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THE WELFARE MAGNET HYPOTHESIS:  
EVIDENCE FROM AN IMMIGRANT WELFARE SCHEME IN DENMARK

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The Welfare Magnet Hypothesis: Evidence From an Immigrant Welfare Scheme in Denmark  
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**ABSTRACT**

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%, with no changes for natives or EU immigrants. The policy was later repealed and re-introduced. Based on a quasi-experimental research design, we find sizeable 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.

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# 1 Introduction

Do generous welfare benefits act as magnets for low-skilled immigrants? This is a classic debate among economists and policy makers, 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 U.S. tend to be clustered in high-benefit states, while [Boeri \(2010\)](#) shows that low-skilled immigrants in the EU 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 casual 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% for immigrants from outside the EU.<sup>1</sup> The welfare scheme was controversial and widely debated. It was repealed in 2012 following the election of a center-left government, and then reinstated 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 reinstatement of the scheme in 2015, the government ran an ad campaign in Lebanese newspapers informing refugees of the reduced benefits.<sup>2</sup> These ads, illustrated in [Figure A.I](#), highlight the 50% 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-differences strategies. The first strategy is a *within-country* approach based on comparing im-

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<sup>1</sup>To be precise, the scheme applied to immigrants from outside the EU *and* the four member countries of the European Free Trade Association (EFTA), namely Iceland, Liechtenstein, Norway, and Switzerland.

<sup>2</sup>Lebanon hosts a number of large refugee camps and provides a common transit for Middle Eastern refugees headed to Europe.

migration flows to Denmark from outside the EU (treatments) and from inside the EU (controls) around the three reforms.<sup>3</sup> The findings are striking: The immigration flows of the two groups evolve in parallel during the twenty 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 (EU vs non-EU) and by immigrant type within region (asylum/family vs 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 is affected by the Danish welfare system. Second, the presence of sizeable 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.<sup>4</sup> Specifically, to the extent that the net fiscal impact of low-income

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<sup>3</sup>To avoid confounding effects of the EU enlargements in the post-reform period, coming from “inside the EU” is defined based on the member countries in the pre-scheme year, 2001.

<sup>4</sup>In the U.S., the welfare reform act of 1996 (PRWORA) denied non-citizens 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.

immigration is negative, it may be individually optimal for countries to lower immigrant welfare benefits.<sup>5</sup> 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, Landaís, Muñoz, and Stantcheva 2019](#)). 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 U.S., 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 sizeable. The welfare scheme that we use for identification has been studied in two existing papers ([Rosholm and Vejlin 2010](#); [Andersen, Dustmann, and Landerød 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 2 describes the policy experiment and data, section 3 presents our results on welfare magnet effects, while section 4 concludes.

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<sup>5</sup>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).

## 2 Policy Experiment and Data

### 2.1 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 EU 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 EU”. 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% 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 7 years. The scheme was repealed in January 2012 and reintroduced in September 2015. Figure A.II in the appendix illustrates the policy experiment, comparing cash benefits for scheme immigrants to cash benefits for non-scheme 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 difference-in-differences designs: (i) a within-country design comparing immigration from outside the EU to immigration from inside the EU into Denmark, and (ii) a cross-country design comparing non-EU immigration in Denmark to non-EU immigration in the other Nordic countries.<sup>6</sup> 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 non-welfare 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

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<sup>6</sup>To avoid confounding effects of the EU enlargements in the post-reform period, coming from “inside the EU” is defined based on the member countries in the pre-scheme year, 2001.

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 of age.<sup>7</sup> 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.<sup>8</sup>

## 2.2 Data

The analysis is based on administrative data from Denmark covering the full population from 1980 to 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 OECD’s International Migration Database over the period 1991-2017.

## 2.3 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% in 1980 to over 10% in 2017.<sup>9</sup> As a result, Denmark now has a foreign-born population share almost as high as the U.S. (13.5%).<sup>10</sup> Immigrants from non-EU countries — those targeted by the welfare scheme — constitute the largest group: they accounted for 67% of all first-generation immigrants in 2001, the

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<sup>7</sup>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.

<sup>8</sup>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.

<sup>9</sup>See Figure A.III.

<sup>10</sup>OECD, International Migration Outlook 2019.

year before the scheme was introduced.

Figure 1 shows migration flows from outside the EU 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 1992-95 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. 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.

### 3 Is There a Welfare Magnet Effect?

#### 3.1 Within-Country Strategy

Figure 2 presents our main results graphically. Panel A shows the net immigration flow from outside the EU between 1980 and 2017, along with a linear trend estimated using pre-scheme data between 1980 and 2001.<sup>11</sup> The time series evidence is striking. Immigration follows a linear trend

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<sup>11</sup>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 (CPR).

in the two decades prior to the welfare scheme, diverges sharply from 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 non-welfare 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 difference-in-differences (DiD) framework. The results are presented in Panel B, in which we compare net immigration flows from outside the EU (treatments) and inside the EU (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 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 (EU vs non-EU). As discussed above and documented in 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.<sup>12</sup> 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.<sup>13</sup>

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 non-welfare factors in the sending countries. As described previously, the main supply shock during the post-scheme period is the Syrian refugee crisis around 2014-16. 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.

Another possible concern is that our measure of immigration is based on residency, which lags

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<sup>12</sup>The figure restricts attention to the period 1997-2017, because the residence permit data is available only from 1997 onwards.

<sup>13</sup>When the scheme was introduced in 2002, the employment rate for refugees was about 10% after one year in Denmark, 19% after two years, and 32% 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.

