University as a Melting Pot: Long-term Effects of International Peers

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Abstract

The increasing globalization of higher education has sparked debates about the impact of international student presence on natives. This paper provides the first evidence on the impact of exposure to international students on the long-term outcomes of native students. I combine unique survey and administrative data from the Netherlands covering one million students across three decades and employ an across-cohort design. My identification strategy relies on idiosyncratic variation in the share of international students within university programs over time. I find that exposure to international students leads native students to *(i)* form more social ties with non-natives, *(ii)* hold more positive attitudes towards migration and learning about other cultures, and *(iii)* seek opportunities abroad. I find precisely estimated zero labor market effects for native students up to 25 years after university entry. These findings suggest that exposure to international students makes natives more internationally oriented and culturally open, without compromising their local economic success.

Keywords: Contact hypothesis, domestic students, foreign students, higher education, labor market, mobility, networks, peer effects, emigration

JEL Codes: F22, I21, I23, I26, J12, J15, J24, J61, O15

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*This version: February 2025. University of Amsterdam (s.avdeev@uva.nl). I am grateful to Hessel Oosterbeek and Bas van der Klaauw for their extensive feedback. I also thank Adam Booij, Michel Beine, Thomas Buser, Monique de Haan, Sander de Vries, Bas Machielsen, Richard Murphy, Morgan Raux, Giuseppe Sorrenti, Dmitrii Shchetinin, Umut Özek, Alexander Yarkin, and many participants at LEER Conference in Leuven, QED Jamboree in Amsterdam, Tinbergen Institute Jamboree in Amsterdam, Workshop on Immigration, Health, and Wellbeing in Verona, KVS New Paper Sessions in the Hague, COMPIE Conference in Amsterdam, Workshop on Economics of Education in Valle Nevado, EEA-ESEM Annual Meeting in Rotterdam, WEEP in Trondheim, MACIMIDE Conference in Maastricht, IZA/ECONtribute Workshop on the Economics of Education in Bonn, CEMIR Junior Economist Workshop on Migration Research in Munich, Workshop on Immigration, Integration, and Attitudes in Mannheim, CPB Research Seminar in the Hague, Annual Conference on Immigration in OECD Countries in Paris for their helpful comments. I gratefully acknowledge Jeroen Kleingeld for his support with the data from the Nationale Studenten Enquête provided by the Landelijk Centrum Studiekeuze. In this paper, I make use of data from the LISS panel (Longitudinal Internet Studies for the Social Sciences) managed by the non-profit research institute Centerdata (Tilburg University, the Netherlands). The non-public micro data used in this paper are available via remote access to the Microdata services of Statistics Netherlands (project agreement 7930). Word count: 7,489.

1 Introduction

Over the past two decades, the number of students studying abroad has tripled globally (UNESCO, 2024). Universities attract international students to increase revenue, expand social networks, and offer students opportunities to work with diverse peers in an international setting. However, politicians have raised concerns about the displacement of domestic students, negative labor market impacts, and the costs of subsidizing the education of foreign students. These concerns, along with broader migration issues, have led to regulations to limit international student inflows in Australia, Canada, Denmark, the Netherlands, and the UK.[1] At the same time, declining college-aged populations in the US have fueled discussions on increasing international enrollments to sustain university funding (Jack, 2024). While these policies target international students, they may have unintended consequences for native students, which remain largely unknown.

This paper examines the impact of exposure to international students during the first year of a bachelor's program on the long-term outcomes of native students in the Netherlands. I employ an across-cohort design, following the approach introduced by Hoxby (2000). Specifically, I exploit idiosyncratic variation in the share of international students within university programs over time. My approach allows for student selection *across* programs, as it does not compare students from different programs but rather compares students *within* the same program *across* different cohorts.

The identification strategy relies on the assumption that students do not select into specific cohorts *within* a program based on the presence of international students. This assumption is plausible for two reasons. First, prospective students are generally unaware of the international student composition in their program until they begin their studies. Second, even if students could estimate the approximate composition, it is unlikely that they would alter their enrollment timing to avoid or seek out international peers. To support this assumption, I provide evi-

[1]The Australian and Canadian government have imposed annual limits on student visas (Ross, 2024; Bhardwa, 2024), the Dutch government is currently reducing the number of English-taught bachelor's programs (Dixon, 2024), and the UK government has restricted international bachelor's students from bringing family members (Jack, 2023b). Denmark imposed caps on international enrollments in 2021 but reversed them in 2023, recognizing the downsides of restricting foreign student inflows (Jack, 2023a).

dence that the share of international students is uncorrelated with the likelihood of native students taking a gap year, "native flight" (e.g., dropping out, switching programs, or transferring universities), and a rich set of pre-determined characteristics at the student, sibling, and parent levels, as well as peer composition and indicators of program quality.

The Netherlands offers an ideal case study for examining the impact of international students on native students. First, its higher education system consists of publicly funded universities that admit all Dutch and EEA+ students without grade-based selection.[2] This institutional setup strengthens my identification strategy, as year-to-year variation in native and international student numbers is unpredictable for both students and universities. Moreover, the absence of grade-based selection eliminates self-sorting among Dutch students and prevents mechanical crowding-out effects. This ensures that changes in outcomes primarily reflect exposure to international students rather than selection bias. Second, the Netherlands provides unique data opportunities by linking administrative records on labor market outcomes and social connections with survey data on natives' attitudes towards internationalization and migration. This allows for a comprehensive examination of both objective outcomes (revealed preferences) and subjective attitudes (stated preferences).

The results show that exposure to international students shifts both the revealed and stated preferences of native students towards greater acceptance of migrants and openness to migration. While enrolled at university, a 10 percentage point increase in the share of international students leads to a 0.12 standard deviation (SD) increase in positive attitudes towards the internationalization of education and a 0.07 SD increase in satisfaction with opportunities to learn about other cultures. Fifteen years after entering university, a 10 percentage point increase leads to *i)* a 5.9% and 4.2% increase in the probability of cohabiting with and marrying a non-native; *ii)* a 0.30 SD increase in favorable views on social integration of foreigners, a 0.17 SD decrease in unfavorable views on foreigners moving into a neighborhood, and a 0.28 SD increase in support for European unification; and *iii)* a 4% increase in the probability of emigration. The results withstand a battery of robustness checks, including adjustments for multiple hy-

[2]EEA+ includes students from the European Economic Area (the EU, Iceland, Liechtenstein, and Norway) plus Switzerland.

pothesis testing, time trends, program size, individual and peer characteristics, different sample definitions, definitions of international students, and a placebo test.

Having established positive effects of international exposure on social ties and openness to migration, I then examine whether the presence of international students affects native students' labor market outcomes. Policymakers often voice concerns that international students might strain educational resources and reduce educational quality, potentially disadvantaging natives in the labor market. However, I find that exposure to international students shows precisely estimated zero effects on employment, income, entrepreneurship, and the share of non-native co-workers up to 25 years after university entry.

Social integration effects are strongest in collaborative and diverse environments and absent in more competitive ones. For example, STEM fields and programs with fewer female students, which tend to be more competitive, show no significant effects on social ties between native and international students. In contrast, non-STEM fields and programs with a more balanced gender composition, foster stronger social integration. Additionally, social ties between native and international students are observed only in larger programs since they offer broader networks and more opportunities for interaction. Exposure to EEA+ peers enhances social integration, likely due to shared cultural and institutional backgrounds. Interactions with non-EEA+ students, who face greater cultural and linguistic differences, do not significantly enhance social integration but increase satisfaction with opportunities to learn about other cultures.

This paper expands existing research by providing new evidence in two areas: *(i)* the role of universities in fostering social cohesion and *(ii)* the long-term impact of international students on their native peers.

First, this paper contributes to the literature on the contact hypothesis, which suggests that contact with minority groups can reduce prejudice.[3] As universities become more diverse, they offer valuable opportunities for reducing stereotypes.[4]

[3]The hypothesis posits that cross-cultural interactions can effectively break down stereotypes and prejudices between groups when a set of conditions, such as common goals, intergroup cooperation, support by social and institutional authorities, and equal status, are satisfied (Williams, 1947; Allport, 1954).

[4]This study also contributes to the literature on universities as (international) marriage markets, an area often overlooked with regard to immigrants. Many studies focus on universities

Previous studies in higher education have explored the impact of being assigned a roommate of a different race on inter-racial prejudice and connections (Boisjoly et al. (2006); Marmaros and Sacerdote (2006); Camargo et al. (2010); Carrell et al. (2019) in the US, and Corno et al. (2022) in South Africa).[5] This paper is the first to test the contact hypothesis on international students. Moreover, using group composition as a treatment rather than roommate assignments tests a more practical and scalable policy approach. My estimated effects are of comparable magnitude to those found in the contact hypothesis literature, demonstrating that even less intensive interactions, such as diverse group exposure, can yield outcomes similar to those of more intensive interventions.

Second, I contribute to the growing literature on the spillover effects of international students in higher education.[6] This study is the first to estimate the impact of exposure to international students on the long-term outcomes of native

as marriage markets for natives but neglect the role of immigrants, who constitute a significant share of the student population in many countries (Goldin, 1992; Wiswall and Zafar, 2021; Artmann et al., 2021; Kaufmann et al., 2021; Ge et al., 2022; Kirkebøen et al., 2022). Furthermore, this research adds to the literature on non-pecuniary returns to education by demonstrating how exposure to diverse peer groups in universities can foster cross-cultural interactions and social cohesion (Goldin and Katz, 2008; Oreopoulos and Salvanes, 2011; Chiappori et al., 2009, 2017, 2018).

[5]Other studies focus on interventions promoting social cooperation in primary and secondary education (Rao (2019) in India, Merlino et al. (2019) in the US, Alan et al. (2021) and Boucher et al. (2024) in Turkey, and Anderberg et al. (2024) in Germany). Beyond educational settings, the contact hypothesis has been examined in diverse contexts, such as military environments in Norway (Finseraas and Kotsadam, 2017), the UK (Schindler and Westcott, 2021), and Spain (Bagues and Roth, 2023), local communities in Indonesia (Bazzi et al., 2019), soccer teams in Iraq (Mousa, 2020), cricket leagues in India (Lowe, 2021), and counties the US (Bursztyn et al., 2024).

[6]This research also adds to the broader literature on peer effects in higher education, including studies on racial diversity (Arcidiacono and Vigdor, 2010; Hinrichs, 2011; Dills, 2017; Lau, 2022) and the impact of roommate exposure (Sacerdote, 2001; Zimmerman, 2003; Stinebrickner and Stinebrickner, 2006; Foster, 2006; Lyle, 2007, 2009; Carrell and Fullerton, 2009; Griffith and Rask, 2014; Jain and Kapoor, 2015). Related work also examines how international students affect native student enrollment (Borjas, 2004; Shen, 2016; Machin and Murphy, 2017; Shih, 2017; Chen, 2021) and the resource implications of international students (Zhu, 2023). Additional research focuses on immigrant effects in primary and secondary education, including Gould et al. (2009) in Israel, Jensen and Rasmussen (2011) in Denmark, Friesen and Krauth (2011) in Canada, Bifulco et al. (2011); Hunt (2016); Billings and Hoekstra (2023); Hung (2023); Figlio et al. (2024) in the US, Geay et al. (2013) in the UK, Ohinata and Van Ours (2013); Maestri (2017) in the Netherlands, Schneeweis (2015); McHenry (2015) in Austria, Tonello (2016); Ballatore et al. (2018) in Italy, and Holmlund et al. (2023) in Sweden. Furthermore, extensive literature explores the effects of immigrant workers on native labor market outcomes (see Dustmann et al. (2016) for a review).

students. Previous studies have focused on short-term outcomes. For instance, Anelli et al. (2023) and Rakesh (2023) examine the effects of exposure to foreign classmates in introductory first-year math courses at two US universities. Anelli et al. (2023) find that exposure to foreign students decreases the likelihood of native students graduating with a STEM degree and has no significant impact on expected earnings, based on country average earnings for their major. Similarly, Rakesh (2023) finds that exposure to foreign students lowers the graduation rate of native students. In the UK, Costas-Fernández et al. (2023) find no effects of exposure on graduation, employment, and earnings six months post-graduation, based on survey data. Likewise, Chevalier et al. (2020) find no impact of exposure to non-English speakers on the academic performance or emigration of native students six months after graduation in the UK. To summarize prior findings alongside my own results, I estimate a meta-analytic pooled estimate, which shows that exposure to international students has no statistically and economically significant effects on the short- and long-term educational and labor market outcomes of natives.

My findings have significant policy implications for current debates on internationalization. Governments in countries like Australia, Canada, the Netherlands, the UK, and the US are implementing policies to regulate the number of international students. Although aimed at managing international student inflows, these policies may unintentionally affect native students. Based on the results of this paper, reducing international student enrollment could weaken social integration, cultural openness, and international opportunities for native students. This is especially concerning, as it affects students during their impressionable years, a critical period when attitudes and preferences are most adaptable. Overall, international students contribute not only through economic benefits like tuition fees but also by fostering social cohesion and promoting a globally oriented perspective among native students.

