The Welfare Magnet Hypothesis: Evidence from an Immigrant Welfare Scheme in Denmark[†]

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We study the effects of welfare generosity on international migration using reforms of immigrant welfare benefits in Denmark. The first reform, implemented in 2002, lowered benefits for non-EU immigrants by about 50 percent, with no changes for natives or EU immigrants. The policy was later repealed and reintroduced. Based on a quasi-experimental research design, we find sizable effects: the benefit reduction reduced the net flow of immigrants by about 5,000 people per year, and the subsequent repeal of the policy reversed the effect almost exactly. The implied elasticity of migration with respect to benefits equals 1.3. This represents some of the first causal evidence on the welfare magnet hypothesis. (JEL F22, H53, I38, J15)

Do generous welfare benefits act as magnets for low-skilled immigrants? This is a classic debate among economists and policymakers, but there is virtually no evidence on the question. The existing literature provides correlational evidence consistent with the welfare magnet hypothesis. For example, Borjas (1999) shows that immigrant welfare recipients in the United States tend to be clustered in high-benefit states, while Boeri (2010) shows that low-skilled immigrants in the European Union are more likely to locate in high-benefit countries. These patterns are suggestive, but it remains an open question if they reflect a causal relationship or if they are driven by confounding factors correlated with benefit levels. To obtain causal evidence, we need variation in welfare benefits that is plausibly orthogonal to other factors driving location choices.

We argue that Denmark provides an ideal setting for studying this question. First, Denmark has one of the most generous welfare systems in the world—benefit rates are even higher than in the other Nordic welfare states—making it a potential welfare magnet. Second, motivated by welfare magnet concerns and general anti-immigration sentiments, Denmark has experimented with immigrant welfare schemes that sharply reduce benefits to certain foreign immigrants. In June 2002, shortly after the formation of a new government supported by a far-right, anti-immigration

party, Denmark introduced a welfare scheme that reduced benefits by up to 50 percent for immigrants from outside the European Union.[1] The welfare scheme was controversial and widely debated. It was repealed in 2012 following the election of a center-left government and then reinstituted in 2015 after the return of a center-right government.

The Danish government has been active in disseminating information about the welfare scheme to potential immigrants. For example, around the reinstitution of the scheme in 2015, the government ran an ad campaign in Lebanese newspapers informing refugees of the reduced benefits.[2] These ads, illustrated in online Appendix Figure A.I, highlight the 50 percent benefit reduction at the top and suggest that the scheme was motivated largely by a desire to reduce the inflow of refugees.

To study the impact of the welfare scheme on migration flows, we consider two difference-in-difference (DiD) strategies. The first strategy is a *within-country* approach based on comparing immigration flows to Denmark from outside the European Union (treatments) and from inside the European Union (controls) around the three reforms.[3] The findings are striking: the immigration flows of the two groups evolve in parallel during the 20 years leading up to the 2002 reform, diverge sharply after the introduction of the immigrant welfare scheme in 2002, converge again following the repeal of the scheme in 2012, and diverge once more when the scheme is reintroduced in 2015. We find that the scheme reduced the net flow of immigrants by almost 5,000 people per year, corresponding to an elasticity of migration flow with respect to benefits equal to 1.3. The second strategy is a *cross-country* approach based on comparing non-EU immigration flows in Denmark and a synthetic control country constructed from the other Nordic countries. Consistent with the within-country approach, the cross-country approach features large and sharp migration effects around each of the three reforms.

To further underpin our interpretation of the data, we decompose the immigration effects by type of residence permit: asylum permits, family permits, and work/study permits. Only those coming on asylum or through family relations are treated by the welfare cuts. Consistent with this, we show that the effects are driven entirely by asylum- and family-based immigration; the effect on those coming for work or study is a precisely estimated zero. This implies that our estimates are not biased by time-varying unobservables that affect all non-EU immigrants relative to EU immigrants. Any threat to identification must come from time-varying unobservables that vary both by region of origin (European Union versus non-European Union) and by immigrant type within region (asylum/family versus work/study).

It is worth highlighting two points on interpretation. First, the effects should be interpreted as capturing location decisions *conditional on migration*. Since Denmark is just one small country, we would not expect the decision to emigrate from, say, Afghanistan to be affected by the Danish welfare system. Rather, it is the decision by an Afghan migrant to locate in Denmark instead of, say, Sweden or Germany that

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[1] To be precise, the scheme applied to immigrants from outside the European Union *and* the four member countries of the European Free Trade Association (EFTA), namely Iceland, Liechtenstein, Norway, and Switzerland.

[2] Lebanon hosts a number of large refugee camps and provides a common transit for Middle Eastern refugees headed to Europe.

[3] To avoid confounding effects of the EU enlargements in the post-reform period, coming from "inside the European Union" is defined based on the member countries in the pre-scheme year, 2001.

is affected by the Danish welfare system. Second, the presence of sizable welfare magnet effects may make it tempting for governments to introduce immigrant welfare schemes like the Danish one, and in fact several countries have introduced or are discussing the introduction of related policies.[4] Specifically, to the extent that the net fiscal impact of low-income immigration is negative, it may be individually optimal for countries to lower immigrant welfare benefits.[5] However, such policies impose negative fiscal externalities on other countries and are, in general, not socially optimal from a global perspective. This tension between local and global welfare when setting benefits for low-income immigrants is analogous to the tension that arises when setting taxes for high-income immigrants (see Kleven et al. 2020). Our findings suggest that the issues surrounding tax competition and the risk of a "race to the bottom" may be equally relevant for welfare policy.

