## Review of International Political Economy

# What is new in the study of policy diffusion?

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Published online: 10 Sep 2009.

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Beth Simmons, Frank Dobbins and Geoffrey Garrett (eds) (2008) *The Global Diffusion of Markets and Democracy*. Cambridge, MA: Cambridge University Press, 384 pp., US$90, £45 (hardcover), ISBN 978-0-521-87889-0, US$32.99, £16.99 (paperback), ISBN 978-0-521-70392-5

Kurt Weyland (2007) *Bounded Rationality and Policy Diffusion: Social Sector Reform in Latin America.* Princeton, NJ: Princeton University Press, 312 pp., US$60, £35 (hardcover), ISBN 978-0-691-12974-7, US$24.95, £14.95 (paperback), ISBN 978-0-691-13471-0

## INTRODUCTION

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 (Knill, 2005; Levi-Faur and Jordana, 2005; Simmons *et al.*, 2008; Weyland, 2007). Renewed interest in the internationalization of policies and in policy diffusion has been paralleled by the debate on the impact of globalization on national economic policies. Two major claims are central to this debate. First, national governments have converged worldwide toward economic policies highly valued in an increasingly integrated international political economy. Second, competition has caused a race to the bottom in trade barriers, capital account regulations, tax rates, and government intervention in the economy in general. Indeed, the degree of variance in the world with respect to capial 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 *et al.*, 2008).

In view of these trends, recent work on policy diffusion claims that existing explanations of policy choices do not pay sufficient attention to the international factors that shape those choices. Comparative political economy has been prolific in the study of domestic responses to international events, and successful at showing how those responses vary depending on local institutions and political and economic conditions. Yet the wave of democratization, economic liberalization, deregulation and re-regulation, together with the sense that policy choices have grown more alike, call into question the specification of explanatory models that take only domestic conditions into account. Research on the internationalization of policies improves the specification of explanatory models to take into account the possibility of horizontal diffusion, that is, the possibility that policy choices in one country affect the policy choices in other countries, so causing policies to converge.

To explore this phenomenon, the general research strategy consists of adding a diffusion component as an independent variable to test the null hypothesis that only domestic socioeconomic and political variables explain a particular policy choice. 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 social reforms are proved to have diffused across space and, in a few cases, very fast. Hence, it appears that previous models of policy choice were missing an important part of the story.[1]

Policy diffusion is not a new topic, though. It dates back to at least 1889 (see Ross and Homer, 1976: 1–2). The fact that observations in political research are most likely not independent is discussed in virtually all methodology texts. Yet, in many comparative studies, the possible existence of diffusion, though briefly acknowledged, is often soon forgotten. Early exceptions are Rogers' (2003) seminal work on the diffusion of innovations and the study by Collier and Messick (1975), who explicitly analyzed the adoption of social security programs in 59 countries as an interdependent policy choice.[2] Thus, if diffusion is not a new social phenomenon, what is new in the study of policy diffusion?

We review two recent books on the diffusion of policies. The collection edited by Simmons *et al.* explores the determinants of convergence in democratization and in a wide range of economic policy choices. The book provides an overwhelming amount of new empirical material demonstrating that economic policy choices are the result of imitating the policies of successful others, of learning from the experience of others, and of strategically adapting to the policy decisions of competitors. All contributions are quantitative analyses that explore policy decisions in a large number of countries over extended periods.

Kurt Weyland's volume offers an interesting complement to Simmons *et al.*'s project in both substance and method. Weyland's book focuses

on the diffusion of social policies (pension reform and health reform) in Latin America. The author is interested in learning as a mechanism of policy diffusion, particularly learning in its bounded version. Using a psychological approach to decision making, his is an in-depth, qualitative study of a few cases that offers interesting information about the politics of policy diffusion and about the welfare consequences of adopting policies following a biased learning process.

