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IMMIGRATION POLITICS

Cleavage Politics and the Populist Right: The New Cultural Conflict in Western Europe. By Simon Bornschier. Philadelphia: Temple University Press, 2010. 245p. $65.40.

Immigration and Conflict in Europe. By Rafaela M. Dancygier. New York: Cambridge University Press, 2010. 345p. $90.00 cloth, $28.99 paper.
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— Cas Mudde, *DePauw University*

In Western Europe, immigration and political conflict are mostly tied to the rise of populist radical right parties. Today few scholars argue that these are single-issue parties, even though the still-popular term "anti-immigration parties" seems to imply this. Rather, they believe that immigration and multiculturalism have given rise to a new political divide, at the expense of the traditional cleavages. This is also the main thesis of Simon Bornschier's *Cleavage Politics and the Populist Right,* which draws upon the theories of prominent scholars in the field (e.g., Piero Ignazi and Herbert Kitschelt), and the data of the cross-national research project directed by Hanspeter Kriesi, "National Political Change in a Globalizing World." Bornschier provides an empirical underpinning of the mainstream understanding that nativist politics is one of the two poles of a new political cleavage.

The introduction discusses the transformation of West European party systems since the 1970s and argues that the "New Left" and the "New Right" constitute the two extremes of a new "cleavage," where Green parties defend "libertarian-universalistic values" and right-wing populist parties "traditionalist-communitarian values" (pp. 1–5). For Bornschier, the latter represents a "counteroffensive" to the former (p. 17). In line with the famous work of Seymour Martin Lipset and Stein Rokkan, a cleavage is defined as "a durable pattern of political behavior of structurally defined groups" (p. 5). Bornschier argues that the continued salience of a specific cleavage depends on it "being kept alive by contemporary political conflicts" (p. 5), that is, by the party system. Only when the traditional cleavages loose salience, because of reduced conflict, can new cleavages arise. Hence, he argues that "the rise of the populist right is a consequence of the growing salience of the new cultural dimension of conflict at the expense of the economic state–market cleavage" (p. 5). Taking it even further, he states "that economic preferences play no role in the mobilization of the populist right" (p. 25).

The first part of the book ("Putting Right-Wing Populist Parties in Context") provides a more elaborate discussion of the larger theoretical framework (Chapter 1) and of the existence of "the extreme right-wing populist party family" (p. 32). The specific party family, which unfortunately is referred to by various names throughout the book, is defined upon the basis of three criteria: a location at the extreme on the ideological axis ranging from libertarian-universalistic to traditionalist-communitarian positions, a populist antiestablishment discourse, and a hierarchical internal structure (p. 35). The empirical test focuses only on the first criterion, however.

The second part of the book ("New Political Divides and Historical Cleavages") presents the specific theoretical model and discusses the data and method. The aim of Chapter 3 is "to develop a model to assess how established conflicts limit the room for parties that are mobilizing on new issue dimensions" (p. 53). First, he specifies the revolution underlying the new cultural cleavage as "the educational revolution of the 1960s and 1970s" (p. 65). He then constructs a fairly complicated typology with eight boxes but only six types (Figure 3.2, p. 67), based on cleavage *salience,* operationalized as polarization; cleavage *closure,* that is, "the organized loyalties of social groups"; and the *match* between issue positions of parties and their social constituencies. Right-wing populist parties will emerge primarily in party systems with unstable alignments and a mismatch between parties and social constituencies. The next chapter discusses the research design and methods. While voter positions are taken from postelection surveys, party positions are taken from the Kriesi project, which means that they are based on a content analysis of the media (one high-quality newspaper and one tabloid per country). The theory is then tested on four elections—one from the 1970s, before the emergence of the new cleavage, and three recent ones—in three countries (France, Switzerland, and Germany, Chapters 5, 6, and 7, respectively). The short conclusion argues that all three cases support the theory and that European integration reinforces the salience of the cultural cleavage (in the two countries where it is salient, that is, France and Switzerland).

The strength of Bornschier's book is not its theoretical development but its empirical operationalization. It provides the first substantial empirical test of the "new cleavage theory," which has been prominent in the literature since the early 1990s. That said, there are some important problems with both the theory and the test. The theory leaves virtually no space for the "internal supply side," that is, the right-wing populist parties themselves. Yet the absence of a successful German right-wing populist party seems much less explained by the actions of the Christian Democratic Union, whose position is not much different from other mainstream right-wing parties in Europe, than by the (perceived) position of the German radical right, which is much more "right-wing" than that of its French and Swiss brethren. It also fails to explain why the Swiss People's Party (SVP) decided to move to the (radical) right, while, for example, the Dutch People's Party for Freedom and Democracy (VVD) did not (or much later, after the breakthrough of a radical right party). Empirically, the

test is not just limited in space (i.e., to three countries), which might be understandable given data and time considerations, but also in time; the last elections are 1999 (Switzerland) and 2002 (France and Germany). Also, party issue salience is measured by how often *newspapers* mention these issues for a particular party during a campaign! Finally, as acknowledged by the author (p. 79), the empirical fit between the party's positions and voters' positions is problematic.