The remainder of the paper is structured as follows. Section 2 provides an overview of the Dutch higher education system. Section 3 describes the administrative and survey data, along with the identification strategy. Section 4 presents the main results, a heterogeneity analysis, robustness checks, and a meta-analysis. Section 5 concludes.

2 Institutional background

Under the Dutch law, students who complete the academic track in secondary education are eligible for admission to any of the 13 research universities without grade-based selection criteria.[7] This policy ensures that the influx of international students does not displace Dutch students or affect their access to university education. Under the EU law, all EU nationals must meet the same criteria as Dutch nationals.[8] This creates an ideal setting for my identification strategy, as year-to-year variation in native and non-native student numbers is unpredictable for both incoming students and university programs.

There are low barriers for international students to enroll, because many programs use English as the primary language of instruction. Moreover, the tuition fees in the Netherlands are not high compared to those in other popular English-speaking destinations, such as Australia, the US, the UK. Faculty salaries are determined by collective labor union agreements, ensuring a consistent level of quality across institutions. This commitment to accessibility and quality has resulted in all universities consistently ranking among the top institutions globally (THE, 2023). As a result of these factors, the share of international students in the Netherlands has seen a remarkable increase, rising from a modest 3% in the 1990s to over 20% by 2019 (see Figure 1).

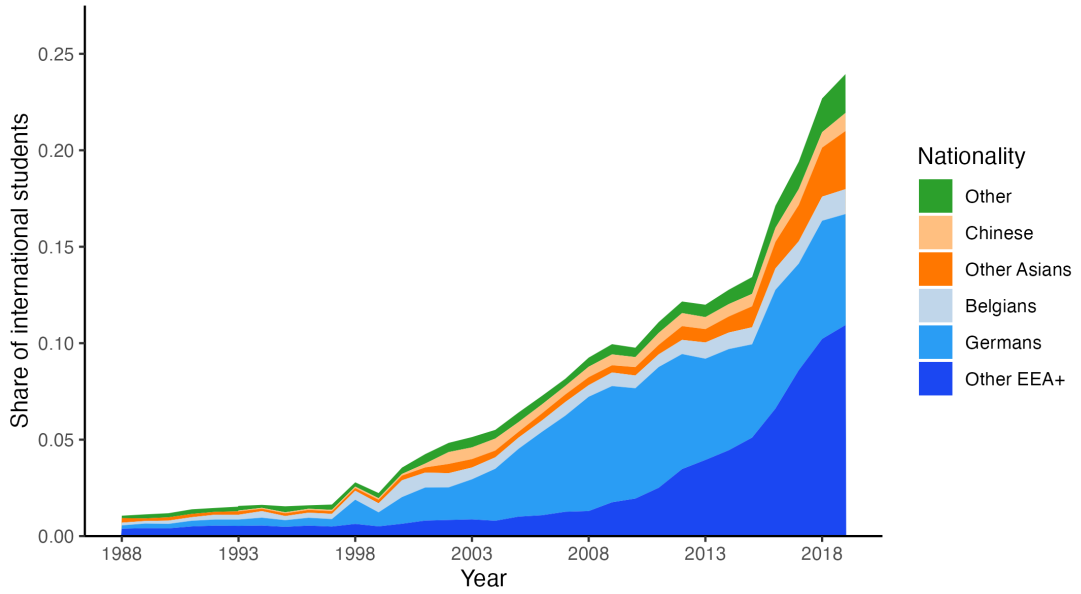
At Dutch universities, students enroll in specific programs from the first year. The first year consists of compulsory courses, ensuring that students in the same program share a common curriculum and interact regularly in peer groups. In later years, students may select elective courses, allowing for more specialization in their field of study. This paper focuses on the first year, where shared compulsory courses provide structured opportunities for peer interaction between native and international students.

Figure 1 shows that around 75% of all international first-year bachelor's students originated from other European countries, with German and Belgian students forming the largest groups. Asian students constituted 16% of the first-year international student body, with the largest groups being Chinese and Indian.

[7] There are a few exceptions in programs like medicine, dentistry, and veterinary (see Leuven et al. (2013) and Ketel et al. (2016) for more details).

[8] These rules also apply to nationals of Iceland, Liechtenstein, Norway, and Switzerland.

Figure 1: The share of international students enrolled in Dutch research universities in 1988–2019



Notes: EEA+ refers to students from the European Economic Area (the EU, Iceland, Liechtenstein, and Norway) plus Switzerland.
Source: Calculated by the author using data from Statistics Netherlands.

The share of international students differs substantially across fields of study. In 2019, Economics, Social Sciences, and Humanities had the highest shares of international students, around one-third. STEM fields followed with around 20%, while Law and Health had the lowest percentages at around 10%. The lower percentages in Law and Health are primarily due to language barriers, as most programs in these fields are taught in Dutch.

Universities have several incentives to attract international students. In the Netherlands, government funding is tied to Dutch and EEA+ student enrollment, making international recruitment financially advantageous. Moreover, non-EEA+ students pay higher tuition fees than their Dutch or EEA+ counterparts, providing additional financial boost. In 2019, tuition fees for Dutch and EEA+ national students were around €2,000 per year, while fees for non-EEA+ nationals were approximately €12,000. Additionally, university rankings frequently consider the share of international students and faculty, enhancing the appeal of a diverse student body. Lastly, there is a growing recognition that the presence

of international students can positively impact the outcomes of native students, potentially through peer effects.

3 Research design

In this section, I discuss the data sources and my identification strategy.

3.1 Data

Administrative data. I utilize nationwide administrative data provided by Statistics Netherlands (CBS), which cover higher education enrollment from 1988 to 2023, secondary education enrollment from 2006 to 2019, tax registry from 2003 to 2022, firm registry from 2006 to 2023, welfare registry from 2003 to 2022, cohabitation registry from 1994 to 2023, marriage registry from 1994 to 2023, migration registry from 1994 to 2023, and death from 1994 to 2023 (CBS, 2025). All registries are linked using unique identifiers. The education registry includes details on secondary school, exam grades, university, program, study duration, dropout, and graduation. The tax registry provides information on employment, entrepreneurship, and earnings. The firm registry contains all firms and their workers. The welfare registry provides information on unemployment and social benefits. The cohabitation and marriage registries include data on flatmate and spouse pairs. The migration registry contains information on the time and country of immigration and emigration. Additionally, I have access to demographic data such as sex, age, nationality, country of birth, neighborhood, and residency. I also link information on parents and siblings for balancing checks.

Survey data. I merge two survey datasets: National Student Survey (NSS) and Longitudinal Internet Studies for the Social Sciences (LISS). The NSS panel includes questions on attitudes towards internationalization (NSS, 2025). The survey has been running since 2010 and contains aggregated responses from native first-year students by program, university, and year of enrollment. These survey data is linked to administrative data using identifiers based on program, university, and year of enrollment.

The LISS panel includes questions on attitudes towards migration. The LISS panel is a nationally representative annual household survey that has been running since 2007 (LISS, 2025). The survey contains individual responses to a wide range of topics related to economic, social, and health outcomes. It consists of around 5,000 households, comprising approximately 7,500 individuals, drawn from a random sample of Dutch households. Since the LISS panel is representative of the full Dutch population, only a subset of survey respondents can be matched to the sample of university students. Approximately 15% of the Dutch population holds a university degree, which corresponds to the share of LISS respondents that could be merged with the administrative data of university students. The merging is done using the same unique identifier that links all registries. The average age of respondents is 30.4.

Sample construction. I focus on full-time students enrolled in bachelor's programs between 1988 and 2019. From the international student sample, I exclude exchange students (0.2% of the sample) due to their short period of study. From the native student sample, I exclude first-generation immigrants (5.5%) due to their uncertain status (whether they should be considered as native or international) and students older than 30 at the time of enrollment (1.5%) as they do not represent the usual undergraduate populations. The results remain robust to the inclusion of first-generation migrants and students older than 30, as reported in the Appendix.

I define a native student as someone who holds Dutch nationality. To define an international student, I apply two criteria: not having Dutch nationality at the time of enrollment and not having completed secondary education in the Netherlands. This definition aims to capture individuals who lack prior exposure to the Dutch education system. I discuss the consequences of using alternative definitions of international students in the Robustness section.

The analysis focuses on the first enrollment of students in bachelor's programs. This approach ensures accurate assignment of the treatment variable, defined as the share of international students at the time of a student's first enrollment. For instance, if a student enrolls in a program, subsequently drops out, and later enrolls in a different program, they are included in the study only once.

3.2 Identification strategy

The aim of this paper is to estimate the impact of exposure to international students on the long-term outcomes of native students. I exploit idiosyncratic variation in the share of international students within a bachelor's program over time, using the following model:

$$Y_{ipc} = \beta D_{pc} + \alpha_p + \tau_c + \varepsilon_{ipc} \tag{1}$$

where Y_{ipc} measures an outcome of interest of a native student; $D_{pc} = \frac{Int_{pc}}{N_{pc}-1}$ is the share of international students; α_p are program fixed effects; τ_c are cohort fixed effects.[9] I cluster standard errors at the program level as the treatment is assigned at this level (Abadie et al., 2023). Additionally, I report q-values using the method proposed by Anderson (2008) to account for multiple hypothesis testing.

The inclusion of program fixed effects accounts for time-invariant characteristics, such as the location of the program, which could influence student selection across programs. Cohort fixed effects control for time-varying factors that affect all students, such as changes in tuition fees. I check the robustness of the main findings against alternative model specifications that include linear and quadratic field specific time trends, program size, and individual and peer characteristics. The results are invariant to the inclusion of these controls and are reported in the Appendix.

3.3 Identifying variation

By leveraging the institutional setting of Dutch and EU law and incorporating program and cohort fixed effects, the primary source of variation in Dutch students' exposure to international peers arises from differences in cohort entry, which are largely driven by birth years. This variation arises from fluctuations in both the number of native students and the number of international students within programs.

First, the number of native students varies due to natural fluctuations in Dutch birth cohorts, leading to random changes in university enrollment sizes.

[9] Appendix 5 provides a detailed description of the variable construction.

Second, the number of international students fluctuates for two reasons. Admission offices at Dutch universities have no control over the number of both Dutch and EEA+ students, resulting in some randomness influenced by population changes in other European countries. Additionally, various push factors from EEA+ and non-EEA+ countries further contribute to these fluctuations. In summary, the variation in the number of both native and international students entering Dutch universities is largely driven by natural cohort fluctuations, external demographic changes, and push factors from other countries, creating a degree of randomness in student inflow.

As discussed in Angrist (2014), there needs to be sufficient variation in the cohort-to-cohort changes of the share of international students. Figures A1 and A2 in the Appendix illustrate the variation in the share of international students, both before and after accounting for the fixed effects. Even after controlling for program and cohort fixed effects, there is still enough variation to precisely estimate the parameter of interest. Specifically, one standard deviation of the share of international students is 12.6 percentage points, while the residualized share has a standard deviation of 5.9 percentage points. To put this in perspective, an increase of one standard deviation in the residualized share translates to adding approximately 8 international students to an average program of 137 students. This year-to-year variation is largely due to Dutch universities having no control over the number of incoming Dutch and EEA+ nationals due to the Dutch and EU laws, leading to idiosyncratic fluctuations in the share of international students.

3.4 Identification challenges

In this section, I discuss three challenges to my identification strategy: selection, reflection, and common shocks.

Selection. My identification strategy does not rule out student selection *across* programs. For example, if students know from previous years that there are many international students in Economics at Maastricht University due to its proximity to the border with Germany and Belgium, and fewer international students in Economics at Erasmus University, and they decide where to enroll based on this

knowledge, this is not a violation of my identification strategy. This is because I do not compare students across different programs (Economics at Maastricht University vs. Economics at Erasmus University). Instead, I compare students *within* the same program *across* different cohorts (Economics at Maastricht University in 2010 vs. Economics at Maastricht University in 2011).

The identification strategy relies on the assumption that students do not select into cohorts *within* a program based on the presence of international students. For example, if students decide to enroll in Economics at Maastricht University in 2011 and not in 2010 because of the different share of international students, then this violates my identification strategy. This is unlikely due to two reasons. First, prospective applicants do not observe the composition of students in the year they enroll until their first day in the classroom. Only when students enroll in a program and start their first day can they observe this composition. That is why I measure the share of international students and all other peer outcomes at the start of the academic year. For example, if some students enroll after the start of the academic year, I do not include them in my sample, and they are not included to measure peer characteristics.

Second, even if students could anticipate the approximate composition, they would have to react to that information to violate my identification strategy. In other words, if students delay (enroll next year instead of this year) or accelerate (enroll this year instead of next year) their enrollment into a specific program based on the share of international students in this program, then it would violate this assumption. There are two cases of how students can do that. Imagine that students can observe the approximate composition of their peers and decide to either delay or accelerate their enrollment. To delay enrollment, students who would not have otherwise taken a gap year choose to take one. Conversely, to accelerate enrollment, students who would have otherwise taken a gap year decide to skip it.

