The Welfare Magnet Hypothesis: Evidence from an Immigrant Welfare Scheme in Denmark: Comment

By Joop Adema*

Agersnap, Jensen and Kleven (2020) study the effect of changes in welfare benefit levels on immigration in Denmark, reporting a large migration elasticity of 1.3. However, their approach reports an ill-defined net migration elasticity, conflates migration with migrant residents turning 30, and is sensitive to origin-country shocks. All of these exaggerate the size of the welfare magnet effect. Using an alternative approach that addresses these concerns, I find substantially lower migration elasticities: at most 0.28 for arrivals and 0.14 for stocks. Moreover, I find that lower benefits do not increase the average skill level of migrants.
JEL: F22, H53, I38, J15

Generous welfare benefits could make destinations more attractive to migrants, particularly for those with worse employment prospects (Borjas, 1999). Agersnap, Jensen and Kleven (2020) (hereafter AJK) provide an analysis of the "welfare magnet hypothesis" by studying the introduction in July 2002, the repeal in January 2012 and the re-introduction in September 2015 of a Danish policy called Starthjælp (English: Start Aid), which strongly reduced welfare benefits for non-EU immigrants in the first seven years of residence. AJK's study is one of the first causal investigations of the hypothesis and has received widespread recognition in academia and the media.[1]. The authors document that non-EU migration is strongly responsive to the level of welfare benefits in Denmark: they report a migration elasticity with respect to welfare benefits of 1.3. This magnitude is comparable to migration elasticities with respect to disposable income documented among high-skilled professionals such as inventors and football players (Kleven et al., 2020). As the results of AJK are driven by asylum and family migrants, the implied elasticities for these subgroups are even larger. Taking these estimates at face value would suggest that governments can steer asylum and family migration by adjusting benefit levels.

AJK use two distinct empirical approaches to examine how net migration responds to changes in benefit levels. Their first approach compares net migration of all individuals from non-EU countries with that of all individuals from old EU countries.[2] For both groups, AJK compute the net flow-stock ratio—defined

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[1]The paper has been cited more than 100 times according to Google Scholar, and has received attention from large Danish and German newspapers (FAZ, 2019; Information, 2019)

[2]Old EU countries refer to EU member states as of 2001, except for Denmark.

as net migration flows relative to migrant stocks in 2001—and detrend the data based on trends before the reform (1980–2001). They quantify the effect using a panel of benefit groups, based on household composition and treatment status (non-EU vs. old EU). They regress the detrended net flow-stock ratio by benefit group on the time-varying level of benefits and group- and time fixed effects. By scaling the regression estimate by the ratio of average benefit levels and the average net flow-stock ratio in 2001, AJK obtain an elasticity of 1.3.[3] Their second approach compares the evolution of the net flow-stock ratio of non-EU migration in Denmark to that in other Nordic countries in a synthetic control framework, which leads to comparable conclusions as the first approach.

In this comment, I point out three consequential shortcomings of AJK's analysis. Each of these biases their results towards stronger evidence for the welfare magnet hypothesis.

First, AJK's first approach calculates the elasticity of net migration flows with respect to benefit levels. As net migration flows can be small or negative, a percentage change in net flows (and thus the elasticity) is ill-defined and incomparable to the literature. A consequence is that small differences in the *level* of net migration can drastically impact and even flip the sign of the elasticity estimate.

Second, AJK measure net migration flows as the annual change in the stock of non-EU migrants. To account for the 2002 reform restricting family reunification for individuals under the age of 24, they restrict their calculation to migrants aged 30 and above. This approach not only captures net migration, but also existing migrant residents reaching the age of 30 (positively) and passing away (negatively). As Denmark received many young migrants in the 1990s, the number of non-EU migrants aged 30 strongly increased during the 1990s. When instead calculating net migration as the difference between gross inflows and outflows, the drop in net migration relative to the trend after the introduction of Start Aid in 2001 was much less pronounced.

Third, both of AJK's empirical approaches disregard the origin country composition of non-EU migration flows. As origin-specific push factors shape migration flows, it is crucial to account for those when constructing counterfactual migration flows in absence of reform. This may be particularly relevant for refugee migrants, who often leave in response to origin-specific (conflict) shocks. Their second approach could provide a credible counterfactual if the origin country composition of non-EU migration to Denmark was similar to that of other Nordic countries. However, I show that the pre-reform composition of origins differ strongly. In particular, before the 2001 (2012) reform, Afghan and Iraqi (Syrian) immigrants were overrepresented in non-EU migration to Denmark compared to other Nordic countries. As emigration of Afghanis and Iraqis (Syrians) strongly decreased (in-

[3] AJK use a panel regression to estimate the effect of benefit levels on net migration across household types, with EU households as controls. However, their standard errors ignore correlation and cross-sectional dependence in the treatment. Given the positive correlation of treatment and outcomes over time and units, this likely understates the uncertainty of their estimates (see Appendix A.5).

creased) after 2001 (2012) due to origin-country shocks, these would have also decreased (increased) migration flows to Denmark in absence of reform in Denmark.

In addition, the new Danish government introduced various other restrictive migration policies in 2002, besides the minimum age requirement for family reunification. Among others, family reunification became much more restrictive regardless of age and Denmark abolished the widely used *de facto* protection status. In response to the 2015 refugee crisis, Denmark further tightened asylum procedures, while neighboring countries also revised their asylum policies. As a result, evidence from the Danish welfare reforms needs to be interpreted with caution and arguably only provides an upper bound of the effect of welfare benefits on migration flows.

I re-analyze the effect of Start Aid on gross migration flows, stocks and monthly asylum applications. I construct origin-specific counterfactuals that rely on the pre-reform propensities to migrate to and from Denmark and time-varying migration from- and to the origin for a reference set of European destination countries. For each of the three reforms, I calculate the change in arrivals, departures, stocks and asylum applications relative to the counterfactual. This approach circumvents all aforementioned concerns, except for the simultaneous policy changes.

My results suggest that the impact of benefits on non-EU migration to Denmark is limited. Although I confirm that the number of migrant arrivals, migrant stocks and asylum applications dropped compared to the counterfactual after the initial introduction of Start Aid (2002-2011), I find no evidence that Denmark became a more popular location for non-EU migrants after the reinstatement of high benefit levels in 2012. After the re-introduction of Start Aid in 2015, Denmark became less attractive to non-EU migrants: asylum applications decreased, departures increased and the stock decreased slightly. Nevertheless, studying the impact of destination country policy changes around the 2015 migrant crisis is challenging as the share of asylum applications lodged in any given country is highly volatile, not in the last place because of changes in policies in other countries.

Combining the estimates of all three reforms, I find that the implied elasticities are considerably smaller than reported by AJK: the elasticities of gross inflows (0.28) and stocks (0.14) are positive, but insignificant. The elasticity of asylum applications is positive and marginally significant (0.77). However, the uncertainty of this estimate is large, which is partially driven by the high volatility of asylum migration during the 2015 refugee crisis.

The welfare magnet hypothesis not only predicts a shift in the extent of migration, but also a shift in migrants' skill composition (Borjas, 1999). I complement prior analysis with a study of the effect of welfare benefits on the educational composition of the migrant pool using the EU Labor Force Survey. Focusing on the same reforms, I find no evidence that the share of tertiary educated arriving in Denmark is higher in years with lower welfare benefits.

This paper is organized as follows. Section I explains the shortcomings of AJK's

analysis, Section II discusses coinciding policy changes not accounted for by AJK, Section III re-analyzes the effect of benefits on migration flows using the origin-specific approach, and Section IV examines whether the level of benefits changes migrant selection. Section V concludes.

I. Critiques

A. *Migration elasticities*

The primary quantitative result in AJK is the elasticity of net migration flows with respect to benefit levels. Because elasticities rely on the relative (percentage) change in an outcome variable, it is undefined when the variable is zero or negative. As net migration (arrivals minus departures) can be negative, the corresponding elasticity is ill-defined. Even when net migration is strictly positive, the elasticity can be highly sensitive to baseline levels—particularly when net flows are small—leading to unstable estimates.

To illustrate how sensitive the elasticity is to the pre-reform level of net migration, I compute percentage changes in net flows to Denmark while uniformly shifting the level of net migration by a constant across the entire time period (see Figure A1). This leaves absolute changes in net migration flows over time unchanged, but alters relative changes in net flows drastically. Had net flows been half their size (a decrease of only 3,700 individuals), the percentage change—and thus the implied elasticity—would have doubled. If net flows had been of the opposite sign, the naively calculated elasticity would have been equal in magnitude but opposite in sign.

Although net migration remains positive throughout the period analyzed by AJK, their estimated elasticity still faces limitations not shared by — more conventional— gross flow and stock elasticities. First, their elasticity cannot be reliably used to predict migration responses to benefit changes when net migration flows are non-positive, or when they are positive but small. Second, net migration flows are generally more volatile than gross flows or migrant stocks, making such estimates more volatile. Moreover, because levels of net migration are small relative to annual fluctuations, net migration elasticities tend to be larger than those based on gross flows or stocks. Using gross flow or stock elasticities also facilitates comparison with a large body of literature on migration responses to changes in disposable income (Kleven et al., 2020).

Appendix A.1 provides further discussion of AJK's elasticity calculation. By scaling the regression estimate using the 2001 net flow ratio—when it was still low—rather than a contemporaneous counterfactual value, AJK likely overestimate the true elasticity.

B. *Measuring net migration*

Because of a coinciding family reunification reform (see section II), AJK's first approach examines net migration of those aged 30 and above. They calculate

net migration as the annual change in the stock of non-EU migrants aged 30 and over. However, this approach does not only capture in- and outflows of non-EU migrants aged 30 or above, but also non-EU migrant residents who turn 30 (positively) and pass away (negatively). As Denmark experienced a large inflow of (young) migrants in the 1990s, in subsequent years many of those turned 30, but only few pass away. AJK's approach thus overestimates net migration and is sensitive to changes in demographic composition of the pre-existing migrant population.

A more conventional measure of net migration, the difference between gross inflows and outflows, does not face this problem. The gross flow data from Statistics Denmark records inflows and outflows of foreign-born individuals by last and next usual residence, respectively.[4] Gross flows by citizenship are also reported by Statistics Denmark since 2007, which I use to validate the use of the residence-based measure of net flows.

Figure 1 compares AJK's approach with the flow-differencing approaches for all individuals (Panel A) and for those aged 30 and above (Panel B).[5] Panel A reassuringly shows that all three approaches yield very similar results for the full population, which confirms that mortality and the limitations of the gross flow data discussed in footnote 4 have a minor impact. As the citizenship-based and residence-based approach yield similar results from 2007 onwards, there is at most a small net imbalance between EU and non-EU migration due to differences in migrants' origin and previous/next residence. Panel B, however, shows that the approaches vary strongly on the subset of individuals aged 30 and above: AJK's approach strongly overestimates the net migration flow and exhibits a much stronger upward trend between 1990 and 2001. This is consequential as AJK also detrend net flows by extrapolating trends before 2002. The average difference between net flows and the trend line between 2002 and 2011 is much smaller using the residence-based gross flow approach: 1,900 instead of 3,900 individuals.

To demonstrate that the discrepancy between the gross flow and stock-based approaches stems from the number of migrants turning 30, Figure A2 presents the age distribution of non-EU migrants in 2001, as well as 5, 10, and 15 years earlier. In 2001, the age distribution among non-EU migrants was relatively flat between ages 30 and 35, but dropped off steeply for those under 30. This implies that, even without welfare policies reform, the number of non-EU migrants reaching age 30 in 2001 would have declined sharply compared to previous trends.

[4]This data has two potential limitations compared to the stock data by country of origin (see Footnote 9). First, individuals may move from- or to countries that are not their country of origin. This only distorts net migration if there is a net imbalance between (i) net migration of non-EU origin individuals between Denmark and EU countries and (ii) net migration of EU individuals between Denmark and non-EU countries. For further discussion, see Appendix A.1. Second, naturalized individuals are excluded. However, not many naturalized individuals will (i) leave Denmark after naturalization as they opted to acquire Danish citizenship, which is generally only possible after 9 years of residence and (ii) and return to Denmark after naturalization.

[5]Following AJK, I exclude individuals from Bosnia and Herzegovina in all analyses in this Section.

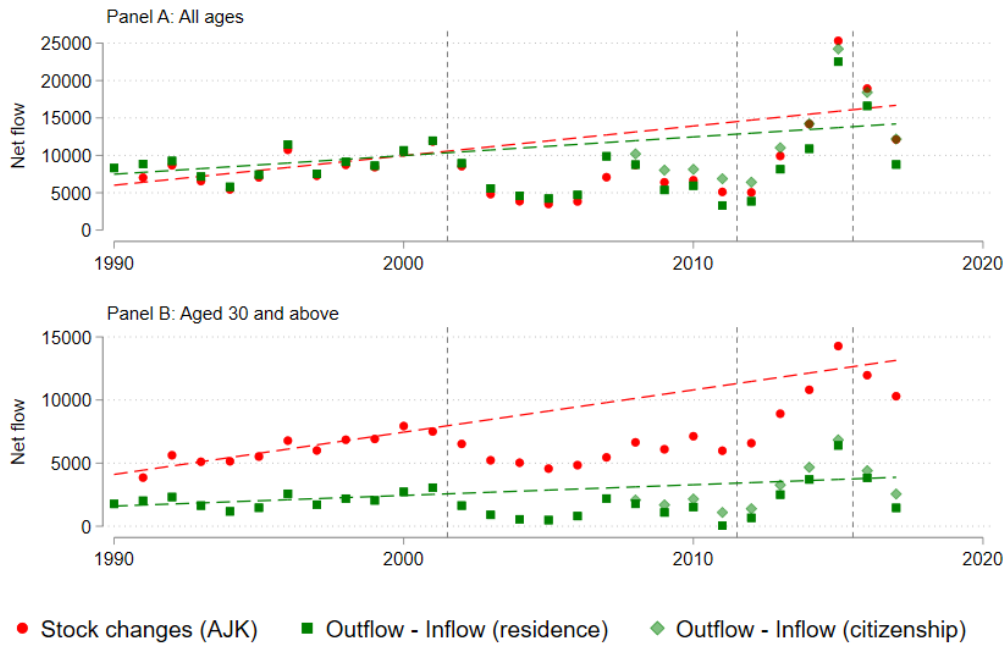

FIGURE 1. NET MIGRATION USING AJK'S AND GROSS FLOW-BASED APPROACHES

Notes: Stock changes and gross flow difference approaches to net non-EU migration flows for (a) all ages and (b) those aged 30 and above. Gross flow differences are calculated based on residence and citizenship (since 2007). Dashed colored lines indicate extrapolated linear trends between 1990 and 2001. Dashed vertical lines indicate the introduction (2002), repeal (2012) and re-introduction (2015) of Start Aid. Data originates from Statistics Denmark tables FOLK2 (stock), INDVAN and UDVAN (flow by residence), and VAN1AAR and VAN2AAR (flow by citizenship).