Our paper contributes to an empirical literature estimating welfare magnet effects, for example Blank (1988), Borjas (1999), Dodson (2001), Gelbach (2004), Kaushal (2005), Fiva (2009), De Giorgi and Pellizzari (2009), Boeri (2010), and Razin and Wahba (2015). Much of the literature has focused on migration responses to state-level variation in welfare benefits in the United States, but the estimates vary greatly across studies and no consensus has been reached. Our main contribution is to provide some of the first quasi-experimental evidence on the existence of welfare magnet effects and to show that these effects can be sizable. The welfare scheme that we use for identification has been studied in two existing papers (Rosholm and Vejlin 2010; Andersen, Dustmann, and Landersø 2019), but they focus on a different question: the impact of lowering welfare benefits on immigrant outcomes (such as employment, earnings, crime, and children) conditional on locating in Denmark. Finally, while we are interested in the effects of welfare benefits on immigration, there is a recent literature studying what is essentially the reverse of our question: the effect of immigration on preferences for redistribution and voting outcomes (Alesina, Miano, and Stantcheva 2018; Alesina, Murard, and Rapoport 2019; Dustmann, Vasiljeva, and Damm 2019). The introduction of a special immigrant welfare scheme in Denmark provides prima facie evidence that immigration may shape redistributive preferences and policy. Importantly, our quasi-experimental approach will not be confounded by such reverse causation channels.

The rest of the paper is organized as follows. Section I describes the policy experiment and data, Section II presents our results on welfare magnet effects, and Section III concludes.

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[4] In the United States, the welfare reform act of 1996 (Personal Responsibility and Work Opportunity Reconciliation Act) denied noncitizens eligibility for welfare benefits. Special immigrant welfare schemes have also been passed in Canada (in 2014), Germany (in 2016), and Austria (in 2019, though the law was ruled unconstitutional and overturned later that same year). Countries where such welfare schemes have been proposed but not yet implemented include Finland, France, Latvia, Lithuania, the Netherlands, and Switzerland.

[5] We do not estimate the net fiscal impact of the immigrant welfare reform in this paper. Even if low-income immigrants collect welfare benefits (and pay little tax) in the first years after arrival, the net fiscal impact may be positive in the long run (including intergenerational effects).

I. Policy Experiment and Data

A. *Policy Experiment*

We use the immigrant welfare scheme in Denmark as a quasi-experiment for studying welfare magnet effects. The scheme was announced in January 2002, passed in parliament in June 2002, and took effect from July 2002. Only immigrants coming from outside the European Union and the four EFTA countries (Iceland, Liechtenstein, Norway, and Switzerland) are subject to the scheme. For simplicity, we will refer to treated immigrants as coming from "outside the European Union." Under the scheme rules, welfare benefits are much lower than the standard, native benefit rate. The largest cut applies to married couples with children for whom the maximum cash benefit is reduced by 50 percent due to the scheme. For other family types, the benefit drop is somewhat lower. In order for immigrants on scheme benefits to transition to the higher native benefits, they have to stay in Denmark for at least seven years. The scheme was repealed in January 2012 and reintroduced in September 2015. Figure A.II in the online Appendix illustrates the policy experiment, comparing cash benefits for scheme immigrants to cash benefits for nonscheme immigrants and natives over time. The figure also shows that benefits in the other Nordic countries have been smooth during the time period studied.

This benefit variation forms the basis of our two DiD designs: (i) a within-country design comparing immigration from outside the European Union to immigration from inside the European Union into Denmark and (ii) a cross-country design comparing non-EU immigration in Denmark to non-EU immigration in the other Nordic countries.[6] It is worth noting that cash benefits are higher in Denmark than in the other Nordic countries *even under the scheme rules*. This difference is partly offset by housing benefits, which are higher in the other countries than in Denmark. Still, even accounting for housing support, welfare benefits for Danish scheme immigrants are similar to the benefit levels of other Nordic countries and higher than the benefit levels outside the Nordics. This does not invalidate the experiment by making Denmark a strictly preferred destination. With idiosyncratic variation in nonwelfare preferences for location, there will be immigrants close to the indifference margin between Denmark and alternative destination countries. It is these marginal immigrants that may respond to the scheme by changing their destination country.

Aside from immigrant benefit reform, there have been many changes to immigration regulation during the period we study. As documented in Table A.I, most of these changes have tightened the rules for non-EU immigrants seeking asylum-based or family-based residence, posing a threat to our empirical approach. This concern is strongest for the 2002 reform, where several regulatory changes were implemented at the same time. The most important of these changes was arguably the "24-year rule," according to which, in order to obtain marriage-based residency, both spouses must be at least 24 years of age. To avoid any confounding effects of the 24-year rule, our main specification restricts the sample to immigrants of at least 30 years

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[6] To avoid confounding effects of the EU enlargements in the post-reform period, coming from "inside the European Union" is defined based on the member countries in the pre-scheme year, 2001.

of age.[7] The other regulatory changes in 2002 cannot be directly controlled for, but they are likely to have had a more limited impact. Ultimately, the strength of our empirical design relies on the presence of three separate welfare reforms, where the last two reform episodes are much less affected by regulation. Specifically, the 2012 repeal of the immigrant welfare scheme (which we find almost exactly reverses the effect of the 2002 introduction of the scheme) was hardly confounded by regulatory changes.[8]

B. *Data*

The analysis is based on administrative data from Denmark covering the full population from 1980 to 2017 (see Statistics Denmark 1980–2017). We combine several registers, linked at the individual level, to get information about immigrant status, country of origin, type of residence permit, and demographics. We are also able to link family members, which is important for correctly calculating welfare benefits. For the cross-country strategy in which we compare Denmark to other Nordic countries, we combine data from the national statistics bureaus of each country with the OECD's International Migration Database over the period 1991–2017.[9]

C. *Migration Patterns*

Denmark has seen a strong upward trend in the number of immigrants over the last four decades. The share of first-generation immigrants in the population has increased from 3 percent in 1980 to over 10 percent in 2017.[10] As a result, Denmark now has a foreign-born population share almost as high as the United States (13.5 percent) (OECD 2019). Immigrants from non-EU countries—those targeted by the welfare scheme—constitute the largest group: they accounted for 67 percent of all first-generation immigrants in 2001, the year before the scheme was introduced.

