# Permanent Residency Policy and Skilled Immigration: Evidence from a Swedish Reform*

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**Abstract**

Aging populations and labor shortages in skill-intensive sectors have led many countries to pursue targeted policies to attract international talent. We study a migration reform in Sweden that offered international doctoral students from outside the EU an easier path to permanent residency. Implemented in 2014, the reform shortened the required period of residence from eight to four years, allowed these students to obtain permanent residency immediately after graduation, and granted their spouses a work permit during their doctoral studies. Using the European students as a comparison group in a difference-in-differences design, we find that the treated international students are 13.5 pp (23%) more likely to stay in Sweden three years after graduation. Higher settlement prospects also increase their language investments and marriage rates during the PhD. These effects are larger for cohorts that have longer exposure to the reform and for those who carry out their doctoral research in STEM. In addition, the reform raises both employment and language investments among the partners of the treated international students. Taken together, the policy increases permanent residency among international graduates as well as leads them and their families to make long-term choices conducive to integration.

**JEL codes**: F22, I23, J12, J24, J61, K37

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## 1 Introduction

High-skilled workers are important drivers of innovation.[1] Skilled international migrants, particularly those in STEM, can contribute to a country's productivity growth (Stuen et al., 2012; Peri et al., 2015).[2] In recent decades, many countries have implemented targeted policies to attract and retain international talent (e.g., the OPT visa extension program for STEM students in the US).[3] These policies are particularly important for countries with aging populations and labor shortages in sectors with high growth potential. Such demographic and labor market challenges have been recognized by many OECD countries, which have responded with measures such as tax incentives and targeted work permits for high-skilled immigrants (Czaika, 2018).[4]

In this context, international university students emerge as an important group in achieving the goal of increasing the supply of high-skilled workers. By attracting and educating these individuals, universities play a pivotal role in providing a potential pool of high-skilled labor to the host country's economy. However, the rewards of these efforts depend on how many of these students choose to stay in the country in the long term (Beine et al., 2023). Therefore, policies that remove obstacles and create incentives for international graduates to remain and contribute to the economy hold the promise of yielding significant returns.

How does a change in permanent residency prospects affect international doctoral students? The first outcome of interest is their settlement in the host country after graduation. There is a growing body of work exploring the role of preferential tax schemes in encouraging high-skilled immigration (Kleven et al., 2020). However, little is known about the effectiveness of other policy instruments such as those changing the possibility of permanent residency or citizenship. This is important as residency or citizenship prospects potentially play a major role in the location choice of high-skilled immigrants, and many OECD countries consider the associated rules as a key policy instrument in attracting foreign talent (Kaushal and Lanati, 2019). Moreover, residency prospects likely influence other critical decisions such as investment in country-specific skills and family

[1] Following the literature, we use the terms 'high-skilled' and 'skilled' interchangeably.

[2] STEM represents the fields of Science, Technology, Engineering, and Mathematics.

[3] A recent example from the EU is the ongoing effort by Germany to ease its citizenship rules. The proposed reform shortens the required period of residence from eight to five years, simplifies the naturalization process, and allows for dual citizenship.

[4] These policies have drawn special attention in countries in the European Union as they attempt to compete with traditional immigrant destinations such as Australia, Canada, the United States, and the United Kingdom.

formation. Yet, there is limited knowledge on how permanent residency policies affect these choices among skilled immigrants.

A Swedish reform in 2014, which took three months from proposal to implementation, offered an easier pathway to permanent residency for international doctoral students i.e., those coming from outside the European Union or the European Economic Area (hereafter, EU/EEA)[5]. This reform made three key changes in the protocols to obtain a permanent resident permit (PRP): instead of having to work for at least four more years after graduation, an international student would be eligible for a PRP immediately after graduation[6]; their spouses or partners would receive a work permit for the entire duration of the doctoral studies; and the family members could apply for a PRP together with the doctoral student.

As the reform applies only to international students and not to their peers who are either Swedes or migrants from other countries in EU/EEA, we use these peers as a comparison group in a difference-in-differences (DID) design to study how the reform affects the international students who graduate after the reform.[7][8] We compile a dataset covering the universe of doctoral students graduating between 2008 and 2017[9] and the choices they make in the following domains: residency in Sweden after graduation, language investments, and family formation, with the last two outcomes measured during the duration of their doctoral studies. For these students' partners, the reform relaxes a constraint for labor market entry and allows them to apply for permanent residency together with the students. Therefore, as a secondary analysis, we also assess whether the reform has an effect on language learning and employment of these partners.

We find that the treated international students are 13.5 percentage points (23%; $p < 0.01$) more likely to stay in Sweden three years after graduation. As Figure 1 shows, the average residency rates for the treatment and comparison groups evolve in parallel

[5] Students from Switzerland also have the same residency rights as those from EU/EEA.

[6] As long as they hold a residence permit for doctoral studies in Sweden for a total of four years within a seven-year period, a condition that rarely binds for these students.

[7] To be precise, to account for anticipation, the first cohort of international students that we consider as ´treated' are those who graduate in 2013.

[8] One may argue that European students are a better comparison group as they are educational migrants as well and probably operate in a choice environment more comparable to that faced by their international peers. However, as we show later, Swedish and international students have similar levels for outcomes such as marriage. Regardless, whenever applicable, we present results separately excluding Swedish students from the comparison group. Except for fertility, these choices of different comparison groups do not have any substantive impact on our results.

[9] These international students start their PhD in years between 1998 and 2013 i.e., before the reform, and therefore are unlikely to be selected in anticipation of the reform.

leading up to the reform but diverge afterward. This divergence is particularly notable for the comparison between international students and their non-native European peers. These two groups had similar post-graduation residency rates before the reform, with a difference emerging around the reform and growing over time with successive cohorts of post-reform graduates.

The reform also encourages international students to make a crucial integration investment during their doctoral studies: learning Swedish. Compared to their non-native European peers, the treated students increase their enrolment at Swedish For Immigrants (SFI), a publicly provided language program, by 5.6 percentage points (21%; $p < 0.05$).

The reform's effects go beyond residency and investment in host-country-specific skills. The higher prospect of permanent settlement in Sweden, and the reduction in residence uncertainties it brings about, increases the rate of marriage or partnership among the treated international students. For the international students who are single at the start of their studies, the probability of getting married during the PhD increases by 3.8 percentage points (11%; $p < 0.10$).[10] Improved residency prospects in Sweden may also increase the value of international students in the marriage market. Indeed, we find higher assortative mating in terms of the partner's education. Finally, we do not find any effects on fertility.

These effects are larger for cohorts with longer exposure to the new rules, namely the international students who graduate between 2015-2017. The treatment effects for these cohorts on residence, marriage, and language investment are 17.1 (30%), 6.5 (27%), and 4.2 (12%) percentage points, respectively ($p < 0.05$). We also find that male students and students who are registered in a STEM field show higher treatment effects, especially for long-term residency and language learning.

A key threat to identification for these analyses is the potential violation of the stable unit treatment value assumption or SUTVA. For instance, if the residency choices of the international students also influence their European or Swedish peers, our estimator would be biased. However, the direction of this bias is ambiguous. Consider the labor market, for example. The sign of the bias in the estimated residency effects could depend on whether the international students and their European or Swedish peers are considered substitutes or complements. We use a shift-share instrumental-variable design to investigate this issue. Combining the pre-reform variation in the share of international

[10] We also find a reduction in family dissolution. For students who are married at registration, separation or divorce during the PhD declines by 1.6 percentage points (32%; $p < 0.10$).

peers across different research fields and the shift offered by the reform, we do not find any evidence of crowding out. If anything, our findings suggest that more international peers staying in Sweden *increases* the probability of a European student making the same choice. This result suggests that our estimates of the reform's effects on the residency decision of the international students are likely to be lower bounds.[11]

Our results are robust to different choices of the countries that form the treatment or the comparison group; the choice of covariates; restricting the sample to a balanced panel of countries that are represented in each cohort of graduates; and a specification that allows for differential time trends based on the student's pre-determined characteristics recorded at registration.

For the analysis with partners, we find that the partners of the treated international students increase their enrolment at Swedish for Immigrants by 3.3 percentage points (17%; $p < 0.10$). The reform has a positive effect on their employment as well. Partners of treated students are 4.9 percentage points (28%; $p < 0.05$) more likely to be employed.[12][13]

Our results contribute to the literature in three ways. First, our work is related to the strand documenting the effects of migration policies targeting skilled immigrants. A growing body of work has investigated how cross-country tax differentials and preferential tax schemes affect high-skilled immigration. This corpus of work consistently demonstrates the efficacy of tax incentives in attracting high-skilled professionals (see e.g., Kleven et al. (2014), Akcigit et al. (2016), Timm et al. (2022), and Kleven et al. (2020)

[11] This also suggests that, if the decision to stay in Sweden after graduation has a positive effect on the other outcomes we consider such as investment in language skills and family formation, our DID estimator using only the non-native European comparison group might be downward-biased. In other words, our DID estimates are potentially lower bounds for the true effect of the reform on these outcomes for international students.

[12] We can rule out that these effects are driven by the increase in marriage among the treated international students. See Section 6.5 for the detailed calculations.

[13] As an ancillary analysis, we also look at the impact of the reform on the volume of incoming international students who register in or after 2014, and their composition. The results are reported and discussed in Appendix B. However, there are two limitations. First, we do not have data on the number of applicants and their qualifications, which would be arguably more appropriate measures for this analysis. Second, a reform in 2011 that introduced tuition fees for international master's students led to a substantial decline of over 60% in the number of these students registering in a Swedish master's program. Since, on average, 10% of these international master's students eventually register in a Swedish doctoral program, this decline potentially confounds our analysis of the effect that the permanent-residency reform might have on the volume and composition of incoming doctoral students. With these caveats in mind, our estimates suggest a decline in the volume of international students registering in 2014 or afterward. Regarding composition, we find small and statistically insignificant effects of the reform on the characteristics of these incoming international students. The characteristics we have information on are: whether a student is – female; married at registration; has a child at registration; registered in a STEM program.

for an overview). Our study is one of the first to offer insights into an alternative policy instrument: access to long-term residency. While high-skilled workers are generally perceived as more mobile and less reliant on permanent residency than their low-skilled counterparts, our results underscore a substantial demand for policies that enhance the prospects of long-term residency among this type of workers.

Second, our paper sheds light on how policies that change permanent settlement prospects affect integration investments and family formation among high-skilled workers. Prior studies have predominantly focused on refugees and low- to middle-skilled workers, individuals who may face a different set of incentives and outside options compared to high-skilled workers. Broadly, these studies find that a higher prospect of permanent residency encourages immigrants to invest in language skills. [14] The evidence for marriages, however, is scant and the previous literature on fertility provides mixed results.[15] We contribute to both of these strands, with empirical evidence that better access to permanent residency motivates high-skilled workers to acquire host-country-specific skills and increases their chances to form partnerships (with no effects on fertility).

The finding on language investments is noteworthy, especially when considering the context of international doctoral students. These students are presumably proficient in English, which serves as another important working language in high-skill occupations in Sweden. Therefore, learning Swedish may hold less significance for them compared to other immigrants.[16] Note that there are no language requirements for permanent residency in Sweden, suggesting that these investments can be partly driven by an interest to assimilate into Swedish society rather than directly improving their chances to stay.

Finally, we add to a growing body of literature exploring the post-graduation choices of international students. Beine et al. (2023), for instance, estimate the transition rate of foreign college graduates to the US labor market. Our results highlight the role of migration policies in shaping these choices.

[14] For instance, Blomqvist et al. (2018) study a migration policy in Sweden for refugees that changed the default from offering permanent to temporary permits and find that this reform decreases the uptake of language training. Similarly, Gathmann and Keller (2017) find that a reform in Germany that made it easier to obtain citizenship increases language skills among female immigrants.

[15] Avitabile et al. (2014) investigate the German introduction of birthright citizenship and find negative effects on fertility among temporary migrants but positive effects on the health and socio-economic outcomes of their children. Amuedo-Dorantes and Arenas-Arroyo (2021), on the other hand, find that higher interior immigration enforcement reduces fertility among undocumented immigrant women in the US.

[16] The evidence is mixed on the returns to language skills for the different skill groups. For example, Berman et al. (2003) find language acquisition to be more important for wage growth in high-skill occupations in Israel. In contrast, Carlsson et al. (2023) find a larger effect of language skills on the probability of employment in low- and medium-skilled Swedish occupations.

The remainder of the paper is structured as follows. Section 2 provides a brief conceptual framework. Section 3 details the migration policy reform. Sections 4 and 5 describe the data and empirical strategy, respectively. Section 6 presents our findings and Section 7 concludes.

## 2 Conceptual Framework

The choice of residency is one of the key decisions that an immigrant makes. Where one chooses to live shapes their labor market choices as well as the decisions one makes in the realms of family and beyond. The decision to settle in a country is closely tied to choices around when to form a family – and with whom – as well as what kinds of country-specific investments one can make to maximize the social and economic opportunities in that country.

When choosing their future place of residence, prospective migrants face a multi-faceted decision-making process. Factors such as labor market opportunities, tax rates, public policies, cultural assimilation prospects, and the possibility of long-term residency may all play a role in this decision (Lee, 1966).

For international students pursuing education abroad, this choice mirrors the broader migrant experience, but with an additional layer. They first choose a country to acquire human capital before deciding where to settle after graduation. The human capital investments these migrants make in the first destination county and the formative experience of the migration itself can shape their preferences, beliefs, and labor market prospects, ultimately influencing their post-graduation residency decisions.

However, there is a substantial lag between these two choices, namely where to migrate to study and where to reside after graduation, especially for doctoral students who would typically spend at least four or five years in the first country. This lag, coupled with uncertainties along the way, can lead to substantive differences between these two decisions. Unanticipated changes in migration legislation, labor markets, or personal circumstances can further complicate the decision-making process.

The reform we investigate, which offers an easier pathway to permanent residency, prompts us to explore how it affects the decision of international doctoral students to stay in Sweden after graduation. Ex-ante, the longer residency or employment requirements that existed before the reform might not have been a binding constraint for these highly educated graduates. However, the reduction in uncertainties resulting from the reform can decrease the perceived costs of staying, particularly for risk-averse individuals. Furthermore, the reform may increase the relative attractiveness of Sweden as a long-term

destination compared to other options.

The prospect of securing permanent residency in a country can influence an individual's incentives to acquire skills specific to that country, such as learning the local language. However, the direction of this influence is theoretically ambiguous, particularly when proficiency in the local language is not a prerequisite for permanent residency, as is the case in Sweden.

On one hand, in the short run, the assurance of permanent status may weaken the incentives for individuals to invest in language skills. With no need to renew temporary permits, some may choose to delay language learning and prioritize their studies or research. On the other hand, the prospect of a longer stay can raise the value of learning the local language. Apart from improving labor market prospects (Rooth and Åslund, 2006), proficiency in the local language can facilitate integration, communication, and participation in the host society. This is a crucial consideration, especially for individuals with children as the language of instruction in most Swedish schools and pre-schools is Swedish.

Similarly, the increased probability of obtaining permanent residency can influence an individual's decision to start a family (Becker, 1991). Several mechanisms come into play. The higher certainty of long-term residence can reduce uncertainties and lower the costs associated with marriage and parenthood. Sweden, in particular, is renowned for its robust support for child development, from birth through late adolescence. Improved access to opportunities offered by the Swedish economy and society can enhance marriage prospects for international students who are single at registration. Furthermore, the reform ensures that partners receive work permits by default, reinforcing this mechanism.

Higher certainty of residence and improved access to socioeconomic opportunities can also help prevent or delay the dissolution of existing families. By increasing the marital surplus for some matches on the margin, this can contribute to higher stability of pre-existing families.

## 3 The Reform

On July 1, 2014, a liberalization of the Swedish migration law took place through the change of the Aliens Act (SFS , 2014:777). This reform was specifically aimed at simplifying the process for international doctoral students to secure a permanent residence permit (PRP) upon completing their studies. Prior to this reform, international doctoral students were classified as 'regular' students according to the law. Consequently, their graduate studies, which typically spanned four to five years, were not considered part of their em-

ployment history. As a result, to qualify for a PRP, these students had to work for an additional four years in Sweden after graduation.[17]

In an official Government report from March 2011, international doctoral students were identified as an important group in achieving the goal of boosting high-skilled immigration (SOU , 2011:28). The report presented three proposals to make Sweden a more attractive destination for international students pursuing doctoral studies and to enhance their ability to remain in the country after graduation. First, the report recommended granting doctoral students the option to obtain a PRP after completing their studies. Second, it suggested extending the period during which international students could seek employment after graduation from 3 to 6 months.[18] Third, the report proposed granting work permits to the partners of international doctoral students.

However, no changes were implemented in 2011 following this report. The only noteworthy development in the political arena was a motion introduced in October 2011 by a member of the Liberal Party. This motion was subsequently rejected by the parliament. This lack of action on the part of politicians spurred a wave of debate articles, particularly from various student and university unions, urging the government to adopt the proposals outlined in the 2011 government report.

Finally, on March 27, 2014, the right-center coalition government, in collaboration with the Swedish Green Party, submitted a relevant proposition (Prop. , 2013/14:213). The proposition, in alignment with the recommendations of the 2011 report, entailed the following changes related to international doctoral students:

- International doctoral students could obtain a PRP after completing their studies if they held a residence permit for doctoral studies in Sweden for a total of 4 years within a 7-year period.
- Family members [19] were granted the opportunity to acquire a working permit for the entire duration of the doctoral studies. Additionally, they were now eligible to apply for a PRP together with the doctoral student.
- Students who had successfully completed their studies would now qualify for a 6-month residence permit to seek employment – an extension from the previous

[17] In other words, they became 'labor migrants' only after graduation and had to fulfill the same requirements that an international 'labor migrant' had to satisfy for PRP eligibility.

[18] This suggestion also applied to international students at the Bachelor and Master levels.

[19] Family members include the doctoral student's spouse, partner, and (unmarried) children under the age of 18.

allowance of 3 months. [20]

To summarize, in theory, the proposed changes would (a) effectively reduce the required residence from eight to four years for the international students to become eligible for a PRP; (b) grant a work permit to these students' spouses or partners for the duration of doctoral studies; and (c) allow the entire family to apply together for permanent residency.[21]

The passage from proposal to approval to implementation was relatively short, namely just over three months. The law was passed without any alterations on June 17, with its implementation set for July 1, 2014.[22]

## 4 Data Description

We assemble a novel and comprehensive dataset comprising the universe of doctoral students who graduate between 2008 and 2017 in Sweden. The information on education is collected by Statistics Sweden on behalf of the Swedish Higher Education Authority (UKÄ), covering all registered students in each semester. This enables us to track individual doctoral students from their initial semester of registration to their final semester. We also have access to other information on their education such as the field of study or research. For details on how we apply this information, please see Appendix A.1.

To address potential endogeneity concerns stemming from the differential selection of incoming students after the reform, we limit the sample to those who first register no later than the second semester of 2013.[23] However, in Appendix B, we investigate how the reform affects the selection of the incoming students.[24] [25]

To determine whether the reform applies to a given student, we use data on the student's country of birth from the Total Register of the Population (RTB). Note, however,

[20] As mentioned above with respect to the 2011 report, this change also applied to international students at the Bachelor and Master levels.

[21] One of the key eligibility criteria for acquiring Swedish citizenship is having a permanent resident permit, so this reform eases the path to citizenship as well.

[22] There was a partial reversal of these changes that came into effect on July 20, 2021. All our measurements precede this change at least by one year.

[23] Only 167 observations are excluded to satisfy this restriction.

