

Spillover Effects of Stricter Immigration Policies

*Cristina Bratu
Matz Dahlberg
Till Nikolka*

STRICTER NATIONAL MIGRATION POLICIES can divert migration flows to countries with looser restrictions. In this brief, we provide evidence for the existence of such spillover effects by estimating the effect of stricter family reunification rules in Denmark on migration behaviour. We show that stricter rules for reunification increased the emigration of those Danish residents affected by the reform to Sweden and that they indeed moved for family reunification reasons. Not all of them stayed in Sweden permanently, and a majority of those leaving returned to Denmark. Our results indicate that potential spillover effects from national migration policies can be substantial and should be taken into account by policy makers, particularly in an integrated economic area such as the European Union. Otherwise, a “race to the bottom” in the setting of migration policies is likely to occur.

Cristina Bratu, Postdoctoral Researcher in Economics at Aalto University

Matz Dahlberg, Professor in Economics at the Institute for Housing and Urban Research, Uppsala University

Till Nikolka, Senior Researcher in Economics at the German Youth Institute

“The Danish reform made Sweden an attractive alternative destination for those unable to reunite in Denmark.”

Introduction

In the presence of wars, global income inequality, and the uneven effects of climate change across the world, the movement of people across countries in search of a better life is a reality that will not cease to exist any time soon. According to estimates based on Ukrainians' desire to move and their preferred destinations, approximately 300,000 Ukrainians may come to Sweden to seek refuge from the ongoing war.¹ Who moves and where? Do governments have the power to affect the number of migrants and the composition of migration flows?² Research on the determinants of migration across countries suggests that stricter entry laws indeed tend to be associated with reduced migration flows.³ Closer to home, recent research has shown that Sweden's decision to grant permanent instead of temporary residence permits to Syrians in 2013 increased the number of Syrian asylum seekers to Sweden.⁴

However, it is not only about *how many* come but also about *who* comes. Countries may design policies intended to attract certain kinds of migrants and deter others. de Haas et al. (2018) show that even though the overall trend has been towards less restrictive migration policies across the Western world since 1945, certain groups, such as the highly skilled, have received more favourable treatment, while other groups, such as irregular migrants and family migrants, have been the targets of stricter rules. In this context, it is easy to see how the stringency of one country's immigration policies might divert flows to countries deemed “substitutes” and that have a looser set of restrictions. We refer to such responses as spillover effects.

Empirically testing whether this theoretical possibility is borne out by the

data is made difficult by the fact that migration is a choice, and so are policy changes. In a recent study, Brekke et al. (2017) use cross-country data and information on asylum policy changes and show that stricter entry policies have deflection effects, which means that asylum seekers move to an alternative destination, particularly if the alternative country can be thought of as a close substitute from the migrants' perspective.⁵ However, a negative correlation between strict policies in one country and increased immigration to similar destination countries does not mean that the direction of causality is from the policy to immigration; the policy may well be a reaction to immigration.

In this policy brief, we present results from a published paper in which we try to overcome the methodological challenges inherent in cross-country studies to verify and quantify the existence of such spillover effects in the context of a major family reunification policy reform in Denmark in 2002, which made it more difficult for Danish residents to form a couple with a non-European (non-EU) spouse.⁶ We argue that the Danish reform made Sweden an attractive alternative destination for those unable to reunite in Denmark.

Family reunification has been one of the most important channels for migration to European Union (EU) countries in the last two decades, and Sweden is no exception. In contrast to labour migrants, migrants who move for family reasons are perceived as less likely to integrate successfully into the labour market and thus more likely to make use of the welfare state.⁷ Governments may therefore try to use changes in rules to influence the number and characteristics of family migrants. For example, requiring a certain income level from the person who wants to

1. Elinder et al. (2022).

2. Unless otherwise specified, we use the word “migrant” in the most general of terms to refer to an individual who resides in a country other than their birth country, regardless of whether their reason for migration is work, study, family or asylum seeking.

3. See, for example, Hatton (2009, 2016), Neumayer (2004), and Ortega and Peri (2013).

4. Andersson and Jutvik (2018) provide trustworthy causal evidence on this question.

5. The literature examining this type of “spillover” effect is quite small. In addition to the paper by Brekke et al. (2017), see, e.g., Bertoli et al. (2011) and Bertoli and Moraga (2013).

6. Bratu et al. (2020).

7. Borevi (2018).

reunite with a partner effectively prevents lower-income individuals from reuniting, at least until they can reach the required level of income.⁸ At the same time, the extent to which family migration flows can be controlled is limited by the fact that governments must uphold the right to family life enshrined in various international conventions.⁹ Despite its importance, the family reunification channel for migration has been studied much less than the labour immigration channel.¹⁰

In the Nordic context, Sweden and Denmark, the countries this brief focuses on, have been polar opposites in regard to rules that target family migrants. At the time of the 2002 reform in Denmark, Sweden had very weak restrictions on those moving for family reasons. The Swedish policies were generally considered liberal in terms of eligibility; that is, in terms of who counted as a family member, since reunification was possible for both married and cohabitating couples (for both homosexual and heterosexual couples) and for children up to the age of 18.^{11 12} No other restrictions on either the sponsor (the person based in Sweden) or the partner (the person wishing to migrate to Sweden on family grounds) were in place at the time. Upon being granted a family permit, migrants have the same rights as permanent residents.¹³