Figure 1 shows migration flows from outside the European Union since 1980, in total and from specific countries. Panel A shows that—apart from spikes in 1995 and 2015—the net flow of non-EU immigration has averaged about 8,000 people per year. A large part of the aggregate flow comes from a relatively limited set of countries, illustrated in panel A by the series for the eight main sending countries. Panel B considers each of these eight countries separately. The most striking feature of the graph is the presence of sharp spikes in 1995 and 2015, which reflect immigration from Bosnia-Herzegovina and Syria, respectively. The Bosnia spike in 1995 is driven by the Bosnian War of 1992–1995 combined with a special Danish law that granted Bosnian refugees residence in Denmark. Since this supply shock occurred well before the first welfare scheme reform, it does not pose a threat to identification.

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[7]We do not cut the sample exactly at age 24 due to the possibility of intertemporal substitution around the age cutoff, i.e., spouses below the age of 24 who postpone moving to Denmark until they have turned 24. We will investigate the robustness of our results to different age cutoffs.

[8]Marriage-based (but not asylum-based) immigration rules were changed in 2011, but the changes were rolled back in 2012, thus being in effect for less than a year. See Table A.I.

[9]Specifically, we use data from Statistics Finland (1990–2019), Statistics Norway (1970–2020), Statistics Sweden (2000–2019a), Statistics Sweden (2000–2019b), and OECD (1990–2017).

[10]See Figure A.III in the online Appendix.

Panel A. Immigration from outside the European Union
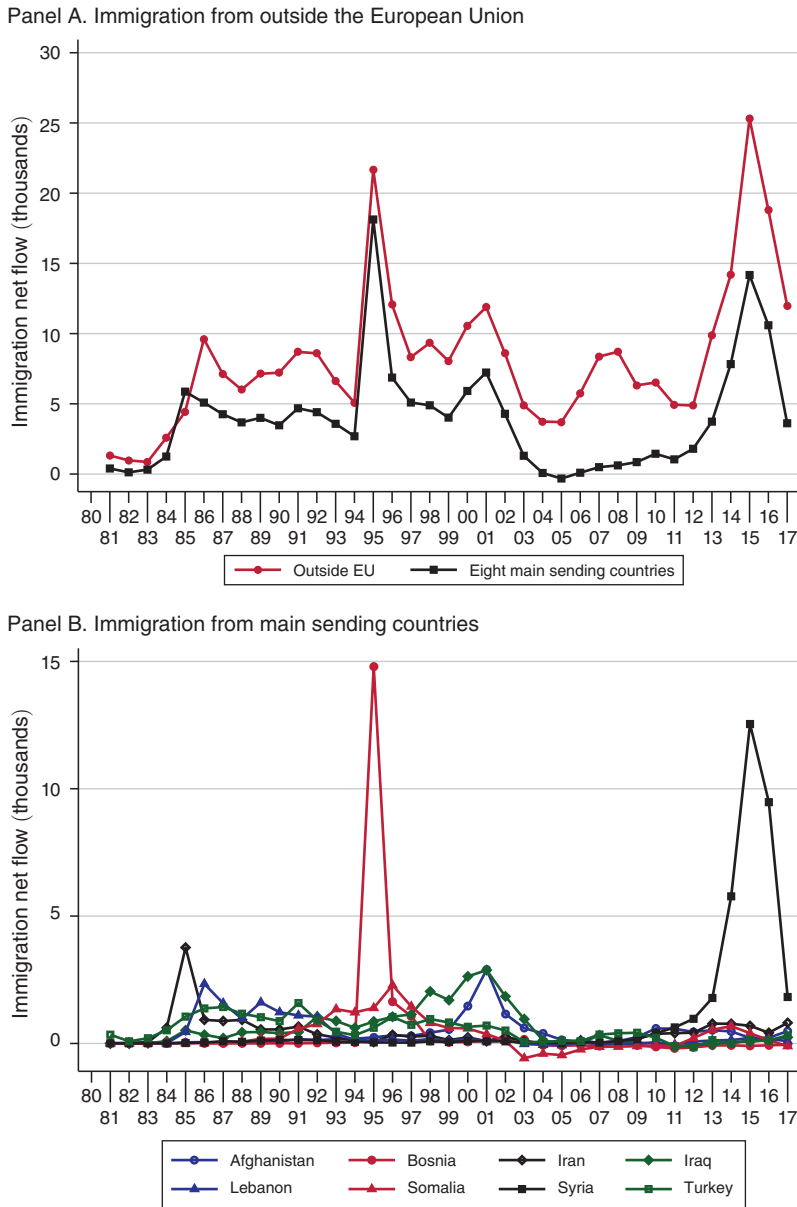

Panel B. Immigration from main sending countries

Figure 1. Migration Flows 1980–2017

Notes: Panel A shows the net flow of immigrants in Denmark from all countries outside the European Union/EFTA as well as from the eight main sending countries outside the European Union/EFTA. The main sending countries are defined as those with the highest average annual net flow of immigrants over the years 1980–2017. Panel B shows the net flow of immigrants from each of the eight main sending countries separately. The annual net flow of immigrants is measured as the year-to-year change in the stock of immigrants.

It does create a large spike in the pre-trend for the treatment group, however, and to avoid this we drop immigrants from Bosnia-Herzegovina in all years. The Syria spike around 2015 is driven by the Syrian civil war. This supply shock does pose a threat to our strategy, because it coincides with the reintroduction of the welfare

scheme in 2015. While some of this spike may be interpreted as a confounding supply shock that creates excess migration to Denmark, some of it may also reflect that, conditional on leaving Syria, the choice between Denmark and alternative countries reflects the Danish welfare system. It is not a priori clear if any bias from the Syrian refugee crisis will be upward or downward since the excess migration happens on both sides of the scheme reintroduction. We do two things to address identification concerns related to the Syrian supply shock: (i) we run the estimations on a sample without Syrian migrants, and (ii) we carry out a cross-country analysis in which we compare Denmark to other Nordic countries, which were also affected by the Syrian supply shock but did not implement any welfare reforms. Our results are robust to both of these checks.