[24] We also do not include students who register before the first semester of 1998, thereby excluding outlying cases with unusually long study periods. 3.3% of the sample is excluded following this criterion and 92% of these are Swedish students

[25] Graduation rates are stable across the successive cohorts of registrants, with a slightly upward trend (see Figure D.1 for the trends of graduation rates by region for the registration cohorts 2001 to 2013). We do not find any substantive evidence of selection into who graduates among the international students, especially in comparison to their European peers, which is the key comparison group we use consistently for all outcomes (see Table D.1).

that the RTB data we have access to often does not identify the exact country of birth but groups these countries into different country-groups. For instance, Germany is exactly identified whereas Djibouti, Eritrea, Ethiopia, Somalia, and Sudan are assigned to one country-group. Using this information, we assign the students to one of three categories: Swedes, EU/EEA citizen, or non-EU/EEA (international) citizen.[26] For more details on the country list and categorization, please see Appendix A.2.

Our study population consists of 25,031 doctoral graduates. Table C.1 presents the summary statistics for some background characteristics stratified by group affiliation. Approximately 17% of the students are classified as non-EU/EEA or international students. Compared to their EU/EEA peers, these students have similar ages but are more likely to be male, registered in a STEM program, and married at first registration.

In terms of composition, the four largest groups of students outside Sweden come from North Asia (18.5%), Central Asia (12.16%), Germany (10.16%), and Iran (9.92%). For a comprehensive breakdown of student distribution among various country groups, see Table A.2 and Figure C.1.

For ease of exposition, in the rest of the paper, we use the following terms for the different groups of students: Swedish (students who are born in or are naturalized residents of Sweden), European (students who are born in countries other than Sweden that are part of the EU/EEA), and International (students who are born in a 'third country' i.e., a country that is not part of the EU/EEA[27]).

Our analysis has the following primary outcomes: residency in Sweden after graduation; family formation (e.g., marriage, separation, and fertility) and investment in Swedish language during the doctoral studies. We choose the period of doctoral studies as the reference period for two reasons. First, this allows measurement for all students regardless of whether they choose to stay in Sweden after graduation. More importantly, choices made during this period reflect the behavioral response that takes place even before a student becomes eligible to apply for permanent residency. This is critical since the easier access to permanent residency brought about by the reform, in theory, can have an impact on all international students and not only on those who choose to stay and apply for a permanent permit.

Information on residency in Sweden comes from the LOUISE dataset, which identifies individuals observed in the data for a specific year as residing in Sweden. The LOUISE

[26] In some country-groups, we have countries from these different categories grouped together. We exclude the students from these groups.

[27] And who are not naturalized residents of Sweden at the beginning of their doctoral studies.

data set, compiled by Statistics Sweden, provides annual data on the labor market and educational outcomes for all individuals registered in Sweden as of December 31 each year. This dataset also provides information on civil status and formal partnership. We identify the partners using the family ID, assuming that individuals sharing the same family ID and with an age difference of less than or equal to 20 are partners. To analyze the impact of the reform on fertility, we use the Swedish Multi-generational Register, which links parents to their children. Combining this with information from RTB, we can observe the year of birth for the children.

For information on language learning, we use the Swedish for immigrants (SFI) dataset, collected by the Swedish National Agency for Education and provided by Statistics Sweden. This dataset includes details such as the date of registration and the specific courses attended by the individual. SFI is a publicly provided and free language program, which, as the name suggests, is designed for immigrants in Sweden who are willing to learn the Swedish language.

## 5 Empirical Strategy

We use a difference-in-differences design leveraging two variations: the timing of the reform and the fact that this reform only applies to international students and not to their European or Swedish peers. Our benchmark regression specification is as follows:

$$y_{ict} = \alpha + \beta D_{ct} + \boldsymbol{\tau} \boldsymbol{X}_{ict} + \boldsymbol{\mu}_c + \boldsymbol{\lambda}_t + \epsilon_{ict}, \tag{1}$$

where $y_{ict}$ represents the outcome for individual $i$, originating from country group $c$, and graduating in year $t$. $D_{ct}$ is a binary variable equal to one for individuals who graduate in 2013 or later and come from a country group $c$ that is outside the EU/EEA.[28] We consider the cohort of international students graduating in 2013 to be *treated* to allow anticipation.[29] $\boldsymbol{X}_{ict}$ denotes a vector of predetermined individual characteristics that are recorded in the first year of registration: whether a student is – female; registered in a

[28] $D_{ct}$ is equivalent to the representation 'Treat X Post' which is also common in difference-in-differences specifications.

[29] Technically, for instance, international students graduating in 2012 can also benefit from this reform *if* they are still in the country when the change in law comes into effect in July 2014. Therefore, this group would have been likely to have stayed regardless of the reform. In other words, they can be considered the 'always takers', thereby having minimal implications for our estimation. Moreover, for outcomes such as language learning and family formation, we measure them for the period of one's doctoral studies. For this group, that period would precede the reform by at least two years. Therefore, these outcomes would be unlikely to be shaped by the reform. As discussed in Section 3, the reform took three months from proposal to implementation, which arguably left limited scope for anticipation.

STEM program; married at registration; has child at registration; and four dummies representing the quintiles based on the student's age in years. $\mu_c$ stands for country-group fixed effects and $\lambda_t$ represents year-of-graduation fixed effects. $\beta$ is the coefficient of interest: the Average Treatment Effect for the Treated or ATT, which represents the causal effect of the reform on the treated international doctoral students. All the forthcoming tables with regression results report this coefficient. $\epsilon_{ict}$ is the error term clustered at the country-group level to account for serial correlation in the outcome among students from a country-group graduating in successive years.[30] Given the relatively small number of clusters, we also present p-values using wild-bootstrapped standard errors (Roodman et al., 2019).[31]

The European peers might be considered a better comparison group, especially since they are also immigrants to Sweden. Indeed, for outcomes such as investment in the local language, they are the only available comparison group. Therefore, whenever applicable, we present the results with and without the Swedish peers included in the comparison group.

To examine potential differences in responses between cohorts exposed to the reform late versus early in their doctoral studies, we also present results where we stratify the post-reform cohorts into two groups: i) students graduating between 2013 and 2014, and ii) students graduating between 2015 and 2017. We estimate Equation (1) separately for these two groups.[32]

Additionally, we present our results using an event study specification as follows:

$$y_{ict} = \alpha + \sum_{t \in (-4,+4),\ t \neq -1} \beta_t D_c + \boldsymbol{\tau} \boldsymbol{X}_{ict} + \boldsymbol{\mu}_c + \boldsymbol{\lambda}_t + \epsilon_{ict}, \tag{2}$$

where we normalize the periods relative to 2013 ($t = 0$), with $t = -1$ as the reference point.[33] $D_c$ is a binary variable equal to one for students from countries outside the EU/EEA. The rest is identical to Equation (1). All the forthcoming event-study figures report the vector of coefficients $\boldsymbol{\beta}_t$ $[t \in (-4, +4),\ t \neq -1]$.

[30] As the exposure to the reform depends on whether an individual's country of birth/citizenship is outside the EU/EEA and hence considered a 'third country', the treatment is at the level of one's origin country. Therefore, following Abadie et al. (2023), we cluster the standard error at the country-group, the most granular information on an individual's country of origin we have access to.

[31] We have either 25 or 27 clusters depending on excluding Swedish students or not. See Appendix A.2 for more information on country groups.

[32] The pre-reform sample remains the same: cohorts graduating between 2008 and 2012.

[33] To improve on power, we stack $t = -5$ and $t = -4$.

For both specifications, the identification of the causal effect of the reform on international students relies on the assumption of (conditional) parallel trends: In the absence of the reform, the evolution in the average outcome over graduating cohorts, conditional on predetermined covariates, would be the same for students from a non-EU/EEA country as that for students from a EU/EEA country. For all outcomes, we report the result from a test of pre-trends i.e., a joint significance test for all leads in the event-study equation 2.[34] For information on the pre-existing differences between the treatment and comparison groups, we also report group-specific pre-reform means in all Tables.

The identification also depends on the stable unit treatment value assumption (SUTVA) i.e., the easier access to permanent residence for international students does not affect the behavior of their Swedish or European peers. This is arguably more relevant for outcomes such as the decision to stay in Sweden in the long run if, for instance, these graduates are substitutes in the labor market or the demand for these graduates is inelastic. We probe this possibility by using a shift-share instrumental-variable (IV) design where the shares are pre-reform shares of international students graduating across research fields and the 2014 reform constitutes the (exogenous) shift (De Chaisemartin and d'Haultfoeuille, 2018).

As robustness and sensitivity checks for the primary outcomes, we carry out four exercises. First, we exclude the covariates, thereby relying on the less restrictive assumption of unconditional parallel trends. Second, we interact the covariates with graduation-year fixed effects. This specification, for instance, allows for differential trends for students with different demographic backgrounds. This is potentially relevant if these demographic characteristics vary across the treatment and comparison groups, and are correlated with the outcome. Third, we restrict the analysis to a balanced panel where all country-groups are represented in each graduating cohort over the study period. [35] Fourth, to assess whether the results are driven by particular groups of treatment or comparison countries, we replicate the benchmark specification (equation 1) with iterative

[34] One may argue that the students of Swedish or European origin should not be considered a control group but rather a group that is 'always treated'. This is arguably more applicable to students from countries in Europe that have recently joined the EU/EEA. The corresponding identification assumption for the validity of our empirical approach is that the treatment effects for these groups have stabilized before the start of our study period. For all practical purposes, the standard practice of investigating pre-trends should suffice for this alternative assumption as well. Also, the last major EU expansion happens in 2004, 4 years before the start of our study period. Moreover, we exclude students from countries that have joined later such as Croatia.

[35] This restriction leads to excluding the following country-groups: Bosnia and Herzegovina, Chile, and Iraq.

exclusion of different groups of countries either from the treatment or the comparison group. This is important if the reform coincides with macroeconomic shocks in certain origin country-groups, which might also affect the behavior of students from those groups and in turn confound our causal analysis of the reform. For instance, if there is a concurrent economic crisis around 2014 in an origin country, students from that country might be more likely to settle in Sweden after graduation independent of the changes in the permanent residency law.

# 6 Results

## 6.1 Effect on Long-term Residency in Sweden

How does an easier pathway to permanent residency affect the international students' decision to stay in Sweden after graduation? Ex-ante, the pre-reform longer residency or employment requirement might not have been a binding constraint for these graduates with high levels of education. On the other hand, the uncertainties associated with such requirements might increase the costs of staying for risk-averse individuals. Moreover, the reform may reduce the relative price of locating in Sweden compared to settling elsewhere. Finally, a key stated objective of the reform was to make it easier for international doctoral students to stay in Sweden after graduation. Hence, we start off by investigating the reform's effect on long-term residency in Sweden.

To investigate this, we use residency in Sweden three years post-graduation as our main outcome.[36] We start by inspecting the raw data. Figure 1 plots, for each cohort, the share of students residing in Sweden three years post-graduation, stratified by country or region of origin, namely Swedes, EU/EEA, and international (non-EU/EEA). Unsurprisingly, over 90% of the Swedish graduates stay in the country three years post-graduation, with a slightly upward trend over the study period. On the other hand, before the reform, in terms of both level and trajectory, the trend in residency for international graduates closely follows that for their European counterparts but starts diverging around the year of the reform, namely 2014. Figure C.2, which plots the residency rates among these

[36] Residency three years after graduation is highly correlated with residency over longer time horizons, thus making this measure a good proxy for long-term residency. For instance, among the pre-reform (2008-12) cohorts of international students, 85% and 78% of those who stay three years post-graduation are still in Sweden five and seven years after graduation, respectively. The corresponding figures for their European peers are 93% and 86%. Among the cohorts graduating between 2013-15, 93% of the international students who stay three years post-graduation are still in Sweden five years after graduation. The relevant figure for their European peers is 92%. Since the latest year for which we have residency data is 2020, we can not estimate the rate of residency seven years post-graduation for these cohorts.

groups for one to four years post-graduation, exhibits a similar pattern. While these provide some descriptive evidence for the reform's effect on long-term residency, we now turn to formal regression analysis to estimate the causal effect.

Table 1 presents the ATT estimates for long-term residency (using Equation 1). Column 1, which uses the full sample, shows that the reform increases long-term residency of the treated international students by 13.5 percentage points, a 23% increase over the pre-reform mean of 58%. [37] The effect is precisely estimated and significant at the 1% level, with both cluster-robust and wild-bootstrap inference. The pretest p-value does not suggest any significant divergence in trends between the international and the combined comparison group of Swedish and EU/EEA students prior to the reform's implementation. Column 2 presents the same estimation but excludes the Swedish doctoral students from the comparison group. The estimate remains highly significant, with a slightly smaller estimated effect of 11.8 percentage points.

In Columns 3 to 6, we estimate the effect separately for those exposed to the knowledge of this reform in later versus earlier stages of their doctoral studies. The effect is more than twice as pronounced for cohorts with longer exposure to the knowledge of the reform.[38]

Using Equation 2, Figure 2 presents the event-study estimates, separately using the entire comparison group and using only the European peers. Corroborating the earlier estimates, these figures show a positive effect of the reform on the long-term residency of international students, with the effect growing over consecutive cohorts of graduates.

#### 6.1.1 Peer Effects on Swedish and European Students

A key threat to identification for the analysis above is the potential violation of the stable unit treatment value assumption or SUTVA. For instance, if the residency choices of international students also influence their European or Swedish peers, the ATT estimator we use can be biased. The direction of this bias, however, is ex-ante ambiguous. For example, if they are substitutes for each other in the labor market, higher residency of

[37] Given the total of 2,874 international students who graduate between 2013-2017, this estimate translates to an additional 388 students staying in Sweden in the long term (over 77 students or 3% of the average graduating cohort).

[38] Speculatively, there are multiple explanations for such heterogeneity. For example, students with longer exposure to the reform have more time to make the necessary investments to maximize the returns to residing in Sweden in the long term. We have supporting evidence for this in the form of higher language investments by these cohorts (see Section 6.2). Another mechanism could be the residency effects for the earlier cohorts generating a positive network externality that leads to higher treatment effects for the later cohorts. It could also be the case that there is a learning curve in familiarizing oneself with the new regulations and the cohorts graduating later can learn from the experience of their older counterparts.

international students may crowd out their peers, leading us to overestimate the reform's effect. On the other hand, if the potential employers consider them to be complements, our estimator would be biased downward.[39] We address this question by using a so-called shift-share instrumental variable (IV) within the difference-in-differences design. The shares are pre-reform shares of international graduates across 60 different research fields. [40] In particular, we have the following empirical setup:

$$\begin{aligned} y_{ift} &= \alpha + \theta N_{ift} + \boldsymbol{\tau} \boldsymbol{X}_{ift} + \boldsymbol{\mu}_f + \boldsymbol{\lambda}_t + \epsilon_{ift} \\ N_{ift} &= \gamma + \sigma Z_{ift} + \boldsymbol{\pi} \boldsymbol{X}_{ift} + \boldsymbol{\kappa}_f + \boldsymbol{\omega}_t + \nu_{ift}, \end{aligned} \tag{3}$$

where $y_{ift}$ is a binary variable indicating whether a doctoral student of Swedish or European origin stays in Sweden three years post-graduation; $N_{ift}$, the endogenous regressor, is the number of international students in the individual $i$'s research field $f$ in year $t$ that stay in Sweden three years after graduation. $Z_{ift}$ is the instrument: for all pre-reform years, i.e., 2008-2012, it is zero; from 2013 onward, it denotes the quintile of the corresponding research field based on its share of graduating students that are international over the pre-reform years 2008-2010. The mean shares across the five quintiles are 0, 3, 8, 12, and 22%, respectively. $\boldsymbol{X}_{ift}$ denotes a vector of predetermined individual characteristics identical to those used in equations 1 and 2 as well as university fixed effects. $\mu_f$ stands for research-field fixed effects and $\lambda_t$ represents year-of-graduation fixed effects. $\theta$ is the coefficient of interest, capturing whether or to what extent the number of international students staying in Sweden affects a Swedish or European peer's choice to stay in Sweden in the long term. $\epsilon_{ift}$ is the error term clustered at the research-field level to account for serial correlation in the outcome among students from a field graduating in successive years. Finally, we estimate this model separately for the Swedish and the European doctoral students.

This IV-DID design entails three identification assumptions. First, (conditional) par-

[39] There, of course, are other potential mechanisms through which one group's behavior might affect the other. For instance, if these individuals search and match in the same marriage market.

[40] Here are ten research fields with their pre-reform shares of international graduates reported in parentheses: Animal Production/Animalieproduktion (0%), Nursing Science/Vårdvetenskap (2.6%), Economic Sciences (6.7%), Microbiology (8.8%), Physiology and Pharmacology (10%), Earth Sciences (12%), Mechanical Engineering (13.4%), Chemistry (16.4%), Biotechnology (21%), and Electrical Engineering, Electronics, and Photonics (26.7%). These research fields, we argue, are reasonable proxies for the relevant labor markets a graduate from a field may participate in if they choose to stay in Sweden. In academia, for instance, there often are centralized job markets organized for particular disciplines, the 'economics job market' being one prominent example. The results are qualitatively similar if we use an alternative proxy such as university-field combinations, and are available upon request.

allel trends hold for both the first stage and the reduced form. Second, the instrument has sufficient explanatory power for the endogenous regressor (i.e., we have a 'strong' first stage). Third, the exclusion restriction i.e., the instrument affects the residency choice of the Swedish or European graduate *only* through the number of international peers in their research field that choose to stay.

Figures C.3 and C.4 present the event-study estimates for both the first stage and the reduced form (for Swedish and European doctoral students, respectively). Formal pretests do not provide any strong evidence for pretrends (please see the Figure notes for details). In both cases, moreover, the instrument appears to generate substantive variation in the endogenous regressor, which we formally show in the forthcoming pooled regression analysis. Finally, in regard to the validity of the exclusion restriction, there is no direct test. In this context, however, it is unlikely that the instrument will affect the final outcome through any channel other than the international students' choice to stay in Sweden as that is the most proximate outcome tied to the change in the law on permanent residency.

Table 2 reports the OLS and IV estimates for the following question: How does the number of international students staying in Sweden affect a Swedish or European peer's choice to stay in Sweden in the long term? Columns 1 and 3 report the OLS estimates for Swedish and European students whereas Columns 2 and 4 show the corresponding IV estimates. All the estimates are positive but vary in magnitude and statistical significance. For the Swedish sample, the IV estimate is small and not significant at conventional levels. For the European students, on the other hand, the IV estimate is substantially larger and significant at the 5% level. One more international graduate from the same research field settling in Sweden three years post-graduation increases the likelihood of a European peer following suit by approximately 1 percentage point (a 1.7% increase over the pre-reform mean of 58%). This evidence of 'crowding in' is a potential explanation for the lower ATT estimate for the residency probability of international students when we restrict the comparison group to Europeans only (see Section 6.1 and Table 1).[41] This also suggests that, if the decision to stay in Sweden after graduation has a positive effect on the other outcomes we consider such as investment in language skills and family formation,

[41] In the pre-reform period, the average European student has 7.29 international peers from the same research field staying in Sweden 3 years after graduation. Using the headline ATT estimate from Section 6.1, namely 23%, the reform leads to 1.68 more international graduates residing in Sweden. This translates to an increase in the probability of the European student making the same choice by 1.65 percentage points (=1.68*0.0098). This is notably close to the difference in the ATT estimates in columns 1 and 2 of Table 1, namely 1.7 percentage points (=13.5-11.8).

our DID estimator using the European comparison group might be downward-biased. In other words, our DID estimates are potentially lower bounds for the true effect of the reform on these outcomes for international students.

#### 6.1.2 Robustness

We conduct a series of robustness tests to assess the sensitivity of our results. First, we exclude the control variables, thereby invoking the less restrictive unconditional parallel trends assumption. In Table C.2, Columns 1 and 2, we present the ATT estimates similar to those in Columns 1 and 2 of Table 1, while excluding all control variables.

Second, to account for potential variations in residency trends among different student profiles, we interact all control variables with graduation year fixed effects. The results are detailed in Columns 3 and 4 of the same table.

Third, we restrict our analysis to country groups where we have graduating students in each year, creating a balanced panel. The estimates are reported in Columns 5 and 6.