Even though Denmark had already started tightening its restrictions in 1992 when it introduced a minimum residency requirement of at least five years and a financial requirement for the sponsor,¹⁴ the family migration policies of Denmark and Sweden were not substantially different at the end of the 1990s.¹⁵ However, from that point onwards, the rules became gradually stricter in Denmark (including, e.g., minimum age requirements for the spouses and proof of attachment to the country, self-support, and language skills), leading to a situation in which the two countries had among the most restrictive (Denmark) and the most liberal (Sweden) family reunification policies in Europe.¹⁶ The turning point was the 2002 reform in Denmark, which brought about changes that we detail in the next section. Given the differences that emerged, it is not unreasonable to expect that the 2002 reform had spillover effects on Sweden.¹⁷

Using the 2002 reform in Denmark, we examine three questions. First, did the reform lead to an increased emigration rate from Denmark for individuals affected by the reform? We use Danish administrative data to answer that question. Second, did the affected individuals who emigrated from Denmark actu-

ally reunite with a partner in their new country? To answer that question, we use Swedish administrative data. Finally, we examine whether those individuals who emigrated to a new country and reunited with a partner stayed in the new country in the long run. To answer that question, we also use Swedish administrative data.

Tougher rules for family reunification in Denmark in 2002

The family reunification reform we study in our paper was announced in January 2002 and implemented in June of the same year. Unlike in the previous regime, it was required that the following conditions be fulfilled before a Danish partner (the sponsor) could be eligible for family reunification with a non-EU partner:¹⁸

- both partners had to be at least 24 years old (the “24-year” rule)
- the partners had to show proof of a stronger affiliation to Denmark than to any other country, measured as their combined number of years of residence in different countries (the attachment requirement)
- the sponsor had to be able to provide financially for the family
- the sponsor had to show evidence of not having received social assistance in the year prior to the application
- the sponsor had to put down bank collateral amounting to DKK 63,413 (in 2008 DKK) in case the family member benefited from social assistance after arrival
- the sponsor had to show proof of adequate housing

The officially stated goal of the reform was to prevent forced and arranged marriages,¹⁹ but the reform has also been interpreted as an attempt to decrease the immigration of third-country nationals.²⁰

From the description of the reunification reform in 2002, it is clear that it became significantly more difficult to reunite with a non-EU partner in Denmark. Fig. 1 shows the number of family tie permits granted in Denmark between 1997 and 2009. We see a sharp drop in family tie permits starting in 2002, with the number of permits stabilizing at a lower level relative to the level before the reform, which suggests that the reform had the intended effect of reducing the flow of attached family members to Denmark, not just immediately but also in the long run.

8. A recent example is the income requirement in the UK, which puts limits on the kind of individuals who can sponsor visas for family members; see Sumption and Vargas-Silva (2019).

9. Borevi (2018).

10. Hatton (2014).

11. Borevi (2015).

12. We should add that since 1997, family members beyond partners and children could be granted a family permit only if the sponsor proved that there was a dependency relationship between the sponsor and the family member that predated the sponsor’s move to Sweden. In this respect, Sweden was stricter than other European countries (Borevi 2015). However, since our analysis concerns only couples, this rule is not relevant in our context.

13. Borevi (2015).

14. Hedetoft (2006).

15. Bech et al. (2017).

16. Bech et al. (2017).

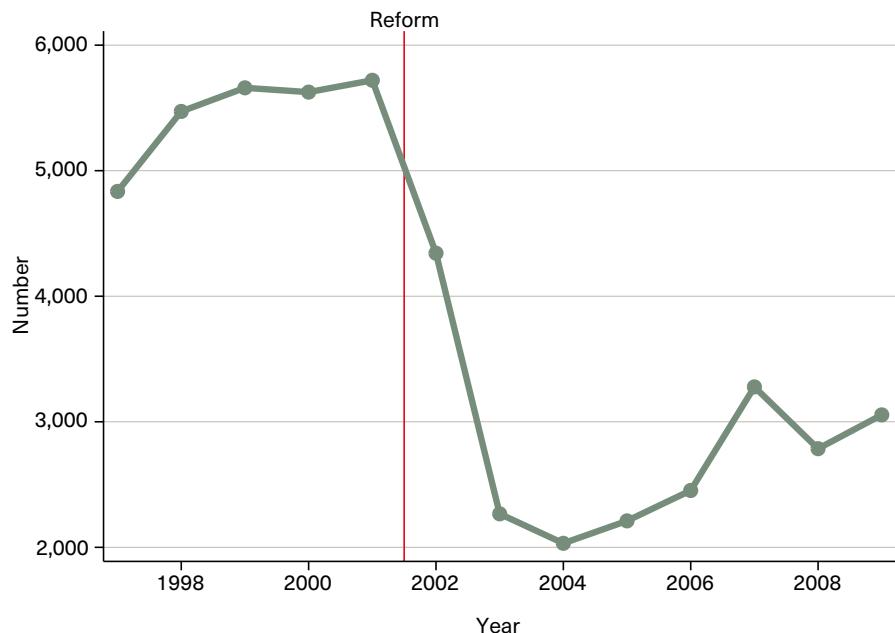
17. At the time of the Danish reform, Norway and Finland also had more liberal family reunification rules than Denmark. However, from the mid-2000s onwards, both countries started increasing the restrictiveness of their family reunification policies (see a detailed account in Bech et al. 2017 for Norway and Pellander 2016 for Finland). In Sweden, this trend did not start until 2010, when a maintenance requirement was introduced, whereby the sponsor had to prove that they could financially support themselves and offer suitable accommodations for the entire family. However, refugees and other groups of migrants were exempt (Bech et al. 2017).