C. Origin country composition

AJK's second approach applies the synthetic control method (Abadie, Diamond and Hainmueller, 2010) to compare non-EU migration in Denmark with that in other Nordic countries. As migration is affected by push factors in the countries of origin, this method only provides a credible counterfactual for total non-EU migration to Denmark if the pre-reform origin country composition of Denmark and its synthetic counterpart are comparable. Panel A of Figure 2 shows that migrant arrivals in Denmark had a very different composition in 2001 than the donor countries and synthetic control. Relative to its synthetic counterpart, Denmark received far more Iraqis and Afghanis, but fewer Russians. Panel B of Figure 2 shows that migration from Iraq and Afghanistan declined sharply across the Nordics after 2001, implying that Denmark's inflows would likely have decreased even without Start Aid.[6] Similarly, Figure A4 and Figure A5 show that before the

[6]Figure A3 shows that this is not driven by Denmark, but also observed if excluding Denmark.

2012 abolition and 2015 re-introduction of Start Aid the composition of migration flows to Denmark also markedly differed from that of other Nordic countries. Most notably, Denmark received a relatively large share of Syrians in 2011. Given the mass displacement from Syria in following years, Denmark would likely have received more Syrian arrivals than its synthetic control. Whether AJK's second approach over- or underestimates the effect on arrivals for total non-EU migration depends on all origin country-specific shocks. Since net migration also depends on departures, not just arrivals, I address both in an origin-based reanalysis in Section III.

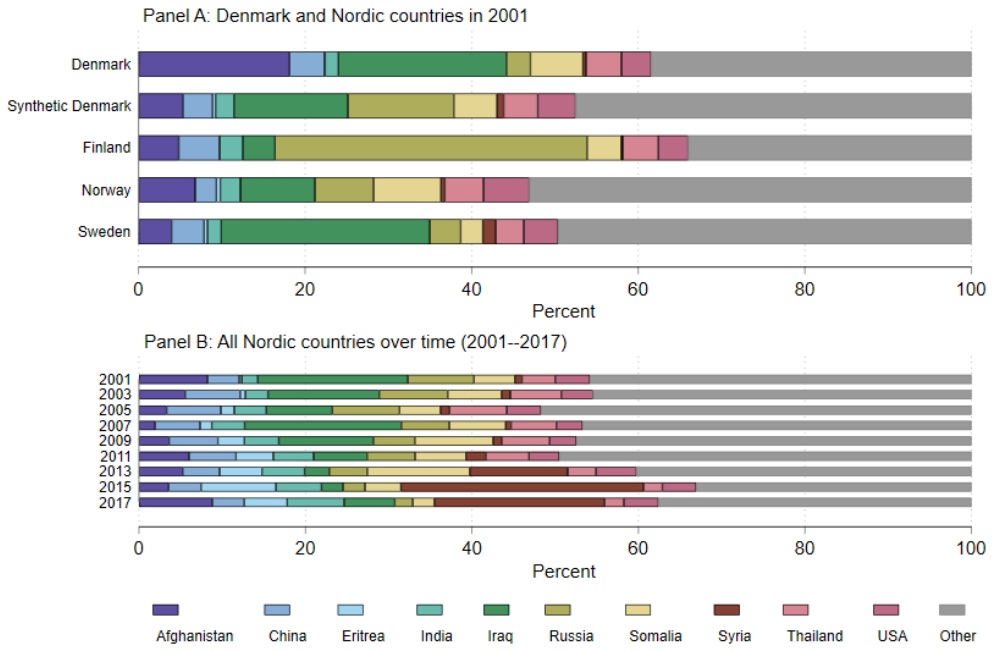

FIGURE 2. COMPOSITION OF NON-EU MIGRATION FLOWS ACROSS DESTINATIONS AND OVER TIME

Notes: Panel A: Composition of non-EU immigration flows to Denmark, its synthetic control (using the weights reported by AJK) and other Nordic countries in 2001. Panel B: Bi-annual composition of non-EU immigration flows to Nordic countries. In both panels, I explicitly show the 10 origin countries with the largest flow to all Nordic countries between 2001 and 2017. Data on migration inflows by country of origin are obtained from the OECD international migration database.

II. Coinciding migration policy changes

The credibility of AJK's estimates hinges on the absence of policy changes affecting non-EU migration to Denmark or other groups used to construct the counterfactual. However, following the 2001 election, the Danish government—backed

by the far-right Danish People's Party—enacted a series of migration policy reforms.[7] Although AJK acknowledge that many policies have changed, they address one explicitly: the introduction of a spousal migration ban for couples where either partner is under 24. Additional restrictions also affecting older migrants were introduced, including a requirement that sponsors should not have received social assistance in the prior year, provide a collateral of 7,500 euro, and proof of adequate housing (Andersen, 2007; Bratu et al., 2020).

A major reform in asylum policy in 2002 was the replacement of the *de facto* status—granted to over half of asylum seekers in 2001—with the more restrictive *B* status, which was rarely granted in subsequent years. Denmark also ended the option to apply for asylum from abroad and the aslyum for applicants from Afghanistan and Iraq became stricter, leading to lower recognition rates and longer processing times (Hvidtfeldt and Schultz-Nielsen, 2022). In 2015, a newly implemented temporary protection status specifically aimed at Syrian refugees heightened uncertainty about their long-term prospects of staying and prevented family reunification during the first three years after arrival.

These changes not only mechanically reduced the number of non-EU migrants receiving legal status but also indirectly diminished Denmark's appeal to asylum seekers and their families. Policies in neighboring countries also matter: in the years following 2002, Sweden and Finland adopted less restrictive policies. Sweden and Germany adopting more welcoming policies in response to the 2015 refugee crisis. Moreover, the growing prominence of the far-right Danish People's party and anti-migration rhetoric may have further reduced Denmark's attractiveness, independently of formal policy changes (Docquier and Vasilakis, 2024).

Taken together, these policy changes likely curtailed non-EU migration to Denmark, particularly those introduced in 2002. As a result, estimates based on the timing of Start Aid's introduction should be interpreted as upper-bound estimates of the impact of welfare benefits on migration.

III. Origin-specific re-analysis

I re-analyze the effect of the Start Aid reforms on migration using an approach that does not suffer from the issues raised in Section I. Using a counterfactual that explicitly accounts for the origin country composition of non-EU migration, I estimate Start Aid's effect on arrivals, departures, stocks, and asylum applications around the introduction, repeal and re-introduction of Start Aid.

A. Empirical Approach

I construct counterfactual flows and stocks for every origin country using origin-specific migration data for Denmark and a set of European reference countries.[8]

[7] See Section A.2 of the Online Appendix for a detailed discussion on relevant policy changes and how these could have affected non-EU migration to Denmark.

[8] Section A.3 of the Online Appendix provides a detailed discussion.

To estimate the counterfactual number of arrivals from an origin country to Denmark, I multiply the share of migrants from that origin choosing Denmark in the last pre-reform year by the time-varying total number of emigrants from that origin country to the reference countries. To estimate the counterfactual number of departures from Denmark in a given year by origin, I multiply the counterfactual stock by the counterfactual re-migration rate, which is the share of migrants that leave their host country again. The counterfactual stock is calculated iteratively from the pre-reform stock and the counterfactual number of arrivals and departures in prior periods. The counterfactual re-migration rate is estimated by multiplying the re-migration rate from Denmark in the last pre-reform year by the ratio between the origin-specific re-migration rate from all reference countries except Denmark in the current year over that in the last pre-reform year.

My approach hinges on the following four assumptions. First, the origin-specific propensity to emigrate over time is unaffected by Start Aid. Second, in absence of reform the propensity to migrate to Denmark would have remained unchanged since the last pre-reform year. Third, the origin-specific re-migration rate from the reference countries is unaffected by reform in Denmark. Fourth, the ratio re-emigration propensity from DK relative to that from reference countries would have remained unchanged since the last pre-reform year in absence of reform. The assumptions above are origin country-specific. As the main question of interest concerns *aggregate* non-EU migration to Denmark, violation of these assumptions for single origin countries could occur, but no systematic violations such that aggregate quantities are affected are allowed. The first and third assumption are plausible because Denmark is small compared to the reference countries. As argued in Section II, the second and fourth could be violated as there are other policy-driven changes in the attractiveness of Denmark, and likely cause an upward bias in the response of migration to Start Aid. However, such violations also undermine the approaches of AJK.

This approach has important advantages over AJK's. First, it does not require the assumption that migrants from different origin countries provide credible counterfactuals. In contrast, my approach uses the migration response of individuals from the same origin to other destination countries. Second, my approach does not rely on extrapolation of a pre-reform trend to construct a counterfactual, as in AJK's first approach. Third, it allows me to separately study the role played by changes in arrivals and departures and separately estimate inflow, outflow and stock elasticities separately.

B. Data

I rely on data on migration flows of foreign citizens by country of previous and next residence and data on the stock of migrants by country of origin from Statistics Denmark (1980-2017),[9] complemented with comparable data from the

[9]The country of origin of a migrant in the Danish stock data is determined in the following way: If both parents are known, the country of origin is defined by the mother's country of birth or citizenship

Organisation for Economic Co-operation and Development (OECD) international migration database for other EU destination countries. To harmonize data across countries, I rely on flow and stock data including individuals aged below 30. As the harmonized OECD data is not available for all EU countries since 2001, I incorporate data from 12 European reference countries.[10] I supplement this with monthly first asylum application data from Eurostat. Unlike migration flows, asylum applications are largely unaffected by mechanical effects due to changes in migration policies (see Section II), and gives a high-frequency account of the relative attractiveness of European destinations for asylum seekers.

C. Results

I aggregate the counterfactual arrivals, departures, stocks and asylum applications across all non-EU countries and calculate the relative difference between the actual and counterfactual numbers, using 2001 as the base year. Figure 3 graphically shows these relative differences.[11] In the years following the initial introduction, arrivals decreased and departures decreased more than in absence of the reform, but by no more than 50%. The stock decreased gradually: by 2011, the migrant stock was 21% lower than in the counterfactual. Although departures initially increased, they decreased afterwards.[12] Asylum applications strongly decrease initially, but partially recover after 2008.

After the repeal of the reform in 2012, departures did not decrease relative to the origin-based counterfactual. Arrivals and asylum applications increased temporarily, but decreased strongly in 2015. These merit conclusions that starkly contrast those of AJK. Figure 4 of AJK shows a strong increase in net migration in 2015 relative to the 2001 stock of migrants compared to other Nordic countries. However, AJK's approach does not account for the fact that the origin country composition of migration to the EU changed drastically due to the large inflow of refugees. Before the repeal of Start Aid Denmark already received a relatively large proportion of migrants from countries that would later become major refugee origin countries, such as Syria.[13] As the reasons that caused many Syrians to choose Denmark in 2011 likely remained relevant, it is unsurprising that many Syrian refugees went to Denmark in subsequent years. Despite the abolition of Start Aid, a smaller share of Syrians went to Denmark in subsequent years,

respectively. If only one parent is known, the country of origin is defined by that parent's country of birth. If none of the parents is known, the country of origin is defined by the person's own country of birth. The country of origin is independent of an individual's current citizenship.

[10] Thee include Austria, Denmark, Germany, Finland, France, Italy, Iceland, Luxembourg, Netherlands, Norway, Spain and Sweden.

[11] Figures A12, A13 and A14 show the development of actual and counterfactual arrivals and departures, stocks and asylum applications over time.

[12] Departures are affected by more restrictive migration policies through two opposing effects. On the one hand, it makes Denmark less appealing to current migrant residents, increasing departures. On the other hand, it reduces the stock of migrants in Denmark on the medium run, reducing departures.

[13] In 2015, 21% of inflows and 29% of asylum applications lodged in the 12 reference countries came from Syria.

suggesting that Denmark became less attractive for them.[14]

After the 2015 re-introduction of Start Aid, asylum applications further decreased relative to the counterfactual. Panel B and D of Figure A15 show that the share of asylum applications going to all other Nordic countries also strongly decreased, coinciding with a strong increase of the share applying in Germany. This suggests that the drop in asylum applications in 2016 and 2017 may actually be driven by changes in Germany (Pries, 2020), rather than by the re-introduction of Start Aid. Figure A17 shows that the drop in asylum applications is absent when only considering Nordic reference countries. This underscores the difficulty of assessing the impact of single policies when there are large and sudden changes in migration flows that may have a heterogeneous impact across destinations.