To examine whether native students react to the share of international students, I look at whether the probability of taking a gap year changes, as well as a battery of other observable characteristics. I test for selection on observables using balancing tests, regressing native students' exposure to international students

with observable characteristics, using the following model:

$$X_{ipc} = \beta D_{pc} + \alpha_p + \tau_c + \varepsilon_{ipc} \tag{2}$$

where X_{ipc} includes student, sibling, and parent characteristics.

Table 1 reports estimates with and without program and cohort fixed effects. Without fixed effects, many estimates are statistically significant. With fixed effects, only one of 32 estimates remains significant at the 1% level. I do not reject the null hypothesis that the coefficients are jointly equal to zero. In line with the scenario discussed above, students do not delay or accelerate their enrollment by taking more or fewer gap years in response to the presence of international students. It is worth noting that the lack of statistical significance is not due to large standard errors but rather due to the small size of both the coefficients and the standard errors.

Further, I regress the characteristics of peers of native students in a program on the share of international students in that program, using the following model:

$$\bar{X}_{pc} = \beta D_{pc} + \alpha_p + \tau_c + \varepsilon_{pc} \tag{3}$$

where $\bar{X}_{pc}$ includes student, sibling, and parent characteristics collapsed to the program level. Table 2 shows that, with fixed effects, only one of 32 estimates is significant at the 10% level. Again, I do not reject the null hypothesis that the coefficients are jointly equal to zero.

Table 3 examines the correlation between the share of international students and indicators of program quality. As proxies for program quality, I use the program size and the outcomes of graduates from the same year the share of international students is measured. I regress the share of international students to whom native students are exposed in their enrollment year on the outcomes of graduates from the same program, university, and year. For instance, I regress the share of international students among first-year students in Economics at Maastricht University in 2010 on the outcomes of students who graduated from Economics at Maastricht University in 2010, such as their employment 5 years post-graduation. The idea is that the outcomes of graduates reflect the quality of the program.

Table 1: Balancing table of characteristics of first-year students, their siblings, and parents

	No fixed effects		**With fixed effects**		
	Coefficient	s.e.	Coefficient	s.e.	Mean
Panel A: First-year students					
Female	−0.006	(0.009)	0.000	(0.002)	0.50
Age at enrollment	−0.109***	(0.022)	−0.018	(0.036)	19.78
Gap year	−0.011**	(0.005)	−0.001	(0.007)	0.33
Second generation immigrant	0.006**	(0.003)	−0.002	(0.001)	0.14
Family size	−0.028***	(0.003)	−0.008***	(0.002)	2.53
Secondary school exam grade	0.011	(0.013)	0.005	(0.005)	0.14
Has a vocational degree	−0.005	(0.003)	−0.004	(0.005)	0.09
Panel B: Siblings					
Female	0.001*	(0.001)	0.001	(0.001)	0.49
Age at enrollment of the sibling	−0.133***	(0.028)	−0.020	(0.040)	18.54
Has a university degree	0.002*	(0.001)	−0.003	(0.002)	0.08
Panel C: Parents					
Father's age at enrollment of the child	0.281***	(0.046)	0.025	(0.024)	50.13
Mother's age at enrollment of the child	0.274***	(0.043)	0.028	(0.024)	47.84
Father is married	−0.004***	(0.001)	0.000	(0.001)	0.90
Mother is married	−0.003**	(0.001)	−0.001	(0.001)	0.87
Father has a university degree	0.031***	(0.004)	0.002	(0.002)	0.18
Mother has a university degree	0.030***	(0.004)	0.002	(0.002)	0.17
Father is employed	0.001	(0.001)	0.001	(0.001)	0.94
Mother is employed	0.004***	(0.001)	−0.001	(0.001)	0.83
Father is an entrepreneur	0.004***	(0.001)	0.001	(0.001)	0.12
Mother is an entrepreneur	0.005***	(0.001)	−0.001	(0.001)	0.11
Father receives unemployment benefits	0.000*	(0.000)	0.000	(0.000)	0.01
Mother receives unemployment benefits	0.001***	(0.000)	0.000	(0.000)	0.01
Father receives social benefits	0.000	(0.001)	−0.001	(0.001)	0.07
Mother receives social benefits	0.000	(0.001)	0.000	(0.001)	0.07
Father's income percentile	0.002	(0.002)	0.000	(0.001)	0.67
Mother's income percentile	0.001	(0.001)	−0.001	(0.001)	0.57
At least one parent has a university degree	0.042***	(0.006)	0.004	(0.003)	0.26
At least one parent is employed	0.001	(0.001)	0.000	(0.001)	0.97
At least one parent is an entrepreneur	0.008***	(0.002)	0.001	(0.001)	0.18
At least one parent receives unemployment benefits	0.001***	(0.000)	0.000	(0.001)	0.02
At least one parent receives social benefits	0.000	(0.001)	0.000	(0.001)	0.12
Average parents' income percentile	0.001	(0.001)	−0.001	(0.001)	0.62
F-test and its p-value	4.46 (< 0.001)		1.26 (0.15)		
N	1,037,345		1,037,345		

Notes: Regressions with fixed effects include program and cohort fixed effects. For siblings, all outcomes are measured one year before their first sibling enrolls in a university. For parents, all outcomes are measured one year before their first child enrolls in a university. N represents the number of first-year students. Standard errors (s.e.), which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 2: Balancing table of peer characteristics of first-year students

	No fixed effects		**With fixed effects**		
	Coefficient	s.e.	Coefficient	s.e.	Mean
Panel A: Peers					
Share of females	0.007	(0.006)	0.002	(0.003)	0.52
Average age at enrollment	−0.081***	(0.024)	0.045	(0.036)	20.04
Share with a gap year	−0.007	(0.005)	0.010	(0.006)	0.38
Share of second generation immigrants	0.007***	(0.003)	0.002	(0.003)	0.14
Average family size	−0.038***	(0.007)	−0.009	(0.006)	2.54
Average secondary school exam grade	0.010	(0.009)	0.011	(0.007)	0.21
Share with a vocational degree	−0.007***	(0.002)	0.001	(0.004)	0.09
Panel B: Siblings of peers					
Share of female siblings	0.001	(0.001)	0.000	(0.002)	0.49
Average age at enrollment of the sibling	−0.111***	(0.024)	0.042	(0.046)	18.77
Share of siblings with a university degree	0.001	(0.001)	−0.001	(0.002)	0.08
Panel C: Parents of peers					
Average father's age at enrollment of the child	0.214***	(0.031)	0.061	(0.040)	50.32
Average mother's age at enrollment of the child	0.198***	(0.024)	0.048	(0.034)	48.00
Share of married fathers	−0.004***	(0.001)	0.000	(0.002)	0.90
Share of married mothers	−0.003*	(0.001)	−0.002	(0.002)	0.87
Share of fathers with a university degree	0.026***	(0.002)	0.001	(0.002)	0.16
Share of mothers with a university degree	0.025***	(0.003)	−0.001	(0.002)	0.15
Share of employed fathers	0.000	(0.001)	0.000	(0.002)	0.93
Share of employed mothers	0.005***	(0.001)	0.000	(0.002)	0.82
Share of fathers who are entrepreneurs	0.003***	(0.001)	0.004	(0.003)	0.11
Share of mothers who are entrepreneurs	0.005***	(0.001)	−0.001	(0.002)	0.10
Share of fathers who receive unemployment benefits	0.000*	(0.000)	0.000	(0.001)	0.01
Share of mothers who receive unemployment benefits	0.000*	(0.000)	0.001	(0.001)	0.01
Share of fathers who receive social benefits	0.001	(0.001)	0.000	(0.002)	0.07
Share of mothers who receive social benefits	0.001	(0.001)	0.003*	(0.002)	0.07
Average father's income percentile	0.001	(0.001)	0.000	(0.002)	0.65
Average mother's income percentile	0.003***	(0.001)	0.000	(0.002)	0.56
Share of parents with a university degree	0.035***	(0.003)	0.000	(0.002)	0.23
Share of employed parents	0.000	(0.001)	−0.001	(0.001)	0.96
Share of parents who are entrepreneurs	0.007***	(0.001)	0.003	(0.003)	0.17
Share of parents who receive unemployment benefits	0.001**	(0.000)	0.002	(0.001)	0.02
Share of parents who receive social benefits	0.002*	(0.001)	0.002	(0.002)	0.12
Average parents' income percentile	0.002*	(0.001)	0.000	(0.001)	0.60
F-test and its p-value	4.34 (< 0.001)		1.07 (0.37)		
N	11,449		11,449		

Notes: Regressions with fixed effects include program and cohort fixed effects. For siblings, all outcomes are measured one year before their first sibling enrolls in a university. For parents, all outcomes are measured one year before their first child enrolls in a university. N represents the number of programs in which first-year students are enrolled. Standard errors (s.e.), which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 3: Balancing table of indicators of program quality

	No fixed effects		**With fixed effects**		
	Coefficient	s.e.	Coefficient	s.e.	Mean
Panel A: Educational outcomes					
Program size	17.754***	(4.712)	5.624	(4.037)	136.75
Share of ever dropped out	0.006**	(0.003)	0.002	(0.004)	0.25
Average time to graduate	−0.075***	(0.018)	0.017	(0.020)	4.65
Share who completed a master's degree	0.044***	(0.004)	−0.001	(0.004)	0.41
Panel B: Social outcomes					
Share who cohabited with a non-native 5 year post-graduation	0.008***	(0.001)	0.000	(0.002)	0.08
Share who cohabited with a non-native 10 years post-graduation	−0.003***	(0.001)	−0.001	(0.002)	0.08
Share who married to a non-native 5 years post-graduation	−0.001***	(0.000)	0.000	(0.001)	0.02
Share who married to a non-native 10 years post-graduation	−0.002***	(0.001)	−0.001	(0.001)	0.03
Emigrated 5 years post-graduation	0.007***	(0.001)	0.000	(0.001)	0.05
Emigrated 10 years post-graduation	0.012***	(0.002)	0.002	(0.003)	0.07
Panel C: Labor market outcomes					
Share of employed 5 year post-graduation	−0.001	(0.001)	0.000	(0.002)	0.95
Share of employed 10 years post-graduation	−0.001	(0.001)	0.000	(0.002)	0.95
Average income percentile 5 year post-graduation	0.003	(0.002)	−0.001	(0.002)	0.68
Average income percentile 10 years post-graduation	0.001	(0.003)	−0.002	(0.002)	0.72
Share of entrepreneurs 5 year post-graduation	−0.004***	(0.001)	−0.002	(0.002)	0.05
Share of entrepreneurs 10 years post-graduation	−0.005***	(0.002)	−0.002	(0.003)	0.08
Share who have income from abroad 5 year post-graduation	0.001***	(0.000)	0.001	(0.001)	0.01
Share who have income from abroad 10 years post-graduation	0.000	(0.000)	0.001	(0.001)	0.01
% of non-native co-workers 5 year post-graduation	0.005***	(0.001)	−0.001	(0.002)	0.07
% of non-native co-workers 10 years post-graduation	0.004***	(0.001)	0.001	(0.002)	0.06
F-test and its p-value	1.82 (0.01)		0.53 (0.95)		
N	10,329		10,329		

Notes: Regressions with fixed effects include program and cohort fixed effects. All outcomes, except for program size, are measured among graduates of the programs in the same year the share of international students among first-year students is measured. A non-native is defined as a person without Dutch nationality. N represents the number of programs from which students have graduated. Standard errors (s.e.), which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

With fixed effects, none of the 20 indicators of program quality is statistically significant at conventional levels, and the coefficients are precisely estimated. These findings suggest that, conditional on program and cohort fixed effects, the share of international students does not correlate with changes in the overall quality of the programs. This lack of significant differences supports the assumption that any observed effects of international students on native peers' outcomes are not driven by systematic variations in program quality.

In total, out of 84 estimates, one would expect to have 1% of coefficients sig-

nificant at the 1% level, 5% at the 5% level, and 10% at the 10% level. The share of significant estimates in my sample is at or below the respective expectations: they are 1.2%, 1.2%, and 2.4%, respectively.

Another concern is that native students may leave the program after enrolling if there is a higher or lower share of international students than they would like to study with. I examine whether exposure to a higher share of international students in the first year affects the probability of dropping out during the first year, switching programs while staying at the same university, or transferring to another university but remaining in the same program. For instance, if natives enrolled in Economics at Maastricht University prefer fewer international students, they might drop out by switching to Business at Maastricht University or transfer to Economics at Erasmus University. Table 4 shows no significant effects on dropouts, program switches, and university transfers, which suggests that there is no "native flight" from programs with a higher share of international students.