18. Rytter (2012).

19. Regeringen (2003).

20. Skyt Nielsen et al. (2007).

Fig. 1. Number of permits granted on family grounds in Denmark.



Notes: The figure captures the number of family tie permits granted yearly to spouses or cohabitants in Denmark from 1997 to 2009.

Source: Bratu et al. (2020).

Spillover effects of the Danish reform

The 2002 reform imposed strict restrictions on Danish citizens wanting to reunite with non-EU spouses. Those affected by the stricter rules had two alternatives: forgo reunification altogether or move somewhere where reunification was possible. We argue that Sweden's geographical proximity to Denmark and its less strict family reunification policies caused the reform to redirect individuals affected by the Danish reform to Sweden: the Danish citizens first moved to Sweden and were then joined by their non-EU spouses via family reunification. In this way, the reform generated unintended consequences or spillover effects.

Apart from its convenient geographical location, what made Sweden a feasible alternative destination was the fact that Danish citizens were able to apply for family reunification with their non-EU partners under either one of two regulatory frameworks: i) *national-level rules*: due to the 1954 Nordic agreement that allows Nordic citizens to travel and move freely across Nordic countries, Danish citizens in Sweden can reunite with a partner under Swedish rules, which, at the time, imposed no requirements on

either partner apart from a minimum age of 18, and ii) *EU rules*: under the EU Free Movement Directive, EU citizens who move from their country of origin to another EU member state can apply for family reunification under EU law, regardless of the nationality of their partner; a Danish citizen moving to Sweden would therefore satisfy the eligibility requirement. EU rules were somewhat stricter than Swedish rules in that the Danish citizen would have had to provide proof of legal residence in Sweden. Under these same rules, EU citizens were also allowed to return to their country of origin and bring the families they had formed abroad with them.

Data

We use Danish and Swedish administrative data to study spillover effects in the form of the redirection of migration flows from Denmark to Sweden. On its own, neither dataset would be sufficient to paint a complete picture. Having access to both means that with the Danish dataset, we can study the *emigration patterns from Denmark* that emerged in the aftermath of the reform. With the Swedish dataset, we can instead study *immigration patterns*

to Sweden to understand whether those who moved from Denmark indeed moved because of the reform. The datasets include administrative data covering the whole population living in Denmark and Sweden in a given year. An important feature is that both countries maintain immigration as well as emigration registers that record, for each individual, the date of emigration or immigration as well as the destination or origin country. The Danish and Swedish administrative data cannot be merged to follow an individual leaving Denmark all the way to Sweden. The next-best approach is to use the datasets separately in such a way that the analyses mirror each other.

We first analyse whether the tightening of family reunification rules affected out-migration from Denmark using the administrative data on the full Danish population. We use data from the years 1995 to 2009 that combine information on the socioeconomic characteristics of individuals residing in Denmark, such as their age, municipality of residence, and family status. For each resident, we also observe the date of emigration and the destination country. Furthermore, we can also link residents to their spouses (whether married or cohabitating).

To analyse the causal effect that a treatment such as a policy change has on individual behaviour, we would ideally like to have a research design in which one group of people is exposed to the policy reform (the treatment group) while another group is exposed to the old rules (the control group) at the same time, with the two groups otherwise being similar. In an experimental design, researchers achieve this goal by randomly allocating people to treatment and control groups. However, experimental designs are rarely available in policy settings, nor can they be applied to a population-wide analysis. However, if we want to empirically study the effect of a policy reform, we need to find an *ex post* control group that is sufficiently similar to the treatment group but that was not affected by the reform. The control group helps to establish the so-called counterfactual, or what would have happened in the absence of the reform. We can then compare the outcomes in the treated group to those in the control group and conclude with reasonable certainty that the difference is due to the reform. Knowing the characteristics of everyone in the population, we can construct a treatment and a control group that we can observe over time. This makes it possible to establish convincing causal evidence of a policy change that would not be possible with data aggregated at

a much less detailed level.

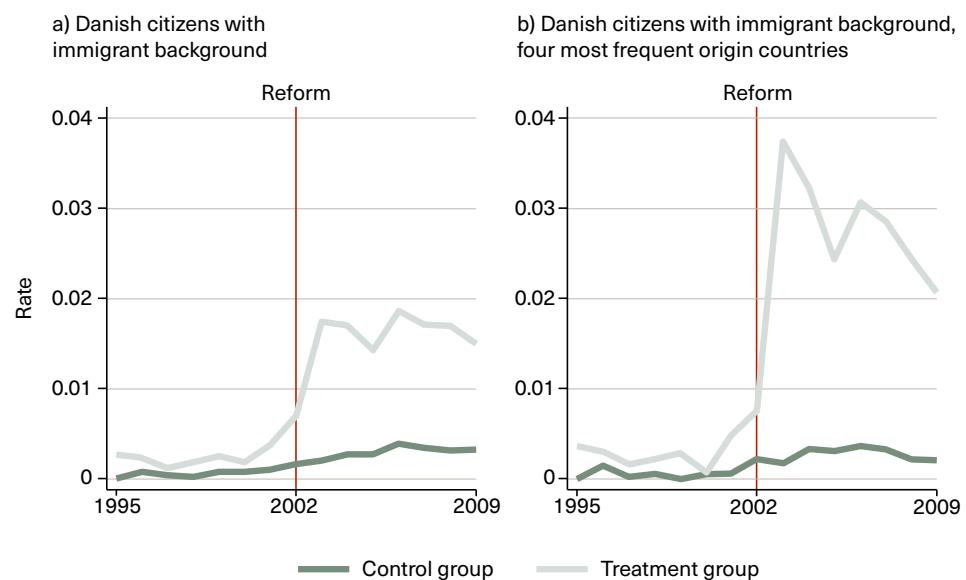
We argue that among Danish residents, Danish citizens with immigrant backgrounds are potentially the most affected by the policy change. These are residents who were either born abroad or who have non-Danish parents who were both born abroad. Thus, they can be expected to have ties to their home countries and therefore to be more likely to form a couple with a partner from abroad. Moreover, as the policy reform required partners to show proof of a stronger affiliation to Denmark than to any other country, it would be less likely that Danish citizens with an immigrant background could fulfil this requirement if they spent part of their lives in their country of birth. From this population, we consider those individuals not cohabitating or married with a partner in Denmark as our treatment group, which faces stricter rules for potential family reunification after the reform. We consider individuals who are cohabitating or married with a non-EU partner as the control group in our main analysis. In this case, the non-EU partner must already be holding a residence permit; hence, this type of couple should not be affected by the family reunification reform.