II. Is There a Welfare Magnet Effect?

A. *Within-Country Strategy*

Figure 2 presents our main results graphically. Panel A shows the net immigration flow from outside the European Union between 1980 and 2017, along with a linear trend estimated using pre-scheme data between 1980 and 2001.[11] The time series evidence is striking. Immigration follows a linear trend in the two decades prior to the welfare scheme, diverges sharply from the trend after the introduction of the scheme in 2002, catches up with the trend following the repeal of the scheme in 2012, and diverges once more when the scheme is reintroduced in 2015. When the scheme is first introduced, immigration falls for four years before reaching a steady state relative to the linear trend. When the scheme is repealed, it takes exactly four years for immigration to catch back up with the trend. The sharp changes around each of the three reforms and the great degree of symmetry across reforms provide strongly suggestive evidence of a welfare magnet channel.

Of course, the time series evidence could be biased by nonwelfare determinants of migration that change over time. We therefore exploit that the scheme rules apply only to non-EU immigrants, which allows us to compare treated and untreated immigrants in a DiD framework. The results are presented in panel B, in which we compare net immigration flows from outside the European Union (treatments) and inside the European Union (controls) over time. Because the two groups trend differently in the raw data, the plotted series have been de-trended by subtracting a linear trend estimated on pre-scheme data. That is, the non-EU immigration series represent the residuals between the actual flow and the predicted flow shown in panel A, and likewise for the EU immigration series.

The patterns in panel B are compelling and consistent with the time series evidence in panel A. The treatment and control series evolve in parallel for a long period prior to the scheme, diverge sharply after the introduction of the scheme, converge when the scheme is repealed, and diverge once more when it is reintroduced. Again, there is a striking degree of symmetry across the different reforms: it takes four years for the full effect of the scheme introduction to materialize, and it takes

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[11] Net immigration flow is measured by the year-to-year changes in the stock of first-generation immigrants residing in Denmark according to the Central Person Register.
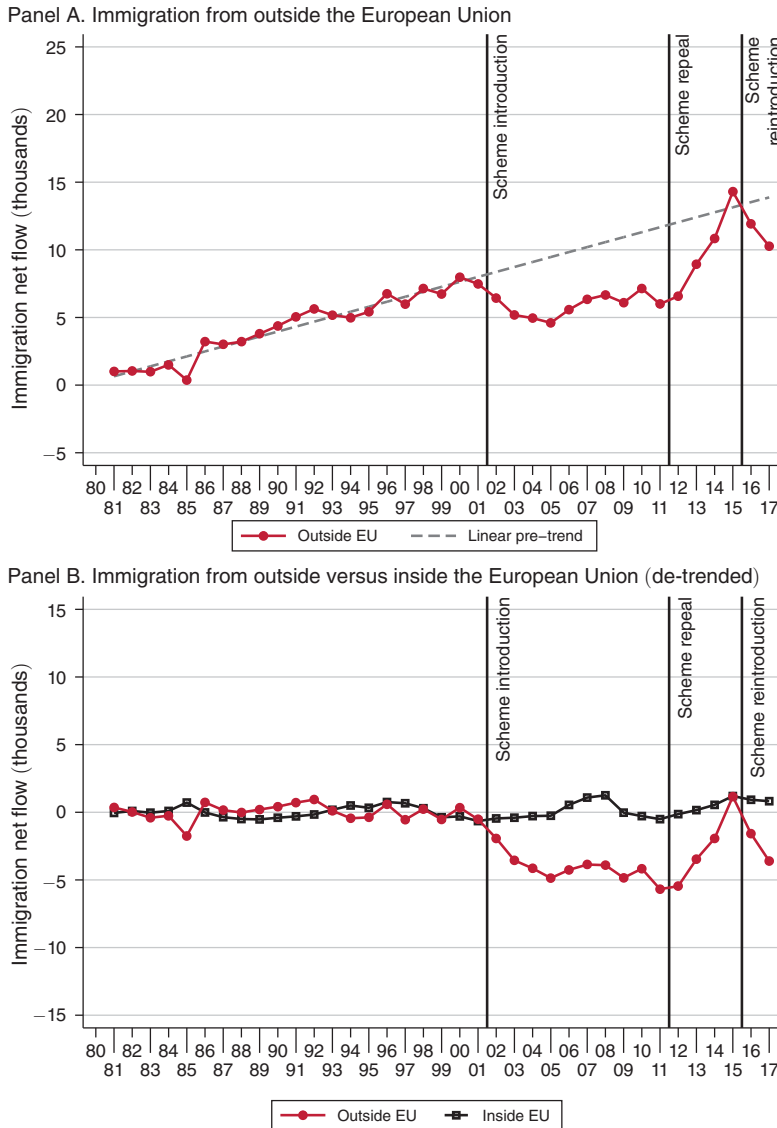
Panel A. Immigration from outside the European Union

Panel B. Immigration from outside versus inside the European Union (de-trended)

FIGURE 2. MIGRATION RESPONSES TO THE IMMIGRANT WELFARE SCHEME

Notes: Panel A shows the net flow of immigrants in Denmark from outside the European Union/EFTA over the period 1980–2017 and a linear trend estimated on the pre-scheme data period 1980–2001. Panel B compares the net flow of immigrants from outside the European Union/EFTA (treatment group) to the net flow of immigrants from inside the European Union/EFTA (control group). Each series has been de-trended using a linear, group-specific trend estimated on pre-scheme data. We use EU membership in the pre-scheme year (2001) to define the control group. The annual net flow of immigrants is measured as the year-to-year change in the stock of immigrants. The sample is restricted to immigrants who are at least 30 years of age, and immigrants from Bosnia are dropped throughout.

exactly four years for the scheme repeal to reverse that effect. Moreover, when the scheme is reintroduced in 2015, the impact during the two years for which we have data is similar to the two-year impacts around the other reforms. The graph suggests that, at full impact, the welfare scheme reduced non-EU immigration by about 5,000

people per year, or 4.2 percent of the pre-scheme stock of non-EU immigrants in Denmark.