Finally, we investigate the sensitivity of our results concerning the choice of comparison and treatment regions. Figure C.6 illustrates the different estimates, along with 95% confidence intervals, as we iteratively exclude one region from the comparison group in each estimation. Figure C.7 carries out the same exercise but for the treatment group.[42]

Collectively, our robustness tests lend support to the reliability of our results. Whether omitting control variables, considering different time trends depending on student profiles, examining balanced panels, or altering the composition of comparison and treatment groups, our findings consistently support the substantial positive impact of the reform on the post-graduation residency rates of international students in Sweden. These results underscore the robustness of our central conclusion regarding the reform's effectiveness in promoting long-term residency among international doctoral graduates.

### 6.2 Investment in Language Skills

The prospect of securing permanent residency in a country can influence an individual's incentives to acquire host-country-specific skills such as learning the local language. However, the direction of this relationship is theoretically ambiguous, especially when eligibility for permanent residency is not contingent on proficiency in the local language, which is the case for Sweden. On one hand, the assurance of permanent status may act as a disincentive for individuals to invest in language skills since they no longer need to

[42] We repeat these exercises in Figures C.8 and C.9, with Sweden always excluded from the comparison group.

concern themselves with renewing temporary permits to stay in the country. It might be intertemporally optimal to defer such investments to a later date and spend that time on doctoral research. On the other hand, the expectation of a longer stay increases the returns to learning the local language, given its potential benefits for integration, communication, and participation in the host society.[43]

To assess the effect of the reform on language acquisition during doctoral studies, we examine enrolment in the Swedish for Immigrants (SFI) program. Our outcome variable is a binary indicator, taking the value of one if a student enrols in any SFI course during their doctoral studies. Using Equation 1, Table 3 reports the relevant ATT estimates with the European students as the comparison group.

In Column 1, we present the results for the entire sample. The reform leads to a 5.6 percentage points increase in SFI enrolment for the treated international students. Given the baseline enrolment rate of 27%, this represents a 21% increase.[44] This effect is significant at the 5% level, with both cluster-robust and wild-bootstrap inference.

Columns 2 and 3 replicate Column 1 separately for cohorts exposed to the reform at different stages of their doctoral studies. Echoing the findings related to long-term residency in Section 6.1, we observe that the impact is more pronounced for graduating cohorts exposed to the reform earlier during their doctoral studies. Visual representations of these estimates using the event-study approach are provided in Figure 3.

We subject these results to the same battery of robustness tests detailed in Section 6.1.2. Table C.3, and Figures C.10 and C.11 present the associated findings. Encouragingly, our results remain consistent and robust across these various sensitivity analyses.

### 6.3 Impact on Family Formation

Similarly to language acquisition, a higher prospect of obtaining a permanent status in a country may affect an individual's willingness to start a family. There are multiple potential mechanisms. The higher ex-ante probability of long-term residence can alleviate uncertainties, and in turn, reduce the costs of marriage and parenthood. Sweden, in particular, is known for offering robust support for child development from birth through late adolescence. The improved access to opportunities offered by the Swedish economy and society can also enhance the marriage market prospects for the treated international students who are single at registration. The reform also ensures that the partners get

[43] This can be particularly relevant for individuals with children as the language of instruction in the majority of the Swedish schools and pre-schools is Swedish.

[44] Conditional on enrolment, over 42% pass at least one course during their studies.

work permits by default, which further reinforces this mechanism and can lead to more assortative mating for the new matches. Analogously, the higher residence certainty and better access to socioeconomic opportunities can help avert or delay the dissolution of pre-existing families by, for instance, increasing the marital surplus for some matches on the margin.

To evaluate how the reform affects family formation, we look at the following key outcomes: the probability of getting married (or engaging in a registered partnership [45]), the probability of separating, and the probability of having children during doctoral studies.

### 6.3.1 Marriage or Partnership

Our outcome variable for marriage is defined as a binary indicator, taking the value of one if an individual changes their civil status to married (or registered partner) at any point during their doctoral studies. The results are presented in Table 4, with two distinct analyses. In Panel A, we use the full sample whereas in Panel B, we focus on students who are unmarried at the time of their initial registration.

In Panel A, Column 1, which employs the full sample, we observe that the reform leads to a 3.2 percentage points increase in the probability of marriage. Given a baseline of 21%, this represents a 15% increase. The estimate is statistically significant at the 1% level using clustered standard errors and at the 10% level using wild-bootstrap inference. Column 2 represents the same estimation but excludes the Swedish doctoral students from the analysis. The estimate remains positive albeit slightly smaller and is no longer statistically different from zero at conventional levels.

In Columns 3 to 6, we estimate the effects stratified by exposure to the reform. We find that the impact is more pronounced among cohorts exposed to the reform earlier in their doctoral studies. As was the case with the full sample, we cannot statistically rule out null effects when using only the European students as a comparison group. This is partly because of slightly smaller estimates (especially for the 2013-2014 cohorts) and partly because of a lack of power (especially for the 2015-2017 cohorts).

Turning to Panel B, which focuses exclusively on students unmarried at the time of their initial registration, we find results that closely mirror those in Panel A but with slightly larger and more precisely estimated effects.

The corresponding event study results are provided in Figure 4, which includes the

[45] Prior to 2009, homosexual couples were not allowed to marry but they could instead engage as registered partners which carried the same judicial interpretation as that for marriage.

Swedish students, and in Figure C.5, which excludes them. Robustness checks for this outcome are presented in Table C.4, and Figures C.12 and C.13.

### 6.3.2 Marital Sorting by Education

Is there more sorting among these additional matches induced by the reform? Identical in structure to the table for results on marriage, Table 5 presents the relevant estimates for marital sorting. The outcome is a binary variable indicating whether, during the time as a doctoral student, a student gets married or registers a partnership with someone with at least two years of tertiary education. Focusing on the sample of all students who are single at registration, there is a 4.9 percentage points increase in the probability of marriage or partnership with someone with at least a university degree. The estimate is significant at conventional levels. The relative increase is 21.3% (over a baseline mean of 23%), which is higher than the relative increase in the rate of marriage for the same sample, namely 10.9% (Column 1, Panel B, Table 4). This suggests higher sorting by education among the new matches that the treated international students make due to the reform. These effects are driven by the cohorts graduating between 2015-17 (Columns 5-6, Panel B), who also exhibits a higher treatment effect for marriage or partnership (Columns 5-6, Panel B, Table 4).

### 6.3.3 Separation or Divorce

To identify separation, we create a dummy variable taking the value one if, during the doctoral studies, an individual changes their civil status from married (or registered partner) to separated (or separated registered partner). We present the results in Table 6. Following the analysis for marriage, in Panel A, we use the full sample, and in Panel B, we only include individuals who are married in the year of their first registration.

In Panel A, we find no clear evidence of any effect as the estimates are small and imprecise. However, in Panel B, when we only include individuals who are married at first registration, we find estimates that tend to be negative, suggesting that the reform reduces the probability of divorce or separation.

Using both Swedish and European students as the comparison group (Column 1), the effect is -1.6 percentage points, which corresponds to a decline of 32% over the pre-reform mean of 5%. When we exclude the Swedish students from the comparison group (Column 2), the effect is larger in absolute magnitude but no longer significant.

By stratifying the sample based on the length of exposure to the reform, we find that the reduction in divorce is higher for those exposed later in their doctoral studies

(Columns 3 and 4) compared to those exposed earlier (Columns 5 and 6), although not statistically different from each other.

#### 6.3.4 Fertility

Finally, we do not find any robust evidence for the reform's effects on fertility, as measured by a binary variable indicating whether a doctoral student becomes a parent of at least one child during their studies. The results are reported in Table 7 and follow the same structure as that for separation. There is some evidence that the reform reduced the probability of parenthood, but this result is neither robust to different selections of the comparison group nor significant according to the wild-bootstrapped p-values.

To summarize, the reform leads to more matches formed in terms of either marriage or registered partnership, with the strongest impact observed among cohorts exposed to the reform earlier in their doctoral studies. These marginal matches are also more assortatively sorted in terms of the partner's education. On separation, there is no clear evidence of an effect in the full sample. However, among individuals who are married or partnered at the start of their studies, there is a suggestive negative impact, indicating a reduced probability of divorce, which is driven by those exposed to the reform later in their doctoral studies. Finally, we do not find any strong evidence of any fertility effects.

### 6.4 Heterogeneity

In previous sections, we document that international cohorts that are exposed to the reform earlier in their studies exhibit higher long-term residency rates, increased participation in SFI programs, and a greater frequency of marriages compared to those exposed later. In this section, we delve into additional dimensions of heterogeneity. Specifically, we conduct heterogeneity analyses for the primary outcomes using the following student characteristics: gender, enrolment in a STEM program, marital status at registration, and parental status at registration. The regression specification is as follows (Feigenberg et al., 2021):

$$y_{ict} = \alpha + \kappa D_{ct} + \zeta D_{ct} * H_{ict} + \phi H_{ict} * D_c + \gamma H_{ict} * D_t + \boldsymbol{\tau} \boldsymbol{X}_{ict} + \boldsymbol{\mu}_c + \boldsymbol{\lambda}_t + \epsilon_{ict}, \quad (4)$$

which follows the same structure as Equation (1) except that we interact one pre-determined characteristic ($H_{ict}$) with the treatment variable ($D_{ct}$), an indicator variable for the post-reform period ($D_t$), and an indicator variable for the treated country-groups ($D_c$). We use the rest of the pre-determined characteristics as regular covariates (in vector $\boldsymbol{X}_{ict}$).

The interaction effect of interest is $\zeta$, representing the differential impact of the reform for individuals with a particular predetermined characteristic. The total effect of the reform on this sub-group is the sum of the baseline effect ($\kappa$) and this additional effect ($\zeta$).

### 6.4.1 Long-term Residence

We begin our heterogeneity analysis by looking at residency in Sweden three years after graduation. The results are presented in Table 8.

In Column 1, we see that the effect on long-term residency is 7 percentage points lower for female international students (statistically significant at the 10% level). Note, however, that the baseline residency rates are higher for these female graduates, namely 62%. The result is similar when excluding the Swedish students from the comparison group (Column 2) but the interaction effect is no longer significantly different from zero.[46] In Columns 3 and 4, we investigate how the effect on long-term residence differs between STEM and non-STEM students. Column 3, which uses the full sample, suggests that the treatment effect for the STEM graduates is almost twice that estimated for their non-STEM counterparts (15.9 vs 8.7 percentage points; the difference is significant at the 1% level). When we exclude the Swedish students in Column 4, the interaction estimate decreases to 4.2 percentage points and is no longer significant. Moving to Columns 5 to 8, we assess heterogeneity based on marital status and parenthood at the time of registration. We find that the effects are more pronounced for those who are married and for those who are parents at the start of their doctoral studies. However, the interaction effect is smaller and loses significance when only the European graduates are included in the comparison group.

Taken together, our analysis indicates that, for long-term residency in Sweden, the reform's effect is larger among males, STEM students, and individuals who have a family when starting their doctoral studies.

### 6.4.2 Investment in Language Skills

We also find interesting heterogeneity regarding language acquisition. In Table C.5, we see that the positive effect on SFI take-up is completely driven by males (Column 1) and STEM students (Column 2). [47] We do not, however, find any differential effects depend-

[46] This is potentially due to the loss of power from using a smaller sample, especially since detecting interaction effects warrants substantially larger samples compared to that needed for the main effect (Brookes et al., 2004; Muralidharan et al., 2020).

[47] Note again that the baseline SFI take-up is higher among female international students (31% compared to the overall average of 27%).

ing on civil status or parenthood at registration (Columns 3 and 4).

These estimates are partly in line with the heterogeneity results we find above in Section 6.4.1 on long-term residence. Sub-groups with higher staying rates also take up more language training before graduation, further corroborating the link between the decision to stay and the choice to start learning the language.

#### 6.4.3 Family Formation

Moving on to family formation, Table C.6 presents the corresponding heterogeneity results. Somewhat consistent with the patterns we see for residency and language acquisition, female international students show a lower effect on marriage or partnership although the interaction effect is not significant at conventional levels (Columns 1-4). We do not find any evidence of heterogeneity for the STEM students.

### 6.5 Effect on Partners

As discussed earlier in Section 3, the reform has the potential to affect not only the international doctoral students but also their partners, operating through two key channels. First, the reform grants the partners of international doctoral students a work permit for the entire duration of the doctoral studies. Second, it streamlines the process for obtaining a permanent residence permit (PRP) for families.

We analyze the effects of the reform on the partners for two key outcomes: their likelihood to engage in language learning and employment. For this analysis, we only use the European students as the comparison group. Since we observe a positive effect of the reform on marriage in Section 6.3, it's important to note that the *unconditional* effect estimates we present may partly result from this increase in marriage. Therefore, we do a bounding exercise to assess whether these unconditional effects are beyond the mechanical increase resulting from higher marriage rates.

#### 6.5.1 Investment in Language Skills

The estimates for the effect on the partners' SFI enrolment are presented in Table 9. In Column 1, where we use the full sample, the estimated effect is 3.3 percentage points, a 17% increase over the pre-reform mean of 19%. Stratifying the post-reform sample by exposure to the knowledge of the reform, we find that partners to international doctoral students who are exposed earlier in their studies increase their enrolment by 4.2 percentage points, which is significant at the 5% level (Column 3; a 20% increase over the baseline). The effect for partners exposed later is 1.5 percentage points and not statistically distinguishable from zero (Column 2).

The reform increases the marriage rate in relative terms by 4.2% (Section 4; Column 2, Panel A, Table 4)[48], which is well below the unconditional effect of 17%, meaning that the reform-induced increase in marriage can only contribute partially to the observed treatment effect on SFI enrolment. In other words, we can rule out that the effect of the reform on partner's SFI take-up is completely driven by the increase in marriage rates among international doctoral students.

### 6.5.2 Employment

To evaluate the impact of the reform on partners' employment, we construct a binary variable that takes the value of one if a partner of a doctoral student has a positive labor income for at least one year during the doctoral studies. The results are presented in Table 10.

In Column 1, the results for the full sample indicate that the reform leads to a 4.9 percentage points increase in the probability of employment for the treated partners. This effect is statistically significant at the 5% level. Given the baseline employment rate of 28%, this represents a 17.5% increase. As is the case for SFI takeup, this implies that the reform-induced increase in marriage by 4.2% can not account for the entire treatment effect.

When we stratify the sample based on exposure to the reform, the expected pattern emerges. Partners of international students exposed to the reform earlier in their doctoral studies exhibit a larger response, with a 6.7 percentage points increase in the employment rate (significant at the 5% level, as shown in Column 3). This narrows the (conditional) difference in employment rates between the partners of international and European students by over 18%. In contrast, partners of students exposed later show a more modest effect, with a 2 percentage points increase, which is not statistically significant (Column 2).

## 7 Conclusion

In this study, we examine a unique Swedish migration policy that eased the pathway to permanent residency for international doctoral students and their family members. Our results provide compelling evidence on how this reform influences several key choices made by these international migrants.

[48] The relative change in marriage rate is estimated as follows: ATT for marriage/(share married at registration + share newly married during the studies), which is $4.2\% = 2.5/(21 + 38.4)$. Both shares are estimated using pre-reform data.

Using Swedish and European students as a comparison group in a difference-in-differences design, we find that the reform significantly increases the rate of long-term residency among international doctoral graduates. This finding underscores the importance of residency policies in skilled immigration, aligning with the goals of host countries to attract global talent and maintain a competitive edge in today's knowledge economy.

Employing a shift-share instrumental variable design, we further explore whether the post-graduation entry of international students into the local labor market displaces their Swedish and European counterparts. We do not find any evidence of such displacement. Instead, we observe a suggestive link between an increased presence of international peers in Sweden and a higher probability of European students making the same choice. This outcome implies that our estimates of the reform's effects on international students' residency choices might, in fact, be conservative.

We also find that the reform positively influences language acquisition among international students during doctoral studies, emphasizing the role of long-term residency incentives in fostering integration and host-country-specific skill acquisition. Another important finding relates to family formation and stability. The reform appears to have encouraged marriage and reduced separation among international doctoral students during their studies, highlighting the broader effects such migration policies can have beyond the economic sphere.

All these effects are more pronounced for cohorts exposed to the knowledge of this reform earlier in their doctoral studies. The timing of exposure to information about long-term residency prospects is therefore critical, given that individuals make forward-looking choices regarding their settlement, the acquisition of host-country-specific skills, and family formation. Additionally, we find suggestive evidence that the effects on long-term residency and language investments are higher for male students and for those undertaking their doctoral research in STEM.

As the reform removes constraints on labor market entry for the partners of international students as well as allows them to jointly apply for permanent residency, we also assess the reform's impact on their language learning and employment. Our analysis confirms that the reform significantly raises both language-program enrolment and employment among the partners of treated international students.

To summarize, this study contributes to the existing literature on skilled migration, emphasizing the critical role of long-term residency prospects in host countries. While prior research focuses on economic factors such as preferential tax schemes, our findings

underscore the potential of immigration policies that offer higher residence certainty in the long run. We also add to the literature by exploring the relationship between permanent residency and language investments among skilled migrants. Prior studies primarily concentrate on refugees and low- to middle-skilled workers, whose incentives and options may differ significantly from those of high-skilled workers.

Lastly, our study offers insights into the far-reaching societal implications of residency policies on family formation, stability, and the effects on partners. These findings underscore the multifaceted influence of these types of policies.

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## Tables

Table 1: Effect on residency in Sweden 3 years post-graduation

| | All | | Cohorts 2013-14 | | Cohorts 2015-17 | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | 0.135*** | 0.118*** | 0.071** | 0.062* | 0.171*** | 0.146*** |
| | (0.024) | (0.027) | (0.028) | (0.034) | (0.023) | (0.028) |
| Observations | 25029 | 6810 | 17135 | 3946 | 19929 | 5277 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 |
| Mean (EU/EEA) | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 |
| Mean (Swedes) | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 |
| Wild-bootstrap p-val | 0.003 | 0.001 | 0.028 | 0.112 | 0.001 | 0.000 |
| Pretest p-val | 0.948 | 0.789 | 0.948 | 0.789 | 0.948 | 0.789 |

Notes: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Using a DID design with Swedish and European students as the comparison group, this table reports ATT estimates of the effect of the reform on international students, estimated with equation 1. The outcome is a binary variable indicating whether a doctoral student stays in Sweden 3 years post-graduation. The odd-numbered columns, i.e., 1, 3, and 5, use both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. The even-numbered columns, i.e., 2, 4, and 6, only use the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout. Columns 1-2 use all post-reform cohorts. Columns 3-4 use the cohorts graduating between 2013-14 whereas columns 5-6 use those graduating between 2015-17. Mean(-) represents the pre-reform mean for the corresponding group. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

Table 2: Residency effects on the Swedish and European Students

| | Swedes | | EU/EEA | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| No of international peers staying in Sweden | 0.0004 | 0.0011 | 0.0023 | 0.0098** |
| | (0.0003) | (0.0009) | (0.0020) | (0.0044) |
| Model | OLS | IV | OLS | IV |
| Observations | 18219 | 18219 | 2506 | 2506 |
| No of clusters | 60 | 60 | 47 | 47 |
| Mean | 0.94 | 0.94 | 0.58 | 0.58 |
| First-stage coef | - | 4.75 | - | 4.99 |
| First-stage coef p-val | - | 0.00 | - | 0.00 |
| First-stage F stat | - | 13.80 | - | 11.96 |
| AR Wald test p-val | - | 0.20 | - | 0.02 |

Notes: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. This table presents the OLS (columns 1 and 3) and IV-DID (columns 2 and 4) estimates of the peer effects of more international students from the same research field staying in Sweden 3 years post-graduation. The corresponding specification is reproted in equation 3. The outcome is a binary variable indicating whether a doctoral student stays in Sweden 3 years post-graduation. The instrument denotes the quintile of a research field based on its share of graduating students that are international over the pre-reform years 2008-2010. The mean shares across the five quintiles are 0, 3, 8, 12, and 22%, respectively. The outcome for the first stage is the number of international graduates in an individual's research field in a given year that stay in Sweden 3 years post-graduation. For the average Swedish and European students, in the pre-reform period, the mean numbers of international peers from the same research field that stay are 6 and 7, respectively. Mean(-) represents the pre-reform main outcome mean for the corresponding group. Standard errors are clustered by research field and reported in parentheses. First-stage F-stats are Kleibergen-Paap cluster-robust rk Wald F statistics. AR Wald test p-values are from the Anderson-Rubin Wald test providing weak-instrument robust inference for testing the significance of the endogenous regressor in the main equation.