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.<sup>14</sup> 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 EU, a total of 12 different benefit groups. We then run the following DiD regression specification:

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

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,  $\gamma_g$  is a group fixed effect, and  $\eta_t$  is a year fixed effect. To make interpretation easier, we convert the benefit amounts into US dollars (measured in 100s), so that the DiD coefficient  $\beta$  captures the impact on immigration from raising monthly benefits by 100 dollars.<sup>15</sup> The estimation of  $\beta$  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 the 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 upwards over time, and these non-welfare 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

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<sup>14</sup>See Figures A.IV-A.V discussed in Section B of the appendix. We also note that asylum applications are more volatile than residencies, especially around the Syrian shock, which reflects that a sizeable 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.

<sup>15</sup>We use the DKK-USD 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.

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% 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% of the stock.<sup>16</sup> In the table, we convert this effect into an elasticity of the immigration net flow with respect to welfare benefits.<sup>17</sup> 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% of the stock per \$100 dollar of benefits in the baseline specification. This represents 91% of the net immigration effect shown in Panel A, i.e. almost all of the effect is driven by reduced inflow rather than by increased outflow. Second, the inflow 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 nat-

<sup>16</sup>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% of the 2001 stock of non-EU immigrants.

<sup>17</sup>We define the elasticity of immigration net flow with respect to welfare benefits as follows

$$\varepsilon = \hat{\beta} \cdot \frac{E[B_{g,2001}]}{E[Y_{g,2001}]}, \quad (2)$$

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.

ural 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.

### 3.2 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 2014-16, i.e. 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 EU 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 EU in each of the four countries.<sup>18</sup> We see that the migration flows evolve similarly

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<sup>18</sup>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.

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 a different timing in their immigration spike. Immigration to Denmark spikes in 2014-15 — before the 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.

## 4 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% for immigrants from outside the EU. Leveraging three scheme reforms and difference-in-differences designs, we find large and sharp effects of welfare benefits on immigration. The decision to migrate is influenced by many non-welfare 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 tax setting (Kleven, Landais, Muñoz, and Stantcheva 2019). 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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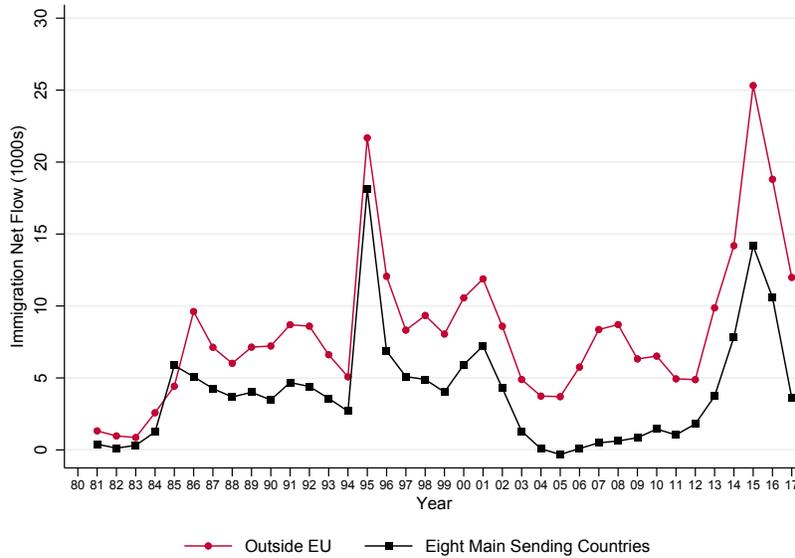
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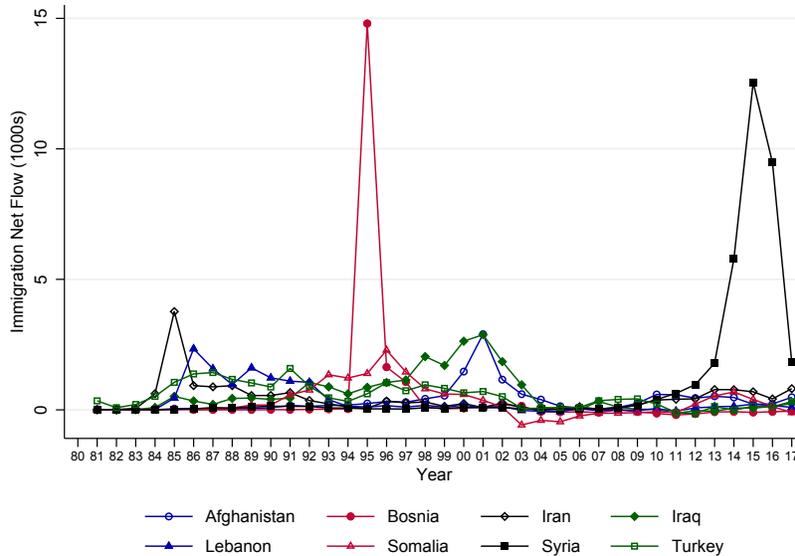
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Figure 1: Migration Flows 1980-2017