As illustrated above, the Danish data allow us to study the emigration patterns of those whom we have identified as being affected and unaffected by the reform. Since we argue that Sweden is an attractive destination country for those potentially wanting to reunite with their non-EU spouses, we use the Swedish data to find reunited couples – both those affected and those unaffected by the reform. We again turn to the design of the reform to identify these two groups. In regard to family reunification, there is always an individual who is already based in the destination country – we call this person the sponsor – and an individual who wishes to move to the destination country in order to reunite with the sponsor – we call this person the partner. In the context of the reform we study, we define a couple as treated if the sponsor is a Danish citizen who has emigrated to Sweden from Denmark and the partner is a non-EU-born spouse who moves to Sweden from a country other than Denmark. The sponsor's connection to Denmark and the partner's lack thereof is what determines the treatment status. Therefore, an untreated couple is one in which both the sponsor and the partner have a connection to Denmark.

Fig. 2. Yearly emigration rates to Sweden for individuals in the treatment and control groups.

Notes: The treatment group includes Danish citizens with an immigrant background who are older than 18 years of age and are not cohabitating or married with a partner in a given year in Denmark. The control group includes Danish citizens with an immigrant background who are older than 18 and are cohabitating or married with a non-EU citizen in a given year in Denmark. The four most frequent origin countries are Turkey, Pakistan, the former Yugoslavia, and Bosnia.

Source: Bratu et al. (2020).



Increased emigration from Denmark to Sweden

We first use the Danish administrative data to analyse whether the 2002 reform increased the emigration rates of Danish residents who were affected by stricter rules for family reunification. To this end, we compare emigration rates to Sweden for the treatment group and the control group after the stricter reunification requirements were implemented. As argued above, Sweden is a potential alternative destination for those who cannot fulfil the stricter family reunification rules in Denmark.²¹ In Fig. 2, the light line represents yearly emigration rates to Sweden from 1995 to 2009 for the treatment group, and the dark line represents the corresponding emigration rates for the control group (the vertical line indicates the reform year). Panel a) in Fig. 2 includes all individuals in the treatment and control groups in a given year and shows that 0.4% of the treatment group emigrated from Denmark to Sweden before the reform (2001) and that this number increased to 1.8% in the year after the reform (2003). In Panel b), we restrict the sample to Danish citizens with an immigrant background from one of the four major countries of origin in Denmark: Turkey, Pakistan, Bos-

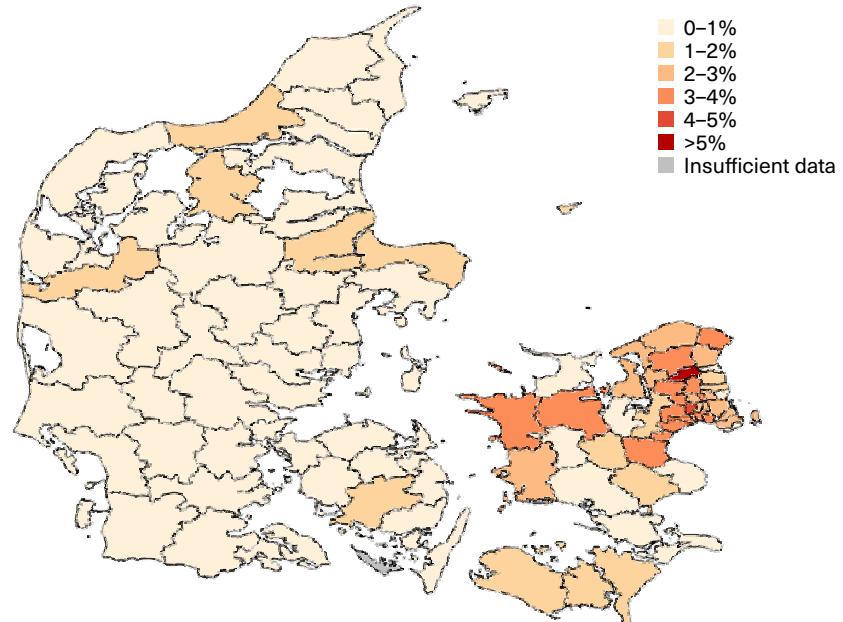
nia, and former Yugoslavia (these countries account for more than 50% of the population with non-EU immigrant backgrounds in Denmark during the sample period). Among those from these origin countries, 0.7% of the treated group emigrated from Denmark the year before the reform (2001); the corresponding figure was 3.9% the year after the reform (2003).²² These patterns strongly suggest that the reform led to an outflow of individuals in the treatment group to Sweden. In contrast, there is no visible change in emigration rates around the reform year for the control group. Before the reform, emigration rates for both the treatment and control groups evolve very similarly, which is a crucial identifying assumption for causal inference in our analysis.

