 2006; Elkink, this issue; Elkins, this issue). It also provides us with useful methodological techniques to model policy choices and with insights about how to treat space for modeling purposes. Contributions differ in the number of years and countries covered; but they all share the same logic: testing the significance of the mechanisms of diffusion mentioned above, controlling for the usual political and economic domestic factors. The approach is overwhelmingly quantitative (Weyland 2004, 2005 is an exception). An indicator of policy appears on the left-hand side of the equation and a battery of independent and control variables appear on the right-hand side. Sometimes, diffusion mechanisms are taken into account through variously specified "spatial lags", which give a weighted average of the policies of others, where weights are theoretically meaningful measures of "proximity" (see for instance Simmons and Elkins, 2004; Elkins, Guzman and Simmons, 2006; Swank, 2006; Franzese and Hays, this issue). This is an appropriate approach for modeling relatively simple spatial dependencies, but also more complex network relationships (see for example Polillo and Guillén, 2005).

In this section, we discuss three issues. The first issue is the problematic alignment between "ontology" and "methodology" (Hall, 2003): there are reasons to believe that the empirical models that researchers have employed do not accurately reflect the nature of the causal processes under inquiry. The second issue is the operationalization of the diffusion mechanisms, which is still rudimentary and often does not allow clear interpretations. The final issue is the fact that scholars have mostly studied explosive diffusion processes, which is a kind of selection bias. Overall, we think that, whereas the methodological approach is certainly sophisticated, it may need further development.

(1) Regarding the *alignment between ontology and methodology*, the most serious methodological problem is the difficulty in taking into account "causal complexity", namely, the fact that there may be multiple causal paths leading to the same outcome and that some causes may operate only in combination with other causes (Ragin, 1987: chapter 2; 2000: chapter 4). The problem is that the assumptions of statistical models may contradict basic features of the diffusion processes that are studied; in other words, ontology and methodology may be poorly aligned (Hall, 2003). Of course, this is a general problem of quantitative analysis and is not a specific drawback of the diffusion literature, but this fact should not prevent scholars working on diffusion from trying to build better specifications for their empirical models. Theoretical refinements like the ones advocated in this article will be useful if the increasing complexity of hypotheses can be captured in model specifications, at least in part.

Like most quantitative studies, the analyses carried out in this literature make strong *"homogenizing assumptions."* A first homogenizing assumption is that any given mechanism is equally relevant (or irrelevant) across all cases: it is assumed that all governments are equally keen to engage in learning, are equally reactive to competitive pressures, or are equally sensitive to emulative pressures. But this need not be the case. It may very well be the case that some governments adopt new policies because they learned from the experience of others, while other governments simply emulate, and still others adapt to a competitive context. An example to illustrate this point is Brooks' analysis of the diffusion of pension privatization in the Organization for Economic Development and Co-operation (OECD), Latin American, and eastern European countries (Brooks, 2005). The main diffusion variable is the experience of peer countries, that is, countries in the same geographic region.[13]

Interestingly, the relevance of these peer dynamics turns out to vary across regions: while in Latin America and eastern Europe peer policies affect the choice to privatize pensions, OECD countries seem immune to such influences.[14] According to the author, this means that "advanced industrial nations are less likely to rely on information from peer decisions to discern the viability of [pension privatization] in their country" (Brooks, 2005: 25). Whether this is a satisfactory explanation is not important for this discussion; rather, we emphasize that this finding shows that a particular diffusion mechanism need not have the same effect in all countries or regions. Unfortunately, Brooks does not report estimates for models that do not take this heterogeneity into account, but we suspect that, had peer dynamics been assumed to work homogeneously across all the sample, their significance would have been much weaker. Thus, Brooks' study illustrates the risk of neglecting causal complexity as well as the convenience of taking heterogeneity into account (see also Volden, this issue).