In the book *Immigration and Conflict in Europe*, Rafaela M. Dancygier addresses immigration and conflict from a different perspective, going beyond mere nativist responses or the electoral successes of radical right parties. By taking an economy-centered approach, rather than the currently more popular identity-based approaches, she aims to explain "why, where, and when immigration leads to conflict in the areas of immigrant settlement" (p. 3). Her parsimonious explanation is based on the interaction of two variables: economic scarcity and immigrant electoral power, which are significantly affected by two other variables, immigration regimes and national political institutions. The theory is presented in the first (theoretical) part of the book, most detailed in Chapter 2.

In the second part of the book, Dancygier tests the key tenets of her theory on the case of postwar Great Britain. In Chapter 3, she takes up the puzzling fact, noted but not explained by academics and officials alike, that "[w]hereas immigrant–native violence has tended to occur between South Asians and whites but much less so between black Caribbeans and whites, immigrant–state violence has been associated with minorities originating from the Caribbean islands but to a much lesser degree with South Asians" (p. 62). Using a wealth of qualitative and quantitative data, she explains the difference in terms of the political influence of the South Asians, arguing that "competition over scarce resources lies at the heart of immigrant–native conflict" (p. 98). In Chapter 4, she looks at the dynamics of racist violence in the Greater London area: Substantiating her quantitative analysis with secondary ethnographic accounts, Dancygier shows that electoral success of radical-right parties and levels of racist crimes are positively correlated at the local level (a topic of much debate in the literature), and that "neighborhood defense" (i.e., nativist backlash) appears only when a politically powerful ethnic minority moves into the neighborhood. Both processes are intensified by economic scarcity at the local level.

In the third part, the author delves more deeply into the mechanisms of her theory, addressing issues like residential concentration, as well as causality and timing, by employing well-chosen comparative case studies. In Chapter 5, she compares two London boroughs, Tower Hamlets and Ealing, to demonstrate "that differences in economic scarcity explain variation in immigrant–native conflict in areas where immigrant groups are politically

powerful" (p. 176). The next chapter compares the Midlands cities of Birmingham and Leicester to show that "the electoral position of immigrant communities crucially shapes local parties' responses to them and, in turn, the potential for each type of immigrant conflict" (p. 215). All case studies are meticulously researched, drawing upon originally constructed data sets and a wealth of secondary ethnographic studies.

In the fourth and final part of the book, Dancygier moves beyond Great Britain to look at how economic and political factors affect the two key variables of her theory, economic scarcity and immigrant political power. Chapter 7 discusses the immigration regime of Germany and compares it (partly implicitly) with that of Great Britain, arguing that Germany's highly centralized and effective guest-worker regime alleviated economic scarcity and, thus, immigrant conflict. Finally, Chapter 8 includes two parts; in the first, the case of France is used to show the ways in which electoral rules undermine immigrant political power and thereby shape the immigrant conflict, while the second takes a bird's-eye view at immigration and conflict across Western Europe, arguing that most cases substantiate the key theoretical claims of the book.

In the short conclusion, Dancygier responds to the culturalist domination in the field, arguing that her main theoretical assertion is that "[r]esource scarcity—not ethnic difference—is the key driver of immigrant conflict" (p. 292). At the same time, "immigrants' capacity to actually obtain scarce resources determines which *type* of conflict will ensue" (p. 293); politically powerful immigrant groups will create a nativist backlash, and thus immigrant–native conflicts, while politically marginal(ized) immigrant groups will focus on the state, that is, immigrant–state conflicts. The conclusion ends with some highly original (and undoubtedly controversial) policy implications, most notably that immigrant–native conflict should be seen as "a sign of immigrant *integration*" and that native hostility is "the rejection of possible *outcomes* of immigration and ethnic diversity, rather than hostility against population change and diversity per se" (p. 295) The author also suggest that centralization of integration policies and privatization of local goods (e.g., housing) can steer immigrant conflict towards the state rather than the native.