In addition, we conduct a more formal test of the results reported above by estimating a so-called difference-in-differences model. This allows us to report statistical inference on the effect of the reform (i.e., how precisely we can estimate this effect) and to check the robustness of the results when accounting for other factors that might influence the results. The results of these analyses, which confirm the findings in Fig. 2, can be found in Bratu et al. (2020).

21. We indeed found that among all emigration events in the treatment group in the year after the reform, an absolute majority involved emigration to Sweden, confirming that Sweden was a de facto attractive destination country for this group.

22. We also compared the emigration rates to Sweden with the emigration rates to all countries for the treatment group (see Figure A.6 in Bratu et al. 2020).

Fig. 3. Average migration rates to Sweden among individuals in the treatment group after the reform (2003–2009) by municipality.



Source: Bratu et al. (2020).

Another indicator that geographic proximity to Sweden plays an important role in the emigration decisions of individuals in the treatment group is provided in Fig. 3. The map shows that the migrants moving to Sweden mainly originate in the eastern municipalities in Zealand, the part of Denmark that is closest to Skåne in Sweden.

Individuals reunite with their partners in Sweden

We use the Swedish administrative dataset that has information on where each individual moves from and to within Sweden, as well as on demographic and socioeconomic characteristics, which allows us to construct treatment and control groups as defined above.

The map in Fig. 3 shows that most of the emigrants to Sweden originated from the easternmost region in Denmark, Zealand. The Swedish data show that affected individuals settled primarily in Skåne, the southernmost region in Sweden and the Swedish region closest to Zealand (Fig. 4). This finding adds to the pool of evidence in support of the reunification reform having caused individuals to move to Sweden for reunification purposes, but it is not

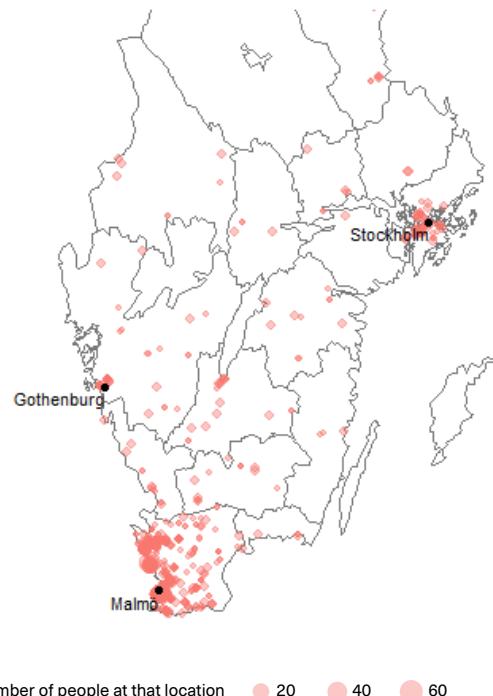
definitive on its own.

We take a further step towards assessing the causal effect of the reform on migration flows to Sweden by using a statistical method called interrupted time series analysis (ITSA). As the name suggests, this method requires i) the use of time series data – in our case, the number of individuals in treated couples migrating to Sweden over time – and ii) the existence of an interruption at a specific point in time – in our case, the 2002 reform. It tracks how the time series evolves before and after the interruption. The effect is estimated by the change in the level and slope of the time series after the intervention relative to the preintervention level and slope, which should be constant in order for the effect to be interpreted as causal.²³ We illustrate how this works in Fig. 5(a), which tracks the number of affected individuals moving to Sweden every six months, starting in 1995 until 2009.

The solid dots represent the number of individuals in the treatment group who immigrated to Sweden for every six-month interval. We see that very few affected individuals moved to Sweden to form a couple before the 2002 reform. The flat time series in the pre-reform period is also reassuring given the ITSA requirements for recovering a