To quantitatively assess the effect of benefit changes on migration for each of the three policy regimes, I perform the procedure outlined above using each last pre-reform year as the base year- I calculate the average relative change for arrivals, departures and asylum applications across migrant origin countries over the respective time periods and for stocks for the last year in the respective time period.[15] I weight origin countries with the value of the counterfactual flows to make the result representative of aggregate changes. To account for serial correlation in migration flows from a specific origin country, I cluster standard errors on the origin level. Table 1 presents the average effects for the introduction (2001), abolition (2011) and re-introduction (2015) and the implied elasticities across the three regimes by dividing percentage changes by the average change in benefits due to Start Aid of 40% (Dustmann, Landersø and Andersen, 2024*a*).

The results indicate that after the initial introduction the decrease in arrivals (-34%), asylum applications (-62%) and migrant stocks (-17%) is statistically significant. After the re-introduction of high benefits, there is no significant increase in any of the four measures relative to its respective counterfactual. After the reinstatement of low benefits, asylum applications decrease significantly, departures increase and stocks decrease. The implied elasticities for arrivals, departures and the migrant stock are small and statistically insignificant. Averaging over the three regimes between 2002-2017 the implied elasticity of arrivals is 0.28 and of stocks is 0.14. The elasticity with respect to asylum applications is positive (0.77) and close to statistically significant.

These results are robust to the choice of reference countries. Instead of relying on a larger set of European countries, I only use Nordic reference countries in a robustness check. On the one hand, this approach benefits from the fact that changes in the attractiveness of these countries are likely to be more similar, due to

[14]Figure A16 shows the yearly share of inflows (left) and monthly asylum applications (right) in the EU being filed between 2011 and 2017 in the Nordic countries (upper), and Germany (lower). In 2011 (2015), Denmark received 6.6% (2.9%) of all arrivals from Syria and 9.5% (3.0%) of all asylum applications by Syrians in the reference countries.

[15]The policy changes did not happen exactly at the end of these years, but I follow AJK and choose these as the base years. As the asylum application data is available at the monthly level, I take the 12 months before the reform was implemented as the base period.

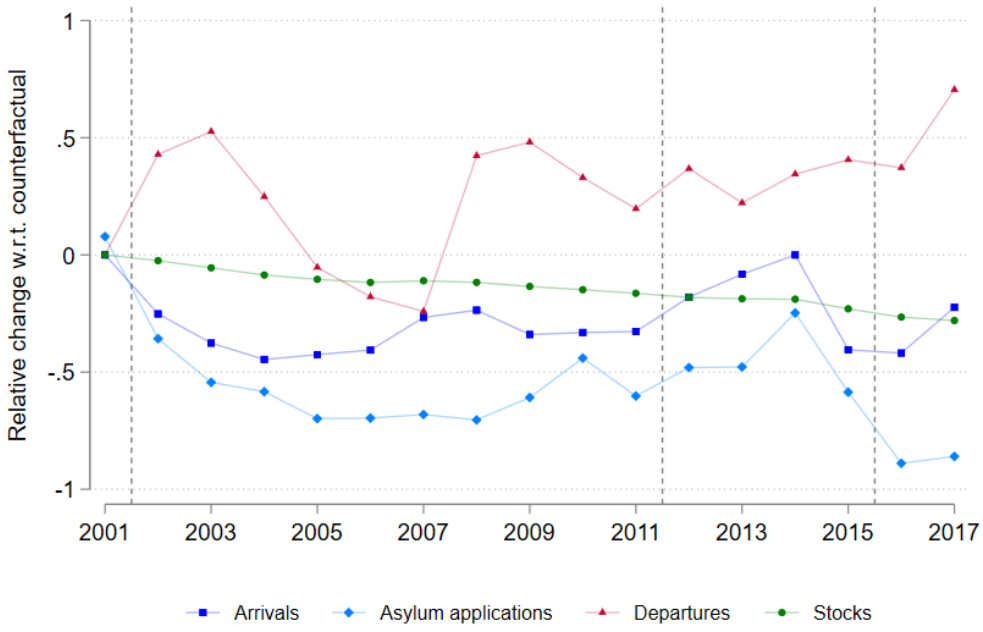

FIGURE 3. CHANGES IN NON-EU MIGRATION RELATIVE TO ORIGIN-BASED COUNTERFACTUAL

Notes: Relative changes between actual and counterfactual migrant arrivals, departures, stocks and asylum applications between 2001 and 2017. For each of the four, the relative difference is calculated as a percentage difference between the realized data and counterfactual relative to the counterfactual. Counterfactuals are constructed according to the approach outlined in Section III.A and Section A.3 of the Online Appendix. The value for asylum applications in 2001 does not coincide with zero as the pre-reform year does not coincide with the calendar year 2001. Data on migration flows originate from the OECD international migration database, data on stocks for all reference countries except Denmark originate from the OECD international migration database, stocks for Denmark from Statistics Denmark table FOLK2, and asylum applications from Eurostat tables migr_asyctzm and migr_asyappctzm.

shared economic conditions or common transit barriers for example. On the other hand, if the reference countries are more similar and serve as closer substitutes for Denmark, migration patterns may be related due to substitution effects. As a result, changes in migration to Denmark and to the reference countries could be negatively correlated, potentially leading to an overestimation of the effect sizes. Figure A17 and Table A1 shows that the results are qualitatively similar, but implied arrival- and stock elasticities sizes are somewhat larger and statistically significant, but the asylum application elasticity is smaller. As percentage changes calculated by our approach are bounded from below but not from above, Table A3 reports estimates of a median estimator: the implied elasticities are similar to that from the mean estimates. Contrary to AJK's first approach, my approach does not consider Denmark-specific pull factors that equally affect all migrants. I redo the analysis for old EU country migrants to test whether the popularity

of Denmark changed for unaffected migrants. The results, shown in Table A2, indicate that if anything departures of EU migrants from Denmark increased and the migrant stock decreased during low-benefit regimes. Hence, at least part of the response of non-EU migration can be explained by pull factors that are common to EU migrants, further suggesting that my estimates are an upper bound of the effects of changes in benefits.

Table 1—Percentage changes in migration and implied elasticities

	(1) Arrivals	(2) Departures	(3) Stock (last year)	(4) Asylum applications
Low benefits (2002-2011)	-0.338	0.168	-0.164	-0.617
	(0.108)	(0.177)	(0.066)	(0.091)
Observations	1192	992	125	5845
High benefits (2012-2015)	0.058	0.168	-0.013	-0.228
	(0.241)	(0.158)	(0.057)	(0.177)
Observations	460	460	115	1810
Low benefits (2016-2017)	-0.027	0.273	-0.020	-0.513
	(0.043)	(0.075)	(0.008)	(0.134)
Observations	274	227	152	1340
Implied elasticity (by regime)	0.283	-0.235	0.137	0.772
	(0.231)	(0.170)	(0.075)	(0.205)
Observations	1926	1679	392	8995

Notes: Average relative changes and elasticities in migration during the initial introduction, repeal and re-introduction of Start Aid. The unit of observation is the country-year for flows and stocks and the country-month for asylum applications. Origin-specific relative changes using the approach outlined in Section III.A. Average relative changes are obtained from a regression of the origin-time specific relative change on a constant, weighting observations with the counterfactual number of migrants and clustering standard errors on the country of origin level. Elasticities are calculated by dividing the relative changes by the approximate average change in benefits (± 40%) across groups, giving each of the three regimes equal weight. See notes to Figure 3 for a description of the data.

IV. Migrant skill composition

A. Theoretical predictions

A long-standing question in migration research is how migrants select into migration and sort across destinations (Borjas, 1987; Grogger and Hanson, 2011). To analyze the impact of welfare benefits on migration, I adopt the framework of Borjas (1999).[16] This framework predicts that if a country reduces welfare benefits, low-skilled migrants are less likely to choose that destination, thereby lowering the average skill level of the migrant pool.

[16]See Figure A18 for an illustration and discussion.

B. Data and Empirical Approach

To test this prediction, I use data from the EU Labour Force Survey (LFS) (Eurostat, 2008—2023). The LFS includes questions on demographic and economic characteristics, including educational attainment. I include all old EU countries (plus Iceland and Switzerland) in the analysis. A drawback of the EU LFS is that it reports broad origin regions rather than exact countries of birth. I include all individuals who arrived from outside the EU between 1995 and 2019, excluding those arriving since the COVID-19 pandemic. Since 2008 the EU LFS records the length of residence, allowing me to infer arrival years.

I examine how the educational composition of arrival cohorts varies with Denmark's Start Aid policy changes by comparing skill levels of non-EU migrants from the same origin region arriving in the same year to Denmark and other old EU countries. To study the effect on tertiary educational attainment, I limit the sample to those aged at least 25 and at most 59 at arrival. The outcome is a binary indicator for holding a tertiary degree. I regress this on an indicator for Denmark interacted with the policy periods, using the last three years before Start Aid (1998–2001) as the reference period:

$$
\begin{aligned}
(1)\quad S_{iodct} = {} & \beta_1 DK_d \times \mathbb{1}(1998 \leq c) + \beta_2 DK_d \times \mathbb{1}(2002 \leq c \leq 2011) + \\
& \beta_3 DK_d \times \mathbb{1}(2012 \leq c \leq 2015) + \beta_4 DK_d \times \mathbb{1}(c \geq 2016) + \\
& \theta_{oc} + \phi_t + \psi_{od} + \epsilon_{iodct}
\end{aligned}
$$

Here, S_{iodct} is a binary indicator equal to 1 if individual i, who migrated from origin o in year c to destination d at t, holds a tertiary degree, and 0 otherwise. I control for origin region by year-of-arrival fixed effects, time fixed effects and origin-destination pair fixed effects. The first controls for compositional differences in the migrant pool that arise from origin region-specific changes in educational attainment and selection into migration. The second controls for changing migrant skill levels over time. As the first set of fixed effects nests cohort fixed effects, this also controls for the effect of time in the destination. The third controls for time-invariant skill differences across bilateral origin region-destination country pairs. This is especially important if migrants from the same region going to Denmark differ in education levels from those going to other countries. Regressions are weighted using EU LFS demographic weights to ensure representativeness of Europe's non-EU migrant population. Standard errors are clustered by origin region–destination pair.

C. Results

Figure 4 graphically presents estimates from equation 1 across three samples: (1) using only Nordic destination countries, (2) using all old EU destination countries, and (3) using a subset with more detailed harmonized origin groups. Across

all three samples, the share of tertiary-educated non-EU migrants to Denmark does not systematically change with the presence of Start Aid. The initial introduction of Start Aid has little effect, and the share of highly educated migrants even increases when benefits are raised. Notably, the higher share persists when benefits are later cut. Comparing periods with and without Start Aid yields small and insignificant effects in all specifications (see Table A4).

A potential concern is selective return or onward migration, particularly for cohorts arriving before 2008, as I observe migrants several years after arrival. If low-skilled migrants who arrived before the reform left at similar rates as those deterred by the reform, this could bias estimates toward zero. To address this, I examine both recent arrivals (within two years) and the full migrant population during the 2012 repeal and 2015 reintroduction. Figure A20 indicates that, if anything, the skill level of recent arrivals rose during the high-benefit regime, while Figure A19 shows that the skill level of the overall migrant stock remained stable.

These findings are surprising, as they contradict the Borjas model. One explanation is Denmark's broader policy shift and anti-migrant rhetoric since 2002. Docquier and Vasilakis (2024) show that highly educated migrants are particularly sensitive to right-wing populism, which may explain their avoidance of Denmark during low-benefit periods.

V. Conclusion

In this comment, I revisited Agersnap, Jensen and Kleven (2020)'s study of the effect of welfare benefits on non-EU migration in Denmark. Using a methodology that avoids the limiations of AJK's analysis, I show that migration flow and stock elasticities are substantially smaller than the net migration elasticity reported by AJK. Moreover, I do not find any evidence that changes in welfare benefits changed the skill composition of migrants arriving in Denmark. These findings bring the Danish case in line with most other studies of the welfare magnet hypothesis. Most studies suggest that welfare benefits play a limited role in migrants' destination choice (De Giorgi and Pellizzari, 2009; Ferwerda, Marbach and Hangartner, 2024; Di Iasio and Wahba, 2024).

Why do benefits matter little in practice? For many visa types, migrants need to show proof of employment or studies, which often exclude benefit entitlement or provide other sources of (future) income. However, benefits could be a relevant determinant of location choice for asylum seekers in high-income countries, especially because refugees' face low employment rates in the first years after arrival (Fasani, Frattini and Minale, 2022). Yet, refugees are not entitled to unemployment benefits during the asylum procedure, and only receive those once (if at all) their asylum claim is approved, which can last between several months and several years (Hvidtfeldt and Schultz-Nielsen, 2022). This strongly reduces the expected value of benefits, particularly for individuals from countries with low recognition rates. Social benefits may matter for spousal family migrants,

many of whom women with low employment rates (Bredgaard and Ravn, 2021). However, a partner's income reduces benefit entitlement, which reduces the scope for welfare-dependent family migration. This is further reinforced by the reforms implemented in 2002, which require the sponsor to not have relied on social assistance in the previous year.

From the perspective of a policy maker of a single jurisdiction (such as a country in the European Union), a high migration-benefit elasticity suggests that benefit cuts are an effective tool in reducing (welfare-dependent) migration, which could trigger a race to the bottom. As welfare benefit cuts have been to shown to negatively affect economic and social outcomes of refugees and their children (Dustmann, Landersø and Andersen, 2024*a*,*b*), this could worsen (refugee) outcomes without strongly affecting the total number of refugees in the EU. My revised estimates may inform policymakers that benefit reductions are, in fact, not very effective in achieving the goal of reducing migration or increasing the average skill level of migrants.