Table 3: Effect on enrolment at Swedish for Immigrants (SFI)

| | All | Cohorts 2013-14 | Cohorts 2015-17 |
|---|---|---|---|
| | (1) | (2) | (3) |
| Reform | 0.056** | 0.038 | 0.065** |
| | (0.022) | (0.026) | (0.024) |
| Observations | 6810 | 3946 | 5277 |
| Comparison group | EU/EEA | EU/EEA | EU/EEA |
| Mean (Intl) | 0.27 | 0.27 | 0.27 |
| Mean (EU/EEA) | 0.16 | 0.16 | 0.16 |
| Wild-bootstrap p-val | 0.020 | 0.178 | 0.014 |
| Pretest p-val | 0.500 | 0.500 | 0.500 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Using a DID design with European students as the comparison group, this table reports ATT estimates of the effect of the reform on international students, estimated with equation 1. The outcome is a binary variable indicating whether a doctoral student enrolls at Swedish for Immigrants (SFI) during their study. The sample comprises 25 country-groups/clusters, with 15 of them forming the Treatment group of international students. Column 1 uses all post-reform cohorts. Column 2 uses the cohorts graduating between 2013-14 whereas column 3 uses those graduating between 2015-17. Mean(-) represents the pre-reform mean for the corresponding group. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

Table 4: Effect on marriage and partnership

| *Panel A: All students* | | | | | | |
|---|---|---|---|---|---|---|
| | All | | Cohorts 2013-14 | | Cohorts 2015-17 | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | 0.032*** | 0.025 | 0.021 | 0.007 | 0.038*** | 0.037 |
| | (0.010) | (0.020) | (0.013) | (0.018) | (0.012) | (0.023) |
| Observations | 25029 | 6810 | 17135 | 3946 | 19929 | 5277 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.21 | 0.21 | 0.21 | 0.21 | 0.21 | 0.21 |
| Mean (EU/EEA) | 0.17 | 0.17 | 0.17 | 0.17 | 0.17 | 0.17 |
| Mean (Swedes) | 0.24 | 0.24 | 0.24 | 0.24 | 0.24 | 0.24 |
| Wild-bootstrap p-val | 0.075 | 0.231 | 0.213 | 0.721 | 0.063 | 0.137 |
| Pretest p-val | 0.658 | 0.628 | 0.658 | 0.628 | 0.658 | 0.628 |
| *Panel B: Students single at registration* | | | | | | |
| | All | | Cohorts 2013-14 | | Cohorts 2015-17 | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | 0.038*** | 0.033 | 0.029* | 0.015 | 0.042*** | 0.041 |
| | (0.013) | (0.024) | (0.017) | (0.023) | (0.013) | (0.026) |
| Observations | 18232 | 5179 | 12458 | 2904 | 14575 | 4006 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 | 0.35 |
| Mean (EU/EEA) | 0.20 | 0.20 | 0.20 | 0.20 | 0.20 | 0.20 |
| Mean (Swedes) | 0.32 | 0.32 | 0.32 | 0.32 | 0.32 | 0.32 |
| Wild-bootstrap p-val | 0.059 | 0.180 | 0.160 | 0.531 | 0.049 | 0.142 |
| Pretest p-val | 0.708 | 0.747 | 0.708 | 0.747 | 0.708 | 0.747 |

Notes: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Using a DID design with Swedish and European students as the comparison group, this table reports ATT estimates of the effect of the reform on international students, estimated with equation 1. The outcome is a binary variable indicating whether a doctoral student gets married or registers a partnership during their study. Estimates in Panel A include all students whereas those reported in Panel B include students who are single in the year of first registration, i.e., neither married nor in a registered partnership. The odd-numbered columns, i.e., 1, 3, and 5, use both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. The even-numbered columns, i.e., 2, 4, and 6, only use the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout. Columns 1-2 use all post-reform cohorts. Columns 3-4 use the cohorts graduating between 2013-14 whereas columns 5-6 use those graduating between 2015-17. Mean(-) represents the pre-reform mean for the corresponding group. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

Table 5: Effect on marital sorting by education

| *Panel A: All students* | | | | | | |
|---|---|---|---|---|---|---|
| | All | | Cohorts 2013-14 | | Cohorts 2015-17 | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | 0.034** | 0.042* | 0.016 | 0.012 | 0.045*** | 0.059** |
| | (0.013) | (0.024) | (0.018) | (0.025) | (0.014) | (0.027) |
| Observations | 24138 | 6377 | 16571 | 3694 | 19220 | 4951 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.14 | 0.14 | 0.14 | 0.14 | 0.14 | 0.14 |
| Mean (EU/EEA) | 0.13 | 0.13 | 0.13 | 0.13 | 0.13 | 0.13 |
| Mean (Swedes) | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 |
| Mean (Intl)\|New partner | 0.90 | 0.90 | 0.90 | 0.90 | 0.90 | 0.90 |
| Mean (EU/EEA)\|New partner | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 |
| Mean (Swedes)\|New partner | 0.88 | 0.88 | 0.88 | 0.88 | 0.88 | 0.88 |
| Wild-bootstrap p-val | 0.095 | 0.117 | 0.534 | 0.665 | 0.037 | 0.047 |
| Pretest p-val | 0.353 | 0.150 | 0.353 | 0.150 | 0.353 | 0.150 |
| *Panel B: Students single at registration* | | | | | | |
| | All | | Cohorts 2013-14 | | Cohorts 2015-17 | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | 0.049*** | 0.057* | 0.024 | 0.019 | 0.060*** | 0.075** |
| | (0.017) | (0.029) | (0.025) | (0.032) | (0.017) | (0.033) |
| Observations | 17341 | 4746 | 11894 | 2652 | 13866 | 3680 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.23 | 0.23 | 0.23 | 0.23 | 0.23 | 0.23 |
| Mean (EU/EEA) | 0.16 | 0.16 | 0.16 | 0.16 | 0.16 | 0.16 |
| Mean (Swedes) | 0.26 | 0.26 | 0.26 | 0.26 | 0.26 | 0.26 |
| Mean (Intl)\|New partner | 0.90 | 0.90 | 0.90 | 0.90 | 0.90 | 0.90 |
| Mean (EU/EEA)\|New partner | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 |
| Mean (Swedes)\|New partner | 0.88 | 0.88 | 0.88 | 0.88 | 0.88 | 0.88 |
| Wild-bootstrap p-val | 0.070 | 0.085 | 0.487 | 0.588 | 0.027 | 0.042 |
| Pretest p-val | 0.264 | 0.146 | 0.264 | 0.146 | 0.264 | 0.146 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Using a DID design with Swedish and European students as the comparison group, this table reports ATT estimates of the effect of the reform on international students, estimated with equation 1. The outcome is a binary variable indicating whether, during the PhD, a student gets married or registers a partnership with at least a university degree. Estimates in Panel A include all students whereas those reported in Panel B include students who are single in the first year of registration, i.e., neither married nor in a registered partnership. The odd-numbered columns, i.e., 1, 3, and 5, use both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. The even-numbered columns, i.e., 2, 4, and 6, only use the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout. Columns 1-2 use all post-reform cohorts. Columns 3-4 use the cohorts graduating between 2013-14 whereas columns 5-6 use those graduating between 2015-17. Mean(-) represents the pre-reform mean for the corresponding group. Mean(-|New Partner) represents the pre-reform mean for the corresponding group conditional on getting married or registering a partnership during the doctoral studies. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

Table 6: Effect on divorce/separation

| *Panel A: All students* | | | | | | |
|---|---|---|---|---|---|---|
| | All | | Cohorts 2013-14 | | Cohorts 2015-17 | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | 0.002 | 0.001 | 0.001 | -0.002 | 0.002 | 0.004 |
| | (0.003) | (0.005) | (0.005) | (0.007) | (0.004) | (0.006) |
| Observations | 25029 | 6810 | 17135 | 3946 | 19929 | 5277 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 |
| Mean (EU/EEA) | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 |
| Mean (Swedes) | 0.03 | 0.03 | 0.03 | 0.03 | 0.03 | 0.03 |
| Wild-bootstrap p-val | 0.660 | 0.767 | 0.905 | 0.755 | 0.653 | 0.548 |
| Pretest p-val | 0.314 | 0.829 | 0.314 | 0.829 | 0.314 | 0.829 |
| *Panel B: Students who are married or in a registered partnership at registration* | | | | | | |
| | All | | Cohorts 2013-14 | | Cohorts 2015-17 | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | -0.016* | -0.028 | -0.023* | -0.069* | -0.013 | -0.007 |
| | (0.009) | (0.024) | (0.012) | (0.035) | (0.012) | (0.028) |
| Observations | 6797 | 1631 | 4677 | 1042 | 5354 | 1271 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.05 | 0.05 | 0.05 | 0.05 | 0.05 | 0.05 |
| Mean (EU/EEA) | 0.06 | 0.06 | 0.06 | 0.06 | 0.06 | 0.06 |
| Mean (Swedes) | 0.09 | 0.09 | 0.09 | 0.09 | 0.09 | 0.09 |
| Wild-bootstrap p-val | 0.085 | 0.278 | 0.087 | 0.074 | 0.318 | 0.813 |
| Pretest p-val | 0.991 | 0.686 | 0.991 | 0.686 | 0.991 | 0.686 |

Notes: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Using a DID design with Swedish and European students as the comparison group, this table reports ATT estimates of the effect of the reform on international students, estimated with equation 1. The outcome is a binary variable indicating whether a doctoral student gets a divorce or separates from a registered partnership during their study. Estimates in Panel A include all students whereas those reported in Panel B include students who are either married or in a registered partnership in the first year of registration. The odd-numbered columns, i.e., 1, 3, and 5, use both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. The even-numbered columns, i.e., 2, 4, and 6, only use the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout. Columns 1-2 use all post-reform cohorts. Columns 3-4 use the cohorts graduating between 2013-14 whereas columns 5-6 use those graduating between 2015-17. Mean(-) represents the pre-reform mean for the corresponding group. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

Table 7: Effect on fertility

| *Panel A: All students* | | | | | | |
|---|---|---|---|---|---|---|
| | All | | Cohorts 2013-14 | | Cohorts 2015-17 | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | -0.068* | 0.025 | -0.060 | -0.005 | -0.061* | 0.039 |
| | (0.034) | (0.026) | (0.036) | (0.030) | (0.036) | (0.028) |
| Observations | 25029 | 6810 | 17135 | 3946 | 19929 | 5277 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.31 | 0.31 | 0.31 | 0.31 | 0.31 | 0.31 |
| Mean (EU/EEA) | 0.28 | 0.28 | 0.28 | 0.28 | 0.28 | 0.28 |
| Mean (Swedes) | 0.37 | 0.37 | 0.37 | 0.37 | 0.37 | 0.37 |
| Wild-bootstrap p-val | 0.571 | 0.427 | 0.413 | 0.939 | 0.634 | 0.271 |
| Pretest p-val | 0.722 | 0.818 | 0.722 | 0.818 | 0.722 | 0.818 |
| *Panel B: Students who are married or in a registered partnership at registration* | | | | | | |
| | All | | Cohorts 2013-14 | | Cohorts 2015-17 | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | -0.094** | 0.045 | -0.076 | 0.071 | -0.095** | 0.022 |
| | (0.041) | (0.048) | (0.048) | (0.064) | (0.039) | (0.067) |
| Observations | 6797 | 1631 | 4677 | 1042 | 5353 | 1270 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.50 | 0.50 | 0.50 | 0.50 | 0.50 | 0.50 |
| Mean (EU/EEA) | 0.65 | 0.65 | 0.65 | 0.65 | 0.65 | 0.65 |
| Mean (Swedes) | 0.36 | 0.36 | 0.36 | 0.36 | 0.36 | 0.36 |
| Wild-bootstrap p-val | 0.399 | 0.374 | 0.507 | 0.304 | 0.292 | 0.745 |
| Pretest p-val | 0.710 | 0.034 | 0.710 | 0.034 | 0.710 | 0.034 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Using a DID design with Swedish and European students as the comparison group, this table reports ATT estimates of the effect of the reform on international students, estimated with equation 1. The outcome is a binary variable indicating whether a doctoral student becomes a parent of at least one child during their study. Estimates in Panel A include all students whereas those reported in Panel B include students who are either married or in a registered partnership in the first year of registration. The odd-numbered columns, i.e., 1, 3, and 5, use both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. The even-numbered columns, i.e., 2, 4, and 6, only use the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout. Columns 1-2 use all post-reform cohorts. Columns 3-4 use the cohorts graduating between 2013-14 whereas columns 5-6 use those graduating between 2015-17. Mean(-) represents the pre-reform mean for the corresponding group. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

Table 8: Effect on residency in Sweden 3 years post-graduation: Heterogeneity

| | Female | | STEM | | Married at registration | | Has child at registration | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Reform | 0.156*** | 0.136*** | 0.087** | 0.088** | 0.105*** | 0.093*** | 0.107*** | 0.099*** |
| | (0.029) | (0.039) | (0.035) | (0.038) | (0.016) | (0.023) | (0.019) | (0.025) |
| Reform X Female | -0.070* | -0.060 | | | | | | |
| | (0.036) | (0.054) | | | | | | |
| Reform X STEM | | | 0.072*** | 0.042 | | | | |
| | | | (0.024) | (0.050) | | | | |
| Reform X Married at registration | | | | | 0.077** | 0.040 | | |
| | | | | | (0.033) | (0.082) | | |
| Reform X Child at registration | | | | | | | 0.139*** | 0.098 |
| | | | | | | | (0.038) | (0.064) |
| Observations | 25029 | 6810 | 25029 | 6810 | 25029 | 6810 | 25029 | 6810 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 |
| Mean (EU/EEA) | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 |
| Mean (Swedes) | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 |
| Mean (Intl)\|(X) | 0.62 | 0.62 | 0.57 | 0.57 | 0.55 | 0.55 | 0.46 | 0.46 |
| Mean (EU/EEA)\|(X) | 0.61 | 0.61 | 0.54 | 0.54 | 0.64 | 0.64 | 0.59 | 0.59 |
| Mean (Swedes)\|(X) | 0.95 | 0.95 | 0.92 | 0.92 | 0.97 | 0.97 | 0.97 | 0.97 |
| Wild-bootstrap p-val (Reform) | 0.004 | 0.002 | 0.010 | 0.042 | 0.001 | 0.003 | 0.005 | 0.006 |
| Wild-bootstrap p-val (Reform X -) | 0.062 | 0.335 | 0.020 | 0.468 | 0.073 | 0.649 | 0.002 | 0.146 |

Notes: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Using a DID design with Swedish and European students as the comparison group, this table explores heterogeneity in the ATT estimates of the effect of the reform on international students, estimated with equation 4, and using the following dimensions: whether a student is – female (columns 1-2); registered in a STEM program (columns 3-4); married at registration (columns 5-6); has child at registration (columns 7-8). The outcome is a binary variable indicating whether a doctoral student stays in Sweden 3 years post-graduation. The odd-numbered columns, i.e., 1, 3, 5, and 7, use both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. The even-numbered columns, i.e., 2, 4, 6 and 8, only use the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout. Mean(-) represents the pre-reform mean for the corresponding group. Mean(-|(X)) represents the pre-reform mean for the corresponding group conditional on the indicator X switching on; e.g., Mean(Intl|(Female)) represents the pre-reform mean of the outcome for the international **female** students. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row.

Table 9: Effect on the partner's enrolment at Swedish for Immigrants (SFI)

| | All | Cohorts 2013-14 | Cohorts 2015-17 |
|---|---|---|---|
| | (1) | (2) | (3) |
| Reform | 0.033* | 0.015 | 0.042** |
| | (0.017) | (0.022) | (0.015) |
| Observations | 6810 | 3946 | 5277 |
| Comparison group | EU/EEA | EU/EEA | EU/EEA |
| Mean (Intl) | 0.19 | 0.19 | 0.19 |
| Mean (EU/EEA) | 0.03 | 0.03 | 0.03 |
| Mean (Intl) pre \|Any partner | 0.31 | 0.31 | 0.31 |
| Mean (EU/EEA) pre \|Any partner | 0.10 | 0.10 | 0.10 |
| Mean (Intl) post \|Any partner | 0.36 | | |
| Mean (EU/EEA) post \|Any partner | 0.11 | | |
| Wild-bootstrap p-val | 0.136 | 0.553 | 0.028 |
| Pretest p-val | 0.469 | 0.469 | 0.469 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Using a DID design with European students as the comparison group, this table reports ATT estimates of the effect of the reform on the partners of the international students, estimated with equation 1. The outcome is a binary variable indicating whether a doctoral student's partner enrolls at Swedish for Immigrants (SFI) during the doctoral studies. All students without a partner during this period are assigned zero. The sample comprises 25 country-groups/clusters, with 15 of them forming the Treatment group of international students. Column 1 uses all post-reform cohorts. Column 2 uses the cohorts graduating between 2013-14 whereas column 3 uses those graduating between 2015-17. Mean(-) represents the pre-reform mean for the corresponding group. Mean(- pre|Any Partner) represents the pre-reform mean for the corresponding group conditional on being married or in a registered partnership during the doctoral studies. Mean(- post|Any Partner) represents the post-reform mean for the corresponding group conditional on being married or in a registered partnership during the doctoral studies. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

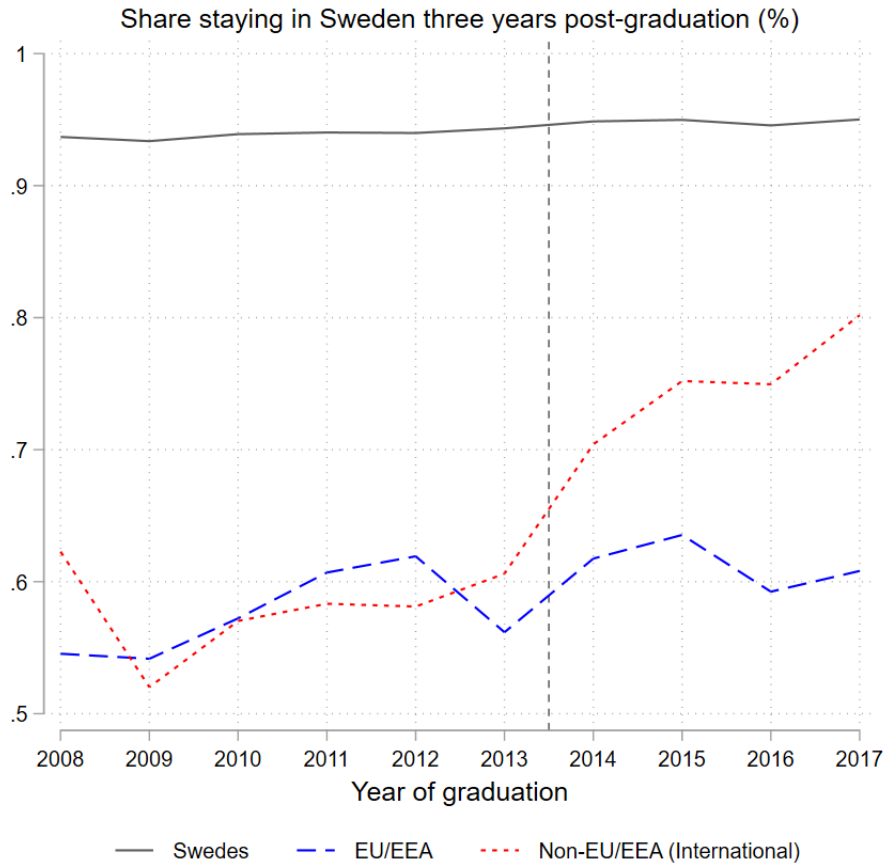
Table 10: Effect on the partner's employment

| | All | Cohorts 2013-14 | Cohorts 2015-17 |
|---|---|---|---|
| | (1) | (2) | (3) |
| Reform | 0.049** | 0.020 | 0.067** |
| | (0.022) | (0.021) | (0.026) |
| Observations | 6810 | 3946 | 5277 |
| Comparison group | EU/EEA | EU/EEA | EU/EEA |
| Mean (Intl) | 0.28 | 0.28 | 0.28 |
| Mean (EU/EEA) | 0.25 | 0.25 | 0.25 |
| Mean (Intl)\|Any partner | 0.46 | 0.46 | 0.46 |
| Mean (EU/EEA)\|Any partner | 0.82 | 0.82 | 0.82 |
| Wild-bootstrap p-val | 0.050 | 0.426 | 0.026 |
| Pretest p-val | 0.795 | 0.795 | 0.795 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Using a DID design with European students as the comparison group, this table reports ATT estimates of the effect of the reform on the partners of the international students, estimated with equation 1. The outcome is a binary variable indicating whether a doctoral student's partner is employed with positive wage income during the doctoral studies. All students without a partner during this period are assigned zero. The sample comprises 25 country-groups/clusters, with 15 of them forming the Treatment group of international students. Column 1 uses all post-reform cohorts. Column 2 uses the cohorts graduating between 2013-14 whereas column 3 uses those graduating between 2015-17. Mean(-) represents the pre-reform mean for the corresponding group. Mean(-|Any Partner) represents the pre-reform mean for the corresponding group conditional on being married or in a registered partnership during the doctoral studies. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