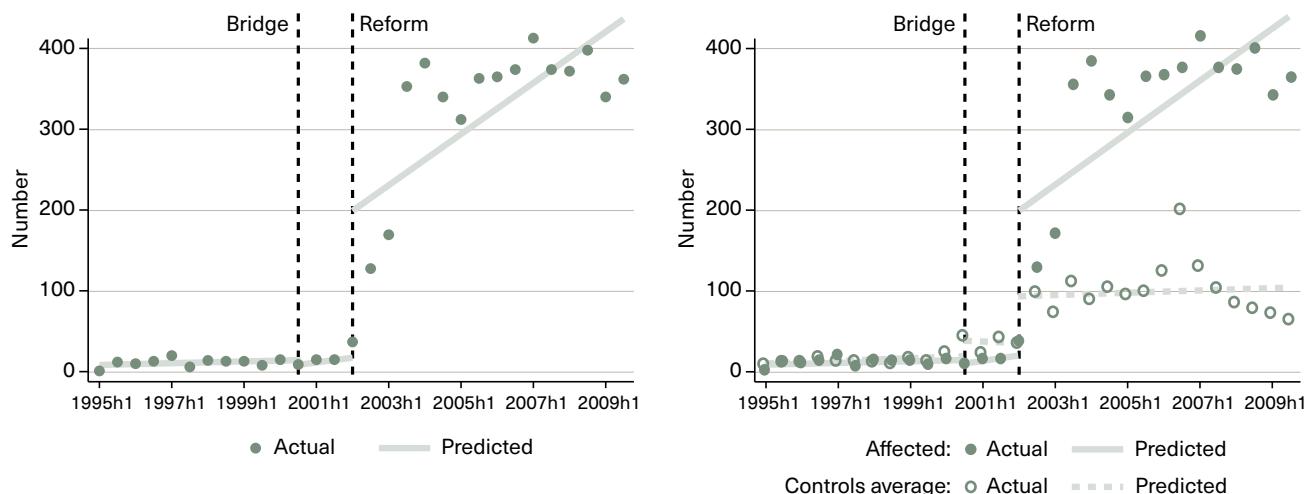
²³ Another requirement is the absence of other reforms at the same time that might have affected the treated group. See the explanation in Bratu et al. (2020) for why this is not a concern in our setting.

Fig. 4. Immigration destinations across the southern half of Sweden.



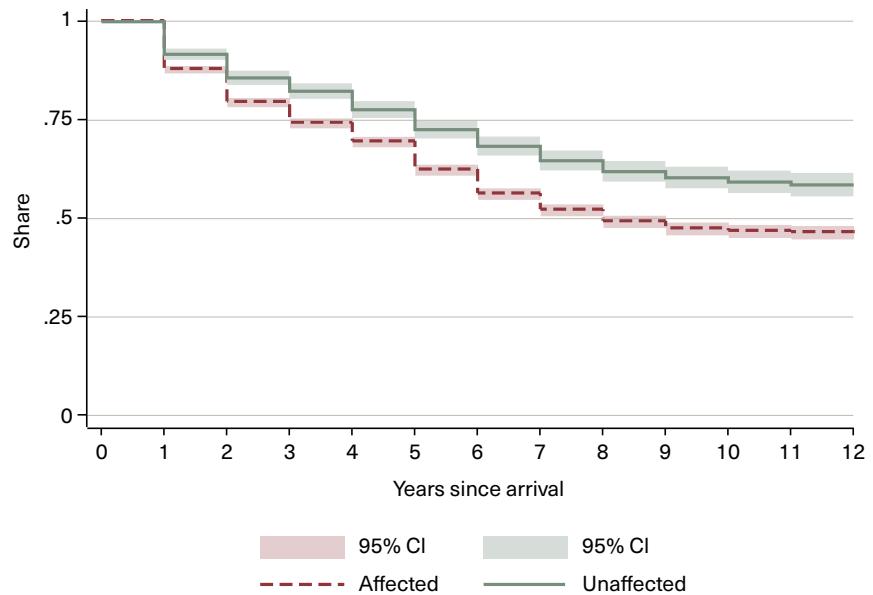
Source: Bratu et al. (2020).

Fig. 5. Stricter reunification rules in Denmark and immigration to Sweden. Panel (a) displays the number of affected individuals reuniting in Sweden. Panel (b) adds a control group consisting of individuals who moved to Sweden for reasons unrelated to family reunification.



Source: Bratu et al. (2020).

Fig. 6. Share of individuals remaining in Sweden. The red (grey) line shows the share of individuals in the affected (unaffected) group who were still in Sweden a given number of years after migration.



Source: Bratu et al. (2020).

causal effect. After 2002, however, we see a sharp increase in the number of affected immigrants: between 2003 and 2009, approximately 300 to 350 individuals migrated to Sweden to form a couple every six months. The change in the slope is visible beginning in the first half of 2002, when the reform was decided on. The time series then reaches a new, higher level relative to the pre-reform period.

Fig. 5(a) shows the immigration patterns of the members of the treated group only. However, we can repeat the exercise and show how the control group behaves as well.

Fig. 5(b) now plots two sets of dots: the solid dots, which, as before, capture the number of treated individuals migrating to Sweden every six months, and the empty dots, which capture the number of unaffected individuals migrating to Sweden. We see that the unaffected group follows a very different time pattern, with a gradual increase in the number of immigrants of this type from 2000 onwards. We note here that the Øresund Bridge linking Copenhagen and Malmö opened in July 2000, making travel between the two cities fast and easy. The fact that we see individuals in the control group moving to Sweden

around that time indicates that they were reacting to the opening of the Øresund Bridge rather than the reform.²⁴

Many reunited couples leave Sweden

Our final set of results reveals that the move to Sweden was temporary for a large number of the affected individuals.

Fig. 6 shows, for each year after migration, the share of individuals still in Sweden by treatment status. We see that approximately 20% (50%) leave within two (eight) years. We further examine the destination countries of those who leave and find that the vast majority (87%) return to Denmark.

Summary and policy implications

Examining the migration effects of the stricter reunification rules in Denmark in 2002, we reach three main conclusions. First, using individual-level administrative data from Denmark, we find that the reform led to a statistically significant increase in the likelihood of

²⁴ In our published paper, we also provide evidence that Sweden was not a particularly attractive destination for migrant couples in general by studying the immigration patterns of reunited couples with a Finnish or Norwegian connection.