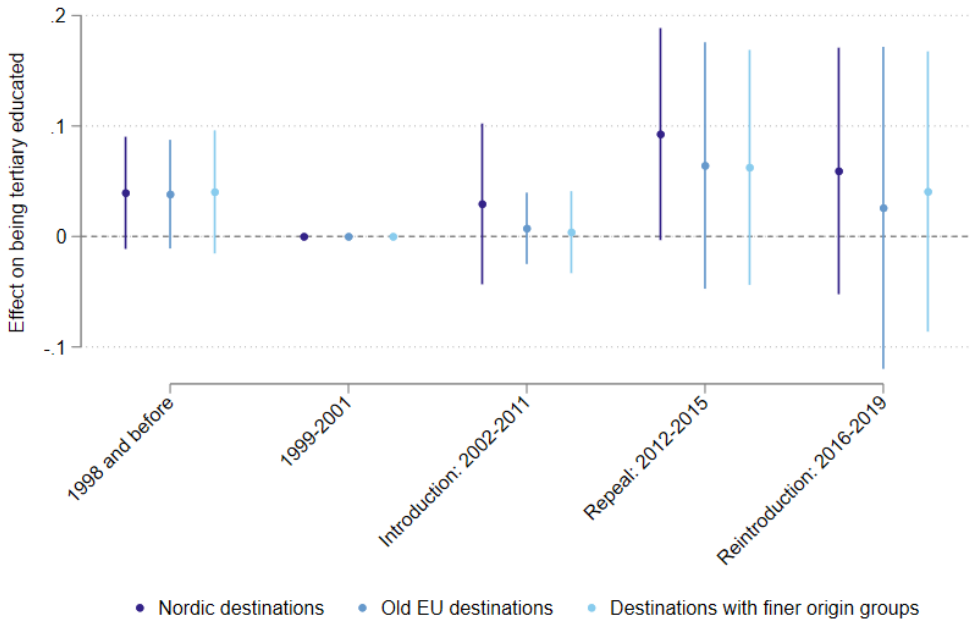

FIGURE 4. WELFARE BENEFITS AND MIGRANTS' EDUCATIONAL ATTAINMENT

Coefficient plot of individual-level regressions of a binary indicator for having a tertiary degree on a dummy for Denmark interacted with dummies for arrival cohorts corresponding to Start Aid Policy regimes, including fixed effects for origin region-by-cohort, time, and origin region-destination pairs. 95% confidence intervals are drawn based on standard errors clustered at the origin region-destination country group. The sample concerns all immigrants aged 25 to 59 upon arrival in the 2008-2023 EU Labor Force Survey. The three estimates differ in terms of included origin regions and destination countries: the first only includes 3 destination countries (Denmark, Finland, and Sweden) and five origin country groups (Middle East & North Africa, Sub-Saharan Africa, South- & South-East Asia, Latin America, and North America & Oceania), the second includes 17 destination countries and the same five origin country groups, and the third includes 14 destination countries (excluding Finland, France and Sweden) where 9 granular origin country groups (North Africa, Sub-Saharan Africa, Near & Middle East, East Asia, South-East Asia, North America, Central America, South America, and Oceania.) are available. Numerical estimates are reported in Table A4.

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A. Supplementary material

A.1. Section I: Critiques

Migration elasticities

To calculate the implied elasticity, AJK's multiply their regression estimate with the average level of benefits and divide by the net migration-stock ratio in 2001.[17] This identifies changes in migration flows relative to a pre-reform year (2001) rather than relative to the counterfactual in the same year. This is particularly consequential for the elasticity estimate when counterfactual net migration ratios are increasing over time, which is the case in Denmark (see Figure 2 of AJK). As the base of the percentage change is smaller than the counterfactual level, this overestimates the migration elasticity. In contrast, a log-log regression with group and time fixed effects, such as in Kleven et al. (2014), does identify the elasticity with respect to the contemporaneous counterfactual.

[17] In symbols: $\epsilon = \hat{\beta}\frac{E[\bar{B}_{2001}]}{E[\bar{Y}_{2001}]} = \frac{\Delta Y}{\bar{Y}_{2001}}\frac{\bar{B}_{2001}}{\Delta B}$

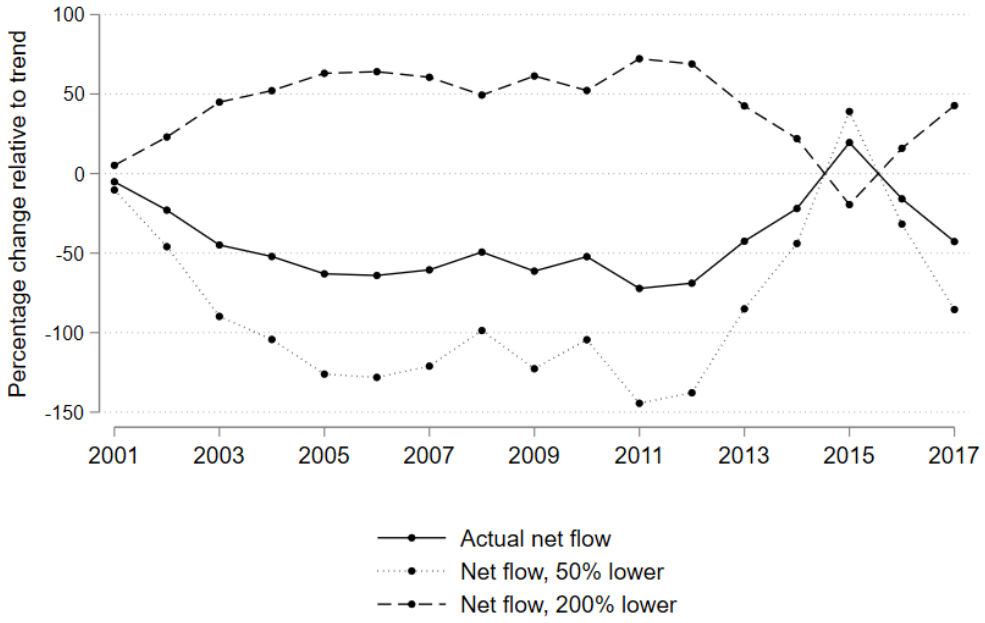


FIGURE A1. SENSITIVITY OF PERCENTAGE CHANGES TO THE LEVEL OF NET FLOWS

Notes: Percentage change in net non-EU migration in Denmark. Net migration is calculated using the stock-differencing approach of AJK, using migrant stocks data from Statistics Denmark table FOLK2. Following AJK, I restrict the data to individuals aged 30 and above and exclude Bosnia. Using 1980-2001 data I estimate a linear time trend in net migration. Following AJK, I extrapolate the time trend to 2002 and beyond, which serves as a counterfactual for migration absent the reform. In the two alternative scenarios the level of net flows across the entire period is changed by 50% (dotted line) and 200% (dashed line). This only changes the denominator used to calculate percentage changes in net flows.

MEASURING NET MIGRATION

I denote immigration by I, emigration by E, turning 30 by T, mortality by D, the stock of migrants measured at the start of the year by S, and net migration by N. I define the origin of a migrant in the same way as Statistic Denmark (see footnote 9). $IR_t^{o,r}$ ($ER_t^{o,r}$) denotes immigration (emigration) of migrants from origin o moving from (to) last residence r to (from) Denmark. For example, $IR_t^{nEU,EU}$ is migration of non-EU migrants previously residing in an EU country. These individuals are missing from non-EU migration flow statistics based on residence. Likewise, $IC_t^{o,c}$ ($EC_t^{o,c}$) is immigration (emigration) of migrants from (to) origin o with citizenship c to (from) Denmark. Net migration, net migration as calculated by AJK, and net migration measures based on net flows by residence

and citizenship can be written as:

$$N_t = I_t - E_t \tag{A1}$$

$$N_t^{AJK} = \Delta S_{t+1}^{30+} = I_t - E_t + T_t - D_t \tag{A2}$$

$$N_t^{res} = I_t - E_t - IR_t^{nEU,EU} + ER_t^{nEU,EU} + IR_t^{EU,nEU} - ER_t^{EU,nEU} \tag{A3}$$

$$N_t^{cit} = I_t - E_t - IC_t^{nEU,EU} + EC_t^{nEU,EU} + IC_t^{EU,nEU} - EC_t^{EU,nEU} \tag{A4}$$

Denmark had a young migrant population in 2001: $T \gg D$. Hence, AJK systematically overestimate net flows to Denmark. If $T_t - D_t$ is time-varying, it may impact AJK's estimates directly and through de-trending of the data based on pre-reform data. Both gross flow-differencing approaches based on publicly available data from Statistics Denmark approximate net migration up to cross-terms related to differential net migration of EU (including Danish) and non-EU migrants by residence and citizenship, respectively. For example, if many non-EU migrants only move to Denmark after having lived in another EU country first, the residence-based measure could underestimate non-EU migration to Denmark.

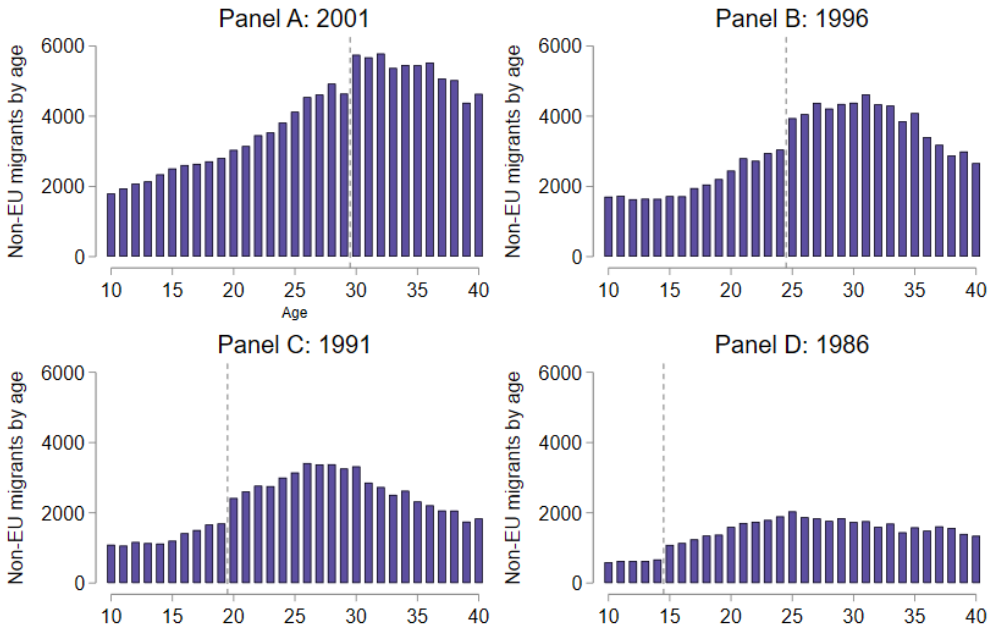
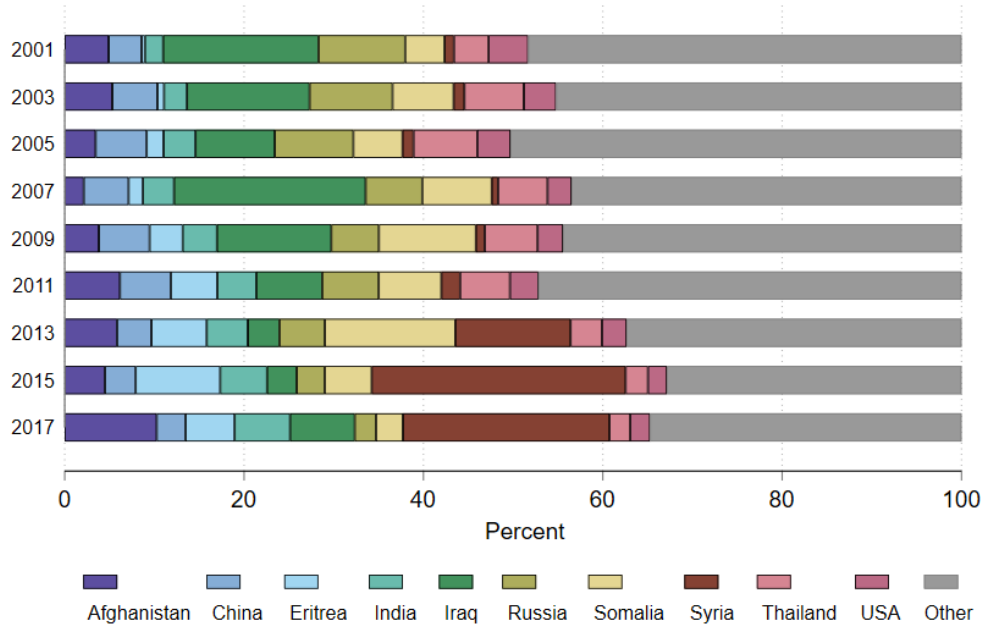


FIGURE A2. AGE DISTRIBUTION OF NON-EU MIGRANTS

Age distribution on non-EU residents in Denmark in 2001, 1996, 1991 and 1986. The dashed vertical lines indicate the boundary between those aged below and above 30 in 2001. We exclude individuals from Bosnia. Data from Statistics Denmark table FOLK2.