The two books under review are important because, through a systematic discussion and testing of the mechanisms by which policies diffuse, they seriously qualify some of the most important claims made in the globalization debate, and conclude that policy convergence has been neither worldwide nor a race to the bottom. The 'Diffusion of Liberalism' project shows that the adoption of liberal economic policies has been confined to a few world regions, with other regions still exhibiting high levels of protection, except for a few isolated countries. Concerning the second claim, the internationalization research agenda shows that policy diffusion does not lead to a unique policy stance. Moreover, the race is not inevitably to the bottom. Competition for scarce resources has certainly been a strong motor of convergence; but competition happens among similar countries (for example, countries similar in risk ratings or in their export destinations) and thus, convergence is toward the policy stance of one's network of competitors, which is not necessarily the policy position of a different network. Thus, there are different races to different bottoms.

Whereas the internationalization debate has indeed added theoretical and empirical rigor to the discussion about how countries influence each other in the international political economy, we in turn revise both books, suggesting possible advances, improvements, and pending tasks for the internationalization research agenda.

## MECHANISMS OF POLICY DIFFUSION AND HOW TO TEST THEM

Simmons *et al.*'s collection is a superb piece of work that takes policy diffusion seriously, providing us with an impressive amount of new data and results on the diffusion of a wide array of policy choices (bilateral investment treaties (BITs), public sector employment, tax policy, privatization, financial liberalization, democratization, and the adoption of international agreements). 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: an indicator of policy (generally dichotomous) appears on the left-hand side of the equation and a battery of independent and control variables appears on the right-hand side.[3]

Simmons *et al.*'s project shows that competition has been central to the diffusion of BITs (Elkins *et al.*, 2008) and to the diffusion of tax policies (Swank, 2008); emulation, demonstration effects, and prevailing discourses were behind the adoption of democracy, the protection of human rights, and the downsizing of the public sector (Gleditsch and Ward, 2008; Lee and Strang, 2008; Wotipka and Ramírez, 2008); waves of pro- and anti-capitalist sentiments and their electoral consequences were linked to the diffusion of financial liberalization (Quinn and Toyoda, 2008); and the concentration of authority in change teams with shared economic views significantly drove the diffusion of privatization (Kogut and McPherson, 2008). In general, the book amounts to an overwhelming rejection of the null hypothesis of independent policy choices by independent units. In other words, all policy and institutional choices explored in the book are proved to have been the result of a diffusion process caused by one or several diffusion mechanisms.

The opening essay by the editors is a discussion about the mechanisms by which policy choices in one country may influence policy choices in others. The authors distinguish between: (1) policy convergence 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 (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. It is the search for legitimacy and status that motivates emulation. *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. Learning comes in two versions: bounded and rational. Both types of learning imply that the acquisition of information is costly. Yet, a bounded learner faces extra costs in the interpretation of information and uses several heuristics to analyze available experience. In so doing, bounded learners incur in different cognitive biases, such as over-representing the success of a close policy experiment (Meseguer, 2005; Weyland, 2007).[4]

One important theoretical point with major empirical consequences is that there is a strong degree of overlap among the different mechanisms of policy diffusion considered in the literature. One version of the learning channel – that of bounded learning – overlaps with the social emulation approach: bounded learners are attracted to the experiences of prestigious

countries much as emulators are, although for different reasons. It is the symbolic rather than the informative value of a particular experience that attracts the attention of emulators (Finnemore, 1996). The social emulation approach overlaps with the coercive account: both approaches recognize that international institutions may cause policies to converge in one case through persuasion and in the other through imposition.[5] 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.[6] This conceptual overlapping makes the specification and the interpretation of empirical tests a fairly arduous exercise.

In testing diffusion mechanisms, three empirical issues are particularly problematic. The first is the extended use of strong homogenizing assumptions: there are reasons to believe that the empirical models that researchers employ do not accurately reflect the complex nature of the causal processes under inquiry.[7] The second issue is the fact that scholars have mostly studied explosive diffusion processes, which is a kind of selection bias. The third issue is that the current discussion about mechanisms of diffusion has not been systematic in exploring the role of politics, both as a motivation for adopting the policies of others and as an intervening variable in the way economic policies diffuse. We discuss these issues in turn and use three of the chapters of the book under review to illustrate our points.