“If policymakers know (or believe) that certain immigration policies, such as generous rules for family reunification, attract immigrants, and if it is assumed that no country wants to be the most generous, a ‘race to the bottom’ in the context of migration policies is likely to materialize.”

emigrating from Denmark for those affected by the reform. Our analysis also reveals that the absolute majority of those potentially affected by the Danish reform emigrated to neighbouring Sweden, a result that is especially pronounced among Danish residents residing in Zealand, the Danish region closest to Sweden with easy connections (bridge, ferry) between the two countries.

Second, using individual-level administrative data from Sweden, we are able to show that the Danish emigrants indeed moved to Sweden for family reunification purposes. Not only did the reform cause an inflow of migrants from Denmark to Sweden but also an inflow of non-EU partners to Sweden. While migration from Denmark to Sweden for family formation purposes was almost nonexistent before the reform, approximately 350 individuals migrated to Sweden every six months after the reform to form a couple with a non-EU partner.

Third, by following the individuals who emigrated to Sweden as a result of the Danish reform over time, we find that not all of them stayed in Sweden permanently. After two years, approximately 20% had emigrated from Sweden, and after eight years, the corresponding figure was approximately 50%. The majority of those who emigrated after forming a couple in Sweden went back to Denmark.

We believe that our results can be generalized along several dimensions. First, even though the Nordic Agreement makes mobility and residency among the Nordic countries special, we do not think that the results are unique to the specific case of Denmark and Sweden. The Free Movement Directive at the European level implies that our results might very well apply to other (adjacent) countries within the EU (even though the magnitude of the effects may be case-specific). Since EU citizens can exercise their right to free movement within the EU, family formation in another country is always possible under the EU reunification rules. Second, we conjecture that behavioural responses such as those that we find are not restricted to the context of family reunification policies; other policies that make a destination country less attractive for immigrants could be equally likely to trigger movement away from the host country towards destinations with less strict rules.

We also note that while we studied the spillover effects between two countries, such responses may well happen within the borders of one country only; for example, across admission catego-

ries. Restrictive asylum policies may trigger irregular migration instead of preventing this type of migration entirely,²⁵ or changes in work permits may affect the selection of students.²⁶ More research is needed to understand to what extent this has happened in Sweden. In 2008, the labour immigration system became entirely employer-driven; that is, employers were free to decide whom to hire from abroad, and there were no skill requirements or quotas. In addition, rejected asylum seekers who had worked for six months and had an employment offer could apply for a work permit. The rationale behind this rule was to give those who had established themselves in the labour market the opportunity to remain in Sweden. It has been argued that, in practice, the reform led to previous asylum seekers who had either lived as undocumented migrants or had gone back to their home country returning as labour migrants.²⁷ In addition, these individuals went mainly into low-skilled occupations, where employers presumably did not have difficulties finding workers. The main goal of the 2008 reform was to instead increase employment in those occupations for which employers could not find workers domestically.²⁸

The purpose of this paper was to study whether a particular immigration policy had effects outside the borders of the country that imposed it. It was beyond the scope of the paper to discuss the potential labour market effects on either the sending or the receiving country. The term “spillover effects” may seem to have a negative connotation, in that Sweden “loses” because of Denmark’s policies. The spillover effect we find is large relative to the status quo before the reform, but it is small in absolute numbers. The arrival of 350 individuals every six months is unlikely to have had any substantial effects, especially since they settle in relatively large urban areas. However, even if the flows had been larger, the evidence thus far on the effects of immigration on receiving countries is far from clear-cut. Ruist (2019) argues that the impact of immigration on receiving economies can vary significantly depending on the reason for migration. Foged and Peri (2016), for example, find positive effects on the wages of natives due to refugee inflows to Denmark, which can be explained by skill complementarities in the labour market. In other contexts, studies have reported at most small negative effects and most often zero effects from (primarily labour) immigration inflows on native wages or employment (see, e.g., Peri 2016 for an overview).

25. Czaika and Hobolth (2016).

26. Kato and Sparber (2013).

27. Emilsson et al. (2014).

28. OECD (2011).

Our analysis of the 2002 reunification reform in Denmark shows that the spillover effects of national migration policies can be substantial. If policy-makers know (or believe) that certain immigration policies, such as generous rules for family reunification, attract immigrants, and if it is assumed that no country wants to be the most generous, a “race to the bottom” in the context of migration policies is likely to materialize.²⁹ According to some scholars, this is precisely what happened in the case of the Swedish family reunification policies in the aftermath of the 2015–2016 “refugee crisis”.³⁰ In June 2016, as a reaction to the large number of refugees who entered Sweden, a temporary restriction was put in place under which asylum seekers eligible for subsidiary protection did not have the right to family reunification. The exemption from the maintenance requirement was also removed for certain groups.³¹ This change has been interpreted as an effort to adjust Sweden’s family reunification policies to match the EU minimum level to avoid being the country with the most favourable rules.³² In the absence of a full harmonization of migration policies across EU member states, countries will likely continue to engage in the type of “policy games” described in this brief.³³

References

Andersson, H. and K. Jutvik (2018), “Do asylum seekers respond to policy changes? Evidence from the Swedish-Syrian Case”, forthcoming in *The Scandinavian Journal of Economics*.

Bech, E. C., K. Borevi, and P. Mouritsen (2017). “A ‘civic turn’ in Scandinavian family migration policies? Comparing Denmark, Norway and Sweden”, *Comparative Migration Studies*, 5(1), 1–24.

Bertoli, S., J. Fernandez-Huertas Moraga, and F. Ortega (2011), “Immigration policies and the Ecuadorian exodus”, *The World Bank Economic Review*, 25, 57–76.