Table 4: The effect of exposure to international students on "native flight"

	Dropped out during 1st year	Switched program at the same university	Switched university to the same program
International share in 10%-points	-0.003	0.002	0.000
	(0.006)	(0.001)	(0.000)
Mean	0.264	0.045	0.016
N	1,037,345	1,037,345	1,037,345

Notes: All regressions include program and cohort fixed effects. Switched program at the same university refers to whether a student changed program but remained at the same university during the first year. Switched university to the same program refers to whether a student transferred to a different university but stayed in the same program during the first year. Standard errors, which are clustered at the program level, are reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Reflection and common shocks. Apart from the selection problem, identifying peer effects is complicated by two more issues: the reflection and common shocks problems, as outlined by Manski (1993). The reflection problem arises when international and native students potentially influence each other's outcomes simultaneously, while common shocks occur when factors such as teacher quality affect the entire group. These issues typically emerge when contempora-

neous outcomes are used as treatment variables. However, in my setting, these problems are mitigated by using the share of international students as the measure of peer influence. This variable is fixed at the start of the program and is unaffected by other variables or common shocks. Besides, by excluding international students from the estimation sample, I ensure that the estimated impact reflects only the influence on native students.[10]

4 Results

In this section, I present the main findings on the impact of exposure to international students on social ties, migration decisions, attitudes towards internationalization and migration, and labor market outcomes. Next, I conduct a heterogeneity analysis based on sex, field of study, share of females, program size, and the origin of international students. Finally, I conclude with robustness checks and a meta-analysis.

4.1 Main results

Effect of exposure on social ties. I estimate the impact of interacting with international students on the likelihood of living together and forming partnerships with non-native individuals 15 years after enrolling in a program. All coefficients represent a 10 percentage point increase in the share of international students in the first year of a bachelor's program. Column (1) of Table 5 shows that a 10 percentage point increase in the share of international students increases the likelihood that native students will live with non-natives by 0.6 percentage points 15 years post-enrollment. This change corresponds to a 5.9% increase relative to the sample mean of 10.2%.

[10]Recent advancements in the econometrics of peer effects have highlighted another challenge: exclusion bias (Guryan et al., 2009; Caeyers and Fafchamps, 2020). This problem arises from the inherent limitation that individuals cannot serve as their own peers, leading to potential biases. When individuals are part of a group, their own characteristics are not included in the group's average calculation, which can create a negative correlation between an individual's characteristic and the group's average characteristic. In my study, exclusion bias is not an issue because I focus only on native students. The group's average characteristic of interest, i.e. the share of international students, remains the same for all native students. When a native student is placed in a peer group, the share of international students in the rest of the group is identical for every native student. Thus, exclusion bias does not lead to negative bias.

Table 5: The effect of exposure to international students on social ties and migration decisions of native students 15 years post-enrollment

	(1) Cohabited with a non-native	(2) Married to a non-native	(3) Emigrated
International share	0.006***	0.001**	0.003***
in 10%-points	(0.001)	(0.001)	(0.001)
Sharpened q-value	[0.007]	[0.043]	[0.024]
Mean	0.102	0.035	0.065
Effect size	5.9%	4.2%	4.0%
Cohorts	1988-2008	1988-2008	1988-2008
N	605,365	587,674	617,092

Notes: All regressions include program and cohort fixed effects. A non-native is defined as a person without Dutch nationality. Effect size represents the ratio of the estimated coefficient to the sample mean of the outcome. Sharpened q-values are adjusted False Discovery Rate p-values, calculated using the method proposed by Anderson (2008), to account for multiple hypothesis testing. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Native students are also more likely to marry a non-native after exposure to more international students at university. A 10 percentage point increase in the share of international students leads to a 0.1 percentage points increase in marriages between natives and non-natives (see Column (2) of Table 5). This translates to a 4.2% increase of the sample mean of 3.5%. These findings illustrate how interactions with international students can foster closer social connections between natives and non-natives, contributing to a more integrated and culturally diverse society.

One explanation for the increased likelihood of cohabiting and marrying non-natives due to exposure to international students is the larger pool of non-natives. More international students in a program increase the chance of cohabitation and marriage between native and international students from the same program. Column (1) of Table A1 in the Appendix shows no effect of exposure on the likelihood of cohabiting or marrying someone from the same program. Columns (2) and (3) reveal that native students are more likely to cohabit and marry non-natives outside their program and university. These findings suggest the increased likelihood of such relationships reflects deliberate choices rather than mechanical effects.

Effect of exposure on migration decisions. Studying alongside international students can broaden native students' perspectives and increase their interest in other countries. For example, it can affect the propensity of native students to seek experiences abroad. Column (3) of Table 5 shows that a 10 percentage point increase in the share of international students increases the probability of emigration by 0.3 percentage points, which is 4% increase of the sample mean of 6.5%. This suggests that universities make native students more internationally oriented.

Effect of exposure on attitudes towards internationalization and migration. Exposure to international students might also shift native students' preferences, making them more open to other cultures. To examine this, I analyze how international student exposure influences natives' attitudes towards internationalization and migration using two survey datasets.

I find that exposure to international students positively affects students' perceptions of internationalization (see Panel A of Table 6).[11] A 10 percentage point increase in the share of international students increases satisfaction with internationalization by 0.053 points (0.12 SD) and satisfaction with encouragement to learn about other cultures by 0.038 points (0.07 SD).

Exposure affects views of natives not only during university but also postgraduation (see Panel B of Table 6).[12] A 10 percentage point increase in the share of international students in a university program leads to a 0.237 point increase in agreement that people of foreign origin who legally reside in the Netherlands should be entitled to the same social security as Dutch citizens (0.30 SD). Furthermore, it leads to a 0.140 point decrease (-0.17 SD) in agreement that it

[11] In Panel A of Table A2, I check for selection bias in programs that participate in the NSS panel and their responses to internationalization-related questions. I find no correlation between the share of international students and the probability of a program being in the NSS panel or answering any of the internationalization-related questions. Panels B and C of Table A2 show the main findings using linear regression with weights and ordered probit regression. The results are consistent with those from the linear regression in Table 6.

[12] In Panel A of Table A3, I check for selection bias of native students into the LISS panel and their responses to immigration-related questions. I find no correlation between the share of international students and the probability of being in the LISS panel or answering any of the immigration-related questions. Panels B and C of Table A3 present the main findings using linear regression with weights and ordered probit regression. The results are similar to those from linear regression without weights in Table 6.

Table 6: The effect of exposure to international students on attitudes towards internationalization and migration of native students

	Coefficient	s.e.	Sharpened q-value	Mean	N
Panel A: Attitudes towards internationalization					
1. How satisfied are you with the level of internationalization? (1 - very dissatisfied, 5 - very satisfied)	0.053***	(0.019)	[0.021]	3.524	123,038
2. How satisfied are you with the level of encouragement to learn about other cultures? (1 - very dissatisfied, 5 - very satisfied)	0.038**	(0.019)	[0.043]	3.197	123,038
Panel B: Attitudes towards migration					
1. Do you agree that people of foreign origin who legally reside in the Netherlands should be entitled to the same social security as Dutch citizens? (1 - fully disagree, 5 - fully agree)	0.237**	(0.095)	[0.026]	3.790	1,184
2. Do you agree that it does not help a neighborhood if many people of foreign origin or descent move in? (1 - fully disagree, 5 - fully agree)	-0.140**	(0.062)	[0.035]	3.260	1,184
3. Where would you place yourself on a scale from 1 to 5, where 1 means that European unification has already gone too far and 5 means that it should go further?	0.238***	(0.068)	[0.007]	2.147	1,170

Notes: All regressions include program and cohort fixed effects. A coefficient represents a 10 percentage point increase in the share of international students. Sharpened q-values are adjusted False Discovery Rate p-values, calculated using the method proposed by Anderson (2008), to account for multiple hypothesis testing. In Panel A, outcomes are based on National Student Survey data. In Panel B, outcomes are based on Longitudinal Internet Studies for the Social Sciences Survey data. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

does not help a neighborhood if many people of foreign origin or descent move in. Additionally, exposure to international students shifts opinions on European unification, with a 0.238 point increase (0.28 SD) in the belief that European unification should go further.

The results indicate that exposure to international students leads to deliberate and lasting social ties between native and non-native students. Native students are more likely to form relationships with non-natives outside their academic environment, reflecting a preference shift. This exposure also influences migration decisions and shapes natives' views on internationalization and migration, highlighting the important role universities play in encouraging long-term social integration and openness.

Effect of exposure on labor market outcomes. After establishing the positive effects of exposure on the social ties and openness to migration among native students, I examine whether the presence of international students impacts the labor market outcomes of natives. Policymakers often express concerns that international students might strain educational resources and negatively affect the quality of education, potentially disadvantaging native students' labor market outcomes. Table 7 presents the findings for employment, income percentile, entrepreneurship, and the share of non-native co-workers. Across all outcomes, the estimates are precisely estimated zero. The precision of the estimated coefficients allows me to rule out any effects more negative than -0.2 percentage points on employment, income, and entrepreneurship, as well as effects more negative than -0.1 percentage point on the share of non-native co-workers. Overall, the findings point to a lack of long-term impact on the labor market.

Table 7: The effect of exposure to international students on labor market outcomes of native students 15 years post-enrollment

	(1) Employed	(2) Income percentile	(3) Entrepreneur	(4) % of non-native co-workers
International share	-0.001	0.001	0.001	0.001
in 10%-points	(0.001)	(0.002)	(0.001)	(0.001)
Sharpened q-value	[0.108]	[0.236]	[0.256]	[0.144]
Mean	0.942	0.718	0.077	0.056
Effect size	-0.1%	0.1%	0.8%	1.3%
Cohorts	1988-2007	1988-2007	1988-2007	1991-2008
N	545,635	545,635	545,635	427,685

Notes: All regressions include program and cohort fixed effects. A non-native is defined as a person without Dutch nationality. Effect size represents the ratio of the estimated coefficient to the sample mean of the outcome. Sharpened q-values are adjusted False Discovery Rate p-values, calculated using the method proposed by Anderson (2008), to account for multiple hypothesis testing. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

This absence of negative effects on labor market outcomes is particularly significant in light of ongoing policy debates that often focus on the potentially detrimental impacts of international student presence on the educational and labor market outcomes of native students. The precision of these estimates refutes concerns about statistically and economically significant negative effects, suggest-

ing that exposure to international peers does not harm natives' local economic success.

4.2 Heterogeneity

In this section, I examine how international student exposure impacts groups with different social dynamics, competitive pressures, network diversity, and cultural similarities. These factors can influence both social integration and professional outcomes, with some environments encouraging stronger cross-cultural connections and others limiting them. By analyzing how exposure effects vary across these underlying features, I aim to identify which contexts enhance or weaken the benefits of international student exposure.

Sex. Prior research suggests that men and women may experience social and professional networks differently (Beaman et al., 2018; Zeltzer, 2020; Ductor et al., 2023; Cullen and Perez-Truglia, 2023). This could influence integration outcomes between native and international students by sex. To investigate this, I interact a female dummy with the share of international students. Panel A of Table A4 indicates no statistically significant differences by sex, suggesting that male and female natives experience similar impacts from exposure to international students.

Field of study. Social engagement patterns can vary by field of study. STEM fields, with their competitive nature and intensive coursework, may provide fewer opportunities for social integration than non-STEM fields. Panel B of Table A4 shows that non-STEM graduates benefit from social integration with international students, while STEM graduates show no significant increase in social ties. This suggests that the competitive environment of STEM fields may limit opportunities for cross-cultural interactions.

Share of females. Research suggests that men tend to be more competitive than women (Niederle and Vesterlund, 2007; Buser et al., 2014). In highly male-dominated settings, competition may take precedence over collaboration, potentially limiting opportunities for social integration. To test this, I examine whether the effect of exposure to international students varies by the gender composition

of a program. I use a 20% female threshold because programs with such a low share of women are heavily male-dominated, where competitive norms are likely to be most pronounced. Panel C of Table A4 shows that in programs with fewer than 20% female students, exposure to international students has no significant effect on social ties. This finding suggests that the competitive nature in male-dominated environments may suppress the social integration benefits of international exposure.

Program size. Larger programs may promote greater social integration, as they offer broader networks and more opportunities for students to engage with peers. Panel D of Table A4 confirms this, showing that natives in larger programs experience stronger effects on social ties and migration decisions. Exposure in larger programs also increases the likelihood of employment in firms with more non-native co-workers, suggesting that these broader environments foster integration and may encourage interest in global career paths.

EEA+ and non-EEA+ students. Cultural and institutional similarities between EEA+ students and Dutch natives may facilitate social connections, as they often share cultural and institutional backgrounds. This familiarity likely lowers barriers to interaction, resulting in stronger integration outcomes. Columns (7) and (8) of Tables A8 and A9 show that exposure to EEA+ students significantly boosts social integration. In contrast, exposure to non-EEA+ students, who may face greater cultural and linguistic differences, shows no significant integration effects. However, exposure to non-EEA+ peers increases satisfaction with opportunities to learn about other cultures, showing that these interactions promote cultural exchange. These results suggest that shared cultural contexts with EEA+ students enhance social ties, while interactions with non-EEA+ students contribute to broader cultural awareness, highlighting the value of diversity in the learning environment.