Dancygier has written a highly original book that deserves readership well outside of the community of scholars of European immigration and radical right. It is essential reading for all scholars of European politics, as well as for European policymakers and politicians. It is the first book to convincingly prove the importance of economic scarcity and, more importantly, immigrant political power for ethnic conflict—factors that have been discussed for decades but seldom with any empirical basis. Moreover, the empirical study of the British case is a textbook example of theoretically informed and methodologically conscious mixed-methods research. In particular, the selection

of cases, so often a weak point in mixed-methods studies, is exemplary. Finally, the study is one of the few to seriously take into account the meso level (without explicitly referring to the term), theoretically a much more convincing level of (economic, political, and social) context that influences individual behavior.

There are, obviously, some critical comments to be made, too. First, while the theory is parsimonious, it is also highly rational. It assumes that both immigrants and natives are fully aware of who distributes how many of which resources to whom. However, surveys show that most people highly exaggerate the influence and number of immigrants in their neighborhood and country. Second, it leaves very little space for individual actors, which is particularly problematic at the local level. For example, local nativist activities are often linked to small groups of individuals; this is primarily the case with violent activities, but in the case of badly organized nativist parties (like in Great Britain), this is also the case with party activity. The British analysis assumes that radical right parties can contest elections everywhere, but parties like the British National Party and National Front are notoriously disorganized and often depend on the availability and will of one or two local people to contest local elections. At the same time, these parties have also been known to target areas with high immigrant conflict, problematizing the correlation between nativist violence and electoral success. Third, the book ignores immigrant–immigrant conflict, despite its regular occurrence (particularly in Great Britain).

Finally, the book is less convincing beyond the excellent case study of Great Britain. The German chapter, for example, overstates the similarities with Great Britain in terms of its immigration history. Most importantly, former colonials and guest workers had not only different political rights but probably also very different expectations about their role in their new country. As one can see throughout Europe today, second- and particularly third-generation "immigrants" challenge the passivity of the first-generation leadership of ethnic groups, arguing that they still operate from the perspective of a guest, whereas the next generations rightly employ a citizen perspective. In other words, the increased immigrant conflict in Germany since 1989 might be less a consequence of a different group of immigrants emerging (i.e., Aussiedler and refugees, who both have legal rights to local goods), and more of the fact that the second and third generations are politically more vocal (which would still fit the theory). Also, the fact that refugees are among the key victims of nativist attacks is probably not a consequence of their economic entitlements, but rather of the fact that most nativist violence took place in East Germany, which had some refugees and virtually no (former) guest workers.

These comments are not meant to take anything away from the importance of this great book. If anything, they are an encouragement for Dancygier and others to continue this type of research, apply the theory and methods to other European countries, and include more contextual variables to further develop the theory. *Immigration and Conflict in Europe* is a landmark study in the field of European politics and should be the benchmark for further research in the field.

Immigration has profoundly changed postwar European societies, and the political consequences are more and more diverse and visible. Opposition to immigration comes in different forms, but no West European country is without it today. At the same time, much research still focuses predominantly on the same parties in the same group of (large) countries, leaving much to be discovered (also because the radical right does particularly well in small countries). Future research should go beyond the trodden paths by focusing on more countries and groups (including nonparties), employing more diverse methods (e.g., ethnography), and asking new questions. For example, has the redefinition of the enemy, from "Turk" to "Muslim," led to a new emphasis on Christianity among nativist groups? Do immigration latecomers (like Italy or Spain) go through similar processes of opposition to immigration as the early-immigration countries (like Britain and France)? And what are the consequences of the shift from immigration to integration for traditional anti-immigration forces? Future studies that address these questions will do well to consult both the books reviewed here as examples of solid, empirical research in the field.

Walled States, Waning Sovereignty. By Wendy Brown. New York: Zone Books, 2010. 168p. $25.95.

The Deportation Regime: Sovereignty, Space, and the Freedom of Movement. Edited by Nicholas De Genova and Nathalie Peutz. Durham, NC: Duke University Press, 2010. 520p. $99.95 cloth, $27.95 paper.
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— Jacqueline Stevens, *Northwestern University*

The state is a central frame of reference for virtually all political scientists. And yet it is astonishing how little attention is paid by political scientists to the ways that states limit, regulate, and restrict the freedom of movement of millions of people in extreme but increasingly common and important instances, through attempts to control borders through policies of detention and incarceration. The two books under review provide welcome attention to these issues.

Thanks to Nicholas De Genova, Nathalie Peutz, and Duke University Press, social scientists now have, in *The Deportation Regime,* an urgently needed compendium providing copious information and analysis of deportation regimes in Europe, the Americas, North Africa, and the Middle East. And kudos as well to *Perspectives,* which had the perspicuity to inform this audience about a volume that is edited by two anthropologists and includes only