(1) Most of the analyses carried out in the book, and indeed in the literature on policy diffusion, make strong *homogenizing assumptions.* For instance, an 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. In practice, more than one mechanism of policy diffusion is likely to operate. Besides, the conceptualization of the mechanisms depicted previously 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 feedback among the proposed mechanisms of diffusion. All the contributions in the book (except for Swank's) assume that each of the diffusion mechanisms 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 policies (competitive pressures) and the kind of policy selected (that of competitors), learning implicitly assumes that actors constantly scan the world in the search for alternatives.[8] An immediate consequence of the lack of a theory of diffusion is that the specification of empirical tests is driven by the attempt to include as many diffusion mechanisms as the data allow in a simple, additive fashion.[9]

In the volume under review, Duane Swank's chapter is an excellent example of how to tackle the complexity we have just mentioned. The author focuses on the diffusion of 'market-conforming' tax policies, namely, cuts in tax rates on capital in 16 Organization for Economic Cooperation and Development (OECD) countries between 1981 and 1998. The main argument is about competition: neoliberal tax policies have spread largely as a consequence of the 1986 US reform, which prompted similar responses in other advanced countries in order to avoid perceived negative consequences in terms of economic competitiveness. Competitive pressures have followed mainly from policy change in the US, and not from international networks of trade and foreign direct investment. Furthermore, policy learning and social emulation do not seem to have played a significant role in this process.

These findings are important in themselves, but Swank's broader conclusion is that diffusion mechanisms (in this case, competition) need not affect all countries in a similar way. Specifically, the competitive pressures stemming from the US reform have had stronger impacts in countries with well-established right-wing governments as well as in liberal (as opposed to coordinated) market economies. Just as learning is enhanced if it conforms to ideological predispositions, competitive pressures are more strongly felt by governments which are ideologically predisposed to perceive them, and which would arguably favor the reforms even without those pressures. The second result also highlights the heterogeneity that is likely to characterize many diffusion processes. While neoliberal tax reforms fit well into liberal political economies, they disrupt the coherence of coordinated political economies, which are therefore less responsive to the competitive pressures stemming from policy change in other countries. Again, the same diffusion mechanism has different consequences in different countries. This point is highly relevant for empirical research, and scholars should certainly be encouraged to explore the multiple ways in which diffusion can play out in different contexts.

(2) The diffusion literature is marked by a strong selection bias in choosing policies to investigate that have spread explosively.[10] 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; Guisinger, 2005; Quinn and Toyoda, 2008). This selection

bias can easily be eliminated by exploring whether some diffusion pattern is also observed in policies that have not converged globally – as is in fact the case. Arguably, the theories and methods employed in this literature can be used to study *any* type of diffusion process, whether global or more confined.

The contribution by Chang Kil Lee and David Strang is a good illustration of how it is possible to study diffusion without incurring a selection bias. The authors show data on public sector employment in 26 OECD countries for the period 1980–97. Although the chapter does not offer a historical perspective of the evolution of public employment and has a small geographic coverage, the work is interesting because this is a case of diffusion that has not been explosive. According to the authors, there is not an aggregate reversal when it comes to the size of the public sector measured by public employment. Yet, they point out that the rates of growth of public employment decreased steadily in the sample, turning negative in 1994 for the first time.[11] This is the phenomenon they seek to explain. Their main hypothesis is that this slowdown had to do with the dominance of the managerialist discourse embedded in the broader neoliberal paradigm that prevailed in the 1990s. They prove it by taking a detour into testing for learning. What the authors do is to relate the change in public employment to various economic results of those that downsize and upsize the public sector. The authors find that, while choices were responsive to the growth results of those countries that reduced public employment, they ignored the favorable results for those countries that increased their public sectors. For Lee and Strang, this asymmetry suggests that rational learning does not enter into the choice of policy. Interestingly, they consider not only the policy that is supposed to have diffused (public sector downsizing) but also the opposite one (public sector upsizing), hence reducing the selection bias mentioned previously and portraying policy choice as a choice between alternative policies.[12] A natural and desirable extension of Lee and Strang's work would be to relate policy choices with the political fortunes of those governments that chose one or the other option, thus adding learning from political results to learning from economic results.