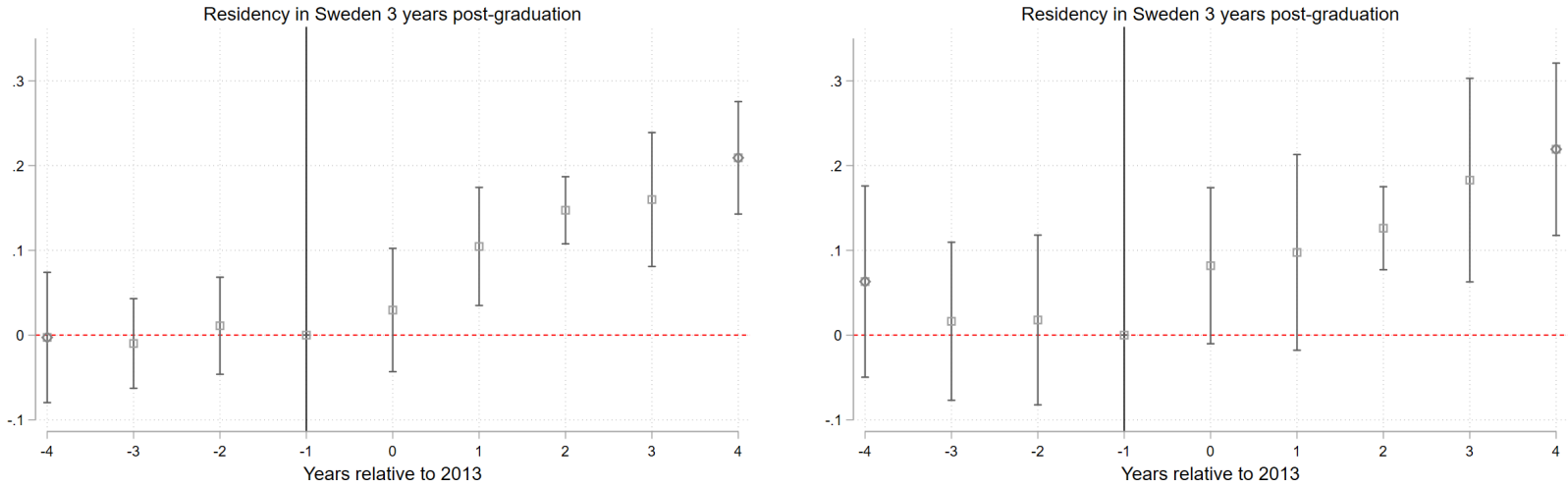
## Figures

Figure 1: Residency in Sweden 3 years post-graduation: Trends across Swedish, European, and International Students




Notes: For the period 2008-2017, this figure reports the trends in the share of doctoral students that stay in Sweden 3 years after graduation, stratified by country/region of origin: Sweden, EU/EEA, and non-EU/EEA (International). The permanent residency reform comes into effect in 2014.

Figure 2: Effect on residency in Sweden 3 years post-graduation: Event study

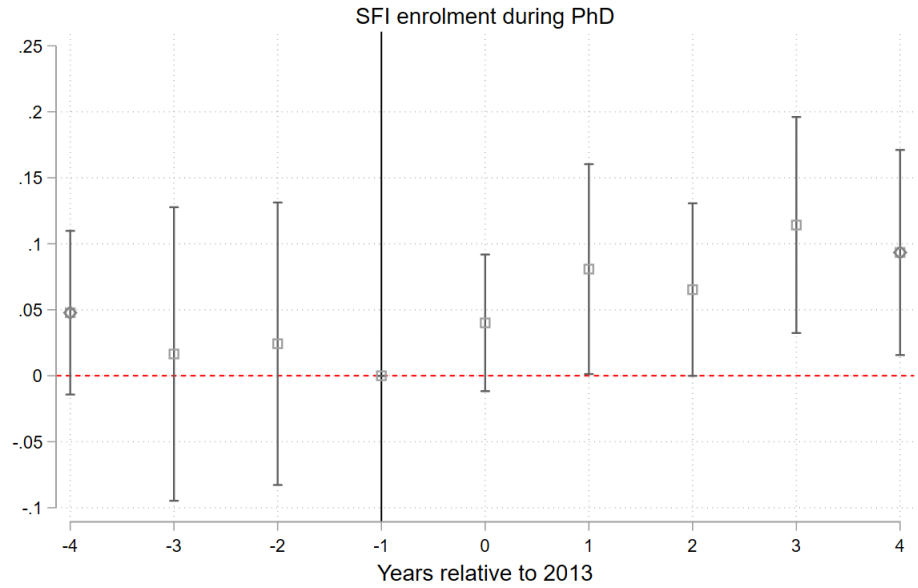



(a) Comparison group: SWE + EU/EEA

(b) Comparison group: EU/EEA

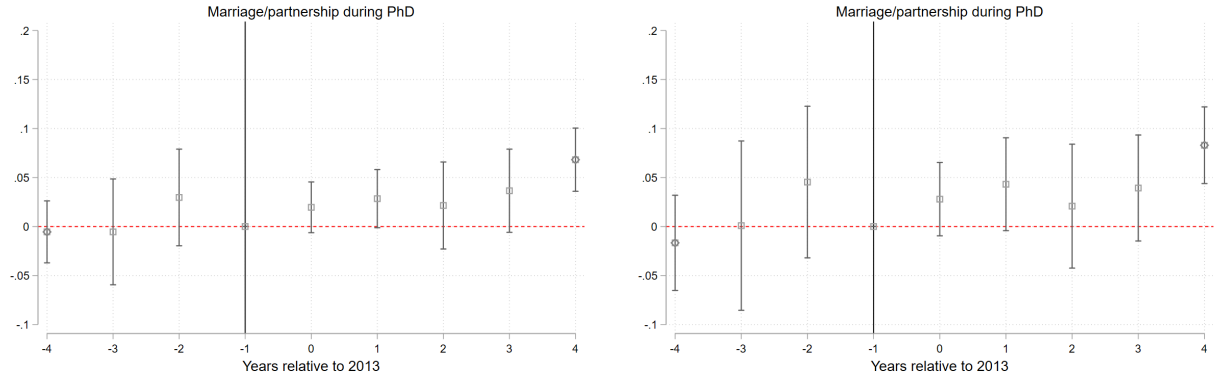
Notes: This figure reports the event-study estimates of the effect of the reform for the different graduating cohorts of international students, estimated with equation 2. Year 0 is 2013. Year t = -1 i.e., 2012, is the omitted group. The reform comes into effect in Year t = +1 i.e., in 2014. For example, the coefficient estimate for Year t = +3 represents the ATT estimate for the cohort of international students graduating in 2016. Ninety percent confidence intervals are displayed around each point estimate. Standard errors are clustered by country-group. The outcome is a binary variable indicating whether a doctoral student stays in Sweden 3 years post-graduation. Panel (a) uses both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. Panel (b) only uses the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout.

Figure 3: Effect on enrolment at Swedish for Immigrants (SFI): Event study




Notes: Using the European students as the comparison group, this figure reports the event-study estimates of the effect of the reform for the different graduating cohorts of international students, estimated with equation 2. Year 0 is 2013. Year t = -1 i.e., 2012, is the omitted group. The reform comes into effect in Year t = +1 i.e., in 2014. For example, the coefficient estimate for Year t = +3 represents the ATT estimate for the cohort of international students graduating in 2016. Ninety percent confidence intervals are displayed around each point estimate. Standard errors are clustered by country-group. The outcome is a binary variable indicating whether a doctoral student enrolls at Swedish for Immigrants (SFI) during their study. The sample comprises 25 country-groups/clusters, with 15 of them forming the Treatment group of international students.

Figure 4: Effect on marriage and partnership: Event study




(a) Sample: All students

(b) Sample: Students single at first registration

Notes: Using Swedish and European students as the comparison group, this figure reports the event-study estimates of the effect of the reform for the different graduating cohorts of international students, estimated with equation 2. Year 0 is 2013. Year t = -1 i.e., 2012, is the omitted group. The reform comes into effect in Year t = +1 i.e., in 2014. For example, the coefficient estimate for Year t = +3 represents the ATT estimate for the cohort of international students graduating in 2016. Ninety percent confidence intervals are displayed around each point estimate. Standard errors are clustered by country-group. The outcome is a binary variable indicating whether a doctoral student gets married or registers a partnership during their study. Panel (a) includes all students whereas Panel (b) includes students who are single in the year of first registration, i.e., neither married nor in a registered partnership. The sample comprises 27 country-groups/clusters, with 15 of them forming the Treatment group of international students.

# Online Supplementary Material

**A Data** **46**
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## A Data

### A.1 Registration, Graduation, and Field of Study or Research

Data on registered doctoral students starts from 1985 and is updated every semester covering all students that are registered in a doctoral program in Sweden during that semester. We have data until the second semester (the Fall semester) of 2021. We define an individual's first semester as a doctoral student as the first semester he or she appears in this dataset.

We combine this registration data with another dataset that records information on the graduates. We use information on all doctoral students who graduate from a Swedish university between 2008 and 2017. In this dataset, we also observe a student's field of study or research.

In 2011, Sweden underwent a revision in its research area classification to align with the OECD framework, known as the Field of Research and Development (FORD). To ensure continuity in tracking research areas across this transition period, we use a translation key provided by the Swedish Higher Education Authority (UKÄ). For details on the classification and how the translation is done, please refer to UKÄ 2016. Our standard reference point is the previous 3-digit code, which has 59 distinct research areas. However, this conversion process is not without intricacies. As some subjects were divided in the older classification but consolidated in the new 5-digit classification, we employ a decision rule to keep the conversion consistent. For subjects in the new classification that can be attributed to more than one group under the old classification, we assign them to the group corresponding to the lowest 3-digit number.

### A.2 Country of Birth

We obtain information on the country of birth from the Swedish Total Register of the Population (RTB). Using this data, we can assign each student to one of the three groups:

- Swedes
- EU/EEA
- Non-EU/EEA (International)

To ensure as accurate an assignment as possible, we apply one exception. Namely, students who resided or engaged in employment in Sweden five years preceding the commencement of their doctoral programs are placed within the Swedish category as these students are likely to have obtained a permanent residency permit already when starting their doctoral programs. These students are labeled as "Non-native Swedes" in Table A.1. This criterion is used for all students regardless of whether their country of birth is inside or outside the EU/EEA. For all other cases, the assignment is determined by the student's country of birth. Table A.1 lists the 30 country groups and their assignments to the three aforementioned groups we use to identify treatment.

Our study includes students who complete their doctoral programs between 2008 and 2017. There were three enlargements of the EU over this period.[49] As all students from countries that joined the EU in 2004 graduated as EU citizens and the joining preceded the year of graduation by at least four years, we treat these individuals as EU/EEA citizens in our main analyses. This classification applies to all countries joining in 2004 except for two countries. Slovenia is omitted from the analyses as they are grouped with countries outside the EU/EEA (30) and Cyprus is assigned to the non-EU/EEA group as they are grouped with countries outside the EU/EEA (44). Countries that joined the EU in the enlargement of 2007 are excluded from the analyses as these countries (Bulgaria and Romania) are grouped alongside countries outside the EU/EEA (36). As a result, individuals originating from these countries are omitted from the analyses due to the unfeasibility of assigning them definitively to either of the three core groups. For the same reason, individuals coming from Croatia are also excluded (30). In addition, to avoid too few observations per cluster, we merged country group 53 (5 observations) with country group 52, leaving us finally with 27 country groups.

[49] In 2004: Cyprus, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovenia, and Slovakia joined. In 2007: Bulgaria and Romania joined. In 2013: Croatia joined.

Table A.1: Country List

| Country Gr. | Country Names | Region | 3-group |
|---|---|---|---|
| 0 | Sweden | Sweden | ”Swedes” |
| 1 | Non-native Sweden | Sweden | ”Swedes” |
| 26 | Finland | Nordic | ”EU/EEA” |
| 27 | Denmark | Nordic | ”EU/EEA” |
| 28 | Iceland, and Norway | Nordic | ”EU/EEA” |
| 29 | Bosnia and Herzegovina | IIT and B & H | ”Non EU/EEA” |
| 30 | Croatia, (North) Macedonia, and Slovenia | | |
| 31 | Poland | Eastern EU | ”EU/EEA” |
| 32 | Ireland, and The United Kingdom | Western EU | ”EU/EEA” |
| 33 | Germany | Western EU | ”EU/EEA” |
| 34 | Greece, Italy, Malta, Monaco, Portugal, San Marino, Spain, and Vatican City State | Western EU | ”EU/EEA” |
| 35 | Estonia, Latvia, and Lithuania | Eastern EU | ”EU/EEA” |
| 36 | Albania, Armenia, Azerbaijan Belarus, Bulgaria, Georgia, Kazakhstan, Kirghistan, Moldova, Romania, Russia, Tajikistan, Turkmenistan, Ukraine, and Uzbekistan | | |
| 37 | Slovakia, Czech Republic, and Hungary | Eastern EU | ”EU/EEA” |
| 38 | Andorra, Austria, Belgium, France, Liechtenstein, Luxembourg, The Netherlands, and Switzerland | Western EU | ”EU/EEA” |

Table A.1: (continued)

| Country Gr. | Country Names | Region | 3-group |
|---|---|---|---|
| 39 | Canada, and USA | N. America and Ocea. | ”Non EU/EEA” |
| 40 | Antigua and Barbuda, Bahamas, Barbados, Belize, Costa Rica, Cuba, Dominica, Dominican Republic, El Salvador, Grenada, Guatemala, Haiti Honduras, Jamaica, Mexico Nicaragua, Panama, S:t Lucia, S:t Vincent, St Kitts and Nevis, and Trinidad and Tobago | C. and S. America | ”Non EU/EEA” |
| 41 | Chile | C. and S. America | ”Non EU/EEA” |
| 42 | Argentina, Bolivia, Brazil, Colombia, Ecuador, Guyana, Paraguay, Peru, Suriname, Uruguay, and Venezuela | C. and S. America | ”Non EU/EEA” |
| 43 | Djibouti, Eritrea, Ethiopia, Somalia, and Sudan | Africa and ME | ”Non EU/EEA” |
| 44 | Algeria, Bahrain, Cyprus, Egypt, Israel, Jordan, Kuwait, Lebanon, Libya, Morocco, Qatar, Saudi Arabia, Syria, Tunisia, UAE, and Yemen | Africa and ME | ”Non EU/EEA” |
| 45 | Angola, Benin, Botswana, Burkina Faso, Burundi, CAF, Cameroon, Cap Verde, Chad, The Comoros, DRC, Equatorial Guinea, Gabon, Gambia, Ghana, Guinea, Guinea-Bissau, Ivory | Africa and ME | ”Non EU/EEA” |

Table A.1: (continued)

| Country Gr. | Country Names | Region | 3-group |
|---|---|---|---|
| | Coast, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritania, Mozambique, Namibia, Niger, Nigeria, Republic of the Congo, Rwanda, Sao Tome and Principe, Senegal, Seychelles, Sierra Leone, South Africa, Swaziland, Tanzania, Togo, Uganda, Zambia, and Zimbabwe | | |
| 46 | Iran | IIT and B & H | ”Non EU/EEA” |
| 47 | Iraq | IIT and B & H | ”Non EU/EEA” |
| 48 | Turkiye | IIT and B & H | ”Non EU/EEA” |
| 49 | Hong Kong, Japan, China, China (Taiwan), North Korea, South Korea | North Asia | ”Non EU/EEA” |
| 50 | Burma, Indonesia, Laos, Malaysia, Philippines, Singapore, Thailand, and Vietnam | C. and S. Asia | ”Non EU/EEA” |
| 51 | Afghanistan, Bangladesh, Bhutan, Brunei, India, Maldives, Mongolia, Nepal, Oman, Pakistan, and Sri Lanka | C. and S. Asia | ”Non EU/EEA” |
| 52 | Australia, Fiji, Kiribati, Nauru, New Zealand, Palau, Papua New Guinea, Samoa, Solomon Islands, Tonga, and Vanuatu | N. America and Ocea. | ”Non EU/EEA” |
| 53 | Unknown or stateless | N. America and Ocea. | ”Non EU/EEA” |

*End of Table*

Table A.2: Share of Students by Country Group

| Country Group | Share (%) | Share excluding Swedes (%) |
|---|---|---|
| 0 | 64.38 | - |
| 1 | 8.41 | - |
| 26 | 0.33 | 1.22 |
| 27 | 0.34 | 1.25 |
| 28 | 0.70 | 2.58 |
| 29 | 0.07 | 0.25 |
| 31 | 0.64 | 2.35 |
| 32 | 0.47 | 1.72 |
| 33 | 2.76 | 10.16 |
| 34 | 2.42 | 8.88 |
| 35 | 0.50 | 1.82 |
| 37 | 0.33 | 1.22 |
| 38 | 1.55 | 5.70 |
| 39 | 0.79 | 2.89 |
| 40 | 0.28 | 1.04 |
| 41 | 0.09 | 0.32 |
| 42 | 0.94 | 3.46 |
| 43 | 0.44 | 1.63 |
| 44 | 0.82 | 3.01 |
| 45 | 0.84 | 3.10 |
| 46 | 2.70 | 9.92 |
| 47 | 0.26 | 0.95 |
| 48 | 0.36 | 1.34 |
| 49 | 5.03 | 18.50 |

Table A.2: (continued)

| Country Group | Share (%) | Share excluding Swedes (%) |
|---|---|---|
| 50 | 1.06 | 3.90 |
| 51 | 3.31 | 12.16 |
| 52 | 0.17 | 0.63 |

*End of Table*

## B The Reform's Impact on the Incoming International Students

In addition to its aim of increasing the number of international students who choose to reside in Sweden after graduation, the reform also sought to enhance Sweden's appeal as a destination for prospective international doctoral students. The following set of results explores how the reform influenced the composition of doctoral students who commenced their studies after the reform was announced.

Ideally, we would have access to data on the number of applicants and their qualifications to comprehensively assess the reform's impact on this front. However, we can only observe the number of students who were accepted, lacking information on the specific criteria leading to their acceptance. Therefore, we investigate how the reform affected the total number of incoming students and their observable characteristics.

To evaluate the reform's impact on the influx of international students, we calculate the annual number of incoming students by country group for the years 2009 to 2019. We then apply Equation 1 at the country group level, excluding the vector of covariates. The findings from this analysis are presented in Table B.1.