**A: Immigration from Outside EU**

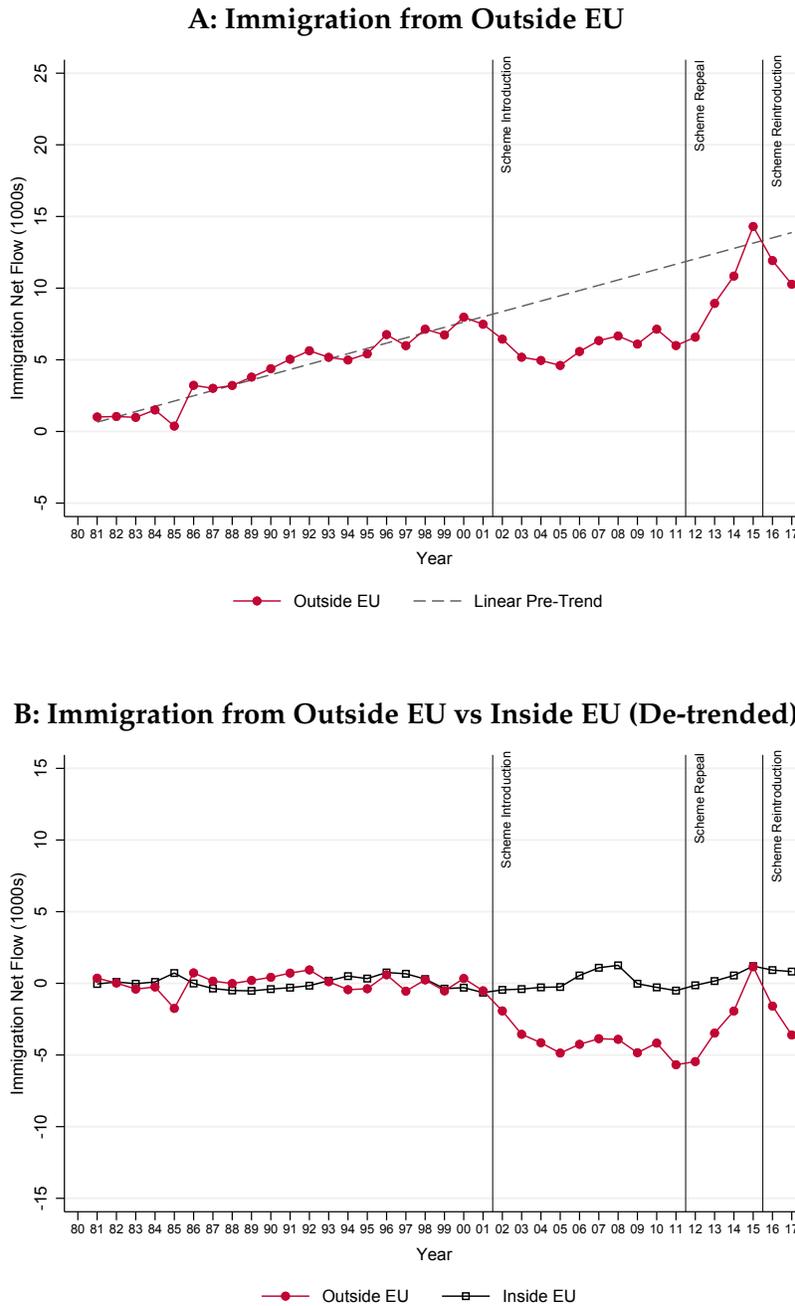


**B: Immigration from Main Sending Countries**



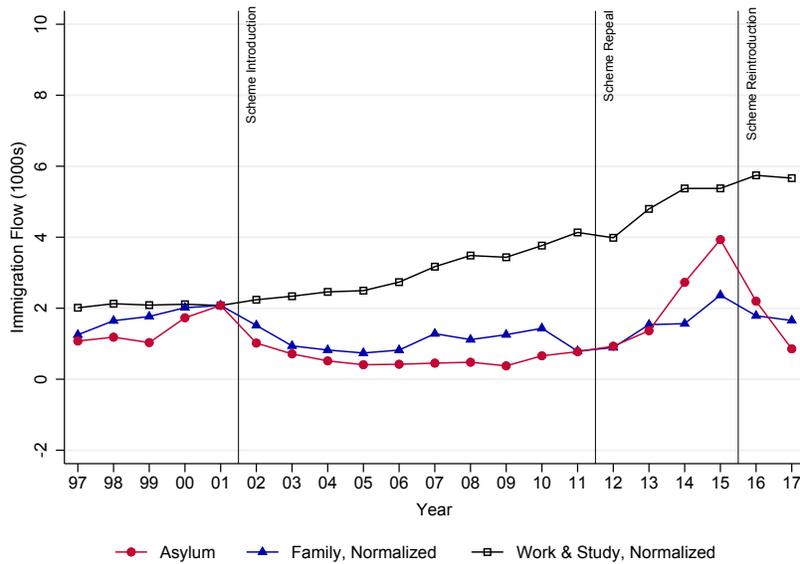
Notes: Panel A shows the net flow of immigrants in Denmark from all countries outside the EU/EFTA as well as from the eight main sending countries outside the EU/EFTA. The main sending countries are defined as those with the highest average annual net flow of immigrants over the years 1980-2017. Panel 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.

Figure 2: Migration Responses to the Immigrant Welfare Scheme



Notes: Panel A shows the net flow of immigrants in Denmark from outside the EU/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 EU/EFTA (treatment group) to the net flow of immigrants from inside EU/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.