4.3 Robustness

The main analysis uses a model with program and cohort fixed effects. Here, I assess the robustness of the findings using alternative model specifications, sample

definitions, treatment definitions, and a placebo test.

Correction for multiple hypothesis testing. Given the number of hypotheses tested, I apply the correction method from Anderson (2008) to control for multiple hypothesis testing. This correction reduces the risk of false positives, and the results remain statistically significant at the 5% level (see sharpened q-values in Tables 5, 6, and 7).

Outcome measurement at 5 and 25 years. In the baseline analysis, I measure outcomes 15 years post-enrollment. To test for effects over shorter and longer horizons, I estimate outcomes at 5 and 25 years post-enrollment in Table A5. Results confirm the baseline findings: no significant labor market effects, increased social ties between natives and non-natives, and higher emigration rates. Notably, there is a significant effect of exposure on the share of non-native coworkers 5 years post-enrollment, indicating that native students initially enter more international workplaces, though this effect diminishes over time.

Including time trends. To control for potential confounding from time-related factors, I include linear and quadratic field specific time trends. Columns (2) and (3) in Tables A6 and A7 show that the results remain robust to this adjustment.

Controlling for program size. To ensure that the results are not influenced by the size of the programs, I include program size as a control variable in the model. Column (4) in Tables A6 and A7 shows that the results remain robust.

Controlling for individual characteristics. To account for possible confounding from individual characteristics correlated with the share of international students, I include individual controls. Column (5) in Tables A6 and A7 shows that the results stay robust.

Controlling for peer characteristics. Although individual characteristics do not impact estimates, the share of international students could be correlated

with peer characteristics. In Column (6) in Tables A6 and A7, I control for these characteristics, and the results remain unchanged.

Including first-generation migrants. The main analysis excludes first-generation migrants due to ambiguity in categorizing them as native or international. Column (8) in Tables A6 and A7 shows that including them does not change the results.

Including older first-year students. To focus on typical undergraduate populations, I exclude students over 30 at enrollment in the main analysis. Column (9) in Tables A6 and A7 shows that including them does not affect the results.

Sensitivity to alternative definitions of international students. Angrist (2014) and Feld and Zölitz (2017) highlight the potential for measurement error to bias estimates of peer effects. The availability of comprehensive administrative data in my research allows for an examination of how different definitions of international students influence the sensitivity of my findings. Various definitions have been used in the literature to identify international students, including:

1. Having a different nationality (Borjas, 2004, 2006; Shen, 2016; Chevalier et al., 2020; Chen, 2021; Anelli et al., 2023; Beine et al., 2023; Rakesh, 2023; Zhu, 2023).
2. Being born in a different country (Machin and Murphy, 2017; Shih, 2017).
3. Residing in a different country before university enrollment (Costas-Fernández et al., 2023).
4. Completing secondary education in a different country.

These variations in definitions pose two main challenges. Firstly, they complicate the comparison of findings across studies, as it becomes unclear whether observed differences in peer effects are due to genuine variations or merely result from differing definitions. Secondly, definitions that inaccurately characterize international students can lead to biased estimates of peer effects.

To estimate the extent of potential biases, I propose a definition of a "true" international student as someone who is both a non-national and did not complete secondary education in the host country. This definition aims to capture individuals who lack prior exposure to the host country's education system.

Tables A8 and A9 in the Appendix shows that the results remain largely consistent across alternative definitions. On average, these definitions yield similar outcomes, suggesting that previous studies using different definitions produce unbiased results, and that differences in estimated peer effects across settings are due to genuine variations.

Placebo exercise. In this section, I present the results of the placebo exercise. Instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university.[13] For instance, a student enrolled in Economics at the University of Amsterdam in 2010 is assigned the share of international students from Economics at Erasmus University in 2010. This process is repeated 10,000 times, each time estimating the effect of this placebo treatment.

The resulting normal-looking distributions of simulated counterfactual effects confirm that the true estimates are located in the tails of the distributions, further validating the robustness of the findings. Figures A3 - A14 in the Appendix display plots with estimates from 10,000 simulated datasets, the mean estimated coefficients of the randomly assigned shares, and the actual estimated coefficients. The mean estimated effect of the randomly assigned shares is centered around zero for all outcomes. In contrast, the actual estimated coefficients lie in the tails of the distribution, providing further support for the validity of the identification assumption.

Overall, these robustness checks underscore the reliability and validity of the main findings, reinforcing the conclusion that exposure to international students positively impacts social cohesion without adverse effects on labor market outcomes. The consistency of the results across alternative models, sample definitions, treatment definitions, and a placebo exercise provides strong evidence that the observed effects are not artifacts but represent a genuine causal relationship

[13]Programs that are only offered at one university in a given year are excluded from the estimation.

(Altonji et al., 2005; Oster, 2019).

4.4 Discussion

To contextualize my estimates, I compare the findings of this study with two bodies of literature: *(i)* the effects of interacting with minority groups on prejudice and *(ii)* the spillover effects of international students on native peers.

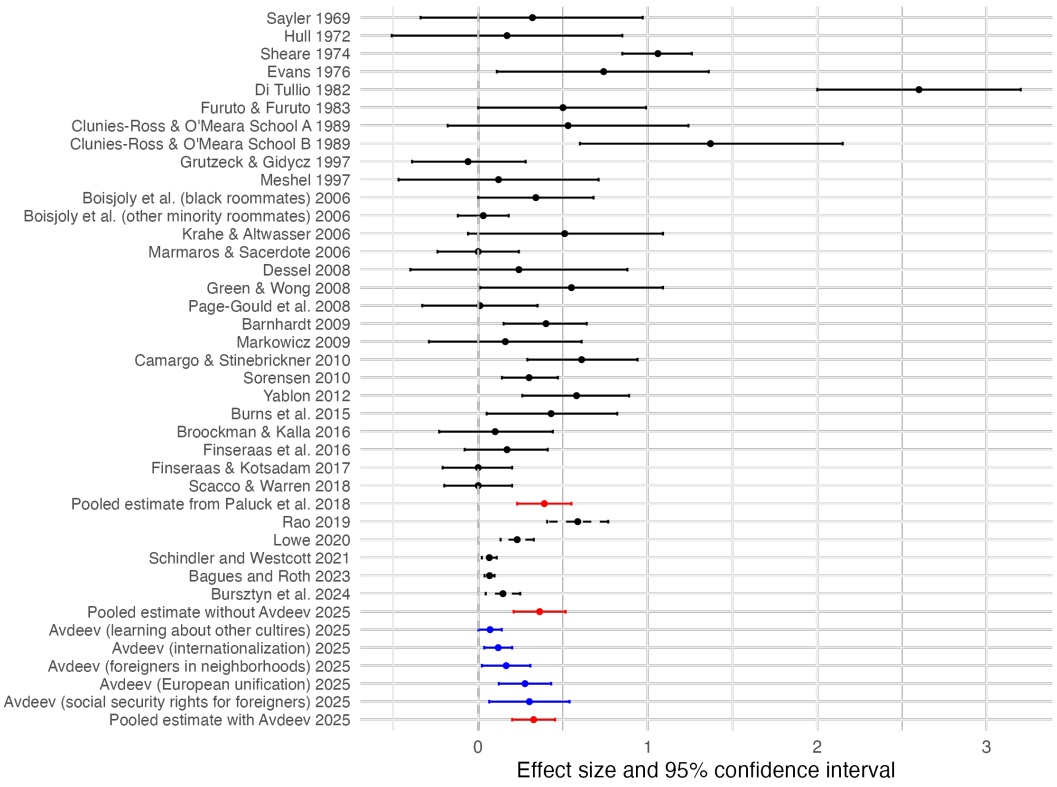
Interaction with minority groups on prejudice. I compare my estimates on the effects of exposure to a higher share of international students on attitudes towards internationalization and migration with studies examining similar outcomes. For instance, Paluck et al. (2019) provides a meta-analysis of the effects of intergroup contact with various minority groups (ethnic, racial, etc.) on prejudice of the majority group. Paluck et al. (2019) estimates a pooled effect size of 0.39 SD with a 95% confidence interval $[0.23, 0.55]$.

My three estimates of the effect on attitudes towards migration are 0.17 SD, 0.28 SD, and 0.30 SD in absolute terms. The latter two fall within the confidence interval of the pooled effect size from Paluck et al. (2019), suggesting that exposure to international students has a comparable impact to interventions studied in the contact hypothesis literature. This finding is notable because, unlike direct interventions, such as structured intergroup programs or assignment of roommates from different backgrounds, exposure to international students occurs more passively through everyday interactions. The fact that even this less intensive form of contact produces similar effects suggests that diversity in regular social environments can meaningfully shape attitudes towards migration.

Peer effects of international students. I then compare my estimates with the existing literature on the peer effects of international students. Figure 3 summarizes estimates from previous studies (Chevalier et al., 2020; Anelli et al., 2023; Costas-Fernández et al., 2023; Rakesh, 2023). Since no meta-analytical study exists, I calculate a pooled random effects meta-analytic estimate for comparison. Additionally, I present findings on how exposure to international students influences the educational outcomes of native students in my sample in Table A11.

While previous studies report small or no effects of international students on

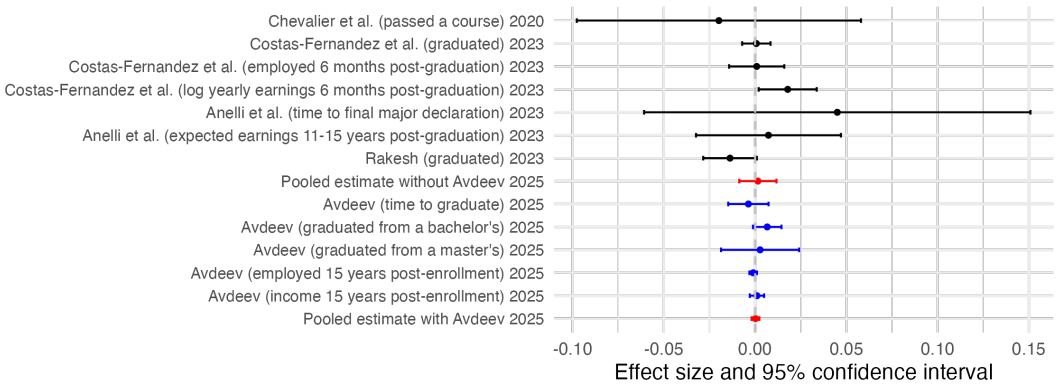
Figure 2: Comparison with estimates from the contact hypothesis literature



Notes: This graph presents estimates from 27 studies (black) included in a meta-analysis by Paluck et al. (2019), 3 pooled random effects estimates (red), estimates from Rao (2019), Lowe (2021), Schindler and Westcott (2021), Bagues and Roth (2023), and Bursztyn et al. (2024) (dashed), and estimates from the current paper (blue). One pooled estimate is from the meta-analysis by Paluck et al. (2019), while the second one additionally includes the estimates from Rao (2019), Lowe (2021), Schindler and Westcott (2021), Bagues and Roth (2023), Bursztyn et al. (2024), and the third one additionally includes estimates from the current paper. Paluck et al. (2019) includes studies that randomly assigned intergroup contact and measured outcomes more than a day after treatment. Effect size represents the ratio of the estimated coefficient to the standard deviation of the outcome. The estimate from Schindler and Westcott (2021) and the estimate for the effect of exposure on attitudes towards foreigners in a neighborhood (foreigners in neighborhoods) are reversed so that a positive coefficient indicates an improved attitude to ensure comparability.

the educational outcomes of native students, they do not establish precise nulls. The pooled meta-analytic estimate, excluding my estimates, is 0.2% with a 95% confidence interval $[-0.009, 0.012]$. My estimates are more precise, allowing me

Figure 3: Comparison with estimates from the international students literature



Notes: This graph presents estimates from 4 studies (black) by Chevalier et al. (2020); Costas-Fernández et al. (2023); Anelli et al. (2023); Rakesh (2023), 2 pooled random effects estimates calculated by the author (red), and estimates from the current paper (blue). One pooled estimate is based on the estimates from previous studies, while the second one additionally includes the estimates from the current paper. All estimates are converted to represent a 10 percentage point increase in the share of international students. Effect size represents the ratio of the estimated coefficient to the sample mean of the outcome. The estimate from Chevalier et al. (2020) is reversed from "failed a course" to "passed a course" to ensure comparability. Table A10 provides an overview of the estimates from previous research. Table A11 shows the estimates of the current paper on educational attainment.

to rule out even very small positive and negative effects. Moreover, I extend the analysis to long-term labor market outcomes, which have been previously unexplored. When my estimates are incorporated into the meta-analysis, the pooled estimate is 0.0% with a tighter 95% confidence interval $[-0.002, 0.002]$. This provides evidence that studying with international students has not only no statistically significant impact but also no economically meaningful effect on the educational and labor market outcomes of native students.

5 Conclusion

This paper provides the first evidence on the long-term impact of international students on native students. Using three decades of Dutch survey and administrative data and exploiting variation in the share of international students within university programs over time, this study identifies several key findings. A higher

share of international students in the first year of a bachelor's program strengthens native students' connections with non-natives, improves attitudes towards migration and cultural learning, and increases openness to migration. Importantly, exposure has no negative effects on native students' educational and labor market outcomes.