A second homogenizing assumption implicitly made in most diffusion studies is that diffusion mechanisms are equally relevant (or irrelevant) across the whole observation period.[15] This is an implausible assumption. It is entirely possible that some mechanisms are more relevant in early stages of diffusion, while others become more relevant in later stages. Such temporal heterogeneity may explain the poor results of learning, especially in its rational version. If learning is highly relevant in some stages but irrelevant in others, then statistical analyses, which look for average effects, will tend to conclude that learning is mildly significant at best. A simple attempt to model such temporal heterogeneity could be interacting diffusion mechanisms with a dummy variable that divides the observation period into "early" and "late" stages, which would indicate whether (and how) the relevance of

diffusion mechanisms varies in the different stages of the diffusion process. This strategy mirrors that adopted in the organizational sociology literature, which argues that early adoptions are more likely to be influenced by functional considerations, while later ones are expected to follow a more emulative logic (see for example Kraatz and Zajac, 1996). Thus, at a minimum, scholars working on diffusion should try to incorporate some heterogeneity into their models through meaningful interactions. And, ideally, scholars should consider recent advances in the quantitative analysis of causal complexity such as the Boolean probit and logit techniques developed by Braumoeller (2003) that allow the statistical investigation of multiple causal paths.

(2) Concerning the second issue, namely, the *operationalization of diffusion mechanisms*, the task is far from straightforward; and, when it is straightforward, the results of that operationalization are not easy to interpret. For instance, almost all studies of policy diffusion include an independent variable referring to agreements with the International Monetary Fund (IMF) or the amount of aid received from international financial institutions (IFIs), or membership of GATT/WTO. These variables are intended to capture the *coercive* part of the story; but, as mentioned above, alternative interpretations to the realist view are possible, and yet impossible to address with such crude indicators.[16] *Competition* is usually operationalized by including in the equation the policy stance of the network of rivals competing for trade or capital. *Emulation* is typically proxied by the policy choices of neighbors and/or countries with which a particular country shares income or productive structures, culture, religion, law traditions, language, colonial past and the like. Finally, *learning* is exceptionally operationalized in ways that we find convincing. Elkins (this issue) is breaking new ground by using experiments to test what is actually a psychological process

and applying it to the choice of political institutions. In turn, Volden (this issue) innovatively adapts the dyadic approach used in international conflict research to investigate how examples of success or failure influence the diffusion of policies.

Figure 1 below illustrates the complex problem of operationalizing diffusion mechanisms. Adcock and Collier (2001: 531) argue that operationalization consists in "developing, on the basis of a systematized concept, one or more indicators for scoring/classifying cases." Although some consensus exists on basic definitions, there is still confusion regarding *conceptualization,* that is, the step from background concepts to systematized concepts. In fact, some of the confusion arises because scholars link the same systematized concepts to different background concepts. There are also problems at the *operationalization* level, that is, in the step from the systematized concepts to the actual indicators. Most indicators of mechanisms of diffusion are not univocally related to a single systematized concept.

The idea that the experience of others supplies relevant information for policy choices can be considered as a background concept --learning. Ideally, a single systematized concept should be associated with this background concept, and, in turn, a single indicator should be associated with the systematized concept. Unfortunately, the situation is much more confusing. At least six different systematized concepts have been put forward that can be linked to this background concept of learning: cultural reference groups, regional trends, expectation of success, success, demonstration effects, and information externalities. Even more problematic, different indicators are used for the same systematized concepts, and the same indicator is used for different systematized concepts. In this context, what does it mean

to say that the policy choices of a given country are influenced by the policies of countries in the same region? It is hard to say.

Figure 1 here

The results of statistical analyses are therefore difficult to interpret because great confusion exists over the link between indicators and concepts. We believe that these conceptualization and operationalization problems have a lot to do with the high level of conceptual overlap among the mechanisms discussed in section 2. Thus, conceptual clarity seems a necessary first step in order to achieve more coherent operationalizations.