In Column 1, employing the full sample of country groups, we observe that from 2014 onward, the number of international students increases by 18.7. However, this effect is not statistically significant. In Column 2, where we exclude Swedes, we obtain a significant negative estimate of 16.3. This suggests a total annual decrease of 245 in the number of international students after the introduction of the reform. However, a major caveat is in order: a reform in 2011 that introduced tuition fees for international master's students led to a substantial decline of over 60% in the number of these students registering in a Swedish master's program. Since, on average, 10% of these international master's students eventually register in a Swedish doctoral program, this decline potentially confounds our analysis of the effect that the permanent residency reform might have on the volume and composition of incoming doctoral students. In fact, a back-of-the-envelope calculation using the decline in the number of master's students (by 2,518) and the average share of these students that typically join a Swedish PhD program afterward (10%), we estimate that the 2011 reform may have led to a loss of 252 potential international PhD students. This loss, therefore, can more than account for the negative estimate we find above.

Turning our attention to the characteristics of incoming students, we explore how the reform affects their composition, as measured by gender, field of study (STEM or non-STEM), marital status at registration, and whether they arrive with children. The results

are presented in Table B.2. In both analyses, whether including all students or excluding Swedish students, we find small and statistically insignificant effects of the reform on the characteristics of incoming international students.

Table B.1: Effect on the number of incoming international students

| | All | EU/EEA |
|---|---|---|
| | (1) | (2) |
| Reform | 18.658 | -16.313* |
| | (34.326) | (8.233) |
| Observations | 297 | 275 |
| Mean (Intl) | 50.66 | 50.66 |
| Mean (EU/EEA) | 40.84 | 40.84 |
| Mean (Swedes) | 1110.50 | 1110.50 |
| Wild-bootstrap p-val | 0.888 | 0.035 |
| Pretest p-val | 0.853 | 0.227 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Using a DID design with Swedish and European country-groups as the comparison group, this table reports the ATT estimates of the effect of the reform on the number of incoming international doctoral students after the reform i.e., in or after 2014. The effects are estimated with equation 1 but at the country-group level rather than at the individual level, and without any covariates. The outcome is the number of students aggregated at the country-group-year level. Column 1 uses all country groups, meaning that we have a total of 27 clusters and 297 observations (27*11). Column 2 excludes the Swedish and non-native Swedish country-groups, yielding a total of 25 clusters and 275 observations (25*11). The number of clusters forming the Treatment group is 15 throughout. Mean(-) represents the pre-reform average number of students per country group for the corresponding group. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the corresponding event-study equation 2.

Table B.2: Effect on the selection of incoming international students

| | Female | | Married/registered partnership | | Had child | | STEM | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Reform | 0.019 | -0.011 | -0.005 | 0.010 | -0.009 | 0.003 | 0.025 | 0.025 |
| | (0.030) | (0.032) | (0.015) | (0.020) | (0.009) | (0.011) | (0.024) | (0.028) |
| Observations | 34829 | 12794 | 34831 | 12796 | 34831 | 12796 | 34047 | 12629 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.36 | 0.36 | 0.28 | 0.28 | 0.11 | 0.11 | 0.65 | 0.65 |
| Mean (EU/EEA) | 0.44 | 0.44 | 0.11 | 0.11 | 0.09 | 0.09 | 0.56 | 0.56 |
| Mean (Swedes) | 0.52 | 0.52 | 0.34 | 0.34 | 0.39 | 0.39 | 0.33 | 0.33 |
| Wild-bootstrap p-val | 0.593 | 0.742 | 0.744 | 0.612 | 0.450 | 0.786 | 0.395 | 0.438 |
| Pretest p-val | 0.576 | 0.069 | 0.185 | 1.000 | 0.602 | 0.837 | 0.586 | 0.968 |

Notes: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. The sample comprises all doctoral students who start their studies between 2009-19. Using a DID design with Swedish and European students as the comparison group, this table reports ATT estimates of the effect of the reform on the selection of international students who start their PhD after the reform i.e., in or after 2014, estimated with equation 1, with age-quintiles as the only covariate. The outcome variables, using information recorded in the first year of registration, are: whether a student is – female (columns 1-2); married at registration (columns 3-4); has a child at registration (columns 5-6); registered in a STEM program (columns 7-8). The odd-numbered columns, i.e., 1, 3, 5, and 7, use both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. The even-numbered columns, i.e., 2, 4, 6, and 8, only use the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout. Mean(-) represents the pre-reform mean of the outcome variables for the corresponding group. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

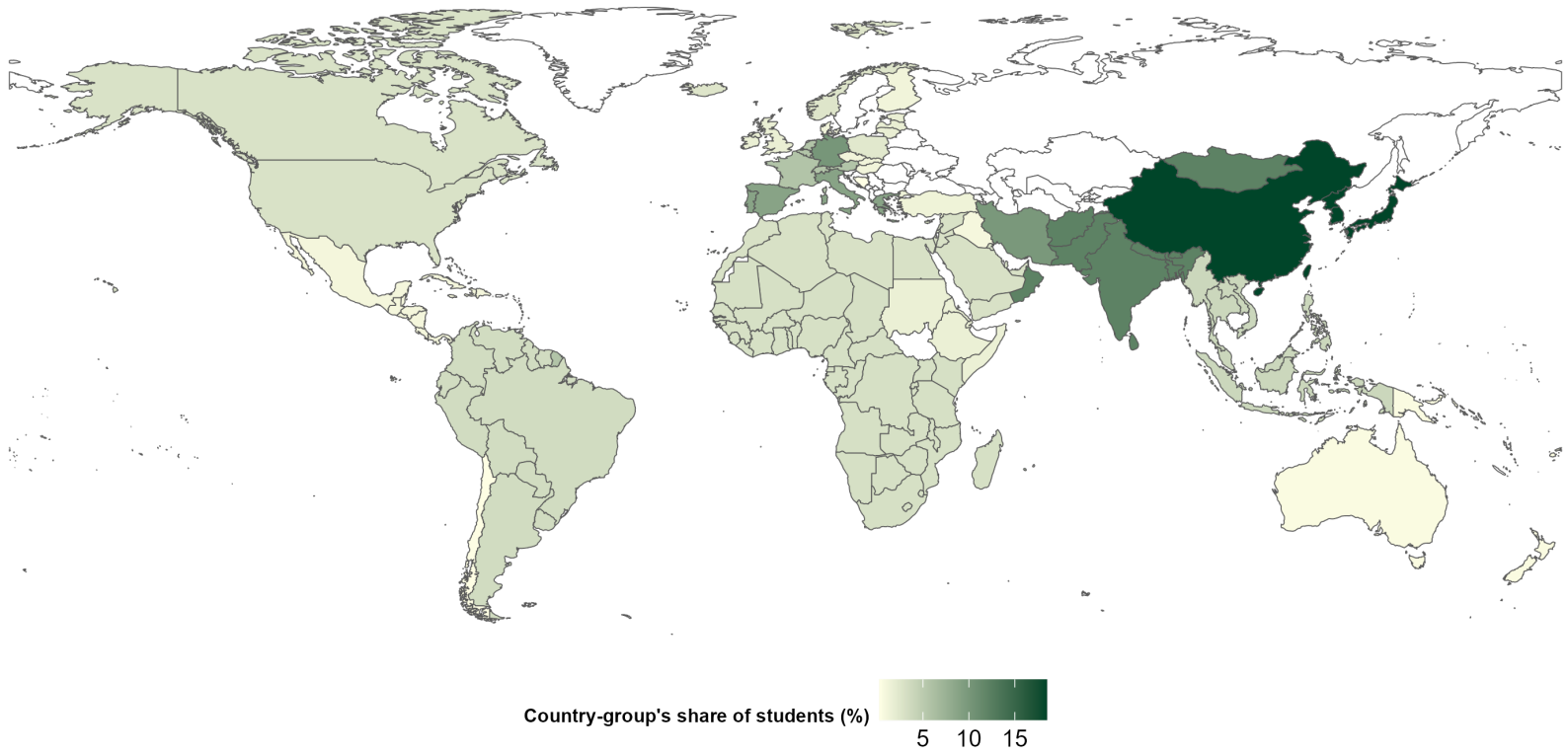
# C Tables and Graphs I

Table C.1: Descriptive Statistics: Doctoral students graduating from Swedish Universities between 2008-2017

| | Swedes | EU/EEA | Non EU/EEA (International) |
|---|---|---|---|
| Females, share | 0.528 | 0.441 | 0.344 |
| | (0.499) | (0.497) | (0.475) |
| Age at first semester (years) | 31.85 | 27.42 | 28.88 |
| | (8.017) | (3.624) | (4.691) |
| STEM student, share | 0.361 | 0.568 | 0.634 |
| | (0.480) | (0.495) | (0.482) |
| Married at first semester, share | 0.284 | 0.115 | 0.312 |
| | (0.451) | (0.320) | (0.463) |
| Has child at first semester, share | 0.318 | 0.084 | 0.138 |
| | (0.466) | (0.277) | (0.345) |
| Observations | 18219 | 2513 | 4299 |

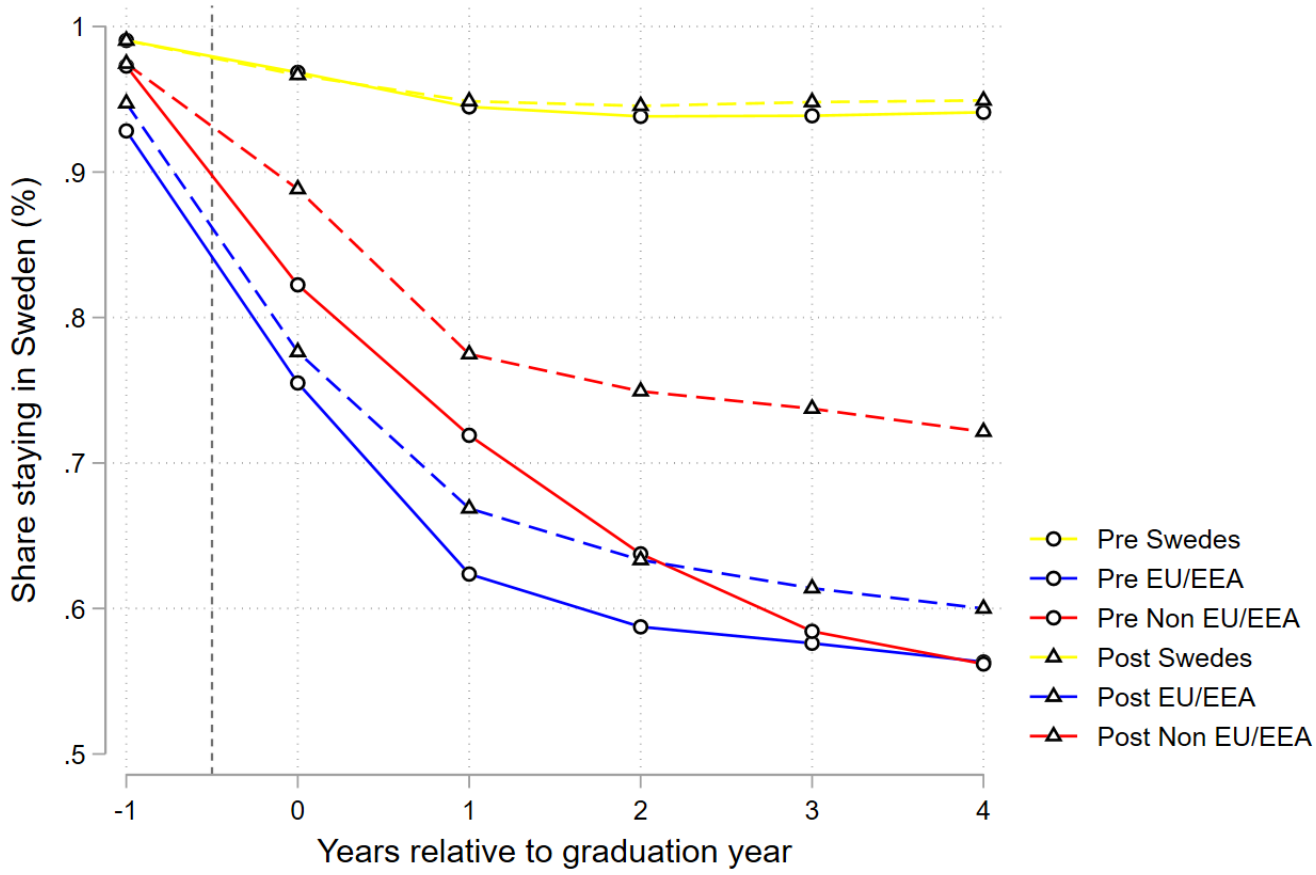
Notes: This table shows the averages, stratified by country/region of origin, for different variables measured at the year of registration. Standard deviations are presented in parentheses.

Figure C.1: Geographic distribution of students born outside Sweden



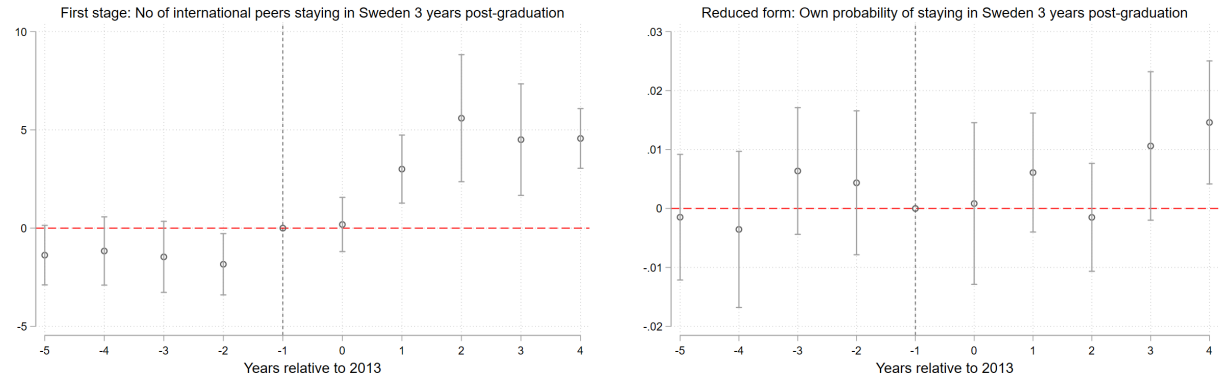

Notes: This map plots the country-group-specific share of doctoral students that are born outside Sweden.

Figure C.2: Residency in Sweden post-graduation: Swedish, European, and International Students




Notes: This figure reports, for -1(1)+4 year(s) relative to graduation, the share of doctoral students that stay in Sweden, stratified by pre- (2008-12) and post-reform (2013-2017) cohorts and country/region of origin: Sweden, EU/EEA, and non-EU/EEA (International).

Figure C.3: Peer effects on residency in Sweden 3 years post-graduation: Swedish doctoral students

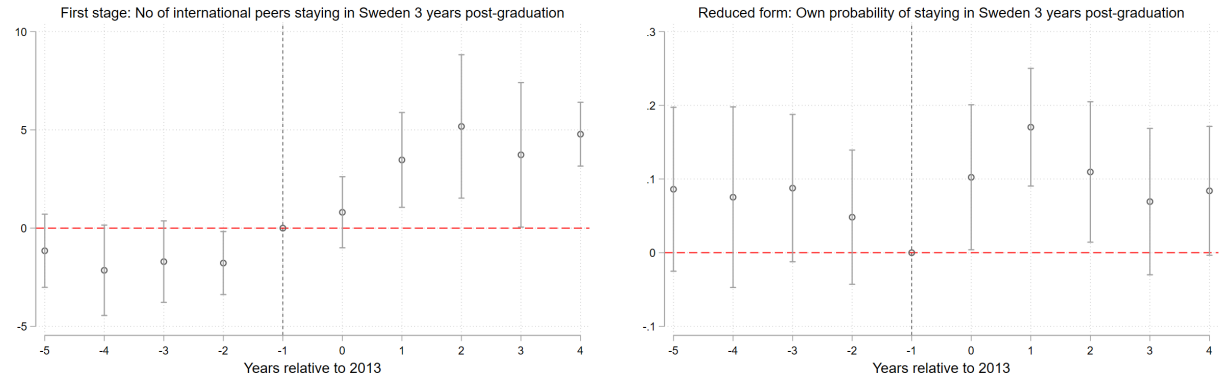



(a) First stage

(b) Reduced form

Notes: This figure reports the event-study estimates corresponding to Equation 3, using the sample of Swedish doctoral students. Year 0 is 2013. Year t = -1 i.e., 2012, is the omitted group. The reform comes into effect in Year t = +1 i.e., in 2014. For example, the coefficient estimate for Year t = +3 represents the IV-DID estimate for the cohort of Swedish students graduating in 2016. Ninety percent confidence intervals are displayed around each point estimate. Standard errors are clustered by research field. There are 60 unique research fields. Panel (a) reports the first-stage estimates. The outcome is the no of international graduates in an individual's research field in a given year who choose to stay in Sweden 3 years after graduation. The instrument is constructed as follows: for all pre-reform years, i.e., 2008-2012, it is zero; from 2013 onward, it denotes the quintile of the corresponding research field based on its share of graduating students that are international over the pre-reform years 2008-2010. The mean shares across the five quintiles are 0, 3, 8, 12, and 22%, respectively. The pretest p-value is 0.41. Panel (b) reports the corresponding reduced-form estimates. The outcome is a binary variable indicating whether a Swedish doctoral student stays in Sweden 3 years post-graduation. The pretest p-value is 0.60.

Figure C.4: Peer effects on residency in Sweden 3 years post-graduation: European doctoral students




(a) First stage

(b) Reduced form

Notes: This figure reports the event-study estimates corresponding to Equation 3, using the sample of European doctoral students. Year 0 is 2013. Year t = -1 i.e., 2012, is the omitted group. The reform comes into effect in Year t = +1 i.e., in 2014. For example, the coefficient estimate for Year t = +3 represents the IV-DID estimate for the cohort of European students graduating in 2016. Ninety percent confidence intervals are displayed around each point estimate. Standard errors are clustered by research field. There are 47 unique research fields. Panel (a) reports the first-stage estimates. The outcome is the no of international graduates in an individual's research field in a given year who choose to stay in Sweden 3 years after graduation. The instrument is constructed as follows: for all pre-reform years, i.e., 2008-2012, it is zero; from 2013 onward, it denotes the quintile of the corresponding research field based on its share of graduating students that are international over the pre-reform years 2008-2010. The mean shares across the five quintiles are 0, 3, 8, 12, and 22%, respectively. The pretest p-value is 0.32. Panel (b) reports the corresponding reduced-form estimates. The outcome is a binary variable indicating whether a European doctoral student stays in Sweden 3 years post-graduation. The pretest p-value is 0.54.