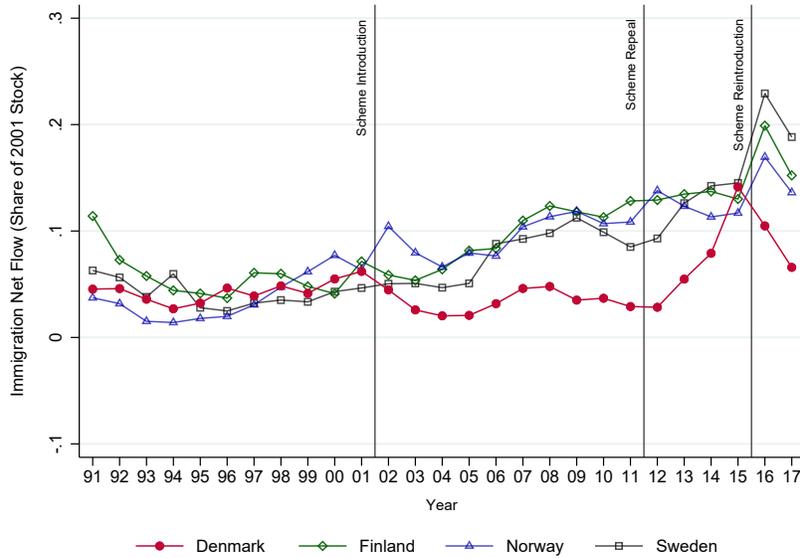
Figure 3: Immigration Inflows by Type of Residence Permit



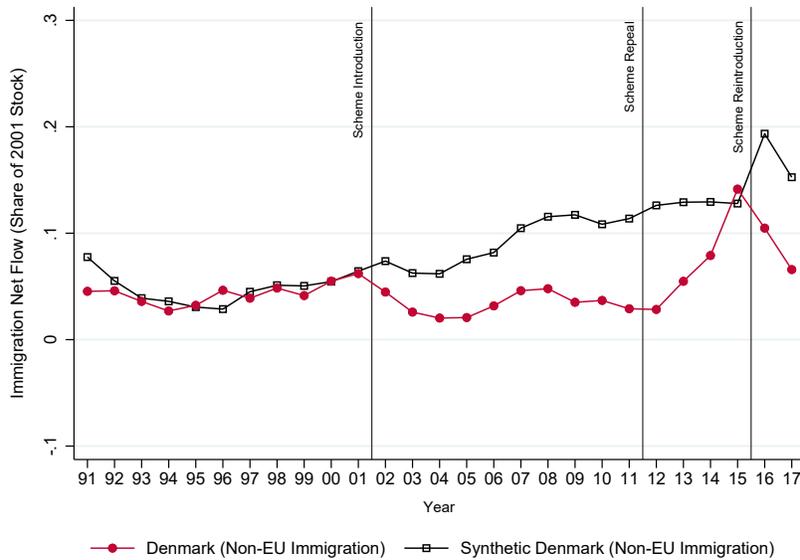
Notes: The graph shows the inflow of immigrants in Denmark from outside the EU/EFTA by type of residence permit (asylum, family, and work/study) over the period 1997-2017. Each series has been de-trended using a linear, group-specific trend estimated on pre-scheme data (1997-2001). The sample is restricted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

Figure 4: Welfare Magnet Effects: Cross-Country Evidence

**A: Non-EU Migration to the Nordic Countries**



**B: Synthetic Control Approach**



Notes: Panel A shows the annual net flow of immigrants from outside the EU/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 0.468, Norway 0.361, Sweden 0.171. 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.

Table 1: Migration Responses to the Immigrant Welfare Scheme

	Age $\geq 30$		Age $\geq 24$		Age $\geq 18$	
	Syria (1)	No Syria (2)	Syria (3)	No Syria (4)	Syria (5)	No Syria (6)
<b>Panel A: Effects on Net Flow (Fraction of 2001 Stock)</b>						
Effect of Benefits (USD 100)	0.0044*** (0.0008)	0.0048*** (0.0008)	0.0046*** (0.0009)	0.0050*** (0.0009)	0.0044*** (0.0010)	0.0049*** (0.0010)
Elasticity of Migration Flow w.r.t. Benefits	1.29 (0.23)	1.41 (0.24)	1.40 (0.27)	1.53 (0.28)	1.35 (0.30)	1.51 (0.30)
Observations	444	444	444	444	444	444
<b>Panel B: Effects on Gross Inflow and its Composition (Fraction of 2001 Stock)</b>						
Effect of Benefits (USD 100):						
Total	0.0040*** (0.0008)	0.0040*** (0.0009)	0.0042*** (0.0012)	0.0042*** (0.0012)	0.0045*** (0.0013)	0.0047*** (0.0014)
Asylum	0.0029*** (0.0007)	0.0028*** (0.0008)	0.0031*** (0.0011)	0.0032*** (0.0011)	0.0032*** (0.0012)	0.0033*** (0.0013)
Family	0.0027*** (0.0007)	0.0027*** (0.0007)	0.0034*** (0.0011)	0.0033*** (0.0011)	0.0038*** (0.0012)	0.0038*** (0.0013)
Work & Study	0.0007 (0.0005)	0.0007 (0.0005)	0.0007 (0.0008)	0.0007 (0.0008)	0.0009 (0.0009)	0.0009 (0.0009)
Observations	252	252	252	252	252	252

Notes: Panel A shows the effect of welfare benefits (in 100s of 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  $\varepsilon$  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, i.e. 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.

## **Online Appendix (Not for Publication)**

### **A Supplementary Figures and Tables**

Figure A.I: Ad Campaign in Lebanese Newspapers and Online, September 2015

7/9-2015

**The Danish immigration authorities are informing about changes of conditions regarding residence in Denmark being implemented by the new Danish government**

Denmark has decided to tighten the regulations concerning refugees in a number of areas.

The Danish Parliament has just passed a regulation to:

- Reduce the social benefits significantly. The social benefits for newly arrived refugees will be reduced by up to 50 percent.