(3) Lastly, we express concern about a third issue: the *selection of policies whose diffusion is researched*. The diffusion literature has been characterized by a strong selection bias in choosing policies to investigate that have spread explosively. A similar concern was raised a few years ago by Strang and Soule (1998). This problem is especially serious for studies of policy innovations such as the internet, independent central banks, and privatization. It is less so for policies with a long history, such as trade and financial policies, where a careful scrutiny does not reveal anything explosive but rather cycles and trends (Simmons and Elkins, 2004; Guisinguer, 2005; Quinn and Toyoda, forthcoming 2007). In any case, there is a tendency for research to concentrate on policies where a good deal of global convergence can be observed. As argued above, convergence is only one of the possible outcomes of diffusion processes. Thus, this selection bias can be easily eliminated by exploring whether some diffusion pattern is also observed in policies that have not converged

-- as is in fact the case. We argue that the theories and methods employed in this literature can be used to study *any* type of diffusion process, whether global or more confined.

4. What is new in this issue?

We have put forward three main points. First, diffusion and convergence are two separate processes. We may observe policy diffusion in clusters of countries, without this implying that policies converge globally. Thus, we can learn about policy diffusion even if we select for research policies whose diffusion has not been global and/or explosive. Second, research on diffusion should relax homogenizing assumptions and take account of the fact that diffusion patterns may vary both across countries and over time. Third, model specifications should address feedbacks among the different mechanisms of diffusion. In particular, it is crucial to advance the conceptualization and empirical treatment of learning in its rational and bounded versions. The contributions we discuss below are suggestive and encouraging attempts to address these challenges. The authors discuss crucial substantive issues and experiment with novel methods in what we believe constitutes path-breaking research in the diffusion of policies and institutions.

In their enlightening article, Thomas Plümper and Christina Schneider provide a thorough criticism of the difficulties of measuring policy convergence using the so-called variance approach – equating convergence with a reduction in the variance of policy positions. Using simulations, the authors persuasively show that research that concentrates on policy convergence measured as such captures only one type of policy convergence: one that is *unconditional*. However, convergence processes are very often conditional on particular variables which cause what the authors describe as convergence clubs and which we referred

to as clusters. For instance, policies may converge to two very different policy equilibria if they diffuse conditional on ideology: leftist countries may cluster around one specific policy stance whereas rightist countries may cluster around another. This kind of conditional convergence would not be captured by a variance approach. Yet these also constitute examples of convergence processes. The discussion of them is linked to the point we raised earlier in this introduction: focusing on convergence processes that look explosive will inform us only of unconditional processes, but researchers may be disregarding conditional ones. As a corollary of this argument, the authors encourage scholars working in this area to improve current theories of diffusion in order to explicitly account for the variables that may cause clusters to emerge. The article also includes suggestions for estimating – rather than measuring – convergence.

Robert Franzese and Jude Hays' contribution perfectly illustrates the claim that not taking into account countries' interdependencies may bias statistical estimations. The authors use spatial lags, which weigh the influence of other countries' policies according to spatially defined characteristics, such as sharing borders or being competitors. Using this technique, the authors replicate Swank and Steimo's (2002) study of the determinants of tax rates on capital in several OECD countries. The replication shows that not taking into account spatial interdependencies leads to an overestimation of the impact of domestic political and institutional variables in the determination of taxes on capital. Moreover, the spatial lags are highly significant, demonstrating that a crucial part of the explanation of the determinants of tax rates had not been accounted for. The authors provide a useful discussion of this technique and explain how to report and interpret the results obtained using this approach. Of particular interest is the possibility of simulating the long-term policy stance of countries, which is ideal

for an informed discussion of the "race-to-the-bottom" debate, in turn central to the literature about the impact of globalization on policies.

Turning to political institutions, Jos Elkink studies the diffusion of democracy using agent-based modeling. Elkink considers a particular diffusion mechanism, namely, herding behavior on others' protests, and finds that this mechanism does explain the observed waves of democracy. The study is innovative in that it identifies and models micro behavior that eventually turns into a macro trend. Thus, it illuminates what most studies do not address, namely, the *micro mechanisms* through which policies and institutions diffuse. While empirical analyses rely on a correlational view of diffusion, Elkink's article focuses on how diffusion process unfold over time, and on how interactions between agents at the micro, individual level (information on democratization movements abroad) have consequences for macro, aggregate patterns of institutional diffusion (the international spread of democracy). This approach is therefore well suited to overcome the simplifying assumptions of statistical analyses, and to explore the complexity of diffusion processes.