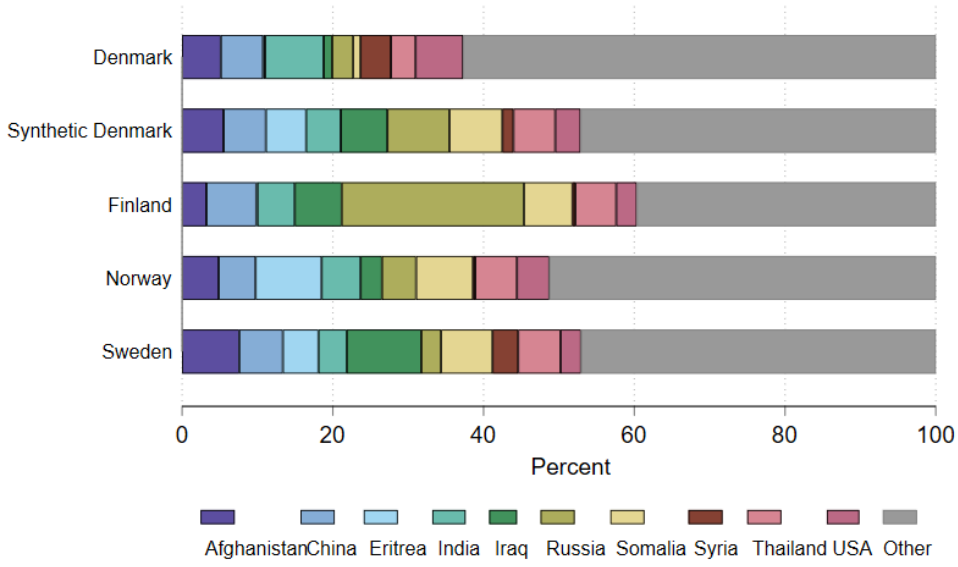
MIGRANT ORIGIN COMPOSITION

FIGURE A3. MIGRANT ORIGIN COMPOSITION OF FLOWS TO OTHER NORDIC COUNTRIES



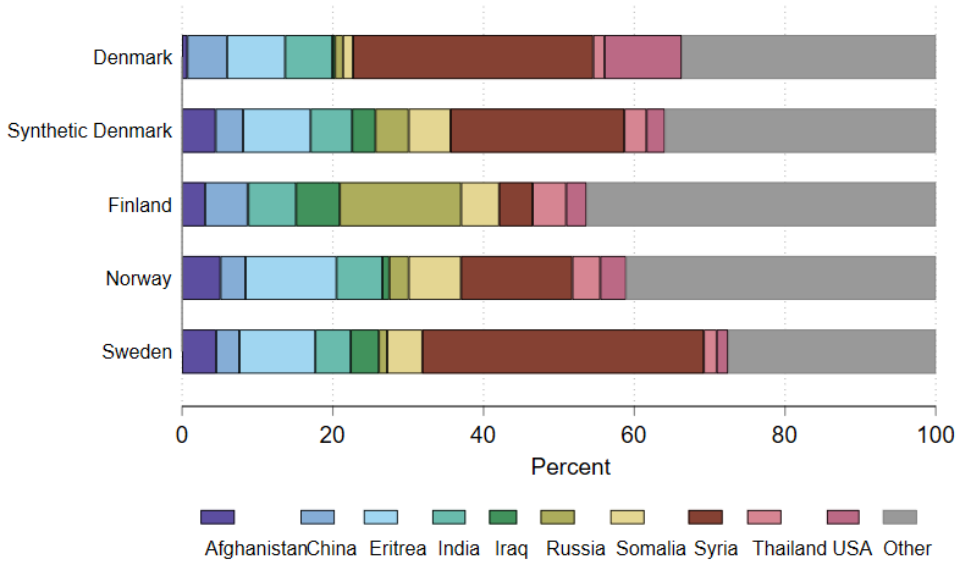
Notes: Bi-annual composition of non-EU immigration flows to Nordic countries excluding Denmark. In both panels, I explicitly show the 10 origin countries with the largest flow to all Nordic countries between 2001 and 2017. Data on migration inflows by country of origin are obtained from the OECD international migration database.

FIGURE A4. MIGRANT ORIGIN COMPOSITION IN 2011



Notes: Composition of non-EU immigration flows to Denmark, other Nordic countries and its synthetic control from AJK, in 2011. I explicitly show the 10 countries with the largest flow to Nordic countries between 2000 and 2017. Data from the OECD international migration database.

FIGURE A5. MIGRANT ORIGIN COMPOSITION IN 2015



Notes: Composition of non-EU immigration flows to Denmark, other Nordic countries and its synthetic control from AJK, in 2015. I explicitly show the 10 countries with the largest flow to Nordic countries between 2000 and 2017. Data from the OECD international migration database.

A.2. Section II: Coinciding migration policy changes

Family reunification

The Danish government implemented various changes to migration policy in 2002 and thereafter. AJK only address one: the ban on spousal family migration when either sponsor or spouse is younger than 24 years old. In addition to the minimum age requirement, since 2002 sponsor and partner needed to show strong affiliation to Denmark, and the sponsor has to provide for the household financially (Andersen, 2007). The sponsor should not have relied on social assistance in the prior year and had to provide a collateral of about EUR 7,500 and provide proof of adequate housing. Bratu et al. (2020) show that these policy changes have induced migration of singles from Denmark to Sweden, also among those 29 and older. The number of accepted spousal reunification requests for refugees dropped in the first half of 2001 from 1198 to 15 in the second half (after the reform) (Statistics Denmark, 2001). This strongly suggests that family reunification also became much more restrictive, also for those aged 24 and above.

Changing protection statuses

Around the initial introduction of Start Aid in 2002, Denmark also abolished the *de facto* protection status and introduced the *B-status*. Kjær (2003) writes: *"... asylum seekers who have so far been granted asylum as de facto refugees, but who will not be eligible for a residence permit in the future. This applies to the majority of the asylum seekers who rely on the risk of being called up for active war service as their motive for applying for asylum. If, however, in the event that they return to their country of origin, they risk the death penalty or being subjected to torture of to inhuman or degrading treatment or punishment, they are eligible for a residence permit as B refugees. Another example of asylum seekers who have been recognised as de facto refugees so far, but who will not be eligible for a residence permit any longer, are applicants who suffer from a considerable subjective fear."* As a consequence, the number of refugee permits issued decreased strongly. In 2001, of all asylum-related refugee permits granted, more than half (3,116) concerned the *de facto* status (Statistics Denmark, 2001). In 2004, two years after the reform, only 132 individuals were admitted on the B-status (Statistics Denmark, 2004), emphasizing that obtaining this status is

much harder for applicants not eligible for Geneva Convention status who would have previously been eligible for the *de facto* status.

Iraq and Afghanistan

The changing situation of two prominent origin countries in 2001 (Afghanistan and Iraq; see Figure 2) and changing origin-specific Danish policies also played an important role in the decreasing number of asylum statuses granted. The European Council on Refugees and Exiles (2003) writes about Afghanistan: *"Prior to the collapse of the Taliban regime, almost all Afghan asylum seekers claiming to be at risk of persecution by the Taliban were granted refugee status in Denmark. [...] According to current practice (from 2002 onwards), only asylum seekers who can establish that they have conflicted with the local warlords presently in charge are granted asylum in Denmark."* and Iraq: *"Until 2002, Iraqis originating from the government-controlled areas were almost systematically granted asylum, as the mere fact of having sought asylum abroad was considered to be a sufficient reason for protection in Denmark. [...] This practice was altered in the spring of 2002, and according to the Refugee Appeals Board's new approach, which is based on a January 2002 report by UNHCR, claims lodged by applicants from Iraq's government-controlled areas are to be examined on an individual basis, and only those applicants who have a persecution-based motivation for seeking asylum may be granted protection."*.

Decreasing recognition rates and increasing processing times

Asylum recognition rates decreased considerably after 2001. Figure A6 shows that recognition rates of asylum claims in Denmark, other Nordic countries and Denmark. Although recognition rates in other countries also decreased after 2001, the drop in Denmark is particularly pronounced. Figure A7 and A8 show that the drop in recognition rates of Afghanis and Iraqis is particularly strong relative to that in other countries in line with changes in the treatment of applicants from Iraq and Afghanistan. At the same time, asylum processing times also increased considerably, from just over 1 year for those receiving a residence permit in 2001, to more than 3 years in 2006 (Hvidtfeldt and Schultz-Nielsen, 2022). Asylum processing are typically dictated by the number of applications. The increase in processing times between 2001 and 2006 happened at a time of a relatively

low (and even decreasing) number of applications. Policy changes thus likely contributed to the increase in processing times.

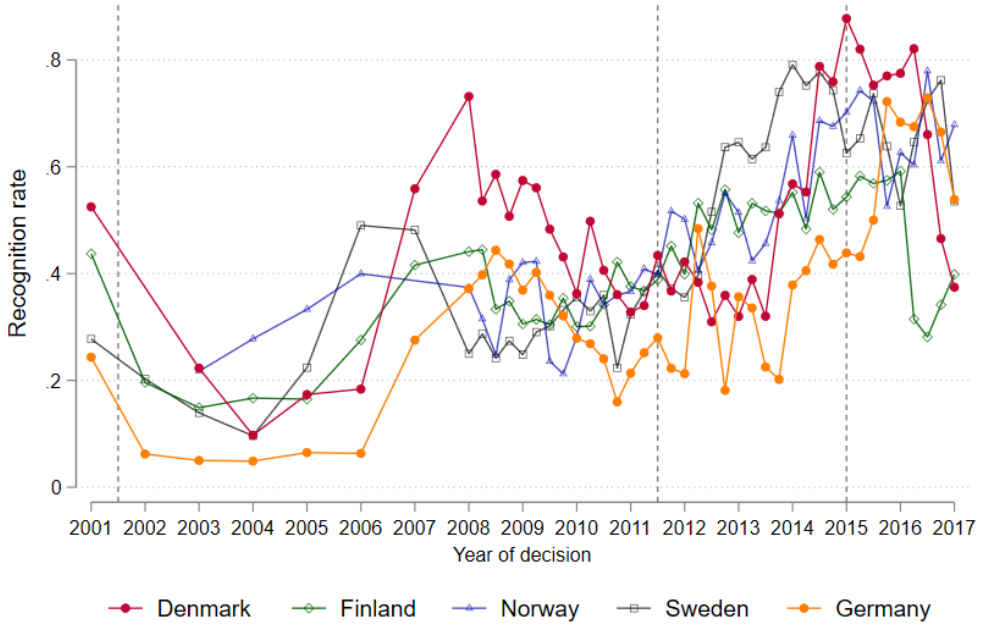


FIGURE A6. RECOGNITION RATES ACROSS SELECTED COUNTRIES OVER TIME

Share of asylum decisions that ended in a legal residence status across selected countries, by year of decision. Data obtained from Eurostat tables migr_asydcfina (1995-2007; yearly) and migr_asydcfstq (2008-2017; quarterly). Data from Denmark for 2002 is missing.

CASCADING EFFECTS FROM REFUGEE TO FAMILY MIGRATION

In 2001, 6,263 individuals received a refugee status and refugees sponsored 5,542 family migrants (Statistics Denmark, 2001), suggesting that every refugee sponsors approximately one family migrant. Together with the changes in refugee policies, this also implies that the base of recently recognized refugees to apply for family reunification decreased after 2002. Hence, policy-driven changes in the number of recognized refugees decrease non-EU migration further through reduced family reunification.

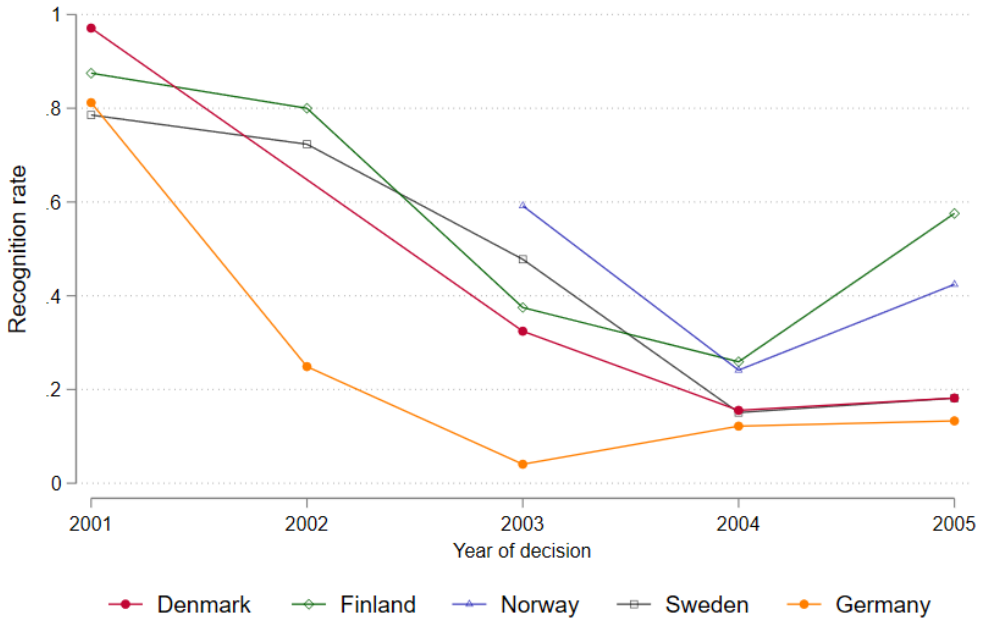


FIGURE A7. RECOGNITION RATES OF AFGHANIS ASYLUM APPLICANTS IN SELECTED COUNTRIES OVER TIME

Share of asylum decisions that ended in a legal residence status across selected countries, by year of decision. Data obtained from Eurostat table migr_asydcfina (1995-2007; yearly). Data from Denmark for 2002 is missing.

CHANGES DURING 2015 REFUGEE CRISIS

The Danish government introduced a new temporary status targeting Syrian refugees applying on 10th of December 2015 or later (Bendixen, 2025). The Article 7(3) status was only for valid for 1 year with subsequent 2-year extensions which only would be extended as long as the reason for flight persisted. Moreover, it does not allow for family reunification in the first 3 years and precluded access to higher education. Many refugees from Syria who otherwise would have the Geneva convention status now only received a weaker form of protection. In 2016, one third of the residence permits granted on asylum grounds were according to Article 7(3) (Statistics Denmark, 2015). Around the same time, Denmark introduced a controversial law to confiscate all assets from refugees in excess of 1,300 EUR (BBC News, 2016).