The government will maintain and ensure that:

- Foreign nationals granted temporary protection in Denmark will not have the right to bring family members to Denmark during the first year.
- Foreign nationals can only be granted a permanent residence permit after 5 years at the earliest. Prior to this they risk having their residence permit revoked.
- In order to obtain a permanent residence permit in Denmark there are language requirements in terms of the ability to speak and understand the Danish language.
- When an application for asylum is regarded manifestly unfounded it is refused in accordance with a particularly expedited procedure.
- All rejected asylum seekers must be returned quickly from Denmark.
- There is a special return centre to ensure that rejected asylum seekers leave Denmark as quickly as possible.



Ministry of Immigration,  
Integration and Housing



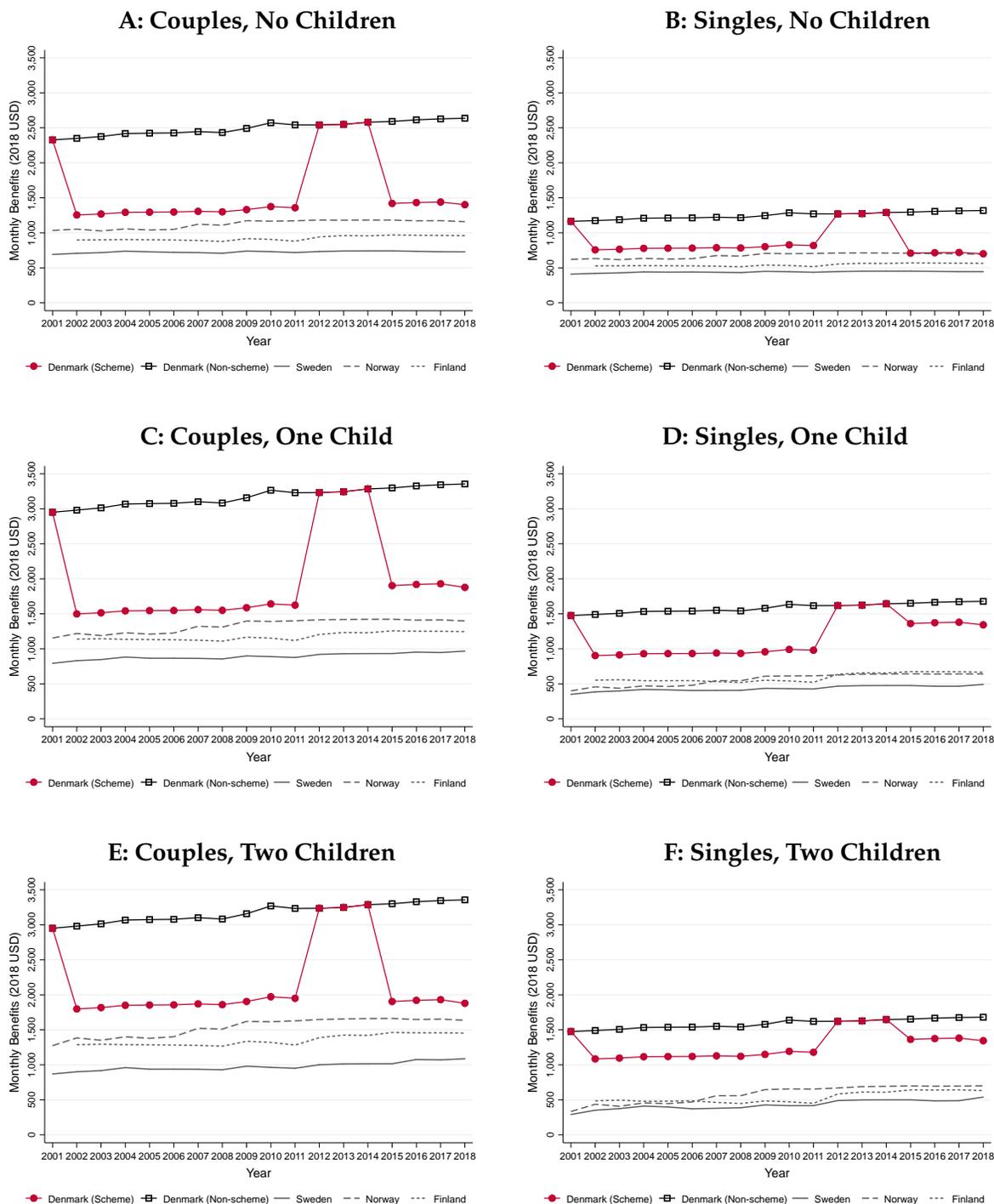
Ministry of Immigration,  
Integration and Housing

The Danish  
Immigration Service



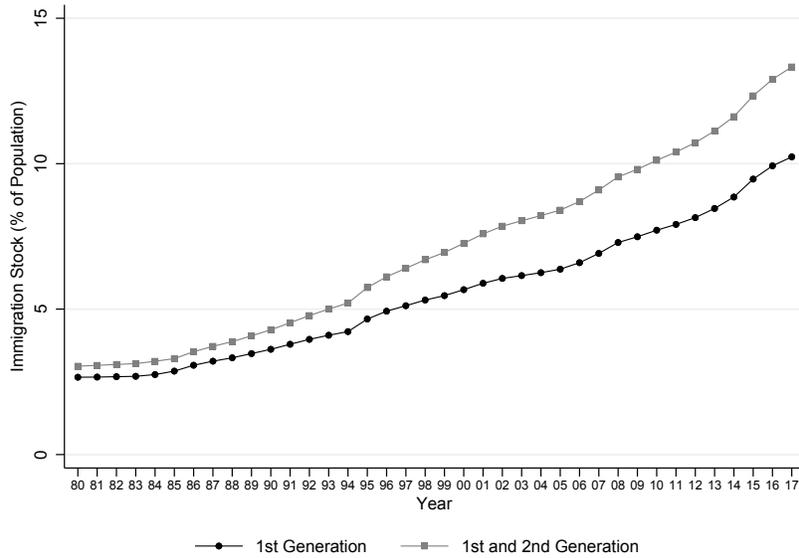
Notes: The two pictures show an ad campaign run by the Danish Government in September 2015 to inform potential immigrants about the recently (re)introduced welfare scheme, along with information about other regulatory rules. The left picture shows the campaign text in English released by the Danish Ministry of Immigration. The right picture shows the printed ad (from NBC News: <https://www.nbcnews.com/storyline/europes-border-crisis/denmark-buys-ads-lebanon-newspapers-aimed-refugees-n423216>). The ad campaign ran online and in Lebanese printed newspapers.