Zachary Elkins' article is a novel attempt to explore the relative relevance of two mechanisms of diffusion – group norms and rational learning – when it comes to making policy recommendations on three political issues: the extension of the franchise to legal immigrants, the adoption of parliamentarism, and the change to proportional representation. The study is novel in that it uses experiments to test these alternative mechanisms of diffusion in the context of political institutions. The subjects were divided into four groups: one control group in which individuals were exposed to facts, and three treatment groups. In the first treatment group, individuals were presented information about the United States in relation to a group of advanced countries, as a clear outlier in its institutions. In the second treatment

group, individuals were given information about experts' opinions on the functioning of these institutions. This second treatment was intended to capture rational learning. Finally, the subjects in the third treatment group were provided with information about the functioning of the same institutions at the state level. The results are interesting. First, it seems that presenting the United States as an outlier with respect to a group has a strong impact, in particular when it comes to approving of proportional representation. In turn, rational learning seems to matter when it comes to recommending the enfranchisement of legal migrants. Being in the third treatment group, the one exposed to information about the performance of institutions at the state level, does not affect subjects' proclivity either to support or to oppose the reforms. The most robust result of the study is that "international" subjects are more likely to recommend the passing of the three political reforms.

Finally, moving to the sub-national level, Craig Volden's article on the abandonment of welfare policies (Temporary Assistance for Needy Families) in the American states successfully addresses the important point raised by Plümper and Schneider. Rather than focusing on the diffusion of successful policies, Volden enquires whether unsuccessful policies are abandoned and why. The question is relevant to the debate on learning, which in principle should be both about policies that work and about policies that do not work. However, most studies have concentrated on learning from success. Volden also does an excellent job in exploring conditional convergence processes: his article shows that state governments learn from governments whose ideology they share. The paper also shows that professional legislatures are more likely to abandon unsuccessful policies. This result is important. It entails that, for learning to occur, a minimum of professionalization is needed. Only in that case will the policy process be conducive to better policy choices and outcomes.