Although the DiD evidence looks compelling, identification could still be compromised by the presence of time-varying immigration factors that vary by country of origin (European Union versus non-European Union). As discussed above and documented in online Appendix Table A.I, this concern is particularly relevant for the 2002 scheme introduction as it coincided with other changes to immigration regulation. One of the main changes—the 24-year rule—is not a confounder here, because the figure restricts attention to immigrants aged 30 and above. But the other changes are not controlled for in the graph. The symmetry of the DiD effects across reforms is particularly informative in this light. The later scheme reforms did not coincide with such regulatory changes—the 2012 repeal in particular is hardly confounded by regulation—and still produced similar effects.

To investigate the threat from time-varying unobservables more broadly, we exploit that our data include information about residence permits. This allows us to check if the changes around each reform are driven by the types of immigrants who are treated by the welfare scheme. There are four main types of residence permits: asylum, family, work, and study. Those coming on asylum and family permits are potentially eligible for welfare benefits and therefore treated by the scheme, while those coming for work or study are ineligible for welfare benefits and therefore untreated. Figure 3 shows immigration flows for the different types of residence permits between 1997–2017.[12] The figure shows that the effects on non-EU immigration are driven entirely by those on asylum and family permits; if anything, immigration by those on work and study permits accelerates following the introduction of the scheme. Furthermore, the figure shows that asylum-based immigration responds more strongly than family-based immigration. This is natural: most newly arrived refugees have very limited job opportunities and therefore no alternative to welfare benefits, making the scheme treatment particularly severe for them.[13]

One type of confounding shock remains a concern, however: exogenous supply shocks driven by war, political unrest, and the like. In practice such shocks affect only asylum-based, non-EU immigration. If these shocks coincide with the scheme reforms, changes in non-EU immigration by those coming on asylum (relative to family, work, or study permits) may reflect nonwelfare factors in the sending countries. As described previously, the main supply shock during the post-scheme period is the Syrian refugee crisis around 2014–2016. Indeed, Figure 3 shows a spike in asylum-based permits relative to other permits during these years. While it is not a priori clear that this variation creates bias—in part because the Syrian refugee spike happens on both sides of the scheme reintroduction in 2015—it does represent a cause for concern. In the next section we investigate this issue by comparing Denmark to similar countries that were also affected by the Syrian supply shock. In this section, we consider a more basic robustness check: dropping Syrian immigrants from the sample throughout. We come back to this below.

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[12] The figure restricts attention to the period 1997–2017, because the residence permit data is available only from 1997 onward.

[13] When the scheme was introduced in 2002, the employment rate for refugees was about 10 percent after 1 year in Denmark, 19 percent after 2 years, and 32 percent after 3–5 years (Andersen, Dustmann, and Landersø 2019). As a result, welfare benefits are the primary source of income for almost all refugees in the first years after arrival.
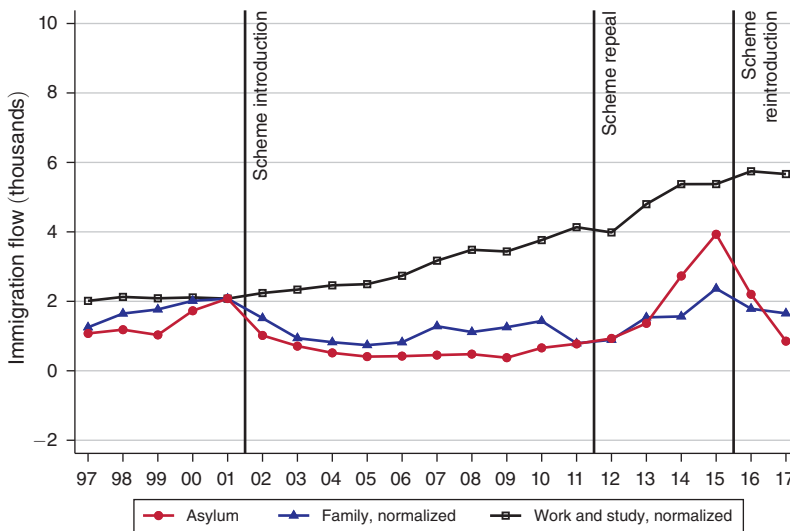
FIGURE 3. IMMIGRATION INFLOWS BY TYPE OF RESIDENCE PERMIT

Notes: The graph shows the inflow of immigrants in Denmark from outside the European Union/EFTA by type of residence permit (asylum, family, and work/study) over the period 1997–2017. Each series has been de-trended using a linear, group-specific trend estimated on pre-scheme data (1997–2001). The sample is restricted to immigrants who are at least 30 years of age, and immigrants from Bosnia are dropped throughout.

Another possible concern is that our measure of immigration is based on residency, which lags behind residence *applications*. It is the decision to apply for Danish residence that should respond to benefit reform, and if the timing of applications is significantly different from the timing of permits, the DiD evidence is less compelling than it seems. We are able to investigate this issue by using that, for asylum seekers, our data includes information about both applications and permits. We present evidence on asylum applications in the online Appendix, showing that the empirical patterns for applications and permits are broadly consistent.[14] This lends additional support to our empirical strategy.