Table C.2: Effect on residency in Sweden 3 years post-graduation: Robustness

| | Without Controls | | Control X Year FEs | | Balanced Panel | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | 0.127*** | 0.118*** | 0.128*** | 0.106*** | 0.135*** | 0.118*** |
| | (0.025) | (0.030) | (0.023) | (0.024) | (0.024) | (0.028) |
| Observations | 25031 | 6812 | 25029 | 6810 | 24925 | 6706 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.58 | 0.58 | 0.58 | 0.58 | 0.57 | 0.57 |
| Mean (EU/EEA) | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 | 0.58 |
| Mean (Swedes) | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 | 0.94 |
| Wild-bootstrap p-val | 0.004 | 0.002 | 0.001 | 0.001 | 0.003 | 0.001 |
| Pretest p-val | 0.972 | 0.817 | 0.869 | 0.266 | 0.983 | 0.736 |

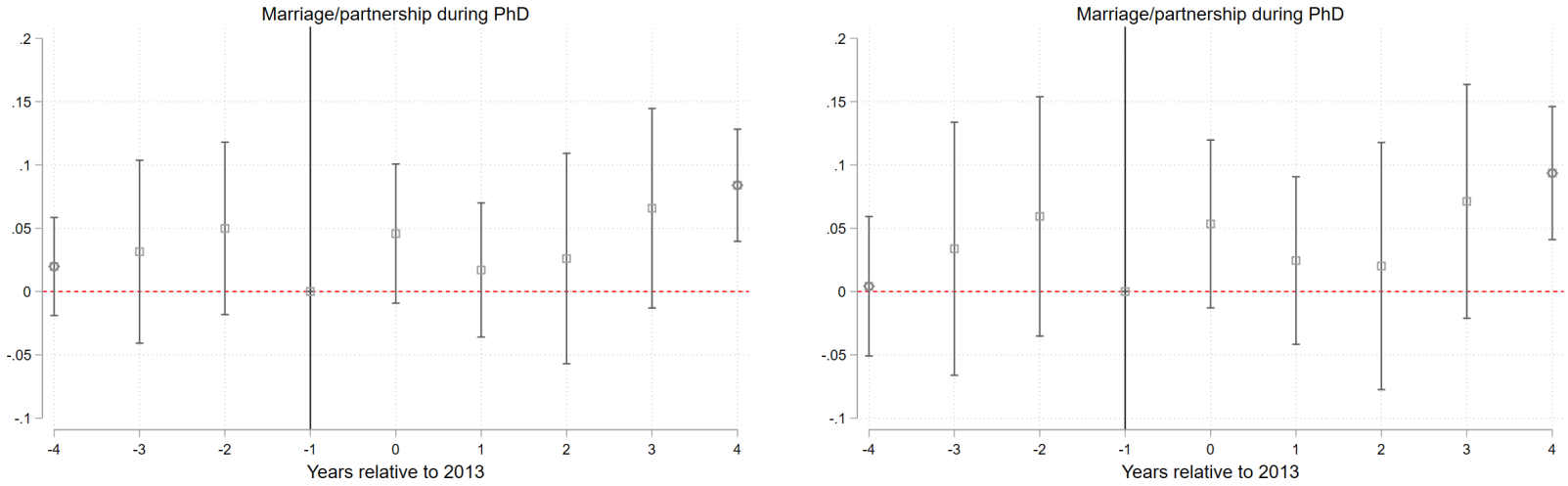
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Using a DID design with Swedish and European students as the comparison group, this table reports ATT estimates of the effect of the reform on international students, estimated with equation 1. The outcome is a binary variable indicating whether a doctoral student stays in Sweden 3 years post-graduation. The odd-numbered columns, i.e., 1, 3, and 5, use both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. The even-numbered columns, i.e., 2, 4, and 6, only use the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout. Estimates are analogous to those reported in columns 1-2 in Table 1, with the following modifications. Columns 1-2 exclude all controls. Columns 3-4 augment the benchmark model by adding interactions between year fixed effects and each of the controls. Finally, columns 5-6 restrict the sample to a balanced panel at the country-year level, thereby excluding 3 country-groups. Mean(-) represents the pre-reform mean for the corresponding group. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

Table C.3: Effect on enrolment at Swedish for Immigrants (SFI): Robustness

| | Without Controls | Control X Year FEs | Balanced Panel |
|---|---|---|---|
| | (1) | (2) | (3) |
| Reform | 0.062** | 0.057** | 0.059** |
| | (0.023) | (0.024) | (0.022) |
| Observations | 6812 | 6810 | 6706 |
| Comparison group | | | |
| Mean (Intl) | 0.27 | 0.27 | 0.27 |
| Mean (EU/EEA) | 0.16 | 0.16 | 0.16 |
| Wild-bootstrap p-val | 0.015 | 0.060 | 0.012 |
| Pretest p-val | 0.572 | 0.136 | 0.419 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Using a DID design with European students as the comparison group, this table reports ATT estimates of the effect of the reform on international students, estimated with equation 1. The outcome is a binary variable indicating whether a doctoral student enrolls at Swedish for Immigrants (SFI) during their study. The sample comprises 25 country-groups/clusters, with 15 of them forming the Treatment group of international students. Estimates are analogous to those reported in column 1 in Table 1, with the following modifications. Column 1 excludes all controls. Column 2 augments the benchmark model by adding interactions between year fixed effects and each of the controls. Finally, column 3 restricts the sample to a balanced panel at the country-year level, thereby excluding 3 country-groups. Mean(-) represents the pre-reform mean for the corresponding group. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

Figure C.5: Effect on marriage and partnership: Event study with only Europeans for comparison




(a) Sample: All students

(b) Sample: Students single at first registration

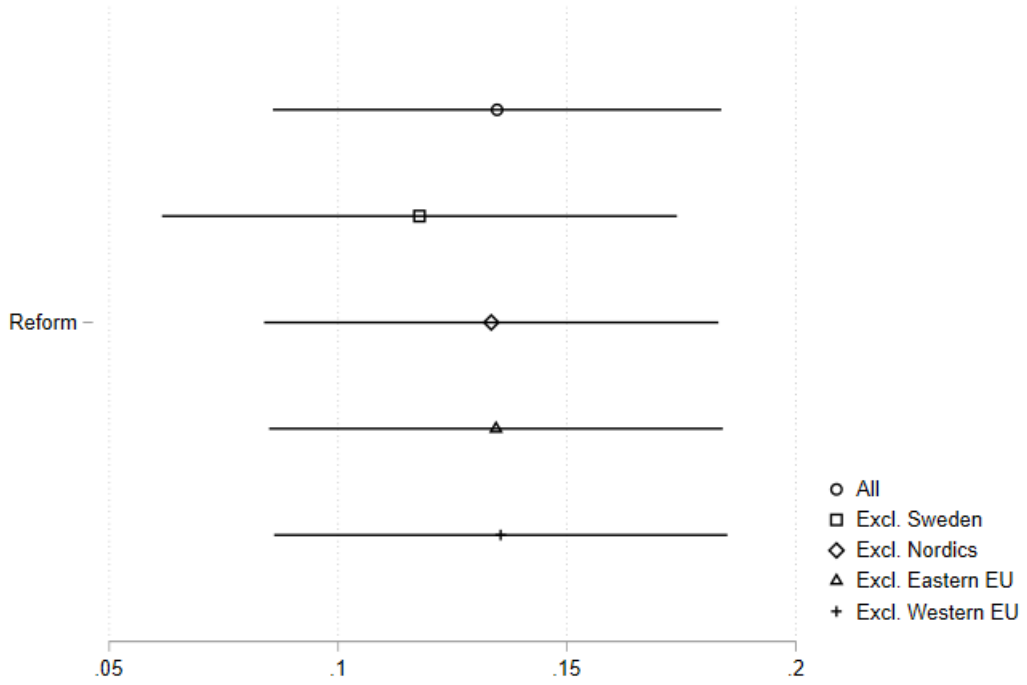
Notes: Using only European students as the comparison group, this figure reports the event-study estimates of the effect of the reform for the different graduating cohorts of international students, estimated with equation 2. Year 0 is 2013. Year t = -1 i.e., 2012, is the omitted group. The reform comes into effect in Year t = +1 i.e., in 2014. For example, the coefficient estimate for Year t = +3 represents the ATT estimate for the cohort of international students graduating in 2016. Ninety percent confidence intervals are displayed around each point estimate. Standard errors are clustered by country-group. The outcome is a binary variable indicating whether a doctoral student gets married or registers a partnership during their study. Panel (a) includes all students whereas Panel (b) includes students who are single in the year of first registration, i.e., neither married nor in a registered partnership. The sample comprises 25 country-groups/clusters, with 15 of them forming the Treatment group of international students.

Table C.4: Effect on marriage and partnership: Robustness

| | Without Controls | | Control X Year FEs | | Balanced Panel | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | 0.078*** | 0.059** | 0.039*** | 0.033 | 0.030*** | 0.024 |
| | (0.016) | (0.026) | (0.012) | (0.020) | (0.011) | (0.020) |
| Observations | 25031 | 6812 | 25029 | 6810 | 24925 | 6706 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.21 | 0.21 | 0.21 | 0.21 | 0.22 | 0.22 |
| Mean (EU/EEA) | 0.17 | 0.17 | 0.17 | 0.17 | 0.17 | 0.17 |
| Mean (Swedes) | 0.24 | 0.24 | 0.24 | 0.24 | 0.24 | 0.24 |
| Wild-bootstrap p-val | 0.016 | 0.047 | 0.067 | 0.139 | 0.099 | 0.270 |
| Pretest p-val | 0.106 | 0.540 | 0.764 | 0.711 | 0.600 | 0.629 |

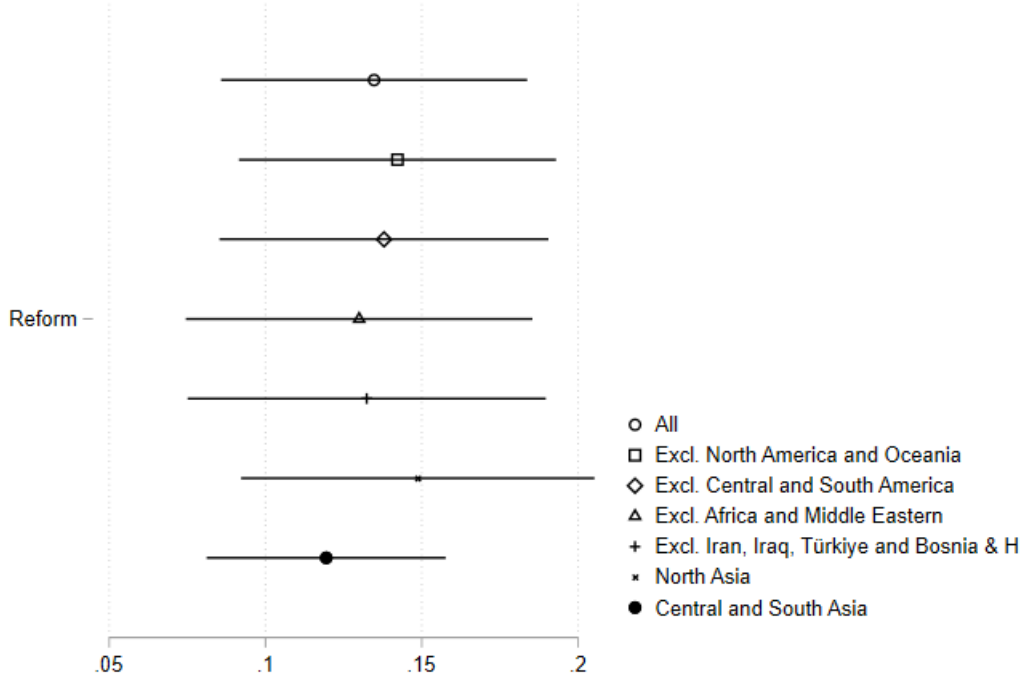
Notes: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Using a DID design with Swedish and European students as the comparison group, this table reports ATT estimates of the effect of the reform on international students, estimated with equation 1. The outcome is a binary variable indicating whether a doctoral student gets married or registers a partnership during their study. The odd-numbered columns, i.e., 1, 3, and 5, use both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. The even-numbered columns, i.e., 2, 4, and 6, only use the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout. Estimates are analogous to those reported in columns 1-2 in Panel A, Table 1, with the following modifications. Columns 1-2 exclude all controls. Columns 3-4 augment the benchmark model by adding interactions between year fixed effects and each of the controls. Finally, columns 5-6 restrict the sample to a balanced panel at the country-year level, thereby excluding 3 country-groups. Mean(-) represents the pre-reform mean for the corresponding group. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

Figure C.6: Residency in Sweden 3 years post-graduation: Effects after iteratively excluding one group of comparison countries in each estimation



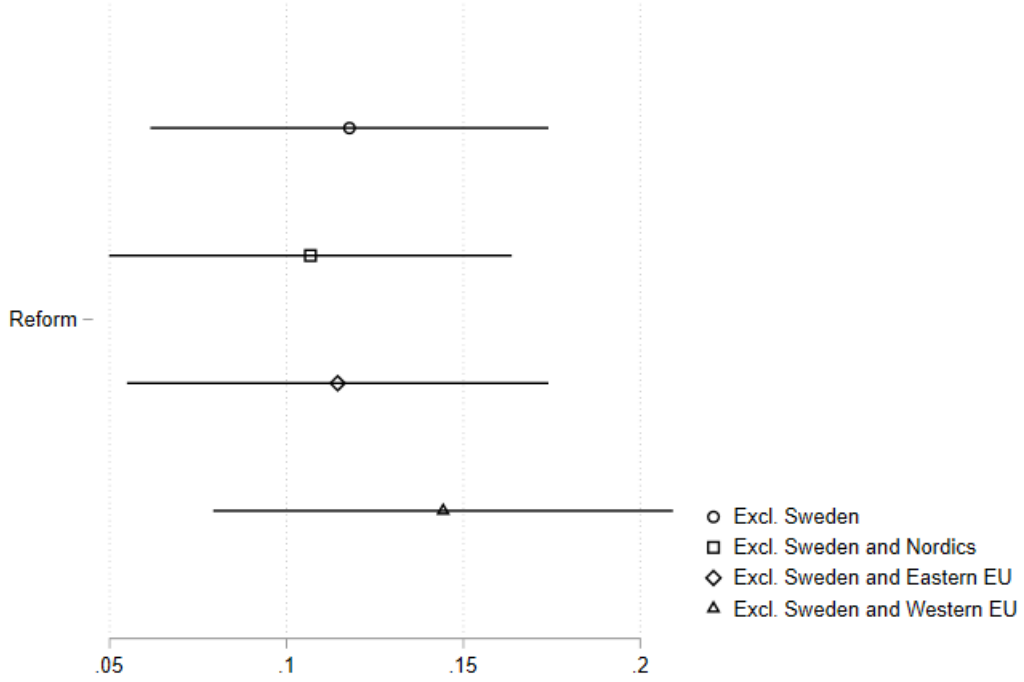

Notes: This figure shows point estimates along with 95 percent confidence interval of the effect of the reform on residency in Sweden 3 years post-graduation estimated with equation (1). For each estimate, one group of comparison countries has been excluded.

Figure C.7: Residency in Sweden 3 years post-graduation: Effects after iteratively excluding one group of treatment countries in each estimation



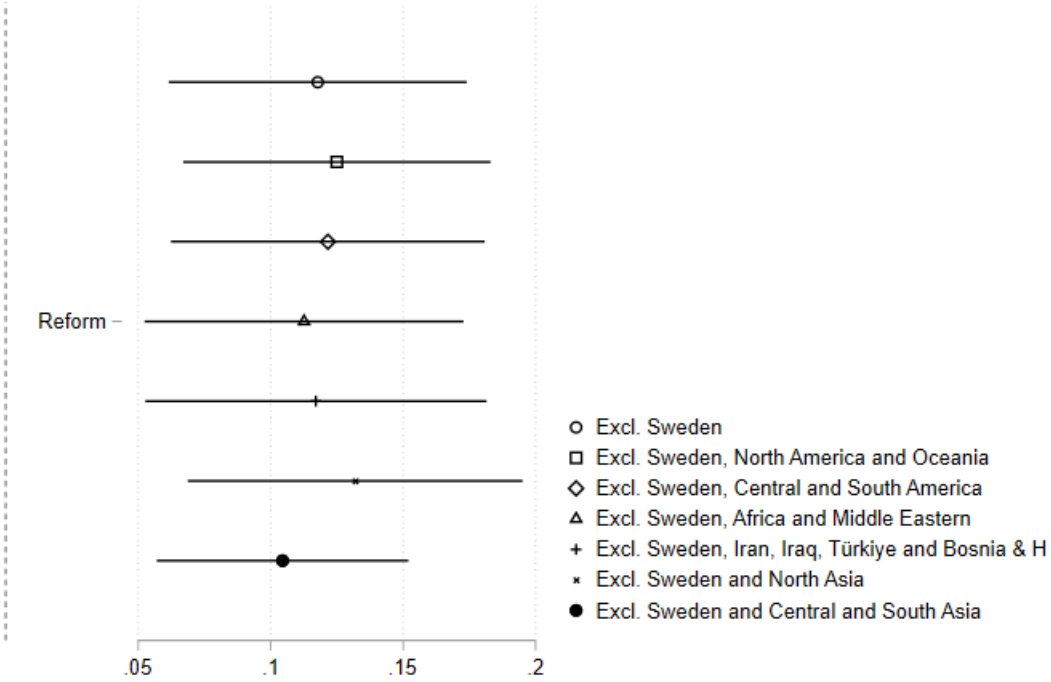

Notes: This figure shows the point estimates along with 95 percent confidence interval of the effect of the reform on residency in Sweden 3 years post-graduation estimated with equation (1). For each estimate, one group of treatment countries has been excluded.

Figure C.8: Residency in Sweden 3 years post-graduation: Effects after iteratively excluding one group of comparison countries in each estimation, with Sweden always excluded from the comparison group



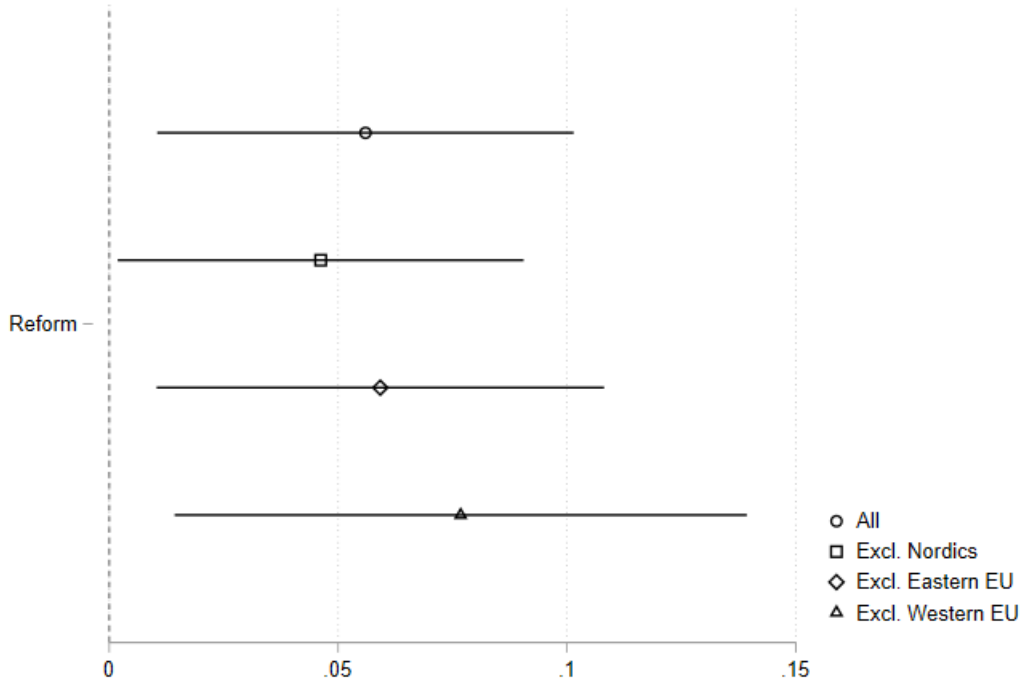

Notes: This figure shows point estimates along with 95 percent confidence interval of the effect of the reform on residency in Sweden 3 years post-graduation estimated with equation (1). For each estimate, one group of comparison countries has been excluded. Sweden is always excluded from the comparison group.