Figure A.II: Cash Welfare in Denmark and the Other Nordic Countries 2001-2018



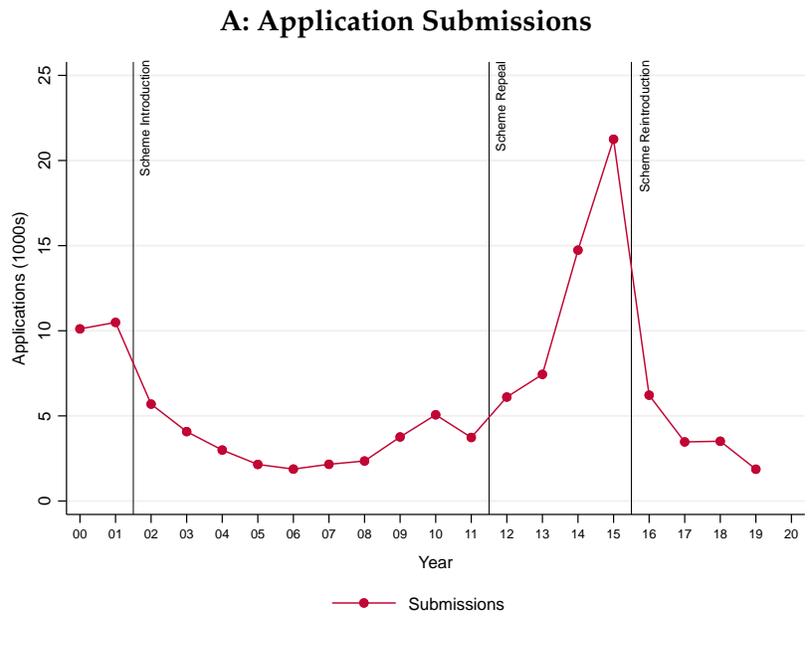
Notes: The figure shows maximum monthly cash benefits (in 2018 USD) for different household types in Denmark and the other Nordic countries. For Denmark, the figure shows both scheme and non-scheme benefits. Information on scheme benefits in Denmark are based on the website of STAR (The Danish Agency for Labour Market and Recruitment). The remaining benefits data are based on our own calculations using the OECD tax-benefit calculator. The calculations give benefits for non-employed households at age 40 who have been out of work for three months. For households with children, the age of the first child is set to 10 and the age of the second child is set to 8. The benefit series include only cash welfare (and not, e.g., unemployment insurance or in-kind benefits such as housing support).

Figure A.III: Immigrant Stock 1980-2017

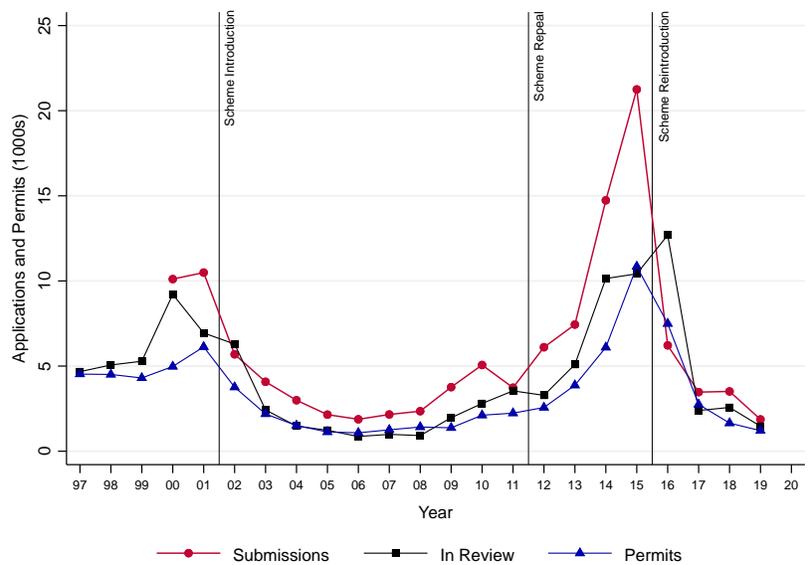


Notes: The figure shows the share of first-generation immigrants and the share of first- and second-generation immigrants in the Danish population. Definitions of first- and second-generation immigrants follow the official definitions of Statistics Denmark: a first-generation immigrant is a person who was born outside of Denmark and where neither of the parents are Danish citizens and born in Denmark. A second-generation immigrant is a person who was born in Denmark and where neither of the parents are Danish citizens and born in Denmark.