(3) Finally, of particular interest is to explore how domestic politics condition policy diffusion. Variables such us the ideological preferences of politicians, the political constraints they face in adopting or implementing policies, and their expected popularity are likely to affect the speed with which policies are adopted and spread. Thus, even if a particular policy is showing good results elsewhere, it may not spread if it is found to be ideologically alien, electorally risky and/or unlikely to be passed (Braun and Gilardi, 2006).

Also, conspicuously absent from the theoretical discussion and the empirical tests is the recognition that political actors may learn not only from the economic results of policies but also from their electoral consequences.

For instance, it is generally considered that the allure of privatization to other governments had more to do with the political success it brought to Margaret Thatcher than with the economic outcomes of privatization (Meseguer, 2004). Indeed, as we have mentioned previously, a theoretical discussion of the politics of policy diffusion is a major gap in the most recent contributions. By this we mean that politics should be brought into the picture not only as an intervening variable to understand how diffusion takes place. Indeed, political results may be a major motivation to learn from and/or to imitate the policies of others.

The chapter by Dennis Quinn and Anna Maria Toyoda on financial liberalization is a very creative way of bringing politics into the diffusion debate. Their chapter covers the change in capital account regulation of 82 countries during the period 1955–99. One positive thing about this chapter is that it does not confine itself to the most recent period of liberalization but in fact acknowledges the existence of previous periods of openness and closure. As the authors describe, the late-1940s to 1960 was a period of liberalization. The 1960s to the mid-1970s was characterized by closure and a retreat from international financial openness. The early-1970s to the 1990s was a period of stasis: global means and medians changed little during that time. The 1990s were a period of liberalization. Thus, the chapter takes into account the existence of several diffusions. Apart from putting this last wave of financial liberalization in historical perspective, we find their main argument particularly appealing in that it takes politics as a central explanation of policy diffusion. They show that financial openness is negatively correlated with the global fate of communist parties (CPs) in those countries without home CPs and with the fate of home CPs in those countries in which CPs have been able to compete in elections throughout the period. The authors take the electoral support for CPs to be a proxy for anti-capitalist sentiments and hence, they argue, it is the evolution of those sentiments that causes the cycles of more or less liberalization. None of the other mechanisms of diffusion they entertained (imposition and competition) yielded predicted power, or they did so in unexpected ways.[13] Hence, according to the authors, financial policy was responsive to local and global anti-capitalism as reflected in the polls. There is of course the question as to why these cycles of pro- or anti-capitalism occur, which they do not survey; but at least one element of the feedback we urge – the one from political results to policy diffusion – is explored by the authors.

Despite the points we have just raised, the volume edited by Simmons *et al.* is a path-breaking book in the study of policy diffusion. It will undoubtedly be taken as point of departure and as a necessary reference in any future research about the international diffusion of policies and institutions.

## BOUNDED LEARNING AND THE DIFFUSION OF POLICY INNOVATIONS

Kurt Weyland explains the diffusion of *policy innovations* using a psychological approach that emphasizes bounded learning. His examples are the diffusion of the radical Chilean model of pension privatization and the moderate spread of health reforms in Latin America in the 1990s. The diffusion of health reforms was less impressive given the non-existence of a clear-cut model. Thus, generic principles rather than a concrete model diffused.