Bertoli, S. and J. Fernandez-Huertas Moraga (2013), “Multilateral resistance to migration”, *Journal of Development Economics*, 102, 79–100.

Borevi, K. (2015). “Family migration policies and politics: Understanding the Swedish Exception”, *Journal of Family Issues*, 36(11), 1490–1508.

Borevi, K. (2018), “Familj, medborgarskap, migration - Sveriges politik för anhöriginvandring i ett jämförande perspektiv”, *Delmi Policy Brief* 2018:5.

Bratu, C., M. Dahlberg, M. Engdahl, and T. Nikolka (2020), “Spillover effects of stricter immigration policies”, *Journal of Public Economics*, 190.

Brekke, J.-P., M. Røed, and P. Schøne (2017), “Reduction or deflection? The effect of asylum policy on interconnected asylum flows”, *Migration Studies*, 5, 65–96.

Brueckner, J. K. (2000), “Welfare reform and the race to the bottom: Theory and evidence”, *Southern Economic Journal*, 66, 505–525.

Czaika, M. and M. Hobolth (2016), “Do restrictive asylum and visa policies increase irregular migration into Europe?”, *European Union Politics*, 17(3), 345–365.

Dahlberg, M. and K. Edmark (2008), “Is there a ‘race-to-the-bottom’ in the setting of welfare benefit levels? Evidence from a policy intervention”, *Journal of Public Economics*, 92, 1193–1209.

De Haas, H., K. Natter, and S. Vezzoli (2018), “Growing restrictiveness or changing selection? The nature and evolution of migration policies”, *International Migration Review*, 52(2), 324–367.

Elinder, M., O. Erikson, and O. Hammar (2022), How large will the Ukrainian refugee flow be, and which EU countries will they seek refuge in?, *Delmi Policy Brief* 2022:3.

Emilsson, H., K. Magnusson, S. Osanami Törngren, and P. Bevelander (2014). The world’s most open country: Labour migration to Sweden after the 2008 law. Malmö University, Malmö Institute for Studies of Migration, Diversity and Welfare (MIM).

Foged, M and G. Peri. (2016), “Immigrants’ effect on native workers: New analysis on longitudinal data”, *American Economic Journal: Applied Economics*, 8(2), 1–34.

Görlach, J.-S. and N. Motz (2020), “Spillovers and strategic interaction in immigration policies”, *Journal of Economic Geography*, 21(2).

Hatton, T. J. (2009), “The rise and fall of asylum: What happened and why?”, *The Economic Journal*, 119, F183–F213.

Hatton, T. J. (2014), “The economics of international migration: A short history of the debate”, *Labour Economics*, 30, 43–50.

Hatton, T. J. (2016), “Refugees, asylum seekers, and policy in OECD countries”, *American Economic Review*, 106, 441–45.

Hedetoft, U. (2006). Denmark: Integrating immigrants into a homogeneous welfare state. *Migration Policy Institute*, 1.

29. We know that this type of strategic interaction takes place among different regions of the same country in the determination of fiscal policies (see, e.g., Brueckner, 2000, and Dahlberg and Edmark, 2008).

30. Borevi (2018).

31. See <https://www.government.se/press-releases/2016/05/proposal-to-temporarily-restrict-the-possibility-of-being-granted-a-residence-permit-in-sweden/> for details.

32. Borevi (2018).

33. See also the discussion in Görlach and Motz (2020).

Kato, T. and Sparber, C. (2013), “Quotas and quality: The effect of H-1B visa restrictions on the pool of prospective undergraduate students from abroad”, *Review of Economics and Statistics*, 95(1), 109–126.

Neumayer, E. (2004), “Asylum destination choice. What makes some countries more attractive than others?”, *European Union Politics*, 5, 155–180.

OECD (2011), *Recruiting Immigrant Workers: Sweden 2011*, Recruiting Immigrant Workers, OECD Publishing, Paris, <https://doi.org/10.1787/9789264167216-en>.

Ortega, F. and G. Peri (2013), “The effect of income and immigration policies on international migration”, *Migration Studies*, 1(1), 47–74.

Pellander, S. (2016). Gatekeepers of the family: Regulating family migration to Finland. Publications of the Faculty of Social Sciences, University of Helsinki.

Peri, G. (2016), “Immigrants, productivity, and labor markets”, *Journal of Economic Perspectives*, 30(4), 3–30.

Regeringen (2003). “Handlingsplan for regeringens indsats i perioden 2003–2005 mod tvangsægteskaber, tvangslignende ægteskaber og arrangerede ægteskaber” [The Government’s Action Plan for 2003–2005 on Forced, Quasi-Forced and Arranged Marriages], Copenhagen: Regeringen i Danmark.

Ruist, J. (2019), “Migration – orsaker och konsekvenser” (SNS Förlag).

Rytter, M. (2012), “Semi-legal family life: Pakistani couples in the borderlands of Denmark and Sweden”, *Global Networks*, 12(1), 91–108.

Skyt Nielsen, H., N. Smith, and A. Celikaksoy (2009). “The effect of marriage on education of immigrants: Evidence from a policy reform restricting marriage migration”, *The Scandinavian Journal of Economics*, 111, 457–486.

Sumption, M. and C. Vargas-Silva (2019), “Love is not all you need: income requirement for visa sponsorship of foreign family members”, *Journal of Economics, Race, and Policy*, 2, 62–76.