To formally estimate the effect of welfare benefits on immigration flows, we collapse the micro data to the level of welfare benefit groups. These groups are defined as those relevant for benefit eligibility (see Figure A.II): married and unmarried individuals with different numbers of children (0, 1, 2+) coming from outside or inside the European Union, a total of 12 different benefit groups. We then run the following DiD regression specification:

$$(1) \qquad\qquad Y_{gt} = \beta B_{gt} + \gamma_g + \eta_t + \nu_{gt},$$

[14] See Figures A.IV-A.V discussed in Section B of the online Appendix. We also note that asylum applications are more volatile than residencies, especially around the Syrian shock, which reflects that a sizable fraction of applications are rejected. This is an important reason for focusing on residencies granted as the main outcome of interest: we are ultimately interested in the effect of benefits on actual immigration, which is better measured by residencies than by applications.

where Y_{gt} is the net immigration flow for group g in year t, scaled by the pre-scheme immigrant stock for the group; B_{gt} is the maximum monthly benefit for a given group and year; γ_g is a group fixed effect; and η_t is a year fixed effect. To make interpretation easier, we convert the benefit amounts into US dollars (measured in hundreds), so that the DiD coefficient β captures the impact on immigration from raising monthly benefits by \$100.[15] The estimation of β in equation (1) is based on richer variation than in the graphical analysis, because it exploits that the benefit changes for non-EU immigrants (relative to EU immigrants) differ across families depending on marital status and number of children. We estimate (1) by weighted least squares using the pre-scheme stock of each group as weights.

As discussed above, immigration flows trend upward over time, and these nonwelfare trends differ across treatment and control groups. The DiD graphs were therefore adjusted for linear, group-specific pre-trends. Consistent with this, the regression analysis is also based on de-trended data. Specifically, the outcome variable Y_{gt} is residualized by a linear, group-specific trend estimated on the pre-scheme data (1980–2001). To obtain consistent standard errors from this two-step procedure, we use GMM to jointly estimate the pre-trends and equation (1) and calculate the associated standard errors.

The results are presented in Table 1. Panel A shows estimates of the effect on net immigration flow (inflow minus outflow), while panel B focuses on the inflow alone and breaks the effect down by type of residence permit. The different columns show different sample restrictions in terms of age and whether Syrian immigrants are included or not. Our baseline specification in column 1 considers immigrants above age 30 and includes Syrian immigrants, corresponding to the analysis in Figure 2. In this specification, we find that the immigration net flow increases by 0.44 percent of the stock for each \$100 increase in benefits. The immigrant welfare scheme reduced benefits by up to \$800 (for married couples with children), implying a total effect on immigration equal to 3.5 percent of the stock.[16] In the table, we convert this effect into an elasticity of the immigration net flow with respect to welfare benefits.[17] This elasticity is equal to 1.29. Reading across the different columns, we see that the estimates are very robust to alternative age cutoffs and to dropping Syrian immigrants. The migration elasticity varies between 1.29 and 1.53 across the different specifications.

Turning to the effects on gross inflow and its composition in panel B, the following insights are worth highlighting. First, the total effect on inflow equals 0.4 percent of the stock per \$100 of benefits in the baseline specification. This represents 91 percent of the net immigration effect shown in panel A, that is, almost all of the effect is driven by reduced inflow rather than by increased outflow. Second, the inflow

[15] We use the Danish krone-US dollar exchange rate of December 31, 2018. In the scheme reform years, we use average benefits over the year based on the month in which the reform was implemented.

[16] This is roughly consistent with the results in panel B of Figure 2. There we see a drop in immigration of about 5,000 people (at full impact), corresponding to 4.2 percent of the 2001 stock of non-EU immigrants.

[17] We define the elasticity of immigration net flow with respect to welfare benefits as follows:

$$\varepsilon = \hat{\beta} \cdot \frac{E[B_{g,2001}]}{E[Y_{g,2001}]},$$

where $E[\cdot]$ denotes weighted averages across treated benefit groups, and the elasticity has been defined relative to the pre-scheme baseline in 2001. Note that the elasticity with respect to welfare benefits corresponds to an elasticity with respect to total net income if those treated by the welfare scheme (newly arrived immigrants on asylum or family permits) have no other earnings opportunities in Denmark in the beginning.

Table 1—Migration Responses to the Immigrant Welfare Scheme

	Age ≥ 30		Age ≥ 24		Age ≥ 18	
	Syria (1)	No Syria (2)	Syria (3)	No Syria (4)	Syria (5)	No Syria (6)
Panel A. Effects on net flow (fraction of 2001 stock)						
Effect of benefits ($100)	0.0044	0.0048	0.0046	0.0050	0.0044	0.0049
	(0.0008)	(0.0008)	(0.0009)	(0.0009)	(0.0010)	(0.0010)
Elasticity of migration flow	1.29	1.41	1.40	1.53	1.35	1.51
with respect to benefits	(0.23)	(0.24)	(0.27)	(0.28)	(0.30)	(0.30)
Observations	444	444	444	444	444	444
Panel B. Effects on gross inflow and its composition (fraction of 2001 stock)						
Effect of benefits ($100):						
Total	0.0040	0.0040	0.0042	0.0042	0.0045	0.0047
	(0.0008)	(0.0009)	(0.0012)	(0.0012)	(0.0013)	(0.0014)
Asylum	0.0029	0.0028	0.0031	0.0032	0.0032	0.0033
	(0.0007)	(0.0008)	(0.0011)	(0.0011)	(0.0012)	(0.0013)
Family	0.0027	0.0027	0.0034	0.0033	0.0038	0.0038
	(0.0007)	(0.0007)	(0.0011)	(0.0011)	(0.0012)	(0.0013)
Work and study	0.0007	0.0007	0.0007	0.0007	0.0009	0.0009
	(0.0005)	(0.0005)	(0.0008)	(0.0008)	(0.0009)	(0.0009)
Observations	252	252	252	252	252	252