5. Conclusion

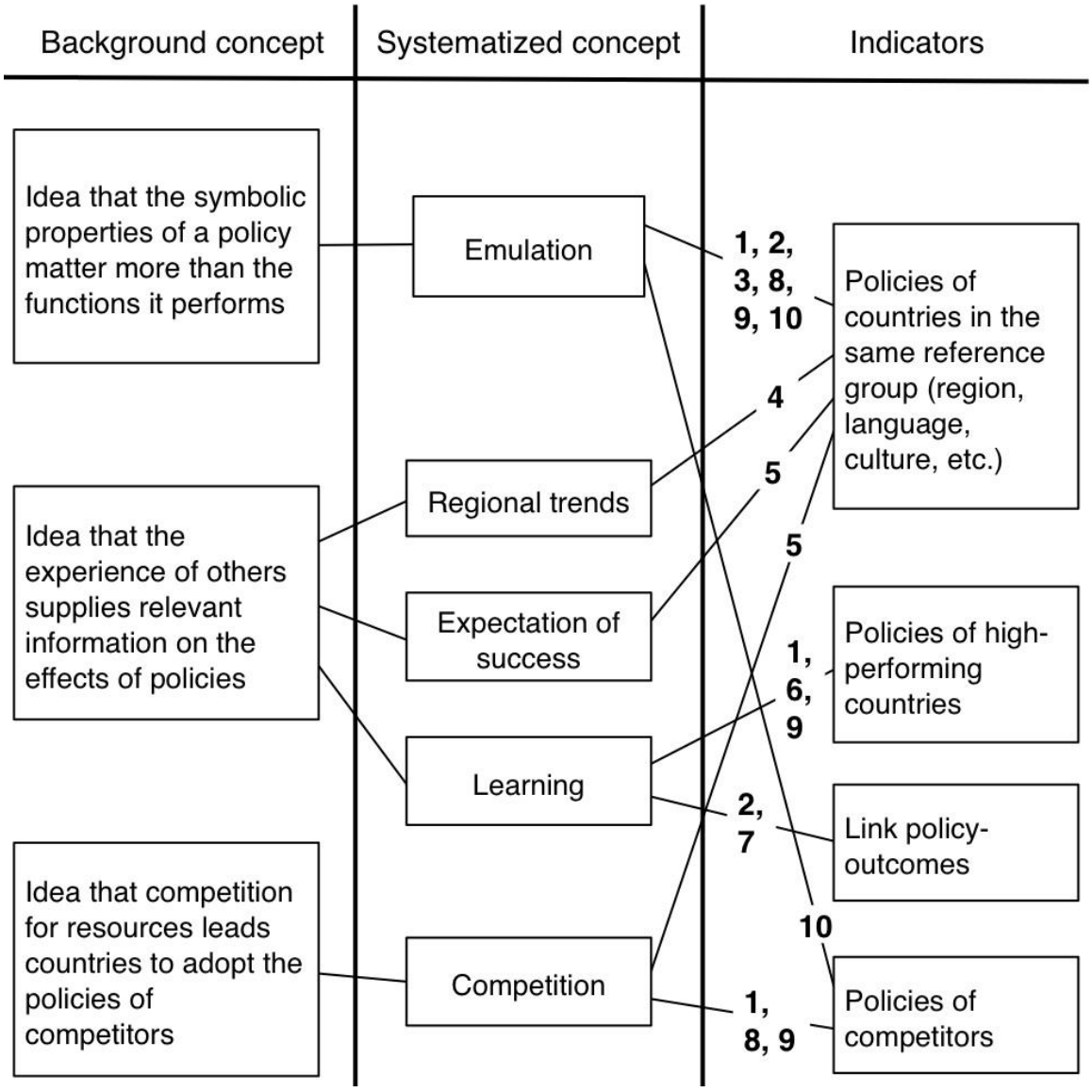
The diffusion of policies and institutions has recently attracted considerable interest from political scientists. While there is a large literature on sub-national policy diffusion (especially in US states), international diffusion has seldom been investigated in a systematic way. This is no longer the case: during the past few years, scholars have convincingly shown that diffusion processes take place also cross-nationally. In this article, we have critically surveyed this recent literature, and we have made three key claims. First, we have argued that, despite the many apparent novelties, there is little really new in these studies: theories, hypotheses and methods are borrowed from well-established research traditions, such as sociological studies of the diffusion of innovations and the literature on policy diffusion in the US states; and the phenomenon itself – diffusion – is of course not new. Second, we have shown that the literature suffers from weak conceptualization and theorization: several diffusion mechanisms are identified (such as competition, learning, and emulation), but their boundaries are blurred, and their operationalization highly problematic. Third, we have argued that more methodological work is also needed. Despite the sophistication of many studies, too little consideration is given to the heterogeneity of diffusion processes: the relevance of the various diffusion mechanisms may vary both across countries and over time. Moreover, feedback among mechanisms is also neglected.

The contributions in this special issue tackle many of these problems, and therefore constitute a major step forward in the research into policy and institutional diffusion. The stakes are high: we need a better understanding of diffusion processes, since the normative implications are very important. Diffusion seems to matter, but is it so strong that it dwarfs national autonomy? If not, how does diffusion interact with domestic politics and institutions?

And what is the main driver of diffusion? Is it learning, in which case countries tend to converge toward the most effective policies? Or is it emulation, which promotes the spread of socially rewarding policies, regardless of their consequences on policy outcomes? But if policymakers learn rather than emulate, what do they learn from? Do they look at the effects of policies on the problems they are intended to solve, or on their re-election chances? And finally, do we need more coordination at the international or even supranational level, or can we stay satisfied with the outcomes of decentralized diffusion processes?

These are fundamental questions that are highly relevant for academics, policymakers, and ordinary citizens alike, but the present state of the literature does not give us clear answers. Although the contributions to this issue are focused on explanatory rather than normative issues, they advance our knowledge of diffusion processes in ways that bring those answers within closer reach.