Figure C.9: Residency in Sweden 3 years post-graduation: Effects after iteratively excluding one group of treatment countries in each estimation, with Sweden always excluded from the comparison group



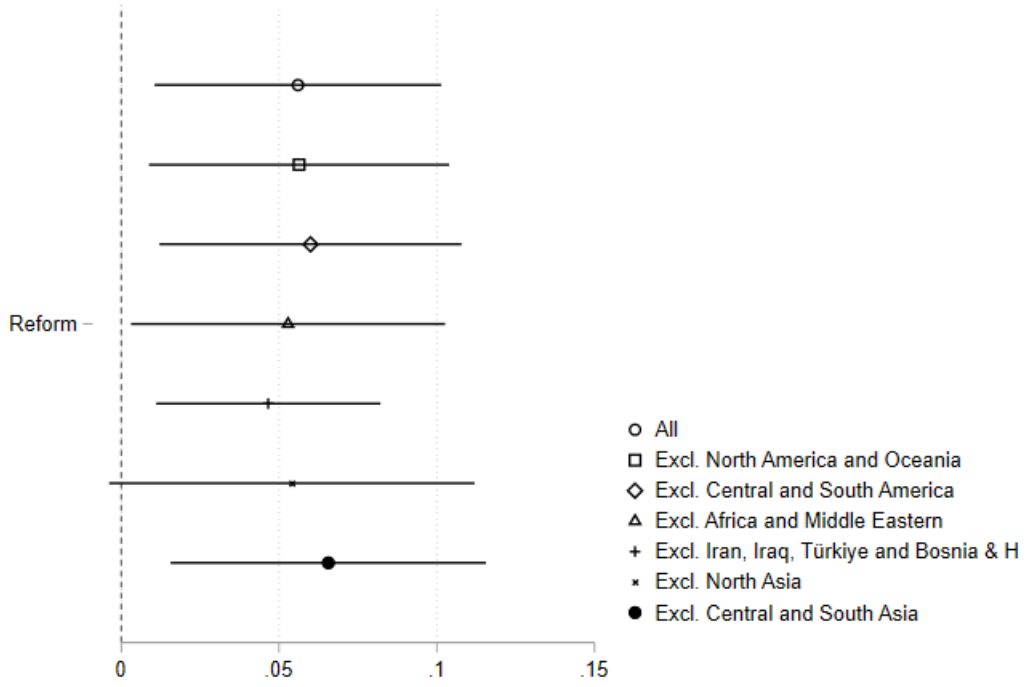

Notes: This figure shows the point estimates along with 95 percent confidence interval of the effect of the reform on residency in Sweden 3 years post-graduation estimated with equation (1). For each estimate, one group of treatment countries has been excluded. Sweden is always excluded from the comparison group.

Figure C.10: Effect on enrolment at Swedish for Immigrants (SFI): Effects after iteratively excluding one group of comparison countries



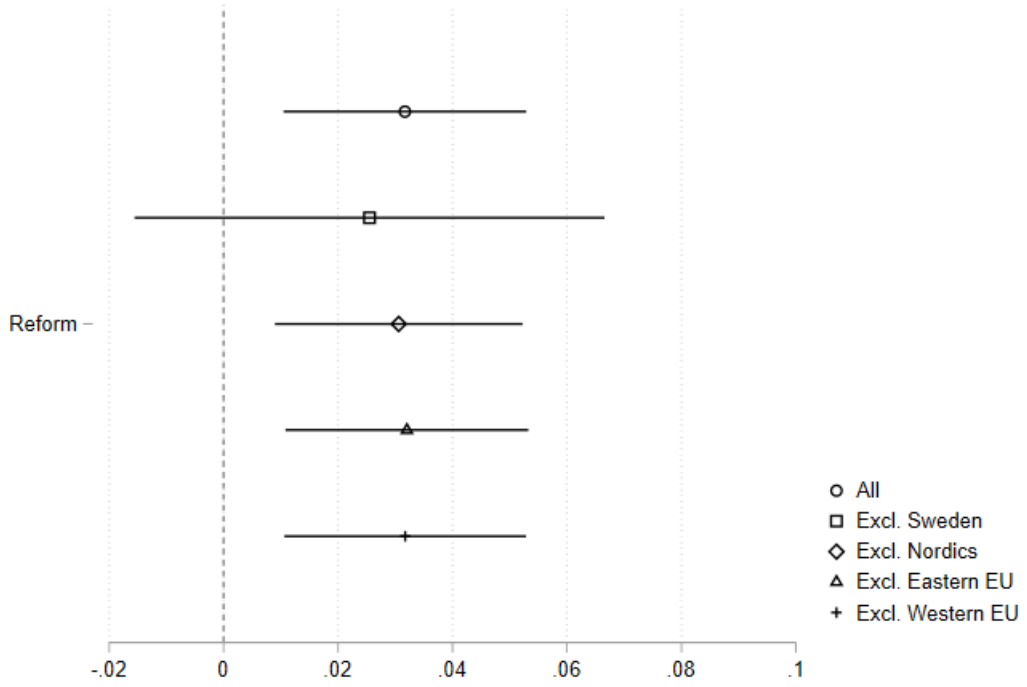

Notes: This figure shows the point estimates along with 95 percent confidence interval of the effect of the reform on SFI enrolment estimated with equation (1). For each estimate, one group of comparison countries has been excluded.

Figure C.11: Effect on enrolment at Swedish for Immigrants (SFI): Effects after iteratively excluding one group of treatment countries in each estimation



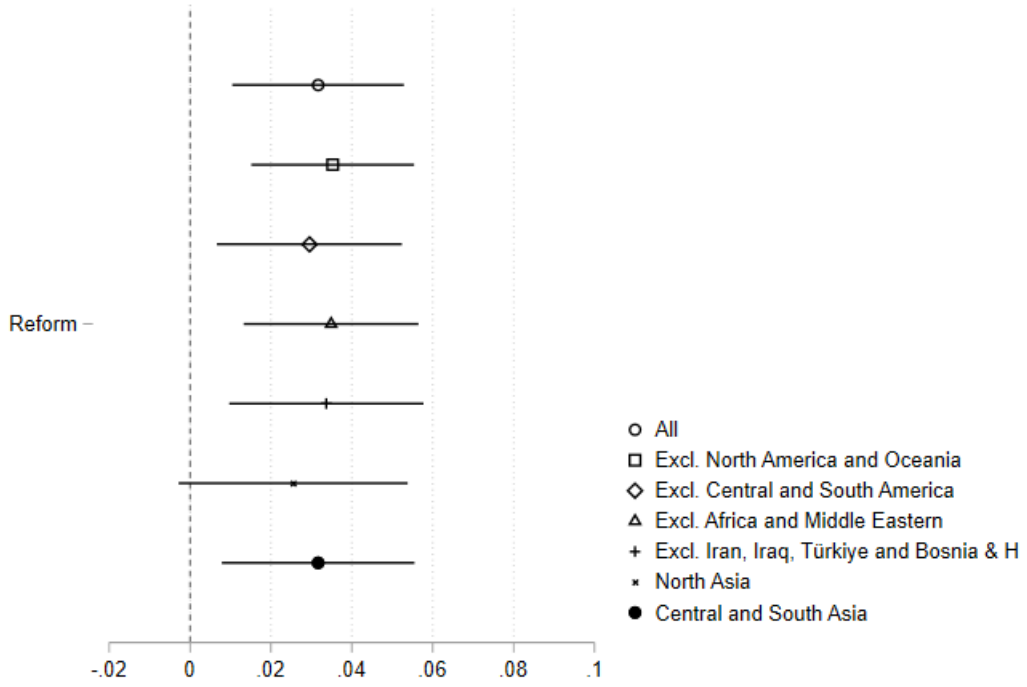

Notes: This figure shows the point estimates along with 95 percent confidence interval of the effect of the reform on SFI enrolment estimated with equation (1). For each estimate, one group of treatment countries has been excluded.

Figure C.12: Effect on marriage and partnership: Effects after iteratively excluding one group of comparison countries in each estimation




Notes: This figure shows the point estimates along with 95 percent confidence interval of the effect of the reform on marriage or partnership estimated with equation (1). For each estimate, one group of comparison countries has been excluded.

Figure C.13: Effect on marriage and partnership:: Effects after iteratively excluding one group of treatment countries in each estimation




Notes: This figure shows the point estimates along with 95 percent confidence interval of the effect of the reform on marriage or partnership estimated with equation (1). For each estimate, one group of treatment countries has been excluded.

Table C.5: Effect on enrolment at Swedish for Immigrants (SFI): Heterogeneity

| | Female | STEM | Married at registration | Child at registration |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Reform | 0.102*** | -0.004 | 0.058* | 0.064** |
| | (0.022) | (0.035) | (0.031) | (0.026) |
| Reform X Female | -0.112*** | | | |
| | (0.031) | | | |
| Reform X STEM | | 0.104** | | |
| | | (0.046) | | |
| Reform X Married at registration | | | 0.008 | |
| | | | (0.069) | |
| Reform X Child at registration | | | | -0.030 |
| | | | | (0.080) |
| Observations | 6810 | 6810 | 6810 | 6810 |
| Comparison group | EU/EEA | EU/EEA | EU/EEA | EU/EEA |
| Mean (Intl) | 0.27 | 0.27 | 0.27 | 0.27 |
| Mean (EU/EEA) | 0.16 | 0.16 | 0.16 | 0.16 |
| Mean (Intl)\|(X) | 0.31 | 0.26 | 0.29 | 0.29 |
| Mean (EU/EEA)\|(X) | 0.16 | 0.20 | 0.13 | 0.04 |
| Wild-bootstrap p-val (Reform) | 0.001 | 0.908 | 0.165 | 0.044 |
| Wild-bootstrap p-val (Reform X -) | 0.008 | 0.055 | 0.922 | 0.746 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Using a DID design with Swedish and European students as the comparison group, this table explores heterogeneity in the ATT estimates of the effect of the reform on international students, estimated with equation 4, and using the following dimensions: whether a student is – female (column 1); registered in a STEM program (column 3); married at registration (column 5); has child at registration (column 7). The outcome is a binary variable indicating whether a doctoral student enrolls at Swedish for Immigrants (SFI) during their study. The sample comprises 25 country-groups/clusters, with 15 of them forming the Treatment group of international students. Mean(-) represents the pre-reform mean for the corresponding group. Mean(-|(X)) represents the pre-reform mean for the corresponding group conditional on the indicator X switching on; e.g., Mean(Intl|(Female)) represents the pre-reform mean of the outcome for the international **female** students. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row.

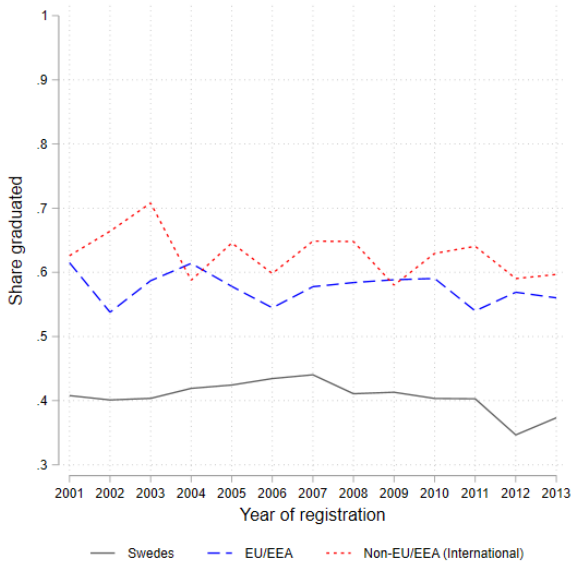
Table C.6: Effect on marriage and partnership: Heterogeneity

| | Female (All) | | Female (Single at registration) | | STEM (All) | | STEM (Single at registration) | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Reform | 0.040*** | 0.045* | 0.050*** | 0.059* | 0.031 | 0.017 | 0.047 | 0.038 |
| | (0.010) | (0.026) | (0.013) | (0.030) | (0.023) | (0.032) | (0.034) | (0.045) |
| Reform X Female | -0.021 | -0.048 | -0.031 | -0.065 | | | | |
| | (0.020) | (0.035) | (0.024) | (0.042) | | | | |
| Reform X STEM | | | | | 0.001 | 0.014 | -0.015 | -0.009 |
| | | | | | (0.028) | (0.042) | (0.046) | (0.058) |
| Observations | 25029 | 6810 | 18232 | 5179 | 25029 | 6810 | 18232 | 5179 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.21 | 0.21 | 0.35 | 0.35 | 0.21 | 0.21 | 0.35 | 0.35 |
| Mean (EU/EEA) | 0.17 | 0.17 | 0.20 | 0.20 | 0.17 | 0.17 | 0.20 | 0.20 |
| Mean (Swedes) | 0.24 | 0.24 | 0.32 | 0.32 | 0.24 | 0.24 | 0.32 | 0.32 |
| Mean (Intl)\|(X) | 0.21 | 0.21 | 0.33 | 0.33 | 0.23 | 0.23 | 0.37 | 0.37 |
| Mean (EU/EEA)\|(X) | 0.18 | 0.18 | 0.21 | 0.21 | 0.17 | 0.17 | 0.19 | 0.19 |
| Mean (Swedes)\|(X) | 0.23 | 0.23 | 0.34 | 0.34 | 0.27 | 0.27 | 0.31 | 0.31 |
| Wild-bootstrap p-val (Reform) | 0.022 | 0.106 | 0.017 | 0.072 | 0.347 | 0.628 | 0.322 | 0.434 |
| Wild-bootstrap p-val (Reform X -) | 0.329 | 0.251 | 0.160 | 0.202 | 0.971 | 0.739 | 0.757 | 0.884 |

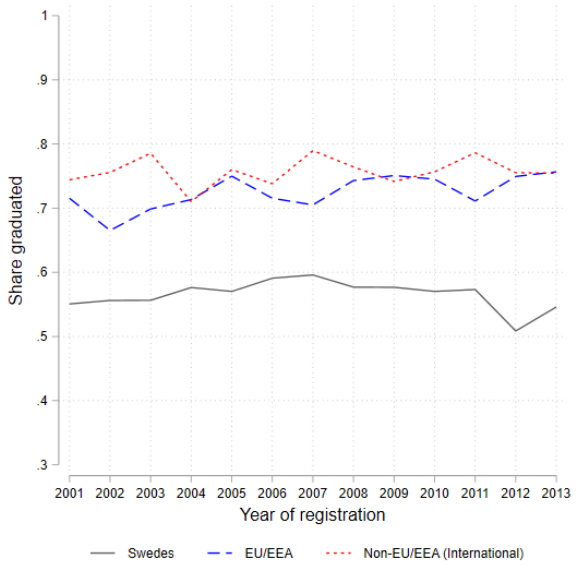
Notes: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Using a DID design with Swedish and European students as the comparison group, this table explores heterogeneity in the ATT estimates of the effect of the reform on international students, estimated with equation 4, and using the following dimensions: whether a student is female (columns 1-4); registered in a STEM program (columns 5-8)- The outcome is a binary variable indicating whether a doctoral student gets married or registers a partnership during their study. Estimates in columns 1-2 and 5-6 include all students whereas those reported in columns 3-4 and 7-8 include students who are single in the first year of registration, i.e., neither married nor in a registered partnership. The odd-numbered columns, i.e., 1, 3, 5 and 7 use both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. The even-numbered columns, i.e., 2, 4, 6 and 8, only use the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout. Mean(-) represents the pre-reform mean for the corresponding group. Mean(-|(X)) represents the pre-reform mean for the corresponding group conditional on the indicator X switching on; e.g., Mean(Intl|(Female)) represents the pre-reform mean of the outcome for the international **female** students. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row.

# D Tables and Graphs II

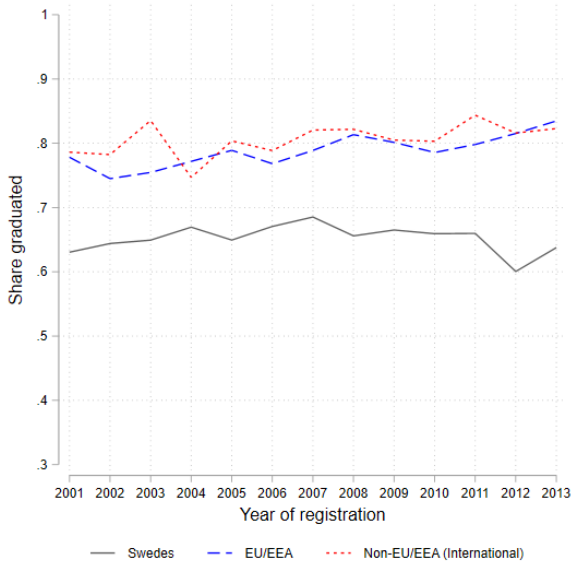
Figure D.1: Share graduating within specific years for registration cohorts 2001-2013 by region



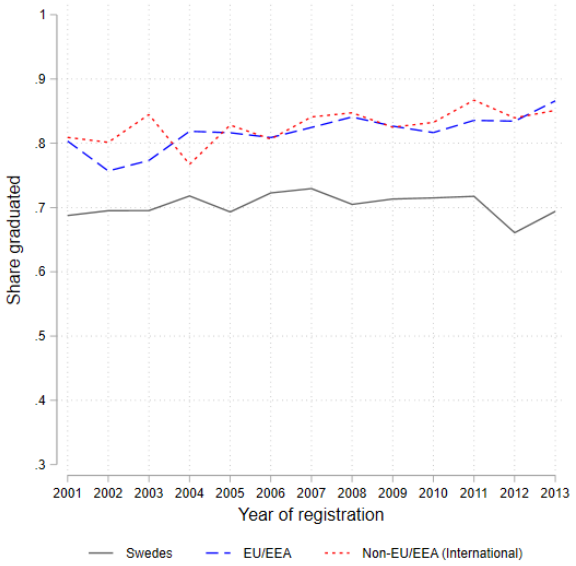

(a) Share graduating within 5 years




(b) Share graduating within 6 years




(c) Share graduating within 7 years




(d) Share graduating within 8 years

Notes: For the registration cohorts 2001-2013, this figure reports the trends in the share of doctoral students that graduated within 5, 6, 7 and 8 years, stratified by country/region of origin: Sweden, EU/EEA, and non-EU/EEA (International).

Table D.1: Composition of graduates

| | Female | | Has a child | | STEM | | Married/registered partnership | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Reform | 0.033* | 0.011 | -0.017 | -0.036* | 0.048 | 0.020 | -0.073*** | -0.053* |
| | (0.016) | (0.020) | (0.017) | (0.020) | (0.028) | (0.033) | (0.023) | (0.029) |
| Observations | 25031 | 6812 | 25031 | 6812 | 25031 | 6812 | 25031 | 6812 |
| Comparison group | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA | EU/EEA + SWE | EU/EEA |
| Mean (Intl) | 0.33 | 0.33 | 0.18 | 0.18 | 0.59 | 0.59 | 0.38 | 0.38 |
| Mean (EU/EEA) | 0.44 | 0.44 | 0.09 | 0.09 | 0.55 | 0.55 | 0.14 | 0.14 |
| Mean (Swedes) | 0.52 | 0.52 | 0.30 | 0.30 | 0.38 | 0.38 | 0.27 | 0.27 |
| Wild-bootstrap p-val | 0.143 | 0.595 | 0.387 | 0.100 | 0.179 | 0.570 | 0.048 | 0.143 |
| Pretest p-val | 0.379 | 0.431 | 0.878 | 0.478 | 0.231 | 0.292 | 0.339 | 0.616 |

Notes: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. The sample comprises all doctoral students who graduate between 2008-17. Using a DID design with Swedish and European students as the comparison group, this table reports ATT estimates of the effect of the reform on the composition of international students who graduate in or after 2013, estimated with equation 1, with age-quintiles as the only covariate. The outcome variables, using information recorded in the first year of registration, are: whether a student is – female (columns 1-2); has a child at registration (columns 3-4); registered in a STEM program (columns 5-6); married at registration (columns 7-8). The odd-numbered columns, i.e., 1, 3, 5, and 7, use both Swedes and European students as the comparison group, with the resulting sample comprising 27 country-groups/clusters. The even-numbered columns, i.e., 2, 4, 6, and 8, only use the Europeans, with the resulting sample comprising 25 clusters. The number of clusters forming the Treatment group of international students is 15 throughout. Mean(-) represents the pre-reform mean of the outcome variables for the corresponding group. Standard errors are clustered by country-group and reported in parentheses. Wild-bootstrapped p-values are reported in the second-last row. Pretest p-values correspond to the joint significance test for leads in the event-study equation 2.

## References

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