Notes: Panel A shows the effect of welfare benefits (in hundreds of US dollars) on immigration net flow as a fraction of the pre-scheme stock and the implied elasticity of immigration net flow with respect to benefits. Panel B shows the gross inflow of immigrants by type of residence permit as a fraction of the pre-scheme stock. The estimates of benefit effects correspond to $\hat{\beta}$ obtained from (1), while the estimates of elasticities correspond to ε obtained from (2). We estimate (1) by weighted least squares using the pre-scheme stock of each group as weights. The specification is run on de-trended outcome variables, that is, the residual between the raw immigration outcome and a linear, group-specific trend estimated on pre-scheme data. The standard errors (in parentheses) are obtained by treating the pre-trend equation and equation (1) as a set of joint moment conditions and calculating the associated GMM standard errors. Across columns, we consider different sample restrictions by varying the age threshold and whether Syrian immigrants are included or not. Immigrants from Bosnia are dropped from the sample in all specifications.

effect is driven entirely by asylum- and family-related migration, with somewhat larger effects on the former. The effect on work- and study-related migration is a precisely estimated zero across all specifications. As discussed, this is critical for the credibility of our results since immigrants coming on work and study permits are untreated by welfare reform. Third, the results are again robust to changing the age cutoff and dropping Syrian immigrants.

To summarize, the within-country DiD analysis reveals large and strongly significant welfare magnet effects. The effects of the scheme reforms are sharp and driven by changes in the inflow of immigrants coming on asylum or family permits, precisely those treated by the scheme. The natural interpretation of these effects is that they represent choice of destination country, conditional on migrating. Because new arrivals in Denmark, particularly refugees, have extremely limited employment opportunities for a period of time, it is natural that they are responsive to the drastic welfare cuts implied by the Danish scheme.

B. *Cross-Country Strategy*

As discussed above, a potential identification concern is the presence of push factors such as war, famine, and economic crises, which increase immigration and

could be correlated in time with the Danish policy experiments. The within-country approach relying on changes in non-EU (relative to EU) immigration driven by asylum and family permits (relative to work and study permits) is not immune to this concern, because the confounding push factors apply specifically to non-EU, asylum-based immigration. The main supply shock during the post-scheme period is the Syrian civil war, which caused a massive influx of refugees to Europe between 2014–2016, that is, around the time of the reintroduction of the Danish welfare scheme. While we have shown that our results are robust to dropping Syrian immigrants from the sample, other smaller supply shocks may still create bias. In this section, we consider a more compelling way of assessing the threat from such supply shocks: a cross-country strategy comparing Denmark to other countries that experienced the same supply shocks but did not change welfare benefits.

For this analysis, we draw on additional data from three other Nordic countries: Norway, Sweden, and Finland. We focus on the Nordic countries because they are culturally and economically similar and therefore more likely to be affected by confounding supply shocks in the same way. We use the synthetic control approach developed by Abadie, Diamond, and Hainmueller (2010) to construct a "synthetic Denmark," a weighted average of the other countries. To construct the weights, we match on annual migration flows from outside the European Union during the pre-scheme period, 1991–2001. More precisely, because migration flows differ in absolute levels due to differences in country size, we match on non-EU migration flows normalized by the pre-scheme stock of non-EU migrants in each country.

Our results are presented in Figure 4. Panel A of the figure shows raw immigration flows from outside the European Union in each of the four countries.[18] We see that the migration flows evolve similarly in the four countries throughout the pre-scheme period, 1991–2001, lending support to the parallel trends assumption. Starting in 2002, immediately following the introduction of the Danish welfare scheme, migration to Denmark begins to decline while the three other countries continue their upward trend. Migration flows to Denmark settle at a much lower level until the repeal of the Danish welfare scheme starting in 2012, after which Denmark rapidly catches up with the other three countries. After the reintroduction of the welfare scheme in 2015, we again see flows to Denmark declining compared to its Nordic neighbors.

Panel B of the figure presents findings from the synthetic control approach. The immigration flows in Denmark and the synthetic control country match closely during the pre-scheme years, and the patterns around the scheme reforms are consistent with what we saw in the within-country analysis: relative to the control country, migration to Denmark declines after the initial reform in 2002, catches up after the repeal in 2012, and again drops following the reintroduction in 2015. This evidence alleviates concerns that the Syrian refugee crisis biases our estimates: the other Nordic countries are exposed to the same supply shock but experience different timing in their immigration spike. Immigration to Denmark spikes in 2014–2015—before the