Figure 1. Problems with the operationalization of diffusion mechanisms



Note: 1) Simmons and Elkins (2004); 2) Elkins, Guzman and Simmons (2006); 3) Gilardi (2005); 4) Way (2005); 5) Brooks (2005), 6) Volden (2006); 7) Meseguer (2004); 8) Swank (2006); 9) Lee and Strang (2006); 10) Polillo & Guillén (2005)

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Endnotes

[1] This is known as bottom-up convergence.

[2] Studies on the diffusion of political institutions are still rare. See Gleditsch and Ward (2006) on the diffusion of democracy, and Woptika and Ramirez (2007) on the diffusion of international treaties.

[3] Theda Skocpol, for example, acknowledges that the social revolutions she studies may not be independent (Skocpol, 1979: 23). She also acknowledges the methodological problems this fact raises, but then retains her assumption of the independence of cases (Skocpol, 1979: 39). She has been criticized for this (Burawoy, 1989: 769).

[4] Diffusion processes have been studied by a number of other literatures, such as those on policy convergence (Bennett, 1991) and policy transfer (Dolowitz and Marsh, 2000). These literatures are decidedly qualitative and have few affinities with the works reviewed here. Given space constraints we do not discuss them, although we recognize their importance.

[5] The distinction among mechanisms of diffusion is not only relevant for analytical purposes. As some authors argue, the fact that policies diffuse because a specific mechanism of diffusion operates may have important welfare consequences. For instance, Weyland (2004) argues that diffusion that is based on simple emulation is likely to result in the adoption of models that are not adequately adapted to the special conditions existing in one country. Thus, blind copying of international policy models may result in suboptimal outcomes at home.

[6] On alternative but substantially similar classifications of mechanisms of diffusion, see Gilardi (2005) and Weyland (2004). The distinction between mechanisms that alter "incentives" from those that alter "information" is from Simmons and Elkins (2004).

[7] In a very recent contribution, Elkins and Simmons (2005:38) agree with our view that interdependence in diffusion is uncoordinated…" [t]hus, the actions and choices of one country affect another, but not through any collaboration, imposition, or otherwise programmed effort on the part of any of the actors."

[8] Barbara Stallings (1992) distinguishes between leverage and linkage. Leverage refers to overt coercion. Linkage has to do with subtler coming to terms on the basis of persuasion. For a discussion of the difficulty of separating imposition from persuasion, see also Brooks (2005). See Finnemore (1996) for a study of international institutions as carriers of values and ideas.

[9] In the sociological jargon, this is called coercive isomorphism.

[10] Arguably, the mechanisms should not all be placed horizontally. Whereas competition seems to be confined to issue areas, the sociological perspective deals with more systemic issues of the sort of world views, world values and the like. We thank Kun Chin Lin for this point.

[11] In the general model that is being tested, Simmons, Dobbin and Garrett (2006) explicitly define diffusion as the influence on policy exerted by the policies of other countries.

[12] Note, however, that this is problematic only to the extent that policy diffusion is made equivalent to policy convergence, which, as we argued in the introduction, it should not be.

[13] The theoretical meaning of this measure is not clearly specified by the author. We discuss operationalization problems later in this section.

[14] Brooks (2005) models this heterogeneity by interacting "peer privatization" with dummies for regions.

[15] Strictly speaking, EHA models do not assume "temporal homogeneity" since the impact of all variables depends on the baseline hazard, which can change over time. Indeed, the capacity to deal with temporal heterogeneity is an advantage of EHA (see for example Strang and Tuma, 1993). Nonetheless, EHA models assume that this heterogeneity is homogeneous, so to speak. The impact of all variables is assumed to vary in the same way over time, while the issue here is precisely that the relative importance of diffusion mechanisms changes over time. The shape of the baseline hazard does not capture this.

[16] It may be due to bad conceptualization and operationalization that these proxies fare poorly empirically. In quite a few studies they are not significant, in others significant but with an unexpected sign, and in yet others significant but not robust to alternative specifications. The possibility that these findings reflect the inability to account for causal complexity cannot be discarded, and indeed should be further investigated.