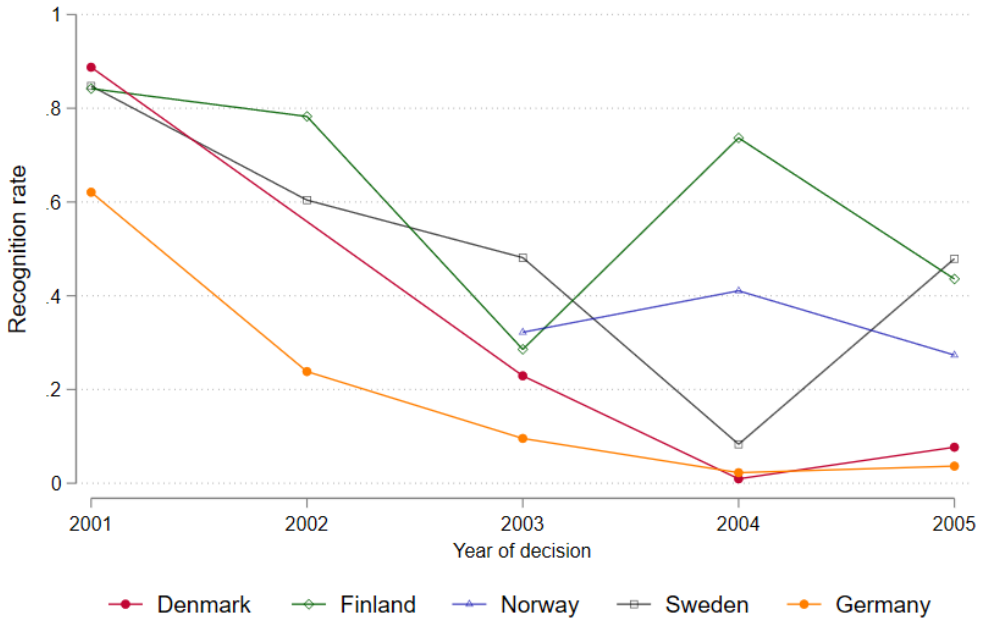


FIGURE A8. RECOGNITION RATES OF IRAQI ASYLUM APPLICANTS IN SELECTED COUNTRIES OVER TIME

Share of asylum decisions that ended in a legal residence status across selected countries, by year of decision. Data obtained from Eurostat table migr_asydcfina (1995-2007; yearly). Data from Denmark for 2002 is missing.

ASYLUM FROM ABROAD

There are two pathways to refugee status without first setting foot in the receiving country: asylum applications at diplomatic missions abroad and refugee resettlement (quota refugees). The Danish government completely ceased to accept asylum applications from abroad at Danish diplomatic missions in 2002. Figure A9 shows that up to 2002 a considerable share of asylum applications was filed from abroad (19% in 2001), but since 2002 it has been zero. Figure A10 shows that the number of quota refugees accepted by Denmark did not change drastically between 2001 and 2015. However, in 2016 Denmark suspended the quota refugee program altogether.

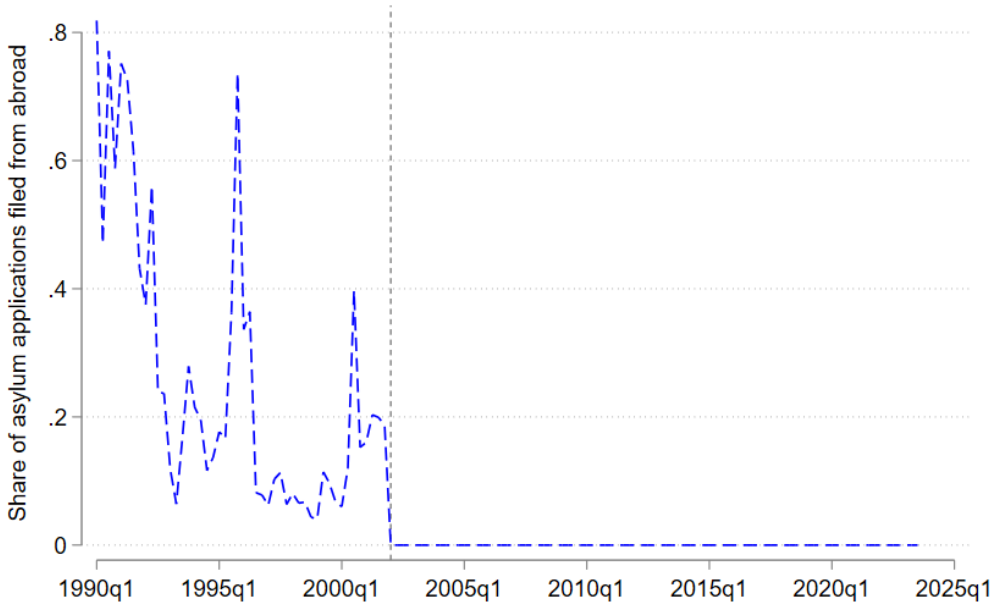


FIGURE A9. SHARE OF ASYLUM APPLICATIONS FILED ABROAD

Share of asylum applications filed abroad. Data from Statistics Denmark table VAN5.

MECHANICAL AND INDIRECT EFFECTS OF POLICIES

The aforementioned policies led to reduced prospects of receiving refugee protection and family migration. These have two distinct effects. First, some of these have a mechanical effect on the number of asylum-related residence permits granted, even if the number of people willing to apply for asylum in Denmark has remained unchanged. Examples of these are the halting of asylum applications from abroad, the abolition of the *de facto*-status and the changing treatment of Afghanis and Iraqis. However, without detailed information of refugee and family migration cases it is impossible to reliably estimate how much of the drop if refugee and family migration is mechanical due to the these changes.

Second, these policies may have decreased the appeal of applying for asylum in Denmark. For example, the likelihood of obtaining asylum and the pace of the asylum process may shape asylum seekers' decision where to apply (Diop-Christensen and Diop, 2022; Di Iasio and Wahba, 2024). Beyond actual policies, a government's stance and rhetoric could also matter for migrants' destination

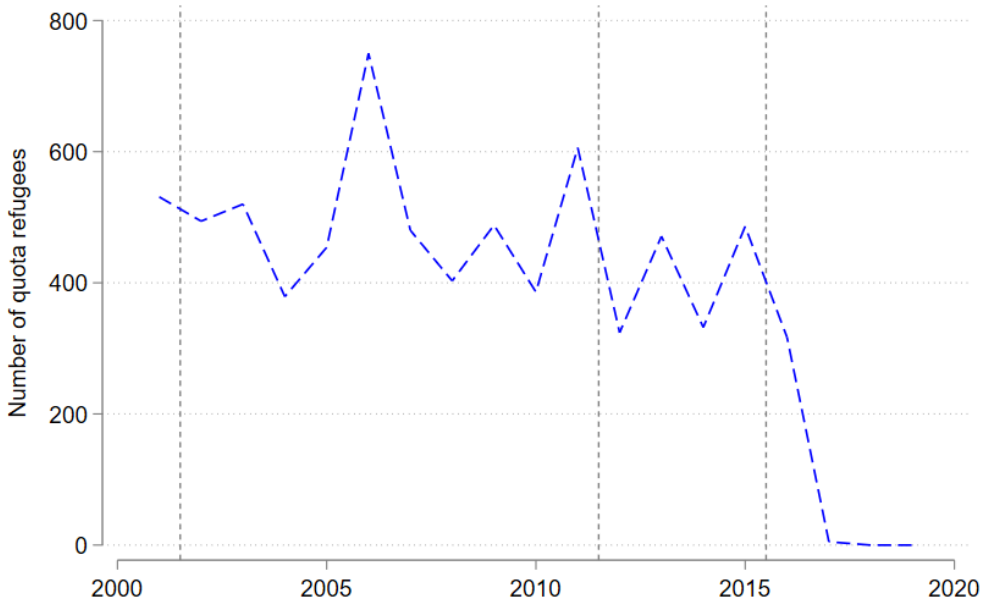


FIGURE A10. NUMBER OF QUOTA REFUGEES

Number of quota refugees from European Council on Refugees and Exiles (2003) for 2001 and 2002 and United Nations High Commissioner for Refugees (UNHCR) from 2003 onwards.

choice. The 2001 elections were held shortly after the 9/11 terrorist attacks, many parties shifted towards more anti-immigration platforms and curbing immigration was one of the main targets of Denmark's newly elected government (Andersen, 2003). With the government's support of the far-right, its public image as a country welcoming immigrants faded. Docquier and Vasilakis (2024) provide suggestive evidence that the effect of government ideology on immigration is not only mediated through immigration policies, suggesting that rhetoric and public image may too influence immigrants' destination choices.

REFUGEE POLICIES IN OTHER EUROPEAN COUNTRIES

As AJK's second approach compares non-EU migration in Denmark to those in other Nordic countries, policy changes in these countries could also affect the estimates. Figure A11 shows the restrictiveness of migration policy changes following the approach suggested by Di Iasio and Wahba (2024) based on the DEMIG

database between 2001 and 2017. During the low-benefit regime in Denmark between 2002 and 2011, migration policy became considerably more lenient in Finland and Sweden. The increasing attractiveness of Finland and Sweden may have increased net migration flows to these countries relative to Denmark for reasons unrelated to policy changes in Denmark. During the Syrian refugee crisis, multiple countries adopted more lenient policies. For example, Sweden granted permanent residence to Syrian refugees (Andersson and Jutvik, 2023) in September 2013 and Germany's Merkel administration signaled a welcoming stance to refugees in August 2015 and stopped enforcing the Dublin procedure, among others (Pries, 2020).

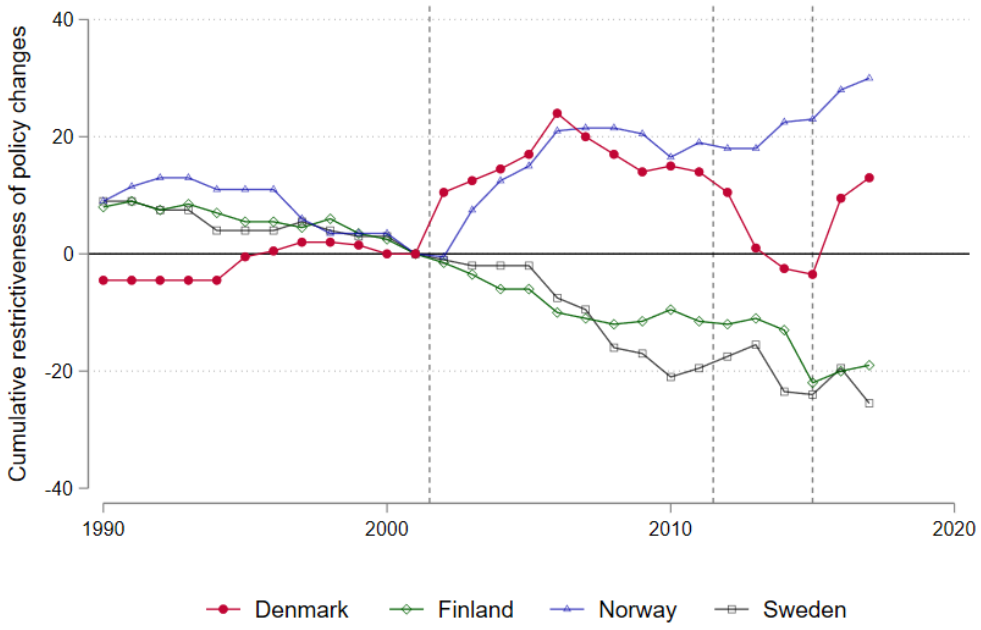


FIGURE A11. CHANGES IN MIGRATION POLICY ACROSS NORDIC COUNTRIES

Cumulative count of restrictiveness of migration policy changes in the DEMIG database (Schreier, Skrabal and Czaika, 2023) relative to 2001. I include all policy changes that entail a change in restrictiveness. Following Di Iasio and Wahba (2024), I assign -1 to a policy change if it became less restrictive, +1 if it became more restrictive. I multiply this number by 1.5 is policies entailed major changes. I exclude policies tailored only to own citizens, EU citizens or only specific nationalities.

A.3. Section III: origin-specific re-analysis

Origin-specific counterfactuals

This section describes how I construct counterfactuals for observed post-reform flows and stocks by origin country introduced in section III. In line with the data from Statistics Denmark and the OECD international migration database, I define migrants based on country of origin rather than country of citizenship; naturalization does not change migration status and thus does not play a role in the analysis. The stock of migrants, registered at the beginning of the year is denoted by S. Net migration M equals immigration I minus emigration E. Net migration from o to Denmark at t can be decomposed as follows:

$$\text{(A5)} \quad M_{o,DK,t} = I_{o,DK,t} - E_{o,DK,t} = I_{o,t}\frac{I_{o,DK,t}}{I_{o,t}} - S_{o,DK,t}\frac{E_{o,DK,t}}{S_{o,DK,t}} = I_{o,t}i_{o,DK,t} - S_{o,DK,t}e_{o,\neg DK,t}\frac{e_{o,DK,t}}{e_{o,\neg DK,t}}$$

I denote the set of reference countries, excluding Denmark itself, by $\neg DK$. Hence, total migration to all reference countries is denoted by $I_{o,t} = I_{o,DK,t} + I_{o,\neg DK,t}$. Immigration can be written as the product of total migration from o and the share of migrants from o going to Denmark, $i_{o,DK,t}$. Emigration can be written as the product of the migrant stock in Denmark in the beginning of the year multiplied by the emigration rate $e_{o,DK,t}$, which can be further factorized into the time-varying emigration rate across reference countries and the ratio of emigration rates in Denmark compared to reference countries. I make the following assumptions:

1) Total migration from o, $I_{o,t}$, is unaffected by reform in Denmark

2) The share of migrants choosing Denmark, $i_{o,DK,t}$, is unchanged absent reform in Denmark

3) Propensity to emigrate from reference countries, $e_{o,\neg DK,t}$, unaffected by reform in Denmark

4) The propensity to emigrate from DK relative to that from reference countries, $\frac{e_{o,DK,t}}{e_{o,\neg DK,t}}$, is unchanged absent reform in Denmark

As Denmark is small compared to other destination countries, changing conditions in Denmark are unlikely to affect total migration from non-EU countries to all EU countries (Assumption 1) and emigration rates of non-EU migrants residing in other EU countries (Assumption 3). Even if this is violated, the impact is arguably small as for a given non-EU origin country the share of migrants going to Denmark and the number of non-EU migrants moving from EU countries onward to Denmark are small.