According to the author, the diffusion of policy innovations is characterized by: (1) an 'S' shape in time, that is, a pioneer takes the lead and many other countries jump rapidly on the bandwagon until this trend eventually tapers off; (2) geographical clustering; and (3) commonality amid diversity, that is, the same policy framework is adopted in varied national settings (see chapters 1 and 2). Weyland contends that these features of the diffusion of innovations are better explained in terms of particular cognitive heuristics that policymakers use to process the information. First, the diffusion of policy innovations evolves in an 'S' shape because policymakers overemphasize initial success. Based on a minimal track record, policymakers jumped to the conclusion that the Chilean pension model had been a success with much promise elsewhere. This is due to the *representativeness heuristic*. Second, the diffusion of innovations shows a geographic pattern because policy diffusion seems to require a close and successful example. This is the *availability heuristic*. And third, the same policy innovation is adopted in countries with very different functional needs due to the *heuristic of anchoring*. This heuristic limits the adaptation of the policy innovation to the particular context, producing the fundamental feature of diffusion, namely, commonality in diversity.

For Weyland, this mode of proceeding makes it evident that policymakers are far from being rational: rather than carefully evaluating the results of the Chilean pension privatization after enough evidence was available, politicians rapidly drew (wrong) conclusions. Rather than scanning all available worldwide evidence about pension privatization, policymakers turned to the experience that was close and relevant to them. Rather than adopting the pension privatization model that could have fit best the characteristics of their national economies, politicians copied the Chilean model *in toto*, making adjustments at the margin. Thus, the author concludes, learning from the Chilean experience proceeded in a clearly non-rational fashion. The author also holds that the fact that governments implementing pension reforms were seeking solutions to real problems rules out 'symbolic emulation' as an explanation for social reform. Finally, Weyland extensively documents that reformers were not responding to outside pressures of International Financial Institutions (IFIs). IFIs

disseminated models and principles and made other experiences available to policymakers. Yet, they did not dictate the content of the reforms.

We find strong and weak points in Weyland's work. The strong points flow from the author's incursion in the realm of social policies. Unlike more technical policy fields, social policies are bound to be both technically and politically controversial. Thus, if we are to gain an understanding of the politics of diffusion, focusing on social policies seems an appropriate way to go. In this respect, the conclusion that Weyland reaches is that learning is more bounded where the technical capabilities of the agencies involved, and polities in general, are less sophisticated. In Costa Rica and Brazil, where the social sector bureaucracies had a high level of expertise, the Chilean model went through amendments that made it more suitable to the local context. But in countries such as Bolivia or El Salvador, where expertise was lacking and the discussion process was led by generalists in the finance ministries, adaptations were less frequent.[14] This is connected to the second strong point in Weyland's work: the exploration of the welfare consequences of policy diffusion. When policies do not undergo the necessary adaptations, they can lead to suboptimal outcomes. In fact, bounded learning in El Salvador and Bolivia led to the reproduction of models that were ill-designed given the characteristics of these countries.

On the weak side, Weyland's work is not a convincing rebuttal of rational learning, nor is it a convincing defense of bounded learning beyond the policy domain that the author studies (pension and health privatization) and his geographic focus (Latin America). In particular, the exploration of a policy that spread explosively (the Chilean pension privatization model) suffers from the selection problem mentioned previously. But, would the psychological approach be useful in explaining the diffusion of innovations globally and not regionally? Why do some policy innovations transcend the regional setting and diffuse worldwide? Moreover, is the psychological approach useful in explaining the diffusion of policies that are not policy innovations? Consider, for instance, some of the policies included in the 'Diffusion of Liberalism' project, such as financial openness and the size of the public sector. Changes in these policies toward more liberal stances do not constitute policy innovations. What does the cognitive approach contribute to explaining changes in these (non-innovative) policies? Are they also driven by bounded learning?