Figure A.IV: Asylum Applications from Outside EU: Annual Data

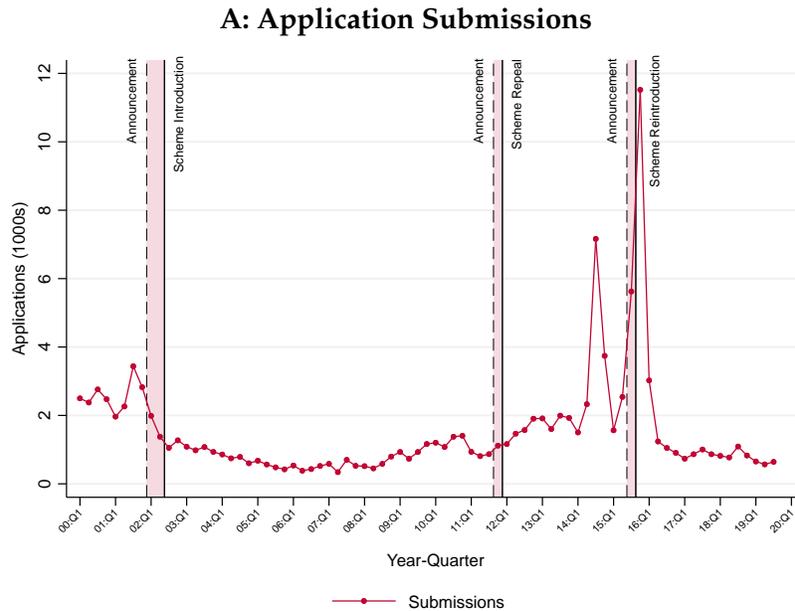


### B: Application Submissions, Applications in Review, and Residence Permits

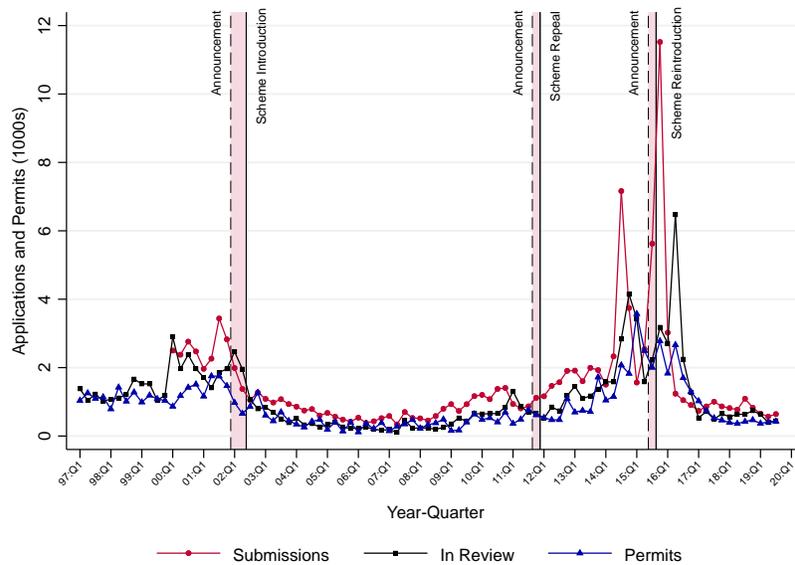


Notes: This figure shows asylum applications to Denmark at the annual level. Panel A shows applications submitted, while Panel B compares applications submitted (dots), applications registered for administrative review (squares), and residence permits granted (triangles). The solid vertical lines mark the timing of scheme reform implementations. Asylum seekers from Bosnia are dropped from the sample. The data are obtained from Statistics Denmark's online database (Statistikbanken, tables VAN5 and VAN77).

Figure A.V: Asylum Applications from Outside EU: Quarterly Data



**B: Application Submissions, Applications in Review, and Residence Permits**



Notes: This figure shows asylum applications to Denmark at the quarterly level. Panel A shows applications submitted, while Panel B compares applications submitted (dots), applications registered for administrative review (squares), and residence permits granted (triangles). The solid vertical lines mark the timing of scheme reform implementations, and the dashed vertical lines mark the timing of scheme reform announcements. Asylum seekers from Bosnia are dropped from the sample. The data are obtained from Statistics Denmark’s online database (Statistikbanken, tables VAN5 and VAN77).

Table A.I: Main Changes to Immigration Regulations, 2000-2017

<b>Date</b>	<b>Reform</b>	<b>Description</b>	<b>Affected group</b>
May 2000	Affiliation requirement	Restricts marriage-based immigration to cases where the spouses' "combined affiliation" to Denmark is at least as strong as to any other country.	Family
June 2002	24-year rule	Marriage-based immigration restricted to couples where both spouses are at least 24 years old.	Family
June 2002	Tightening of affiliation requirement	Affiliation requirement tightened to require that spouses have a "combined affiliation" to Denmark stronger than to any other country.	Family
June 2002	Stricter criteria to obtain permanent residency	Required time in Denmark to apply for permanent residency extended from 3 to 7 years; tighter restrictions for convicted criminals.	Asylum and Family
June 2002	"De Facto" refugee concept replaced with "protection status"	A tightening of approval conditions for asylum seekers who do not meet criteria set by international conventions.	Asylum
April 2007	Immigration test	Requirement to pass a test in Danish language and society to obtain family-based immigration.	Family
May 2010	Altered criteria to obtain permanent residency	Criteria for attainment of permanent residency changed to include requirements on employment, education etc., but required time in Denmark shortened to 4 years.	Asylum and Family
June 2011	Tightening of affiliation requirement	Affiliation requirement tightened again to require that spouses have a substantially greater affiliation to Denmark than to any other country.	Family
June 2011	Point system for marriage-based immigration	Marriage-based immigration now made conditional on obtaining a number of points based on criteria such as past employment, education and language skills.	Family
May 2012	Rollback of 2011 reforms	Immigration reforms from 2011 (point system for marriage-based immigration and tightened affiliation requirement) rolled back.	Family