This study contributes to the literature on the contact hypothesis and higher education by focusing on the understudied group of international students. The findings underscore the dual importance of social and human capital considerations in shaping higher education funding and immigration policies. A better understanding of these relationships can help educational leaders and policymakers make informed decisions that enhance educational outcomes while fostering a more inclusive society. Policies that restrict international student enrollment may, therefore, limit broader societal benefits beyond tuition revenue.

Previous research has shown that international students generate positive fiscal returns (Bolhaar et al., 2019). The current paper demonstrates that they do not harm native students' educational and labor market prospects while contributing to social cohesion. Future research could explore the effects of international students on housing markets to further assess their broader impact.

Appendix A: Variable Construction

In this section, I describe how I construct the variables.

Cohabitation. Each year, I check whether a person is registered as living with someone at the same address. I then filter for non-native individuals at the same address. The variable equals 1 if the person has been registered as living with a non-native person, and 0 otherwise.

Marriage. Each year, I check whether a person is married or in a registered partnership. I then select only non-native spouses. If a person has more than one marriage or registered partnership in a year, I retain the latest record. The variable equals 1 if the person has been married or in a registered partnership with a non-native person, and 0 otherwise.

Emigration. This variable equals 1 if a person is no longer registered as living in the Netherlands and has not been reported as deceased, and 0 otherwise.

% of non-native co-workers. This variable represents the percentage of co-workers with non-Dutch nationality in a firm in the Netherlands.

Employed. This variable equals 1 if a person is employed and 0 otherwise.

Income percentile. For each individual, I calculate their income percentile within their same-age group for a given year, separately for men and women. This variable ranges between 0 and 1.

Entrepreneur. This variable equals 1 if a person is an entrepreneur and 0 otherwise.

LISS survey questions. Since LISS is a panel survey, I calculate the average response for each individual to a given question over years.

NSE survey questions. For the NSE survey, which provides only aggregated responses, I assign each student the average response to a given question.

Appendix B: Tables

Table A1: The effect of exposure to international students on the social ties of native students

Panel A	Cohabited with a classmate	Cohabited with a non-native outside the program	Cohabited with a non-native outside the university
International share in 10%-points	-0.003	0.006***	0.005***
	(0.002)	(0.001)	(0.001)
Mean	0.063	0.101	0.100
Cohorts	1988-2008	1988-2008	1988-2008
N	605,365	605,365	605,365
Panel B	Married to a classmate	Married to a non-native outside the program	Married to a non-native outside the university
International share in 10%-points	-0.001	0.001**	0.001*
	(0.001)	(0.001)	(0.001)
Mean	0.015	0.035	0.035
Cohorts	1988-2008	1988-2008	1988-2008
N	587,674	587,674	587,674

Notes: All regressions include program and cohort fixed effects. A non-native is defined as a person without Dutch nationality. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2: The effect of exposure to international students on attitudes towards internationalization

	Coefficient	s.e.	Mean	N
Panel A: Selection into the NSS panel				
1. Is a program observed in the NSS panel? (0 - no, 1 - yes)	-0.007	(0.011)	0.911	381,671
2. How satisfied are you with the level of internationalization? (0 - not answered, 1 - answered)	0.000	(0.000)	0.354	347,615
3. How satisfied are you with the level of encouragement to learn about other cultures? (0 - not answered, 1 - answered)	0.000	(0.000)	0.354	347,615
Panel B: Linear regression with weights				
1. How satisfied are you with the level of internationalization? (1 - very dissatisfied, 5 - very satisfied)	0.060***	(0.019)	3.524	123,038
2. How satisfied are you with the level of encouragement to learn about other cultures? (1 - very dissatisfied, 5 - very satisfied)	0.046***	(0.017)	3.197	123,038
Panel C: Ordered probit regression				
1. How satisfied are you with the level of internationalization? (1 - very dissatisfied, 5 - very satisfied)	0.156***	(0.011)	3.524	123,038
2. How satisfied are you with the level of encouragement to learn about other cultures? (1 - very dissatisfied, 5 - very satisfied)	0.068***	(0.010)	3.197	123,038

Notes: A coefficient represents a 10 percentage point increase in the share of international students. In Panels A and B, all regressions include program and cohort fixed effects. In Panel C, all regressions include the share of international students after residualizing it for program and cohort fixed effects. In Panel B, I weigh observations by the ratio of students who answered the survey to the total number of students enrolled in the program. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A3: The effect of exposure to international students on attitudes towards migration

	Coefficient	s.e.	Mean	N
Panel A: Selection into the LISS panel and answering a question				
1. Is a person observed in the LISS panel? (0 - no, 1 - yes)	0.000	(0.000)	0.001	1,037,345
2. Do you agree that people of foreign origin who legally reside in the Netherlands should be entitled to the same social security as Dutch citizens? (0 - not answered, 1 - answered)	-0.038	(0.033)	0.880	1,346
3. Do you agree that it does not help a neighborhood if many people of foreign origin or descent move in? (0 - not answered, 1 - answered)	-0.038	(0.033)	0.880	1,346
4. Where would you place yourself on a scale from 1 to 5, where 1 means that European unification has already gone too far and 5 means that it should go further? (0 - not answered, 1 - answered)	-0.036	(0.033)	0.869	1,346
Panel B: Linear regression with weights				
1. Do you agree that people of foreign origin who legally reside in the Netherlands should be entitled to the same social security as Dutch citizens? (1 - fully disagree, 5 - fully agree)	0.280**	(0.127)	3.790	1,184
2. Do you agree that it does not help a neighborhood if many people of foreign origin or descent move in? (1 - fully disagree, 5 - fully agree)	-0.158**	(0.062)	3.260	1,184
3. Where would you place yourself on a scale from 1 to 5, where 1 means that European unification has already gone too far and 5 means that it should go further?	0.160*	(0.083)	2.147	1,170
Panel C: Ordered probit regression				
1. Do you agree that people of foreign origin who legally reside in the Netherlands should be entitled to the same social security as Dutch citizens? (1 - fully disagree, 5 - fully agree)	0.295***	(0.104)	3.790	1,184
2. Do you agree that it does not help a neighborhood if many people of foreign origin or descent move in? (1 - fully disagree, 5 - fully agree)	-0.189**	(0.102)	3.260	1,184
3. Where would you place yourself on a scale from 1 to 5, where 1 means that European unification has already gone too far and 5 means that it should go further?	0.252***	(0.102)	2.147	1,170

Notes: A coefficient represents a 10 percentage point increase in the share of international students. In Panels A and B, all regressions include program and cohort fixed effects. In Panel C, all regressions include the share of international students after residualizing it for program and cohort fixed effects. In Panel B, I weigh observations by the number of students from the program. The scale of question 4 in Panel A, and question 3 in Panels B and C is reversed compared to the original. Standard errors, which are clustered at the program level, are reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A4: Heterogeneous effects of exposure to international students on the outcomes of native students 15 years post-enrollment

Panel A: Sex	Cohabited with a non-native	Married to a non-native	Emigrated	% of non-native co-workers	Employed	Income percentile	Entrepreneur
International share	0.006***	0.002*	0.002	0.000	-0.002**	0.001	0.001
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
x 𝟙 [Female = 1]	-0.001	-0.001	0.001	0.001	0.002	0.001	-0.001
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)
Mean for males	0.115	0.044	0.072	0.065	0.952	0.698	0.069
Mean for females	0.089	0.026	0.058	0.048	0.932	0.738	0.074
N	605,365	587,674	617,092	427,685	545,635	545,635	545,635
Panel B: Field of study							
International share	0.007***	0.002**	0.003***	0.001	-0.001	0.001	0.000
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
x 𝟙 [STEM = 1]	-0.011***	-0.001	-0.005	-0.003*	0.000	0.003	0.002
	(0.004)	(0.003)	(0.004)	(0.002)	(0.003)	(0.004)	(0.003)
Mean for non-STEM	0.098	0.033	0.060	0.051	0.940	0.718	0.082
Mean for STEM	0.115	0.043	0.084	0.076	0.950	0.718	0.058
N	605,365	587,674	617,092	427,685	545,635	545,635	545,635
Panel C: Share of females							
International share	0.006***	0.002**	0.003***	0.001	0.001	0.001	0.001
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
x 𝟙 [Share is below 20% = 1]	-0.012**	-0.002	-0.005	-0.002	-0.007**	-0.001	-0.003
	(0.006)	(0.004)	(0.005)	(0.003)	(0.003)	(0.004)	(0.003)
Mean for above 20%	0.100	0.033	0.063	0.053	0.941	0.714	0.082
Mean for below 20%	0.116	0.047	0.084	0.079	0.954	0.740	0.045
N	605,365	587,674	617,092	427,685	545,635	545,635	545,635
Panel D: Program size							
International share	0.008***	0.001	0.004***	0.002**	-0.002*	0.001	0.001
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
x 𝟙 [Size is below average = 1]	-0.008***	0.001	-0.006**	-0.003***	0.002	-0.003	0.001
	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)
Mean for above average	0.098	0.033	0.061	0.053	0.946	0.732	0.076
Mean for below average	0.111	0.040	0.075	0.064	0.934	0.683	0.080
N	605,365	587,674	617,092	427,685	545,635	545,635	545,635

Notes: All regressions include program and cohort fixed effects as well as their interactions with an indicator variable. A non-native is defined as a person without Dutch nationality. In Panel D, the indicator equals 1 if the program size is smaller than the average, which is 137. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A5: The effect of exposure to international students on the outcomes of native students 5 and 25 years post-enrollment

Panel A: 5 years post-enrollment	Cohabited with a non-native	Married to a non-native	Emigrated	% of non-native co-workers	Employed	Income percentile	Entrepreneur
International share in 10%-points	0.007***	0.000	0.002***	0.002***	-0.003	0.001	0.000
	(0.001)	(0.000)	(0.000)	(0.001)	(0.006)	(0.004)	(0.000)
Mean	0.034	0.003	0.009	0.069	0.386	0.384	0.011
Cohorts	1989-2018	1988-2018	1988-2018	2001-2018	1998-2017	1998-2017	1998-2017
N	953,536	953,059	959,569	520,609	653,526	653,526	653,526
Panel B: 25 years post-enrollment							
International share in 10%-points	0.004*	0.003**	0.000	0.000	0.002	0.002	-0.001
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
Mean	0.118	0.053	0.082	0.057	0.915	0.719	0.129
Cohorts	1988-1998	1988-1998	1988-1998	1988-1998	1988-1997	1988-1997	1988-1997
N	311,269	300,019	315,777	236,120	265,944	265,944	265,944

Notes: All regressions include program and cohort fixed effects. A non-native is defined as a person without Dutch nationality. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A6: Robustness to alternative models and samples

Panel A: Cohabited with a non-native	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
International share	0.006***	0.006***	0.006***	0.006***	0.006***	0.006***	0.006***	0.008***	0.006***	0.007***
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	605,365	605,365	605,365	605,365	605,365	605,365	605,365	637,722	615,407	650,178
Panel B: Married to a non-native										
International share	0.001**	0.001**	0.001**	0.002**	0.001*	0.002**	0.002**	0.002**	0.001*	0.002**
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	587,674	587,674	587,674	587,674	587,674	587,674	587,674	617,032	597,495	629,122
Panel C: Emigrated										
International share	0.003***	0.003***	0.003***	0.003**	0.003***	0.002**	0.002**	0.005***	0.002**	0.004***
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	617,092	617,092	617,092	617,092	617,092	617,092	617,092	651,647	627,140	664,268
Panel D: % of non-native co-workers										
International share	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001*	0.001	0.001*
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	427,685	427,685	427,685	427,685	427,685	427,685	427,685	447,155	432,393	453,027
Panel E: Employed										
International share	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	545,635	545,635	545,635	545,635	545,635	545,635	545,635	572,271	555,092	583,877
Panel F: Income percentile										
International share	0.001	0.001	0.001	0.000	0.002	0.002	0.001	0.000	0.001	0.001
in 10%-points	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
N	545,635	545,635	545,635	545,635	545,635	545,635	545,635	572,271	555,092	583,877
Panel G: Entrepreneur										
International share	0.001	0.001	0.001	0.001	0.001	0.000	0.000	0.001	0.001	0.001
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	545,635	545,635	545,635	545,635	545,635	545,635	545,635	572,271	555,092	583,877
Controls										
Field specific linear time trend		✓					✓			
Field specific quadratic time trend			✓				✓			
Program size				✓			✓			
Individual characteristics					✓		✓			
Peer characteristics						✓	✓			
Sample										
First-generation immigrants								✓		✓
Students older than 30 at enrollment									✓	✓

Notes: All regressions include program and cohort fixed effects. A non-native is defined as a person without Dutch nationality. Individual characteristics include sex, age at enrollment, migration status, family size, pre-vocational diploma, gap year indicator. Peer characteristics include the share of females, average age at enrollment, the share of second generation migrants, average family size, the share with a pre-vocational diploma, the share with a gap year. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A7: Robustness to alternative models and samples