Also, Weyland's defense of bounded learning poses some doubts. For instance, the author contends that the reason why the pace of the adoption of the policy innovations eventually tapers off is that the initial upsurge in enthusiasm is followed by a sober evaluation 'as more evidence about the reform's costs and benefits becomes available' (p. 50). What we read here is that policymakers may be temporarily bounded, but eventually they carefully evaluate the results of policies much as rational learners would. In the same vein, the author minimizes the relevance of the subsequent

adaptations that several countries incorporated in their reforms in response to other policy experiences (interestingly, Latvia and Sweden) and to solve the shortcomings of the Chilean model as more came to be known about its performance. Again, what we read here are not irrelevant adjustments but deviations that demonstrate that, after a while, policymakers look at other experiences beyond their geographic area of 'availability', and closely scrutinize the results of policies. All in all, bounded learning may be a good tool for explaining the regional diffusion of policy innovations, but it has not been demonstrated that this approach can explain the global convergence of policies that are not strictly new. Policy stances are eventually adjusted in the light of the evidence. Thus, bounded learning may be an adequate characterization of short-term behavior. Finally, the idea of exploring the significant variation among regions when it comes to adopting policies is certainly attractive. As the editors of the 'Diffusion of Liberalism' project show in the introductory chapter, sub-Saharan Africa, the Middle East and south Asia have consistently lagged behind Latin America and east Asia on the road to liberalism. Indeed, some regions have made little progress. Thus, the most interesting question may not be why a successful policy innovation that originated in Latin America spread in Latin America, but why a successful policy innovation that originated in Africa has not spread in Africa. The point is that much is to be gained in our understanding of diffusion from exploiting not only intra-regional comparisons but also inter-regional ones.

In summary, Kurt Weyland's work is an enlightening study. Carefully documented research on policy diffusion of the type Weyland carries out is, without doubt, essential for scholars to make progress in improving our understanding of policy diffusion.

## CONCLUSIONS

Recent research on the internationalization of policies has explored why countries rapidly adopted liberal politics and market policies. The novel hypothesis tested is that convergence in choices might have been neither the outcome of an independent discovery of best practice nor the outcome of unilateral imposition. Rather, the hypothesis entertained in these works is that policies spread horizontally, that is, that policy choices in one country influenced policy choices in other countries, resulting in the adoption of the same policy. How and why would this happen?

A government may adopt the same policy stance as a competitor to prevent the loss from not following suit. Alternatively, the logic of convergence may be cooperative in the sense that mutual benefits may result from shared policies. Also, a government confronted with uncertainty about what to do may learn from the policy choices of others or may simply imitate what peers perceived as successful do, without further reasoning

about the causes of success. In any case, demonstration effects are likely to be not only about economic results but also about political outcomes.

From a substantive point of view, there is little innovation in the internationalization research agenda: especially in the sociological literature, diffusion has been well explored. At the theoretical level, the mechanisms put forward to explain the channels through which policies may diffuse are well known to the discipline, and some of them are misplaced in the discussion. Moreover, although it is acknowledged that there is a high degree of overlap among mechanisms little emphasis is given to feedback among them and to the interaction between diffusion mechanisms and domestic conditions.

From an empirical point of view, there are some methodological innovations and an impressive amount of new data and results. Without underestimating the value of having new empirical evidence about old mechanisms, we think there is room for potential improvements in the empirical tests. First, contributions have focused on policies that diffused rapidly, even though a second look revealed that, for some of them, diffusion needed to be qualified. Second, most empirical tests have been confined in time to the latest wave of marketization and democratization, overlooking for the most part the fact that periods of diffusion existed before. Hence, one is left with the question as to whether the mechanisms and results obtained would be applicable to other 'diffusions'. Third, more effort should be devoted to integrating heterogeneity into our models.

On the positive side, however, the results of the empirical tests are an invitation to think about some issues: for instance, why some policies spread faster than others or why policies cluster rather than converge globally. It is surprising to find that countries influence each other even when policies do not converge and that clusters of policies may appear conditional on some factor – geographic proximity, competition, institutional affiliations, shared ideologies etc. Expanding the empirical research to new policy domains and/or regions that seem resistant to the latest wave of liberalization may throw some light respectively on the factors that cause policy clusters and on the robustness of the results found so far.