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[18] We note two limitations of the cross-country data. First, because we are unable to restrict migration by age for the other Nordic countries, the analysis in this section includes migrants at all ages. As a result, the patterns around the 2002 reform may be influenced by the 24-year rule. Second, to eliminate the pre-scheme Bosnia shock in a consistent way across countries, we have to exclude migrants from all countries of the former Yugoslavia.
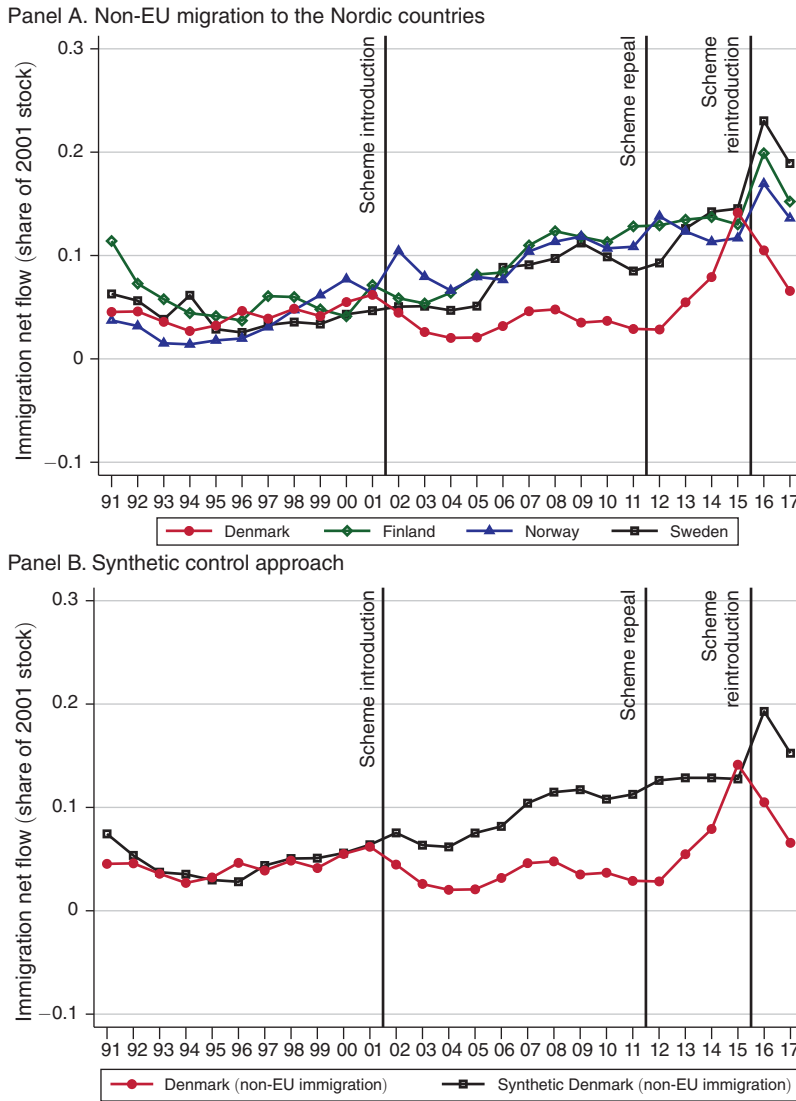
Panel A. Non-EU migration to the Nordic countries

Panel B. Synthetic control approach

Figure 4. Welfare Magnet Effects: Cross-Country Evidence

Notes: Panel A shows the annual net flow of immigrants from outside the European Union/EFTA to each of the four Nordic countries, divided by the country's 2001 stock of non-EU/EFTA immigrants. Panel B retains the same data series for Denmark as panel A but combines the three other Nordic countries into one synthetic control country, labeled "synthetic Denmark." This series is constructed as a weighted average of Finland, Norway, and Sweden to minimize the sum of squared errors in the pre-scheme (1991–2001) immigration flows between Denmark and synthetic Denmark. This yields the following weights: Finland 0.424, Norway 0.397, Sweden 0.179. Unlike the within-country analysis, no age restriction has been placed on migrants. To correct for the pre-scheme Bosnia shock, immigrants from the former Yugoslavia are dropped throughout.

reintroduction of lower benefits—while it remains flat in synthetic Denmark during these years and only starts spiking in 2016. In fact, panel A shows that the 2016 spike happens in every Nordic country except Denmark. This is consistent with a model in which refugees choose where to seek asylum based in part on benefit levels

and where the reduction in Danish welfare benefits causes some Syrian migrants to forgo Denmark in favor of other destination countries.

The findings in Figure 4, taken at face value, imply that Denmark's welfare scheme had a considerably larger effect on immigration than estimated from the within-country approach. Note, however, that since we are unable to restrict the sample by age in the cross-country analysis, the response also captures any effect of the 24-year rule around the initial 2002 reform. Additionally, if the other Nordic countries serve as substitutes for migrants who would have chosen Denmark absent the welfare scheme, the synthetic control series will overstate the true counterfactual level of migration to Denmark. In fact, if migrants' only alternatives to Denmark were the other Nordic countries, the effect in Figure 4 would be roughly doubled relative to the true causal effect: any migrant who avoids Denmark due to welfare reform would instead appear in the control group and thus be counted twice. Of course, migrants do have options outside the Nordics, so we would expect this source of bias to be less dramatic. Nevertheless, while the cross-country analysis provides clear evidence of a welfare magnet effect, and seems to rule out that our within-country results are driven by confounding supply shocks coinciding with the Danish reforms, we caution against attaching too much importance to the specific magnitudes found here. Our estimates from the previous section are better measures of the magnitude of the welfare magnet effect.

III. Conclusion

The possibility of welfare magnet effects has been debated for a long time, but there is virtually no causal evidence on the question. In this paper, we provide quasi-experimental evidence on such effects using a Danish welfare scheme that cut benefits by up to 50 percent for immigrants from outside the European Union. Leveraging three scheme reforms and DiD designs, we find large and sharp effects of welfare benefits on immigration. The decision to migrate is influenced by many nonwelfare factors, but our evidence suggests that, conditional on moving, the generosity of the welfare system matters for the choice of destination country.

Does our evidence imply that the immigrant welfare scheme is an optimal policy? The answer to this question depends partly on the net fiscal impact of immigration (which we do not estimate) and partly on whether we take a local or global perspective. If the net fiscal impact of low-income immigration is negative, local governments have an incentive to deter immigration by cutting means-tested welfare benefits. Assuming that governments care only about the welfare of native residents, welfare schemes like the Danish one may be optimal for individual countries. However, the allocation of immigrants across countries is essentially a zero-sum game, implying that such schemes are not globally optimal. The migrants who avoid Denmark due to the welfare scheme end up in other destination countries, imposing fiscal externalities on them. In the extreme, this may create a race to the bottom in the setting of welfare benefits, similar to the race to the bottom discussed in the context of the tax setting (Kleven et al. 2020). Avoiding such effects may require international policy coordination. In any case, while the welfare magnet effects we find may be consistent with the political motivations behind the scheme, they cannot be taken as evidence that the scheme is socially optimal.

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