Panel A: Satisfaction with the level of internationalization	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
International share	0.053***	0.053***	0.053***	0.053***	0.053***	0.052***	0.052***	0.053***	0.054***	0.053***
in 10%-points	(0.019)	(0.019)	(0.019)	(0.020)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
N	123,038	123,038	123,038	123,038	123,038	123,038	123,038	129,064	123,918	130,256
Panel B: Satisfaction with the level of encouragement to learn about other cultures										
International share	0.038**	0.038**	0.038**	0.037*	0.038**	0.043**	0.042**	0.038**	0.037**	0.037**
in 10%-points	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)	(0.019)	(0.019)	(0.019)
N	123,038	123,038	123,038	123,038	123,038	123,038	123,038	129,064	123,918	130,256
Panel C: Social security rights for foreigners										
International share	0.237**	0.237**	0.237**	0.257**	0.226**	0.258***	0.260***	0.230**	0.237**	0.230**
in 10%-points	(0.095)	(0.096)	(0.097)	(0.106)	(0.093)	(0.081)	(0.097)	(0.092)	(0.095)	(0.091)
N	1,184	1,184	1,184	1,184	1,184	1,184	1,184	1,255	1,197	1,271
Panel D: Foreigners in neighborhoods										
International share	-0.140**	-0.140**	-0.140**	-0.158**	-0.116*	-0.125**	-0.125*	-0.119*	-0.140**	-0.119*
in 10%-points	(0.062)	(0.063)	(0.064)	(0.066)	(0.061)	(0.063)	(0.069)	(0.063)	(0.062)	(0.062)
N	1,184	1,184	1,184	1,184	1,184	1,184	1,184	1,255	1,197	1,271
Panel E: European unification										
International share	0.238***	0.238***	0.238***	0.234***	0.248***	0.194**	0.181**	0.206***	0.238***	0.205***
in 10%-points	(0.068)	(0.068)	(0.069)	(0.065)	(0.070)	(0.079)	(0.084)	(0.068)	(0.068)	(0.068)
N	1,170	1,170	1,170	1,170	1,170	1,170	1,170	1,239	1,183	1,254
Controls										
Field specific linear time trend		✓					✓			
Field specific quadratic time trend			✓				✓			
Program size				✓			✓			
Individual characteristics					✓		✓			
Peer characteristics						✓	✓			
Sample										
First-generation immigrants								✓		✓
Students older than 30 at enrollment									✓	✓

Notes: All regressions include program and cohort fixed effects. A non-native is defined as a person without Dutch nationality. Individual characteristics include sex, age at enrollment, migration status, family size, pre-vocational diploma, gap year indicator. Peer characteristics include the share of females, average age at enrollment, the share of second generation migrants, average family size, the share with a pre-vocational diploma, the share with a gap year. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A8: Sensitivity to alternative definitions of international students

Panel A: Cohabited with a non-native	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
International share	0.006***	0.006***	0.005***	0.006***	0.005***	0.005***	0.008***	0.003
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.006)
N	605,365	605,365	605,365	605,355	605,365	605,365	605,365	605,365
Panel B: Married to a non-native								
International share	0.001**	0.002**	0.001	0.000	0.001**	0.002***	0.001	0.003
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
N	587,674	587,674	587,674	587,664	587,674	587,674	587,674	587,674
Panel C: Emigrated								
International share	0.003***	0.003**	0.003***	0.003***	0.003***	0.002*	0.004***	-0.001
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)
N	617,092	617,092	617,092	617,082	617,092	617,092	617,092	617,092
Panel D: % of non-native co-workers								
International share	0.001	0.001	0.001	0.001	0.001*	0.001*	0.002	-0.002
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
N	427,685	427,685	427,685	427,685	427,685	427,685	427,685	427,685
Panel E: Employed								
International share	-0.001	-0.001*	-0.002	-0.001	-0.001	-0.002**	-0.002	0.000
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)
N	545,635	545,635	545,635	545,625	545,635	545,635	545,635	545,635
Panel F: Income percentile								
International share	0.001	0.001	-0.001	0.000	-0.001	-0.003**	0.000	0.006
in 10%-points	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.006)
N	545,635	545,635	545,635	545,625	545,635	545,635	545,635	545,635
Panel G: Entrepreneur								
International share	0.001	0.001	0.001	0.001	0.002*	0.002**	0.000	0.004
in 10%-points	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)
N	545,635	545,635	545,635	545,625	545,635	545,635	545,635	545,635
Definition of international students is based on								
Nationality and high school completion	✓							
Nationality		✓						
High school completion			✓					
Residency before enrollment				✓				
Country of birth					✓			
Parent's country of birth						✓		
EEA+ nationality							✓	
Non-EEA+ nationality								✓

Notes: All regressions include program and cohort fixed effects. A non-native is defined as a person without Dutch nationality. EEA+ nationality refers to individuals from the European Economic Area (the EU, Iceland, Liechtenstein, and Norway) plus Switzerland. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A9: Sensitivity to alternative definitions of international students

Panel A: Satisfaction with the level of internationalization	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
International share	0.053***	0.056***	0.052***	0.054***	0.051***	0.046***	0.061***	0.101
in 10%-points	(0.019)	(0.019)	(0.018)	(0.021)	(0.019)	(0.020)	(0.021)	(0.071)
N	123,038	123,038	123,038	123,038	123,038	123,038	123,038	123,038
Panel B: Satisfaction with the level of encouragement to learn about other cultures								
International share	0.038**	0.042**	0.028	0.030	0.038**	0.047**	0.033	0.140**
in 10%-points	(0.019)	(0.019)	(0.019)	(0.021)	(0.019)	(0.020)	(0.022)	(0.065)
N	123,038	123,038	123,038	123,038	123,038	123,038	123,038	123,038
Panel C: Social security rights for foreigners								
International share	0.237**	0.229**	0.202**	0.221**	0.235**	0.217***	0.201**	0.775
in 10%-points	(0.095)	(0.095)	(0.095)	(0.094)	(0.095)	(0.083)	(0.097)	(0.559)
N	1,184	1,184	1,184	1,184	1,184	1,184	1,184	1,184
Panel D: Foreigners in neighborhoods								
International share	-0.140**	-0.140**	-0.129**	-0.128**	-0.145**	-0.136**	-0.123**	-0.602
in 10%-points	(0.062)	(0.062)	(0.060)	(0.064)	(0.067)	(0.067)	(0.054)	(0.421)
N	1,184	1,184	1,184	1,184	1,184	1,184	1,184	1,184
Panel E: European unification								
International share	0.238***	0.237***	0.218***	0.230***	0.247***	0.231***	0.247***	0.598
in 10%-points	(0.068)	(0.066)	(0.067)	(0.067)	(0.066)	(0.073)	(0.059)	(0.371)
N	1,170	1,170	1,170	1,170	1,170	1,170	1,170	1,170
Definition of international students is based on								
Nationality and high school completion	✓							
Nationality		✓						
High school completion			✓					
Residency before enrollment				✓				
Country of birth					✓			
Parent's country of birth						✓		
EEA+ nationality							✓	
Non-EEA+ nationality								✓

Notes: All regressions include program and cohort fixed effects. A non-native is defined as a person without Dutch nationality. EEA+ nationality refers to individuals from the European Economic Area (the EU, Iceland, Liechtenstein, and Norway) plus Switzerland. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A10: Overview of estimates on the impact of international students in higher education

Paper	Country	Data	Sample	Measure	Relevant outcomes	Results
Chevalier et al. (2020)	UK	Administrative data from one university and a survey of graduates	4,032 1,581	9%-points (SD)	Failed a course Abroad 6 months post-graduation	0.029 (0.058) 0.015 (0.032)
Costas-Fernández et al. (2023)	UK	Administrative data and a survey of graduates	509,870 315,215 124,305	18.5%-points (SD)	Graduated from a university Employed 6 months post-graduation Log yearly earnings 6 months post-graduation	0.001 (0.006) 0.001 (0.009) 0.033*** (0.015)
Anelli et al. (2023)	US	Administrative data from one university	16,828	4.4%-points (SD)	Time to final major declaration Expected earnings 11-15 years post-graduation	0.100 (0.120) 0.031 (0.086)
Rakesh (2023)	US	Administrative data from one university	29,246	10%-points	Graduated from a university	-0.011* (0.006)

Notes: Measure denotes the unit used to quantify the share of international students. "SD" signifies that a paper quantified this measure in terms of standard deviations, while the term "%-points" indicates the magnitude of these standard deviations. Relevant outcomes are those findings from the literature that can be directly compared to the outcomes of this study.

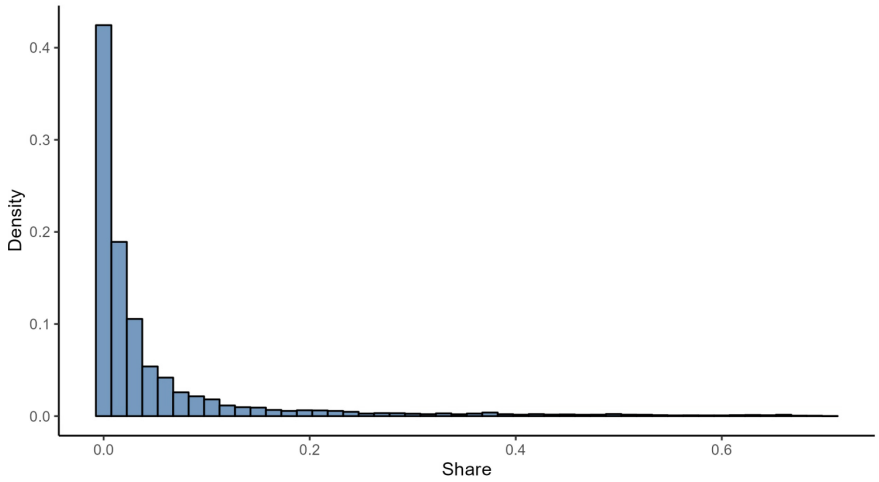
Table A11: The effect of exposure to international students on native students' educational outcomes

	Time to graduate	Bachelor's degree completion	Master's degree completion
International share in 10%-points	-0.017	0.005*	0.001
	(0.026)	(0.003)	(0.004)
Mean	4.613	0.754	0.366
N	782,589	1,037,345	1,037,345

Notes: All regressions include program and cohort fixed effects. Standard errors, which are clustered at the program level, are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

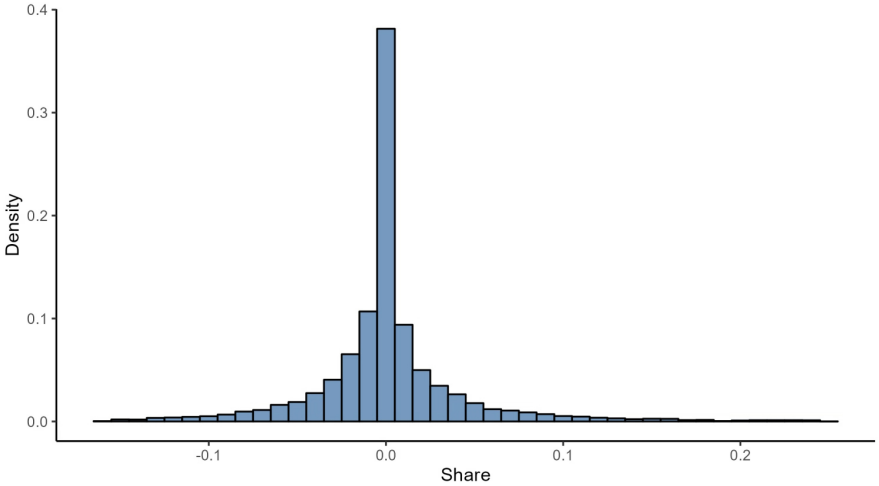
Appendix C: Figures

Figure A1: Histogram of the share of international students by program × year of enrollment



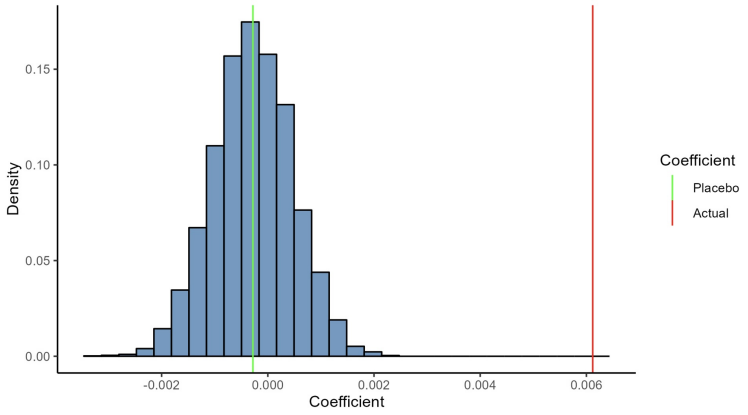
Notes: This histogram shows the plot of the share of international students calculated by program × year of enrollment. For display clarity, the top 1% of the data is winsorized.