We also think that the literature on diffusion should make a crucial move from invoking mechanisms broadly related to schools of thought in international relations or public policy to state much more refined and unified hypotheses about what type of mechanisms are expected to be relevant and when. Letting the data speak has been useful to unfold quite unexpected patterns; but this strategy may not be the best way to proceed from this point on. Without being exhaustive, we propose that further research should establish theoretically informed hypotheses on issues such as: (1) why some policies diffuse faster than others; (2) why regional patterns of policy diffusion vary so much; (3) why partisan politics retains predictive power to explain some policy adoptions but not others; (4) what

mechanisms of diffusion are likely to be influential in early as opposed to late phases of policy diffusion; and (5) how patterns of policy diffusion are affected by political variables such as the proximity of elections, expectations about incumbents' survival, the distributive impact of the policy under consideration, and the impact of particular institutional settings on the diffusion of policies (see the discussion in Swank, 2008).

In our opinion, the most interesting avenue of research will be one capable of explaining policy change and any policy diffusion. Some of the mechanisms proposed in the literature can explain why the adoption of some policies accelerates over time, but cannot explain which policies diffuse in the first place. It is one thing to argue that some policies diffuse because at some point they are taken for granted; it is another to provide an explanation as to how and why a policy comes to be taken for granted. We posit that learning (whether rational or bounded) from economic outcomes and/or electoral outcomes and the impact of that learning on how alternative policies are perceived should provide a good prediction of which policies diffuse.

To conclude, research on the internationalization of liberal policies and institutions poses many new questions. While answering them depends crucially on better theory and better methods, the endeavor is worthwhile as it may provide the true political economy of diffusion that is yet to be developed.

## ACKNOWLEDGEMENTS

## NOTES

1 Studies on the diffusion of political institutions are still rare. See Gleditsch and Ward (2008) on the diffusion of democracy, and Wotipka and Ramirez (2008) on the diffusion of international treaties.

2 A well-established literature concentrates on the diffusion of policies in the US states (for example, see Berry and Berry (1990, 1999), Berry *et al.* (2003), Mintrom (1997), Mintrom and Vergari (1998), Shipan and Volden (2006), Volden (2002, 2006)).

3 Event history analysis (EHA) has been the standard (although not unique) tool to obtain estimates of the probability of adopting a particular policy (Berry and

Berry, 1990, 1999; Strang and Tuma, 1993; Strang and Soule, 1998; Hedström, 1994; Hedström *et al.*, 2000). More and more frequently, a spatial dimension is added, which takes diffusion mechanisms into account through variously specified 'spatial lags' (see, for instance, Simmons and Elkins (2004), Elkins *et al.* (2008) and Swank (2008)). This is an appropriate approach for modeling relatively simple spatial dependencies, which can also account for more complex network dependencies (see, for example, Polillo and Guillén (2005)).

4  Note that in the constructivist literature, there is an added problem to information acquisition, which is information interpretation or not knowing how to know (Blyth, 1997). The distinction between 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 (2007) 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.

5  In the sociological jargon, this is called coercive isomorphism.

6  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.

7  To be sure, our own work on diffusion does not escape these criticisms.

8  For alternative but substantially similar classifications of mechanisms of diffusion, see Gilardi (2005) and Weyland (2007). The distinction between mechanisms that alter 'incentives' and those that alter 'information' can be found in Simmons and Elkins (2004).

9  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), which allow the statistical investigation of multiple causal paths.

10  A similar concern was raised a few years ago by Strang and Soule (1998).

11  Nonetheless, the authors acknowledge important variation in choices. For instance, some developing states with small public sectors like Greece and Portugal, and others like Norway and Austria with already large public sectors, increased public employment (p. 3).

12  It is has been quite frequent to model learning only from success (Simmons and Elkins, 2004). Yet, it seems obvious that policy-making is usually a choice among alternative policies and that policymakers learn both from failure and from success.

13  An important finding of this chapter is that the capital account policies of leading economies in no way compelled emerging markets to liberalize.

14  This point that poor polities imitate more than learn is also raised, for instance, in Sharman's (2006) work on tax havens.

## NOTES ON CONTRIBUTORS

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