<b>Date</b>	<b>Reform</b>	<b>Description</b>	<b>Affected group</b>
Feb. 2015	Introduction of "temporary protection status"	Certain asylum seekers fleeing conflicts or war will be granted "temporary protection status," which requires periodic renewal until the conflict ends or permanent residency is attained.	Asylum
Jan. 2016	Longer waiting period for family-based migration for refugees on temporary protection status	Waiting period for refugees on temporary protection status to apply for visas for family members extended from 1 to 3 years.	Asylum and Family
Jan. 2016	Stricter criteria to obtain permanent residency	Required time in Denmark to obtain permanent residency extended from 4 to 6 years and subject to some stricter requirements on e.g. employment, language tests.	Asylum and Family
Sept. 2016	Suspension of participation in UNHCR resettlement program	Danish Government suspends participation in the UNHCR resettlement program, through which Denmark had received around 500 refugees annually until this point.	Asylum
May 2017	Longer waiting period to obtain permanent residency	Required time in Denmark to obtain residency further extended from 6 to 8 years.	Asylum and Family

Notes: This table summarizes the main changes to immigration regulation over the period 2000-2017. Many other adjustments to immigration regulation have not been included in the table, as they changed only minor details of the legislation.

## B Asylum Applications

Our main specification uses residency as the measure of immigration. However, in the first instance, it is the decision to *apply* for Danish residency that should respond to benefit reform. Due to the lag between applying for residency and the final decision about residency, the timing of the DiD evidence in Figure 2 may be less compelling than it appears. The length of the lag varies by residency type and between individual cases, and in most cases we cannot observe it directly in the data. However, for asylum seekers specifically, we do observe the number and timing of applications (in addition to actual permits granted). This gives a measure of the desirability of Denmark as a destination country without any potential timing issues.

Graphical evidence on this outcome is shown in Figure A.IV on an annual level. Panel A shows the total number of submitted asylum applications to Denmark since year 2000. We see a pattern very similar to that of the treated group in Figure 2 in our main analysis. The number of asylum applications declines markedly after the introduction of the welfare scheme, from around 10,000 to under 5000 within the space of a few years after the reform. It then increases again after the abolishment, and drops yet again after the reintroduction of the scheme. This pattern is thus fully consistent with what we see in our main analysis.

The series in Panel A is the best measure of asylum applications, but it is only available from 2000 onwards. Therefore, in Panel B of Figure A.IV, we add two alternative series which are available further back in time. The first of these series shows the number of applications under administrative review. This series excludes applications that were submitted, but rejected in an initial stage before undergoing a rigorous review. This series contains some lag relative to the one for total applications in Panel A. The second additional series shows the number of residence permits granted for asylum seekers. Because this occurs after the review stage, the potential lag for this series is greater still. Nevertheless, the three series track each other fairly closely.

One important point to notice from Panel B of Figure A.IV is that the spike in asylum applications around 2015 is much larger than the spike in residence permits granted. This suggests that a sizeable part of the spike in asylum applications around this time came from individuals who did not fulfill the UNHCR's criteria for refugee status. This illustrates part of the reason why we use actual residencies rather than applications as our main outcome: Not every application leads to migration, and while variation in applications are useful for validating the existence of behavioral responses, the economically important question relates to the impact of benefits on actual

migration.

Figure A.V shows the same series as Figure A.IV, but on a quarterly level.<sup>19</sup> In this figure, we have included two vertical lines for each reform, the first indicating the announcement of the reform and the second indicating the actual implementation. Since the announcement and implementation fall within the same calendar year for each of the three reforms, this distinction was not necessary for the annual graphs. However, with the higher-frequency data considered here, it is useful to indicate both given behavior may plausibly start to react to the policy already at the point of announcement. The quarterly series are more noisy as one would expect, but the patterns remain consistent with immigration responses to the benefit reforms. We do notice that the large spike of asylum applications in 2015 occurs just after the reintroduction of the welfare scheme. This is not inconsistent with our interpretations, however, as a slight lag in the reaction to reforms is to be expected due to e.g. informational frictions.<sup>20</sup>

Finally, to relate and contrast these results to those obtained in our main analysis, it is worth highlighting the following points. First, the outcome variable considered here includes only one of the two types of migration treated by the welfare scheme; our main analysis also includes family-based migration. Second, the analysis here considers migration inflow, whereas our main analysis considers net flow. Third, we do not have a control group for our asylum applications outcome, so even though we observe sharp changes around the reforms, it is hard to obtain a quantitative measure of the magnitude of the welfare magnet effect from this data alone. The main purpose of this analysis is to show that the timing of changes in applications data is not too dissimilar from the timing of changes in residence data, suggesting that our estimates do capture immigration responses to welfare benefits rather than confounding effects of other immigration determinants.

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<sup>19</sup>Unlike our main analysis where the outcome variable (net immigration flow) is observable only on an annual basis, asylum applications are observable on a quarterly basis.

<sup>20</sup>For instance, the Danish government's ad campaign in Lebanese newspapers (see Figure A.I) ran in September 2015, just after the reintroduction of the welfare scheme. Its purpose was to inform potential migrants of the welfare cuts. However, while many Syrian refugees passed through Lebanon on their way to Europe, it is likely that many of those who applied for asylum in Denmark in the final quarter of 2015 would have already been further along in their journey at this point.