I also assume that in absence of Start Aid the propensity to migrate to Denmark ($I_{o,t}$) would have been unchanged relative to the last pre-reform year (Assumption 2). In addition, I assume that the ratio of emigration rates from Denmark relative to that from other EU countries (Assumption 4) would have been unchanged. This is plausible as return migration is predominantly driven by origin-country shocks rather than shocks in other destinations, as most emigration of non-EU citizens in EU is return migration rather than onward migration to a third country. Both of these assumptions require that there are no other policy changes in Denmark or in the reference countries between the last-pre reform period and the period of interest.

The assumptions above are origin country-specific. As the main question of interest concerns *aggregate* non-EU migration to Denmark, violation of these assumptions for single origin countries could occur, but no systematic violations such that aggregate quantities are affected are allowed (by taking the expectation of the sum over all origin countries). Hence, the assumptions are jointly sufficient but not necessary condition for my estimates to be unbiased. Using Assumptions 1-4, origin-specific immigration, counterfactual emigration and the migrant stock read as follows:

$$I^{cf}_{o,DK,t} = I_{o,t} i_{o,DK,t_0} \tag{A6}$$

$$E^{cf}_{o,DK,t} = S^{cf}_{o,DK,t} e_{o,\neg DK,t} \frac{e_{o,DK,t_0}}{e_{o,\neg DK,t_0}} \tag{A7}$$

$$S^{cf}_{o,DK,t} = S^{cf}_{o,DK,t-1} + I^{cf}_{o,DK,t-1} - E^{cf}_{o,DK,t-1} \tag{A8}$$

Here, $S^{cf}_{o,DK,t_0} = S_{o,DK,t_0}$. I obtain the counterfactuals up to a horizon $h = t - t_0$ by iteratively evaluating expressions A2-A4 for every horizon h. Using counterfactual $X^{cf}_{o,DK,t}$ and observed data $X_{o,DK,t}$ we calculate percentage changes as

$\frac{X_{o,DK,t}-X^{cf}_{o,DK,t}}{X^{cf}_{o,DK,t}}$. This approach hinges on the availability of data across destination countries and requires an appropriate choice of reference countries. As detailed in- and out-migration data by origin country over time is not available for all countries, I rely on a set of 12 European countries in my main analysis. These are Austria, Germany, Denmark, Spain, Finland, France, Italy, Iceland, Luxembourg, Netherlands, Norway and Sweden.

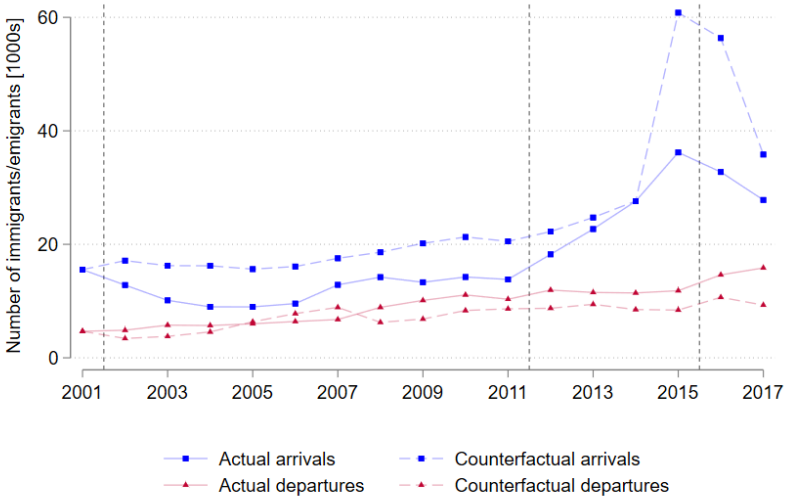


FIGURE A12. ACTUAL AND COUNTERFACTUAL NON-EU MIGRATION FLOWS

Notes: Actual and counterfactual in- and outflows between non-EU countries and Denmark. For details on the analysis and the data, see notes to Figure 3.

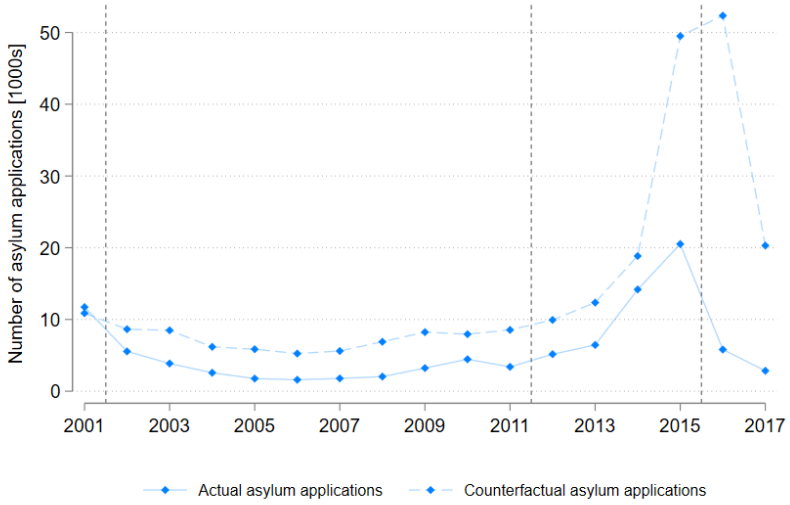


FIGURE A13. ACTUAL AND COUNTERFACTUAL ASYLUM APPLICATIONS FLOWS

Notes: Actual and counterfactual asylum applications from non-EU countries in Denmark. For details on the analysis and the data, see notes to Figure 3.

FIGURE A14. ACTUAL AND COUNTERFACTUAL NON-EU MIGRATION STOCKS

Notes: Actual and counterfactual migration stocks of non-EU individuals in Denmark. For details on the analysis and the data, see notes to Figure 3.

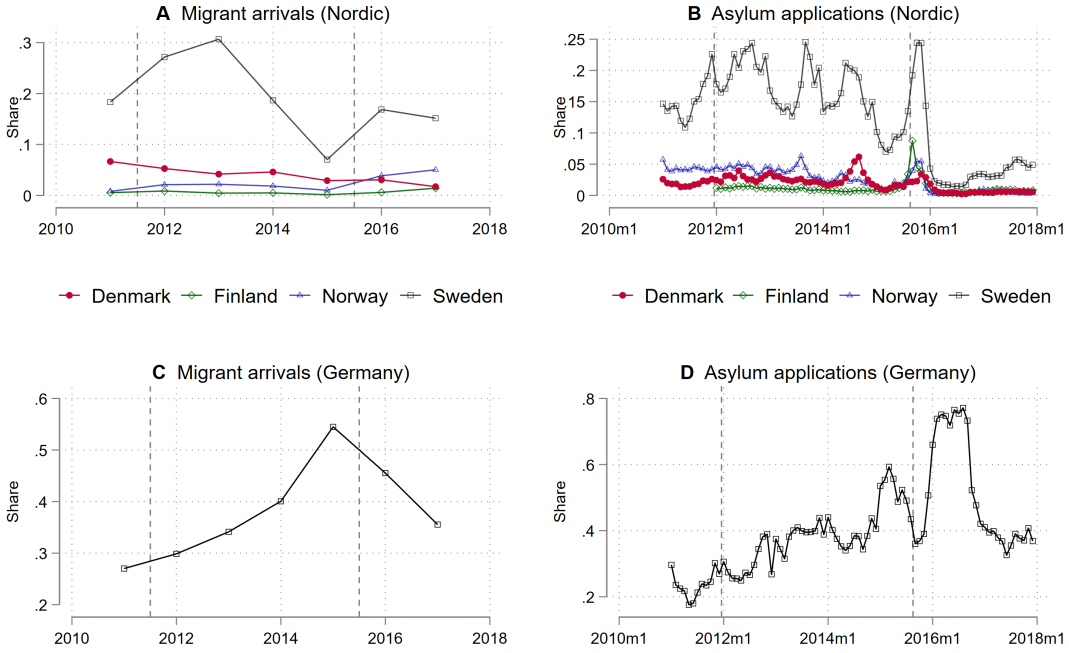


FIGURE A15. SHARE OF MIGRANT ARRIVALS AND ASYLUM APPLICATIONS TO NORDIC COUNTRIES AND GERMANY.

Notes: Share of all migrant arrivals (A and C) and all asylum applications (B and D) in 12 reference countries (see the main text for the list of countries) going to particular countries (A and B: Nordic countries, C and D: Germany). Data on arrivals originates from the OECD international migration database and on asylum applications from Eurostat tables migr_asyctzm and migr_asyappctzm.

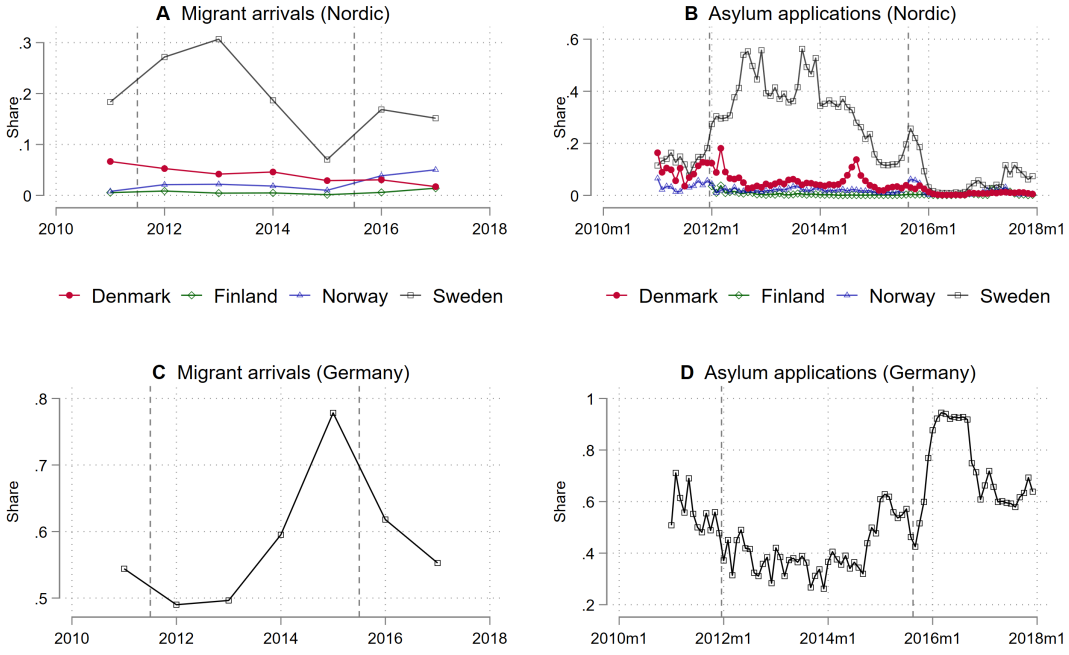


FIGURE A16. MIGRANT ARRIVALS AND ASYLUM APPLICATIONS FROM SYRIA TO NORDIC COUNTRIES AND GERMANY.

Notes: Share of migrant arrivals from Syria (A and C) and asylum applications submitted by Syrians (B and D) in 12 countries (see the main text for the list of countries) going to particular countries (A and B: Nordic countries, C and D: Germany). Data on arrivals originates from the OECD international migration database and on asylum applications from Eurostat tables migr_asyctzm and migr_asyappctzm.

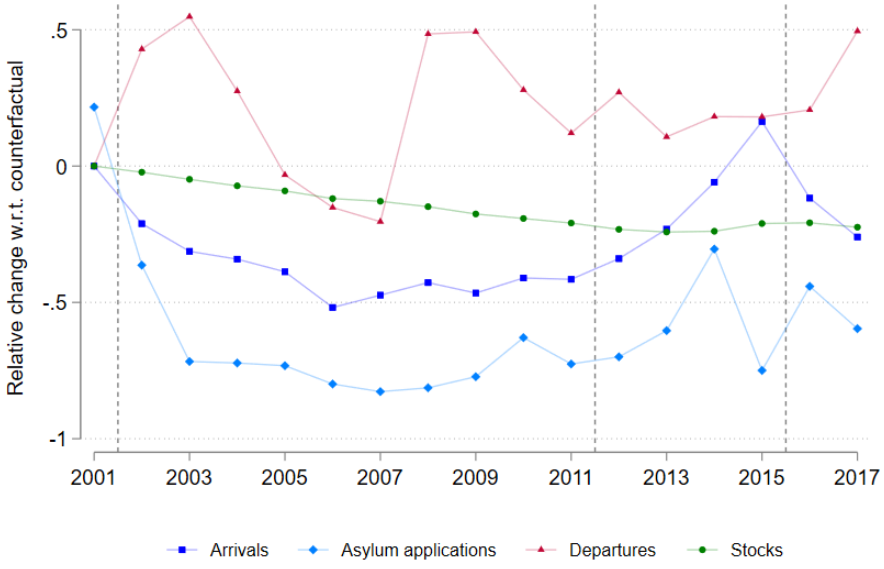


FIGURE A17. CHANGES IN NON-EU MIGRATION RELATIVE TO ORIGIN-BASED COUNTERFACTUAL USING NORDIC REFERENCE COUNTRIES

Notes: Relative differences between actual and counterfactual migrant arrivals, departures, stocks and asylum applications between 2001 and 2017. See notes to Figure 3 for details on the data and methodology. Only Nordic countries are used as reference countries.