Figure A2: Histogram of the residualized share of international students by program × year of enrollment



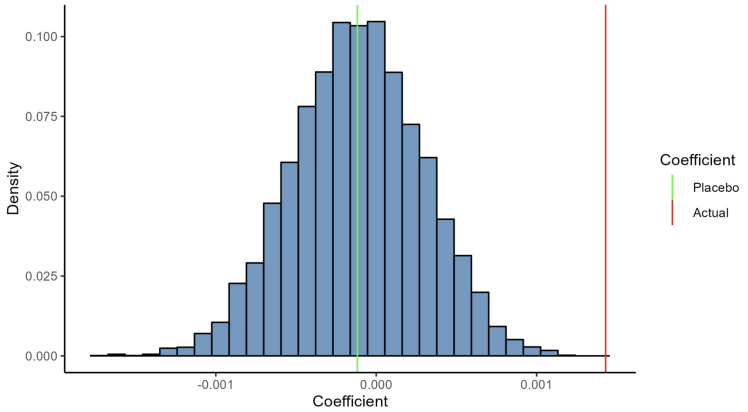
Notes: This histogram shows the plot of residuals of the share of international students calculated by program × year of enrollment after partialing out for program and cohort fixed effects. For display clarity, the top and bottom 1% of the data are winsorized.

Figure A3: Counterfactual treatment effects for cohabitation with a non-native



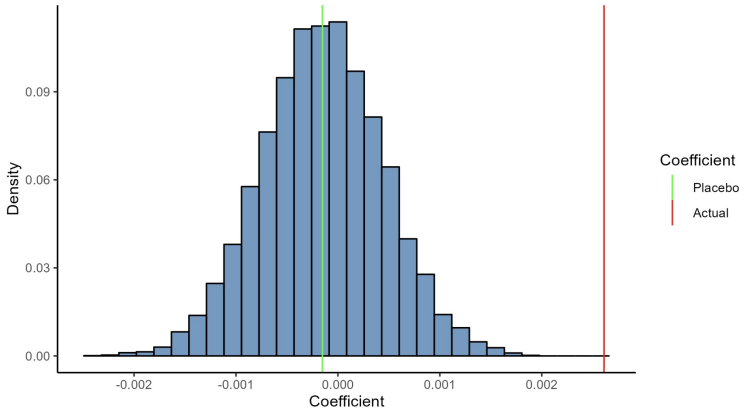
Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). All regressions include program and cohort fixed effects.

Figure A4: Counterfactual treatment effects for marriage to a non-native



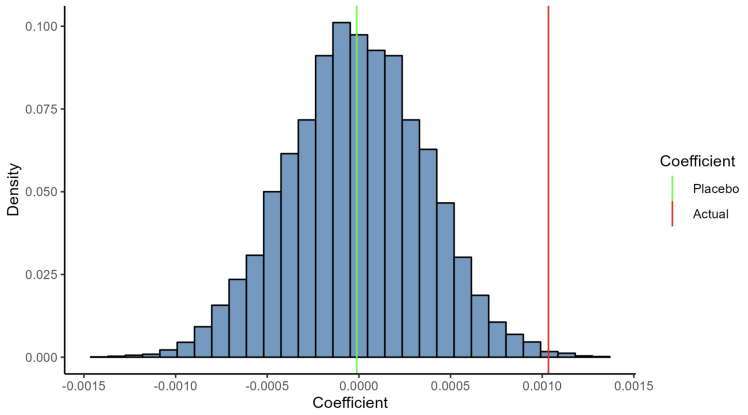
Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). All regressions include program and cohort fixed effects.

Figure A5: Counterfactual treatment effects for emigration



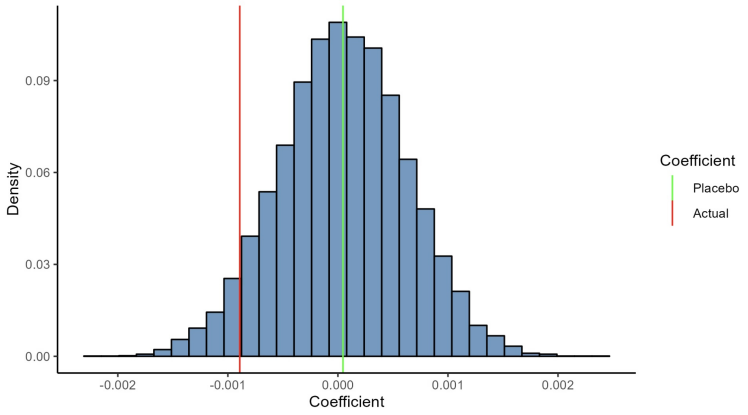
Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). All regressions include program and cohort fixed effects.

Figure A6: Counterfactual treatment effects for the share of non-native co-workers



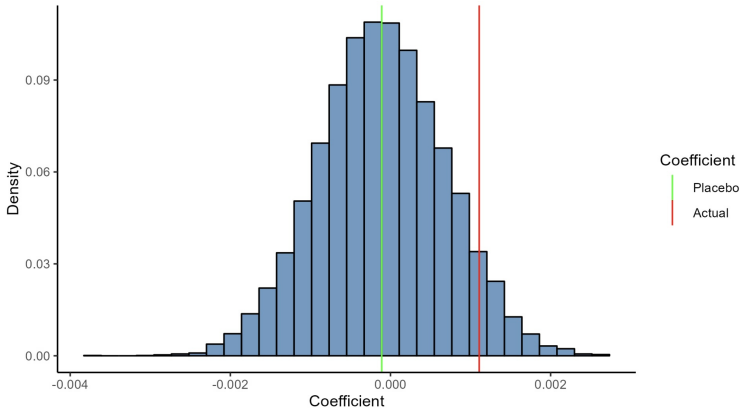
Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). All regressions include program and cohort fixed effects.

Figure A7: Counterfactual treatment effects for employment



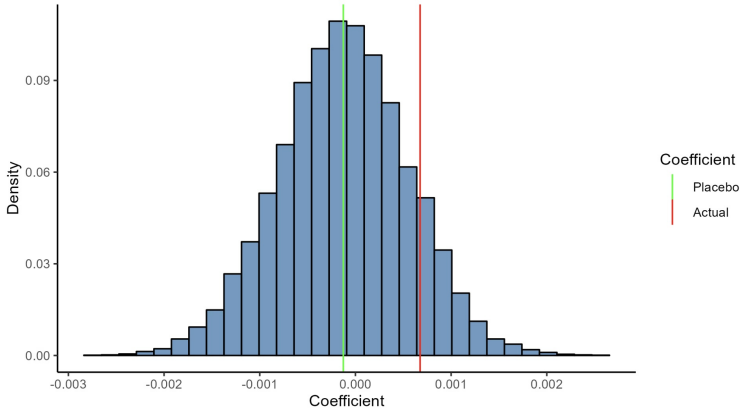
Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). All regressions include program and cohort fixed effects.

Figure A8: Counterfactual treatment effects for income percentile



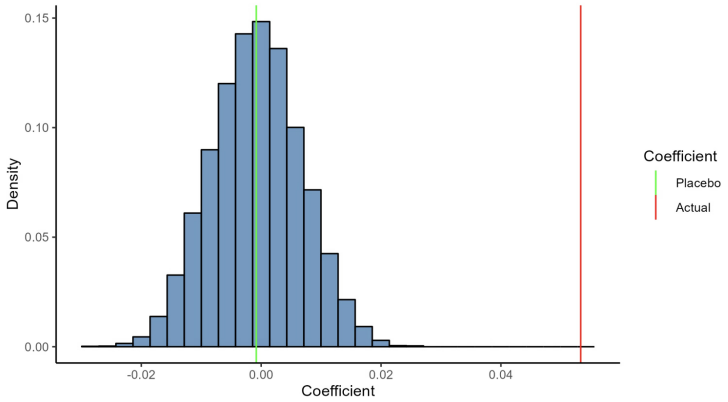
Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). All regressions include program and cohort fixed effects.

Figure A9: Counterfactual treatment effects for entrepreneur



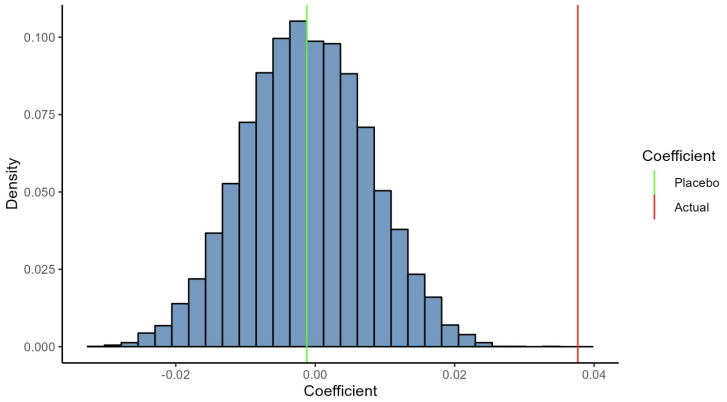
Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). All regressions include program and cohort fixed effects.

Figure A10: Counterfactual treatment effects for satisfaction with the level of internationalization



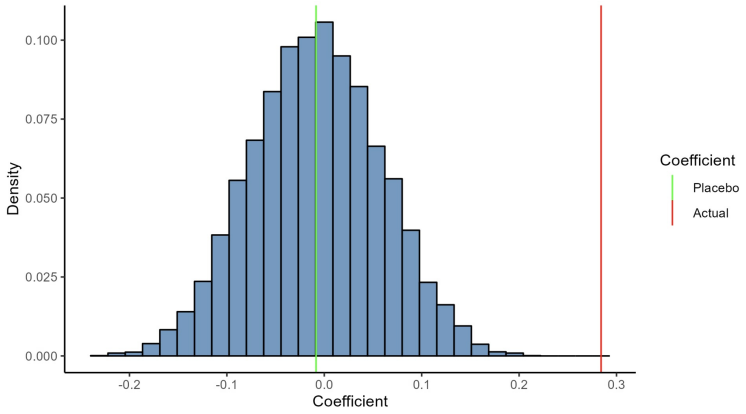
Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). All regressions include program and cohort fixed effects.

Figure A11: Counterfactual treatment effects for satisfaction with the level of encouragement to learn about other cultures



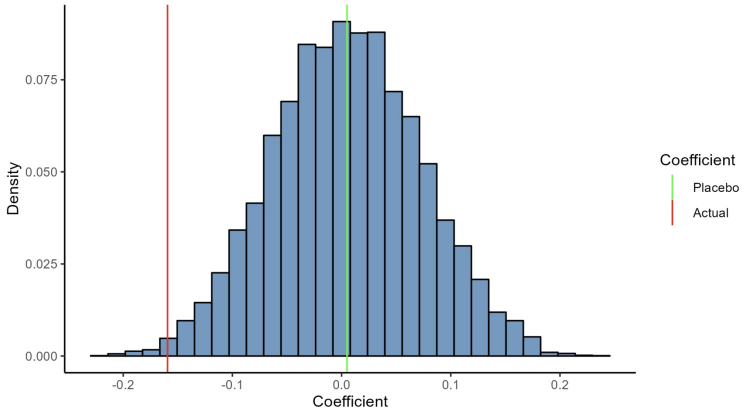
Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). All regressions include program and cohort fixed effects.

Figure A12: Counterfactual treatment effects for social security rights for foreigners



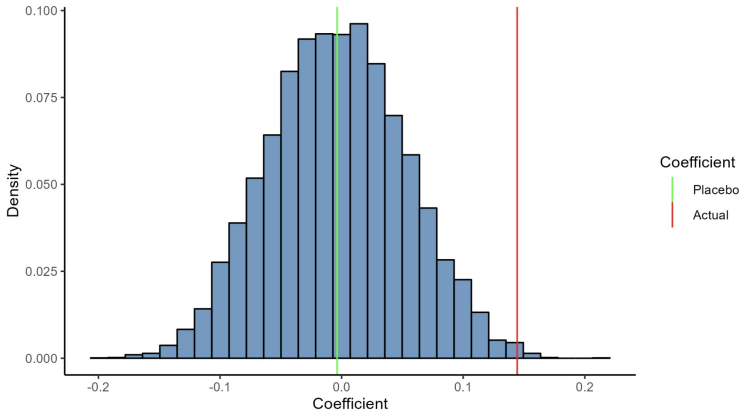
Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). A non-native is defined as a person without Dutch nationality. All regressions include program and cohort fixed effects.

Figure A13: Counterfactual treatment effects for foreigners in neighborhoods



Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). All regressions include program and cohort fixed effects.

Figure A14: Counterfactual treatment effects for European unification



Notes: The plot displays estimates from 10,000 simulated datasets where instead of using the actual share of international students that native students are exposed to, I assign them a share of international students from the same program and year but a different university. It also shows the mean estimated coefficients of the randomly assigned shares (green), and the actual estimated coefficient (red). All regressions include program and cohort fixed effects.

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