TABLE A1—RELATIVE CHANGES AND ELASTICITIES DERIVED FROM ORIGIN-SPECIFIC COUNTERFACTUALS USING NORDIC REFERENCE COUNTRIES

	(1) Arrivals	(2) Departures	(3) Stock (last year)	(4) Asylum applications
Low benefits (2002-2011)	-0.410	0.174	-0.209	-0.746
	(0.095)	(0.178)	(0.065)	(0.045)
Observations	1180	992	125	5175
High benefits (2012-2015)	0.234	0.124	0.039	-0.268
	(0.149)	(0.144)	(0.020)	(0.252)
Observations	457	460	115	1627
Low benefits (2016-2017)	-0.242	0.268	-0.067	-0.133
	(0.121)	(0.075)	(0.034)	(0.147)
Observations	269	227	152	1166
Implied elasticity	0.703	-0.265	0.262	0.454
	(0.112)	(0.163)	(0.060)	(0.447)
Observations	1906	1679	392	7968

Notes: Alternative to the results shown in Table 1 only using other Nordic countries as reference destination countries. See notes to Table 1 for details on the data and analysis.

Table A2—Relative changes and elasticities derived from origin-specific counterfactual using "old" EU origin countries

	(1) Arrivals	(2) Departures	(3) Stock (last year)
Low benefits (2002-2011)	0.206	0.381	0.169
	(0.126)	(0.087)	(0.113)
Observations	130	140	14
High benefits (2012-2015)	0.442	-0.018	0.102
	(0.084)	(0.039)	(0.024)
Observations	56	56	14
Low benefits (2016-2017)	0.016	0.158	-0.055
	(0.032)	(0.036)	(0.012)
Observations	28	28	14
Implied elasticity (by regime)	0.240	-0.370	0.024
	(0.073)	(0.056)	(0.054)
Observations	214	224	42

Notes: Alternative to the results shown in Table 1 using instead EU-15 countries minus Denmark as origin countries. Asylum applications are omitted as very few asylum applications originate from individuals from other EU countries. See notes to Table 1 for details on the data and analysis.

Table A3—Relative changes and elasticities derived from origin-specific counterfactual (median regression)

	(1) Arrivals	(2) Departures	(3) Stock (last year)	(4) Asylum applications
Low benefits (2002-2011)	-0.560	0.155	-0.099	-0.658
	(0.143)	(0.270)	(0.100)	(0.140)
Observations	122	102	125	57
High benefits (2012-2015)	-0.103	-0.042	0.013	-0.425
	(0.337)	(0.117)	(0.013)	(0.064)
Observations	115	115	115	43
Low benefits (2016-2017)	-0.017	0.115	-0.013	-0.781
	(0.087)	(0.097)	(0.006)	(0.203)
Observations	137	114	152	48
Implied elasticity	0.411	-0.579	0.081	0.420
	(0.267)	(0.205)	(0.020)	(2.029)
Observations	146	130	152	69

Notes: Alternative to the results shown in Table 1 estimation using a median regression on the average changes by origin country across years with robust standard errors. To estimate a median regression with weights using the Stata-command qreg, I first aggregate the data on the origin country level. See notes to Table 1 for details on the data and analysis.

A.4. Section IV: Migrant skill composition

TABLE A4—WELFARE BENEFITS AND MIGRANTS' EDUCATIONAL ATTAINMENT

	(1)	(2)	(3)
1998 and before	0.040	0.038	0.040
	(0.024)	(0.025)	(0.028)
1999-2001	0.000	0.000	0.000
	(.)	(.)	(.)
Introduction: 2002-2011	0.029	0.007	0.004
	(0.034)	(0.016)	(0.019)
Repeal: 2012-2015	0.093*	0.064	0.063
	(0.045)	(0.056)	(0.054)
Reintroduction: 2016-2019	0.059	0.026	0.041
	(0.052)	(0.073)	(0.064)
Observations	97028	441654	338616
R^2	0.09	0.14	0.18
Average dependent variable	0.439	0.353	0.334

Notes: Numerical results underlying Figure 4. For details on the sample and data, see notes to Figure 4.

A.5. Inference

To estimate the effect of benefits on migration, AJK regress net flows on benefit levels and group- and year fixed effects on a panel of 12 benefit groups between 1980 and 2017, net of a trend estimated from 1980-2017 data.[18] AJK calculate heteroskedasticity-robust standard errors that account for estimation noise introduced by de-trending the data. However, their inference does not account for serial correlation within groups and cross-sectional dependence across benefit groups. As treatment is assigned on the origin country level and persists over time, inference should account for this (Abadie et al., 2023).[19] Not doing so can be consequential for two reasons. First, migration flows are strongly serially correlated and the treatment only changes three times over a 37-year period. Second, migration flows of different benefits groups are likely positively correlated as they originate from the same origin countries and are subject to the same origin-specific push factors. Hence, the standard errors reported by AJK considerably underestimate the uncertainty. However, as I do not have access to the micro-data from Statistics Denmark necessary to calculate the number of migrants by benefit group, I can not assess by how much standard errors would increase when accounting for this.

[18] Benefit levels differ by benefit groups are stratified over marital status, the number of children (0, 1, 2+) and country of origin (old EU- and non-EU). Only the non-EU family groups face changing benefit levels over time.

[19] Although there are difference in the extent of benefit changes between benefit groups, the vast majority of variation is within groups over time.

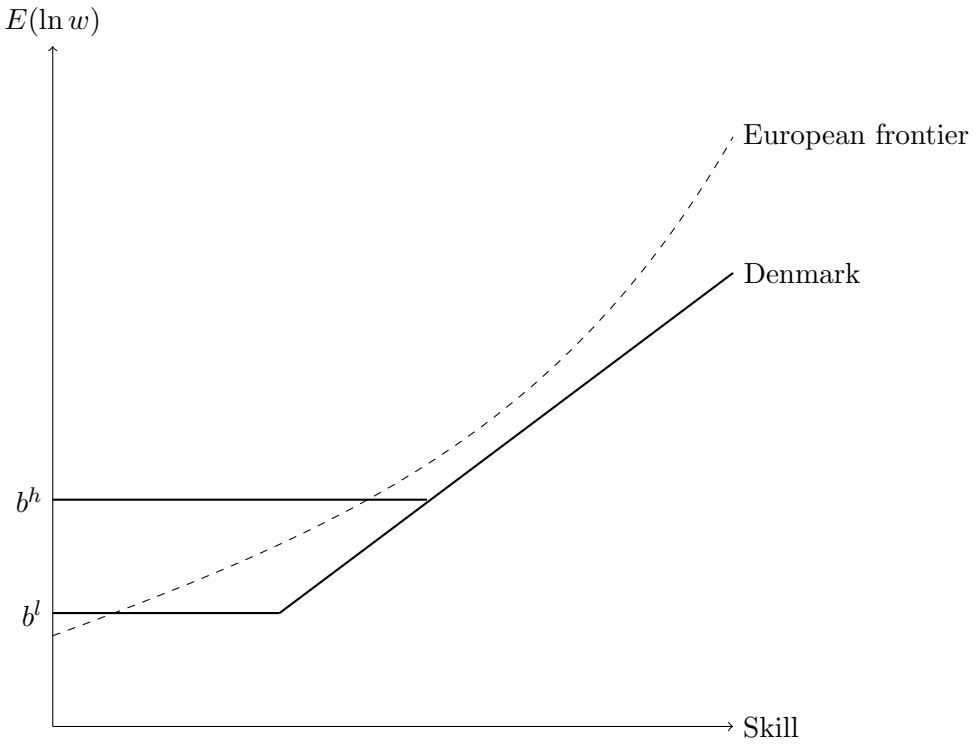


FIGURE A18. SORTING OF MIGRANTS INTO DENMARK

This Figure illustrates how migrants with different skill levels sort into Denmark by simplifying the Borjas (1999) model. The y-axis shows expected income; the x-axis represents migrant skill. The solid curve depicts the income-skill relationship in Denmark, with a lower bound determined by welfare benefits. The dashed curve represents the income frontier in other European countries, assumed to be convex and above Denmark's curve, reflecting countries with similar average incomes but differing returns to skill. This is realistic as private returns to education in Denmark are moderate relative to other European countries (Broecke, 2015; Psacharopoulos and Patrinos, 2018), and pre-tax returns to skills are relatively low (Hanushek et al., 2015). Denmark's labor taxes are lower than those in many Central European countries and comparable to other Nordic countries (Enache, 2024).
Assuming that the individual-specific utility of living in Denmark and other European countries is uncorrelated and independent of the skill level, the likelihood that an income-maximizing migrant chooses Denmark is proportional to the vertical distance between the two income curves. A reduction in welfare benefits from b^h to b^l lowers the appeal of Denmark for low-skilled migrants but not for high-skilled migrants. The model thus predicts that Start Aid increased the average skill level of migrants.

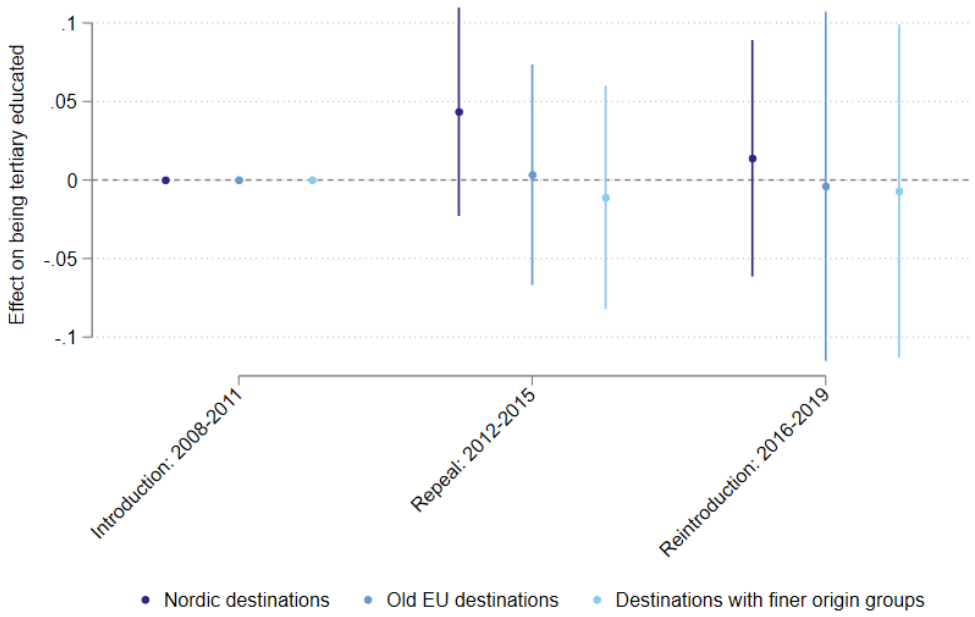


FIGURE A19. START AID'S EFFECT ON EDUCATIONAL ATTAINMENT OF THE MIGRANT POPULATION

Coefficient plot of individual-level regressions of a binary indicator for having a tertiary degree on a dummy for Denmark interacted with dummies for time periods corresponding to Start Aid Policy regimes, including fixed effects for origin region-by-cohort, time, and origin region-destination pairs. 95% confidence intervals are drawn based on standard errors clustered at the origin region-destination country. For details on the samples and data, see notes to Figure 4. This Figure restricts the sample to those who arrived between 2008 and 2019.

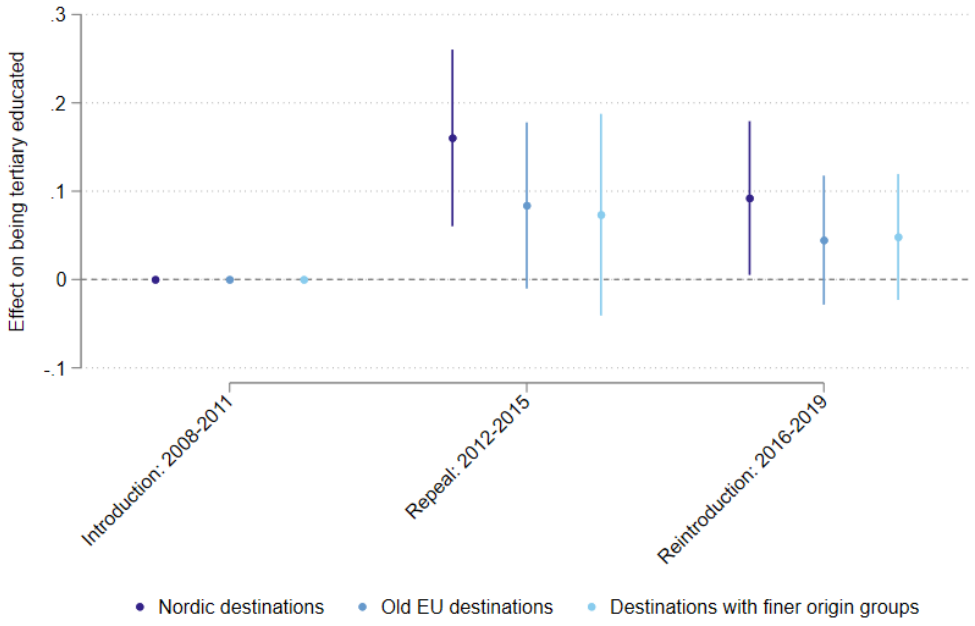


FIGURE A20. START AID'S EFFECT ON EDUCATIONAL ATTAINMENT OF RECENTLY ARRIVED MIGRANTS

Coefficient plot of individual-level regressions of a binary indicator for having a tertiary degree on a dummy for Denmark interacted with dummies for arrival cohorts corresponding to Start Aid Policy regimes, including fixed effects for origin region-by-cohort, time, and origin region-destination pairs. 95% confidence intervals are drawn based on standard errors clustered at the origin region-destination country group. For details on the samples and data, see notes to Figure 4. This Figure restricts the sample to those who arrived between 2008 and 2019 and were interviewed within two years of arrival.